Mind as Machine: A History of Cognitive Science, Volume 1&2

MARGARET A. BODEN

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A History of Cognitive Science

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For Ruskin and Claire,
Jehane and Alex,
and Byron, Oscar, and Lukas . . .

—and in memory of Drew Gartland-Jones (1964–2004)

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Tell me where is fancy bred?
Or in the heart or in the head?
How begot, how nourished?

(William Shakespeare,
The Merchant of Venice, III. ii)

What a piece of work is a man! how noble in reason! how infinite in faculty! in form and moving, how express and admirable! in action how like an angel! in apprehension how like a god! the beauty of the world! the paragon of animals!

(William Shakespeare, *Hamlet*, II. ii)

Even Clerk Maxwell, who wanted nothing more than to know the relation between thoughts and the molecular motions of the brain, cut short his query with the memorable phrase, "but does not the way to it lie through the very den of the metaphysician, strewn with the bones of former explorers and abhorred by every man of science?" Let us peacefully answer the first half of this question "Yes," the second half "No," and then proceed serenely.

Our adventure is actually a great heresy. We are about to conceive of the knower as a computing machine.

(Warren McCulloch 1948: 143)

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Finally, I thank my children and close friends for their love and support, and for not being bored stiff when I talked about how the writing was going.

M.A.B.

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PREFACE

Cardinal Mazarin's librarian had a low opinion of history books. In what time he could spare from his master's collection of 40,000 volumes—opened to the public in 1644, Thursdays only! (J. A. Clarke 1970)—Gabriel Naudé wrote some brief tracts himself. One was the first-ever book on library science (Naudé 1627/1644). Another, less well-known today, was a cry of outrage against historians (Naudé 1625/1657).

His specific accusation was that they'd maligned the growing band of automata makers as dangerous dabblers in illicit magic, instead of recognizing them as brave pioneers of the mathematical arts. His general complaint was that the authors of history books are "a sort of people seldome or never representing things truly and naturally, but shadowing them and making them according as they would have them appear" (1625/1657: 9).

That was a spot too fierce. ("Seldome"? "Never"??) After all, Naudé believed that his own history of automata making was close to the truth. But his basic point was correct. Every history is a narrative told for particular purposes, from a particular background, and with a particular point of view.

Someone who knows what those are is in a better position to understand the story being told. This preface, then, says what this history aims to do, and outlines the background and viewpoint from which it was written.

i. The Book

This is a historical essay, not an encyclopedia: it expresses one person's view of cognitive science as a whole. It's driven by my conviction that cognitive science today—and, for that matter, tomorrow—can't be properly understood without a historical perspective. In that sense, then, my account describes the field as it is *now*. It does this in a second sense too, for it features various examples of state-of-the-art research, all placed in their historical context.

Another way of describing it is to say that it shows how cognitive scientists have tried to answer myriad puzzling questions about minds and mental capacities. These questions are very familiar, for one doesn't need a professional licence to raise them. One just has to be intellectually alive. So although this story will be most easily read by cognitive scientists, I hope it will also interest others.

These puzzles are listed at the opening of Chapter 1. They aren't all about 'cognition', or knowledge. Some concern free will, for instance. What is it? Do we have it, or do we merely appear to have it? Under hypnosis, do we lose it? Does any other species have it? If not, why not? What is it about dogs' or crickets' minds, or brains, which denies them freedom? Above all, how is human free choice possible? *What type of system*, whether on Earth or Mars, is capable of freewill?

My account is focused on ideas, not anecdotes: it's not about who said what to whom over the coffee cups. Nevertheless, the occasional coffee cup does feature. Sometimes, a pithy personal reminiscence can speak volumes about what was going on at a certain time, and how different groups were reacting to it.

Nor does it explore sociopolitical influences at any length, although some are briefly mentioned—for instance, the seventeenth-century respect due to the word of a 'gentleman', the twentieth-century role of military funding, and the post-1960 counter-culture. In addition, I've said a little about how various aspects of cognitive science reached—or didn't reach—the general public, and how it was received by them. What's printed in the newspapers, accurate or (more usually) not, has influenced the field indirectly in a number of ways—and it has influenced our culture, too.

Mainly, however, I've tried to show how the central ideas arose—and how they came together. To grasp what cognitive science is trying to do, one needs to understand how the multidisciplinary warp and weft were interwoven in the one interdisciplinary field.

My text, too, holds together much as a woven fabric does. It's best read entire, as an integrated whole—not dipped into, as though it were a work of reference. Indeed, I can't resist quoting the King of Hearts' advice to the White Rabbit: "Begin at the beginning, and go on till you come to the end: then stop."

I realize, however, that many readers won't want to do that—though I hope they'll read the whole of Chapter 1 before starting on any of the others. Moreover, even reading a single chapter from beginning to end will typically leave lots of loose ends still hanging. Most of the important topics can't be properly understood without consulting *several* chapters. Freewill, for example, is addressed in more than one place (7.i.g–h, 14.x.b, and 15.vii). Similarly, nativism—alias the nature/nurture debate—is discussed in the context of:

* psychology: Chapters 5.ii.c and 7.vi;
* anthropology: 8.ii.c–d and iv–vi;
* linguistics: 9.ii–iv and vii.c–d;
* connectionism: 12.viii.c–e and x.d–e;
* neuroscience: 14.ix.c–d;
* and philosophy: 2.vi.a and 16.iv.c.

So besides the Subject Index, I've provided many explicit cross-references, to encourage readers to follow a single topic from one disciplinary chapter to another. Peppering the text with pointers saying "see Chapter *x*" isn't elegant, I'm afraid. But I hope it's useful, as the best I could do to emulate links in hypertext. (The King of Hearts, of course, hadn't heard of that.)

These pointers are intended as advice about what to look at next. They're helpful not least because I may have chosen to discuss a certain topic in a chapter *other than* the one in which you might expect to find it. (The theory of concepts as 'prototypes', for example, is discussed in the anthropology chapter, not the psychology one.) My placements have been decided partly in order to emphasize the myriad interdisciplinary links. So no chapter that's dedicated to one discipline avoids mention of several others.

History, it has been said, is "just one damn thing after another". Were that true, this account would be hardly worth the writing. In fact, any history is a constructed narrative, with a plot—or, at least, a reasonably coherent theme.

The plot can always be disputed (hence some of Naudé's scorn), and in any case usually wasn't obvious to the dramatis personae concerned. Several examples of work experienced at the time as thrilling new beginnings are described here, and with hindsight it's clear that some of them actually *were*. But I'll also describe examples where it looked as though the end had already come—or anyway, where it wasn't known whether/when there'd be a revival. As for future episodes of the story, no one can know now just what they'll be. I'll indicate some hunches (17.ii–iii), but with fingers firmly crossed.

In the case of cognitive science, *theme* is as problematic as *plot*. The field covers so many different topics that a single theme may not be immediately obvious. At a cursory glance, it can seem to be a hotch-potch of disparate items, more properly ascribed to quite distinct disciplines. Indeed, some people prefer to speak of "the cognitive *sciences*", accordingly (see 1.ii.a).

The key approaches are psychology, neuroscience, linguistics, philosophy, anthropology, AI (artificial intelligence), and A-Life (artificial life)—to each of which I've devoted at least one chapter. Control engineering is relevant too, for it provides one of the two theoretical 'footpaths' across the many disciplinary meadows of cognitive science (see Chapters 1.ii.a, 4.v–ix, 10.i.g, 12.vii, 14.viii–ix, and 15.viii.c.).

Ignorance of the field's history reinforces this ragbag impression. So does a specialist fascination with particular details. But my aim, here, is to see the wood as well as the trees. I want to help readers understand what cognitive science as a whole is trying to do, and what hope there is of its actually doing it.

Each discipline, in its own way, discusses the mind—asking what it is, what it does, how it works, how it evolved, and how it's even possible. Or, if you prefer to put it this way, each discipline asks about *mental processes* and/or about how the *mind/brain* works. (That doesn't prevent them asking also whether the emphasis on the mind/brain is too great: some say we should consider the mind—or rather, the person—as *embodied*, too. And some add that we should focus on *minds*, not on *mind*: that is, we should remember the essentially social dimension of humanity.)

Moreover, each discipline, in so far as it's relevant to cognitive science, focuses on computational and/or informational answers—whether to recommend them or to criticize them (see Chapter 1.ii).

These questions, and these answers, unify the field. In my view, the best way to think about it is as *the study of mind as machine*. As explained in Chapter 1.ii.a, however, more than one type of machine is relevant here. In a nutshell: some for digital computing, some for cybernetic self-organization or dynamical control. Much of the theoretical—and historical—interest in the field lies in the tension that follows from that fact.

In short, I've tried to give a coherent overview, showing how the several disciplines together address questions that most thinking people ask themselves, at some time in their lives.

Many trees would need to be felled for a fully detailed history of cognitive science, for every discipline would require at least one large volume. The prospect is daunting, the forests are already too empty, and life is too short. This account has a more modest aim: despite its length, it's a thumbnail sketch rather than a comprehensive record.

That means that decisions have to be made about what to mention and what to omit. So my story is unavoidably selective, not only in deciding what research to include but also in deciding which particular aspects of it to highlight.

Some of my selections may surprise you. On the one hand, you may find topics that you hadn't expected. For instance, the psychological themes include emotion, personality, social communication, and the brain's control of movement. (In other words, cognitive science *isn't* just the science of cognition: see Chapter 1.ii.) Other perhaps unexpected themes include evolutionary robotics, the mating calls of crickets, and the development of shape in embryos. However, all those topics are relevant if one wants to understand the nature of mind.

Moreover, quite a few of the people I discuss aren't in the mainstream. Some have been unjustly forgotten, while others hold views that are (currently) distinctly off-message.

Indeed, some aren't even in a sidestream, since they deny the possibility of *any* scientific explanation of mind. And some, such as Johann von Goethe, are highly unfashionable to boot. Other authors recounting the history of the field might not mention any of them. Nevertheless, I try to show that they're all relevant, in one way or another. Sometimes, admittedly, it's largely a question of *Know your enemy*! (see the 'aperitif' to Chapter 16). But even one's intellectual enemies usually have things of value to say.

On the other hand, I deliberately ignore some themes and names which you might have expected to encounter. In discussing linguistics, for example, I say almost nothing about phonetics, or about automatic speech processing. These aren't irrelevant, and they figure prominently in more specialist volumes. But the general points I want to stress can be better made by addressing other aspects of language.

Similarly, in my account of cybernetics only a few people feature strongly: Norbert Wiener, John von Neumann, Warren McCulloch, Gordon Pask, W. Grey Walter, W. Ross Ashby, and Kenneth Craik. Others (such as Gregory Bateson and Stafford Beer) are only briefly visible, but might have been featured at greater length. And some bit players don't appear in my pages at all. In a comprehensive volume devoted solely to cybernetics, one could try to mention all of them (see Heims 1991). In a history spanning cognitive science as a whole, one can't.

That space constraint applies in all areas, of course—so please forgive me if I haven't mentioned Squoggins! Indeed, please forgive me if I haven't mentioned someone *much* more famous than Squoggins: the characters in my narrative are numerous enough as it is.

Even those who do appear could have been discussed more fully, so as to do justice to the rich network of formative influences behind any individual's ideas. With respect to the origins of A-Life, for example, I mention the coffee-house conversations of von Neumann and Stanislaw Ulam (Chapter 15.v.a). But just how much credit should be given to Ulam? To answer that question—which I don't try to do—would require many more pages, including a discussion about how sowing an intellectual seed should be weighed against nurturing the developing plant. In short, to detail *every* researcher of any historical importance would be impossible.

Still less could one specify all *current* work. For such details, there are the numerous specialist textbooks—and, better still, the professional journals and conference proceedings. However, I've mentioned a range of up-to-date examples, in order to indicate how much—or, in some cases, how little—has changed since the early days.

Sir Herbert Read once said that whereas the art historian deals with the dead, the art critic deals with the living, an even more risky thing to do. Although I've written this book primarily as a historian (which of course involves a critical dimension), I've dipped my toes into the riskier waters of contemporary criticism too. That's implicit in my choices of what recent work to mention, and what to ignore. And in the final chapter, I've said which instances of current work I regard as especially promising. However, those choices are made from what's already a highly selective sample: contemporary cognitive science contains many more strands than I've had space to indicate.

So the bad news is that some things which merit discussion don't get discussed. The good news is that if you find the recent examples I've selected intriguing, you can be sure that there are more. Tomorrow, of course, there will be more still.

ii. The Background

One of the founders of cognitive science expressed Naudé's insight in less disgusted terms. As Jerome Bruner put it:

> The Past (with a capital letter) is a construction: how one constructs it depends on your perspective toward the past, and equally on the kind of future you are trying to legitimize. (Bruner 1997: 279).

The future I'm trying to legitimize here is one in which interdisciplinarity is valued and alternative theoretical approaches respected—and, so far as possible, integrated.

As for my perspective on the past, this springs from my own experience of the field over the past fifty years. Indeed, it's even longer than that if one includes my reasons for being drawn to it in the first place. For I was already puzzling over some of its central questions in my early-teenage years.

(I was born in 1936. I mention that, and give other researchers' years of birth whenever I could discover them, less to record the appearance of particular individuals on this planet than to indicate the passage of intellectual *generations*.)

I first encountered cognitive science in 1957, at the University of Cambridge. I'd just completed the degree in medical sciences there, during which time I'd been especially interested in neurophysiology and embryology.

The medical course was almost uniformly fascinating (although the biochemistry was fairly low on my list of priorities). I remember being intrigued by Lord Adrian's work on spinal reflexes and action potentials, and spellbound by Andrew Huxley's hot-off-the-press lecture on muscle contraction—which had earned him a standing ovation from the usually blasé medical students (2.viii.e). Likewise, I'd been amazed by Alan Turing's paper on morphogenesis, and entranced by D'Arcy Thompson's writings on mathematical biology (15.iii–iv).

I now had one year to spare before going—or so I thought—to St Thomas's Hospital in London. There, I would do my clinical training, as a prelude to a career in psychiatry.

My College expected me to spend the year specializing in neurophysiology, which indeed I found absorbing. And Cambridge was a superb place to do it. Besides the awe-inspiring Adrian–Huxley tradition, exciting new work was being done by Horace

Barlow: 14.iii.b. (He was one of my physiology demonstrators: many's the time he helped me to coax a frog's leg to move in a physiology practical.)

But that would have meant doing lengthy experiments on cats, and the comatose rabbits pinned out in my pharmacology practicals had been troubling enough. The neurophysiological experiments that could then be done were fairly broad-brush, since single-cell research had only just begun (2.viii.f). But I don't know that a unit-recording approach would have made much difference to the way I felt. (For a description of what this involves today, see J. A. Anderson *et al.* 1990*a*: 215.) My qualms were largely irrational, of course: not only would I not have felt quite the same about rats, but the cats would be anaesthetized, or decorticate, or even decerebrate. Nevertheless, I hesitated.

As for psychology as an alternative, I'd originally planned to do this in my third year—but the course at Cambridge had turned out to be too rat-oriented, and too optics-based, for my taste. I'd already gate-crashed all the psychopathology lectures, and for six weeks worked as a resident nursing assistant at Fulbourn mental hospital nearby. But mental illness, the psychological topic which interested me most, figured hardly at all in the curriculum. Perhaps that was because precious little could be done to help. (Psychotropic drugs were still a rarity: Largactil, aka chlorpromazine, was being given to schizophrenics on the ward I nursed on at Fulbourn, but that was because the hospital's director was exceptionally forward-looking.)

Moreover, I now knew something I hadn't realized until after my arrival at Cambridge, namely that universities offered degrees in philosophy. This was a revelation. (Without it, I'd probably have ignored my qualms and turned to the cats.)

I'd discovered philosophy while I was still at high school, and found it deeply engaging. I remember reading Bertrand Russell with excitement, cross-legged on the floor in the second-hand bookshops on London's Charing Cross Road. I also remember plaguing several of my schoolteachers with questions that were philosophical in intent. But I had no idea that one could study philosophy at university.

Now, some five years later, I'd discovered that it was an option available in the final year at Cambridge, after completing the exams in medical sciences. I hadn't lost my love for philosophy, and this seemed to be my one and only chance to do something about it.

So I decided, against all (and I do mean *all*) advice, to spend my interim year studying what was then called Moral Sciences—a label that elicited relentless teasing from my fellow medics. I planned to concentrate as far as possible on the philosophy of mind and of science. And despite opposition from an unimaginative Director of Studies, I insisted on being taught by Margaret Masterman—who was neither a Fellow of Newnham nor a University faculty member, and who was far too original and eccentric to be popular with the College authorities.

I found my philosophical studies so exciting that the 'one' year turned into two. Meanwhile, my medical contemporaries and I received our degrees in 1958 from Lord Adrian himself, who was Vice-Chancellor at the time. (We each knelt down with our two hands between his, transfixed—in my case, anyway—by the University's huge golden seal-ring on one of his long, slender fingers.)

During those two years, and alongside some (very different!) supervisions with the logician Casimir Lewy, Masterman taught me weekly at the Cambridge Language

Research Unit, or CLRU. This had been founded in 1954—one year before I arrived in Cambridge (and two years before artificial intelligence was named).

The Unit wasn't an official part of the University but an independent, and distinctly maverick, research group directed by Masterman. Most of its funding came from military agencies in the USA (11.i.a). Its home was a small brick building tucked away on 'the other side' of the river. There were apple trees in the garden, and Buddhist gods carved on the big wooden doors. ("The place is full of gods," Masterman had said to me when I first phoned her to ask for directions. I couldn't imagine what she might mean.)

It was an exciting place, and not just because of the gods. Nor even because several members, seeking to combine science and religion, had founded the Epiphany Philosophers. This was a small community for worship and discussion, who met sometimes in a chapel hidden behind a wall upstairs and sometimes in a fenland mill. It was later widely taken as the inspiration for Iris Murdoch's novel *The Bell*. (Murdoch had studied philosophy in Cambridge in 1947–8.) The Epiphany Philosophers notwithstanding, what was most exciting about CLRU was its intellectual diversity and originality.

Masterman's research group in 1957 included a number of people specializing in the study of language:

* Karen Sparck Jones, now a distinguished researcher in information and language processing (Sparck Jones 1988);
* Richard Richens, a pioneer of machine translation who was by then a senior figure in the Commonwealth Abstracts Bureau (Richens 1958);
* Robin Mackinnon Wood;
* and Frederick Parker-Rhodes, who could read proficiently in twenty-three languages and who (like Masterman) saw metaphorical, not literal, language as primary (Parker-Rhodes 1978).
* Several members of CLRU were then working on automatic Chinese–English translation (Parker-Rhodes 1956; Masterman 1953), helped by Michael Halliday, who became involved with CLRU while Lecturer in Chinese at Cambridge.

Yorick Wilks and Martin Kay, now professors of artificial intelligence and computational linguistics at Sheffield and Stanford universities, joined them very soon after I left.

Another member of the language group at that time was Roger Needham. He was working at the still-new Computer Laboratory at Cambridge, where Maurice Wilkes had built the first relatively easy-to-use computer only a few years before, in 1948–9. Much later, he succeeded Wilkes as its Head, and recently directed Microsoft's UK research laboratory (sadly, he died in 2003). He and his wife, Sparck Jones, immediately aroused my admiration, for building their house with their own four hands. They were living on-site in a caravan surrounded by mud—hence their well-worn wellington boots—while also doing high-level intellectual work.

Among the others I encountered in the Unit was physicist Ted Bastin. He and Parker-Rhodes were developing a highly maverick account of quantum theory, with quanta as self-organizing entities. This is now (so I'm told: I can't make head or tail of quantum physics) a standard alternative view, with several web sites devoted to Parker-Rhodes.

In addition, the exceptionally original cybernetician Pask—today, the object of even more numerous web sites—was literally a back-room boy. He was usually hunched over his DIY computer, which he'd cobbled together out of biscuit tins and string.

Last but not least, philosophers Richard Braithwaite—Masterman's husband—and Dorothy Emmet were frequently around. I shared their interest in the philosophy of religion (a subject I later taught for many years) and, above all, in a scientifically grounded philosophy of mind.

Braithwaite—whom I saw more often—was a leading philosopher of science, and also held the Knightsbridge Chair of Moral Philosophy at Cambridge. Much concerned to integrate science with other areas of life, he'd recently recommended the theory of games as a tool for the moral philosopher (Braithwaite 1955). And he combined a broadly positivistic philosophy of science with Christian beliefs (Braithwaite 1971)—or rather, with a practical commitment to the moral principles illustrated by Christian stories. (Rumour has it that when called upon to recite the Creed at his public baptism service, his full-voiced "I believe . . ." was preceded *sotto voce* by "I will behave in all ways as if . . .".)

Emmet, who'd very recently (1950–3) given the Stanton Lectures in the Philosophy of Religion at Cambridge, held the Chair of Philosophy at the University of Manchester. She knew of the growing excitement about the potential of computing, for the prototype of the world's first stored-program electronic computer had been operational in Manchester since 1948. Indeed, Turing—who wrote some of the first programs for the full version of the Manchester machine—had worked there also. As early as 1949, Emmet's philosophy seminar had discussed 'The Mind and the Computing Machine', with Turing present as one of the discussants (16.ii.a).

That's not to say that she was a devoted Turing fan. True, her department had developed an electrical machine for teaching symbolic logic, already in use for some years. Designed by Wolfe Mays and Dietrich Prinz (who was closely involved in the design of the Manchester computer), it had been exhibited at the annual British philosophy conference (Mays and Prinz 1950; Mays *et al.* 1951). But Mays' device wasn't based on Turing's ideas. Rather, it was inspired by the keyboard-and-rods Logical Piano, originated in 1869 by Stanley Jevons to illustrate the formal principles of validity—see 2.ix.a. (Jevons had been Professor of Logic at Owens College, the forerunner of the University of Manchester.)

Nor did Emmet and her Manchester colleagues agree with Turing that there was no good reason to deny that some conceivable digital computer could *think*. In the departmental seminar he attended, she'd objected that a machine could not be conscious. Michael Polanyi had added that whereas a machine is fully specifiable, a mind is not. And Mays had argued trenchantly that computers are, as John Searle (1980) would later put it, all syntax and no semantics (Manchester Philosophy Seminar 1949).

In the Cambridge apple orchard, however, Turing's influence was strong (see Chapters 4.i and 16.ii). He'd died in 1954, only one year before my arrival in Cambridge. And he'd been close to Braithwaite. Soon after the publication of Turing's seminal paper in *Mind* (A. M. Turing 1950), Braithwaite had chaired a BBC radio debate, in which Turing participated, on the possibility of machine intelligence. (The transcript is in Copeland 1999: 445–76.) Some years before that, they'd been fellow Fellows at

King's College. Indeed, Braithwaite was one of the only two people to have requested an offprint of Turing's 'Computable Numbers' paper written in 1936 (Hodges 1983: 123–4). And, so he told me later, it was he who'd pointed out to Turing its relationship to Gödel's work (letter from R.B.B. to M.A.B., 21 Oct. 1982).

By the time of my becoming a once-to-thrice-weekly visitor to CLRU in 1957, Turing's vision was rarely discussed there in general terms. When it was, the emphasis was more on his technological predictions than on his philosophical views. Believing those predictions to be well grounded, the denizens of CLRU focused rather on the exciting challenges involved in bringing them to fruition.

In other words, the interdisciplinary community amidst the apple trees was making early attempts in the mechanization of thought. In particular, they were trying to identify, and formalize, some of the structural principles informing learning and language.

Pask, for example, was doing pioneering work on adaptive machines, using a wide variety of devices he'd built himself (Pask 1961). Some of his ideas may be viewed as early attempts in AI and A-Life, but he saw them as research in cybernetics (see Chapter 4.v.e). He was largely inspired by Ashby's self-equilibrating Homeostat of the 1940s (Ashby 1948). And he received strong encouragement from McCulloch, one of the founders of the cybernetics movement in the USA—whom I was to meet six years later (Pask 1961: 8).

Four years earlier, in 1953, Pask (with Mackinnon Wood) had constructed Musicolour, an array of lights that adapted to a musician's performance. It had toured various theatres, ending up in a Mecca dance hall. Being a devotee of Mecca dance halls at the time, I much regretted never having encountered it. (I didn't know that it had acquired a reputation for bursting into flames—Mallen 2005: 86.) And in 1958 he started building self-organizing chemical systems that "learned", "evolved", and grew their own "sensors" (sound detectors)—Pask (1961: 105–8; see 4.v.e and 15.vi.d).

But his main interest at that time was in adaptive teaching machines (Pask 1961, ch. 6). Rejecting the easy notion that one size fits all, he was trying to make his machines respond to individual differences between people's thought patterns, or cognitive styles (4.v.e). He'd been designing adaptive teaching machines since 1952, and his SAKI (Self-Adaptive Keyboard Instructor) of 1956, which taught people how to do key punching efficiently, was the first such system to go into commercial production (Pask 1958; 1961: 96 ff.).

Unfortunately, I saw only very little of Pask in his back room at CLRU. A few years after leaving Cambridge, however, I would visit his makeshift office–laboratory in Richmond, where he was exploring yet more ambitious automatic teaching aids (Pask 1975*a*).

Bastin, too, was interested in cybernetics. Much of his spare time went into building a self-equilibrating machine (Bastin 1960). This was inspired by Grey Walter's electromechanical "tortoises" (Grey Walter 1950*a*,*b*), which I'd seen exhibited at the Festival of Britain a few years earlier, in 1951 (4.viii.a). But it also involved ideas about hierarchy, which he was applying to quantum physics as well as to life (Bastin 1969).

The main efforts at CLRU, however, were in the study of language (9.x.a and d). Masterman's group was doing research on what's now called Natural Language

Processing, or NLP (Wilks, forthcoming). They ranged widely over topics later claimed for AI and cognitive science. These included machine translation, the representation of knowledge for information retrieval, and the nature and process of classification. Although their theory of classification was never described in print as computational "learning", it dealt with issues later so described by AI (10.iii.d and 13.iii.f).

Masterman was one of the first people in the world to attempt machine translation, and she made semantics, not syntax, the driving force. She was deeply influenced by certain aspects of Ludwig Wittgenstein's later philosophy of language. Despite her gender—Wittgenstein was notorious for his mysogyny—she'd been one of his favourite students, to whom he'd dictated the lectures later known as *The Blue Book* (Monk 1990: 336). Indeed, she described herself to me on our first meeting as "the only person in England who really understands Wittgenstein". (Modesty wasn't one of her virtues.)

Accordingly, she handled translation by way of a computational thesaurus (Masterman 1957, 1962). More subtle than word-for-word dictionary look-up, her approach enabled word ambiguities to be resolved by inspecting the penumbra of concepts associated with neighbouring words in the text. Or rather, it made this possible in principle. In practice, the method was far from infallible: she delighted in telling people that Virgil's sentence *agricola incurvo terram dimovit ararat* had come out as *ploughman crooked ground plough plough*. This couldn't have happened without the thesaurus, because only *ararat* has a root likely to be listed against *plough* in a dictionary.

The work was practical as well as theoretical, asking how concepts and their semantic interrelations could be implemented in computers—"could be", rather than "were": computing facilities in 1957 were primitive (see 3.v.b). Using CLRU's data, Needham did some classification experiments on the EDSAC-2 in the Computer Laboratory. But this machine (in use until 1964) was far too small to handle a comprehensive thesaurus like Roget's. Moreover, no machine-readable thesaurus existed.

Some genuinely computational, though very primitive, work was done at the CLRU in the 1950s, using a Hollerith punched-card sorter. It wasn't until 1964, five years after I left, that the Unit received its first electronic computer: an ICL 1202, with 200 registers on a drum (Sparck Jones, personal communication).

Because of these practical difficulties, the language team often had to do pseudo-computational tests. That is, they often worked 'mechanically' with paper lists, in the way required for the procedures using punched-card apparatus then being devised at the Unit (Masterman *et al.* 1957/1986: 2). (Perhaps the Buddhist gods were witnessing the first instantiation of Searle's Chinese Room?—16.v.c.)

Masterman was a stimulating, if often infuriating, presence. Her conversation and teaching were peppered with provocative, sometimes deeply insightful, remarks. She encouraged my interest in the philosophy of mind. At her urging, I sent an early essay on 'free will' (i.e. the nature of intentions) to Gilbert Ryle, who published it in *Mind* eighteen months later, in April 1959 (Boden 1959). And her computational thesaurus was highly intriguing: how *could* one get the farmer to plough his ground in English, as well as in Latin?

However, it seemed to me, as an occasional looker-on, to be a technological project, not a psychological one. It clearly rested on intuitions about how people understand language. But I never heard it described as an exercise in the psychology of language—still less, as part of a general project aiming to understand all mental processes in computational terms.

Nor did I have the wit to recognize that possibility for myself—although if I'd interacted more often with Pask, I probably would have done. Masterman's research emphasized (semantic) *structure* rather than *process*, and didn't immediately suggest a way of conceptualizing mental processes as such.

Although I felt that it must somehow be connected with the puzzle of how thought of any kind is possible in a basically material universe, I couldn't see how to generalize it to the mind as a whole. I found her work interesting. But—or so I thought at the time—it wasn't relevant to the issues that most concerned me, and which had fascinated me as a schoolgirl even before becoming a medical student.

These were the nature and evolution of mind, the mind–body problem in general, and free will and psychopathology in particular. I was intrigued by paranoia, multiple personality, automatisms, and hypnosis. And I was especially puzzled by psychosomatic phenomena, such as hysterical paralyses and anaesthesias.

In these cases, there's no bodily damage: under hypnosis, the 'paralysed' arm moves normally, and the 'anaesthetized' skin is sensitive. Still more puzzling, the bodily limits of the clinical syndrome are inconsistent with the gross neuromuscular anatomy, and seem to be determined instead by what the layman-patient *thinks of* as an 'arm' or a 'leg'. For example, the movements that the 'paralysed' patient is unable to make don't correspond to any specifiable set of spinal nerves. They can be described only by using the non-anatomist's concept of an arm, thought of as bounded by the line of a sleeveless shirt. In other words, the mind appears not only to be influencing the body, as in normal voluntary action, but even overcoming it. How could this be?

Machine translation didn't help me to answer such questions. The most promising avenues, I thought, lay elsewhere: in the philosophy of mind and psychology, and in psychiatric medicine.

My intention at that time was to become a psychiatrist. The foray into philosophy was merely temporary. But in May 1959, when I was revising for my Moral Sciences finals and looking forward to going on afterwards to St Thomas's Hospital, I was unexpectedly invited (at Braithwaite's suggestion) to apply for a philosophy lectureship at the University of Birmingham. This was "unexpected" in more senses than one. I'd never considered such a possibility for a moment. Nor was there much time to think about it, for the interviews were to take place only three days later.

Since I wasn't sure that I wanted the job, and didn't think I'd get it anyway, I was totally relaxed on the day. To my amazement, they offered it to me at interview. (It turned out that the little piece in *Mind* had helped.) But I asked for forty-eight hours to think it over: *medicine or philosophy?* was a difficult decision. Masterman's very strong support (she showed me her written reference, when she found that I was dithering) was one of many factors that influenced me to accept the offer.

(I also sought advice from my former pathology supervisor, today a distinguished Emeritus Professor of Pathology. He observed that having a wife with a medical degree, like his—whom I could see hanging out the washing in the garden with pegs between her teeth, while he smoked his pipe in his armchair—would always help a family to get a second mortgage if needed. This remark, in that all-too-familiar domestic context, was less persuasive than he'd intended.)

It was a strong department (Peter Geach was one of the luminaries), and I was very happy there. However, I soon got bored. For Birmingham's Chinese walls between disciplines impeded my interests in the philosophy of psychology and biology. I considered returning to medicine (St Thomas's said "Yes, come!"), but having forfeited my state studentship to earn my own living I could no longer afford to do so.

Instead, I followed the suggestion of my old Cambridge friend Charlie Gross (who a few years later would discover the 'monkey's hand' neurones in the monkey's brain: 14.iv.b). He said, "There's this man Bruner at Harvard, who's been doing some work I think you'd find interesting—and there are scholarships you can apply for to go to the States." So I applied for a Harkness Fellowship, which enabled me, in the autumn of 1962, to cross the pond to study cognitive and social psychology with Bruner. (When I first met him, he was chatting with George Miller. "Here's our double-first from Oxford," he said to him. "Cambridge!" I protested—and "Welcome to Yale!" came quick as a flash from Miller.)

By the time I left for the USA, I'd already decided to go to the just-initiated University of Sussex when I got back to England. This was because, most unusually, it was conceived from the start as an interdisciplinary institution. I was already committed to interdisciplinarity, of course. But, sailing happily through the storms on the magnificent *Queen Mary* (the roughest voyage for twelve years), I never imagined that my colleagues and I would eventually found Sussex's Cognitive Studies Programme (later the School and now the Centre for Cognitive Science), which in 1973 world-pioneered degrees integrating AI, philosophy, psychology, and linguistics. The idea couldn't even have occurred to me.

Barely a week after docking at Manhattan, however, it might have done. The conceptual leap from computation to psychology, and to the mind–body problem, happened (for me) a mere two days after arriving in the other Cambridge.

The occasion was my first sight of the remarkable book *Plans and the Structure of Behavior*, by Miller, Eugene Galanter, and Karl Pribram (1960). I picked it up while browsing in a second-hand bookshop on Massachusetts Avenue. Why I did so, I'll never know. It was a hideous object: bound in a roughly textured cloth, a dull rust in colour (my least-favourite hue), horribly coffee-stained, and defaced by heavy underlining on almost every page. But it changed my life.

Nor was I the only one, for it was highly influential (see Chapter 6.iv.c). I soon discovered that it was on Bruner's reading lists at Harvard's new Center for Cognitive Studies, founded only a few months before—and not just because Miller was the co-founder! It was recommended also for Phil Stone's seminar on 'Computer Simulation', for which I was to do my first programming. (We wrote our programs in Victor Yngve's early list processing language COMIT, using punched cards for MIT's pre-release prototype of the IBM-360—not officially announced until 1964.)

But all that was still to come. Already primed by Masterman and Pask, my thinking was instantly triggered by this coffee-stained volume. Leafing through it in the bookshop, it seemed to offer a way to tackle just those questions which had bothered me as a schoolgirl.

It was an intoxicating attempt to apply specific computational ideas—hierarchies of Test–Operate–Test–Exit procedures (TOTE-units)—to the whole of psychology. Unlike Masterman, it focused on process as well as structure. And it ranged from animal learning and instinct, through memory and language, to personality, psychopathology, and hypnosis. Self-confessedly vague and simplistic, and often careless to boot, it was nevertheless a work of vision.

Its computational ideas soon informed my own work. In 1963 I wrote a paper applying them to William McDougall's rich theory of the purposive structures underlying normal and abnormal personality (Boden 1965). And a few years later, I addressed one of my long-standing puzzles by outlining how a robot could have a paralysis conforming not to its actual wires-and-levers anatomy, but to its programmed 'concept' of what an arm is (Boden 1970). Its behaviour, I argued, would therefore be describable in intentional terms. That is, what it was 'doing', and how it might be 'cured', could be stated only by reference to the descriptions and instructions in its program.

In the interim, I'd returned to England (and moved to Sussex in 1965), and was writing my first book: *Purposive Explanation in Psychology* (1972). Begun as my Ph.D. thesis (the first purely theoretical thesis that the Harvard department had ever allowed), this took me eight years to finish. The delay was explained only partly by the amount of intellectual work involved: the publisher's airmailed advance copy reached me in hospital on the day after the birth of my second baby. (Both were deep purple on arrival.)

In that book, I developed a fundamentally physicalist but non-reductionist account of purpose, and other intentional concepts. That is, I offered an essentially functionalist philosophy of mind—though using my own terminology, not Hilary Putnam's (I came across his work later). I compared my account of mind, and of the mind–body relation, with a wide range of theories in psychology and philosophy. And I focused most closely on McDougall—not as an unquestioned guru, but as an intellectual sparring partner.

What had drawn me to McDougall was his deep insight into the complex structure of the human mind, and his many explicit arguments against psychological reductionism (2.x.b. and 5.ii.a). Most of these, but not all, I thought to be valid.

It turned out later that there were several personal links, as well. On researching McDougall's life history (1871–1938), I was intrigued to discover that, after completing his degree in medical sciences at Cambridge, he'd taken up the same clinical scholarship at St Thomas's Hospital which had been offered to me in 1959, and again in 1962. He, like me, had moved from medicine to psychology, with philosophy of mind constantly in the background. And he, too, had gone from one Cambridge to the other: he was a professor at Harvard for several years.

There was even a link that made me one of his intellectual grandchildren, by means of a sort of apostolic succession. For when I said to Bruner, in the spring of 1964, that instead of doing experiments on information density in disjunctive concepts (a mind-numbing topic which he, intending to be helpful, had suggested to me) I wanted to study McDougall's theory of purpose, he told me—after a gasp of amazement—that McDougall had been his teacher at Duke University.

Whereas I'd been recommended to go to Harvard Graduate School by Charlie Gross, Bruner—so he told me—had been specifically warned against it by McDougall. McDougall's broadly ranging psychology had been highly influential until it was suddenly eclipsed by behaviourism. He remained bitter about this for the rest of his life, and left the newly behaviourist Harvard in disgust to set up his own outfit at Duke.

When the young Bruner announced his intention of travelling north to Massachusetts, McDougall had gruffly warned him about the intellectual corruption (he never minced his words) that awaited him there.

By the time I arrived at Harvard, a quarter-century later, the behaviourist "corruption" was less strong. Or anyway, that was true in Bruner and Miller's Center for Cognitive Studies, if not in the Psychology Department as such—whose denizens included Burrhus Skinner and Richard Herrnstein. Even so, McDougall's name had vanished from the curriculum. (Hence Bruner's gasp of amazement.) It survived only as one of many items on a three-page mimeographed list of long-dead worthies, circulated as potential essay topics for Gordon Allport's seminar on the history of social psychology.

What alerted me to him initially was the title of his book *Body and Mind* (1911), which Allport had included on the list alongside the author's name. On consulting his work (long-unborrowed from the Harvard library), I found that McDougall's ideas had a welcome subtlety and depth, and a refreshing concern for real life, whether psychiatric syndromes or everyday pursuits. His psychology dealt not only with cognition, but with motivation and emotion too—and, significantly, with how these three types of mental function are closely integrated in individual personalities.

Besides those strong points, his writings abounded with philosophical as well as empirical questions. These attracted me, although they were anathema to most (Anglo-American) textbook writers of the 1950s and 1960s, who believed—wrongly—that psychology had finally 'escaped' from philosophy. In truth, they'd simply accepted the current philosophical fashion (operationalism, or logical positivism), without stopping to question it carefully.

That's not to say that I agreed with all his philosophical arguments, or that I accepted his robust defence of "animism". Far from it. McDougall had been combatively anti-mechanistic, even claiming that purposive behaviour requires a special form of energy (*horme*), intrinsically directed to instinctive goals. That, for me, was a step too far: purposive explanation is one thing, purposive energy quite another. But it was important to understand why, when considering intentional phenomena, he'd felt it necessary to say this.

As part of my critique (Boden 1972), I suggested—what probably made him turn in his grave—that his many insights about personality and psychopathology could be simulated in computational terms. If this was indeed possible, those theoretical insights could be saved, and even clarified, without positing any mysterious energy. And if it could be done for McDougall's avowedly anti-mechanistic psychology, it could in principle be done for any other. (Sigmund Freud's purposive theory would have been less suitable as an exemplar, for he believed that psychology does have a mechanistic base.)

In sketching specific ways of doing this, I had to extrapolate from the computer models that already existed. In 1963, when the book was begun, there were only a handful of candidates.

By the time it was finished, early in 1971, there were many more. These ranged from work in computer problem solving, through programs for vision and language, to models of analogy, learning, and various aspects of personality. For instance, preliminary reports on Terry Winograd's research, whose official publication in 1972 suddenly raised the visibility of computer modelling in the wider intellectual community (see 9.xi.b and 10.iv.a), had been made available informally in 1970.

These new examples, whether successful in their aims or not, were clearly relevant to my book's central claim: that purposive behaviour is intelligible in computational terms, and could in principle be simulated in computers. But although I was able to refer briefly to a few of them, they were in general too numerous—and many were too late—to be added to my already lengthy manuscript.

So I decided that, as soon as my first book was finished, I would write an extended footnote to it. This would detail what could—and, just as importantly, what could not—be done by computer modelling in the early 1970s, and what would be needed for the many remaining obstacles to be overcome.

The resulting footnote, *Artificial Intelligence and Natural Man* (Boden 1977), ran to 537 pages. (By this time, the term 'artificial intelligence' had largely replaced 'computer simulation'.) It devoted a chapter each to the philosophical, psychological, and social implications of AI. Indeed, in one sense they were what the book was really about. Most chapters, however, described the AI as such.

For the sake of readers knowing nothing about AI and highly suspicious of computers to boot, it contained not one line of code. It was also highly critical, in the sense that it identified countless mismatches between existing AI programs and real minds. Nevertheless, it was assigned—alongside Patrick Winston's very different *Artificial Intelligence* (1977)—as a compulsory text for AI courses at MIT and Yale. I was told it was the first time they'd assigned two books, rather than just one. It was also used as the basis of various psychology courses in the UK and USA, including the Open University's first Cognitive Psychology course. (Later, in 1993, I was delighted to be elected an early Fellow of the American Association for Artificial Intelligence, in part for having written it.)

That book *was* fully comprehensive. Had Squoggins been working at the time, he would very likely have been included. For it mentioned virtually every AI program of any interest, including many available only as privately circulated reports or working papers. And it gave closely detailed explanations and critiques of many of them. It ranged over diverse aspects of mind: from language and vision to neurosis and creativity—on which last I promised myself a whole volume, later (Boden 1990*a*, 2004). And it identified theoretical challenges many of which *still* remain to be met. (So the second edition, in 1987, was unchanged except for an added 'update' chapter.) In short, it provided a near-exhaustive description of the state of the art of AI at the time.

Such a project is no longer possible: even 1,080 A4-sized pages aren't enough for a fully comprehensive account (Russell and Norvig 2003). If it's not possible for AI, still less is it feasible for cognitive science as a whole. Too much water has flowed under the bridge. There are too many unsung Squogginses out there, and too many branching implications that could be explored. That's even more true if one takes a *historical* approach, for then the potential subjects multiply yet more relentlessly.

I hope these far-from-comprehensive pages will tell an illuminating story, nonetheless. As Naudé realized (fuelling his attack on historians in general), the facts one chooses to relate, and how one decides to link them, will depend on one's background and perspective. In this book, I've aimed to show how cognitive science has developed so as to help solve problems about mind, brain, and personality that have intrigued me ever since I was a girl.

M.A.B.

Brighton
January 2006

1

SETTING THE SCENE

Once upon a time there was a teddy bear called Twink—and with those few words, the scene is set. We know what we're talking about. Twink's story can begin . . .

This story can't begin so quickly, however. For we *don't* yet know what we're talking about. Some readers may know very little about cognitive science at this stage. Even more to the point, those who are already familiar with it think of it in varying ways. That was true right from the start, and it's even more true now. (So it's no accident that the summary chapter of a recent book is subtitled: 'It's Cognitive Science—But Not As We Know It'—M. W. Wheeler 2005: 283.)

One of the founders of the field, when asked to define it, confessed that "Trying to speak for cognitive science, as if cognitive scientists had but one mind and one voice, is a bum's game" (G. A. Miller 1978: 6). And twenty years afterwards, two long-time leaders edited a book called *What Is Cognitive Science?* (Lepore and Pylyshyn 1999). You'd think they'd know by now! But no: even in the textbooks, never mind coffee conversations and idle chat, definitions differ.

I shan't list them: the boredom barometer would shoot through the roof. However, the differences do make a difference. This will become clearer throughout the following pages, as we see how theory and practice have changed over the years (in some cases, coming full circle). Meanwhile, before starting the story, some scene setting may be helpful.

One way of saying what we're talking about is to give some examples of the wide-ranging questions studied by cognitive science. I'll do that in Section i. And I'll do it in everyday language: the technicalities can wait until later.

Another is to give a definition of the field, even if this can't be presented as *the* universally agreed definition. I'll do that in Section ii. This, I hope, will help to show why I've decided to tell the story in the way I do.

Finally, in Section iii, I'll identify a number of traps that lie in wait for anyone discussing the field's intellectual history.

1.i. Mind and its Place in Nature

A host of intriguing questions about mind and its place in nature occur to most thinking people. (The FAQs of the mind, Web-users might say.) As explained in the Preface, some have puzzled me for almost as long as I can remember—and I usually found that my friends were puzzled by them too. They centred on the nature of mind and the mind–body problem; the evolution of mind; freedom and purpose; and how various psychopathologies are possible.

Most of the topics studied in cognitive science fall under one of these broad categories. And those which don't, such as the nature of *computation*, are closely related to them.

a. Questions, questions . . .

We're intrigued by consciousness, for example. We know there are close correlations between brain events and conscious states—but why is that so? The answer seems to be that our brains generate our consciousness. But how do they do this, in practice? Even more puzzling, how *can* they do this, in principle?

Or maybe we only *think* we know this? Some people argue that it *doesn't even make sense* to suggest that there are correlations between conscious states and brain states. How could anyone with any common sense be led to make such a deeply counter-intuitive claim? Perhaps "common sense" itself is radically misguided here (and was radically different in other historical periods)?

What about dogs and horses: are they conscious? And snails, flies, newts . . . ? For that matter, what about newborn babies: are they conscious in *anything like* the sense in which adult humans are? What of machines? Could a machine be conscious—and if not, why not?

People often wonder whether a creature has to have a brain, or something very like one, to be intelligent. If so, why? Is a brain (as well as eyes) needed to *see*, for example? What do the visual brain cells do that the retinal cells don't? What about intelligent *action*? How, for instance, does the brain convert an Olympic diver's intention to dive into the finely modulated bodily movements that ensue? If we knew this, could we drop talk of intentions and refer only to brains instead?

Consider chimps, or cats: what can *their* brains do, and what can't they do? And what can they do without the mammalian (and avian) glory, the cerebral hemispheres? Given that *Homo sapiens* evolved from lower animals, what does this tell us about our mental powers? Can anything interesting be learnt about the human mind by studying distantly related species such as frogs, or insects?

As for machines, just how—if at all—must an artifice resemble a real brain if it's even to *seem* to support a mind? And *even if* studying insects can teach us something about ourselves, what about studying inanimate tin cans—like a Mars robot, or an automatic controller in a chemical factory? How could these things (*sic*) possibly be relevant?

What mental powers does a human brain provide, and how does it manage to do so? How is free will possible? And creativity? Are creative ideas unpredictable, and if so why? What are emotions—and do they conflict with rationality, or support it?

Are our abilities inborn, or determined by experience? And how does the brain get its detailed anatomical structure: from genetics or from the environment—or perhaps even from spontaneous self-organization? (Is that last suggestion mere hand-waving, more magic than science?)

Do we all share psychological properties that mould every human culture? Perhaps the same underlying sense of beauty: maybe in symmetry, or expanses of water? Or the same tendency towards religious belief? If so, is that because we've evolved that way? Or are evolutionary explanations of human psychology mere Just So stories, no more plausible than the delightful tale about The Cat Who Walked By Himself (Kipling 1902)? Superficially, at least, cultures are hugely diverse . . . but can they harbour *just any* conceivable idea?

In mental illnesses of various kinds, what's gone wrong: something in the brain, or something in the mind? What's the difference?

Sometimes, people say that only living things can have a mind. Is that true? If so, why? What is life, anyway? And how did it arise in the first place? Could a living thing be created by us?

Last, but by no means least, coffee-table chat abounds with puzzles about language. For instance, people wonder what *counts* as a language: why not birdsong? Can any non-human animals learn a language? If not, is that merely because we're better at learning, or because language is a human instinct? And what, exactly, does that mean? Is language needed for thought, or can some dumb animals think?

Can two different languages ever express exactly the same thought? Or is perfect translation impossible? Could a machine converse with us in English, or French—and would it understand us, even if it did? Imagine a machine that appeared to be solving problems and using language just like us: would that prove that it was truly intelligent?

None of these questions is new. (That's largely why listing them is a scene-setting equivalent of saying "Once upon a time, there was a teddy bear . . .".)

Some date back to Aristotle. Many, including those about language-using machines, were discussed in the 1630s by René Descartes. Others were considered by Immanuel Kant, Johann von Goethe, or Wilhelm von Humboldt in the late eighteenth century. The rest surfaced in the nineteenth, or very early twentieth, century (see Chapter 2).

Originally, then, most were discussed by philosophers. Some still are (the difference between *mind* and *brain*, for example). But even those need to be considered in light of the scientific data available.

Most of our Twink-questions were later developed—and some answered—by traditional scientific research in psychology, anthropology, neurophysiology, or biology. Since the 1940s, however, *every one* has been further sharpened by work in cognitive science.

b. How to find some answers

Cognitive science tries to answer these questions in two closely related ways. Both of them draw on machines. But the machines in question are very unlike what used to be thought of as a machine.

Forget steam-engines and telephones: these new machines can be hugely more complex even than an E-type Jag, or a jet plane. Indeed, the capacities of modern jets, from the much-lamented Concorde to stealth bombers, are largely due to their having these new machines inside them. It follows that to think of minds *as* machines, as cognitive scientists in general do, isn't so limiting—nor so absurd—as it may seem to someone who has only pre-1950 machines in mind.

Specifically, cognitive science uses abstract (logical/mathematical) concepts drawn from artificial intelligence (AI) and control theory, alias cybernetics (see Section ii.a, below).

* AI tries to make computers do the sorts of things that minds can do. These things range from interpreting language or camera input, through making medical diagnoses and constructing imaginary (virtual) worlds, to controlling the movements of a robot.
* Control theory studies the functioning of self-regulating systems. These systems include both automated chemical factories and living cells and organisms.

These concepts (of computation and control) sharpen psychological questions because they can express ideas about mental processes more clearly than verbal concepts can. Moreover, when implemented in computer models they can test the coherence and implications of those ideas more rigorously. Often, they show that a previously favoured theory has unsuspected gaps in it. Sometimes, they suggest how those gaps might be filled. They can show that a theory *might be*, *could be*, true—although to know whether it *is* true, we need psychological and/or neuroscientific evidence as well. Some important questions have been answered in this way which couldn't have been answered otherwise.

Consider language and machines, for instance. It's now clear that computers can (seem to) use natural language, up to a point. What's not yet clear is just where, in practice or principle, that point lies. How good can we expect future computer prose, or computer conversation, to be? And what problems will have to be overcome to get there? For that matter, what are the problems which have already been overcome, to get to where we are now? And are these problems linguistic, psychological, or philosophical—or perhaps a mixture of all three?

Thirty years ago, a medical friend told me he'd spent the afternoon visiting an immigrant family from India, whose 8-year-old son had been translating across three languages for his elders. "You'd never get a computer to do that!" he said.—Maybe, maybe not. But if not, why not? And if so, how?

Only five years after this pessimistic comment, the European Union's translation system achieved 78 per cent intelligibility for its 'raw' text, and 98 per cent for the tidied-up version. Unlike the little boy, this program could handle only two languages at a time. Ten years later, however, another one could switch between forty-two different language pairs. But the boy—by then, in his early twenties—still had the edge. He could translate remarks about anything, within reason, whereas these programs could deal only with relatively specialized topics.

What my friend didn't seem to realize was that if AI research could enable a computer to use even *one* language properly, translating it into another would be easy by comparison. Or rather, translating it helpfully, usefully, acceptably . . . would be relatively easy. Translating it perfectly is another matter. But then, it's not clear that a human being, whether 8 years old or 80, could produce a *perfect* translation of anything interesting. Even *Please give me six cans of baked beans* will cause problems, if one of the languages codes the participants' social status by the particular word chosen for *Please*.

Nor did he stop to ask how the 8-year-old did it—still less, how his own children had learnt their mother tongue. He simply took it for granted that language learning happens. But how? After all, vocabulary isn't the only problem: there's grammar, too.

Different languages have different grammars. Or at least, they appear to. (The order of adjective and noun varies, for instance: think of *the red house* and *la maison rouge*.) But perhaps all languages share some underlying 'universal' grammar? If so, what is it? And how is it related to the syntactic rules that bedevil us when we encounter a new tongue? How did the family's young translator manage to cope with three distinct grammars, from different language groups? As for the rules of one's mother tongue, how are these learnt, and how are they represented in the mind/brain?

All these questions, and many more, have had to be faced by cognitive scientists working in psycholinguistics and/or natural language processing (NLP). And a great deal has been learnt in the process, even if many mysteries—and some bitter controversies—remain (see Chapters 9 and 12.vi.e and x.d–e).

"Not too fast!" you may say. "Computers can handle language to some extent, and even translate it usefully too. But do they *understand* the language they use?" (Don't let's stop, here, to ask what it is for *human beings* to understand the language they use—but see Chapters 7.ii.d, 12.x.g, and 16.)

You may even mention the Chinese Room, an intriguing idea that's hit the mass media worldwide (16.v.c). This example is intended to show that the answer to your question is "No". A monoglot English speaker could spend weeks following formal rules for shuffling slips of paper bearing *squiggles* and *squoggles*, without ever realizing that they are Chinese characters which, for readers of Chinese, can be used to deliver true answers to meaningful questions. The moral is supposed to be that AI programs are intrinsically meaningless, and that for understanding you need a brain.—And, it's often added, you need a brain for consciousness too: a robot, no matter how human-like, would be a non-conscious zombie.

An equally well-known argument claims that even if the language produced by a computer program, or a robot, were indistinguishable from that produced by a human being, that wouldn't prove that the thing was really intelligent. "Passing the Turing Test", as this is called, wouldn't guarantee intelligence, understanding, or consciousness. The Test-passer might simply be a zombie.

Both these arguments have been hotly debated within cognitive science—although each is much less important *for the practice of AI* than most people imagine (see 16.ii.c). There's still no unanimity on whether they're well founded. Indeed, some cognitive scientists hold that *there can be no such thing* as a zombie—not because the technology is too difficult (although in fact it may be), but because the very notion is incoherent. On this view, science-fiction novels and Hollywood scenarios about zombies are, literally, non-sense (14.xi and 16.iii–v).

Whether the technology really is too difficult is disputed also. The vast majority of cognitive scientists would say that it is, at least for the foreseeable future. But one leading research team, initially with a prominent philosopher on board, is betting that it isn't. They're hoping to build a (literally) conscious robot, with a mind like that of a young child (see Chapter 15.vii.a).

(*An aside*: That last sentence was true when I wrote it, in the mid-1990s. Now, in 2005, the project has ground to a halt. The roboticist team leader always had other fish to fry in his research time, and is now buckling under a heavy administrative load as well; as for the research students who were working on it, in snatched moments of their spare time, they've left to take up jobs elsewhere. However, the leader still believes that the project is feasible, and he might even revive it some day. Given that fact, the following paragraphs can stand, as though the plan were still being actively pursued.)

Even ignoring the issue of consciousness, they face hugely challenging problems. To build a robot that even *seems* to have the intelligence of a 5-year-old, they must provide all the relevant perceptual discriminations, motor skills, learning power, problem-solving ability, and language mastery.

For each of these, they must depend on work done by other cognitive scientists. For example, they need a computer vision system modelling the child's visual powers: so they need to know *what these are* and *how they work* (see Chapters 7.v, 14.iv and vi.b–d). They need a powerful theory of perceptuo-motor control, for generating appropriate movements of the eyes, head, and fingers (14.vii and x). They should also enable the robot to switch smoothly between stable gaits, such as crawling, standing, walking, and running (14.v and ix.b). (In fact, they've avoided those problems by giving their robot a pedestal in place of legs.) The system's capacity for learning must be built on the work that's been done in this area (see Chapters 10.iii.d, 12, 13.iii.d, and 14). As for enabling the robot to develop language, they must rely on research into some of the psycholinguistic questions outlined above (Chapters 7.ii and vi, 9, and 12.vi.e).

Strictly, they should also simulate the temper tantrums of 'the terrible twos', a stage of infancy that all parents will remember with a shudder. And, if only to preserve their own sanity, they should enable the robot to develop the greater self control—which is to say, the greater freedom (7.i.g)—of the 5-year-old. But the control of temper tantrums is even more difficult to model than stable walking or running is.

Indeed, you may think that the appropriate word here isn't "difficult", but "impossible". Certainly, many people believe that a computational psychology can't have anything to say about emotions, still less freedom.

Well, it can, and it does (see Chapter 7.i). This challenge was mounted over forty years ago, and was soon taken up by Herbert Simon, one of the high priests of computational psychology. At that time, too, a computer simulation of neurosis was developed in which different levels of 'anxiety' selected different defence mechanisms to repress the 'troubling' thought. In 1983 the authors of the Gifford Lectures on Natural Religion gave a computational analysis of personality and freedom (and religious belief) in which emotion figured prominently. Several philosophers have analysed human freedom in terms of a certain type of computational (cognitive and emotional) complexity. And a very recent program models the emotion-guided activity of a nursemaid caring for a dozen babies, each of whom has to be fed, watered, changed, cuddled, entertained, and prevented from falling into the river or crawling towards a busy road.

A nursemaid is free to choose what to do at every moment. But her choices are far from random. To the contrary, they're constrained by the goals she wants to achieve (which may conflict: she only has two hands); by the priorities she holds (feeding is necessary, lullabies aren't); by her deliberations about consequences (no-cuddles will produce an unhappy baby); by her judgements of urgency (even the hungriest baby can be temporarily ignored, if another is nearing the road); and by her emotional reactions (sometimes, she must rescue the baby *immediately*, without stopping to think). On some occasions, she 'has no choice': the danger *must* be averted, and it must be done *now*; and the baby *must* be fed, soon. But the sense in which she (sometimes) has no choice is fundamentally different from the sense in which a non-human animal, such as a cricket for example, (always) has no choice about what to do next (see Chapter 15.vii). She's free, it isn't. Moreover, her freedom doesn't depend on randomness, or on mysterious spiritual influences: to the contrary, it's an aspect of *how her mind works*.

The nursemaid research group has even analysed the computational structure of grief. The emotion of grief is more than mere feeling. It involves irrational behaviour driven by obsessional thoughts, continual distraction, depression, anger, and guilt—all of which gradually pass, over many months, as mourning does its work. (Just what "work" is that? These cognitive scientists suggest an answer: Chapter 7.i.f.)

Grief is possible only for humans, although dogs sometimes seem to *sorrow*. A cricket simply cannot grieve. It lacks the necessary mental architecture: the concepts, knowledge, motives, values, and social commitments required to generate—or to overcome—the deeply disturbing emotion of grief. And unlike a human baby, who can't grieve either (even though it can 'miss' an absent carer), it has no way of developing them. Its mind, if one wants to use that term at all here, is very simple. It can't even learn to recognize objects or patterns as one of *a general class*—such as another cricket.

To be sure, crickets manage. They've survived. They even do some apparently clever things, such as locating a potential mate at a distance. However, they do this unthinkingly. They rely on a hardwired biological trick, an anatomical detail evolved for this function alone. Similarly, a frog locates its food by relying on perceptuo-motor reflexes linking cells in its retina and brain to muscles that make it jump to just the right spot (Chapter 14.iv and vii).

People, too, sometimes use such biological tricks—for instance, in locating the source of a sound (14.viii.c). But their perception and learning involves much, much, more. Even without language, mammals (and birds) can do what crickets cannot: they can learn to recognize new stimulus patterns, and can generalize those patterns over different class members (see Chapters 12 and 14).

Moreover, some of the detailed brain structures that enable mammals to *see*, or to *hear*, arise by spontaneous self-organization in the womb. (So the fact that a newborn baby, or kitten, already has a certain perceptual ability *doesn't* prove that it was specifically coded in the genes.) This may seem surprising, even magical. But computer modelling has shown how such anatomical self-organization is possible (Chapter 14.vi.b and ix.c).

You may be sceptical. You may feel, for instance, that this general approach is merely an example of what Donna Haraway (1944–) calls "cyborg science", more a mark of the times than of the truth (Haraway: 1986/1991).

As she puts it, the many sciences currently informed by the concepts of information and computation involve a "reinvention of nature". They express a pervasive world-view, or "lived social reality", in which human minds and human beings "are constructed as [jointly] natural–technical objects". In her opinion, this view couldn't have arisen without post-Second World War military technology and its aggressive political background.

That last charge is true (see Chapters 4.vi.a, 11.i, and 12.vii.b). To a large extent, the "reinvention" charge is true also. Whether it follows that cognitive science is, as Haraway claims, deeply suspect and epistemologically compromised is quite another matter (Section iii.b–d, below).

Even if you're not an admirer of Haraway's writings, which include many provocative claims about the late twentieth-century Zeitgeist, you may nevertheless be sceptical about cognitive science. You may simply suspect that cognitive scientists have been seduced by the technology. Perhaps they're like the proverbial 'hacker' (11.ii.e), or those people from all walks of life who sit hunched over their bedroom computer for hours on end? Computers, after all, are only too capable of enticing people to waste their time. (Although, sadly, "waste" isn't always the right word: taking control over computers, which are usually much more predictable than other human beings, provides some devotees with their main source of ego strength and contentment—Shotton 1989: chs. 8 and 10.)

However, this type of research, vulgarly trendy though it may appear, is driven by a philosophical view of understanding and explanation that has deep and ancient roots. That is, it's an expression of the "maker's knowledge" *(verum factum)* tradition. This holds that in order to understand something properly one has to be able to make it. In other words, observation and abstract argumentation aren't enough.

One leading proponent of *verum factum* was Giambattista Vico (1668–1744), who famously argued that only the humanities can provide us with genuine knowledge, because they study the creations (not of God but) of human beings (Perez-Ramos 1988: 189–96; Miner 1998). Specifically, history—for Vico, the key to the understanding of human minds and cultures—involves an active re-creation of the thoughts of the people being studied. The eighteenth- and nineteenth-century Romantic philosophers used essentially similar arguments to prioritize art over science (see 2.vi.c and 9.iv). Others were more inclusive, applying *verum factum* to the natural sciences too. So for many of the early modern scientists, a scientific experiment was seen as a *construction*, and theory-based technology was an intellectual justification of 'pure' science.

In short: if you can build it, you can understand it. Cognitive scientists would agree—and their constructions include not only theories but computer models too.

c. Never mind minds?

There's an even more difficult question, one which threatens to undermine the rationale of cognitive science as a whole. Namely: Maybe we'd be better off if we avoided talk of 'mind' altogether?

One way of doing that would be to avoid psychological language entirely (cf. Chapter 5.i.a). But at what cost? Gossip would be impossible—an advance in morality, perhaps, but not in the gaiety of nations. And scientific studies of the topics that fascinate gossips would be impossible too. We could describe the bodily movements, but couldn't say what *action* was being performed, or what *purpose* was being followed. Similarly, we could say how the brain cells are responding, but not what they're *doing*.

Less radically, one could retain psychological language but gloss it in purely behavioural terms, or perhaps in the abstract, functionalist, terms of information processing. Then, scientific studies (of these types) would be justified. The second of these is the position taken by the vast majority of cognitive scientists.

Or—an option that's recently grown increasingly popular *within* cognitive science, as we'll see—one could say that 'mind' was *invented* by Descartes, not just described by him (Rorty 1979: 17–69). On that view, this Cartesian fiction (*sic*) separates the individual both from their own body and from other human beings—and the physical environment, too (see 2.iii.a–b). The implication is that cognitive science should stress embodiment rather than intellectualist reasoning, and social engagement rather than individualistic action and thought.

A much more radical approach would be to argue that both 'mind' *and* 'body' are concepts constructed on some deeper philosophical base, and are highly misleading when taken—by scientists, for example—as fundamental realities (see 14.xi and 16.vi–viii). That would eliminate many puzzles, but only by dismissing hope of *any* scientific explanation of psychology (and any naturalistic account of meaning). Cognitive science, on this view, wouldn't just be difficult: it would be non-sense.

You don't have to be a rocket scientist to guess that I don't share that last view. Perhaps you don't share it, either. But I'm not going to counter it yet. Indeed, we shan't consider it at length until Chapter 16.vi–viii (although it will push its nose above the surface in Section iii.b below, and also in Chapter 14.xi).

Even there, I shan't be able to give a knockdown argument against it: it's perhaps the deepest division in philosophy. To make things worse, it's often closely allied with a form of relativism that would undermine *all* scientific knowledge (see Section iii.b, below). However, many people—including many scientists—aren't even aware of this division, and don't take it seriously if they are. Throughout most of these pages, then, I'll continue to speak of 'mind' and 'mental' phenomena as though such talk were relatively unproblematic. That is, I'll assume that minds and mental phenomena do exist, even while admitting that there are many disagreements about just how they should be described.

Similarly, I'll assume (until Chapter 16) that *some* scientific psychology is in principle possible. Even if it isn't, the first fourteen chapters won't be irrelevant. For a science-denier should offer an alternative interpretation of the facts discovered by science—for which task, they need to know something about what these facts are (see 16.viii.a).

All the examples I've mentioned in this section fall under the centuries-old questions about the mind that were listed at the outset. As remarked there, most of us have mused on these at some time or other. For cognitive scientists, they're a prime concern. And as we'll now see, they ask them in a particular way, which was inconceivable before the late 1930s.

1.ii. The Scope of Cognitive Science

Cognitive science is a catholic field, in three ways:

* First, it covers all aspects of mind and behaviour. (That was illustrated by the wide range of questions listed above.)
* Second, it draws on many different disciplines in studying them.
* And third, it relies on more than one kind of theory. Broadly speaking, it's the study of *mind as machine*—a definition that covers various types of explanation, as we'll see.

a. Of labels and cans

In a neat and tidy world, where every label fitted what's inside the can, cognitive science would be the science of cognition (knowledge). Indeed, it's often defined that way. However, things aren't so simple.

In fact, cognitive science deals with all mental processes. Cognition (language, memory, perception, problem solving . . .) is included of course. But so are motivation, emotion, and social interaction—and the control of motor action, which is largely what cognition has evolved *for*.

You may feel that these types of psychological process aren't clearly distinguishable. If so, you're in good company. The 'holistic' belief that they're intimately intertwined is both very old-fashioned and very new. Its heyday was 200 years ago (see 2.vi). It never died out entirely: the cybernetic psychoanalyst Lawrence Kubie, for instance, said that "the various areas of psychic life are so interdependent that no one of them can be [experimentally] assayed alone and apart from the others" (1953: 48). However, it did go out of favour with the scientific community, resurfacing only very recently (Chapters 14.x.c, 15.vii.c, and 16.vii.c). Even now, it's an unorthodox view. For the moment, then, let's go along with the common assumption that cognition, motivation, emotion, social interaction, and bodily action can be considered separately.

Given that cognitive science isn't focused only on cognition, the label is highly misleading. Why, then, were these words chosen in the first place?

Today, they don't trip off Everyman's tongue. At the time, however, they were less arcane than one might think. Both had recently been popularized by social psychologists discussing "cognitive dissonance" (Festinger 1957). That terminology had even entered the media. Many journalists had summarized their explanations of the power of advertising, and of high pricing, on consumer behaviour. And the newspapers had had a field day in rehashing the social psychologists' reports about a recent cult in the mid-West of the USA (Festinger *et al.* 1956). These people had expected to be rescued from The End Of The World on a certain day by aliens in spaceships—only to see no EOTW, no spaceships, and *no* diminution of their faith in the cult leader (see 7.i.c).

In any event, professional psychologists were perfectly familiar with "cognition" as a technical term. But it had originally been coined, some two centuries earlier, specifically to *exclude* motivation and emotion. So, again, why choose it?

One of the two men mainly responsible—George Miller and Jerome Bruner—has explained it like this:

> In reaching back for the word "cognition", I don't think anyone was intentionally excluding "volition" or "conation" [aka motivation] or "emotion" (Hilgard 1980). I think they were just reaching back for common sense. In using the word "cognition" we were setting ourselves off from behaviorism. We wanted something that was *mental*—but "mental psychology" seemed terribly redundant. (Miller 1986: 210)

In short, they intended "cognitive" science to address cognition *and more*.

A glance through *Plans and the Structure of Behavior* (G. A. Miller *et al.* 1960) confirms this. That inspirational book (see Preface, ii, and Chapter 6.iv.c) discussed animal behaviour, instinct, and learning, as well as human memory, language, problem solving, personality, mental illness, and hypnosis. Social and cultural matters were touched on also. No aspect of mental life was excluded.

(It was left to another early volume, however, to highlight political, bureaucratic, and economic behaviour: Guetzkow 1962. The editor, Harold Guetzkow, had been a close friend of Simon when they were both graduate students at Chicago. At that time, Simon's interests—like Guetzkow's—had been in economics and management science, not psychology: see 6.ii.a.)

The breadth of coverage in *Plans and the Structure of Behavior* was seen—by readers who were sympathetic at all—as being just as it should be. Virtually all the founding fathers of cognitive science (Noam Chomsky excepted) had asked how motives and emotions interact with cognition, and several had also mentioned psychopathology. In short, these more sexy matters (literally!) were often discussed in the early days.

That didn't last. Because motivation, emotion, and social interaction (whether in small groups or in societies) are even more difficult to study—and to simulate—than cognition is, they were soon put onto the back burner. They were left there for thirty years, while cognition got almost all the attention. Vast amounts of research were done on perception, language, problem solving, concepts, belief, memory, and learning. The name of the field reflects this.

In fact, the label on this particular can was changed several times. In the early 1960s, the field was known by the more neutral "computer simulation". A Harvard graduate course was run under this rubric, and books appeared with titles such as *Computer Simulation of Personality* (Tomkins and Messick 1963), *Simulation in Social Science* (Guetzkow 1962), and *Computer Simulation of Behaviour* (M. J. Apter 1970). As research on cognition became more dominant, however, three new names emerged: cognitive studies, cognitive sciences, and cognitive science.

"Cognitive *Studies*" was chosen in 1961 by Bruner and Miller (the lead author of *Plans and the Structure of Behavior*) to name their new research centre at Harvard. This comprised a wide variety of psychologists, leavened by a few linguists and computer specialists and the occasional interdisciplinary philosopher. Nelson Goodman (1906–98), who co-founded Harvard's "Project Zero" studying representation and education in art, was in house when I was there, for example. These psychologists didn't do simulation as such, although Miller had co-published with Chomsky on mathematical models of language (9.vi.a). But they typically used ideas drawn from early AI, and from information theory, in their seminars and experiments (6.iv.d).

The "Cognitive" in the centre's title reflected the two co-founders' main interests: perception, language, memory, and problem solving. Even when Bruner studied values, he focused on their effect on *perception* (6.ii.a). Nevertheless, Miller later admitted that "conative" and "affective" phenomena (i.e. motives and emotions) should also be mentioned in the definition (see the quotation above, and also G. A. Miller 1978: 9).

"Studies" had become "sciences" by 1973. Already used in everyday chat by an Edinburgh research group for a couple of years, "cognitive sciences" first appeared in print in a defence of AI-based psychology, then under attack by a world-famous mathematician (Longuet-Higgins 1973: 37; cf. 11.iv). And the singular version—cognitive *science*—appeared soon afterwards, in two widely read collections of papers (Bobrow and Collins 1975, p. ix; Norman and Rumelhart 1975: 409).

Now, that's the label which is used most often. Even so, the editors of the recent *MIT Encyclopedia of the Cognitive Sciences* (R. A. Wilson and Keil 1999) chose the plural version—which highlights the fact that several very different disciplines are involved in the field.

The singular form, by contrast, highlights the intellectual links between them. That's why I've chosen to use it here. For as we'll see, there have been countless instances of work in one discipline being radically influenced by work in another. That's not surprising. To understand the mind (mind/brain) properly, one doesn't only need to look at it from all directions: one must also *integrate* the various views.

b. Two footpaths, many meadows

The field would be better defined as the study of "mind as machine". For the core assumption is that the same type of scientific theory applies to minds and mindlike artefacts. More precisely, cognitive science is *the interdisciplinary study of mind, informed by theoretical concepts drawn from computer science and control theory*.

These concepts change, as time passes. (Many examples of such change are described in later chapters.) So cognitive scientists don't believe that today's computer-related concepts suffice to explain the mind. Rather, they believe that they're a good beginning, and that later explanations will use concepts drawn from what then happens to be the best theory of what computers do (see Chapter 16.ix.f).

My "two-footpaths" definition, above, carries a health warning. As we'll see later, one highly influential alternative definition of the field specifically *excludes* control theory. It allows only explanations in terms of formal symbol manipulation (see Chapters 12.x.d and 16.iv.c–d). Cybernetics, and even connectionism, is therefore said to lie outside cognitive science.

For reasons which I hope will become clear throughout the narrative, I regard that definition as much too narrow. It's true, however, that cognitive science has seen—and is still seeing—competition, as well as cooperation, between computer science and cybernetics as ways of thinking about the mind. Indeed, the pendulum-swings between these two intellectual sources are a central, and fascinating, aspect of the story (see especially Chapters 4, 10, and 12–15).

As remarked in the Preface, the main disciplines involved are psychology, linguistics, AI, A-Life, neuroscience, and philosophy—and, though it's relatively rarely mentioned, anthropology. Certain areas of biology, such as ethology and evolutionary theory, are also included (and, at the fringes, some aspects of biochemistry are relevant too: 15.x.b). Moreover, the many examples in Section i.a (above) imply that the relevant research ranges all the way from mate finding in crickets to grammar, and even grief, in human beings.

The history of cognitive science is marked by a deep, and continuing, interdisciplinarity. This is a more intellectually intimate relationship than mere multidisciplinarity. Again and again, researchers in one area have borrowed *theoretical ideas*, not just *data*, from another.

Certainly, many specialist sub-areas (and sub-sub-areas . . .) have emerged over the past half-century. Each has its own conferences, journals, and textbooks. Moreover, their personnel rarely communicate. "Fair enough!" you may say. "If someone's interested in depth vision or learning, why should they bother with English grammar?" Well, they don't need to, in order to tackle their current problems. It remains true, nevertheless, that stereopsis and learning are studied in the way they are today partly because of mid-century work on syntax and mid-1980s research on past-tense verbs (see Chapters 7.vi.a and 12.vi.e). In short, even the most 'separate' specialisms share life-giving historical roots.

They share some central assumptions, too. All areas of cognitive science are informed by computational concepts, and driven by computational questions. In other words, all cognitive scientists use such concepts as core theoretical terms. This isn't the same thing as using computers. Biochemists or geologists, and non-computational psychologists too, often use computers as research tools (to do statistics, for example)—but their *theories* aren't computational. (Nor is it the same thing as building computer models: many cognitive scientists do this—but many don't.)

Broadly speaking, computational concepts are of two main types. On the one hand, they're drawn from computer science, AI, and software engineering. On the other hand, they hail from information theory and control engineering—in a word, cybernetics.

This dual definition, like my catholic definition of the field (above), carries a health warning. 'Computation' is often understood as Alan Turing defined it (see Chapter 4.i.b–c). Indeed, his definition remains the only rigorous one. And it *doesn't* cover cybernetics, nor even connectionist AI. Nevertheless, many people today—including computer scientists—use the term more widely. In other words, ideas about what computation *is* have become more extensive. (We'll see in Chapter 16.ix that, despite the undeniable loss of rigour, there are good reasons for this.) I'm one of many who use the term more widely than purist symbolists do. In general, the context should show when I'm using the term to refer to ideas from only one of these two sources.

The two sides of the computational coin were first clearly distinguished in the mid-twentieth century (see Chapter 4), although each had been prefigured much earlier (2.vii–x). Over the years, the theoretical concepts involved have developed into a varied group, defining many different types of information processing, virtual architecture, and computer model. This development, which hasn't been without hiccups, has involved both competition and cooperation between the two sides—first seen as competing in the late 1950s (Chapter 4.viii).

For example, research based on 'dynamical systems' falls on the cybernetic side of the fence, and is typically peppered with disparaging remarks about symbolic AI (14.ix.b, and 15.viii.c–d and xi). In particular, dynamicists claim that they can explain the temporal aspects of cognition, which earlier approaches ignored. But many people who work in this area were trained in AI, and depend heavily on it—for instance, when using genetic algorithms as ways of evolving dynamical systems (15.v). Similarly, most connectionist AI is closer to cybernetics, and has often been fiercely opposed to GOFAI—that is, to Good Old-Fashioned AI (Haugeland 1985: 112). Nevertheless, some researchers have tried to combine these two approaches (7.i.e–f, 12.viii–ix, and 13.iii.c).

As for the hiccups, Chapter 12 describes the birth *and renaissance* of connectionism—and the Sleeping Beauty phase in between. A-Life, too, had its Sleeping Beauty phase, from which it awoke one year later than connectionism. Psychology and philosophy have reflected these changes, offering very different theories of mind at different times.

The two computational pathways wound through many disciplinary meadows. The meadows were close neighbours at the beginning. Indeed, in the 1940s and 1950s—when distinct disciplines were being deliberately, outrageously, juxtaposed—highly *inclusive* consciousness-raising meetings were important (see Chapters 4.v.b and 6.iv.a–b). From the mid-1960s, however, the specialisms reasserted themselves. A second phase of "outrageous" interdisciplinarity was launched in 1987, at a party in the New Mexico desert (see 15.x). But most of the party-goers, though newly enthused, went back home to work in their own specialist houses.

This affects how our tale can be told. Chapters 2 to 6, by and large, move along a single time line, taking us from antiquity up to the mid-twentieth century. In Chapters 7 to 16, the time line branches. Each discipline has its own chapter (although AI has three, and we'll backtrack about 500 years for linguistics). Even so, most of the important topics feature in *several* 'disciplinary' chapters. For the same two computational pathways are there throughout, connecting the different disciplinary meadows with each other.

c. Why computers?

Computers as such are *in principle* less crucial for cognitive science than computational concepts are.

To be sure, computer technology (both digital and analogue) is an important player in the narrative. Computer modelling has a prominent role because it's often needed *in practice* to confirm—or even to discover—the full implications of a computational theory. Indeed, advances in software design (especially high-level programming languages: 10.v) and computer engineering may be needed before such theoretical modelling can be attempted. Turing himself was unable to develop many of his ideas because of the primitive state of computers in his day (15.iv). As he put it:

> At my present rate of working I produce about a thousand digits of programme a day, so that about sixty workers, working steadily through about fifty years might accomplish the job, if nothing went into the wastepaper basket. Some more expeditious method seems desirable. (A. M. Turing 1950: 61)

But cognitive scientists don't always build computer models. Chomsky's linguistics, John von Neumann's cellular automata, and David Marr's early brain theories, for example, were formal models—not functioning simulations (see Chapters 9.vi, 15.v, and 14.iv, respectively).

Some highly influential discussions weren't even *formal*. Marvin Minsky's "society of mind" theory (12.iii.d) and his earlier account of "frames" (10.iii.a), Michael Arbib's schema diagram for control of the hand (14.vi.c), and Robert Abelson's work on the structure of belief systems (7.i.a) are all cases in point. Indeed, the two seminal papers co-authored by Warren McCulloch and Walter Pitts (4.iii.e, 12.i.c, and 14.iii.a) were published in the early to mid-1940s, before the first modern digital computer had been built. It was another ten years before computer simulations of psychology were feasible. Some people would argue that *serious* simulation wasn't possible before the late 1980s—if then (see Chapter 14.vi.d).

Because computational concepts are essential, AI—or AI/A-Life—is a central discipline. Not all of AI is germane, however. Workers in AI—and A-Life—can have either of two motives. (Some have both.) The first is to build computer systems that are useful in some way. These range from automatic translators and financial networks to robot toys and remote-controlled surgeons. The second is to use software and/or robotics to help us understand human and animal minds (or life), or even *all possible* minds (or life). Let's call these 'technological' and 'psychological' (or 'biological') AI/A-Life, respectively. Only the latter project falls squarely within cognitive science.

Occasionally, this project has a further motive: to build, or anyway to start on the road towards building, a *real* intelligence, or a *real* living thing. These aims have driven some of the most well-known AI/A-Life research. And they've been very widely discussed for over fifty years—in terms (for example) of the Turing Test, strong AI, and strong A-Life (Chapter 16.ii, v.b–c, and ix.b). Nevertheless, they're minority tastes.

Despite the sensational quarter-truths peddled by the media ever since the 1950s, most researchers in AI/A-Life haven't argued—and probably haven't believed—that an AI program could actually be an intelligent mind, or that a merely virtual 'creature' could really be alive. Some have even denied it, claiming that *embodiment* is needed for life and intelligence (see Chapters 15 and 16.vii and x.) In short, this third motive has sometimes played a role in psychological AI/A-Life, but it isn't essential to it.

Technological AI is usually irrelevant to cognitive science—so is only rarely mentioned in my narrative—because it seeks to do something *irrespective* of how the mind/brain does it. The developers of IBM's Deep Blue, which beat the world chess champion Gary Kasparov in New York on 11 May 1997 (winning a prize of $100,000 dollars in the process: see 16.ii.c), were happy to use dedicated computer chips. These enabled the program, processing 200 million positions per second, to rely on exhaustive look-ahead over eight moves. Anyone studying how human beings play chess would avoid this biologically unrealistic hardware.

There's one type of situation, however, where even purely technological AI is relevant: namely, if someone believes that certain tasks simply *cannot* be done by computers.

For instance, Hubert Dreyfus's judgement (in 1965) that no program could play even "amateur" chess was falsified only a year later, when he himself was defeated by a program (H. L. Dreyfus 1965: 10; Papert 1968, para. 1.5.1). And his claim that no computer would ever play chess at a human level unless it could distinguish perceptually between "promising" and "threatening" areas of the board (H. L. Dreyfus 1972, pp. xxix–xxxiii, 208) was decisively refuted by the performance of Deep Blue. The fact that its exhaustive "counting out" strategy, to use Dreyfus's term, isn't one that humans can use is irrelevant.

Admittedly, the distinction between the two types of AI isn't clear-cut. For instance, I said in the Preface that Margaret Masterman's pioneering work on machine translation and classification was technological, but also guided by strong intuitions about how people process language. It's not that she wasn't interested in how the mind works—although, as a post-Fregean philosopher, she was wary of 'psychologism' (Chapter 2.ix.b). But detailed psychological questions would have been premature. There was no experimental evidence enabling her to decide, for example, that one of two thesauri was the more realistic. She had to rely on other, more intuitive, criteria.

Even today, fifty years later, most technological AI is grounded in intuitions about human thinking. What the writers of expert systems call "knowledge engineering", for example, includes a method of questioning human experts, to help them make their expertise explicit (10.iv.c). Sometimes, data from experimental psychology and/or neuroscience influence the program too. Some industrial applications even use special-purpose hardware chips modelled on the mammalian visual system (12.v.f). But the interpretation of the AI systems as models of actual mental processes isn't the object of the exercise.

d. What's in, what's out

Not all of *psychology* is germane to cognitive science, either. All psychological *data* are relevant, in the sense that cognitive science, if it is to succeed, must one day explain them. But many psychological theories aren't computational. This history narrates how some theoretical psychology became computational—and the dramatis personae are selected accordingly. (The same applies, *mutatis mutandis*, to anthropology, linguistics, and neuroscience.)

I'll say relatively little, for example, about the behaviourists, or Sigmund Freud—despite their importance for the history of psychology in general. With respect to computational psychology, behaviourism was significant as something to be reacted against, not developed (see Chapters 5 and 6.i–iii).

As for Freud, his psychodynamics (Chapter 5.ii.a) inspired some early AI simulations of neurosis, and a model of the effects of anxiety on speech (7.i.a and ii.c). In a broad sense, it informed Minsky's work on the society of mind (12.iii.d), and spread from there to Daniel Dennett's philosophy of consciousness (14.xi.b and 16.iv.a–b). It contributed to Arbib's schema theory, and especially to the application of schema theory to religion (7.i.g). And it provided examples and ideas that fed into Aaron Sloman's work on the architecture of grief (see Chapter 7.i.f). So I mention Freud in all those contexts—but I don't focus closely on him.

Even "cognitive psychology" doesn't always fall within cognitive science (Chapter 6.v.b). For instance, David Clark's (1996) explanation of—and therapy for—anxiety disorders (such as phobias, panic attacks, and post-traumatic stress disorder) analyses them in terms of the person's underlying beliefs about danger: it's cognitive, but doesn't involve explicit reference to computational concepts or theories. Admittedly, the term "cognitive psychology" was first defined in a computational context (Neisser 1967). But some cognitive psychologists, including that author himself in later years, specifically reject computational theories (7.v.e–f).

You may be surprised to see neuroscience included in this account. For a recent dictionary of psychology states that "cognitive scientists rarely pay much attention to the nervous system", and that cognitive science and neuroscience are "almost mutually exclusive" (N. S. Sutherland 1995: 83). The explanation given there is that "cognitive science deals with the brain's software, neuroscience with its hardware".

As a quick summary, that's correct. The cognitive scientists of the 1960s and 1970s adopted, or defined, an *abstract* (functionalist) philosophy of mind, and most still do (see 16.iii–iv). But facts about the brain have inspired various forms of AI, and of its cousin, cybernetics (Chapters 4.iii–vii and 12). Moreover, the intellectual traffic is increasingly two-way. Computational ideas inspired one of the most famous papers in neurophysiology, 'What the Frog's Eye Tells the Frog's Brain' (Lettvin *et al.* 1959), and they've been used for thirty years to model the brain (see Chapter 14). Neuroscientists today regularly use computational ideas, asking not only which cells and neurochemicals are involved but also what functions they're computing and/or modulating.

You may not expect A-Life to be counted as a member discipline, either. For A-Life researchers usually make a point of distancing themselves from traditional cognitive science, rejecting its representational ("Cartesian") view of mind. Even when they do so, however, they may admit that they "count as cognitive scientists" in so far as they try to create "working artifacts that demonstrate basic cognitive abilities" (I. Harvey 2005: first page).

Moreover, A-Life bears a new name on an old bottle. It dates back to mid-century cybernetics, and its three founding fathers—von Neumann, Turing, and W. Ross Ashby—were crucial also for the rise of other areas in cognitive science. In addition, many people believe that life and/or embodiment is *necessary* for mind (Chapter 16.vii and x). If they're right, then A-Life would be essential to our discussion even without the strong historical links.

The inclusion of anthropology may surprise you also. For it's rarely mentioned as being part of cognitive science. But we'll see in Chapter 8 that this is truer today than it was forty years ago, and that part of the reason is that much of the relevant work now appears under a different label—namely, evolutionary psychology.

As for philosophy, it plays a key role in the story. Indeed, it's rarely absent. Besides having its own chapter (Chapter 16), it has dedicated sections in several others (see Chapters 2.ii–iii, vi, and ix.b; 7.iii; 9.iv–v and viii; 10.iii.f and vi.a–d; 12.x; and 14.viii and xi). And it provides many passing remarks throughout the story.

This isn't merely because, before the scientists got their act together, only philosophers were saying anything about the mind. It's also because the scientists themselves have repeatedly raised philosophical questions—and they still do.

That's why there are so many different definitions of cognitive science, and so many competing camps (and changing fashions) within it. My pages don't tell a happy tale of consensual colleagues harmoniously seeking the same sort of truth. Personal rivalries and self-regard have played their part, of course: we're talking about human beings here, after all. But the deepest divisions have been at philosophical fault lines, so that *what could possibly count* as the truth, or as an explanation, isn't always agreed.

For example, cognitive scientists differ profoundly on how the mind–body distinction should be understood. A few even reject the distinction entirely. They also differ on what can count as a *representation*, a term that's often used within the field—and in some influential definitions of it.

Moreover, some philosophers outside the field argue that cognitive science can't illuminate *any* aspect of mind—not even cognition (or representation) itself. Such fundamental disputes have their own history, recounted in Chapter 16.iii–viii. Meanwhile (as explained in Section i.c, above), I'll continue to use "mind" as a useful shorthand, setting these basic philosophical questions aside until then.

The existence of philosophical disagreements between cognitive scientists may partly explain N. Stuart Sutherland's acid comment that cognitive science is "[an] expression [that] has come into being mainly in order to allow workers who are not scientists to claim that they are" (N. S. Sutherland 1995: 83). "Surely", someone might say in Sutherland's defence, "a *science* must have some fundamental philosophy on which all its practitioners agree?" This objection is too confident: even quantum physics would be excluded, given the notorious disagreement among physicists about its interpretation. With respect to cognitive science, it's partly because the theoretical foundations are still in dispute that it's so fascinating.

In sum, my view of cognitive science is relatively broad. It covers seven disciplines, and various forms of explanation. In particular, it includes both symbolic and cybernetic theories, despite the differences between them. Many people define the field in a much narrower way (see 12.x.d, 14.ix.b, and 16.iv.c–d). To do that, however, is both philosophically controversial and historically misleading. Cognitive science is a rich intellectual tapestry, woven over the years from many different threads.

1.iii. *Caveat Narrator*

Before embarking on my story of how cognitive scientists came to think as they do today, eight caveats are needed—for me as author, and for you as reader. (So it's *caveat lector* as well.)

The first three concern the temptations of Whig history (Butterfield 1931) and of over-idealized views of science. The fourth warns against the seductiveness of heroic accounts of creativity. The next two point out the difficulty in identifying an idea as a discovery—or even as new (which isn't the same thing). The seventh focuses on the role of rhetoric and publication in gaining a place in history. And the last reminds us that whether a new idea is favoured can depend on what sort of thing people with influence are prepared to count as an 'explanation'.

These are very *general* dangers. They make history-telling inherently problematic, a matter of more than just good plain fact. Indeed, a pessimist might argue that *Joe Bloggs was born in year x* is as near to plain fact as one can possibly get. I don't think the situation is quite as hopeless as that (see subsection b, below). But the historical claims in the following chapters, whether made by me or quoted from someone else—in their personal recollections, perhaps—are all prey in principle to the dangers listed here.

I'll mention some examples in this section as illustrations—but only very briefly: each one is discussed at greater length later. And if you return to this chapter after having read all the others, you'll be able to add many further instances.

There's a ninth, higher-order, warning too: the accepted history of the field is itself *part of the field*, helping to shape researchers' judgements and aims. However, because of the difficulties noted in the other caveats, there's no such thing as 'the' accepted history. For cognitive scientists with different theoretical agendas often differ about who did what, why they did it, and what the consequences were.

One illustration concerns the twenty-year connectionist 'winter': was it due to the undeveloped state of technology, to theoretical weakness, to the self-serving activities of two highly combative men, or to the fact that one of them had a very old friend in very high quarters? I'll ascribe it in some measure to all four (Chapter 12.iii.e). But that's not what it looked like at the coal face—and even there, it looked different depending upon which seam one was working on.

In brief: myths matter. History gets told over the coffee cups, as well as (sometimes) being made there—and the telling can affect the making.

a. Beware of Whig history

I don't assume nor, I hope, imply the Whiggist view that everything gets progressively better and better until we reach today's date.

Still less do I assume that it gets better smoothly. I've already mentioned the Sleeping Beauty phase of connectionism, for instance. Hotly disputed theoretical alternatives have surfaced, disappeared, and resurfaced over the years, and one aim of this book is to help readers understand why.

The Whiggist assumption, within the history of science, leads to two common types of anachronism.

On the one hand, ideas that don't fit into the progressivist narrative are ignored. This is usually because, from the historian's own viewpoint, they're incorrect or misguided. They may even be overlooked because they've become near-unintelligible. It can be difficult to understand what questions scholars and scientists of the past were asking, and why, if their concerns didn't align with what counts today as a scientific enquiry (N. Jardine 1991).

On the other hand, past ideas may be tendentiously misrepresented. This may be done by stating/implying that earlier thinkers were trying to solve *our* problems, and/or by using today's terminology in describing their work. It's all too easy to project our own concerns onto ancient writings that bear some superficial resemblance to ours, in order to make a progressivist story appear more plausible.

It's also possible, of course, to be correct in attributing something very like our interests to a past thinker, but incorrect in assuming—because of Whiggist presuppositions—that their work was actually influential. Conversely, if someone was unrepresentative of their age, we may fail to look closely enough to find interesting parallels between their thought and today's ideas.

Within cognitive science, there are specific examples that should warn us against all these varieties of Whiggism. They include arguments relating to fundamental issues, such as disputes about whether Descartes's account of mind was an advance on Aristotle's (Chapters 2.iii and 14.x–xi), or whether Goethe's biological views were well understood, and rightly scorned, by most of his scientific successors (2.vi.d–f, and 15.iii.a and vii.c). They also include narrower issues, such as whether Humboldt's views on language were as similar to Chomsky's as Chomsky himself has claimed (9.v.d–g). In addition, some work should have been more influential than it has been (7.i.e–f). An uncritically Whiggist approach would either exaggerate its early influence or ignore it entirely.

As for backwards projection of our own concepts onto the past, Descartes—contrary to what is now widely believed—did not deny (what we call) conscious experience to animals. Nor did he ascribe it to them, either. This wasn't because he was agnostic about the facts of the matter, but because he lacked the particular concept of consciousness used in formulating the question (see Chapter 2.ii.d–e). I argue also that we shouldn't attribute any version of the modern idea of 'man as machine' to those early Greek philosophers who posited materialism, nor to any ancient engineers inspired by them (Chapter 2.i). It's similarly anachronistic to assume that Charles Babbage had any intention of likening minds to machines, for he didn't: he was *not* an early cognitive scientist (see 3.iv).

Some historical figures with (some) ideas remarkably close to our own had a negligible—perhaps even regressive—influence on the later development of those ideas. Usually, this was because their ideas were so far ahead of their time that their contemporaries couldn't appreciate them.

Babbage, again, is (arguably) an example. He's often described as a crucial player in a Whiggist story about the development of computer technology, but some experts believe that knowledge of his work actually delayed the invention of modern computers (Chapter 3.vi.a). And Jacques de Vaucanson is often wrongly assumed to have had no scientific intent in building his automata, largely because most of his fellow artificers didn't—and because he committed the sin of exhibiting them for money (2.iv). Most of his contemporaries were under the same illusion, so even they didn't go to him for scientific inspiration.

As for more recent examples, Petr Smirnov-Troyanskii's pioneering ideas about machine translation, part-patented in 1933, were ignored even in his native Russia (9.x.a). John Clippinger's intriguing mid-1970s model of the effects of anxiety on speech fell into a black hole from which it hasn't yet emerged (7.ii.c). And even Karl Lashley's now famous discussion of 'The Problem of Serial Order in Behavior' (K. S. Lashley 1951*a*) caused barely a ripple when it was first delivered in 1948, or first published in 1951 (5.iv.a). It came into its own in 1960, when—thanks to late 1950s work in AI and linguistics—cognitive scientists were at last in a position to appreciate it.

Nevertheless, there are many areas where one can point to definite progress. And the value I have in mind here—given that "progress" can't be defined in a value-neutral way—is *closeness to the truth in describing and explaining aspects of the world*. This includes both how real people *actually* manage to think about real and imaginary worlds, and how it's *possible* for any creature—man, mouse, or Martian—to do so.

The growth of our understanding of cellular automata is one clear example of progress (15.v–viii). The advance of neuroscience is another (2.viii, 7.v.b–d, and 14), and of interactive human–computer interfaces yet another (9.xi.f and 13.v–vi). A fourth (related) example concerns the idea of mind-expanding 'cognitive technologies'. These were prefigured by a physicist in the 1940s, named by an experimental psychologist in the 1960s, and clarified/complexified over the next forty years by psychologists, computer scientists, and philosophers (10.i.h, 6.ii.c, 13.v, and 16.vii.d respectively).

Even when significant controversy remains, much may have been found out. That applies, for instance, to the degree of 'modularity' in human minds (7.vi.d–i); to the relation between autism and 'Theory of Mind' (7.vi.f); to how we understand religious concepts (8.vi); and to how we compensate for the fact that our rationality is limited (6.iii, 7.iv, and 8.i.b). Indeed, *every* chapter instances—quite apart from the amassing of new empirical data—the gradual clarification and development of theoretical ideas.

This constitutes scientific advance, even if the idea eventually turns out to be an explanatory cul-de-sac. (As Karl Popper put it, science grows by conjectures *and* refutations: K. R. Popper 1963; likewise Francis Bacon, in *The New Organon*, "Truth emerges more readily from error than from confusion": Bacon 1620.)

I make no apology, therefore, for the strong whiff of Whiggism that attends many parts of my text.

b. Losing the Legend

That claim—that we've already seen some progress in understanding the real world—may raise a few readers' eyebrows. Indeed, it may raise their blood pressure too. For, as evidenced by the 'science wars' that have raged for the last thirty years, social constructivists—supporters of 'the strong programme' in the sociology of knowledge—reject the possibility of scientific progress (as just defined) *in principle*.

That's because they deny that science is the study of some objective reality which exists, and which has certain properties rather than others, independently of human minds. In other words, they reject *realist* accounts of science, and *objectivist* accounts of truth.

One root of this position, which was officially inaugurated by Edinburgh's Science Studies group in the early 1970s (Bloor 1973), and backed by a quasi-anthropological study of the Salk Institute soon afterwards (Latour and Woolgar 1979), is the work of Thomas Kuhn. And cognitive science, arguably, offers examples illustrating Kuhn's remark that science "progresses" because old scientists die (1962: 150). I say "arguably" partly because most of the field's leaders are still alive. But partly, too, because the new ideas resisted to the end by some of those who've died—connectionism, in the case of the late Simon and Allen Newell for instance—are still *competing theoretical positions*, rather than new *paradigms* unquestioningly accepted by everyone else (see Chapters 12.viii–ix and x.d, and 15.viii.b–c).

Another root is neo-Kantian philosophy (introduced in Chapter 2.vi), or what's often called Continental philosophy—including the version of it known as postmodernism. For the science wars are a special case of what Simon Blackburn (2005) dubs "the truth wars", which oppose relativism to realism in respect of *all* areas of knowledge/belief. He sees this opposition as "arguably the most exciting and engaging issue in the whole of philosophy" (2005, p. xx). Certainly, it's fundamental to the philosophy of mind, as well as of science: that's why I flagged it very early on, in Section i.c above. Indeed, some disputes within and about cognitive science today are grounded in this philosophical debate (7.vii.c, 14.xi.a, and 15.viii–xi).

However, neo-Kantianism is so radically different from the Cartesian–empiricist Anglo-Saxon approach that the two are, to borrow Kuhn's term, virtually incommensurable. The one prioritizes sophisticated interpretation, or hermeneutics; the other relies on scientific objectivity, as understood in the experimental tradition. Consequently, they have very different ideas on what counts as a proper scientific attitude, especially with respect to the biological sciences—and a fortiori the human sciences of psychology and anthropology.

Both philosophical positions can be defended, to be sure (see 16.vi–viii). And historical evidence can be marshalled for each of them: the case for constructivism in seventeenth-century science, for example, has been made with some spirit, and much fascinating detail, by Steven Shapin and Simon Schaffer (Shapin and Schaffer 1985; Shapin 1994). But there's no clincher argument on either side. At some point, one must opt (*sic*) for one or the other.

For my part, I regard the constructivists' position as fundamentally irrational, even though they did have some important insights—which need to be remembered if one is to understand the history of this, or any other, scientific field (see below). The case against them was put in a nutshell nearly 400 years ago by Bacon:

> After the human mind has once despaired of finding truth, everything becomes very much feebler; and the result is that they turn men aside to agreeable discussions and discourses, and a kind of ambling around things, rather than sustain them in the severe path of inquiry. (Bacon 1620: 56)

Bacon himself, of course, was largely responsible for defining what is now familiar as well-honed scientific method. So today's constructivists are even more shocking than Bacon's contemporaries, in their rejection of "the severe path of inquiry".

The irrationality of their approach has been clearly summarized by philosophers Susan Haack (1996) and Noretta Koertge (2000), and put more tendentiously—but with panache—by the sociologist Ernest Gellner (1992). A serious philosophical rebuttal can't merely ask, "If science is mere cultural convention, why would anyone ever board an aeroplane?", and leave it at that. The arguments for constructivism, and also those against it, are more subtle. But this isn't the place to recount them (for an excellent discussion, see Blackburn 2005: esp. chs. 6–7). So I'll merely say that I see relativist and anti-realist philosophies of science as both self-defeating and fundamentally implausible. The relativism undermines every philosophical claim; and the rejection of realism, despite science's practical successes, ignores what philosophers of science call IBE, or inference to the best explanation (Harman 1965; Lipton 1991).

If realism is correct, as the vast majority of practising scientists (and most readers of this book?) assume, then scientific progress—in the sense defined above—is in principle possible. And in cognitive science, it has actually happened. In one discipline, it's happened *despite* opposition from highly influential constructivists in the area concerned (Chapter 8.ii.b–c).

So where's the caveat? Well, even where there has been progress, it hasn't always happened in a 'purely scientific' way. Personal and political interests sometimes influence the generation and/or acceptance of ideas in science, as they do in the arts. This is part of what the constructivists have been pointing out.

In other words, science in practice *doesn't* fit what the physicist, and science-policy expert, John Ziman (1925–2004) has called "the Legend". The Legend is "the stereotype of science that idealizes its every aspect", representing it as wholly objective and rational (Ziman 2000*a*: 2). If that mismatch isn't recognized, one's historical view of the field will be misleading.

In Chapter 2, for example, I describe the rise of Cartesianism within science in general and neurophysiology in particular. This took place very quickly: "From 1700 on, [such ideas] were taken to 'go without saying'; and, in practice, they often went unsaid" (Toulmin 1999: 108). The rapid spread of Cartesian ideas happened partly because they supported the sorts of experiment and theory already pioneered by Galileo Galilei and William Harvey, and taken up by the nascent Royal Society in London and Académie royale des sciences in Paris. (Three cheers for the Legend!) But there were sociopolitical reasons, too.

People weary of the religious disputes and bigotry that had been brewing in Europe since the late sixteenth century, and which had recently led to the ruinous—and destabilizing—Thirty Years War, welcomed Descartes's stress on clarity and agreement (even certainty) independent of religion. This was especially true in England and France, and in 'establishment' circles (Toulmin 1999: 119–25). And, of course, the gentlemen members of the two "Royal" societies were establishment figures par excellence. So they had more than purely scientific reasons for favouring Descartes.

That's not to say that these nebulous political considerations were consciously recognized by the individual thinkers concerned—still less, that the novel ideas were *generated* because of them. Even far less nebulous political influences may be invisible to the people whose creative work is being accepted largely because of them. For example, we'll see below (in subsection c) that Cold War politics encouraged the explosion of Abstract Expressionism in post-war New York, and fostered its acceptance by art critics around the world. However, this fact has come to light only relatively recently. At the time, it was hidden *even from the artists themselves.*

Sometimes, political influences are more easily visible. So, for instance, progress in cybernetics and AI has been (and still is) largely fuelled by military aims and funding, as opposed to the disinterested pursuit of truth (Chapters 4.vi.a, 9.x.a, 11.i, and 12.vii.b). That's a large part of what Haraway was saying (Section i.a, above), although she supplemented it with much more questionable claims—about the "need" for a feminist philosophy of science, for example (for a sensible rebuttal, see Koertge 2000).

Personal interests have entered the picture too. A hugely influential Report on AI prepared for the UK's Science Research Council in 1972 would probably never have been commissioned, and for sure would have been very different, if the personality of the UK's leading AI scientist had been other than it was (see 13.iv.a–b). A disinterested document this was not.

Much the same applies to an equally influential, and apparently highly abstract, attack on connectionist AI (12.iii). The published version pulled no punches, and the draft had contained many vitriolic remarks which friends persuaded the authors to remove.

Perhaps you're surprised, even shocked? The more one buys into the Legend, the more surprised one will be. The Legend has had powerful defenders—above all, among followers of Popper (1935, 1963). Popper himself was a sophisticate: he saw his theory as an idealization (a "rational reconstruction") of science, an account of how it should be done rather than a description of how it is in fact done. (Predictably, he dismissed Kuhn as a *philosopher* of science, seeing him as a sociologist/historian instead.) But cruder versions of the Legend still abound, often written by practising scientists (e.g. L. Wolpert 1992).

In such accounts, personal prejudices and sociopolitical factors are glossed—where they're even recognized—as unwanted subjective intrusions into objective study. While they may, and perhaps should, often determine which problems will be considered by science, they're held to be irrelevant to the content of the solutions offered. James Watson (1968), for instance, admits to the personal ambitions that drove him and Francis Crick in their search for the structure of DNA, but regards these as irrelevant to what they said in their scientific papers. The claim is that theory is value-free, even if choice of research topic isn't.

But that's too quick. For one thing, choice of research topic is hugely relevant in understanding the history of an entire scientific field. For another, even the driest theories are sometimes infected by personal/political values. (So a claim about the superiority of one programming language, or programming style, over another—*Boring*!—was typically stated, during the years of the Cold War, in terms pitting liberal democracy and egalitarianism against authoritarian regimes: Chapter 10.iv.a.) As for theories carrying implications regarding the nature of human beings, the scope for personal/political issues to prejudice clear judgement is significant (see Chapter 8.ii). Third, the core point of the constructivists' case, science is influenced also by *conceptual* assumptions, which can even affect researchers' *perception* of the 'data' (see 16.iv.e).

Ziman gives countless examples (mostly drawn from physics and chemistry)—and careful arguments—showing that the Legend not only isn't true, but couldn't be true. It underplays the sociology and psychology of science, and distorts its epistemology.

However, the Legend's polar opposite (constructivism) isn't true either—as I've said, above. Indeed, Ziman wrote his book to *defend* science against the most extreme of these attacks. Besides his many anti-Legend examples, he also cites a host of cases showing how science, as a body of theory and as a social institution, works as a relatively objective, rational, enterprise—despite the frailties of its human practitioners.

Why did Ziman feel the need to do this? After all, he wasn't a professional philosopher. Can't science be left to look after itself? Well, apparently not:

> Science is under attack. People are losing confidence in its powers. Pseudo-scientific beliefs thrive. Anti-science speakers win public debates. Industrial firms misuse technology. Legislators curb experiments. Governments slash research funding. Even fellow scholars are becoming sceptical of its aims. (Ziman 2000*a*: 1)

The "fellow scholar" mentioned here was the historian Gerald Holton, a long-time friend of science whose recent work had, perhaps surprisingly, given some credit to the "anti-science" position (Holton 1992, 1993).

Ziman, too, gave constructivism some credit. He even went so far as to declare himself a postmodernist:

> Our investigation thus arrives at a paradoxical conclusion. Academic science, the spearhead of modernism, is *pre-modern* in its cultural practices: and yet it turns out to be *post-modern* in its epistemology.
>
> Contrary to the Legend, science is not a uniquely privileged way of understanding things, superior to all others. It is not based on firmer or deeper foundations than any other mode of human cognition. Scientific knowledge is not a universal "metanarrative" from which one might eventually expect to be able to deduce a reliable answer to every meaningful question about the world. It is not objective, but reflexive: the interaction between the knower and what is to be known is an essential element of the knowledge. And like any other human product, it is not value-free, but permeated with social interests. (2000*a*: 327)

He hastened to add, however, that "terms such as 'modernism' and 'post-modernism' are very ill-defined . . . Most scientists only know of them as slogans, uttered wholesale by the partisans of the most diverse fashions and fads."

That's true. Even the "partisans" use the term postmodernism in differing ways. When it was used by the French philosopher Jean-François Lyotard (1924–98) in the late 1970s, in his study of "knowledge in computerized societies", he'd attacked all overarching philosophies ("meta-narratives" or *grands récits*) of human progress. As though his little (110-page) book weren't succinct enough, he put it in a nutshell:

"Simplifying to the extreme," he said, "I define *postmodern* as incredulity towards metanarratives" (1984, p. xxiv). So he attacked Marxism and Christianity, modernist aesthetics (which trumpeted the restorative 'transcendence' of art), and scientism too. His main target, indeed, was the "legitimacy" of scientific knowledge, and its "mercantilization" owing to "the computerization of society" (1984: 3–9). In other words, postmodernism was explicitly opposed to the growing role of science and computers in the late twentieth century.

Indeed, science had already been targeted by the literary scholar Roland Barthes (1915–80). In his essay on 'The Death of the Author' (written in May 1968, during the revolutionary events on the streets of Paris), he'd rejected its 'quasi-theological' claim to objectivity:

> Literature . . . by refusing to assign a "secret", an ultimate meaning, to the text *(and to the world as text)*, liberates what may be called an anti-theological activity, an activity that is truly revolutionary since to refuse to fix meaning is in the end to refuse God and his hypostases—*reason*, *science*, *law*. (Barthes 1968/1977: 147; italics added)

Renaissance scholars, too, had seen the world as a text, to be interpreted by us. But they'd supposed it to be God's text, expressing His hidden meaning. The scientific revolution had consisted, in large part, of rejecting these hermeneutic (intentional) accounts of the world in favour of empirical ones—hence the subtitle of Bacon's 1620 book: *or, A True Guide to the Interpretation of Nature* (and cf. Glanville 1661–76). Now, four centuries later, Barthes was describing the world as a text with *no* author, whose multifarious "meanings" aren't actual messages (or scientific laws) for us to discover but possible conceptualizations for us to construct.

Lyotard himself admitted that his thesis "makes no claims of being original"—"or", he added (as a relativist must), "even true" (p. 7). It revived the neo-Kantian contrast between scientific and humanist knowledge emphasized by Wilhelm Dilthey (1833–1911) and Max Weber (1864–1920), and by many philosophers today (see Chapter 16.viii.b). Where Dilthey (1883) had spoken of empirical/scientific versus historical/hermeneutic knowledge, and Weber of *Naturwissenschaft* versus *Geisteswissenschaft* (i.e. natural science versus human, or interpretative, science: Shils and Finch 1949), Lyotard spoke of science versus narrative. (And often of discourse rather than knowledge, for the legitimization of a given type of discourse *as* knowledge was being questioned.) Moreover, he was offering a political critique (of global capitalism) as much as an exercise in 'pure' epistemology.

Lyotard's influence on political and aesthetic discourse (and on the practice of art) was immense. But science, and its computerized offshoots, didn't escape. And Barthes, even though he'd focused primarily on literary texts, engendered hostility to science—more specifically, to science's claims to objectivity—in his readers.

As postmodernism flourished in the 1980s and 1990s, becoming (to traditional minds) ever more outrageous and bizarre, some followers of Lyotard and Barthes—often encouraged by the Science Studies work mentioned above—passed from deep scepticism to genuine incredulity, *even with respect to scientists' answers to 'properly' scientific questions*. They didn't confine their doubts to the social/behavioural sciences, which many philosophers believe lie outside science's reach (see 16. viii.a–b), for even the objectivity of physics was suspect. Calls were made, for example, for a specifically feminist philosophy—and practice—of science (e.g. Harding 1986; Longino 1990). As one illustration, the feminist anthropologist Emily Martin (1991) made a bitter sociopolitical attack on theories of reproductive biology (for a sanity-restoring reply, see P. R. Gross 1998). It was as though, in these postmodernists' eyes, the metanarrative was not just scientism but science itself.

That was many steps too far for Ziman. He was an opponent of scientism—but not of science. His version of postmodernism, then, is very different from that of the virulently anti-science faction in the science wars.

In short, the Legend is false. The disinterested pursuit of truth is rarer than many choose to believe, and the conceptually innocent ditto is impossible. On those points at least, the constructivists have had important things to say—things which must be borne in mind when thinking about the history of cognitive science.

c. The counter-cultural background

The science wars were part of a more general intellectual/political movement, which the sculptor Theodore Roszak (1969) dubbed the counter-culture. This was politically prominent in the 1960s and early 1970s, and still intellectually prominent in the 1980s and 1990s—by which time it had largely metamorphosed into the postmodernist movement. On the whole, counter-culturalism favoured non-scientific, and even explicitly anti-scientific, ways of thinking. (For examples of counter-cultural anti-science diatribes, see P. R. Gross and Levitt 1994/1998; Parsons 2003.) It wasn't only the arts that were celebrated. Religions in general, and 'New Age' spirituality, flourished. Crystals were more respected as amulets than as chemicals on the laboratory bench.

Computers were even less favoured than chemicals. Then available only to very large organizations, they were seen by most 1960s–1970s counter-culturalists not as a useful tool but as a threat. And the image of mind as machine was the deepest threat of all: "Technocratic assumptions about the nature of man, society, and nature [i.e. cybernetics]", said Roszak, had "warped" the experience of scientists, scholars, and policy-makers at source. "In order to root out those distortive assumptions, *nothing less is required than the subversion of the scientific world view*" (1969: 50; italics added).

This movement had philosophical roots in the nineteenth and early twentieth centuries (Romanticism, Marxism, and Continental phenomenology). In that guise, the culture it was countering was Cartesian modernism, and especially Enlightenment optimism about the reach of science.

The philosophical arguments were reinforced/overlain by mid-century disillusion about some of the intended (e.g. Hiroshima) and unintended (e.g. pollution) effects of science. Rachel Carson's environmentalist book *The Silent Spring* (1960), and her *New Yorker* articles on the ill effects of industrial chemicals, led to heated debates in the US Congress. It wasn't only decisions about technology that were questioned: explicit attacks were mounted against taxpayers' money being given for science education and research in general (R. C. Atkinson 1999). (Science education in the USA had been hugely boosted around 1960, because of the shock presented by the Soviets' Sputnik, a football-sized satellite orbiting the earth every 90 minutes: see 11.i, preamble.)

But the anti-science campaign also drew major strength and inspiration from political events—above all, the Cold War. This dominated Western, and especially American, politics and military planning from 1947 on. (There was some lessening of tension during the 1970s, but it was ratcheted up again by Ronald Reagan in the 1980s: see Chapter 11.i.c.) So the counter-culture was driven by a growing unease about the effects of technology (from nuclear weapons to agrochemicals), and about the arms race and MAD (Mutually Assured Destruction) calculations of the Cold War.

It was further inflamed by the passions involved in the mid-1960s civil rights movement in the USA. Not least, it was fostered by discontent/disillusion about the Vietnam war—in which more US bombs were dropped on that undeveloped country "than had been dropped by all combatants in *all previous wars combined*" (P. N. Edwards 1996: 137). Huge sums of money for scientific (including AI) research aiding the Vietnam adventure were made available from the mid-1960s to early 1970s, although lack of success in both the research and the war led to a tightening in the early 1970s.

Encouraged by intellectual/political leaders such as Chomsky (9.vii.a), Herbert Marcuse (1964), and Angela Davis, young people were enthused to protest against these political forces. Student activism in the USA began in 1964 with the Free Speech Movement at Berkeley, which led to overreaction by the authorities and eventually to riots and tear gas on campus (McGill 1982). Those confrontations, and many others across the world, were fuelled by the student-led *événements* in Paris in May 1968, which are still frequently mentioned today. Even those young people who didn't join in the violence were often broadly sympathetic.

Science and technology were seen as enemy forces. Both were crucial to Cold War rhetoric, and to Cold War military preparations, whether offensive or defensive—and generous funding was provided by the US (and UK) governments accordingly. (Some of it reached AI and other areas of cognitive science: 6.iv.f and 11.i.) Moreover, that funding was clearly visible to the public, and thus provocative to those whose politics might lead them to be provoked. Roszak himself was one of those, and his *Counter-culture* book provided a host of references to the angry and/or despairing writings of many others (1969; cf. 1986).

He wasn't concerned only—or even primarily—with bombs. Scorning the "commonplace contemporary idiocies which small minds are now busily elaborating into a *Weltanschauung*", he rued "the degradation of human personality" that he believed was resulting from the use of Wiener's cybernetic metaphors for mind.

This effect, he said, was no mere philosophical bagatelle. For it was influencing military policy too: "Not even Jonathan Swift could have invented such pernicious lunacy as the balance of terror or thermonuclear civil defense" (1969: 295). The "lunacy" was rooted in scientific and technological research in general, and especially in the work of RAND, the Stanford Research Institute, "and ever so many other military–industrial–university think-tanks". (Both RAND and SRI were crucial to the rise of AI, as we'll see in Chapters 10.ii.a and 11.i.)

Sciences having no direct military relevance were despised too, especially if they were seen to imply a non-humane image of mankind. Even at the outset of the Cold War, social science in general, and behaviourism in particular, was already being lambasted in the popular press. For example, an editorial in *Life* magazine commented viciously on Burrhus Skinner's utopian (or dystopian?) novel *Walden Two* (1948). Its author, the cultural commentator John K. Jessup, also reviewed the book for *Fortune*, where he said:

> If social scientists share Professor Skinner's values—and many of them do—they can change the nature of Western civilization more disastrously than the nuclear physicists and biochemists combined. (quoted in Skinner 1979: 369)

In the counter-culture's opinion, then, the sciences were deeply compromised (and technology, even more so). The arts, and religion, were seen as the saviours of civilization. For in this neo-Romantic version of the *verum factum* tradition (i.b above), the creative arts were both truly free and essentially intelligible. In particular, they were untouched by the contaminating fingers of the military–industrial complex.

That's a historical irony, since Cold War influences on the arts in the USA were in fact even deeper, if less costly, than those on science. But they were much less provocative, because apart from Senator Joseph McCarthy's early 1950s banning of "subversive" authors from government-funded libraries (and his committee's protests at a US drama group's staging "two productions of some Russian guy called Anton Chekhov"—Caute 2003: 62), they were deliberately—and for many years, successfully—hidden.

Or rather, they were hidden in the USA. In the Soviet Union, comparable pressures on artistic style and content were clearly explicit. The dramatist Konstantin Simonov declared that "in defense of Communism, most sacred of all things, all powers must be employed, including art" (Caute 2003: 108). This was official policy. Lenin himself had banned Russian modernism (e.g. Marc Chagall and Vasily Kandinsky) in the early 1920s, insisting on "revolutionary realism" instead; by mid-century, painters had to join the Union of Soviet Artists, whose rules prescribed adherence to socialist realism (Caute 2003: 510, 519). And in 1947 the Director of the Academy of Arts had announced: "In educating young artists we must make it absolutely clear that penetration of the walls of Soviet art schools by this or that decadent influence from the capitalist West is absolutely out of the question" (Caute 2003: 514). He meant modernist painting, from Paul Cézanne (and even the Impressionists) onwards—and especially Abstract Expressionism, which was making its mark in New York at that time.

The rapid rise of Abstract Expressionism in the 1950s, and the displacement of Paris by New York as the 'hub' of modern art, weren't due to an excess of creative genius in the studio lofts of SoHo. To be sure, Mark Rothko and Jackson Pollock painted as they did (and Roszak sculpted as he did), even before the Cold War started, for their own reasons: aesthetic, not political. (Similarly, scientists developed their theories for their own reasons: scientific, not political—although some of the *applications* were specifically military.) These artists were drawn to abstraction, and to individual expression, by pressures internal to art and their own psychology. But their huge public success wasn't due only to aesthetic values, nor even to enthusiastic art critics/collectors like the notorious Clement Greenberg. It was hugely encouraged by US government investment. For the politicians, it wasn't art for art's sake but art for America's sake.

The government's interest wasn't mere cultural imperialism, a wish to be top dog in the ateliers of the world. Admittedly, Americans would take pride in the fact that painters were increasingly looking to New York, not Paris, for inspiration. The art critic Robert Hughes says, "It would be foolish to claim that 1945–70 in New York rivaled 1870–1914 in Paris. America has never produced an artist to rival Picasso or Matisse, or an art movement with the immense resonance of Cubism" (Hughes 1991: 3–4). Nevertheless, he remembers:

> In the early 1960s, when I was a baby critic in Australia, it seemed that faraway New York had become a truly imperial culture, heir to Rome and Paris, setting the norms of discourse for the rest of the world's art. . . . One saw this triumph from afar. . . . In Australia one's response to it came out as a sigh—resignation to one's own cultural irrelevance. (Hughes 1991: 3, 4)

> Thirty years ago, Abstract Expressionism was pretty well a mandatory world style. We in Australia looked at it with awe. . . .
>
> This act of unwonted humility was made by thousands of people concerned with the making, distribution, teaching, and judgment of art, not only in places like Australia but throughout Europe and—not incidentally—in America in the mid-1960s. They resigned themselves to an imperial situation. (pp. 5–6)

> When Americans in the fifties and sixties eagerly claimed that their art had superseded that of Europe, their eagerness itself was a period phenomenon. . . . The idea that Europe was culturally exhausted was an important ingredient of American self-esteem. (p. 7)

However, there was more to it than that. Specifically, there was also a Cold War dimension. If Rothko and Pollock had painted tractors, or even meticulous still lifes, the government money wouldn't have been forthcoming. For there were two unspoken political messages. On the one hand, there was a clear distinction and a choice: Abstract Expressionism was the aesthetic opposite of the Realist representational art favoured in—or allowed by—the Soviet Union. On the other hand, the American artists' freedom to shock, to counter accepted artistic canons and to express their individuality in doing so, was a visible sign of the freedom generally available in the West.

But such messages, to be truly effective, had to be unspoken. If it was only the Soviet Union who directed their artists, individual thought in the West being no affair of government, then US government support had to be invisible. (A second reason was that modernist art wasn't popular with the US taxpayer. An ex-CIA man later admitted that "It had to be covert because it would have been turned down if it had been put to a vote in a democracy"—Caute 2000: 550.)

Accordingly, much of the investment came via supposedly independent bodies, such as the Museum of Modern Art (MOMA) and the National Endowment for the Arts (NEA), founded in 1965 (Guilbaut 1983). These institutions had access to Rockefeller largesse. (Governor Nelson Rockefeller, who had been supporting modernist painting since the mid-1940s, had set the ball rolling in 1960 by initiating the New York State Council on the Arts, having failed to persuade Congress to sponsor a national body.) They not only bought the New York School's canvases, but also sponsored Expressionist exhibitions across the USA and abroad.

Europe was the prime target, because of its proximity to (and sympathies for) communism. But non-aligned India and Japan were targeted too. For example, a huge exhibition of American art was sent to New Delhi in 1967, and a two-day seminar, led by Greenberg, was held in the hope of encouraging young Indian artists to study in New York rather than Paris—or, worse, Moscow (D. Guthrie, personal communication). The intention was to impress a political moral on an international audience, by contrasting the rigid social realism of Soviet painting with the liberated artistic expression of 'the Free World'. (After the Cold War was declared 'won' in the early 1990s, NEA funding for the visual arts plummeted accordingly: D. Guthrie, personal communication.)

Analogous activities went on in the literary and musical worlds and in film, theatre, and dance too (F. S. Saunders 1999; Caute 2003, pts. II–IV). Large sums of money were silently channelled through supposedly neutral bodies such as the Rockefeller and Ford Foundations, and the Congress for Cultural Freedom—which was set up for these specific purposes. The National Endowment for the Humanities (NEH) supported work by writers whose left-leaning politics would have led to their being spurned in the McCarthyite period a decade earlier.

(Over the years, the NEA and NEH were hugely beneficial to the arts in America. A recent NEA chairman reported that, since 1965, the number of state-supported arts agencies had grown from 5 to 56, and local ones from 400 to 4,000; non-profit theatres had burgeoned from 56 to 340; symphony orchestras had grown from 980 to 1,800, opera companies from 27 to 113, and dance companies had multiplied eighteen times—Ivey 2000: 3.)

Where literature was concerned, spreading the message internationally was a problem. The NEH couldn't send exhibitions of writing around a multilingual world. Nor, as a "National" body, could they support foreign writers directly. So the CIA stepped in: the transatlantic literary journal *Encounter*, and several 'progressive' European magazines including Germany's *Der Monat* and France's *Preuves*, were funded in large part by the CIA. CIA money ($750,000) was used also for the New York Metropolitan Opera's tour of Europe in 1956, and the Agency sponsored the Boston Symphony Orchestra's 1956 European visit.

What's relevant for our purposes here is that the covert political agenda in the arts wasn't fully uncovered until the mid-1980s. Its discovery caused outrage, among the people who had long been suspicious only of science.

To be sure, *Encounter*'s political stance had already been questioned in the 1960s, largely because it "dealt gently, if at all, with topics such as race and Vietnam" (Collini 2004: 10). Conor Cruise O'Brien, who then had no inkling of the financial arrangements, commented in 1963 that the editorial policy seemed consistently designed to support the US government. As he said later, when the truth had come out: "the beauty of the operation . . . was that the writers of the first rank, who had no interest in serving the power structure, were induced to do so—unwittingly" (quoted in Collini 2004: 10). The magazine's funding-cover was conclusively blown in 1967, leading the English co-editor Stephen Spender, who'd been innocent of the CIA involvement, to resign (J. Sutherland 2004). But the *visual* arts remained relatively untouched by such rumours.

Only "relatively": in the early 1970s, Max Kozloff (1973) highlighted the political implications of the sudden success of the New York school, and the American muralist Eva Cockcroft (1974) had some even more pungent things to say—linking MOMA, the CIA, and the Rockefellers. But the cat was well and truly let out of the bag in the 1980s by the French art historian Serge Guilbaut, in his fascinating book *How New York Stole the Idea of Modern Art* (1983). The major scandal arose from his July 1984 article of the same title in the art magazine *Commentary*, and from back-up articles by others, including Cockcroft, in the August 1986 number (several are reprinted in Frascina 2000). Since then, evidence of comparable—indeed, cooperative—cultural/intellectual meddling by Britain's intelligence services has come to light (Dorril 2000).

Given that the counter-culturalists of the 1960s and 1970s didn't realize that art was (unknowingly) acting in the service of capitalism, they couldn't be enraged by this. Their political animus was reserved, rather, for *science*. And after all, it was the scientists and technologists, not the artists, who were more directly involved in the governmental war machine.

The history of cognitive science was coloured as a result. It wasn't to be all hostility, however. As we'll now see, late-century theoretical shifts within the field led the attitude of the counter-culture to change: from being firmly against cognitive science, to being (to some extent) in favour of it.

d. The counter-cultural somersault

Even an anti-science movement can have preferences *within* science. For example, the mentalistic aspect of the cognitive revolution was more attractive to the early counter-culturalists than behaviourism was. However, computers—given their place in military–industrial technology, and in the growth of global communication—were a special focus of suspicion. (We've already seen, for instance, that Lyotard's key text was explicitly aimed at "knowledge in computerized societies".) In particular, mind-as-machine was anathema (Roszak 1986).

That's an over-simplification. For instance, it doesn't apply to one of the most influential members of the counter-culture, the biologist–artist Stewart Brand (1938–). Brand's first *Whole Earth Catalog* (1968), a compendium of "tools" for environmentally friendly living, inspired a host of back-to-nature projects worldwide. (The 1972 edition sold over one and a half million copies, and won a US National Book Award.) He had criticisms of technology aplenty, but he wasn't an enemy of technology as such. Far from it. He'd helped Douglas Engelbart to demonstrate the first computer mouse at a now-famous meeting in San Francisco (see Chapter 10.i.h). And he shared Engelbart's faith that personal computers, and IT in general, could help towards more ecologically viable lifestyles. Computer software was featured in the first *Catalog*, and increasingly in the following ones. So one of the early heroes of the counter-culture was also a computer enthusiast and computer visionary.

Moreover, a few brave artists in the mid-1960s were beginning to experiment with computer art (see Chapter 13.vi.c). Indeed, a major international exhibition (the first) was held in London in 1968 (Reichardt 1968). Many of the visitors were excited—though not the art critic of *The Guardian*, who described it as a "frivolous activity" (A. Sutcliffe, personal communication). But it was the relative *weakness* of the counter-culture in late 1960s Britain that had enabled the show to go ahead. One of the artists involved recalls that it "could at this time not have taken place in Paris. The revolutionary students would have swept it away" (Nake 2005: 59). The organizer herself has said: "The same venture in Paris would have needed police protection" (quoted in Klutsch 2005: 109). And a historian has commented:

> Could it be that the ICA's "happy accidents" flourished so well because they were staged in an atmosphere of breathtaking *naïveté*? Only a few lone voices seem to acknowledge the more serious and inevitably unhappy accidents that litter the history of cybernetics. . . . [In Great

Britain] the subversive momentum of 1968 never unfurled in the same way, with the same force, as it did in continental Europe or the United States. . . . Against this backdrop, [the London exhibition] offered a light-hearted view of the modern world without raising too many (if any) objections or stirring fears. (Usselmann 2003: 391 ff.)

AI/cybernetics, by contrast, did stir fears—and was a prime target for criticism accordingly. From the late 1970s on, it suffered high-profile counter-cultural attacks by Dreyfus and Joseph Weizenbaum (11.ii). And cognitive science as a whole, which has AI at its intellectual core, was targeted too (Roszak 1986).

Chomsky's specifically political attacks on orthodox social scientists, and on their policy advice to the US government, were partly responsible: after all, he was highly respected *as a cognitive scientist*. But since the field had unfashionable—and apparently threatening—things to say about human beings and human nature, it would have been attacked by the counter-culture even without him. And, significantly, it wasn't only the intellectuals who disapproved of AI and cognitive science. These powerful cultural forces led US politicians to approve a temporary drop in military funding for AI, and a skewing towards civilian applications (see 11.i.b).

The component disciplines were each affected. So in the philosophy of mind and of psychology, neo-Kantianism (including the newly published Kuhn) soared in popularity in the UK and USA. Cognitive anthropology was nipped in the bud in the early 1970s, and all but destroyed by the 'literary' turn in the discipline (8.ii.b–c). At much the same time, many social psychologists reacted strongly against experimental, computational, and information-theoretic approaches (6.i.d). A feisty best-seller written by a well-known professional psychologist in Great Britain was called *The Cult of the Fact*, a title that says it all (Hudson 1972). One leading cognitive scientist was so worried by these professional developments that he wrote a book, and organized a high-visibility conference, to protest against them (see 6.i.d).

In short, cognitive science in its early years faced hostility from counter-cultural critics. In its later years, however, they *welcomed* certain intellectual changes that took place within the field.

The change of heart was initiated by the public availability of the personal computers that had been dreamt of by Brand and Engelbart, and by the development of the Internet, which allowed for what Haraway called "network identities" (cf. Chapter 13.v.d and vi.e). But this intellectual somersault was soon reinforced by specific theoretical aspects of late-century cognitive science.

For instance:

* Computational psychology highlighted situatedness, embodiment, and epigenesis (Chapters 7.iv.g, v.b–f, and 15.vii–viii).
* In the late 1970s, the AI researcher Terry Winograd left MIT spiritually as well as geographically to join Fernando Flores and Hubert Dreyfus in California (9.xi.b), and
* Minsky and Seymour Papert started drafting their theory of the decentralized "society" of mind (12.iii.d).
* Dennett, following Minsky, described the "self" not as a unitary thing but as a laboriously constructed—and not fully coherent—"narrative" schema that helps to guide our choices (7.i.e and 14.xi.b).

* In the 1980s Rodney Brooks and Randall Beer rejected GOFAI in favour of situated robotics (13.iii.b, 15.vii and viii.a).
* GOFAI researchers started studying "distributed cognition", wherein coherent behaviour emerged from the action of many separate "agents"—with no central controller (13.iii.d).
* Similarly, PDP connectionism flourished, and seduced the public at large—and even some postmodernist philosophers (Globus 1992; Canfield 1993; E. A. Wilson 1998)—largely because of its decentralized approach (12.vi and x).
* Much the same happened when A-Life hit the scene, offering what some saw as a near-magical alternative to traditional cognitive science (15.x.a, and S. R. L. Clark 1995).
* The magic was seemingly underlined by the emergence of unexpected properties through computerized evolution (15.vi).
* Some anthropologists used the ideas of situated action and distributed cognition to analyse the behaviour of human groups (8.iii).
* Brian Cantwell Smith started work on a radically new, and admittedly highly eccentric, "participatory" account of the nature of computation as such (16.ix.e).
* The availability of personal computers encouraged a wide range of experiments in computer art, including interactive and/or evolutionary art (13.vi.c).
* And, by the end of the century, the technology of 'virtual reality' enabled people to experiment with the presentation, and perhaps even the construction, of *self* in very 'non-Cartesian' ways (13.vi. d–e).

These ideas fitted well with certain aspects of the late-century counter-culture. For example, postmodernists in literary and aesthetic circles—who'd already proclaimed "the death of the author" (Barthes 1977)—welcomed the implications of non-hierarchical control, and of user-directed computer technologies such as hypertext and interactive art (Chapters 10.i.h and 13.v.d and vi.c).

Alongside their undermining of the authorial signature had been their undermining of the unitary self. So, likewise, they had some sympathy with theoretical work that presented the mind and/or self as a virtual machine, consisting of many interacting agents as opposed to a unitary Cartesian centre (Chapters 7.i.e–f, 12.iii.d, and 16.iv.a–b). By the same token, they welcomed the playfulness in self-presentation that was made possible by the Internet (13.vi.e).

Connectionism was explicitly favoured by some postmodernist writers. Several claimed that Jacques Derrida's notions of deconstruction and *différance* were largely homologous with PDP ideas, even suggesting that his approach was scientifically authorized by them (Globus 1992; Canfield 1993). The feminist philosopher Elizabeth Wilson rejected that last paradoxical claim but she, too, dressed connectionism in deconstructionist clothes (1998, esp. 14, 24–30, 196 ff.). She saw PDP as providing an opportunity "not merely to rethink cognition, but also to rethink our [i.e. counter-culturalists'] reflexive [self-aware, not automatic] critical [i.e. postmodernist] recoil from neurological theories of the psyche" (Wilson 1998: 14). Connectionism couldn't solve the philosophical/political questions she and like-minded colleagues were interested in—but, despite its provenance in (normally suspect) biology and cybernetics, it merited their attention:

> Can we [i.e. postmodernists, and feminists in particular] think the subtlety of neurology and cognition on their own terms? Can we read the internal machinations of traditional empiricism in ways that do not return us to the routinized accusations [from the counter-culture, against biological science] of essentialism, reductionism, and political stasis? Specifically, does connectionism offer a political reading of psyche, cognition, and biology not despite its neurocomputational inclinations, but *because* of them? (Wilson 1998: 14)

Her answer was *Yes!* Connectionism, she argued (and she might have added "epigenesis"), offered feminists a way to accept scientific findings about the body without being trapped in a simple-minded biological determinism. On the contrary, it attributed "a fundamental mobility" to the mind/brain (p. 203).

A-Life, in particular, drew interest from some previously hostile sources. The feminist Sarah Kember approvingly described A-Life as "a discipline which developed precisely at the end of the cold war and which rejected the militarist top-down command and control and the masculinist instrumental principles of AI" (Kember 2003, p. vii). Explicitly contrasting cultural practices and self-images influenced by A-Life with "the previous race of cold-war cyborgs" (i.e. GOFAI-based models), she declared: "Posthuman identity, informed by the discourses of artificial life, centres symbolically on the humanization of HAL . . ." (p. 116)—where the emotionless HAL of *2001: A Space Odyssey* was said to exemplify the "failure" of the AI project. (Whether AI, or even GOFAI, has indeed failed is another question: see 13.vii.b.)

Kember also said that A-Life is "a cultural discourse [describing] posthuman life", where

> The posthuman is cyborgian in the sense of its enmeshment, at all levels of materiality and metaphor, with information, communication and biotechnologies and with other non-human actors. (Kember 2003, p. vii)

The pervasive "information" and "communication" she had in mind included telecommunications in general, but especially the Internet. And the "other non-human actors" ranged from semi-autonomous software agents (13.iii.d), through robot surgeons and automated mechanics (13.vi.b), to computerized companions (13.vi.d) and humans-as-avatars (13.vi.e).

But Kember wasn't attacking this millennial form of man–machine identity. Unlike her predecessor Haraway, whose critique of GOFAI-based "cyborg" culture had *predated* the rise of A-Life, she didn't see the post-human cyborg as a largely destructive "product of cold war AI". On the contrary, she saw the cultural legacy of situated AI and A-Life as liberating, in its stress on autonomy and emergent organization.

She even believed that it offered "the resolution of the science wars", because it enabled one "to become independent of the distinction between nature and culture which forms [their] 'epistem-onto-logical' ground" (p. 216). On that view, the battles raging around the Legend were fated not to be won or lost, but to die away. (They haven't died yet: see Blackburn 2005.)

Philosophically, the flight from centralization and the abstract was supported not only by feminism but also by the phenomenologists' notion of "situated" intelligence and "embodiment" (16.vii.a). However, it seeped into the intellectual air being breathed by people who'd never read a word of phenomenology—and who had scant sympathy for feminism.

Even US army generals were affected, despite their scorn for the explicit pronouncements of the counter-culture (P. N. Edwards 1996: 72, 111). They protested against the centralization brought about by computer-based approaches to military matters, as a result of which officers on the ground were, literally, losing control. Their strategic, and sometimes tactical, decisions were pre-empted by game-theoretic simulations, and their logistic decisions taken over by cost–benefit analyses devised by statisticians. Besides threatening their status and self-esteem, this Pentagon-driven trend favoured formal theory over hands-on application. That is, it substituted often unrealistic abstractions for discriminating responses to specific situations in the real world. (What's more, official reports of what actually happened in the Vietnam war were hugely unreliable as a result: P. N. Edwards 1996: 137–40.)

As for the counter-culture's attitude to computer technology, that somersaulted too. Roszak's complaints about "distortive assumptions" had been explicitly aimed at Norbert Wiener, despite the cyberneticist's attempts to humanize his approach (Wiener 1950). And with the advance of computing—and digital computers—in the late 1960s and 1970s, which depended crucially on military funding (11.i), counter-culturalists had become even more disaffected. By the early 1980s, however, these cultural doomsayers were being explicitly countered by people whose views vied with them for popularity.

Alvin Toffler, for instance, published a widely serialized best-seller, which achieved twelve printings and twelve translations within two years, that challenged "the chic pessimism that is so prevalent today" (1980: 2). Remarking that "Despair—salable and self-indulgent—has dominated the culture for a decade or more", he argued at length that this attitude was "unwarranted". The reason for optimism, he said, lay largely in the computer-based technologies, including new forms of communication, to which Roszak had been so hostile.

As we've seen, certain applications of personal computers were viewed by late 1980s counter-culturalists as philosophically liberating. The terminology of "emergence", and/or "life", made New Age souls—and journalists—even more receptive (15.x.a). Visual, performance, and installation artists, for instance, were quick to respond (Whitelaw 2004).

Granted, computers remained something of an embarrassment—and they still are. One art critic recently defended his approval of interactive art like this:

> [These new aesthetic theories] propose personal and social growth through technically mediated, collaborative interaction. They can be interpreted as aesthetic models for reordering cultural values and recreating the world. As much as these theories depend on the same technologies that support global capitalism, they stand in stark contrast to the profit-motivated logic that increasingly transforms the complexion of social relations and cultural identity into a mirror-reflection of base economic principles. (E. A. Shanker 2003: 6)

In short, the computer hasn't been all-conquering. Theories resting on computer technologies still arouse philosophical/sociopolitical suspicion in certain quarters.

So far, we've focused on how the counter-culture responded to cognitive science. But what about influences in the opposite direction?

The theoretical developments mentioned above weren't primarily caused by the sociopolitical values of the counter-culture. Indeed, the intellectual seeds had been sown long before its rise, and nurtured out of public view for twenty years (4.viii, 12.i–ii and iv–v, and 15.iii–vi). However, some of the relevant concepts may have occurred to the scientists concerned partly as a result of it. For new ideas are often hugely overdetermined, in the sense that *many* influences and associations play a part in their generation (Boden 1990*a*: 186–98, 244–8).

For example, consider what Stephen Toulmin has called the postmodernist "revaluation of the concrete", and the closely associated distaste for top-down hierarchical control—even in hard-headed business management (Toulmin 1999, ch. 5). This was reflected in a number of ways within cognitive science:

* by connectionist work on distributed control and bottom-up emergence in anthropology, AI, and psychology (8.iii and 12);
* by GOFAI work on 'agents' (13.iii.d–e), interactive interfaces (13.v–vi), and even AI programming languages (10.vi.b);
* by A-Life and situated robotics (13.iii.b–d, 15.vii and viii.a–b);
* and by some aspects of philosophy (12.x).
* It was explicitly endorsed by Papert, in relation to various late-century trends in psychology (Turkle and Papert 1990).
* Arguably, it was even indicated by the new willingness of laboratory neurophysiologists to hobnob with practising physicians (see 14.i.a).

Irrespective of whether these late-century scientific ideas were part-caused by sociopolitical influences, such influences helped determine whether they met a receptive audience. They were more readily accepted, even *within* the field, because of this particular Zeitgeist.

In intellectual history in general, remarks of that type are common. Indeed, the pioneering, and punctilious, historian of psychology Edwin Boring (1957) often cited the Zeitgeist in his work. On the very first page of his *magnum opus*, he said:

> Discovery and its acceptance are [limited] by the habits of thought that pertain to the culture of any region and period, that is to say, by the *Zeitgeist*: an idea too strange or preposterous to be thought in one period of western civilization may be readily accepted as true only a century or two later. Slow change is the rule—at least for the basic ideas. On the other hand, the more superficial fashions as to what is important, what is worth doing and talking about, change much more rapidly . . . (Boring 1957: 3)

Later chapters featured the Zeitgeist too (his index lists a dozen entries, some pointing to several pages).

Boring is sometimes mocked as a result, by critics who feel that he wheeled in this Hegelian notion almost as an extra character in his script. (For instance, he spoke of individual scientists becoming "the means by which the *Zeitgeist* prevails": p. 23.) Perhaps he was tempted by the animistic flavour of the term, which literally means "the spirit of the times", concepts/assumptions that inform virtually all aspects of a given culture. But whichever word one chooses to use, the point is that—as a pervasive intellectual background, informing virtually all areas of life—the Zeitgeist is a real phenomenon. In the second half of the twentieth century, then, counter-cultural ideas and values had a significant effect on the history of cognitive science.

In sum, although my narrative of cognitive science is predominantly 'internalist', dealing with the data and theoretical ideas as such, it's partly 'externalist' too. We'll see repeatedly that social and personal factors played a role within the scientific community (and a fortiori in the public media: P. N. Edwards 1996). Sometimes, these influences were explicitly acknowledged by the scientists concerned (e.g. Resnick 1994: 6–19). More often, they were implicit—betrayed by the choice of theoretical terminology and/or illustrative metaphor (e.g. AI workers' descriptions of heterarchical programming: see 10.iv.a). But in either case, they were there. Since the Legend—though highly attractive to many scientists—is false, that's only to be expected.

e. Hardly hero worship

The fourth warning concerns the Romantic myth of the creative prodigy. The thinkers I've chosen to discuss weren't intellectual heroes solely responsible for the ideas I attribute to them. Sometimes, it's even doubtful whether they *could* have had the idea independently. Herbert Simon, for instance, has said as much about the three-man origin of what's often (wrongly: 10.i.b) called the first AI program:

> We [three] were in closest communication during the whole period, through long association had developed an extraordinary capacity to communicate even our subtleties to each other, and *the whole product must be regarded as joint and inseparable. I am firmly convinced that none of us alone had much chance of accomplishing [it]*. (quoted in McCorduck 1979: 139; italics added)

Quite apart from other *individuals*, the writers discussed in this narrative were influenced and/or encouraged by the social context at the time. Indeed, one historian of science has said that the "great man" view of history might well be replaced by a "great opportunities" view, "with the emphasis on the socially given possibilities rather than on the people who exploit them" (Fleck 1982: 217). In AI, for instance, socially driven changes in funding policies have offered encouragement and discouragement alike (Chapters 11.i and v.b, and 12.iii.e and vii.b).

Occasionally, of course, people's self-serving rhetoric suggests the contrary. I've not come across any example so extreme as James Watson's *The Double Helix*, whose unfairness to several other DNA pioneers, including Rosalind Franklin, led Harvard University Press to drop its plans to publish it (Maddox 2002: 312). But a few famous names in cognitive science are guilty to a lesser degree (see 14.v.d). Certainly, some individuals were exceptionally fertile thinkers. Turing, McCulloch, von Neumann, Marr, and Simon are examples—which is why each of them features at length in *several* chapters. But even geniuses aren't lone geniuses.

An important idea is rarely, if ever, due to only one mind. Indeed, it often arises near-simultaneously in several. Given that creativity involves either novel associations of familiar (and shared) ideas, or the exploration and transformation of structured conceptual spaces acquired from one's culture, this is only to be expected (Boden 1990*a*, 1994*b*).

It's made even more likely by the fact that science has been an increasingly communal enterprise since the mid-seventeenth century (2.ii.b–c). Today, that's often flagged by multi-authorship: most of the scientific papers cited in the References have more than one author (one lists twenty-five). But even the most prolific credit listings are likely to omit the names of people who were, in fact, part of the communicative network that made the discovery possible.

One of the most important techniques in connectionist AI, for instance—namely, back propagation—was independently discovered by at least four people, and prefigured by several others (12.vi.d). And the group who actually got the credit for it were just that: a *group*, who collaborated for several years to improve one member's initial idea before publishing it.

Sometimes, this has bizarre, even comic, consequences. The Nobel Prize committee, when considering Ivan Pavlov in 1903, were much discomfited by his frequent declarations in his *Lectures on the Work of the Main Digestive Glands* (1897/1902) that his discovery of the conditioned reflex was "the deed of the entire laboratory". It's clear from the discussions recorded in the Nobel archives (Todes 2001) that Pavlov's modesty nearly prevented his winning the prize.

But perhaps "modesty" is the wrong word here? For Pavlov was (rightly) proud of his role as a pioneering—and visionary—laboratory manager. The more that scientific research depends on complex equipment and/or multidisciplinary cooperation, the more important this role becomes. Indeed, J. Robert Oppenheimer became world-famous as the scientific manager of the Manhattan Project, not as its leading researcher. Of course, he had enough scientific knowledge and imagination to have a good nose for new ideas—even (when heading the Princeton Institute after the war) some in the nascent cognitive psychology (Bruner 1983: 96, 121). So did Sydney Brenner. When Brenner was director of the Molecular Biology Unit in Cambridge, he offered an unused cubbyhole to the young Marr—who had scant interest in molecular biology, but was exploring highly abstract ideas about the brain (see 14.v.b).

In that particular case, there's no ambiguity: Brenner provided the space, not the intellectual content. In general, however, assigning individual responsibility for creative ideas isn't straightforward. And the more that people are interacting, the more this is true.

(Discovery and invention are in much the same boat, so far as group influences are concerned. Seymour Cray, the charismatic inventor of the supercomputer, couldn't have designed 'his' machine without a rich network of technical and social relationships—even including his child's willingness to accept help with her algebra from invisible "elves", working their magic long after her bedtime: MacKenzie and Elzen 1991.)

This fact lies behind the many horror stories about supervisors stealing their research students' ideas. No names, no pack-drill—except to pay tribute to the psychologist Edward Tolman. He opened his major book by thanking his research students not merely for doing most of the experimental work, but for having many of the theoretical ideas—"which ideas I have often no doubt quite shamelessly appropriated as if they were my own" (Tolman 1932, preface). To be sure, they might not have had them without Tolman's prompting. And they might not have been able to develop them as coherently as he did. Nevertheless, the ideas recounted in Tolman's book weren't just *Tolman's*. His own view was that "If it had not been for those students, this book could not have been written."

Finally, people may be unable to recall just who said what to whom—and even just who wrote what down. For instance, George Mandler was named in two different reports of a 1959 conference on language learning as the author of a provocative "manifesto" attacking both behaviourist and early-cognitive approaches (Mandler 2002*a*: 348, 350). (Specifically, it criticized the "glib invocation of 'schemas,' structures,' and 'organization' " and the "mere postulation" of new mechanisms and processes.) Mandler now—over forty years later—admits to being *one of three* initiators, and "probably" one of three authors. However, another member of the trio says that he himself didn't take part in the actual writing (which took place one evening), even though he'd contributed some of the ideas.

This isn't a case, for once, of an unpleasant priority dispute: the person who was given the public credit is loath to take it undeserved. Rather, the people concerned simply can't remember.

And what if they could? Even then, their memories couldn't necessarily be taken at face value. For as Mandler points out (2002*a*: 348), memory is construction, not recall (see Chapter 5.ii.b). If—as is usual—they were motivated to *claim* priority, instead of shrugging it off, the constructive process might have been biased accordingly. The same applies, of course, to all the other personal recollections quoted in this history.

f. Discovering discoveries

The next three warnings concern judgements of originality—of ideas, as well as people. The attribution of an original idea to one person or group rather than another, and even its recognition as important, or as a 'discovery', are complex social processes (Brannigan 1981). These judgements aren't subject to cut and dried criteria. They involve social negotiation and rivalry, as well as historical enquiry and theoretical argument. Let's consider *discovery* first.

Discovery is a highly loaded term (Sturm and Gigerenzer 2006). An idea deemed by some people as a discovery may not be so regarded by others—even by those whom one might expect to appreciate it.

A famous example is Charles Darwin's paper on natural selection, which—alongside one by Alfred Wallace on the same topic—was read at the Linnaean Society on 1 July 1858. (Whether this counts as 'simultaneous' discovery is doubtful: Darwin had been working on this idea for many years before Wallace came up with it; however, both had been inspired by Thomas Malthus's *Essay on the Principles of Population*.) This first public presentation of the theory of evolution failed to impress the Linnaean's president, Thomas Bell. His Presidential Address some ten months later declared:

> The year which has passed has not, indeed, been marked by any of those striking discoveries which at once revolutionize, so to speak, the department of science on which they bear.

Only those little words "at once" can save Bell, today, from ridicule. As Janet Browne (2002: 42) has remarked, his verdict, though "accurate enough in the short term", has become known as "one of the most unfortunate misjudgments in the history of science".

Some cognitive scientists—though by no means all—would say much the same of Minsky and Papert's unrelenting dismissal of connectionist AI (Chapter 12.iii). And, almost exactly 100 years after Bell's misjudgement in London, a similar blindness occurred at MIT (C. G. Gross 2002: 85). In 1959 Jerome Lettvin, the lead author of what would become perhaps the most famous paper in neuroscience (see 14.iii.a), wasn't invited to a two-week local seminar closely related to his (MIT-based) research. In the event, a visitor from England (Horace Barlow) arranged an invitation for him, and a second—much less famous—paper by Lettvin's team was tagged on at the end of the official Proceedings (Lettvin *et al*. 1961).

Sometimes, someone makes a discovery they don't count as a discovery—not because they don't realize its interest (although this happens too), but because they can't explain it. Lettvin still hasn't published a remarkable finding of the late 1950s, concerning a discrimination made by the frog's retina which seems to be far too complex for a retina to make (see 14.iv.a). Because it's a mystery in theoretical terms, Lettvin has never reported it officially. In his eyes, then, he's noticed something but discovered nothing.

In other cases, an idea is used to great effect in a specific context, but without anyone's recognizing its wider implications. One example is the Watt governor (Chapter 4.v.a). This was copied in countless machines of the steam age, but it was seen as a mechanical trick not a theoretical principle. Its importance as an example of a general type of control mechanism wasn't recognized for ninety years—and even so, it wasn't fully appreciated for another seventy. Indeed, James Watt wasn't the first to use such a mechanism: it's been found in some ancient Greek automata and fourteenth-century clocks. One might say, then, that feedback was *invented* long before it was *discovered*.

Some judgements about what counts as a discovery are grounded in explicitly heroic assumptions. So people may say: "If so-and-so thought of it, it must be important", or "If so-and-so was involved, he must have been the leader" (very rarely "she", of course). Sometimes they don't even realize that they're doing this: the name of the Nobel prizewinner Simon was often wrongly put first in references to papers co-authored by his younger colleague Newell (see Chapter 6.iii.b).

By contrast, some individuals are treated as anti-heroes. One example is the French mathematician Louis de Branges, recently described by a reviewer as personally "cranky", but "not a crank" (Sabbagh 2004). His latest work, in which he claims to have solved a famous mathematical problem (the Riemann hypothesis), is being systematically ignored by his peers, and science journalists are being told not to bother with it—by people who themselves haven't actually read it. (Apparently, it is fiendishly difficult even for professional mathematicians, and would require "a team" of experts working for at least six months.) De Branges is known to have done fine work in the past, but he's very unpopular (there's some suggestion that he may have Asperger's syndrome: see Chapter 7.vi.f). In short:

> It may be that a possible solution of one of the most important problems in mathematics is never investigated because no one likes the solution's author . . . The entire mathematical profession is turning its back on what could be the most important development in the last hundred years of mathematics. (Sabbagh 2004)

I can't think of such an extreme case in cognitive science. But it's certainly true that personal animosities often hinder, and sometimes even prevent, proper consideration of new ideas. For example, AI people in the late 1960s failed to take proper account of Dreyfus's criticisms, because they resented his savage attack on them—not to mention his technical ignorance (11.ii.b).

Such heroic/anti-heroic assumptions are often buttressed by social snobberies of various kinds. These include the superior trust accorded to the word of a "gentleman" (Shapin 1994), which was crucial to the emergence of scientific communities in the seventeenth century (Chapter 2.ii.b).

On the negative side, they include suspicion of uneducated 'country bumpkins' (such as the champion of the neurone theory, Santiago Ramón y Cajal: 2.viii.c), and systematic undervaluing of the contribution of technicians (Shapin 1989; Schaffer 1994). For example, Richard Gregory's influential work on visual illusions (6.ii.d) owed much to the ingenuity of his technician Stephen Salter (later, a professor of engineering who invented an ingenious way of harnessing wave power). Gregory is generous enough to acknowledge this, in print as well as conversation. Many others wouldn't.

Group loyalties enter the picture, too. Judgements of originality and/or value can be strongly influenced, for instance, by chauvinistic nationalism. In his inaugural address in January 1996, President Clinton called the electronic computer an American invention—to the chagrin of compatriots of Turing, Max Newman, Thomas Flowers, Frederick Williams, and Maurice Wilkes (3.v.b–d).

Such judgements can be skewed also by the 'not-invented-here' syndrome. This systematically distorts the reminiscences and the bibliographies of workers in at least one leading AI laboratory, and at least one leading department of linguistics (Chapter 9.viii.a and ix.a).

It even prevented proper recognition being given to the only working AI program to be presented in the final days of the famous Dartmouth Summer School in 1956 (Chapters 6.iv.b and 10.i.b). Although it electrified a few people present there, the program wasn't taken up as the paradigm for AI. Far from it. One of the originators could afford to be 'philosophical' about this years afterwards, but it had rankled at the time:

> [The new field of AI] was going off into different directions. They [i.e. Minsky and John McCarthy] didn't want to hear from us, and we sure didn't want to hear from them: we had something to *show* them! . . . In a way, it was ironic because we already had done the first example of what they were after; and second, they didn't pay much attention to it. But that's not unusual. The "Not Invented Here" sign is up almost everywhere, you know. (H. A. Simon, interview in Crevier 1993: 49)

As for *which* groups are involved in evaluating a discovery, that can rest largely on chance. Frank Rosenblatt's work on "perceptrons" became very widely known, very quickly, because he'd been communicating with physicists as well as psychologists (12.ii.e). This was more accidental than intellectual. He'd been working alongside physicists (in his university's Aeronautical Laboratory) because he'd been borrowing their computer: the psychologists didn't have one.

An idea may be hailed as a discovery largely because the context in which it's put forward makes it highly significant. For example, Goethe is commonly credited with having discovered a particular similarity between the bones of the rabbit's jaw and ours. In fact, someone else had noticed it before him. Goethe, however, placed this anatomical fact in the context of an ambitious philosophy of the unity of nature. The rabbit's jawbone was of interest because he related *all* vertebrate skulls to a single archetype, and because he saw morphological archetypes in the structure of flowers as well as skulls (2.vi.e).

g. So what's new?

To view something as a discovery is to see it both as *valuable* and as *new*. The Goethe example (above) shows that an idea may be more or less valued depending on its intellectual context. Judgement, even negotiation, must enter in. But what about the newness? One might think that novelty is a more cut-and-dried matter. In fact, however, it's a minefield for the unwary.

There are three very different reasons for this. One is obvious, even boring: namely, unavoidable ignorance. No one can be *sure* of knowing about every previous thought that's relevant to a given topic, nor even of having read everything that's been published about it (especially if some of the texts exist only in a language one doesn't read).

Moreover, publication is sometimes long delayed, and/or long unread, so that later work is mistakenly believed to have been the pioneer. One example is Paul Werbos's algorithm defining what was later called "backprop" (12.iv.d). Not only did this lie buried for many years in a computer manual (unread by psychologists), but wider publication was at first deliberately suppressed by a governmental committee, because other results in the same report were politically embarrassing. It was only later that Werbos's priority could be recognized. Another example is Konrad Zuse's work on computers, which remained unpublished for many years—and wasn't translated from the German for several years after that (3.v.a, 10.v.f). In short, judgements of historical novelty can only be provisional. (Maybe they should carry a government health warning?)

The second difficulty in assigning priority is more interesting: no creative idea is entirely new. It's either a novel combination of familiar ideas, or an exploration or transformation of a culturally accepted style of thought (Boden 1990*a*, chs. 3–5). Even in the latter case, where the new idea can be so surprising as to seem *impossible*, there will be some intelligible relation with the previous way of thinking. It may take a long time, and considerable persuasion, for someone/everyone to recognize this in a particular instance (such as the holographic theory of memory, or the travelling-depolarization theory of nervous conduction: 12.v.c and 2.viii.e). But the point stands, nevertheless. It follows that there are differing *degrees* and varying *types* of novelty, never absolute newness.

It's the third reason which is especially relevant here, however. Namely, people are human—often, all too human. Priority claims can be grounded in various kinds of moral frailty: from deliberate deception, through uncritical self-deception, to lazy (i.e. avoidable) ignorance. Instances of each of these can be found in cognitive science.

For a start, there's the unfortunate habit of representing other people's work as one's own. Occasionally, this boils down to outright *theft*. Two examples, both utterly trivial in the grand scheme of things but pretty annoying to me: Many years ago, I was sent a book for review that was 75 per cent lifted, largely word-for-word, from parts of one of my own—cited only twice. (I felt unable to point this out, and merely said that the book was "highly derivative"; but another reviewer, Donald Michie, did so—thanks, Donald!) And recently, while googling on the Web, I found some "Detailed Comments" on heterarchy (10.iv.a) comprising pp. 125–42 of the same book—which wasn't even mentioned. (On regoogling in April 2005, I could no longer find this entry; it must have been removed.) The limit of this person's originality was to make a few cosmetic changes, including one—adding the words "I'm afraid" to a critical remark of mine—specifically intended to strengthen the impression that he was the author of the stolen comments.

I don't know of any non-trivial examples quite as shameless as this, although I've been told (in confidence) of several that come very close. But it's pertinent to note that the pseudonymous Father Hacker includes the theft—and judicious renaming—of old ideas as a key item of advice in his spoof 'Guide for the Young AI-Researcher' (*AISB Quarterly* 2003; see Chapter 13.vii.a).

More common than such shameless theft is the deliberate suggestion that one's new ideas are more original, and/or more independent, than they actually are.

An exceptionally dishonourable example was directed against a cognitive scientist—let's call him Dr X—who worked in the hard sciences before turning to matters of the mind. During that period, he'd had idea A about topic B. When he was preparing a report for publication, his colleague Dr Y—who was thinking along the same lines, on idea A′—persuaded him to delay publication until his own thought was more advanced, so that they could co-author an even stronger paper. Dr X agreed, and waited for Dr Y to contact him. In vain: soon afterwards, Dr Y published alone—and later received a Nobel Prize for this work.

Within cognitive science as such, there are a number of cases of people exaggerating, even deliberately misrepresenting, their own originality. Sometimes, the earlier author isn't even mentioned—in which case only evidence from informal conversations (e.g. between the two individuals involved) can support the charge of deliberate plagiarism. Since this evidence is (*a*) unpublished and (*b*) often given in confidence, I haven't specified any such charges in my story: but I'm sorry to say that I could have done.

More commonly, the original author *is* mentioned—but in a misleading way. As one colleague remarked to me: "The art of plagiarism is to make a marginal and inaccurate citation of earlier work, but to give the impression that it is new and yours." For example, Marr (who has been accused several times of exaggerating his priority: 14.v.d) used this ploy. His early work on stereopsis has been described as "a minor variation" of a model published by someone else, who was cited dismissively by Marr in a low-profile footnote (J. A. Anderson and Rosenfeld 1998: 231–2). "Low-profile" not just because it was a footnote, but because it came almost at the end of a long list of footnotes. A generous acknowledgement this was not.

A more general way of exaggerating the novelty of one's own work is to describe it not as an exciting new development within an *existing* field, but as something quite different. Many pioneers mentioned in later chapters were guilty of this, in that they ignored/denied the close links—historical, sociological, methodological, and even philosophical—between their work and earlier forms of cognitive science.

Consider, for instance, a 'historical' paper by the situated roboticist Rodney Brooks (1991*b*). In the opening paragraphs he mentioned "traditional" AI, implying (correctly) that his new approach was part of the AI enterprise as a whole. In the main body of the text, however, he repeatedly used the term "Artificial Intelligence" to mean only symbolic AI—implying that his approach was even more new, even more revolutionary, than it was (cf. 13.iii.b). Other examples of this self-glorifying rhetorical strategy include the frequent attempts to distance connectionism from AI as a whole (Boden 1991), and to exclude A-Life from "cognitive science" because it doesn't share the Cartesian assumptions of the traditionalists (15.viii.b–c).

Even more common than deliberately overplaying one's originality is the *ignorant* suggestion that one's ideas are new. George Miller was bitterly accused by Newell and Simon of not giving them proper recognition as the originators of many ideas in *Plans and the Structure of Behavior* (Chapter 6.iv.c). However, as Miller said later, "they were old familiar ideas; the fact that they had thought of it for themselves didn't mean that nobody ever thought of it before" (1986: 213). In the event, he redrafted the text and added zillions of footnotes "so they would no longer claim that those were their ideas". They had some excuse, since they hadn't been trained as psychologists. Nevertheless, and despite Simon's knowledge of Gestalt research, they were evidencing the shameful ignorance of past work that pervaded American psychology from the 1920s to early 1960s (see Chapter 5.i.b).

A more excusable example concerns logic programming (Chapter 10.v.f). Its first occurrence (so far as is known) was due to Zuse, in 1946. But his ideas weren't taken up by the early computer scientists. Moreover, the description of his Plankalkul language wasn't published until 1972 (nor translated from the German until four years later). So "the early computer scientists", who were based in England and the USA (3.v.b–e), didn't know about them. The people who "pioneered" logic programming in the early 1960s may therefore be forgiven for believing that they were the very first to think along these lines.

Some cases of sincere-but-mistaken claims to originality rest not on ignorance so much as on *failure to recognize* the similarities between one's own ideas and others'. This failure, in retrospect, can be very surprising. Thus Simon recalled in his autobiography that he and his father—an early servo-engineer who'd designed gun-turret controls for battleships in the First World War—didn't appreciate their shared interests until it was almost too late:

> It wasn't until [the end of the Second World War] that I realized that his whole life had been spent in what you might call protocybernetic work, and that it was just a direct ancestor to this whole business. And until the last year or two of his life—he died in 1948—we never had a conversation about this. *He used to tell me about his work, but that was about his work, and I used to tell him about what I was doing, but that was about what I was doing*, and I don't think the thought crossed either of our minds, certainly not until about 1947 or 1948, that these had any relation to each other. And I don't really understand that now. (Simon, quoted in McCorduck 1979: 130; italics added)

Perhaps the explanation was a form of what the Gestalt psychologists had called "functional fixedness" (5.ii.b). Much as someone may be unable to tie two far-separated strings together because they see a nearby pair of pliers only as pliers, not as *a weighty object suitable for use as a pendulum bob*, so Simon (and his father) apparently thought in terms of "his work" and "my work"—labels which prevented them from noticing the conceptual links. Someone with an emotional and/or professional investment in classifying an idea as "my discovery" may be especially likely to fall victim to this type of blindness.

Failing to recognize the similarity can depend on failing to be consciously aware of the other person's idea. Paul McCartney was accused (in July 2003) of having drawn the superb melody, and some of the words, for *Yesterday* from a song released when he was only 11 years old. (The older version was played on BBC Radio, and the likeness was striking.) McCartney had already reported that he'd been so surprised at waking up with the tune (but no lyrics) "all there" that he'd thought at the time that something like this might have happened. He'd even asked his friends and fellow Beatles if they'd ever heard the tune before, but no one had.

This case of unrecognized memory wasn't identified as such until some fifty years after the original memory was formed. So priority claims, however sincere, can only be provisional. For instance, the neuroscientist Michael Arbib sincerely believed that he'd thought up the name *Rana computatrix* for his computerized frog all by himself, but realized years later that he'd been inspired by a similar name used by someone else (see 14.vii.c). Ideas about names for frogs, computerized or not, are of course relatively trivial. But who can know how many analogous cases concern ideas much more significant than that?

Sometimes, people deliberately suggest that they've done relevant previous work when in fact they haven't. The Nobel prizewinner David Hubel has confessed to a deception of this kind:

> [Torstein Wiesel and I had gone to a lecture by Vernon Mountcastle in the late 1950s] in which he had amazed us by reporting on the results of recording from some 900 somatosensory cells, for those days an astronomic number. We knew we could never catch up, *so we catapulted ourselves to respectability* by calling our first cell No. 3000 and numbering subsequent ones from there. When Vernon visited our circus tent we were in the middle of a three-unit recording, cells number 3007, 3008 and 3009. We made sure that we mentioned their identification numbers. (Hubel 1982: 516; italics added)

This was just a youthful prank, of course. But less innocent examples sometimes appear in print. They ride on the fact mentioned above, that by and large scientists trust each other's empirical reports even if they disagree on their theoretical interpretation. ("By and large", because in important cases the reported experiments must be replicated by someone else.)

Another prank, confessed years after it happened, was meant to suggest not just originality but effortless superiority:

> Alan Kay, Marvin Minsky, and I got together and did some back-of-the-envelope calculations—we actually killed about five minutes to find an envelope so we could later say we did back-of-the-envelope calculations—on how much knowledge would be required [for an AI system embodying common sense: see 10.viii.c] and how much time it would take. That was a million frames over ten years. (D. B. Lenat, interviewed in Shasha and Lazere 1995: 233)

Pretty harmless, admittedly . . . but some attempts to influence people's judgement by providing misleading information are more blameworthy than this one.

Even if someone honestly cites inspiration by, or similarity to, a previous worker, it's often not straightforward to decide who "discovered" the idea—because it's not clear *what*, exactly, the relevant "new" idea is.

The development of hypertext is an illustration (Chapters 10.i.h and 13.v). Individuals commonly cited as crucial originators include Vannevar Bush, Douglas Engelbart, Joseph Licklider, Theodor (Ted) Nelson, and Alan Kay. And Licklider, a leading influence on library science, was well aware that librarians had identified some of the core problematic issues long before—if not quite as early as Gabriel Naudé (see

Preface). To understand the history of hypertext, then, one must distinguish carefully between statements of:

* a general conceptual framework;
* abstract organizational principles;
* desirable/conceivable end-results;
* in-principle-possible technical methods;
* feasible computational techniques;
* commercially efficient designs;
* and specific improvements of pre-existing technologies.

Without doing this, one can't say just what was "new" about an individual's (or a research group's) contribution. The same applies, of course, to many other discoveries besides hypertext.

h. Rhetoric and publication

The seventh warning concerns not what ideas people come up with, but how they present them. For the history of science is not only about what people thought, but also about what they were *thought* to have thought. And that, in turn, depends on how—and where—they chose to express them.

The identification of discoveries (both as *new* and as *valuable)* can depend heavily on the discoverer's rhetorical skills, or lack of them. We'll see in Chapter 9.vii.b, for instance, that Chomsky's review of Skinner's *Verbal Behavior* became famous largely because of its sparkling, stinging wit. By contrast, several classic papers in cognitive science—even including *the* classic paper (Chapter 4.iii.f)—were all but ignored on first publication, because of their notational and/or mathematical difficulty.

Arguably, one case in point is the early work of the highly creative cognitive scientist Stephen Grossberg. He was perhaps the first to formulate three ideas that are influential today under the names of other people: Hopfield nets, the Marr–Albus model of the cerebellum, and Kohonen self-organizing maps.

(These things are tricky. Although Kohonen 1982 didn't cite Grossberg directly, he did cite four papers by Christoph von der Malsburg. The earliest of these had clearly acknowledged Grossberg as its prime inspiration: see Chapter 14.vi.b. But without following that particular paper-trail, readers might be unaware of the kinship between Kohonen's ideas and Grossberg's. This is, of course, a general point: the fact that Bloggs didn't cite Squoggins, or even hadn't read Squoggins, *doesn't* mean that he wasn't influenced by Squoggins.)

Grossberg also pioneered many more notions—including back propagation—that are commonly attributed to others, if not actually named after them. As he puts it:

> This has, all too often, been the story of my life. It's tragic really, and it's almost broken my heart several times. (J. A. Anderson and Rosenfeld 1998: 179)

> Trying to live with so many false [priority] claims has been difficult for me at times. If I try to get credit where it is due, then people who want the credit for themselves often mount a disinformation campaign in which they claim that all that I think about is priority. Because I have been a very productive pioneer, who innovated quite a few ideas and models, that can create quite a chorus of disinformation! If I don't try to get credit for my discoveries, then

I am left with the feeling that eventually most of my ideas may become attributed to other people . . . (pp. 186–7)

Grossberg's own diagnosis of his "tragic" life history identifies three causes:

The problem is that, [1] although I would often have an idea first, I usually had it too far ahead of its time. *Or [2] I would develop it too mathematically for most readers*. Most of all, [3] I've had too many ideas for me to be identified with all of them . . . [From the mid-1960s on] many things that I discovered started getting named after other people. (J. A. Anderson and Rosenfeld 1998: 179; italics added)

The key sense in which his work was "too far ahead of its time" was the unfamiliarity of the mathematics. He was talking about brain and behaviour as a complex dynamical system (as he put it: "nonlinear, nonlocal, and nonstationary"—1980: 351), a theoretical approach that didn't become popular in cognitive science until the late 1980s. But it's the second point that's of special interest here: the rhetorical style.

His early work was largely unintelligible even to the few psychologists who took the trouble to read it (see Chapters 12.v.g and 14.vi.a). He combined intellectually demanding (and unfamiliar) mathematics with a host of interdisciplinary details, most of which would be unfamiliar to any individual reader. They were there because he was trying to show the unsuspected theoretical *unity* behind hugely diverse data. His writing was unusually voluminous too: 500 pages for his first-year graduate report (1964), and many long and richly cross-referenced journal articles. Faced with this challenge from a youngster they'd never heard of, most people gave up before reaching the end, if they could summon up the courage to start reading at all.

Scientifically speaking, the weakness was theirs—not his. It's now taken for granted (which it wasn't then) that the problems he was discussing demand a fair degree of mathematical literacy on the researcher's part. Specifically, they require non-linear mathematics. (Broadly speaking, this is a type of mathematics in which a very small change can bring about a much larger, and global, change.) In general, indeed, one can't expect cutting-edge ideas to be easy to understand. Humans being human, however, new scientific claims have to compete for attention. In a cynical mood, one might even say that they have to compete *in the marketplace* for attention. So any rhetorical obstacles put in readers' way will tend to obscure the science, however worthy it may be. Grossberg wasn't easy to read.

By contrast, the group who (wrongly) got the credit for discovering back propagation went to exceptional lengths to describe their work in an intelligible way (12.vi.a). If they didn't emulate the sparkle of Chomsky's famous review, they did achieve clarity. This doesn't count as a *scientific* achievement. But, for good or ill, it does—and did—make a difference in bringing scientific ideas into historical prominence.

Sometimes, the rhetorical obstacles are so great that a primary author gains recognition largely through the efforts of a secondary one. For instance, Richard Montague's theory of semantics was nigh opaque to most cognitive scientists, even including linguists accustomed to dealing with formalism. Without Barbara Partee's (1975) more accessible introduction, his ideas might have been virtually ignored except by his fellow logicians. In fact, they became hugely influential in theoretical linguistics—and underlay the most important challenge to Chomsky (9.ix.c–d).

If one thinks of rhetoric as *presentational* skill, verbal or otherwise, one can even find examples in robotics. We'll see in Chapter 4.vii.b, for instance, that whereas William Grey Walter's mobile "tortoises" caused a sensation in the early 1950s, his—even more interesting—learning machine did not. The problem, apparently, was that this was built as an ugly metal box sitting boringly on a bench. Even though it was sometimes connected (wired) to a mobile robot, its significance wasn't recognized at the time. Another example is the robot arm built by Minsky when he was a boy: no one paid any attention—until he put the sleeve from a flannel shirt on it (Newquist 1994: 62).

The line between rhetorical and theoretical difference, in turn, is often unclear. I implied (above) that the people who independently 'discovered' backprop, Hopfield nets, or the Marr–Albus rule each discovered *the very same thing*. But this is debatable—which is why I said that Grossberg is "arguably" a case in point, and had "perhaps" discovered many ideas later attributed to others.

In general, it's easier to decide such matters when (as in these examples) the idea is expressed mathematically. Two authors might use an identical mathematical equation, or two equations that can be *proved* equivalent. That's not possible for verbally expressed theories, where the many subtleties of interpretation and association of ideas complicate matters hugely. Indeed, showing that two verbal theories or concepts are equivalent is analogous to translating from one natural language to another—a deeply problematic enterprise (9.iv.b and x).

Even with mathematical examples, however, there can be difficulties. A third person, Shun-Ichi Amari, is sometimes said to have "discovered" Hopfield nets (also known as additive nets). But whereas Amari gave only fourteen lines and a mathematical equation, John Hopfield drew out the implications at length, showing how they could be explored in computer simulations. Largely because it was so much easier for cognitive scientists to understand and be inspired by Hopfield, he got the credit. Yet Amari had defined the identical mathematical function—and Hopfield had cited him (see 12.v.f).

As for Grossberg, he'd defined additive nets even earlier, while he was still a student. But he hadn't given a succinct account of them (although he did so later, as we'll see: 12.v.g). Nor had he presented this new idea alone. He'd combined it with many others in describing the processes within a dynamical brain–behaviour system, so that even mathematical psychologists were unable to appreciate it. The scientific value was greater than that of Hopfield's paper (see 14.vi). But the impact, at the time, was less.

Besides the difficulties in getting recognition after a discovery has been published, there's the problem of getting it published in the first place. Strictly, official publication isn't necessary for an idea to be recognized. Chomsky's mimeographed research notes, like Ludwig Wittgenstein's lectures, were read by some experts long before appearing 'in press' (Chapter 9.vi.a). So were Brian Smith's research notes on the nature of computation (16.ix.e). But publication certainly helps.

If Bertrand Russell and Alfred North Whitehead hadn't had some cash to spare in 1910, cognitive science might have arisen much later than it did. For they had to help subsidize the publication of their epochal *Principia Mathematica* (see 4.iii.b). Work on cellular automata, by contrast, might have burgeoned earlier, had von Neumann's lectures not remained unpublished for many years after his death (15.v.b). Occasionally, wider publication is actively prevented. For example, the pioneering work on computing done at Bletchley Park in the Second World War remained an official secret for over thirty years (3.v.d). And the ‘true’ discoverer of backprop was unable to circulate his idea more accessibly because of the political sensitivities of a US government department (12.vi.d).

In addition, the source of publication matters. One could publish an idea in *The Beano*, and its scientific value wouldn’t be affected—but its reception by other scientists, and therefore its place in history, would. In the seventeenth century acceptance, or even respectful consideration, required the word of a gentleman. Today’s equivalent is the peer-reviewed journal. And the perceived relevance and quality of the peers make a difference. In the 1950s it was acceptable, even if not the authors’ preferred choice, for an important neurophysiological paper to be published in the *Proceedings of the Institute of Radio Engineers*, because of the influence of cybernetics (see Chapter 14.iii.a). Today, no biological reader would see it. And the people popularly associated with backprop made a point of publishing this idea simultaneously in the high-prestige *Nature* and in a more accessible, easily affordable, but well-respected source: an MIT Press trade book, aimed at students and professionals in all areas of cognitive science.

Last but not least, it follows from what’s just been said that the editors and reviewers of the professional journals have the power to block, or to censor, the ideas sent into them. This can affect even well-known scientists, if they’re presenting ideas not favoured by the editor concerned. Bruner himself has come up against this problem:

> The human sciences are about human beings in specific situations, and why should I hide that fact? You know, the editor of the *Journal of Experimental Psychology* typically *deletes anything of that sort* from my papers. That’s probably why I don’t write as much for the *Journal of Experimental Psychology* any more. (quoted in Shore 2004: 157; italics added)

And stories are rife about as-yet-unknown writers being spurned by their chosen journals. Indeed, work which eventually won a Nobel Prize was initially rejected by an “elder statesman” reviewing it for the editor of the *Psychological Review* (Chapter 7.iv.f).

i. An explanatory can of worms

A new field of enquiry typically spawns new journals (see 6.v.c and 10.ii.b). It’s easy to assume that this is because there are so many discoveries, of an increasingly detailed nature, that there wouldn’t be room for them in the existing journals—even if the editors happened to be interested in them.

That’s true, to be sure. But it’s also true that the existing editors may reject *the general approach* underlying all the discoveries—in which case, they’ll probably refuse to recognize them *as* discoveries. (Similarly, researchers are usually loath to offer jobs to youngsters who disagree with them: Newell was a refreshing exception, as we’ll see in Chapter 10.v.b.) The problem, then, isn’t a mere lack of space, or even a mere lack of interest, but a (supposed) lack of intellectual respectability. New journals are founded, accordingly, which do regard these new ideas as respectable.

Bruner’s current publication difficulties (mentioned above) illustrate this point. For they’re due less to specific, and unacceptably shocking, ideas on his part than to a general disagreement about what counts as explanation in psychology. Having started out in the 1950s as a straightforward, if highly creative, experimentalist (6.ii.a–c), he’s now prepared also to consider *interpretative*, hermeneutic, accounts (8.ii.a). In a nutshell, this means that he relies on his intuition for "narratives" (a critic might say "gossip"), to understand "human beings in specific situations". But whether *human beings in specific situations* is, as he claims, a proper subject for a theoretical psychology is problematic.

The eighth caveat, then, is that Bruner's little story about his problems with the *Journal of Experimental Psychology* has opened a philosophical can of worms that we'll encounter at various points in later chapters. And there are two species of worm in the can.

On the one hand, it's highly controversial whether theoretical (as opposed to therapeutic) psychology should hope to deal with *particularities*. Some people, such as Bruner (and his long-time Harvard colleague Gordon Allport: 1942, 1946), argue that to understand specific events in individual human lives we need empathetic interpretation, not naturalistic science (Chapter 7.iii.d). Indeed, some philosophers argue that psychological phenomena *in general* (not just particularities) can't, in principle, be explained in scientific terms (14.x.c and 16.vi–viii). To the editors of the *Journal of Experimental Psychology*, such a view is an abomination. Naturally, then, they won't be willing to publish papers expressing it.

On the other hand, it's also disputed whether showing how certain things—either particularities (e.g. Joe Bloggs's saying "It wasn't me!") or general phenomena (e.g. language)—are *possible* really counts as 'explaining' them (7.iii.d). Since that's precisely what the computational approach enables us to do, cognitive scientists have a stake in this abstract philosophical dispute.

You'll have noticed that the FAQs listed in Section i.a weren't about what Joe Bloggs did or didn't do last Saturday, nor about what he'll do next week. Rather, they concern *how it's possible* for him—or anyone else—to do the sorts of things that he does every day of his life. If cognitive science can explain that (and if it really is an 'explanation'), it will have done what it set out to do.

1.iv. Envoi

As Naudé recognized (Preface, preamble), others would have told this tale differently. For as we've seen, people disagree about *just what counts* as cognitive science. They also disagree about what message they'd want to send in writing about it. Instead of relaying (as I've tried to do) the enormous interest and promise of the field, and its necessary interdisciplinarity, some would pen an account of the futile pursuit of a philosophical illusion (Chapters 11.iv.a and 16.vi–viii).

What's more, the many factors mentioned in Section iii make historical attribution within cognitive science a difficult, and delicate, matter. Such judgements can turn out in more than one way.

If one worried too much about all that, however, no history could even be attempted. So here goes. Now that we know what we're talking about, let's begin the story.

2

MAN AS MACHINE: ORIGINS OF THE IDEA

'Machine as man' is an ancient idea, and an ancient technical practice. Ingenious android machines whose movements resembled human behaviour, albeit in highly limited ways, were already being built 2,500 years ago. 'Man as machine' is much more recent.

Admittedly, its birth date can be disputed. Some might even say that the two ideas are equally old. That claim, that 'man as machine' is also an ancient idea, could be argued in two very different ways.

One could say that Plato sowed the seeds of a formalist account of knowledge which later inspired the rationalist philosophical tradition in general, and (symbolic) AI and cognitive science in particular (H. L. Dreyfus 1972). This is true. In that sense, intellectualism goes way back to Plato. But to describe 'man as machine' as a concept favoured by Plato would be anachronistic. It's plausible only in retrospect, only after the association of the idea of formalism with man-made calculating machines and digital computers.

The second argument for the antiquity of 'man as machine' as a guiding idea is stronger. For uncompromisingly materialist, even mechanistic, philosophies—of soul, as well as body—were proclaimed five centuries before Christ. However, these were highly programmatic. They didn't, and couldn't, guide systematic empirical research.

With hindsight, some Presocratic speculations seem remarkably apt. For instance, Democritus (*c.*460–370 BC) suggested that discrimination by taste and smell is possible because atoms (or combinations thereof) of different shapes enter the mouth and nostrils, where they fall into a variety of suitably shaped depressions in the sense organ. In essence, modern science teaches something similar. But our concept of atom is also very dissimilar. And whereas we can add theoretically detailed and empirically tested chapter and verse, Democritus couldn't. Nor did he suggest, as René Descartes was to do some 2,000 years later, that his mechanistic ideas be systematically tested by experiment. Before the rise of modern science, the experimental method wasn't an option.

Accordingly, much as atomic theory in chemistry is normally dated to John Dalton, not to Democritus, so 'man as machine' is best seen as an idea arising four centuries ago (with Descartes), not twenty-five. Indeed, if 'man' denotes the mind as well as the body, we have to pass on to the eighteenth century to see this vision developed as a philosophical theme, and to the late nineteenth to see it explored by scientific research.

Even at that time, talk of *mind* as machine was taken to mean merely that the mind, like the body, works according to scientific principles. Nineteenth-century marvels such as the telegraph or telephone were sometimes used as analogies in describing how the brain works, as earlier technologies had been before them (Fryer 1978; McReynolds 1980). Such comparisons lasted well into the twentieth century, and were still prominent in popular writing in the 1950s. But they were applied to brains, not to minds or thought processes as such.

Today, by contrast, 'man as machine' is usually interpreted more strongly (see Chapter 1.ii). It's now taken to mean that there are scientifically intelligible principles capable of controlling not only human (and animal) bodies and minds, but also *significantly lifelike or mindlike artefacts that we might actually try to build*. Understood in that way—which excludes clocks, and even telephones—the idea is very new. Although some assume that Charles Babbage originated it 160 years ago, it's more securely dated to around the time of the Second World War.

But that's to get ahead of our story: Babbage and his post-war intellectual successors are discussed in Chapters 3 and 4. In this chapter, I outline how *machine as man* was eventually joined by *man as machine*, explaining the remarks about alternative birth dates made above. I sketch how mind was later added to body in the interpretation of this slogan. And I mention some pioneering attempts to study 'man as machine' in science.

These efforts, initially in general physiology and later in neurophysiology and psychophysics too, prefigured ideas that are influential, and sometimes highly controversial, in cognitive science today. Virtually every topic mentioned in this chapter, then, will resurface in a later one. In short, nothing is discussed here simply because it *happened*, but because it's still relevant now.

The first six sections are ordered chronologically. Section i discusses some ancient examples of 'machine as man'. Sections ii–vi sketch the rise of mechanistic science—and especially of 'man as machine'—in the seventeenth and eighteenth centuries, and the neo-Kantian reaction to it.

The remaining sections focus mainly on the nineteenth and early twentieth centuries, and cover distinct themes. Sections vii and viii concentrate on general physiology (including embryology) and neurophysiology, respectively. Section ix describes early logic machines. And Section x turns to scientific psychology: mind as mechanism, but not yet as machine.

2.i. Machine as Man: Early Days

Automata building was hale and healthy in ancient times, but weakened in the Dark Ages. In the late Middle Ages, it rallied. By the early modern period, from about 1500, automata were increasingly common in Europe—and by the seventeenth century, they were among the technological marvels of their time. Only at the end, however, did these changes reflect changes in how scholars were thinking about *human beings*.

a. Ancient automata and Dark Age decline

'Machine as man' was exemplified in ancient Egypt (a 'talking' wooden head of the Jackal God of the Dead now sits in the Louvre), and was a familiar engineering practice in ancient Greece. Aristotle's *De Anima*, in the fourth century BC, described a number of moving statues built by Daedalus. One was a figure of Venus, worked by being filled with mercury, which had to be tethered to prevent it from running away.

By the first century BC this ingenious practice had blossomed further. Hero of Alexandria (roughly AD 10–62) wrote many treatises on "automata", or self-moving things—the word is his coinage. He described machines worked by gears, levers, valves, pistons, and pulleys, mostly powered by water, falling weights, or steam (Drachmann 1963: 19–140). Some of them involved feedback devices, first used by the Egyptians over 300 years earlier.

Some of the most interesting, in engineering terms, bore no relation to the human body: a handheld mechanical calendar of about 87 BC, which computed the solar equinoxes and the phases of the moon, used thirty intermeshing toothed gear wheels like those later used in clocks—and in Babbage's calculating engines, described in Chapter 3.ii–iii (de Solla Price 1975). But many early automata mimicked bodily actions, whether of men or animals.

A few ambitious engineers even attempted both of these within the same piece. For instance, among the constructions listed by Hero was a statue of Hercules with a snake:

> On a pedestal is a small tree, around which is coiled a snake. Nearby stands the Archer Hercules. An apple also lies on the pedestal. If the apple be raised a little by hand Hercules shall then discharge his arrow at the serpent, and the serpent shall hiss. (from Hero's *Pressure Machines and Automatatheatres*, quoted in Klemm 1957: 36)

Yet more lifelike behaviour was seen in Hero's construction of the birds and the owl. When the water from a fountain reached a particular level in a hidden tank (with a syphon mechanism in it), the birds were caused to sing, the owl to turn 'threateningly' towards them, and the 'frightened' birds to fall silent once more.

An automaton even featured in the political machinations of ancient Rome. Automated extravaganzas often formed part of the circuses in the Coloseum, but this device was built for a more specific political purpose. Mark Antony's funeral oration following the assassination of Julius Caesar, and his passionate collapse in front of Caesar's bier, had aroused the masses gathered in the Forum to fever pitch. Then, this happened:

> An unendurable anguish weighed upon the quivering crowd. Their nerves were strained to the breaking point. They seemed ready for anything. And now a vision of horror struck them in all its brutality. From the bier Caesar arose and began to turn around slowly, exposing to their terrified gaze his dreadfully livid face and his twenty-three wounds still bleeding. It was a wax model which Antony had ordered in the greatest secrecy and which automatically moved by means of a special mechanism hidden behind the bed. (G. Walter 1953: ii. 237)

This automaton did as much as Mark Antony's speech to whip up the "collective frenzy" and "wild ecstasy" at the funeral. As Caesar's dead body was burnt, soldiers hurled their swords and insignia into the flames, and women threw in "their jewels and the sacred amulets taken from the necks of their children". The history of Rome was decisively affected as a result.

(Fearing that this story was too good to be true, I tracked down the original sources and asked a classicist friend to translate them from the Latin, as well as asking a medievalist whether she believed the story. It turned out that the automated avatar had been described by Appian, and that simple wax models of the deceased were typically presented at important funerals in Roman, medieval, and Renaissance Europe.)

But being sponsored by Mark Antony, reported by Aristotle, or even analysed by Hero didn't grant respectability. These projects weren't seen, nor (with one qualification remarked below) were they intended, as having any philosophical or scientific interest. Indeed, machine building of any sort was scorned in antiquity by the high-born and the educated, even including some engineers themselves.

Plato expressed the fourth-century (BC) Greek's attitude to engineers, even to one whose skills could save a whole city. He reported Socrates' saying to Callicles (in the *Gorgias* 512bc): "You despise him and his art, and sneeringly call him an engine-maker, and you will not allow your daughter to marry his son, or marry your son to his daughter." This attitude was fully justified, said Plato—not for reasons of social snobbery, but because only a life guided by purely intellectual activity is truly worth living.

Contempt for practical skills lasted into the technologically sophisticated Roman Empire—and through into seventeenth-century England and the British Empire too (see Section ii.b below, and Chapter 3.ii.b). So Seneca (*c*.55/1932, letter xc) dismissed all handicrafts and useful technologies—glass, piped central heating, architecture, and even shorthand—as "the inventions of the meanest slaves". (Little did he know that science would develop in the West, not China, largely because of the invention of transparent glass: Macfarlane and Martin 2002.) Techniques of mining, and domestic tools, he declared, "were invented by some one whose intellect was nimble and keen, not massive and sublime: so was everything else the quest of which involves bowed shoulders and earthward gaze" (ibid.)

Likewise, the first-century historian Plutarch eulogized Archimedes, who 400 years earlier had designed many military engines, including a crane which picked up enemy ships and dashed them against the rocks, and a compound pulley that enabled him to draw a fully laden merchant ship across the beach by a mere hand movement. All very impressive, said Plutarch—but most admirable of all was Archimedes' judgement of this work as unworthy of written record. (This, from a historian!) As he put it:

> [Archimedes] would not deign to leave behind him any commentary or writing on such subjects; but, repudiating as sordid and ignoble the whole trade of engineering, and every sort of art that lends itself to mere use and profit, he placed his whole affection and ambition in those purer speculations where there can be no reference to the vulgar needs of life . . . (from Plutarch's *Lives*, quoted in de Solla Price 1975: 51)

In Europe's Dark Ages, even such "sordid and ignoble" knowledge was largely lost, and automata building went into abeyance. (In China, however, where a mechanical orchestra had been built for the Emperor in the third century BC, similar practices flourished—until they waned in the fifteenth century.)

b. In fashion again

At the time when China's automata were disappearing, automata building had resurfaced with a vengeance in Europe. It did so via the Arabs, who had translated Hero's treatises in the ninth century and who were enthusiastic practitioners of the art. (And of algebra, too: our word comes from the Arabic *al-jabr*: reunion, or connection; and our word algorithm recalls al-Khwarizmi, a ninth-century Arab mathematician.)

For instance, the twelfth-century mechanical engineer Ibn al-Jazari wrote *The Book of Knowledge of Ingenious Mechanical Devices*, which was soon translated into Latin. This discussed the design, manufacture, and assembly of fifty automata powered by hydraulics, pneumatics, and gears. Some were workaday instruments, such as clocks or pumps. But some were androids, including one—which looked like a 5-year-old boy—called the "boon-companion". Described by its inventor in drily meticulous detail, so that it could be understood—and perhaps rebuilt—by others (D. R. Hill 1974: 115–17), its "outside appearance and purpose" was recorded by al-Jazari as follows:

It is a kneeling figure made of jointed copper. He holds a goblet in his right hand with fingers extended along its stem, and in his left hand he holds a waterlily by its stalk. It was one of the customs of the king in those days, when they were drinking, to leave some [of the wine] in the goblet and this, when it had collected, was drunk by a boon-companion designated for that duty. This boon-companion [i.e. the model] is placed in front of the head of the carousal. When he drinks a goblet the steward takes it, pours what is left in it into the boon-companion's goblet, and stands aside. When left by himself he [i.e. the boon-companion] lifts the goblet in his hand until its rim is between his lips [where it stays] for a while. Then he lowers the goblet from his hand and nods his head several times. This happens every time wine is poured into the goblet. His left hand moves and is observed by the head of the carousal until after a while it reaches a certain position. (al-Jazari, trans. D. R. Hill 1974: 115; all phrases in square brackets are Hill's)

At that point, the automaton would do something which his human equivalent would never do—and which few members of the "carousal" would ever forget:

Then the head of the carousal says to someone he wishes to make fun of, and who does not know the purpose of the boon-companion, "So-and-so, take this boon-companion, who drinks wine and hides a secret. Put him on your knee, drink, and give him [wine] to drink." So he takes him without argument, puts him on his knee, drinks and gives him drinks. He does not finish two or three goblets before [the boon-companion] pours on to him all he has drunk since the beginning of the carousal, wetting his clothing. The wine flows beneath him, making him a target for laughter. (ibid.)

But the boon-companion could also be used less mischievously:

This [practical joke: M.A.B.] is appropriate at certain times, but it is more usual for the king, when he knows the boon-companion is about to discharge what he has drunk, to have him carried outside the company and given two or three goblets, so that he discharges what he has drunk. [Then] he is brought back into the company. (ibid.)

News of such machine-as-man delights travelled from Islam to Sicily, then to the court of Frederick the Great, and eventually throughout Europe. The "news" often concerned technicalities as well as gossip. For instance, al-Jazari's writings provided a 1,600-word explanation of just how the boon-companion's hidden syphon worked, and similarly detailed accounts of his other automata. So Western engineers were not only inspired but also instructed. Countless European automata were built as a result.

Medieval automata in Europe included a device built by the Spanish theologian and missionary Ramón Lull (*c*.1234–1316)—and inspired by his experience of Islamic culture in North Africa. This wasn't an android toy to delight the senses, but a practical device to support the mind. That is, it was an early attempt to mechanize logical (non-numerical) reasoning. In his writings on the *Ars Generalis Ultima* or *Ars Magna (Ars Inveniendi Veritatum)*, Lull claimed to have schematized the content of all natural and theological knowledge, and to have formulated rules for reasoning within it. These were, in effect, forerunners of Venn diagrams (M. Gardner 1958: 28–59).

As Lull's first Latin label declared, his system was intended as a truly *general* problem-solver, capable of generating all knowable truths. He believed that it embodied "an Art of thinking which was infallible in all spheres because based on the actual structure of reality, a logic which followed the true patterns of the universe" (Yates 1982: 12). His justification, given in *The Hundred Names of God*, was that: "If understanding followed no rule at all, there would be no good in the understanding nor in the matter understood, and to remain in ignorance would be the greatest good." (This was specifically aimed at the Arab theologian Averroes, who taught that something could be false in philosophy but true in theology—a special case of the intriguing cultural–cognitive phenomenon discussed in Chapter 8.vi.b–e.)

Lull built several machines to embody this knowledge and reasoning. They're remembered today largely because—along with the *Ars Magna* itself—they inspired Gottfried Leibniz to build his calculating machine four centuries later (Section ix.a, below).

They'd also inspired the famous/infamous Cornelius Agrippa (1486–1535), and the equally renowned and controversial John Dee (1527–1607). Whether these two men should be seen as (respectable) "mathematical" scholars or "alchemists", or as (dangerous) "magicians" was a question that exercised Leibniz's near-contemporary Gabriel Naudé (1600–53). As remarked in the Preface (preamble), Naudé favoured the former description.

Jonathan Swift was less impressed, and put his famed talent for mockery into top gear. In *Gulliver's Travels*, he described a professor in Laputa's Lagado Academy who'd built a forty-handled frame that filled the room. The frame contained many squares of wood, linked by slender wires and carrying bits of paper displaying "all the words of their language, in their several moods, tenses, and declensions, but without any order". The order was provided by the professor's forty students, who turned the iron handles at random. Whenever they found three or four words juxtaposed which could form part of a sentence, they told four students assigned as scribes to write the word strings down. The professor's intention, said Swift, was that:

> the most ignorant person at a reasonable charge, and with a little bodily labour, may write books in philosophy, poetry, politics, law, mathematics and theology, without the least assistance from genius or study. (Swift 1726: 227)

(With the benefit of nearly 300 years of hindsight, one might see Swift as a soulmate of Senator Proxmire, who complained bitterly that the US government was funding apparently trivial—but ultimately useful—research on the sexual behaviour of the screw-worm fly: Chapter 6.iv.f.)

Some of Lull's machines were worked by levers or cranks (hence Swift's image of the many iron handles). But the most famous was a set of fourteen concentric discs of metal, wood, and cardboard (M. Gardner 1958). Each disc bore up to sixteen different symbols (denoting different types of knowledge) on different segments. The discs could be individually rotated, and their possible alignments allowed for a huge variety of symbol combinations—in other words, a huge variety of propositions, or 'truths'.

The machine's usefulness (for Lull) included the fact that it showed the dogma of the Trinity to be logically possible, so that Jews and Muslims—who shared the core beliefs of monotheism—might eventually be persuaded to accept it as true. However, his work was condemned by Pope Gregory XI, who saw it as confusing faith with reason. (This was understandable: *no* religion is expressible in strict logical form: see Chapter 8.vi.b–d.) Nevertheless, the Church eventually blessed Lull, who'd also said that faith as well as reason was needed to appreciate the highest Christian beliefs.

Rotating discs, of course, aren't so immediately engaging as android devices. Most of the many medieval examples of machine as man were less austere—and less 'useful'—than Lull's, bearing some visible likeness to human or animal forms and movement. Among these was a talking iron head constructed by Albertus Magnus in the thirteenth century—which was deliberately destroyed by his pupil Thomas Aquinas.

Some four centuries later, one writer said Aquinas had done this "because he thought it the Deuil, whereas indeede it was a meere Mathematical inuention" (quoted in A. Marr forthcoming). Another, namely Naudé, said he'd done it "meerely because he could not endure its excesse of prating" (Naudé 1625/1657: 254). As these deflationary remarks suggest, some seventeenth-century scholars understood automata as legitimate examples of the engineering arts. But others "who are so easily carryed away with the slender assurance of a common opinion" (Naudé 1625/1657: 254) saw them, still, in more threatening terms: not as mathematical wonders, but as dangerous magic. The "common opinion" to which Naudé referred, in his impassioned defence of the automata makers, included many of his literate contemporaries—not just the unlettered masses.

One of the hundreds of self-moving machines that had graced the Renaissance was a mechanical lion built by Leonardo da Vinci. Intriguingly, it combined (contrasted?) life with engineering. The lion "being brought into a large Hall before Francis the first, King of France . . . after he had a while walked vp and downe, stoode still opening his breast, which was all full of Lillies and other flowers of diuers sortes" (Marr forthcoming, n. 66).

In the late sixteenth century, added inspiration came from Hero's writings, which were translated into European languages from 1575 onwards (Boas 1949; Marr forthcoming, nn. 21–41). Although some copies reached the Bodleian soon after publication, they didn't circulate widely in England or France. Indeed, as late as 1648 John Wilkins (who would soon help to found the Royal Society) complained that "discourses [on automata] are for the most part . . . of great price and hardly gotten". And Naudé himself didn't possess any of Hero's works in his library (Marr forthcoming, n. 32). Nevertheless, they'd already been cited by various other authors, and had been highly influential in Italy.

The result was a plethora of "Androides" (Naudé 1625/1657: 254) and animal-like machines in the palaces and public spaces of Europe. (The word "android" was used by Naudé's English translator in the 1650s; but the *OED* dates its coinage to 1728, in the *Chambers's Cyclopaedia*'s description of Albertus Magnus' ill-fated talking head:

see Section ii.c.) By the middle of the seventeenth century, Europe could glory in two artificial armies of 100 men, besides the flesh-and-blood variety—sadly, more numerous. By the eighteenth, it was graced by countless moving ornamental figures and fountains. Some of these even toured the Continent. Visitors to an exhibition held in the Opera House in London's Haymarket in 1742 marvelled at Jacques de Vaucanson's mechanical musicians playing flute, tabor, and pipe, and at his all-too-lifelike digesting duck (see Section iv).

But, but . . . These wonders were focused on finding ingenious tricks to produce observable movements—or, sometimes, sounds: the hissing of snakes, the banging of drums, the blowing of trumpets. There was no attempt to model the mind as well as the body. (Strictly, this remark applies only to the later examples, for the mind–body distinction as we know it didn't exist before the mid-seventeenth century: see Sections ii–iii.)

Indeed, it's not clear that the automata builders were trying to model the body with a view to *understanding* it. With the interesting exception of the gifted engineer Vaucanson (1709–82), discussed in Section iv, even the post-Cartesians were challenging their practical ingenuity rather than their biological curiosity. In terms of the distinction drawn in Chapter 1.ii.b, these were technological, not scientific, enterprises.

The reason was that, prior to the seventeenth century, there was no philosophical tradition encouraging people to think about bodily function or behaviour in detailed mechanistic terms. There was Democritus, of course—but his atomic materialism could be pursued in practice only in the most general terms.

Some automata, even in the early days, may have been intended—like some early models of heavenly bodies—not as trivial toys but as tests of materialism. That is, they may have been artefacts "whose very existence offered tangible proof, more impressive than any theory, that the natural universe of physics and biology was susceptible to mechanistic explanation" (de Solla Price 1964: 9). But if so, they were general existence proofs rather than specific demonstrations of bodily mechanisms. As such, they weren't without interest in a pre-scientific and/or largely anti-materialist culture. However, if one wanted to know how our bodies actually work, more was required.

2.ii. Descartes's Mechanism

The emergence of 'man as machine' as a motivating theme for experimental science was due primarily to Descartes (1596–1650). As we'll see, his approach was *mechanistic* in two different, though closely related, senses:

* On the one hand, he believed that the principles of physics can explain all the properties of material things, including living bodies.
* On the other hand, he often drew explicit analogies between living creatures and man-made machines, seeing these as different in their complexity rather than their fundamental nature.

His approach was also *mechanical*: its principle of activity was the movement of one body on collision with another. But a philosophy can be mechanistic without being mechanical. Indeed, the mechanical philosophy died only a generation later, with Isaac

Newton (1643–1727). For Newton, the key principle of activity wasn't the familiar causation-by-contact but the "occult" force (action-at-a-distance) of gravity.

a. From physics to physiology

Moves towards mechanization (in the first sense) were already happening when Descartes was a young man. For others at the time had similar ideas. Galileo's advances in physics, and his insistence that "the great book of the universe . . . is written in the language of mathematics", had initiated what was later called "the mechanization of the world picture" (Dijksterhuis 1956). And comparable intellectual moves were being made in biology, too.

Even before Galileo (1564–1642), the structure of the human body had been greatly clarified by Andreas Vesalius (1514–64). His anatomical atlas of 1547 drew on careful dissection, and some animal vivisection, to correct many traditional beliefs due to Aristotle and Galen. (Leonardo's still earlier anatomical illustrations were as yet unknown.)

Moreover, the sixteenth-century physician Jean Fernel (1497–1558), whose textbook of physiology—it's his word—was still widely used in Descartes's time, had rejected Aristotle's view that all human bodily movement is informed by the rational soul (Sherrington 1946). He'd pointed out that many movements—such as breathing, shifts of position in sleep, and some movements of the eyes and eyelids—are independent of both will and sensation.

But Fernel *didn't* draw the conclusion that the body is a machine. Even these involuntary movements, and all the actions of animals, he thought to be due to some vital principle informing the material body—namely, the Aristotelian animal soul. The first person to put machine analogies to good effect in physiology was Descartes's contemporary William Harvey (1578–1657), widely regarded as the father of experimental physiology.

Like Vesalius before him, Harvey corrected some ancient anatomical beliefs, notably about the heart and blood vessels. And in relating anatomy to physiological function he took an explicitly mechanistic approach, describing the circulatory system on the analogy of water pipes fed by a pump (W. Harvey 1628, 1649).

Harvey supported his claims by a systematic programme of physiological experiments. His ingenious studies included careful quantitative measurements of blood volume, and close observations of blood flow in many different animals. These included cold-blooded creatures such as bees, wasps, shrimps, slugs, snails, mussels, frogs, newts, and eels . . . whose hearts beat much more slowly than those of mammals.

Significantly, this work attracted violent hostility from medical professors and practitioners. In part, this was mere social snobbery: like engineering, slugs and snails weren't considered fit topics for the attentions of gentlemen (Shapin 1991: 304–12). More to the point, many scholars weren't ready to accept the comparison of human physiology with that of lowly animals, or—Vesalius notwithstanding—to favour observation and experiment over ancient anatomical authorities. One leading opponent mentioned Harvey's work on "slugs, flies, bees, and even squill-fish" and wrote sarcastically: "We congratulate thee upon thy zeal. May God preserve thee in such perspicacious ways"; and he continued, "Dost thou declare, then, that thou knowest what Aristotle did not?" (quoted in Chauvois 1957: 222).

But even Harvey didn't regard the body as merely a machine: he believed that medicine must recognize the influence of the soul as well as the body in explaining disease. To be sure, biologists' talk of "souls" didn't necessarily imply anything supernatural. The souls posited by Aristotle to explain the properties of living things were a series of increasingly powerful organizing principles informing matter. Only the "rational" soul was—perhaps—metaphysically distinct (Matthews 1992; cf. Chapter 14.xi.a). But since these principles couldn't be detailed, it was all too tempting to see the lower degrees of soul as metaphysically mysterious also.

By contrast, Descartes aimed to banish talk of any type of soul from medicine and biology. His own physiological experiments, on the circulation of the blood, were crude by Harvey's standards. But it was he, not Harvey, who—at a time when radical scepticism was rampant, and occultism and magic rife—provided the philosophical rationale for a programme of physiological research that continues today. That's why he, too, is sometimes described as the father of scientific physiology.

His first publication, *Discourse on the Method of Rightly Conducting the Reason and Seeking for Truth in the Sciences* (1637), was a brief essay laying out the principles of his lifetime's work. It contained references to a much longer (scientific) treatise, whose publication he'd suppressed in 1634 on hearing of the Church's condemnation of Galileo; and its main points were elaborated in his later writings. (The quotations below are taken from Parts V and VI of the *Discourse*, unless otherwise noted.) It was written in French, not Latin, to invite those not blinded by the scholastic study of traditional authorities to consider how new knowledge might be discovered.

There, and in other works, he argued that a systematic experimental physiology was philosophically respectable, practically necessary, and even financially feasible. This scientific optimism, as we'll see, was grounded in four Cartesian claims.

The core claim was that the body is a mechanistic system, in the *first* sense distinguished at the opening of this section. That is, it functions according to the laws of physics (for Descartes, mechanics: see above), like everything else in the material world. This, he said, is true of all living things. It's therefore reasonable to expect that we might learn something about human physiology from studying animals, newts not excepted.

Descartes had no qualms, therefore, in suggesting in his *Dioptrics* that a tube of water fixed to the front of the eye could improve someone's sight, since "Vision will occur in the same manner as if Nature had made the eye longer than it is." He even said that the pupil of the biological eye could then be removed entirely, since it would become "not only useless but even deleterious, insofar as it excludes, by its smallness, rays that could otherwise proceed toward the edges of the back of the eye". As a recent scholar has remarked, this would provide "a hybrid, a fusion of machine and organ, superseding the eye God gave us, but no less 'natural' " (Des Chene forthcoming).

No one could actually do that yet, of course. But the point is that—on Descartes's view—'integrated' internal prostheses, and even artificial sense organs, were possible *in principle*. Now, over three centuries later, they exist. For examples of the former, consider plastic heart valves or electronic pacemakers. As for the latter, instead of Descartes's tube of water consider Paul Bach-y-Rita's 'visual' tickling pads, located on the back or on the tongue (Bach-y-Rita 1984, 2002). Even artists have got in on the act: the Australian artist Stelarc deliberately pushes/questions the boundary between man and machine in his extraordinary performances involving specially designed robots 'wired' into his own nervous system (Stelarc 1984, 2002; see Chapter 13.vi.c).

It was this rigorous mechanism which led Descartes, mistakenly, to explain the heart's action in a way very different from Harvey. One of the first to acclaim Harvey's work, he had much to say, in Part V of the *Discourse*, in praise of "the English physician". Besides his many post-mortem dissections (and his frequent visits to abattoirs and gallows), he followed Harvey in vivisecting a few animals—fishes, eels, and a hare—to study the action of their hearts (Lindeboom 1979: 106–22). And he, too, related anatomical structure to physiological function. For example, he explained the different number of membranes, or "little doors", in the mitral and bicuspid valves in terms of the shapes of the heart openings they closed (oval or round, respectively). But he disagreed on a crucial—and philosophically relevant—point.

For Harvey, the heart was a mechanical pump, whose muscular contraction (systole) pushed the blood out into the arteries. For Descartes, it was a heat engine, and diastole was the active moment. The heart, he said, was the hottest part of the body. The expansion of the heart wall, and so the flow of blood out of the heart, was caused by the rising pressure of the venous blood, which will "promptly expand and dilate" on entering the heart ventricles, "as liquids usually do when they are allowed to fall . . . into some very hot vessel".

This (mistaken) theory, you'll notice, relied on purely physical principles, not on some unexplained vital property: the heart's inherent power of muscular contraction. "In supposing the heart to move in the way Harvey describes, we must imagine some faculty or inherent power that causes the movement," said Descartes—and this he was not prepared to do. (It wasn't until very much later, in my own student days, that the contractile power of muscle began to be understood: see Preface, ii, and Section viii.e, below.)

b. Science as cooperation

As a metaphysician, Descartes celebrated the solitary ego. As a scientist, he stressed the need for cooperation. From the vantage point of the twenty-first century, we can appreciate his prescient claim that *collective* empirical study would be necessary to discover the actual mechanism of the material world.

Francis Bacon (1561–1626) had said much the same already. He'd argued in *The Advancement of Learning* (1605) and *The New Organon* (1620) that "natural philosophy" (*science*, in its current meaning, is a late nineteenth-century word) should be built on facts, not theories. And he gave detailed advice on how to do inductive reasoning systematically—and reliably. This covered both the observation of "Nature free and at large (wherein she is left to her own course and does her work her own way)" and also experimentation, wherein "by the art and hand of man [Nature] is forced out of her natural state, and squeezed and moulded".

Significantly, he added that scientists needed not only the real world but also each other. Whereas the medieval scholastics had resembled spiders, weaving fragile webs of words out of their own innards, scientists should emulate the bees, collecting nutriment from the real world *and working on it collectively in the hive*. Shortly before his death, he'd imagined a country with a scientific community called Solomon's House, "dedicated to the study of the works and creatures of God" (Bacon 1624).

Solomon's House, however, was still but a dream. As Lord Chancellor of England, Bacon had had access to the king. He'd tried repeatedly to persuade James I to found a college for experimental science, to encourage learned societies fostering scientific research, and to establish chairs of experimental science at Oxford and Cambridge. But none of those things happened in his lifetime—which ended just before Descartes wrote the *Discourse*.

Descartes was no Baconian. Whereas Bacon had exulted in his vision of scientific fact gathering, Descartes saw this activity as a regrettable practical necessity. His rationalism led him to say that every scientific law follows deductively from the first principles of physics—which, in turn, follow from the nature of God. Ideally, then, science could be conducted from the armchair—and he himself inferred various physical laws (such as the conservation of "motion") a priori.

But unfortunately, this could be done (by human minds) only up to a point. Only God, he said, has the intellectual power to carry out the deductions to the most detailed level; and only God knows which of the many theoretically possible worlds He has freely chosen to create. Human beings therefore have to rely not only on logical deduction but also on empirical observation.

Moreover, for Descartes (unlike Bacon) observation must be theoretically guided. That's why, in discussing the heart, his a priori mechanism had won out over Harvey's careful experiments and his claim to have "publicly shown, that arteries increase in volume because they fill up like bags or leather bottles, and are not filled up because they increase in volume like bellows" (W. Harvey 1628: 13). In the *Discourse*, Descartes made no mention of Harvey's telling observations of blood flow in newts and the like, giving more weight to machine analogies. He declared that "those who do not know the force of mathematical demonstration and are unaccustomed to distinguish true reasons from merely probable reasons" may not realize that the heart action as described by him "follows as necessarily from the disposition of the organs . . . as does that of a clock from the power, the situation, and the form, of its counterpoise and of its wheels".

But he did believe that the painstaking experimental research needed to apply Galilean principles to biology would involve the active cooperation of many people over many years. This (second) claim led him to publish the *Discourse* in order that:

> the best minds would be led to contribute to further progress, each one according to his bent and ability, in the necessary experiments, and would communicate to the public whatever they learnt, so that one man might begin where another left off; and thus, in the combined lifetimes and labours of many, much more progress would be made by all together than any one could make by himself. (Part VI)

This would be feasible, he remarked, if their time was funded by wealthy philanthropists. As for his own work, he closed the *Discourse* with thanks to "those by whose favour I may enjoy my leisure without hindrance", as opposed to "any who may offer me the most honourable position in all the world".

Descartes's hopes (and Bacon's) for the cooperative development of experimental science were soon to be realized. Informal communication networks of scholars—what

Robert Boyle called "the invisible college" (de Mey 1982: 133 ff.)—had been developing in Europe from around 1630. For science to move forward as Descartes had hoped, these invisible colleges would have to be made visible (cf. de Solla Price and Beaver 1966). In the 1660s they were officially chartered by Charles II and Louis XIV as the Royal Society of London and the Académie royale des sciences in Paris (Hunter 1981, 1994).

The two monarchs may have been part-moved by scientific curiosity, but it doubtless helped, in prompting their generosity, that the people on the receiving end of their sponsorship were drawn from the highest levels of society. All the early Fellows and Academicians were "gentlemen", and a goodly few of these were high-born aristocrats. Boyle, for instance, was a son of the Earl of Cork, and brother to a crony of the king (Shapin 1994: 138).

Significantly, this fact made *science*, not just *sponsorship*, possible. Cooperation could readily take place, because people in that social stratum shared a cultural expectation of trustworthiness (Shapin 1994, chs. 3 and 5).

This wasn't mere wishful thinking or complacent self-congratulation. All children naturally develop trust, including discrimination between more and less reliable informants (Harris and Koenig forthcoming). If they didn't, cultural sharing of non-observational knowledge would be impossible. But in certain social groups, being seen as trustworthy is especially important.

In Descartes's time, men of gentle birth faced huge social costs on being judged dishonourable. The code of chivalry had encouraged deadly duels in defence of one's honour, a practice that continued—though much abated—into the nineteenth century. By the seventeenth, the gentry had learnt, as a result, to practise probability.

That is, they'd realized the importance of making it clear when one *wasn't* committing oneself to a fact, but merely expressing a challengeable opinion. Books of gentlemanly etiquette urged them to protect themselves by using expressions such as "almost". Indeed, two popular sayings were: "Almost was never hang'd" and "Almost and very nigh, save many a lie" (Shapin 1994: 114). They were even counselled not to relate astonishing facts which they *knew* to be true, for fear of tarnishing their reputation—a form of self-denial still practised by scientists today (see Chapter 14.iv.b). This distinction, originally developed largely to avoid the social disaster of being branded a liar or incompetent, could be directly transferred to the new scientific discourse of 'empirical data' and 'theoretical speculation'.

In short, the gentleman's culture (and language) of credibility eased the acceptance of other scientists' observational reports. Everyone didn't have to repeat everyone else's experiments, even if they disagreed—politely—on the theory.

Initially, this was true *provided that* the observers were leisured amateurs, with no pecuniary interest in 'observing' one thing rather than another. So the observations of the Royal Society's Robert Hooke, who was officially a waged technician, were often subjected to doubt, or to verification by others, because of his lowly social status. His lack of social standing overshadowed his intellectual brilliance.

("Brilliance" is no exaggeration: L. Jardine 2003. Among many other achievements, it was Hooke who, in a letter to Newton, first suggested the inverse square law of gravitational attraction; and besides originating the eponymous Hooke's Law of elasticity, he also helped in the discovery of what's now called Boyle's Law.—None of that, unfortunately, endeared him to Newton: see Chapter 5, preamble.)

Gradually, these social conventions were being developed into relatively explicit canons of acceptable scientific behaviour, or "epistemological decorum" (Shapin 1994: 193). New conventions concerning how to do experiments, how to report them, and how to handle the scientific conversations provoked by them, were painstakingly established. (Whether it follows, as two historians of seventeenth-century science have argued, that the "facts" and "laws" accepted as a result are mere social constructions, not objective truths, is another matter: see 1.iii.b, and Shapin and Schaffer 1985.)

As a result, Descartes's hope was realized: one man could begin where another left off. Indeed, the Royal Society saw itself as following in Descartes's footsteps—and Bacon's. It took their advice not only on how to make new empirical discoveries (hence its motto, *Nullius in verba*: "On no man's word"), but also on fostering fruitful communication. It was Solomon's House, on London ground.

c. Cartesian cooperation develops

But how was the entry to the House to be widened? In Descartes's time, that wasn't clear. When he expressed the hope that scientists "would communicate to the public whatever they learnt", what he meant was very different from what we'd mean by those words now. It even differed from what would be meant a mere hundred years after him.

In his time, the early seventeenth century, there were only a few philosophical/scientific books, and only a few readers too—experimental philosophy was still very much a minority taste. The books were expensive, and published in the scholar's lingua franca, Latin. (Although the *Discourse* was—shockingly—written in the vernacular, so as to reach outside the charmed circle of scholars, Descartes's other works appeared in Latin as usual.) Moreover, scientific discussion was carried on primarily by individual correspondence, not in public places or in generally accessible print. The Royal Society's *Journal-Book* provided a permanent record, but it wasn't a public document. Descartes's dream of a growing community of cooperating scientists would require significant cultural changes in methods of communication.

Over the eighteenth century, the situation was transformed. Gutenberg finally came into its own, as western Europe—especially, England—became a "print culture" (Porter 2001, ch. 4). England was prominent here because of legal changes associated with the political revolutions of the 1640s and 1688, which not only reduced censorship but also enabled presses to be set up across the land. (Previously, not even advertisements or theatre tickets could be printed outside London, York, or Oxbridge.)

The reading public burgeoned—one might rather say, it *began*—as books became commonplace and Latin took a back seat. And not only books: newspapers and periodicals (including magazines *about books*) were founded, provincial as well as metropolitan, and ephemeral pamphlets abounded. The numbers speak for themselves:

> About 6,000 titles had appeared in England during the 1620s; that number climbed to almost 21,000 during the 1710s, and to over 56,000 by the 1790s. . . . Between 1660 and 1800 over 300,000 separate book and pamphlet titles were published in England, amounting perhaps to 200 million copies all told. (Porter 2001: 73)

These writings in the vulgar tongue reached way beyond the gentry: "Prodigious numbers in inferior or *reduced* situations of life", as one 1790s bookseller put it, could potentially benefit—thanks in part to his own pioneering of cheap remaindering (Porter 2001: 75). A visiting Irish clergyman in 1775 had wondered at seeing workmen reading the gazettes: "a whitesmith in his apron & some of his saws under his arm, came in [to the tavern], sat down and called for his glass of punch and the paper, both of which he used with as much ease as a Lord" (Clifford 1947: 58). And Samuel Johnson had remarked in the late 1770s that "General literature now pervades the nation through all its ranks," virtually every household being "supplied with a closet of knowledge" (Altick 1957: 41).

Most of these publications concerned religious, political, and moral–personal matters—the latter addressed not only in printed sermons but also in the new literary genre of the novel (J. M. Levine 1992). But some dealt with scientific topics, such as the exciting discoveries of Newton (whose *Principia* had appeared in 1687, and *Opticks* in 1704), and the empiricist philosophy of John Locke (1690).

Locke was championed several times by Joseph Addison in 1712, in the pages of his recently founded *Spectator*. And Addison wasn't alone: the demanding volumes of Locke, and especially of Newton—both early members of the Royal Society, and one its long-time President—were repeatedly rewritten by other hands. They even appeared as brief abstracts in the *Young Students' Library*, the *Reader's Digest* of the 1690s. So too did Thomas Sprat's (1667) account of the growing *community* of science, namely the Royal Society.

By the early 1700s, readings from these popularizing books, and Newtonian lectures with ingenious practical demonstrations, were regularly given in London coffee houses. By mid-century, this was happening in provincial cities too (Porter 2001: 142–5). In a word, science was now fashionable (Schaffer 1983). At last, most literate young men (of course!) had the opportunity to learn about it and to be excited by it.

No doubt, the popularizers' writings weren't wholly reliable. (*Plus ça change* . . .) But they whetted the appetite. (And in Sprat's case, they even pointed to the source community *qua* community.) Publications started to appear whose main aim was to educate, even to entice. The first modern English encyclopedia, the *Lexicon Technicum* of 1704, was slanted towards science, which was prominent also in Ephraim Chambers's two-volume *Cyclopaedia, Or an Universal Dictionary of Arts and Sciences* of 1728. This not only gained Chambers election to the Royal Society and burial in Westminster Abbey, but spurred the French Philosophes to embark on the hugely influential *Encyclopédie* (see Chapter 9.iii.d).

Shortly afterwards, books written specially for children appeared for the first time (Porter 2001: 348). Moreover, the publisher of the phenomenally successful *Little Goody Two-Shoes* (sixty-six editions between 1766 and 1850) also produced a popular children's book on science in 1794. Its official title was *The Newtonian System of Philosophy* (and its encouraging subtitle *Adapted to the Capacities of Young Gentlemen and Ladies* [*sic*]), but it was popularly known as *Tom Telescope* (Secord 1985). It depicted a young man of that name, invited by some children into their nursery to entertain them with six lectures/demonstrations on Newtonian physics. *Tom Telescope* was a staple on children's bookshelves for nearly a century.

By the beginning of the twentieth century, such scientific primers were regularly intriguing young minds. We'll see in Section viii.f, for instance, that one of these was read by Alan Turing as a child, who credited it in later life with having opened his eyes to science. Perhaps you, too, remember being drawn in long ago by brightly coloured pages on *Pond Life*, or by *The Story of a Molecule*? (And perhaps your eyes were opened wide by the devilish illustrations in the first undergraduate textbook of cognitive science? See 6.v.b.)

In the late twentieth century, the researchers themselves were sometimes doubling as popularizers, deliberately writing in such a way as to encourage budding scientists, aka graduate students, to work with them (Chapter 12.vi.a). Indeed, 'trade' books (and TV programmes) were increasingly focused on science, infuriating the purists but enlarging the interested public. In 1991, for instance, two influential books on consciousness—one by a science journalist (Nørretranders 1991), the other by a professional cognitive scientist (Dennett 1991)—appeared from non-academic publishers. Both sold widely, arousing interest in the topic and, in the latter case, contributing provocative original ideas (14.x.a).

Meanwhile, scientific journals had become an accepted tool—and trumpet—of professionalism. The Royal Society's pioneering *Proceedings* and *Philosophical Transactions* continued, spanning all areas of science—including cognitive science. And new research areas had spawned new journals, as we'll see, from the *Bulletin of Mathematical Biophysics* in the 1930s to the *Journal of Consciousness Studies* in the 1990s. There was even a revival of science-as-correspondence, as email blossomed. Eventually, important draft manuscripts became openly available on the World Wide Web (Chapter 7.i.e): Solomon's House in cyberspace.

Besides these examples of scientific collegiality, late twentieth-century cooperation had achieved a new form: extensive research collaboration. Multi-authored work had become the scientific norm (and email allowed people based in several different countries to work together). Indeed, one of the papers cited in Chapter 15.x.b lists no fewer than twenty-five names. Those twenty-five collaborators were biochemists and geneticists, working in "wet" A-Life. And a nine-author paper cited in Chapter 10.iv.c involved medical clinicians and AI scientists, collaborating in order to test an "expert system" designed for prescribing antibiotics.

In cognitive science proper, multi-authoring is common—but much less luxuriant, rarely rising above four. Long strings of names typically occur when the use of complex instrumentation means that expert technicians are cooperating with the theoreticians. So, for instance, two seven-author papers cited later concern a brain-imaging study of schizophrenic hallucinations (7.i.h), and recordings from "face-and-gaze" cells in visual cortex (14.iv.b). But sometimes, multi-authorship reflects an especially wide interdisciplinarity (as in Bedau *et al.* 2000, cited in Chapter 15.xi.c).

This research practice makes it even more difficult than usual to attribute a discovery to this person or that one (see 1.iii.e–f). Readers of the "manifesto" for cognitive science (G. A. Miller *et al.* 1960), which listed a triumvirate on the title page, joked that one author thought of it, another wrote it, and the third believed it (6.iv.c and 17.i). They were making an important intellectual point about the book, but the joke was funny partly because such inter-author distinctions are rare. Only the cognoscenti know just which person was mostly responsible for just which aspects of a multi-authored publication. (The journal *Nature* recently tried to rectify this situation, by inviting authors to spell out their individual contributions at the end of the paper: "After a slow start, more and more authors are responding to this situation": *Nature* 2005*a*.) And an eminent name may swamp a lesser one: an ingenious way of reading the minds of very young babies was largely due to a graduate student, but because his co-author was a very famous man his contribution is often forgotten (6.ii.c).

Multi-authorship wasn't what Descartes had had in mind. He'd foreseen the need for collegial communication: scientists would talk to each other, across the table or in letters and (rarely) publications. But then they'd each go off and do their own thing. By the millennium, however, both types of cooperation were in full swing.

To cap it all, jet travel enabled the oldest method of all: talking face to face. Chatting over the coffee table is especially helpful when those holding the coffee cups differ in outlook and expertise, since explanation and enthusiasm can readily be combined. It's no accident, then, that several key interdisciplinary gatherings are recognized as having been crucial for the birthing of cognitive science and of specific areas within it (Chapters 4.v.b, 6.iv.b, 12.v.b and vii, and 15.ix.a). And established conference series, like established journals, helped to keep the conversations going.

In brief, the scientific cooperation that Descartes had dreamed of was thriving within cognitive science. Admittedly, it encompassed more than a little ill-tempered rivalry too (see Chapters 4.viii, 9.viii.a and ix.a, 11.ii.a–e, and 12.iii–vii). But the communication channels were there.

As for the wealthy philanthropists he'd asked for, they were doing their bit. A few were precisely that: wealthy individuals, personally encouraging research they regarded as interesting. Edward Fredkin and Hugh Loebner both offered prizes of $100,000 for specific AI achievements (see Chapter 16.ii.c), and some of the UK research mentioned in Chapter 7.i.f was supported by Gerry Martin's Renaissance Trust.

Others were commercial firms (such as Bolt, Beranek & Newman), private organizations (like the Rockefeller and Alfred P. Sloan Foundations), public charities (like the Imperial Fund for Cancer Research), or privately funded universities. Some of these put millions of dollars into cognitive science, but even so small a sum as $7,500 could have a huge effect (Chapter 6.iv.f).

The most generous sponsor of all was the taxpayer. The Royal Society had long used its private money to sponsor research by non-Fellows: so in 1768 it had funded Lieutenant James Cook's voyage to Tahiti on the *Endeavour*, to observe the transit of Venus on behalf of the astronomers. (Later, they'd elect Cook as a Fellow.) Now, 200 years later, the Society was partly government-funded—and it supported some endeavours in cognitive science. For instance, Christopher Longuet-Higgins and Horace Barlow were both Royal Society research professors for many years (see Chapters 6.iv.e, 12.v.c, and 14.iii.b); and several relevant symposia were sponsored by the Society—and by its 'twin', the British Academy (e.g. Longuet-Higgins *et al.* 1981; Boden *et al.* 1994; Parker *et al.* 2002).

Other tax-funded work was paid for by governmental departments—notably, the USA's Department of Defense, or DOD (Chapter 10.ii.a, 11.i, and 12.vii.b). Although much of the DOD money was targeted on specific applications, or on unsolved scientific problems chosen for their practical relevance, some was set aside for speculative, blue-sky, research. And some of that was given with no strings attached: laboratory directors could spend it on whatever they liked, including high-risk maverick projects. This resembled the attitude of (some) aristocratic patrons three centuries earlier. Neither Descartes nor Locke (who was generously supported by Lord Ashley, Earl of Shaftesbury) ever had to submit specific research proposals for vetting by lesser minds.

So, for instance, Paul Armer, director of RAND's computer science when NewFAI was getting off the ground, was told "Here's a bag of money, go off and spend it in the best interests of the Air Force" (interview in McCorduck 1979: 117). Similarly, Joseph Licklider at the DOD's Advanced Research Projects Agency (ARPA) not only funded the first university AI labs but let them decide what to spend the money on. Without that intellectual leeway, the early history of cognitive science would have been very different (Chapters 6.iii.b, 10.ii.a, and 11.i.a–b).

In addition, taxpayers' money came via national universities and research councils, and from centralized research organizations—such as the UK's National Physical Laboratory, France's CNRS (Centre national pour la recherche scientifique), and several of Europe's Max Planck institutes. If one of these agencies reversed its funding policy, this could have huge repercussions on cognitive science research (see 9.x.f, 11.i and iv, and 12.iii.e and vii.b).

We take this state of affairs for granted. So much so, that one paper contains the joking plea: "More research is needed, send money!" (Mayhew 1983: 215). Long before the days of jet aeroplanes or income tax, Descartes couldn't assume that international get-togethers or public funding would be available. He did, however, point out the need.

d. Descartes on animals—

A programme of systematic physiological, including neurophysiological, research was presaged by Descartes's third claim: for animals, the body is all there is. And his repeated descriptions of animals as machines show Descartes to be a "mechanist" in both senses defined at the opening of Section ii.

Descartes didn't merely state that animals are machines. He offered hypotheses about just what sort of machines they might be, and encouraged experimentalists to test them. And he repeatedly stressed the analogy between animate motion and the movements of the marvellous lifelike devices then visible in Europe's public places, some of which were triggered into motion by deliberate or unintentional movements on the spectator's part (in effect, an early form of interactive art: see 13.vi.c). They included the moving statues of Diana and Neptune, nymphs and satyrs, in the spectacular grotto fountains of the royal palace at Saint-Germain.

He first saw these fountains when he was 18 years old, and recovering from a breakdown. One commentator has speculated that "these glistening, fizzing statues in their eerie torchlit world became the surrogate friends of a brilliant intellect unable to cope with people" (Jaynes 1970: 224). However that may be (Descartes as the ancestor nerd?), Descartes later insisted that animals, in effect, are even more marvellous automata: "if there were machines with the organs and appearance of a monkey, or some other irrational animal, we should have no means of telling that they were not altogether of the same nature as those animals".

This applied to animals' behaviour, as well as to their physiology. Descartes made this clear in a letter to the Marquess of Newcastle:

Doubtless when the swallows come in spring, they operate like clocks. The actions of honeybees are of the same nature, and the discipline of cranes in flight, and of apes in fighting, if it is true that they keep discipline. (Descartes 1646: 207)

The nervous system, he insisted, is no less mechanical than the circulation of the blood. It functions by means of "the animal spirits, which resemble a very subtle wind, or rather a flame which is very pure and very vivid, and which [proceed from the brain] through the nerves to the muscles, thereby giving the power of motion to all the members". He even dreamt up a mechanical explanation of why the muscle stops contracting at the right time. When it has reached the appropriate length, he said, it pulls on a tiny thread inside the hollow nerve, which closes the valve in the brain through which the animal spirits were flowing to the muscle. (This may have been the first proposal for a feedback mechanism in the nervous system: cf. Chapter 4.v.c.)

In animals, Descartes wrote, there's nothing more than this. In other words, their behaviour is based in what we now call reflexes (see Section viii, below). Specifically, they have no soul of any kind. There is therefore no reasoning faculty, and no judgement or self-conscious thought behind any animal behaviour. This applies even to what we'd normally describe as action based on perception, as when we say (to take one of his own examples) that the lamb saw/smelt the wolf, and ran away to escape from it.

(Some of Descartes's mechanistically inclined contemporaries were willing to say that animals are machines, but *not* that they lack souls. Alphonsus Borelli (1608–79), for instance, suggested a purely mechanical explanation for how the muscles can "thicken" without acquiring any extra matter, and how they can move the bones to which they're attached. The first question wouldn't be satisfactorily answered until the mid-twentieth century: see viii.e below. But the *source* of movement, said Borelli, lay in the animal's soul.)

e. —but just what did he mean?

Here, we must avoid a subtle misunderstanding whose roots go back 300 years—and for which cognitive scientists in general have recently been castigated (Baker and Morris 1996: 69–100, 124–38).

Descartes is widely believed to have taught that animals have no consciousness, no experiences of vision, hearing, or pain. Indeed, his follower Nicolas Malebranche (1638–1715) explicitly argued that animals can't feel pain. Significantly, however, Malebranche's argument wasn't that animals are machines and therefore can't feel pain. Rather, it was that animals (being machines) aren't moral beings, so can't sin—and pain is God's punishment for sin.

That animals are machines meant, to Descartes, that they have no rational soul—nor any Aristotelian sensitive soul either. (St Thomas Aquinas had said that animals are machines too, but he'd been denying only the rational soul.) It *did not* mean that they weren't "sensitive", that they couldn't "see", "hear", or "smell", or even (*pace* Malebranche) have "pain". Nor did it mean that they couldn't endure the passions of "fear", "hope", and "joy" (Cottingham 1978). However, all these terms were interpreted by Descartes differently from how we understand them—hence my use of scare quotes (cf. Malcolm 1973).

Descartes himself put it like this:

> I should like to stress that I am talking of thought, not of . . . sensation; for . . . I deny sensation to no animal, *in so far as it depends on a bodily organ*. (quoted in Cottingham 1978: 557, italics added)

In other words, by calling animals "sensitive", or "sentient", Descartes seems to have meant what we mean by 'responsive'—which (for us) leaves it open whether or not there is (what we call) conscious experience. (More accurately, I should have said "for *most* of us": see Chapters 14.xi and 16.iv.)

Similarly, by "pain" Descartes meant an abnormal state of the body that is potentially harmful to the creature and which, when (mechanically) discerned by the body's "inner sense", leads it to try to avoid it in various ways. And by "hunger", he meant a physical state, caused by an empty stomach, which (when sensed) prompts food seeking. In all these cases, Descartes insisted that animal "sentience", including animal "passions", can be wholly explained in terms of mechanical processes in the body. He even posited seven different types of sensory nerve, for the five external and two inner senses.

Crucially, Descartes could neither have asserted nor denied what most philosophers today (but not all: see 16.viii) regard as obvious: that many animals have sensory experiences, or "raw feels", or "non-conceptual content", or a subjective feeling of "what it is like to be . . . ". These concepts weren't in his repertoire, and they couldn't have been added by a ten-minute tutorial. A more judgemental way of putting this point is to refer to his "notorious confusion . . . between mere consciousness and reflexive [self-referential] consciousness" (B. A. O. Williams 1978: 286).

For Descartes, there was the responsive ("sensitive") body, and there was the rational soul—the origin of judgement, concepts, free choice, and self-consciousness. Arguably, he posited a third kind of property (though not a third ontological category), namely "confused perceptions", possessed only by humans (Cottingham 1986: 122–32). He believed that human perception is grounded in the sense organs, but also involves judgement. For example, we say we see men walking down the street—but, according to Descartes, all we really *see* is cloaks and hats (or even moving colour patches). Because perception involves judgement, it may be mistaken. He instanced someone's claiming to have a "sensation" of pain in a phantom limb: this isn't sensation but perception, he said, since it involves (erroneous) judgement. Animal 'perception', by contrast, is mere responsiveness, with no admixture of thought or judgement.

Suppose that a time traveller from our own century had asked him whether animals, besides being responsive, also have "raw", mental but non-judgemental, conscious experiences—which seems to be what we now mean by "sensations". They'd have been disappointed. He wouldn't have understood the question.

In short, Descartes didn't—couldn't—*deny* that animals have such experiences. But in saying that they're sensitive, or that they see, hear, smell, and have pains, he wasn't *asserting* it, either.

It follows that one must be very careful in juggling the cross-centuries ambiguities when one asks whether cognitive scientists—and, for that matter, the early physiologists—have radically misunderstood Descartes. Consider this comment, for instance:

> Since consciousness is standardly seen as including sentience, the suggestion [made by "analytic philosophy" in general and "cognitive science" in particular] that the force of the *Bête-Machine* Doctrine was to deny consciousness to animals is thus 180 degrees off course. (Baker and Morris 1996: 91)

The use of the term *sentience* is problematic here. As we've seen, Descartes ascribed sentience to animals. But—unlike the Aristotelians—he explained sentience mechanically, not in terms of a "sensitive" soul, an organizing principle not found in animate nature. And—unlike his twentieth-century successors—he didn't interpret it as a form of non-judgemental consciousness. Given that these very points were made by the authors of the criticism just quoted, their description of cognitive science's view of Descartes's position as "180 degrees off course" is bizarre.

Part of the problem, of course, is that we don't know just what *we* mean by consciousness, experience, sensations—in other words, what today's philosophers call *qualia*. Indeed, some philosophers of cognitive science explicitly *deny* the existence, even in humans, of experiential qualities over and above the functional properties of the brain. The comforting, and widespread, notion that the concept of consciousness—if not its explanation—is clear to everyone whose IQ is larger than their shoe size melts away when put under a philosophical spotlight (see Chapters 14.xi and 16.iv).

In my judgement, it's nearer the truth to say (anachronistically) that Descartes denied animal consciousness than to say (also anachronistically) that he admitted it. Certainly, if—as many people believe—sensory experience is something over and above brain states, inexplicable by objective science, then he would not have ascribed it to animals. In that sense, the misunderstanding isn't so much of a misunderstanding after all.

Moreover, it's been the usual interpretation of his work since the late seventeenth century, when the empiricist notion of 'pure' experience (ideas, sense data) untainted by judgement or reflection became widely current. In discussing anyone's historical influence, one must normally focus on what people thought they said, even if this differs from what they actually did say. For these two reasons, I'll sometimes assert, without further qualification, that Descartes denied animal consciousness.

f. Vivisection revivified

Besides his mechanism (in both senses), Descartes had a theological reason for denying that animals have rational souls. He argued in the *Discourse* that the soul was "separable" from the body (see Section iii)—and therefore immortal. And in a letter of 1649, he wrote to a doubter: "It is less probable that worms, gnats, caterpillars, and the rest of the animals should possess an immortal soul than that they should move in the way machines move."

But there was, and still is, an alternative. Perhaps animals do indeed have souls—some principle that "animates" the body in some way, perhaps even involving a lowly form of consciousness—but, unlike Christian souls, these die with them.

This broadly Aristotelian position seems to have been held by the Jansenist priest Antoine Arnauld (1612–94), who doubtless shared Descartes's scepticism about the immortality of caterpillars:

I fear that this belief [that animals have no soul] will not carry persuasion into men's minds, unless supported by the strongest evidence. For at the first blush, it seems incredible that there is any way by which, without any intervention of the soul, it can come to pass that the light reflected from the body of a wolf into the eyes of a sheep should excite into motion the minute fibres of the optic nerves and, by the penetration of this movement to the brain, discharge the animal spirits into the nerves in the manner requisite to make the sheep run off. (trans. Haldane and Ross 1911: ii. 85–6)

Descartes's reply was unyielding. Citing the example of someone who, in falling, automatically puts out their hand to protect their head, he asked: "why should we marvel so greatly if the light reflected from a body of a wolf into the eyes of a sheep should be equally capable of exciting in it the motion of flight?" (trans. Haldane and Ross 1911: ii. 104).

Although he didn't go on to say so, it was clear that the "strongest evidence" requested by Arnauld would necessitate neurophysiological research—including experiments on living animals. As we've seen, Descartes did some such experiments himself, and recommended others to do them.

Many before him, including Vesalius and Harvey, had done so too. And although the experimental animals' suffering could, if one wished, be minimized, pain relief was impossible (awaiting the discovery of anaesthesia in the late nineteenth century).

What's not clear is whether the Cartesians in general did wish to minimize the suffering, or whether they thought there was no suffering to minimize. As we've seen, Malebranche argued—on *theological* grounds—that animals don't feel pain. But we've also seen that (despite what's commonly claimed) we can't be certain whether Descartes himself believed this. Nor is it certain whether his animal–machine hypothesis specifically encouraged physiologists towards increased cruelty, or even studied indifference.

There's one 'eyewitness' report that Cartesians associated with the Port-Royal school (Chapter 9.iii.b–c) did unpleasant experiments with an attitude of what looked like appalling callousness, confident in the belief that animals could feel no pain:

They administered beatings to dogs with perfect indifference, and made fun of those who pitied the creatures as if they felt pain. They said the animals were clocks . . . They nailed poor animals up on boards by their four paws to vivisect them and see the circulation of the blood which was a great subject of conversation. (Fontaine's *Mémoires de Port-Royal*, quoted in Leiber 1988: 313)

However, this report may not be trustworthy. Not only was it written years after the supposed events, but its author "held Cartesianism to be scientistic blasphemy" so had every reason to malign Descartes's followers (Leiber 1988: 314).

There are also reports of late seventeenth-century experiments which troubled one—but apparently not all—of their perpetrators:

Besides all this, I have also imitated Bilsius' experiment on the movement of chyle, when I was at Amsterdam; but I did not find that diversity in the blood, although I kept the dog alive up to the third hour who would have lived even through the whole day in this torment; but since the first attempt did not provide any certain result, I may admit that I tortured them with such lengthy cruelties with horror. The Cartesians boast hugely of the certainty of their philosophy: I wish they could persuade me as certainly as they are persuaded themselves, that there is no soul in animals and that there is no difference between the nerves of a live animal and the wires in a machine which is set in motion when you touch them, dissect them, burn them: then I'd be more willing to probe the entrails and vessels of an animal for a number of hours, since I see that there are many things which ought to be investigated, which one cannot expect to do in any other way. (Lindeboom 1979: 64 n., trans. S. Medcalf)

This comment—left in the Latin in the 1979 edition, so as not to upset the readers—seems to imply that Cartesian experimenters, confident that animal suffering is an illusion, didn't even try to minimize it. *Zero* is low enough to salve anyone's conscience.

However that may be, there were indeed many things—physiological questions suggested by the mechanistic approach—that could be investigated in no other way. Vivisection therefore increased hugely as a result of Cartesianism (Rupke 1987; Daly 1989). It was practised by the nascent Royal Society: Hooke, for instance, vivisected a dog in 1667, to study respiration (Shapin 1994: 393). And it was defended towards the end of the century by Locke's hero Boyle (1627–91), who saw its critics as superstitious sentimentalists:

The veneration wherewith men are imbued for what they call nature has been a discouraging impediment to *the empire of man over the inferior creatures of God*: for many have [described it as] something impious to attempt. (Boyle 1744: 363; italics added)

Dr Johnson, one might think, was no sentimentalist. Yet, a hundred years after Boyle, he complained bitterly about the "arts of torture" practised by some medical men, whose "favourite amusement" seemed to be "to nail dogs to tables and open them alive" (Wiltshire 1991: 125–9). However, such strictures had little or no effect at the time: Royal Society members such as Stephen Hales continued their experiments on live frogs, dogs, and horses (Daly 1989). Not until the 1870s would official action be taken to limit these practices (see Section v.a, below). Meanwhile, Cartesian mechanism—and *perhaps* Cartesian views on animal consciousness—encouraged them in the service of science.

g. Human bodies as machines

The fourth claim made in the *Discourse* went even further. Human bodies, too, are machines—in *both* of the two senses distinguished above. Because they're governed by the laws of physics, medicine must be based on scientific physiology. One can even discover certain anatomical/physiological facts a priori, by considering the body as a mechanical, even a machine-like, system.

Thinking of muscles as levers of varying length, for example, Descartes argued (in the *Treatise on Man*) that many movements—such as the movements of the eyes—*must* involve the relaxation of certain muscles at the same time as the contraction of others. And this reciprocal action, he said, must involve active nervous inhibition of some muscles, as well as excitation of their antagonists. We now know that his armchair hypothesis was correct (see Section viii.d). But for two centuries it was thought absurd, even by physiologists who had deserted the armchair for the laboratory: "Several who witnessed the fact did not report it, hesitating to accept it as true" (Sherrington 1940: 166).

Moreover, said Descartes (in the *Discourse*), human bodies are comparable to automata:

And this will not seem strange to those who [know] how many different *automata* or moving machines can be made by the industry of man . . . From this aspect the [human] body is regarded as a machine which, having been made by the hands of God, is incomparably better arranged . . . than any of those which can be invented by man.

Indeed, he himself discussed how one might build an android activated by magnets. And a historian of technology has remarked:

Legend has it that [Descartes] did build a beautiful blonde automaton named Francine, but she was discovered in her packing case on board ship and dumped over the side by the captain in his horror of apparent witchcraft. (de Solla Price 1964: 23)

Sadly, this story is probably no more than ''legend''. But Descartes was adamant in seeing the body as no more than a wonderfully complex machine. Thus in reply to another objection from Arnauld, he wrote:

[the body alone is responsible for] the beating of the heart, the digestion of our food, nutrition, respiration when we are asleep, and even walking, singing, and similar acts when we are awake, if performed without the mind attending to them. When a man in falling thrusts out his hand to save his head he does that without his reason counselling him so to act, but merely because the sight of the impending fall penetrating to his brain, drives the animal spirits into the nerves in the manner necessary for this motion, and for producing it without the mind's desiring it, and as though it were the working of a machine. (trans. Haldane and Ross 1911: ii. 103–4)

'Man as machine' was now being unambiguously argued—and even supported by references to 'machine as man'.

2.iii. Cartesian Complications

The *Discourse* contained two further claims also, each more ambiguous—as regards the prospects of a future cognitive science—than the other four.

One of these licensed psychophysiology and psychophysics, but excluded 'pure' psychology from the reach of science. It also laid philosophical tripwires for people studying the relation of thought and perception to the external world. The other ruled out many, though not all, sorts of AI.

a. The mind is different

Descartes's fifth claim was that a human being has a mind (a rational soul) as well as a body—and that these are radically different. The mind is not a machine, and is nothing like a machine. Indeed, in so far as the human body is guided by the mind, it isn't a machine either:

And as a clock composed of wheels and counter-weights no less exactly observes the laws of nature when it is badly made, and does not show the time properly, than when it entirely satisfies the wishes of its maker, and as, if I consider the body of a man as being a sort of machine so built up and composed of nerves, muscles, veins, blood, and skin, that though there were no mind in it at all, it would not cease [even in sickness] to have the same motions as at present, *exception being made of those movements which are due to the direction of the will, and in consequence depend upon the mind* . . . (Descartes 1641/1642: 195; italics added)

The mind, Descartes argued, doesn't exist in the material world, but in the realm of self-conscious substance. Consciousness and intelligence pertain to the mind, not the body. Indeed, a mind just *is* a thinking thing, a psychological subject essentially distinct from the body. That, by contrast, is an extended, or material, thing. So whereas in animals sensory processes are linked *directly* to motor actions, in people there is what one might call a sensori-motor sandwich: sensory input and motor output (both aspects of the body) separated by conscious thought (the mind). (As we saw in Section ii.d, Descartes—by our lights—conflated consciousness and reason, or thinking, since he didn't differentiate consciousness as awareness, including *qualia*, from self-reflexive experience.)

This was a novel claim—and, many philosophers would argue, a disastrous one (14.xi and 16.vi–viii). Descartes was saying something very different from Aristotle, who had taught that living things are informed by various levels of soul, or *psyche*, these being animating principles that aren't separable from the body (Matthews 1977, 1992). He was saying something different, too, from his compatriot Michel de Montaigne (1533–92), whose scepticism had spurred Descartes's search for *certainty*, and who'd insisted on "the equality and correspondence between ourselves and beasts" (1580: 167). In particular, Montaigne had ridiculed the belief that we possess a special faculty ("a reasonable soul") able to see the truth, pointing out that our mental faculties, like those of animals, are inseparable from our bodies:

> It's certain that our apprehension, our judgement, and the faculties of our mind [*âme*] in general suffer according to the movements and alterations in the body, which alterations are continual. Isn't our spirit more lively, our memory more available, and our speech more lively in health than in illness? And don't joy and gaiety make us receptive to ideas that would present themselves to our minds in quite a different way in unhappiness or melancholy? (Montaigne 1580: 269; my trans.)

Descartes allowed these facts to be so (how could one deny them?). But for him, they were *psycho-physiological* facts in the strict (i.e. Cartesian) sense, whereas for Montaigne they weren't.

This is why some people argue that Descartes wasn't so much describing the mind, as *inventing* it: why had no one else noticed the inner realm of consciousness (Rorty 1979: 17–69; Putnam 1999)? As we'll see in Chapter 16.viii, such people reject Descartes's separation of mind and body—though a form of dualism usually persists, in the guise of *cause* and *reason*.

One implication of Descartes's fifth claim was that a purely psychological science is impossible. Our conscious choice, and our reason, is not only not subject to the laws of physics, but is unconfined by any law. On the contrary: "It is only will, or freedom of choice, that I experience in myself [as unlimited]; so that it is in this regard above all, I take it, that I bear the image and likeness of God" (1641/1642, Meditation IV). Despite the many limits on our understanding, he said, we can always avoid error by freely refusing assent to a doubtful idea. (As a strict rationalist, "satisficing" wasn't in his vocabulary: see 6.iii.a.) Admittedly, he described the "passions", and complex conflicts between "natural appetites" and "the will", in terms of interacting streams of movement of the animal spirits (1649). However, he didn't suppose the will itself to be determined by the body, nor subject to psychological laws or systematic explanation.

Psychophysiology, by contrast, was tacitly allowed. Our daily experience assures us, Descartes said, that mind and body are closely related. Not only does my arm rise when I will it to do so, but brain injuries—and drunkenness—lead to various changes in consciousness. Indeed, he argued that each distinct mode of consciousness is accompanied by a different state of the brain.

In principle, then, the path lay open for some future experimental science in which these mind–body linkages might be mapped. One example would be the late twentieth-century use of brain scans by cognitive neuroscientists, aiming to discover which areas of the brain are active when someone thinks of one kind of thing rather than another (Chapter 14.x.b).

This argument, you may notice, took for granted that it *makes sense* to suggest that mental events and brain states may be correlated. Some early twentieth-century philosophers, Maurice Merleau-Ponty for instance, rejected Cartesianism at such a fundamental level that it followed that this wasn't so: his language of "mind" and "brain" wasn't acceptable, except as a (philosophically misleading) everyday shorthand. Today, their followers argue that cognitive science is radically flawed as a result (Chapters 14.xi and 16.vi).

But that was for the future. In the mid-seventeenth century, Descartes's assumption became widely accepted. Indeed, it still is. The overwhelming majority of the readers of this book, I've no doubt, assume that hypotheses about mind–brain correlations obviously make sense—and that many are very likely true.

"How can be there be any question about it?" you may be wondering: "Isn't brain-scanning discovering more instances every day?" Well, perhaps. I shan't ask you to consider the counter-arguments until we discuss brain and consciousness in Chapter 14.x–xi. Meanwhile, I'll go along with the prevailing view that talk of mind–brain correlations not only makes sense, but is very often true. (To anticipate: I'll *still* go along with it, after the discussion; but at least we'll have seen why it is that intelligent people have sometimes denied it.)

b. Birth of a bugbear

Quite apart from the radical anti-Cartesian challenge, which we're ignoring here, there was—and is—a catch. On Descartes's view, body and mind are so different, one material the other immaterial, that there can be no intelligible relation between them.

Their systematic association, therefore, can be due—said Descartes—only to God's benevolent fiat. This reference to divine fiat was interpreted in three ways. One of these ignored the divinity while keeping the systematicity, and—though battered—survived to modern times.

The first interpretation was the doctrine of occasionalism. Briefly suggested by Descartes himself, occasionalism was developed by his contemporary Géraud de Cordemoy (of whom, more in Chapter 9.iii.a) and his successor Malebranche, among others. It claimed that, on the occasion of my willing to move my arm, my arm is caused by God to move; and on the occasion of my hand being injured, God causes me to feel pain. An act of will is thus only the occasional cause of a voluntary action, not the real cause.

The notion that God intervenes in the human world—performing miracles, punishing communal sin by famine, and so on—was in those days a commonplace. What occasionalism added was systematicity. The doctrine was approved in theological circles, because of its stress on the unceasing activity of God in sustaining our everyday life.

The second interpretation, which Descartes usually favoured, was subtly different. It saw God as establishing a two-way causal interaction between mind and body. This was called "Natural" causation, to contrast it with "efficient" (mechanical) causation. Though metaphysically unintelligible, Natural causation is sanctioned by God as an enduring part of human Nature, which is a unique "union" of mind and body. (Sometimes, Descartes even described human Nature as a third philosophical 'primitive'—Cottingham 1986: 127–32.) This type of causation doesn't require continual divine intervention, but relies on the Natural union between mind and body. The will really does cause the arm to move, by a mysterious type of causation provided to human beings by God.

This metaphysical miracle is highly detailed, providing countless brain–mind correlations—in principle, open to experimental study. However, there's no intelligible reason why *this* brain state rather than *that* one should accompany a particular mental state. As Descartes put it:

> God could have constituted the nature of man in such a way that this same movement in the brain [physically caused by a certain movement of the nerves in the foot] would have conveyed something quite different [from a sensation of pain-in-the-foot] to the mind . . . [indeed] it might finally have produced consciousness of anything else whatsoever. (Meditation VI)

This implies that psychophysiology can provide only correlational data, not theoretical intelligibility (see 14.x–xi). Philosophically, its findings can be understood only in the context of divine benevolence.

However, Descartes was often read in a third way. Here, his references to God's benevolence in establishing (Natural) mind–body causation were ignored. He was supposed instead to have posited a (natural) causal interaction between mind and body, one that we might hope to understand. Descartes himself invited this interpretation, for he sometimes answered objections in terms apparently referring to some sort of natural causation, citing familiar analogies such as rudders and gravity.

Princess Elizabeth of Bohemia, for instance, wrote to him in 1643: "I beg of you to tell me how the human soul can determine the movement of the animal spirits in the body so as to perform voluntary acts—being as it is merely a conscious substance." And she added that movement is something that can be caused only by material contact between extended things. Descartes (1643) admitted that "what your Highness is propounding seems to me to be the question people have most right to ask me in view of my published works". His reply, which appealed to the analogy of gravity, didn't convince Elizabeth. She wrote back saying she found it unintelligible, and that she "could more readily allow that the soul has matter and extension than that an immaterial being has the capacity of moving a body and being affected by it".

Two years later, and partly as a result of this correspondence, Descartes wrote *The Passions of the Soul* (published in 1649, a few months before his death). There, he suggested that mind–body interaction is mediated by movements, or (in recognition of "the conservation of motion") by changes in the direction of movement, of the stalk of the pineal gland—which he compared to the rudder of a ship. His choice of the pineal gland wasn't arbitrary: since it was a single organ in the midline of the brain, he thought it might provide the basis for the integration of the different senses—for example, sight and touch. (His choice wasn't entirely original, either: Fernel had said that the pineal gland controls the passage of the animal spirits between distinct ventricles in the brain.)

The causal interaction, he said, works both ways. As regards voluntary movement:

> the whole action of the soul consists in this, that solely because it desires something, it causes the little gland to which it is closely united to move in the way requisite to produce the effect which relates to this desire.

Purely physical causation can then take over, because the gland is in contact with the animal spirits flowing through the superior ventricle of the brain. Likewise, movements of the pineal gland caused by the animal spirits coming from the sense organs lead—in human beings only, not in animals—to consciousness. In speech, for example, a person's lip movements are (originally) caused by their conscious thoughts, and (eventually) lead to conscious understanding in people whose ears are physically affected by the air disturbances produced by speech.

For the next 300 years, many of Descartes's readers thought that this was a respectable scientific hypothesis. That's not to say that the pineal gland was universally regarded with awe. To the contrary, the *Spectator* magazine in 1712 published a spoof report on this "lover's gland", saying that it "smelt very strong of Essence and Orange-Flower Water", and on dissection was found to contain a cavity "filled with Ribbons, Lace and Embroidery" (Porter 2001: 502 n. 35). But if the notional anatomy was widely ridiculed, the metaphysics wasn't.

However, perhaps it should have been. For it was subject to the same philosophical objections that Princess Elizabeth, among others, had already raised.

To be sure, the animal spirits might move, and be moved by, the pineal gland. But how could such movements—or any other purely physical changes—cause, or be caused by, consciousness? Correlations between mind and body there may be, and many of them. (Remember, we're here ignoring the later suggestion that this 'obvious fact' isn't even mistaken, but fundamentally confused.) But dualistic interaction, in either direction, is unintelligible *in Descartes's own terms*.

Even if we drop Descartes's commitment to mental substance, there's still a causal/metaphysical gap between bodily processes on the one hand and conscious states, intentionality, and reason on the other. (The still-unsolved problems of how to explain mind–body correlations and intentionality are discussed in Chapters 14.x–xi and 16.iv–x, respectively.)

This fifth Cartesian claim, the metaphysical separation of mind and body, presents a further ambiguity relevant to cognitive science. For Descartes's writings were to be influential in two radically different philosophical traditions.

On the one hand, his mechanism and recommendation of experimental science were accepted by empiricists, from his contemporary Thomas Hobbes onwards.

For Hobbes, even the idea of *infinity* (in mathematics or theology) was an extrapolation from experience. He took mechanism fearlessly into psychology and political philosophy. For instance, he likened society—"the Commonwealth"—to "*Automata* (Engines that move themselves by springs and wheeles as doth a watch)" (1651: 1). He even saw *thinking* as a form of *computation*. So, for instance, he said:

When a man *Reasoneth*, hee does nothing else but conceive a summe totall, from *Addition* of parcels; or conceive a Remainder, from *Substraction* of one summe from another: which (if it be done by Words,) is conceiving of the consequence of the names of all the parts, to the name of whole; or from the name of the whole and one part, to the name of the other part. And though in some things, (as in numbers,) besides *Adding* and *Substracting*, men name other operations, as *Multiplying* and *Dividing*; yet they are the same; for Multiplication, is but Adding together of things equal; and Division, but Substracting of one thing, as often as we can. These operations are not incident to Numbers onely, but to all manner of things that can be added together, and taken out of one another. For as Arithmeticians teach to add and substract in *numbers*; so the Geometricians teach the same in *lines*, *figures* . . . degrees of *swiftenesse*, *force*, *power*, and the like; The Logicians teach the same in Consequences of words; adding together *two Names*, to make an *Affirmation*; and *two Affirmations*, to make a *Syllogisme*; and many *Syllogismes* to make a *Demonstration* . . . [Writers of Politiques and Lawes, similarly.] In summe, in what matter soever there is a place for *addition* and *substraction*, there is also place for *Reason*; and where these have no place, there *Reason* has nothing at all to do.

Out of all which we may define, (that is to say determine,) what that is, which is meant by this word *Reason*, when wee reckon it amongst the Faculties of the mind. For REASON, in this sense, is nothing but *Reckoning* (this is, Adding and Substracting) of the Consequences of general names agreed upon, for the *marking* and *signifying* of our thoughts; I say *marking* them, when we reckon by our selves; and *signifying*, when we demonstrate, or approve our reckonings to other men. (1651: 18–19)

Moreover, this "reckoning" was effected purely by movement within the body—as were sensation, imagination, and voluntary choice too:

All [the] qualities called *Sensible*, are in the object that causeth them, but so many motions of the matter, by which it presseth our organs diversely. Neither in us that are pressed, are they anything else, but divers motions; (for motion, produceth nothing but motion). (p. 3)

For after the object is removed, or the eye shut, wee still retain an image of the thing seen, though more obscure than when we see it. And this is [*Imagination*, or *Fancy*, which] therefore is nothing but *decaying sense*, and is found in men, and many other living Creatures, as well sleeping, as waking. (p. 5)

There be in Animals, two sorts of *Motions* peculiar to them: One called *Vitall* [the heart beat, breathing, digestion, etc.]. The other is *Animal motion*, otherwise called *Voluntary motion*; as to *go*, to *speak*, to *move* any of our limbes, in such manner as is first fancied in our minds. [I have already shown] That sense, is Motion in the organs and interiour parts of mans body . . . And that Fancy is but the Reliques of the same Motion . . . And because *going*, *speaking* and the like Voluntary motions, depend always upon a precedent thought of *whither*, *which way*, and *what*; it is evident that the Imagination is the first internall beginning of all Voluntary Motion. And although unstudied men, doe not conceive any motion at all to be there, where the thing moved is invisible; or the space it is moved in is, (for the shortness of it) insensible; yet that doth not hinder, but that such Motions are. These small beginnings of Motion, within the body of Man, before they appear in walking, speaking, striking, and other visible actions, are commonly called ENDEAVOUR. (p. 23)

[If a man should talk to me of] *A free Subject*; *A free-Will*; or any *Free*, but free from being hindered by opposition, I should not say he were in an Errour; but that his words were without meaning; that is to say, Absurd. (p. 20)

In other words, our thoughts, goals, and decisions aren't merely correlated with invisible bodily motions: they *consist in* bodily motions. Such a radical expression of mechanism was a step too far for Descartes, as we'll see.

Empiricism and scientific progress were soon explicitly linked by the philosopher Locke, an early Fellow (from 1668) of the Royal Society. He described himself as merely "an under-labourer in clearing the ground a little, and removing some of the rubbish that lies in the way to knowledge". And he identified the current "master-builders of the commonwealth of learning" as the scientists Boyle, Thomas Sydenham, "the great Huygenius" and "the incomparable Mr. Newton" (J. Locke 1690, Epistle).

On the other hand, Descartes's emphasis on the epistemological primacy of the individual consciousness (the *cogito*) led first to Kantianism and then to neo-Kantian idealism and Continental phenomenology (see Section vi, below). Although empiricism is much the stronger tradition in modern cognitive science, phenomenological critiques—and research programmes—are increasingly being promoted (see Chapters 14.xi and 16.vi–viii). Indeed, this is one aspect of the general cultural challenge to Cartesian modernism that blossomed in the 1960s and is still in bloom (Roszak 1969; Toulmin 1999).

Ironically, a leading phenomenologist has recently used the technology of virtual reality, or VR (see 13.vi), to revive—if only to dismiss—Descartes's hypothesis of the evil demon, or *malin génie*. Because Descartes believed that he was trapped inside his own thoughts and perceptions, he said it was metaphysically possible that he was dreaming, or that some evil demon might be deceiving him into thinking that his sensations, including his bodily sensations, were caused by real material objects when in fact they weren't. He mentioned the phenomenon of the phantom limb, for instance, in which an amputee feels pain seemingly caused by an injury in their non-existent leg. He was relieved of this anti-realist doubt only by his argument that God, whose existence he had now proved to his own satisfaction, wouldn't allow us to be deceived in that way. Today, Hubert Dreyfus regards VR in general, and the film *The Matrix* in particular, as constituting "Descartes' Last Stand" (H. L. Dreyfus 2000; cf. H. L. Dreyfus and Dreyfus 2002). The realist/anti-realist implications of VR are discussed in Chapter 16.viii.c.

c. The prospects for AI

If Descartes's fifth claim had mixed implications for the biological and psychological aspects of cognitive science, the sixth carried mixed messages regarding the prospects for AI.

Bearing in mind the many ancient and post-Renaissance automata, Descartes allowed that there might be machines like monkeys, having all their behavioural capabilities. But, he argued, there could never be a convincing android automaton. A humanoid robot capable (for instance) of fleeing from an approaching wolf or tiger, and of using its hands to prevent itself from falling, was certainly possible. But one with language, reason, or general intelligence was not:

> If there were machines resembling our bodies, and imitating our actions as far as is morally possible, we should still have two means of telling that, all the same, they were not real men. First, they could never use words or other constructed signs, as we do to declare our thoughts to others. [A machine, if touched at a certain spot, could cry out that it was hurt, but it could not] be so made as to arrange words variously in response to the meaning of what is said in its presence, as even the dullest men can do. Secondly, while they might do many things as well as any of us or better, they would infallibly fail in others, revealing that they acted not from knowledge but only from the disposition of their organs. For while reason is a universal tool that may serve in all kinds of circumstances, these organs need a special arrangement for each special action . . .

Descartes wasn't speaking here about mere technological difficulties. He believed that these were principled philosophical reasons why intelligent language-using automata could never be built. These reasons also forbade any ascription of language to animals, no matter what sounds they might be taught to utter (see Chapters 9.ii.b and 7.vi.c).

It would follow that psychological AI is doomed to failure. The mind is nothing like a machine, and even brain events have no intelligible relation to consciousness. The notion that a man-made automaton could help us understand human psychology (thinking and consciousness) is therefore absurd.

By contrast, some—but not all—sorts of technological AI are, on this view, achievable. Computer vision, for example, is possible. So is verbal data processing, including so-called expert systems, wherein the word strings are composed beforehand by the programmer. Robots simulating animal behaviour, no matter how complex, are feasible. And brain-inspired modelling, such as connectionist pattern recognition, could be successful.

However, natural language processing in general—good machine translation, for instance—is impossible, as is the automation of common sense. Even with the help of non-human strategies or tricks, the Turing Test (16.ii.c) couldn't even *seem* to be passed (Gunderson 1964*a*). As we'll see (in Chapters 9.x and 16), these anti-AI claims still have committed supporters today—some of whom cite Descartes as an intellectual ancestor.

In sum, then, Descartes's vision of 'man as machine' was both ambitious and limited. It concerned only the body, not the mind. Nevertheless, it assured scientists that they could hope to discover (though not to understand) the detailed correlations that exist between human minds and human brains. And, since bodies are machines, science could explain how the nervous system works, and how it interacts with other bodily organs. Indeed, the entire range of animal behaviour could be understood in this way.

2.iv. Vaucanson's Scientific Automata

In the late seventeenth century, two very different projects gained inspiration from Cartesian mechanism. One was experimental physiology itself: man—or rather, man's body—as machine (see Section v). The other was automata building: machine as man.

a. Fairs and flute-players

The post-Cartesian designers of automata were concerned with what Naudé had called Mathematicks, not Magick (see Preface). But, like their predecessors whom he had defended so strongly, they were more interested in Mathematicks than in Man.

In other words, their efforts were technological rather than scientific. At best (as remarked in Section i.b), they were existence proofs of mechanism in the broadest sense. Most twentieth-century roboticists dismiss them as scientifically uninteresting. One such, for example, complained that "these eighteenth-century gadgets were developed purely for their entertainment value" (Raphael 1976: 258).

This modern critic specifically included Vaucanson's automata in his complaint. Describing these as "the entertainment sensations of the courts of Europe", he compared them to the "audioanimatronics" of Disneyland and contrasted them with modern robots built "to test our current theories" about "how certain biological systems behave". In part, this is correct: though there was no dedicated theme park, Vaucanson's robots were exhibited to general delight in a Parisian fair, and in London's Haymarket too (see Section i.b, above). But if the comparison is just, the contrast is not.

To be sure, the young Vaucanson's scandalous attempt to build automatically flying angels, which caused his hurried departure from the religious order in which he was then studying, was probably a mere fancy (Bedini 1964: 36). It may even be fanci-ful: an alternative story has it that he built androids to wait at table for some distinguished visitors, and was dismissed the next day despite their having been favourably impressed (G. Wood 2002, ch. 1).

However, his famous automata were neither fancies nor fanciful. Nor were they undertaken "for the sole purpose of making money", as one historian of technology has claimed (Bedini 1964: 37). Indeed, it was common in those days for scientists to demonstrate their work to the public for money (Schaffer 1983). But Vaucanson was unusual, perhaps even unique, in often prefacing his demonstrations with an explanation of how his automaton worked, before setting it in motion to the wonder of the audience. (He did this, for instance, when his flute-player was shown to a select audience in a Paris mansion, for which an entrance ticket cost the equivalent of a workman's weekly wage.)

Unlike most other eighteenth-century gadgets, Vaucanson's were intended to show, in relatively specific terms, how our bodies work (Doyon and Liaigre 1956; Fryer and Marshall 1979). That is, they were theoretically motivated simulations of actual bodily processes. An early example (now lost, and only briefly mentioned in the contemporary documents) apparently involved several "anatomies": a model of a group of animals of different species. But the species that would be remembered were duck and man.

Sometimes, Vaucanson focused on chemical processes. His famous mechanical duck, made of gilded copper, accepted food into its mouth; (seemingly) broke it down inside its stomach; passed it through its rubber-tube intestines; and then mimicked the usual digestive denouement. (Although the denouement was genuine, the digestion wasn't. After his death, it was discovered that the duck contained two hidden chambers: one pre-loaded with evil-smelling mash, the other used to store the swallowed corn—Landes forthcoming, n. 4.)

Vaucanson's primary aim, here, wasn't to exhibit an amusing clockwork toy, but to illustrate his theory of digestion as dissolving ("Dissolution") and "to represent the Mechanism of the Intestines" (Vaucanson 1738/1742: 21). Indeed, he sometimes removed the duck's outer covering so as to let its innards show: "My Design is rather to demonstrate the manner of the Action, than to show a Machine" (p. 22).

But Vaucanson seems to have been even more interested in observable behaviour—or rather, the anatomy that made behaviour possible. His duck did a number of engaging tricks, such as quacking, splashing about on water, stretching its neck to peck corn from people's hands, rising on its feet, and flapping its wings—each of which contained over 400 articulated parts. As he put it, in a letter to the Abbé de Fontaine:

> I don't believe the Anatomists can find any thing wanting in the Construction of the Wings. The Inspection of the Machine will better shew that Nature has been justly imitated, than a longer [written] Detail, which wou'd only be an anatomical Description of a Wing . . . Not only every Bone has been imitated, but all the Apophyses or Eminences of each Bone. They are regularly observ'd as well as the different Joints . . . (Vaucanson 1738/1742: 21–2)

Sadly, the duck is now lost, as is a copy made in Germany in 1847 (however, a master automata maker is now trying to rebuild it as best he can: A. Marr, personal communication). Even 200 years ago, in 1805, it was already a sorry sight. Owned at that time by the Duke of Brunswick's doctor, in Helmstadt, it was sitting in "an old garden house", "utterly paralyzed" and "mute" (G. Wood 2002, ch. 1; Riskin forthcoming). It could still eat, but had lost its feathers. In short, it—and the flute-player alongside it, whose "playing days were past"—was in "the most lamentable state".

These descriptions came from the pen of its august visitor Johann von Goethe. As we'll see in Section vii.c, Goethe had little sympathy with the analytic/scientific motives that had energized Vaucanson. His visit to the dilapidated duck was an exercise in sceptical curiosity, not a pilgrimage of honour. But even he seemed somewhat depressed by its aged decline.

More significant, in this context, were Vaucanson's automatic musicians built in the late 1730s: the flute-player and the tabor-and-pipe player. These were life-size manikins, each with mobile lips, tongue, and fingers (made/padded with leather). The flute-player took the form of a shepherd boy, while its drummer cousin was a seated faun—half-man, half-goat—closely modelled on a statue in Louis XIV's Tuileries gardens.

To sound the German flute and the three-hole pipe, air was impelled through the mouth by three sets of bellows, which could deliver air at different pressures. (Vaucanson chose to use the German flute because of its reputation for difficulty.) The tabor-and-pipe player held a drumstick in its right hand, with which it could play single or double strokes, drum rolls, or time-keeping beats synchronized with the pipe held in its left hand.

Both machines could play a range of melodies: the flautist eleven, the piper–drummer over twenty (a score of minuets, plus some other dance tunes). Features such as pitch, speed, timing, echo, and crescendo could be varied by Vaucanson at will. These variations weren't made by interfering with the performance on the fly, but by adjusting mechanical parameters before the performance began: "[my] Machine, when once wound up, performs all its different Operations without being touch'd any more" (p. 23).

The flute-player was first exhibited (in 1737) at the fair of Saint-Germain, amidst ribbons, rattles, and freaks of various kinds. The wondering Parisians could be forgiven for thinking it a mere toy. But Vaucanson's scientific aims were made clear in a paper (describing the flute-player) addressed to the French Académie royale des sciences and in a personal letter to the Abbé de Fontaine (describing the drummer and the duck). These were translated in a pamphlet accompanying the London exhibition (Vaucanson 1738/1742).

No doubt, most of the general public visiting the exhibition didn't read it. Had they done so, they'd have realized that Vaucanson was even more ambitious than appeared at first sight.

b. Theories in robotic form

It was clear from Vaucanson's (largely unread) writings that his automata weren't mere gimmickry, but test-models of theories of instrumental playing. That is, they were intended as studies of how people move their lips, tongue, and fingers, and how they regulate their breathing, so as to make specific musical sounds with these wind instruments. (These "hows" should be interpreted charitably: unlike the A-Life work described in Chapter 15.ii.a, Vaucanson's machines weren't modelling individual muscles, but were focused on the movements of observable body parts.)

As Vaucanson put it, they were attempts to simulate "by Art all that is necessary for a Man to perform in such a Case" (1738/1742: 21). Having opened his paper by describing the structure of the flute, and (in great detail) the various movements involved when it's played by a human musician, he continued:

> These, Gentlemen, have been my Thoughts upon the sound of Wind-Instruments and the Manner of modifying it. Upon these Physical Causes I have endeavour'd to found my Enquiries; by imitating the same Mechanism in an *Automaton*, which I endeavour'd to enable to produce the same Effect in making it play on the German Flute. (p. 12)

A few pages later, he said:

> [My theory suggests that if certain movements are executed] it will then follow . . . according to the Principle settled in my First Part, the flute will give a low Sound: and this is confirmed by Experience. (p. 17)

One can almost hear his sigh of satisfaction and relief. But such reports, as is usually the case in scientific papers, underplayed the amount of work, and frustration, involved in getting the desired result. Vaucanson's descriptions of the movements executed by the automaton were highly detailed. Even so, they omitted a great deal:

> the fear of tiring you, GENTLEMEN, has made me pass over a great many little Circumstances, which tho' easy to suppose are not so soon executed: the Necessity of which appears by a View of the Machine as I have found it in the Practice. (p. 20)

This comment is an eighteenth-century version of the computational modeller's experience today: one doesn't know what bugs the system contains, nor what crucial processes have been omitted from the theory, until the robot is tested or the program is actually run.

As for the scientific value of Vaucanson's simulations, his translator John Desaguliers—a Fellow of the Royal Society of London, and inventor of the planetarium—had no doubts. In his Preface, he said: "this Memoire . . . in a few Words gives a better and more intelligible Theory of Wind-Musick than can be met with in large Volumes".

How, "better"? Well, Vaucanson had carefully described how each note, within three octaves, was produced by the flute-player. For the higher octaves, he found that extra mechanical variability must be added to the four basic operations. As well as studying single movements, he asked what combinations of movements are needed for certain effects. Pointing out the possibility of "a prodigious Number of Mechanical Combinations", he used his machines to find out what the results of some of these combinations might be. And he came up with some surprises ("Things which could never have been so much as guessed at").

For instance, he discovered (what Karl Lashley was to stress 200 years later) that the way in which a note is played—the bodily movements involved—may depend on the previous note. The need for context sensitivity had actually been discovered by him earlier, with respect to his mechanical duck. He had found that if the duck were sometimes to flap its wings, and sometimes to rise on its legs, a given mechanical part had to serve different functions in either case:

> Persons of Skill and Attention [will observe that] . . . what sometimes is a Centre of Motion for a Moveable Part, another Time becomes moveable upon that Part, which Part then becomes fix'd. (pp. 21, 23)

Again, it turned out that the three-holed pipe needed air pressures ranging from 56 pounds down to only 1 ounce to play the notes in its repertoire. And very fast tunes were performed better by the pipe-player than by human beings, because human tongue movements are too slow (compare the distinction between *competence* and *performance*: Chapter 7.iii.a).

Vaucanson wasn't the only eighteenth-century simulator. Others included two of his compatriots, the surgeons François Quesnay and Claude-Nicolas Le Cat—both of whom built simple models of the circulatory system, intended to teach anatomy and possibly to aid in therapy. In addition, various clumsy efforts were made to build prosthetic hands/limbs for amputees (Riskin 2003).

But these examples *didn't* lead to a proliferation of imitators in the following century. Partly, that was because Vaucanson's engineering skills outclassed those of all but the most ingenious of automata makers. More importantly, the Zeitgeist of Enlightenment rationalism gave way to that of Romanticism (see vi.c–d, below).

This favoured holistic over analytic science, and vitalism over mechanism. Accordingly, the countless nineteenth-century automata were *not* further exercises in scientific simulation, but—as in pre-Enlightenment times—mere toys, or engineering challenges. For instance, hidden pedals and levers were more common than moving body parts as such. Often, people went out of their way to deny that the bodily movements of human speech, namely the movements of tongue, larynx, epiglottis . . . , could be simulated artificially, and to insist that they could occur only in a living organism (Riskin 2003). Even limb movements weren't considered fit for *scientific simulation*: rather, they were superficially copied, for purposes of entertainment or prosthetics. It wasn't until the mid-twentieth century that automata for theoretical simulation became prominent once more (Chapters 4.viii.a–b and 10.iii.c).

c. Robotics, not AI

By that time (the mid-twentieth century), an extra—psychological—dimension was being added, over and above the aims of Vaucanson. For he'd been concerned with *only one half* of the Cartesian metaphysical divide.

Descartes himself would have enjoyed the flute-player, and would have endorsed Vaucanson's scientific hopes. For Vaucanson was studying the body, not the mind. *Psychological* aspects of musical performance and appreciation weren't at issue. Strictly, then, Vaucanson—the first scientific roboticist—wasn't doing psychological AI, *even though* he was modelling motor behaviour.

(Not until very recently could machine models be used to suggest how it's possible for people to understand the structure of music, and to apply this understanding both in listening intelligently and in performing expressively: Longuet-Higgins 1979, 1994; Longuet-Higgins and Steedman 1971. Even now, such studies can arouse scepticism. The editor of *Nature* rejected a paper that used the example of *Colonel Bogey* in describing how a computer could transcribe melodies into musical notation, suggesting—seriously? sarcastically?—that the computer be asked to transcribe something from Wagner instead. It was; it did; and publication followed—H. C. Longuet-Higgins, personal communication; 1976.)

To be sure, a curious quirk of history links Vaucanson with IBM. Vaucanson was a pioneer in the development of machine tools, such as metal-cutting lathes. While working as an inspector in silk factories in France, he had the idea—improved in 1801 by Joseph Jacquard (1752–1834)—for an apparatus using punched cards for weaving brocades automatically. Punched cards had already been used in France to control weaving, but they had to be hand-fed into the loom one by one. Vaucanson suggested stringing them together and feeding them in automatically, in sequence (Hyman 1982: 166). He even built a loom controlled by punched cards moved by a perforated cylinder.

One and a half centuries later, Herman Hollerith used electromechanical punched-card machines for analysing the results of the US Census of 1890. Manual analysis of the 1880 data had taken almost ten years (T. I. Williams 1982: 348), and the population had grown since then.

Hollerith left the Census Bureau in 1896 to start the Tabulating Machine Company, which in 1924 formed the core of IBM. And IBM eventually became the world's leading computer manufacturer—despite the early prediction of its Director, Thomas Watson, that only a handful of such machines would be needed worldwide. Even in the 1960s, state-of-the-art computers (the prototype IBM 360, for instance: see Preface, ii) were still programmed by ordered packs of punched cards.

But intriguing linkages of this sort aren't the same thing as real historical influence. Some members of the French Académie and the Royal Society recognized the scientific interest of Vaucanson's work on automata, but most of his contemporaries didn't.

His successors didn't recognize it either. One hundred years later, the committed mechanist Hermann von Helmholtz (1821–94) praised Vaucanson's "inventive genius" in aiming "to imitate living creatures", mentioning the flute-player "which moved all its fingers correctly". But he saw it as directed towards the "great problem" of "practical mechanics", as an exercise significant to technology, not biology (Helmholtz 1854: 137–8). Even now, as we've seen, Vaucanson's work is dismissed as "entertainment" by people doing research of an essentially comparable type.

Nor did the technological possibilities in the mid-eighteenth century encourage engineering projects of this—scientifically oriented—kind. Even the engineering wizard Vaucanson probably couldn't have done much more. If one wanted to further Descartes's vision of man as machine, the best method wasn't building automata, but studying physiology.

2.v. Mechanism and Vitalism

From the mid-1600s onwards, increasingly many scientists tried to explain physiological processes—respiration, for example—in terms of physics and chemistry. It's no accident that this project didn't look really persuasive until the late nineteenth century (see Section vii.a). Even then, some life scientists remained unconvinced.

a. Animal experiments: Are they needed?

Biology couldn't be mechanized without important advances in non-biological sciences, such as the theory of gases and the chemistry of combustion. It also required an extensive programme of experiments involving live animals—some of which included vivisection. (Only "some", because one can study some of the effects of a drug or a gas on a living animal *without* doing vivisection, which involves surgery.)

The need for experimentation on animals wasn't universally accepted at the time, however. This isn't a comment about the opinions of outsiders—such as Dr Johnson, whose qualms were cited in Section ii.e, above. Rather, it concerns doubts among the scientists themselves.

There were three different reasons for holding back. One was a doubt about the relevance of *animals*, another a doubt about the relevance of *experiments*, and the third a worry about the *morality* of certain sorts of animal experiment.

Many people still regarded human beings as so special that nothing useful could be learnt from work on animals. And this attitude survived into the late nineteenth century. Fully 200 years after Harvey had been scorned for investigating slugs, snails, and squill-fish, physiologists still felt bound to defend the study of "all animals", because "without such comparative study of animals, practical medicine can never acquire a scientific character" (Bernard 1865: 122–6).

Some influential biologists even regarded live experimentation as irrelevant in understanding lowly snails or squill-fish, because it destroyed the holistic character of life itself. The zoologist Georges Cuvier (1769–1832), for example, argued that:

> All parts of a living body are interrelated; they can act only in so far as they act all together; trying to separate one from the whole means transferring it to the realm of dead substances; it means entirely changing its essence. (quoted in Bernard 1865: 60)

And in 1823 the distinguished anatomist Charles Bell (1774–1842) declared:

> Experiments have never been the means of discovery; and a survey of what has been attempted of late years in physiology, will prove that the opening of living animals had done more to perpetuate error, than to confirm the just views taken from the study of anatomy and [the observation of] natural motions . . . (quoted in Robert M. Young 1970: 47)

(Such holistic qualms are still with us: many researchers have similarly criticized work, whether in neuroscience or in computer modelling, that neglects the *whole* animal—see Chapters 14.vi–vii, 15.vii, and 16.x.b.)

Other critics, unconvinced by Descartes's (supposed) denial of animal consciousness, saw such work as morally questionable—sometimes, *irrespective* of its scientific value. To be sure, in the eighteenth and early nineteenth centuries the view that nature (or even Nature) was created by God primarily for humans' use and delight was still widespread. Clergymen and scientists alike (often, the two were the same) commonly made this view explicit. For instance, the Quaker geologist William Phillips declared that mankind is "the Lord of Creation" and that "everything is intended for the advantage of Man" (Phillips 1815: 191, 193). However, such remarks were more likely to be made in the contexts of chemistry, geology, or botany than of zoology.

Eventually, the law declared zoology to be a special case. In 1876 the British Parliament's Cruelty to Animals Act set up the first statutory committee to legalize and regulate the treatment of experimental animals. Coinciding with the foundation of the Physiological Society of London, this Act encountered considerable opposition from the general public (R. D. French 1975). For many non-scientists felt that animal experiments shouldn't be regulated so much as banned.

Similar feelings are regularly aimed today at research carried out under the UK's Animals (Scientific Procedures) Act passed in 1986. Some high-profile physiologists—and their families—have endured threats, and even violence, accordingly. And several commercial companies for breeding and/or experimenting on animals have recently been closed down by a combination of imaginative financial pressures and physical assaults. Indeed, an entire village has been attacked in various ways, in an attempt to prevent the inhabitants having anything to do with the staff and owners of a nearby guinea-pig farm; the final straw was the theft of a relative's remains from the graveyard of the village church, and the farm was closed down a few months afterwards (*Nature* 2005*b*).

The Animals Act requires experiments on living animals to be justified by a detailed cost–benefit analysis before a licence is granted. The numbers of animals, the species selected, and the severity of the procedure (the potential for suffering) are all carefully considered, as is the likely benefit to science and/or human or veterinary medicine if the experiment goes ahead. Due in part to the findings of ethologists such as Jane Goodall (Chapter 7.vi.b), additional rules were introduced in the 1990s to protect wild-caught primates from experimentation, and to tighten the rules with regard to captive-bred primates.

In all cases, the government's inspectors have to be convinced that no less severe means are available. They consider "the three R's": Reduction, Replacement, and Refinement (J. A. Smith and Boyd 1991). The second of these is relevant because the possible alternatives may not involve any animals at all. Sometimes, experiments can be done *in vitro* using tissue cultures, or perhaps even *in silico*, using computer simulations of metabolic functions (Balls 1983; and see 15.viii.b). A charity called FRAME (Fund for the Replacement of Animals in Medical Experiments) has been sponsoring the development of such techniques for several decades.

Some questions, of course, can be answered only by vivisection—and some of these are considered sufficiently important, in scientific and/or medical terms, to be allowed under the law. Most British physiologists today, despite occasional grumbles about the bureaucratic delays and paperwork involved (more burdensome than in any other country), are content to have an Act of Parliament that protects animals from unjustifiable suffering. As explained in Section ii.d, however, we'll never know for sure whether Descartes himself would have seen any need for the Act, or for FRAME.

b. Holist chemistry

In the early days of scientific physiology, even the committed experimentalists weren't all unremitting mechanists, as Descartes was. Many of them doubted whether physics and chemistry could suffice to explain living processes.

This doubt was an intellectually respectable position in the seventeenth century, and wasn't unreasonable even in the nineteenth. The chemistry of respiration would take 200 years to clarify. And the familiar phenomenon of 'animal heat'—the ability of living animals to conserve a body temperature different from (and often higher than) that of their environment—appeared actually to contravene the laws of physics (Goodfield 1960, chs. 6 and 7). In general, the self-organizing properties of biological creatures were highly mysterious (and they still aren't fully understood: see Chapter 15).

Small wonder, then, that several versions of vitalism flourished long after Descartes's death (Lenoir 1982). Some were derived from philosophical idealism, gaining popularity (in Germany and elsewhere) in the form of *Naturphilosophie* (see Section vi). These posited a vaguely specified vital principle, animating the living body to give it capacities different from those of the inorganic world. Occasionally, chemistry was even said to be utterly irrelevant to biology.

Other forms of vitalism posited an additional (vital) force fully consistent with physics and chemistry (perhaps even obeying something like a Newtonian inverse-square law), but not reducible to them. Sometimes, a mysterious power of self-organization was ascribed to *all* matter, by analogy with crystal growth (T. Brown 1974). Although this was a retreat from Descartes's mechanism, it wasn't intended as a rejection of materialist science in general. It was comparable, rather, to late twentieth-century attempts to 'informationalize' matter and/or to imbue it with some primitive form of consciousness (Chapter 14.x.d).

With advances in chemistry and experimental physiology, vitalism of both types (and especially the first) became less common. Nevertheless, it survived—even among people using detailed chemical knowledge to advance the study of living things.

For example, in 1842 the organic chemist Justus von Liebig (1803–73) published a ground-breaking book on *Animal Chemistry*. This hugely clarified the processes of digestion and respiration. In the same book, however, he argued that life involves a "vital force", one which—like the known forces in physics and inorganic chemistry—must be regulated by certain laws. This "independent force", he said, is responsible for the growth and maintenance of the living creature's bodily matter and form:

The vital force *causes a decomposition* of the constituents of food, and *destroys the force of attraction* which is continually exerted between their molecules; it *alters the direction of the chemical forces* in such wise, that the elements of the constituents of food *arrange themselves in another form* . . . it forces the new compounds to assume forms *altogether different* from those which are the result of the attraction of cohesion when acting freely . . . [By] its presence in the living tissues, their elements *acquire the power of withstanding* the disturbance and change in their form and composition, which external agencies tend to produce; *a power which, simply as chemical compounds, they do not possess.* (quoted in Goodfield 1960: 137; italics added)

Many vitalists were fundamentally sympathetic to mechanism, but faced apparently insuperable problems in applying it to life. With respect to living bodies, they would have been undiluted (or Newtonized) Cartesians if they could. And, after the work of the physiologist Claude Bernard, discussed in Section vii.a, they *could*—at least with respect to fully developed adult animals. But Liebig was different.

For all his superb experimentation, Liebig's view of organic chemistry, and of its place in nature, was holistic rather than analytic. For instance, he stressed its agricultural and even ecological aspects, rather than following the majority of his professional peers in studying a host of increasingly specialized laboratory reactions (Merz 1904–12: ii. 394–6). And, as we've seen, he was convinced that some vital principle could actually override chemical mechanisms.

Liebig's holism, and his suspicion of purely chemical accounts of life, dated from his youth. As a young man, he was one of many German scientists profoundly influenced by a fundamentally non-mechanistic philosophical movement, neo-Kantian idealism. As remarked above, this was one of the two opposing world-views that eventually developed from Descartes's work.

2.vi. The Neo-Kantian Alternative

Neo-Kantian ideas were hugely influential in *humanistic* circles in the early nineteenth century. But even at the height of their popularity, they were less prominent in physiology than Cartesian mechanism was.

That's even more true today. Nonetheless, they've surfaced in various areas of cognitive science. For example, some cyberneticians—for instance, Pierre de Latil (4.v.a)—recommended the Kantian account of "organisms" as systems following *internal* laws of order, or organization, so functioning at the same time as an end and as a means. Linguistics (and NLP—natural language processing) has engaged with, and to some extent supported, Wilhelm von Humboldt's neo-Kantian account of language (Chapter 9.iv and x). And A-Life includes a number of people sympathetic to neo-Kantian views of biology (Chapters 15.iii and vii, and 16.x). Moreover, such views underlie a fundamental critique of orthodox cognitive science that's now arising from several sources (see 13.ii.b–e, 15.viii.c, and 16.vi–viii).

For our purposes here, the crucial ideas concern epistemology, the philosophy of science, and biological holism. All three topics were discussed by the key writer in this philosophical tradition, Immanuel Kant (1724–1804).

a. Kant on mind and world

Kant was rooted in Descartes, but grew radically critical of him. For he'd tried to solve a central problem in Cartesian philosophy, one that had become clearer with the development of empiricism through the eighteenth century—especially, with the work of David Hume (1711–76).

According to Descartes, our conscious perception represents, and to a significant extent corresponds with, external (material) reality. Its disciplined application in science helps us to see where it's reliable (for instance, when it represents material things as having size and shape) and where it's misleading (for instance, when it represents them as having colour). Some non-correspondences, such as colour, have systematic links with the real ("primary") qualities of underlying reality. Science is thus a *realist* enterprise: it tells us about the external world, which exists independently of human beings—and, more to the point, of human perceptions.

But, given this representational theory of mind, our senses can give us only indirect knowledge of the material world. In other words, we can't get beyond our conscious thought, to see the world directly—or to check the correspondence between our empirical observation and the real world. As Addison put it (in explaining Locke's version of this doctrine) in *The Spectator*:

> Our Souls are at present delightfully lost and bewildered in a pleasing Delusion, and we walk about like the Enchanted Hero of a Romance, who sees beautiful Castles, Woods, and Meadows . . . but upon the finishing of some secret Spell, the fantastick Scene breaks up, and the disconsolate Knight finds himself on a barren Heath, or in a solitary Desart. (Addison and Steele 1712: 546–7)

For Descartes, there was only one way of avoiding this bleak denouement. Ultimately, he said, we have to rely on God's goodness in not misleading us:

> For since He has given me . . . a very great inclination to believe that [my sensory experiences] are conveyed to me by corporeal objects, I do not see how He could be defended from the accusation of deceit if these ideas were produced by causes other than corporeal objects. Hence we must allow that corporeal things exist.

This position was unstable. It led eventually to the empiricist Hume's scepticism about the existence of the external world and the reliability of causal relations. Kant, saying that Hume had "roused me from my dogmatic slumbers", tried to restore philosophical respectability to science. Specifically, he tried to rescue Newtonian physics.

The rescue of science from Hume's scepticism was to be achieved by a "Copernican revolution" in epistemology (Kant 1781). Kant claimed that the objectivity of physics lies not in its veridical representation of an external reality, but in the agreement among scientists that reality is to be described in certain (basically Newtonian) ways. In other words, its "objectivity" is actually inter-subjectivity.

But this agreement, according to Kant, isn't a matter of convention, or choice. The "intuitions" of space and time, and the basic "categories" (such as identity and causation) of science and of empirical perception in general, aren't arbitrary. On the contrary, they're built into the human mind a priori. We cannot help but perceive the world as structured by these principles. As Kant put it: "Categories are concepts which prescribe laws *a priori* to appearances, and therefore to nature, the sum of all appearances" (1781: 102).

A single-paragraph digression, not important at this point in our story but crucial for later chapters: To explain how intellectual concepts and abstract categories can be applied to experience, Kant posited "schemata". A schema isn't a phenomenal image, but *a capacity to form images* of a certain type. So someone who can apply the concept of a triangle to a variety of triangular things does so by way of a schema. Even the highly abstract categories—cause, substance, necessity—he said, are applied to experience by way of (transcendental) schemata. For example:

> The schema of cause . . . is the real upon which, whenever posited, something else always follows. It consists, therefore, in the succession of the manifold [experience], in so far as that succession is subject to a rule. (1781: B183–4)

Kant's contemporaries and immediate successors paid less attention to his notion of "schema" than to his position on objectivity and the real world. But it was destined to play a central role in cognitive science. It was picked up, and richly developed:

* by the neurologist Sir Henry Head in 1911 (see Chapter 4.vi.a);
* next, by the psychologist Sir Frederic Bartlett in 1932 (5.ii.b);
* by his student Kenneth Craik, who renamed it *model*, in 1943 (4.vi);
* by the neurophysiologist Warren McCulloch (*neural nets representing universals*) in 1947 (12.i.c);
* and by cognitive scientists such as Robert Abelson (*script*), Marvin Minsky (*frame*), and Michael Arbib (back to *schema* again) in the 1960s (7.i.c, 10.iii.a, and 14.vii.c respectively).
* It was also resurrected within 1960s philosophy of mind, which depicted the mind as a set of *functional capacities* (16.iii–iv)—making "experience as such" (*qualia*) philosophically problematic as a result (14.x–xi).

In short, people in cognitive science today who speak of mental models, schemas, or representations, and especially those (e.g. the evolutionary psychologists) who regard these as inborn, are—knowingly or unknowingly—echoing Kant (7.vi.d–f, 10.iii.a, 12.x, and 14.vii.c and viii; cf. Brook 1994). Indeed, many *do* do so knowingly: Kant's name is often mentioned.

As just remarked, however, Kant's contemporaries were more concerned with his claims about the metaphysical and epistemological status of the real world. According to him, there is indeed something—the "thing-in-itself"—which really exists outside us, completely independent of our thinking. But we can know nothing about it as it exists in itself:

> Things in themselves would necessarily, apart from any understanding that knows them, conform to laws of their own. But appearances are only representations of things which are unknown as regards what they may be in themselves. As mere representations, they are subject to no law of connection save that which the connecting faculty prescribes. (Kant 1781: 103)

In short, all our concepts of the external world are informed by the empirical intuitions and categories with which the mind is imbued. Other rational creatures, if such there be (Martians? angels?), might have a different set of empirical principles; but these are of necessity inconceivable to us.

Kant had intended his philosophy to save Newtonian science from Humean scepticism, and from arbitrary relativism. But in extending Descartes's stress on the epistemological primacy of consciousness, he offered hostages to absolute idealism.

If the thing-in-itself is unknowable, why posit it at all? And if our concepts of reality are inescapably mind-based, why not focus on the constructive activities of the mind—instead of on the 'real' world? This epistemological/metaphysical volte-face was the seed of the counter-cultural anti-realism that beleaguers science today (see Chapter 1.iii.b–c). But before it could flower in the form of today's social constructivism, it had nearly 200 years to develop.

Inevitably, the late eighteenth and early nineteenth centuries saw the rise of a variety of neo-Kantian idealist philosophies. These all stressed the creative power of mind (and life) and the epistemologically secondary nature of materialist science.

Most neo-Kantian philosophers reacted also against the atheistic rationalism of the Enlightenment. They were drawn more to religion and spirituality than to science, and more to (their interpretation of) biology than to physics (see below). They favoured intuition, imagination, and the specific individual—whether person or culture—over logic, experiment, and universal law. And many viewed the whole of Nature as a sort of organism, imbued with something akin to intelligence. Many, too, saw Nature as a system akin to a developing embryo, expressing the self-creative power of the divine.

Accordingly, even Newtonian physics, with its picture of inert matter pushed here and there by external forces, became suspect. Electricity—with its capacity to repel as well as to attract—was thought to be a more crucial, more philosophically promising, phenomenon than billiard balls. A fortiori, none of these idealistically inclined thinkers would have allowed that the nature of any living thing could be illuminated by comparing it to a man-made machine.

An example of neo-Kantianism focused on the creative power of language will be discussed in Chapter 9.iv. Others feature in subsections c–f, below. Before we can understand where they were coming from, however, we have to consider what Kant said about biology.

b. Biology, mechanism, teleology

A hundred years after its publication, Newton's *Principia* was still unrivalled as a theory of the inanimate world. As we've seen, Kant himself regarded it as an a priori aspect of human understanding. But he insisted that the animate world had to be understood differently (Korner 1955: 180–2, 196–214; Grene and Depew 2004: 92–127). Indeed, he discussed them separately. Whereas he justified physics in his *Critique of Pure Reason* (1781), he discussed biology only later, in the *Critique of Judgment* (1790, pt. 2)—which was concerned with aesthetics and teleology.

The life sciences, according to Kant, weren't on a par with physics. On the one hand, there could never be a scientific (mechanistic) *psychology*, because of the "autonomy" of human consciousness and voluntary action. On the other, there couldn't be a wholly mechanistic *biology*, because the purposive self-organization of living things—although effected by Newtonian processes—can't be analysed in Newtonian terms. Living things are purposive wholes, which means not that they are designed for a purpose but that their parts can't be properly understood without being (teleologically) related to the whole organism.

Kant didn't deny the value of the currently rising science of physiology. To the contrary, he declared as a guiding maxim that "All production of material things and their forms *must be considered as* being possible in accordance with merely mechanistic laws" (italics added). Nevertheless, he also declared a second maxim, that "Some products of material nature *cannot be considered* in accordance with *merely* mechanistic laws (their consideration requires an altogether different law of causality, namely, that of final causes)" (italics added). In other words, living organisms function in accordance with physics, but can't be explained solely by physical causes. They are *purposive* beings, requiring teleological explanation. Their purposes aren't relative to those of some other creature (like the purpose of a tool) but are inherent in the organism itself, which is therefore not a means to an end but an end-in-itself.

In saying this, Kant *wasn't* claiming that, at the metaphysical level, there are both mechanistic and teleological causes. Rather, he was saying that although the whole of biology runs by purely mechanistic causation, *our thinking of it* must also bear biological purposes in mind: mechanism as metaphysics, teleology as method (Korner 1955: 209). Partly, he said, this was helpful because thinking about the purpose of an organ might lead to mechanistic hypotheses about how it works ("For where purposes are considered as the conditions of the possibility of certain things, means have to be assumed . . ."). More importantly (for him), viewing living things as ends-in-themselves demands a certain—non-exploitative—moral attitude towards them.

Significantly, he wasn't sure just how far a mechanistic biological science could reach. He made two remarks about this which would later resonate strongly, both with his followers in the early nineteenth century and with modern biologists of various stripes (see below, and Chapter 15.iii, viii, and ix.c):

> When we consider the agreement of so many genera of animals in a certain common schema, which apparently underlies not only the structure of their bones, but also the disposition of their remaining parts, and when we find here *the wonderful simplicity of the original plan, which has been able to produce such an immense variety of species* by the shortening of one member and the lengthening of another, by the involution of this part and the evolution of that, there gleams upon the mind a ray of hope, however faint, that *the principle of the mechanism in nature, apart from which there can be no natural science at all*, may yet enable us to arrive at some explanation in the case of organic life. (Kant 1790: 418; italics added)

> [This hypothesis, which has probably occurred to most acute scientists, is daring but not absurd—like] the generation of an organized being from crude inorganic matter . . . . [It's not absurd to suppose that] certain water animals transformed themselves by degrees into marsh-animals, and from these after some generations into land animals. In the judgment of plain reason there is nothing *a priori* self-contradictory in this. But experience offers no example of it. (Kant 1790: 420)

He might have added that no one had managed to suggest a mechanism by which such evolutionary changes could take place. That, of course, is what Charles Darwin (and modern genetics) eventually provided.

Meanwhile, prior to Darwin, Kant's followers—especially Goethe (d–f, below)—focused less on the second quotation above than on the first. (And few people, if any, disagreed with him that the self-organization of life from crude matter was "absurd": see Chapter 15.x.b.)

c. Philosophies of self-realization

One of the neo-Kantian movements mentioned at the end of subsection a was the anti-rationalist *Naturphilosophie*, a form of German idealism espoused by Friedrich von Schelling (1775–1854), and others.

Kant himself, as we've seen, had argued that there could never be a mechanistic biology, because living things—being both causes and effects of themselves—are holistic teleological systems that can't be understood in purely physical terms. As he put it, they are self-organizing systems that propagate their own organization, and generate their own purposes, or ends. The followers of *Naturphilosophie* agreed, and took this view even further.

Their positions differed, some being more consonant with experimental physiology than others (Lenoir 1982; Cunningham and Jardine 1990). In general, however, they regarded life, adaptation, embryological development, and the origin of species as aspects of the self-creation of a divine mind immanent in nature. Most of them didn't believe in evolution, but those who did saw it also as due to some self-realizing creative force. And most of them argued for hylozoism: the view, now revived as the Gaia hypothesis (Lovelock 1988), that the earth as a whole is an organism.

The 'father' of this movement was Schelling, whose books *Ideas on the Philosophy of Nature* and *On the World Soul* were published at the close of the eighteenth century. Schelling saw the whole of Nature as infinite self-activity, an organic system constantly striving towards self-realization—an idea applied to *history* by his close friend Georg Hegel. As Schelling put it: "Nature is visible Spirit, Spirit is invisible nature."

He argued that all physical forces manifest the same "pure activity" of self-realizing Nature. Nature, he said, is—indeed, can only be—a balance of opposed forces (an idea inherited from Kant). So our "first principle" in studying it must be to "go in search of polarity and dualism throughout all nature" (quoted in Stern 1988, pp. ix–x). Since the dualism was a matter of balance, not absolutist metaphysics, he regarded knowledge as fundamentally identical with willing, and action with perception. And he saw art as superior to science. For the self-realizing aesthetic intelligence, being free of all abstraction, actually creates the world (Margoshes 1967). (This was a Romantic version of the *verum factum* tradition: see Chapter 1.i.b.)

Schelling was enormously influential in non-scientific circles. He is 'the' philosopher of Romanticism, and a crucial predecessor of existentialism—and of Freudian psychodynamics. But despite his prioritizing of art, he had some influence on science too.

To be sure, his more mechanistically minded contemporaries would have endorsed a much later judgement of his *Naturphilosophie*: that it was "fantastic to the verge of insanity" (Singer 1959: 385). Nevertheless, it had a significant impact on science in Germany, and elsewhere (Lenoir 1982; Merz 1904–12, vol. i, ch. 2; vol. ii, ch. 10). Darwin's contemporary Richard Owen was a zoologist in this neo-Kantian tradition. And the chemist Liebig, as remarked in Section v.b, adopted its holism and its stress on the uniqueness of life.

Not least, Schelling had a sympathetic hearing also from Goethe (1749–1832), who ensured that he was appointed to a university chair.

d. Goethe, psychology, and neurophysiology

Today, Goethe is remembered chiefly as a Romantic poet, or as yet another pantheistic neo-Kantian idealist. He would have demurred on two counts.

First, he didn't see himself as a Romantic: he regarded most Romantic artists as third-rate poseurs, and described the movement itself as a "disease" (I. Berlin 1999: 112). More to the point, he was keenly interested in scientific questions. Indeed, he regarded his scientific and literary works as equally important—and criticized the heavy arts-bias of Romanticism, accordingly.

One might almost say that, like Charles Snow (1959) after him, he lamented the split between "the two cultures". But that would be anachronistic. Certainly, Goethe had little patience with Romanticism's dismissal of science—understood as the systematic observation and study of the natural world. But he had even less with the analytical empiricism that Snow understood to constitute "science". (Hence his lack of intellectual sympathy for Vaucanson's duck.)

His own approach was relentlessly holistic. He was deeply suspicious of the Galilean–Cartesian programme of mathematizing science, and favoured careful—respectful—observation of the phenomena of Nature over analytic experimentation on them.

It's no accident, then, that the most detailed modern commentary on Goethe's science was written by an ex-student of the physicist David Bohm, whose *Wholeness and the Implicate Order* became something of a cult book in the 1980s. As Henri Bortoft (1996: 258 ff.) puts it, "the difference between a genuinely holistic perspective [such as Goethe's] and the analytical counterfeit" is that the former enables one to see "multiplicity in the light of unity, instead of trying to produce unity from multiplicity". We can recognize, indeed glory in, the rich diversity of the One instead of reducing all difference to uniformity.

So, for instance, in the anthropology of religion (my example, not Goethe's: see Chapter 8.vi.d–e), we can explore the many culturally different ways in which certain universally shared psychological principles come to be expressed, instead of reducing all finalized religions to the activation of one (or several) psychological module/s. Or, to take an example discussed at length by Goethe himself, we can see all the different individual plants as members of (better: expressions of) the one species, or class, instead of focusing on individual plants and seeking sets of shared properties in terms of which to define the class.

As for how we arrive at the concept of the species/class, if not by analytic property-counting, Goethe argued that this could be done by an act of intuitive perception in which we actually participate in the natural phenomenon we're studying. If that sounds overly mystical (and to orthodox scientific ears, it does), we'll consider some examples below.

Because of his holism, Goethe downplayed the relevance of experimental physiology much as Cuvier had done (see Section v). He anticipated some of Schelling's ideas about living organisms (and hylozoism). His insistence on the fundamental unity of natural phenomena was distilled in two philosophical concepts: "primal polarity" (similar to Schelling's explanatory dualism) and "primal phenomena". In applying these concepts to various scientific areas, he produced theories very unlike the empiricist norm.

Most cognitive scientists have read not one word of Goethe's science—and if they did, they'd probably recoil with horror. Nevertheless, his emphasis on painstaking observation, as applied to his own subjective experiences, made him an important forerunner of Gestalt psychology and of the phenomenological movement in Continental philosophy (see Chapters 5.ii.b and 16.vi–viii). He also anticipated a type of biological explanation (i.e. morphological explanation) that's gaining influence today (15.ix.c). And he's sometimes quoted by cognitive scientists and philosophers committed to the dynamical systems approach (15.viii.b–c and ix, and 16.x.a).

That's not to say that his detailed arguments were acceptable, or repeated by his twentieth-century successors. Important scientific and/or philosophical insights may well be introduced on unconvincing grounds, to be widely dismissed—even ridiculed—until better arguments are found.

For example, Goethe used his notion of primal polarity in attacking Newtonian physics—and excoriating Newton himself. He offered (in 1791) a fundamentally non-mechanistic account of the optics and perception of colour (Boring 1942: 112–19; Magnus 1906: 100–50). His optics was 'non-mechanistic' in being based on the qualitative aspects of visual perception, rather than mathematical analysis of the physical properties of light. A defender might say that he was addressing (psychophysiological) questions that Newton had not asked. But he didn't put it that way: he saw his optics as an alternative to Newton's, not a complement to it.

So he denied that white light is made up of the seven spectral colours. He argued instead that it's a fundamental, and unanalysable, aspect of the world. Newton's prism, he said, was a medium that led the polar opposites of light and dark to cooperate so as to produce colours. And this cooperation involves the activity of the eye: whenever the eye perceives a colour, it demands the complementary colour. He argued that there are two primary colours, yellow and blue, to which all the others are related. His polar distinction between yellow and blue was based on subjective criteria, not on optics. It was comparable (he said) to other dichotomies such as bright–dark, warmth–cold, active–passive, proximity–distance, and even acid–alkali.

Not surprisingly, mechanists who based their work on physics, or who aimed to do so, had scant sympathy with this subjectivist approach. But Goethe persisted, publishing (in 1810) a two-volume treatise on colour running to 1,411 pages. A leading historian of psychology has described this book as "an example of how personal pride distorts the use of evidence and how frustration induces scientific activity" (Boring 1957: 99).

Despite such charges of "distorting" the evidence, Goethe's treatise did have some influence in mechanistic physiology. For his extensive and subtle descriptions of colour experiences identified many important, and previously unnoticed, visual phenomena. These prompted some physiologists to work on the perception of colour.

One of these was Johannes Müller (1801–58)—yet another person, besides Harvey and Descartes, sometimes described as the father of experimental physiology. That's largely because of his monumental *Handbook of Human Physiology*, published between 1833 and 1840. As his handbook clearly showed, Müller was working within the mechanistic tradition.

But his mechanism, like that of so many others at the time, was qualified. Not only did he believe in mind-to-body causal interaction, but he was a vitalist. He couldn't endorse the project solemnly announced in 1845 by four of his students, to try to explain all living processes in terms of physicochemical laws (Boring 1957: 708). Two of those students were destined to have a huge influence in the gradual mechanization of mind. One was Helmholtz, who was soon to publish his paper on the law of conservation of energy. Another was Ernst Brücke, a future teacher of Sigmund Freud—who always insisted that there must be some mechanistic base to psychological phenomena (see Preface, ii, and 5.ii.a).

Müller's doctrine of "specific nerve energies" (see Section viii.a) was in part an attempt to deal mechanistically—and non-idealistically—with the subjective aspects of colour vision. The patterns in various types of nerve, Müller argued, cause different sorts of experience, and are themselves systematically caused by distinct types of stimulation originating in the external world. Here he was echoing Descartes, who had posited seven types of sensory nerve (see Section iii).

This approach was developed further by Helmholtz, who posited three types of colour receptor in the retina (Helmholtz 1860). Indeed, Helmholtz described Müller's law as comparable in importance—for psychophysiology in general—to Newton's law of gravitation. He also saw it as "the empirical exposition of the theoretical discussion of Kant on the nature of the intellectual process of the human mind" (from the *Handbuch der Physiologischen Optik* of 1896, p. 249; quoted in Merz 1904–12: ii. 483).

Nevertheless, a decade after Goethe's death, Helmholtz poured scorn on Goethe's ideas on psychophysics. He gave a lecture in which he echoed "the denunciation heaped by all physicists on [Goethe's] researches in their department, and especially on his 'theory of colour' " (Helmholtz 1853: 34).

Helmholtz justified this denunciation at length. He pointed out that, in his anti-Newtonian experiments (using a prism), Goethe had been hampered by impure colours and faulty apparatus. He described Goethe's account of the physical phenomena as "absolutely irrational" (p. 50). And he countered Goethe's subjectivist theory with a robust defence of man as machine:

> Even nature is, in the poet's eyes, but the sensible expression of the spiritual. The natural philosopher, on the other hand, tries to discover the levers, the cords, and the pulleys which work behind the scenes, and shift them. Of course the sight of the machinery spoils the beautiful show, and therefore the poet would gladly talk it out of existence, and ignoring cords and pulleys as the chimeras of a pedant's brain, he would have us believe that the scenes shift themselves, or are governed by the idea of the drama. (Helmholtz 1853: 50)

Helmholtz's point wasn't that *only* lever-and-pulleys explanations are allowed, for he himself posited "unconscious inferences" in perception (see Chapter 6.ii.e). These were needed, he argued, to compensate for the incompleteness of the stimulus. For instance, any given viewpoint will show only one face of a three-dimensional object, and one object may be part-hidden by another—yet we normally have no difficulty in recognizing them. However, Helmholtz took it for granted that those inferences are somehow grounded in "machinery", alias neurophysiology.

Scorning Goethe's "resolute hostility to the machinery that every moment threatens to disturb his poetic repose", he ended by saying:

we cannot triumph over the machinery of matter by ignoring it . . . We must familiarize ourselves with its levers and pulleys, fatal though it be to poetic contemplation.

One could be forgiven for inferring that Helmholtz thought Goethe worthless as a scientist, except perhaps as an observer of visual phenomenology or as an intellectual gadfly. But one would be wrong. In the same breath as dismissing Goethe's "egregious failure" in the psychophysiology of vision, Helmholtz referred to his "immortal renown" in another branch of biological science—namely, morphology.

e. The birth of morphology

Morphology, a word coined by Goethe, is the study of organized things. It concerns not just their external shape, but also their internal structure and development—and, crucially, *their structural relations to each other*.

Goethe's morphology was much wider in scope than the biological taxonomies of Carl Linnaeus and Cuvier. For he intended it to cover both living and inorganic nature, even including crystals, landscape, language, and art. But Helmholtz's interest, like ours, was with its application to biology.

Putting it that way, however, is anachronistic—and underplays Goethe's originality. For it wasn't yet clear that there's a unified subject, here. The inclusive term *biology*, meaning the life sciences in general, wasn't coined until 1796 (by which time Goethe was nearing 50), and was widely accepted only after Darwin. And not until the 1840s, well after Goethe's death, was it clear that *both* plants and animals are made of cells (Merz 1904–12: i. 193–5). In short, the fundamental unity of animals and plants wasn't yet clear in Goethe's lifetime.

Nevertheless, he applied similar ideas to both. In his *Essay on the Metamorphosis of Plants* (1790), he argued that superficially different parts of a flowering plant—such as sepals, petals, and stamens—are derived by transformations from the basic, or archetypal, form: the leaf. Later, he posited an equivalence (homology) between the arms, front legs, wings, and fins of different animals. All these, he said, are different transformations of the forelimb of the basic vertebrate type. And all bones, he claimed, are transformations of vertebrae.

In other words, he combined meticulous naturalistic observation with a commitment to the fundamental unity of nature. For instance, he's widely credited with a significant discovery in comparative anatomy. Namely, that the intermaxillary bone—which bears the incisors in a rabbit's jaw—exists (in a reduced form) in the human skeleton, as it does in other vertebrates. (Strictly, he *rediscovered* this fact (Sherrington 1942: 21–2), and *restated* the claim that sepals are a type of leaf (Goethe 1790: 73): cf. Chapter 1.iii.f.) The issue was "significant" because some people had used the bone's seeming absence to argue that God created a special design for human beings, marking them off from the animals. Goethe, by contrast, related human skulls to the archetypal vertebrate skull, much as he related sepals to the archetypal leaf.

Goethe himself didn't think of morphological transformations as temporal changes, still less as changes due to Darwinian evolution—which was yet to be defined. Rather, he saw them as abstract, quasi-mathematical, derivations from some Neoplatonic ideal in the mind of God. But these abstractions could be temporally instantiated.

So in discussing the development of plants, for instance, he referred to actual changes happening in time as the plant grows. He suggested that sepals or petals would develop under the influence of different kinds of sap, and that external circumstances could lead to distinct shapes, as of leaves developing in water or in air (see 15.ii). Similarly, some of the other Naturphilosophen spoke of temporal transformations in the embryo, as it ascends through the scale of animal being from animalcule, to mussel, to fish, to mammal. But they weren't evolutionists, so didn't—as the fervent mechanist Ernst Haeckel (1834–1919) later did—gloss this as "ontogeny recapitulating phylogeny".

The point of interest for our purposes is that Goethe focused attention on the restricted range of basic forms (primal phenomena) in the organic world. He encouraged systematic comparison of them, and of the transformations they could support. He also suggested that only certain forms are possible: we can imagine other living things, but not just *any* life forms. In a letter of 1787, he wrote:

> With such a model [of the archetypal plant (*Urplanz*) and its transformations] . . . one will be able to contrive an infinite variety of plants. They will be strictly logical plants—in other words, even though they may not actually exist, they could exist. They will not be mere picturesque and imaginative projects. They will be imbued with inner truth and necessity. And the same will be applicable to all that lives. (quoted in Nisbet 1972: 45)

Similarly, in his essay on plant metamorphosis (1790), he said: "Hypothesis: All is leaf. This simplicity makes possible the greatest diversity."

Critics soon pointed out that he overdid the simplicity. He ignored the roots of plants, for instance. His excuse was telling:

> It [the root] did not really concern me, for what have I to do with a formation which, while it can certainly take on such shapes as fibres, strands, bulbs and tubers, remains confined within these limits to a dull variation, in which endless varieties come to light, but without any intensification [of archetypal form]; and it is this alone which, in the course marked out for me by my vocation, could attract me, hold my attention, and carry me forward. (quoted in Nisbet 1972: 65)

To ignore apparent falsifications of one's hypothesis, or even challenges to one's general approach, so shamelessly seems utterly unscientific in our Popperian age. And some of Goethe's contemporaries complained about it, too.

But his attitude stemmed from his idealist belief in the essential unity of science and aesthetics. He even compared the plant to a superb piece of architecture, whose foundations—the roots—are of no interest to the viewer. More generally: "Beauty is the manifestation of secret laws of nature which, were it not for their being revealed through beauty, would have remained unknown for ever" (quoted in Nisbet 1972: 35). For Goethe, this language had an import much richer than the familiar appeals to theoretical simplicity, symmetry, or elegance. It underlay, for example, his rejection of Newton's optics and his stress on the details of visual phenomenology.

Questions about such abstract matters as the archetypal plant were very unlike those being asked by most physiologists at the time. If a body is not just a flesh-and-blood mechanism, but a transformation of an ideal type, how it happens to work—its mechanism of cords and pulleys—is of less interest than its homology.

Indeed, for the holist Goethe the mechanism may even depend on the homology. Perhaps a certain kind of sap, a certain chemical mechanism, will induce a primordial plant part to develop into a sepal rather than a petal. But what's more interesting—on this view—is that sepals and petals are the structural possibilities on offer. How one describes the plant or body part in the first place will be affected by the type, and the transformations, supposedly expressed by it. It's not surprising, then, that Goethe was out of sympathy with the analytic, decompositional methods of empiricist experimentalism.

Initially, Goethe's morphology attracted scepticism even from descriptive (non-experimental) biologists. This was partly because of the poetical manner in which he wrote. And his close association with *Naturphilosophie*, not to mention his bizarre account of optics, didn't help.

But in 1830, two years before his death, his morphological ideas were publicly applauded by a highly respected biologist, Étienne Geoffroy Saint-Hilaire (Merz 1904–12: ii. 244). Geoffroy agreed with Goethe that comparative anatomy should be an exercise in "rational morphology", a study of the successive transformations—rational, not temporal—of basic body-plans.

Largely because of Geoffroy's influence, Goethe's ideas on morphology were cited approvingly after his death by a number of leading scientists. These included Haeckel and Thomas Huxley (1825–95), and even the self-proclaimed mechanist Helmholtz. Indeed, in the lecture quoted above, Helmholtz credited Goethe with "the guiding ideas [of] the sciences of botany and anatomy . . . by which their present form is determined", and praised his work on homology and transformation as "ideas of infinite fruitfulness" (Helmholtz 1853: 34, 30).

f. Goethe's eclipse

"Infinite fruitfulness" isn't on offer every day. So why were Goethe's ideas largely forgotten by the scientific community? Surely, such an encomium from such a high-profile scientist—and committed mechanist—as Helmholtz would be enough to guarantee close, and prolonged, attention?

Normally, yes. However, only six years after Helmholtz spoke of Goethe's "immortal renown" in biology, Darwin (1809–82) published *On the Origin of Species by Means of Natural Selection* (1859). This radically changed the sorts of enquiry that biologists found relevant. One might even say that they changed the sorts of enquiry that biologists found *intelligible* (see N. Jardine 1991).

Biological questions were now increasingly posed in ways that sought answers in terms of either mechanistic physiology or Darwinian evolution—that is, "descent with modification". (Darwin avoided the term "evolution", which was widely used to posit Lamarckian change in biology or to refer to embryonic development.) After the turn of the century, genetics soon became an additional source of biological enquiry. This might have happened earlier, but the work on hereditary "factors" of Gregor Mendel (1822–84) remained unknown until it was rediscovered, in 1900, by the founders of modern genetics.

The mix of physiology, evolution, and genetics was a heady brew. It quickly became the biological orthodoxy, eclipsing *Naturphilosophie* in all its forms—Goethe included.

One might wonder at the speed with which this happened. After all, Darwin himself was unsure of the mechanism of heredity, and accepted the evolutionary theory of

Jean-Baptiste Lamarck (1744–1829), who taught that acquired characteristics can be inherited. The mentalistic form of Lamarckism, which suggested that creatures can successfully strive for greater adaptation, was broadly consonant with *Naturphilosophie*. It fitted well with its vitalism, if not with its non-evolutionary structuralism. But even some of Goethe's sympathizers rejected Lamarck's theory. Geoffroy, for instance, criticized not just the mentalism but the adaptationism too. Moreover, Lamarckism was discredited ten years after Darwin's death, when August Weismann (1834–1914) attributed heredity to the cell nucleus, or "germ plasm", as opposed to the cell body. If acquired changes to the cell body weren't inheritable, Lamarck must have been mistaken.

In the late nineteenth century, then, *Naturphilosophie* as a form of biology was first cast into the shadows, and soon virtually eclipsed. To be sure, a recent bibliography lists over 4,500 titles on Goethe *as a scientist* (Magnus 1906/1949, pp. xiii, 249–53). But many of these were old and/or written by literary scholars rather than scientists. The life sciences were widely reinforced or reinterpreted as neo-Cartesian, *not* neo-Kantian, projects. Darwin encouraged systematic comparisons between different organs and organisms, as Goethe had done. But Darwin posited no ideal types. He explained morphological similarity in terms of contingency-ridden variation and selective descent, or coincidental likeness between environmental constraints.

In short, morphological—as opposed to metabolic—self-organization largely *disappeared* as a scientific problem.

That's not to say that the problem had been solved. It still survived in embryology (see Section vii, below). Moreover, Goethe's morphology was to be revived by the biologist D'Arcy Thompson in 1917, and is sympathetically regarded by some developmental biologists today (see Chapter 15.iii, and Webster and Goodwin 1996, esp. chs. 1 and 5). So Charles Sherrington's comments that "were it not for Goethe's poetry, surely it is true to say we should not trouble about his science", and that metamorphosis is "no part of botany today" are less true now than they were when he made them, some sixty years ago (Sherrington 1942: 23, 21).

Nevertheless, Goethe is still only a minority taste. From the 1860s onwards, his scientific work was sidelined, even scorned. Instead, Darwin and Bernard were paramount.

2.vii. The Self-Regulation of the Body

At much the same time as Darwin was preparing to publish on evolution, some other central mysteries regarding biological self-organization were being solved in essentially Cartesian terms. The leading figure, here, was the medic and physiologist Claude Bernard (1813–78).

a. Automatic equilibria

Bernard was hugely influential in both the practice and the philosophical justification of experimental physiology. Indeed, he was elected to both French Académies, of science and humanities. He argued that the science of physiology should underlie biology in general, and medicine in particular.

Like Descartes, he described an organism as "a machine which necessarily works by virtue of the physico-chemical properties of its constituent elements" (Bernard 1865: 93). But unlike Descartes, he took seriously the striking differences between the autonomous flexibility of living organisms and the passive predictability of stones, gases, and clocks.

His solution to this seeming paradox wasn't to deny biological determinism, as some of his contemporaries did. Rather, it was to attribute the (low-level) physical variation seen in organisms to (higher-level) adaptive powers of self-regulation found only in living things. Physiology, he argued, requires new concepts not needed in physics or chemistry.

In that sense, and *only* in that sense, he agreed with those who felt that there must be some sort of vital principle. But these new concepts, he argued, are ultimately grounded in those more basic sciences, knowledge of which is essential:

> In a word, biology has its own problem and its definite point of view; it borrows from other sciences only their help and their methods, not their theories. This help from other sciences is so powerful that, without it, the development of the science of vital phenomena would be impossible. (Bernard 1865: 95)

Animal heat, for example, is due—he said—to self-equilibrating mechanisms that continuously regulate the body's response to changing environmental conditions such that body temperature is conserved. In suggesting (and experimentally demonstrating) how such mechanisms might work, Bernard clarified many physico-chemical, and anatomical, details. He explained major aspects of the generation and regulation of body heat, and of digestion and nutrition. Among his many significant discoveries were the function of the vasomotor nerves in varying the rate of blood flow in different parts of the body, and the complex role of the liver in regulating blood sugar levels.

Just as important, and especially relevant for our story, was his general concept of the internal (or microcosmic) environment:

> I believe I was the first to insist upon this idea that there are for the animal really two environments: an external environment in which the organism is situated, and an internal environment in which the tissue elements live . . . The invariability of the internal environment is the essential condition of free independent life: the mechanism which permits this constancy is precisely that which insures the maintenance in the internal environment of all conditions necessary to the life of the elements [the tissue cells] . . . Far from being indifferent to the external world, the higher animal is, on the contrary, narrowly and wisely attuned to it in such a way that, from the continual and delicate compensation, established as if by the most sensitive balance, equilibrium results. (Bernard, quoted in Bodenheimer 1958: 415)

Adding that "these conditions are the same as needed for simple organisms", he suggested that plants and lowly animals have less "freedom" from environmental pressures because their compensating mechanisms are less well developed. But they, too, show the spontaneity characteristic of life.

Bernard was uncompromising about the relevance of physics and chemistry in understanding how such biological autonomy is possible. Unlike von Liebig, he insisted that it can be explained in fundamentally mechanistic terms:

> We cannot, therefore, admit in living organisms a free vital principle struggling against the influence of physical conditions. *The opposite has been proved*, and thus all of the contrary

conceptions of the vitalists are seen to be overthrown. (Bernard, quoted in Bodenheimer 1958: 416; italics added)

b. The embarrassing embryo

In truth, however, the opposite had not been fully proved. Mysteries remained, especially with regard to embryology.

To be sure, some complex, and previously mysterious, physiological phenomena had been explained by Bernard. And it was reasonable to expect that others would eventually be understood in much the same way. Indeed, Walter Cannon's influential work on "homeostasis" in the 1920s would describe a wide range of metabolic processes in terms very similar to Bernard's (Cannon 1926, 1932). Later still, the cybernetic movement of the 1940s would apply similar ideas to control engineering, too (Chapter 4.v–vii).

This last amplification wouldn't have discomfited Bernard. He'd been more than willing to apply his concept of the internal environment to man-made machines:

> We easily understand what we see here in the living machine, since the same thing is true of the inanimate machines created by man. Thus, climatic changes have no influence at all on the action of a steam engine, though everyone knows that exact conditions of temperature, pressure, and humidity inside the machine govern its movements. For inanimate machines we could therefore also distinguish between a macrocosmic environment and a microcosmic environment. In any case, the perfection of the machine consists in being more and more free and independent, so as to be less and less subject to the influence of the outer environment. (Bernard 1865: 98)

But some of Bernard's biological claims were mere statements of mechanistic faith, as most of Descartes's had been. If they now look more plausible than they did when he made them, that's not because of ideas that can be traced directly back to him.

In particular, this was true of his remarks about embryological development:

> But the term "vital properties" is itself only provisional; because we call properties vital which we have not yet been able to reduce to physico-chemical terms; but in that we shall doubtless succeed some day. So that what distinguishes a living machine is not the nature of its physico-chemical properties, complex as they may be, but rather the creation of the machine which develops under our eyes in conditions proper to itself and according to a definite idea which expresses the living being's nature and the very essence of life.
>
> When a chicken develops in an egg, the formation of the animal body as a grouping of chemical elements is not what essentially distinguishes the vital force. This grouping takes place only according to laws which govern the chemico-physical properties of matter; but the guiding idea of the vital evolution is essentially that of the domain of life and belongs neither to chemistry nor to physics nor to anything else. In every living germ is a creative idea which develops and exhibits itself through organization. (Bernard 1865: 93)

Perhaps so—but the nature of this "creative idea" was unknown. Self-organization in general was still not well understood, and biological morphology in particular was a mystery (see Chapter 15).

Consequently, the forms of vitalism that survived Bernard's work—some of which endured well into the twentieth century—drew their plausibility largely from difficulties within embryology and/or evolutionary theory.

The experimental embryologist Hans Driesch (1867–1941), for example, posited teleological "entelechies" guiding the development of the embryo. His theory attracted attention not least because he'd started out as a mechanist but, just before the turn of the century, rejected mechanism for vitalism.

This change was prompted by his experimental results showing (for instance) that each half of the two-cell stage of a sea-urchin egg could, if separated, develop into a whole embryo. Driesch soon abandoned experimental science for philosophy, and gave the first systematic statement of his vitalism as the Gifford Lectures, a series concerned with issues in the philosophy of "natural" theology (Driesch 1908).

But in doing this, he hadn't abandoned *science*. Lord Gifford's brief in founding the Gifford Lectures in 1885 had been clear:

> *I wish the lecturers to treat their subject as a strictly natural science*, the greatest of all possible sciences, indeed, in one sense, the only science, that of Infinite Being, without reference to or reliance upon any supposed special exceptional or so-called miraculous revelation. I wish it considered just as astronomy or chemistry is. (Deed of Foundation, 1885; italics added)

(It's no accident, then, that several scientists cited in later chapters delivered the Gifford Lectures too—including Sherrington, J. Z. Young, Donald MacKay, Christopher Longuet-Higgins, and Arbib. See also Chapter 8.vi.b–d.)

Driesch wasn't cheating. Although he'd rejected mechanist science, he hadn't rejected science as such. Or rather, he hadn't rejected what he understood science to be: the systematic study of natural phenomena, guided by experiment and observation. To describe his vitalism as unscientific, or even anti-scientific—as is often done, today—is anachronistic. For it presupposes a *modern* (neo-Cartesian) concept of science, and of biological science in particular. As we'll see (in Chapter 15.iii and viii), this concept itself is now being questioned. If some vitalists were enemies of science, broadly understood, Driesch was not.

c. Creative evolution

The most influential anti-mechanist account of evolution was developed by the late Romantic philosopher Henri Bergson (1859–1941). In 1907 he posited a mysterious creative principle, or spirit (*élan vital*), which he argued must play a directive role in evolution (Bergson 1911*a*).

In part, Bergson was driven (like Goethe and Driesch) by a recognition of holism. He didn't believe that Darwinian variation and natural selection could explain the wholeness of evolved organisms. In addition, he was puzzled by the appearance of novelty and especially of ever-increasing complexity. If ammonites and barnacles lived, and reproduced, successfully then why should fish and mammals ever arise? His answer was that there's a creative principle present in all living things, which relies on material processes for its expression.

So instead of the Cartesian mind–body dualism, he offered a life–matter dualism. The *élan vital* was something different from matter, but closely interdependent with it.

Mind–body dualism was reinterpreted accordingly (Bergson 1911*b*). Memory, for example, was seen by Bergson not as a store of material traces in the brain, but as enduring patterns of activity constituted by self-organizing associative processes—finding expression in the matter of the brain, but not to be identified with it. As for human consciousness, he often described this as the highest expression of the vital principle—and as the way in which human beings can become aware of it.

We ourselves, he argued, have some experience of the *élan vital* through the consciousness of our existence and self-creation through time. (That sentence is deliberately ambiguous, because Bergson himself wasn't clear about whether consciousness and/or "real duration" were supposed to be separate from the *élan vital*, or distinct aspects of it. Many passages, however, suggest the latter.) We know from experience that human life is becoming, not mere being. It is change, not stasis—nor even repetition. The same applies, he said, but in a lower degree, to other living things. The *élan vital* is "a current of consciousness" that has infiltrated living matter and which passes from one generation to the next by way of the reproductive cells. On this quasi-mystical view, the *élan vital* enables/directs the laws of matter and physical energy to produce novel structures which aren't necessitated by them but are somehow contingent. Complexity increases as biological evolution proceeds because the vital impulse itself consists in a creative potential that constantly strives to find expression in new, and fuller, ways.

Human consciousness, for Bergson, was the highest expression of this impulse so far. And despite repeatedly denying (as Darwin did, too) that evolution is directed to specific, pre-selected, goals he did say that humanity was "prefigured" in the creative evolutionary process at its start. He even said, some years later, that the appearance of human beings is the *raison d'être* of evolution on earth (1946, introd.). Moreover, he believed that essentially (though not superficially) similar beings are prefigured in the evolution that's surely happening on other planets.

Bergson's form of vitalism was far from clear, even when compared with other vitalistic philosophies. We've seen, for example, that the relation between life (*élan vital*) and mind (consciousness) was obscure: they were certainly very closely related, but just what sort of relation this was supposed to be is arguable. Again, he was unclear, even self-contradictory, about whether evolution is or isn't directed towards some end.

In general, his work contained more rhetorical flourishes than careful arguments. It was hugely popular in the early decades of the century, especially because of his attacks on "closed" societies and authoritarian religions (Bergson 1935). But it rarely convinced people who weren't initially sympathetic.

In particular, it didn't convince those committed to orthodox science or materialism. Bergson's neo-Romantic view was unpopular with most professional biologists, even before they knew just what a "gene" was. Once genes had been identified with DNA in the 1950s, the synthesis of molecular biology with Darwinism eclipsed Bergson much as Goethe had been eclipsed by Darwinism itself.

As is the way with eclipses, however, he eventually reappeared—if only as a minority taste. His mysterious dualism was replaced towards the end of the century by a definition of matter itself as intrinsically active, so enabling creative self-organization in life, mind, and evolution (Chapter 16.x.a).

2.viii. The Neurophysiological Machine

Bernard's work was situated within a tradition of *general* physiology, concerned with widespread biological processes such as respiration, nutrition, and thermo-regulation. Descartes himself had written on such matters, in connection with the circulation and functions of the blood. But, as we've seen, he'd also done the philosophical groundwork for an experimental *neurophysiology*, whose main focus would be the role of the central nervous system in mediating between perception and behaviour.

(This distinctly Cartesian way of posing the problem informs most neurophysiology, and most cognitive science, today. Nevertheless, it hasn't gone unchallenged: see 15.vii and 16.vi–viii.)

a. Getting on one's nerves

Neurophysiology took longer to develop than other sub-areas of physiology. Nevertheless, by the end of the nineteenth century the mechanistic basis of nervous function was already evident. For many decades after Descartes, the "animal spirits" of the nerves were thought to exert a force found only in living things (sometimes termed *vis viva*). The discovery of electricity in the mid-eighteenth century raised the question whether this force, then termed "animal electricity", was the same as the physicists' variety.

In a series of experiments on artificial electric fish, Henry Cavendish—whose ancestor the Marquess of Newcastle had been assured by Descartes of the automatism of swallows in spring—proved in 1776 that these two electrical forces were one and the same (Chapter 15.ii.a). And at about the same time, in 1780, Luigi Galvani showed that passing an electric current through a frog's leg (detached from the body) causes the muscles to contract. Since nerves were already known to cause muscular contraction in whole animals, the implication was that the nervous impulse itself is electrical—in the sense understood in physics.

Throughout the first half of the nineteenth century, this idea spread among the educated populace. One person enthused by it was Alfred Smee (1818–77), surgeon to the Bank of England and self-styled "electro-biologist". In several popular books written in the 1840s, he went way beyond frogs' legs: he argued that the full range of human thought and behaviour could be deduced from the electrical properties of the nervous system (e.g. Smee 1849, 1850). Sensory properties such as redness or roundness, he said, cause specific electrical events in the brain, which in turn lead to electrical activity in the appropriate muscles. And an idea, on his view, is a collection of such properties. Smee's writings introduced many readers to electro-physiology as a way of seeing the brain as a biological machine. (He also had something provocative to say about "brainlike" artificial machines: see ix.a, below.)

The increasingly mechanistic view of nervous action was brought to a head by Helmholtz, who in the mid-nineteenth century measured the speed of the nervous impulse (Helmholtz 1850). His experiment was intellectually as well as physically electric, galvanizing neurophysiology and much educated opinion alike. Its demystifying effect was compounded by the fact that nerve action turned out to be surprisingly slow. Many physiologists had assumed it to be instantaneous, or comparable to the speed of light, but Helmholtz clocked the nervous impulse at less than 100 miles an hour.

Clearly, the case for regarding animal—and human—nervous systems as scientifically intelligible mechanisms was strong. Strong, but not yet universally agreed. Helmholtz's own father refused to accept his son's results, saying, "I regard the idea and its bodily expression, not as successive, but as simultaneous, a single living act, that only becomes bodily and mental on reflection" (Boring 1957: 48).

As for what the nerves were doing, an eighteenth-century physiologist might have said, in anticipation of Gertrude Stein, that a nerve is a nerve, is a nerve . . . But early in the next century (in 1811), the anatomist Bell and the physiologist François Magendie (in 1822) independently discovered that where a spinal nerve connects with the spinal cord, it splits into two parts—one sensory, one motor. (Later, Bell proved that the nervous impulse can pass down a nerve in one direction only. So sensory nerves were afferent, and motor nerves efferent, with respect to the brain.)

Bell saw the difference between the dorsal and ventral roots of the spinal nerves as a special case of a general principle concerning the central nervous system:

> the nerves which we trace in the body are not single nerves possessing various powers, but bundles of different nerves, whose filaments are united for the convenience of distribution, but which are distinct in office, as they are in origin from the brain. (Bell 1811: 114)

Some time later, he showed that although some cranial nerves are mixed, as spinal nerves are, others are purely sensory or purely motor.

He also suggested that the nerves subserving the six senses are somehow physiologically distinct. Six senses, not five: it was Bell who discovered the need for kinaesthetic feedback—and the reciprocal innervation of flexor and extensor muscles which Descartes had hypothesized 200 years earlier (see Section ii.f). For example, he pointed out that when the eyeball is pressed one experiences not pressure, but light. He inferred that changes in the optic nerve, even if originally caused by pressure, can lead only to visual sensations. In general, he said, electricity powers the nerves—but we don't perceive *electricity*.

This suggestion seemed to offer a non-idealist account of the secondary qualities, such as colour and warmth, in so far as it explained them in relation to distinct physiological mechanisms. More accurately, it posited a systematic link between specific sensory qualities and distinct physiological mechanisms. From a Cartesian point of view, anatomists' perceptions of the nerves, whether of their secondary or primary qualities (their whiteness or their size), are themselves mere representations of the material world.

Bell's idea about the multiple specificity of nervous function was echoed in 1826 by Müller, partly influenced by Goethe's phenomenological work but leaning also on his own experiments on the spinal nerves. Müller developed the idea at length (in 1838) as the doctrine of specific nerve energies—which, as we've seen, Helmholtz took to be the pivotal law of neurophysiology.

b. Reflections on the reflex

The functions of the spinal nerves were investigated also in studies of reflex movement. The word "reflex" is Marshall Hall's (1833), but Thomas Willis (then Professor of Medicine at Oxford) in 1664 had spoken of "reflexion" in the brain, and Descartes himself had described what we'd now call reflex action (see Section ii.d, above). So it's an old idea.

The first relevant experiments were done in the mid-eighteenth century, by Robert Whytt in Edinburgh. He showed that reflex movements can occur as a result of sensory stimulation even when a frog's spinal cord is separated from its brain.

Almost 100 years later, similar work by Müller and Hall on newts and snakes aroused controversy. The debate concerned whether reflex movements are ever mediated by the brain (as well as by the spinal cord), whether they're involuntary, and whether they're conscious. Some physiologists argued that all nervous action is conscious to some degree, others that consciousness requires activity in the brain.

In Russia, the early reflexologist Ivan Sechenov (1829–1905) turned the question on its head. In *Reflexes of the Brain* (1863), he argued that "all acts of conscious or unconscious life are reflexes". Sechenov's book was officially banned as a result, and he was (unsuccessfully) taken to court by the Petersburg Censorial Committee for undermining public morals (Boring 1957: 635). Clearly, reflexology was both radical and risky.

By the turn of the twentieth century, it was more widely accepted. Indeed, Ivan Pavlov (1849–1936) was awarded the Nobel Prize in 1904 for his work on digestion and conditioned reflexes.

Pavlov's prime interest was physiology, not psychology. (He founded the physiology laboratory at Russia's Imperial Institute for Experimental Medicine.) He started by studying how dogs come to produce saliva and gastric juices on hearing a bell previously associated with the presentation of food (Pavlov 1897/1902). This work, unlike Sechenov's earlier pronouncements, *didn't* obviously threaten ideas of consciousness: even humans don't salivate consciously. But in the new century, he broadened his interests and studied behaviour (Pavlov 1923/1927, 1925/1928). The issue of consciousness became problematic again when his students and—especially—the behaviourist psychologists applied his theory of conditioning to types of motor behaviour often attributed to conscious intentions (see 5.iii.a–c).

But even Sechenov hadn't denied animal consciousness. Few, if any, late nineteenth-century neurophysiologists shared Descartes's opinion that, irrespective of what happens in their brains, non-human animals are never conscious. (Perhaps one should rather say, "what was widely believed to be Descartes's opinion": see Section iii.d.) This belief had become doubly problematic after the publication of Darwin's theory of evolution. To regard animals as mere (non-conscious) automata not only went against nineteenth-century common sense, but implied a stark discontinuity between *Homo sapiens* and other species.

Darwin's colleague Huxley, in his paper 'On the Hypothesis that Animals Are Automata, and its History' (1874), offered an account of mind–body relations that specifically admitted animal consciousness. His view was later named "epiphenomenalism" by James Ward (1902).

Epiphenomenalism was intended by Huxley—and gratefully received by most experimental biologists—as a philosophical justification of research in neurophysiology (and psychophysiology). However, it inherited major metaphysical problems from Descartes, while also being counter-intuitive on two crucial points. Indeed, twentieth-century neurophysiology was bedevilled by similar problems—as the new century's research is also (Chapter 16.i.b).

Accepting the Cartesian mind–body split, Huxley denied that consciousness can cause physical change (which it seems to do, when someone performs a voluntary action), because such mind-to-body interaction was inconsistent with Helmholtz's law of conservation of energy. He even implied, without actually saying so, that there's no mind-to-mind causation either. In a telling technological metaphor, he compared consciousness to the smoke emitted by a steam-engine passing behind a hedge: the only thing visible, but merely an inconsequential side effect of the powerful machinery underneath.

However, he agreed with Descartes that conscious feelings and sensations are somehow caused by brain states—leaving unsolved the puzzle of how body-to-mind causation is possible. (Being the first self-confessed "agnostic", Huxley could hardly rely on Descartes's theological account of Natural causation.) This neo-Cartesian puzzle is still unsolved—some argue that it's insoluble (see 14.x–xi).

Although most early neurophysiologists were concerned with the peripheral nervous system, some anatomists worked also on the brain. Bell, for example, generalized his idea about functional specificity from the nerves to the brain.

He suggested that different parts of the brain do different things, and distinguished various aspects of brain specialization—such as motor and sensory centres. He didn't consider only the brain's gross anatomy, the distinction between cerebrum and cerebellum for example. On the contrary, he stressed the importance of discovering the pathways and interconnections of individual nerves:

> My view about the differentiation of nervous function will explain the apparently accidental connection between the twigs of nerves . . . and *it will give an interest to the labours of the anatomist* in tracing the nerves. (Bell 1811: 114; italics added)

The labours of the anatomist, at the time Bell was writing, depended primarily on careful dissection. Where the brain was concerned, the aim was to distinguish, and to trace, bundles of nerve fibres differing only slightly in colour, texture, or density. Given that techniques for firming up the porridge-like consistency of the brain were still primitive, this was a tall order. Thirty years later, the method of nerve sectioning became available also, whereby the anatomist could trace the degeneration of nerve fibres separated from their cell bodies. But this method came too late for Bell.

c. From nerves to neurones

A "nerve", for Bell, was *not* a neurone. It was a structure visible to the naked eye, a thread forming part of the nervous system.

Microscopy wasn't well enough advanced to be very helpful in identifying what we know to be neurones. Not until 1833 would it show that there were both cells and fibres in the brain: the grey and white matter, respectively. Besides the relatively low optical resolution of the instruments, cell staining in general wasn't discovered until the mid-nineteenth century. And it was even later, in 1873, that Camillo Golgi (1843–1926) originated 'Golgi staining', which was needed to make individual brain cells visible under the microscope.

Even after the advent of Golgi's method, it wasn't evident that the brain contains neurones. In other words, it was still unclear whether the brain is a continuous network, with cell bodies positioned along the nerve fibres like isolated beads on a reticulated string, or whether the network is discontinuous. Up to the final years of the century, most neuro-anatomists assumed the former. By 1906, to be sure, neurones were gaining favour. But Golgi himself took pains to reject "the current opinion"—namely, the neurone doctrine—in his Nobel Prize acceptance speech that year (Shepherd 1991: 261).

Golgi's unrelenting speech was especially ironic, for he was sharing the Nobel Prize with the person who had provided the first proofs of the neurone theory: the Spanish histologist Santiago Ramón y Cajal (1852–1934). In 1888 he had shown that the brain contains a host of discrete units (Ramón y Cajal 1901–17: 321–41). Using an improved version of Golgi staining (but an out-of-date microscope), he observed units differing in size and shape, some of which are found only in specific parts of the brain, such as the cerebellum.

The details of his drawings—microscopic photography was impossible—were astonishing (see Figures 2.1–2.3). In many cases, the cell bodies sprout a mass of tiny dendrites at one end and a single fibre (the axon) at the other—which divides into dendrites at its extremity. The dendrites from different cells intermingle at the (newly observed) synapses, but aren't physically continuous.

In short, the brain is made up of individual cells, many of which are neurones. (The others include the glial cells.) Ramón y Cajal hypothesized that only neurones carry messages, that each neurone does this in one direction only (from the cell body, through the axon), and that nerve impulses are transmitted only by contact between neurones. I say "hypothesized", but in the final case that was too weak:

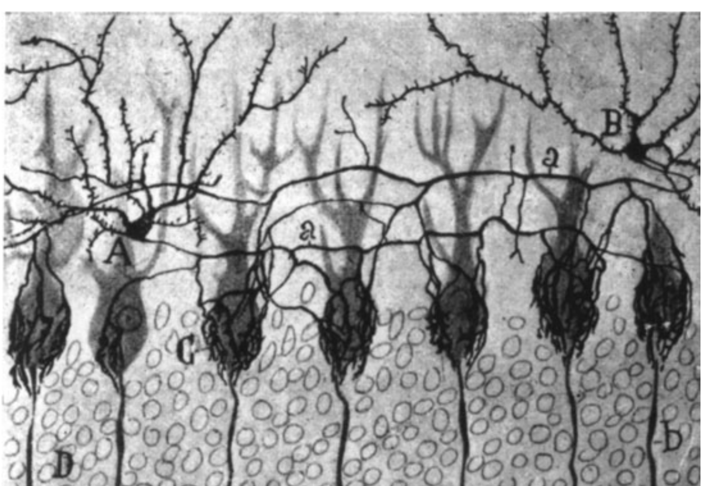


Fig. 2.1. Drawing of neurones, done in 1888, by Ramón y Cajal. Original caption: Transverse section of a cerebellar lamella. Semidiagrammatic. A and B, stellate cells of the molecular layer (basket cells), of which the axon (a) produces terminal nests about the cells of Purkinje (C); b, axon of the Purkinje cell. Reprinted from Ramón y Cajal (1901–17: 330)

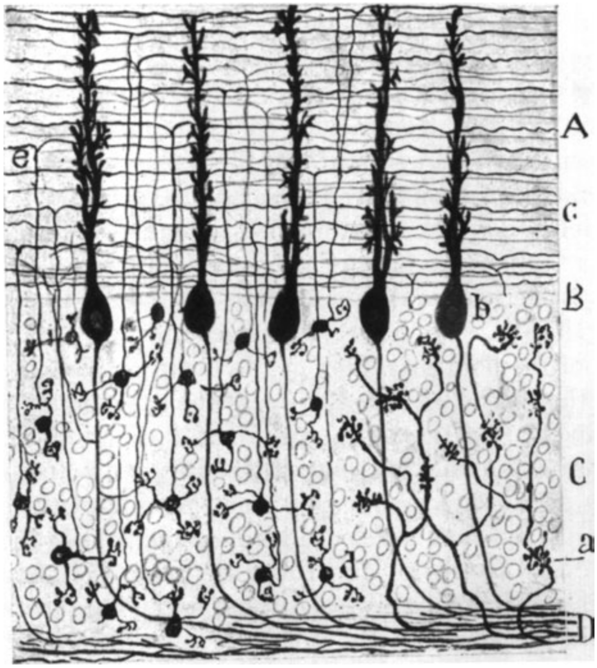


Fig. 2.2. Drawing of neurones, done in 1888, by Ramón y Cajal. Original caption: Longitudinal section of a cerebellar convolution. A, molecular layer; B, layers of Purkinje cells; C, granular layer; D, white matter; a, tuft of a mossy fibre; b, body of a Purkinje cell; c, parallel fibres; d, granule cell with its ascending axon; e, division of this axon. (Semidiagrammatic). Reprinted from Ramón y Cajal (1901–17: 331)

This fortunate discovery [of the cerebellar climbing fibres], one of the most beautiful which fate vouchsafed to me in that fertile epoch, formed *the final proof* of the transmission of *nerve impulses by contact*. (Ramón y Cajal 1901–17: 332; first italics added, second in original)

Did his fellow neuro-anatomists accept this "final proof"? Not at all. Largely because of the hold of the reticular theory on their minds—and also because they couldn't immediately replicate his findings (it turned out that there was something special about the tap water he used to rinse his slides for thirty minutes or so)—these discoveries were initially ignored. It didn't help that Ramón y Cajal was an untutored country boy from Zaragoza, not a university researcher (see Chapter 1.iii.f). As he put it in his (fascinating) autobiography, his papers were greeted with "silence", "excessive reservedness", and even "contempt" (Ramón y Cajal 1901–17: 352–3). One contemporary neurologist later recalled (in 1913):

The facts described by Cajal in his first publications were so extraordinary that the histologists of the time—fortunately I did not belong to the number—received them with the greatest scepticism. The distrust was such that, at the anatomical congress held in Berlin in 1889, Cajal, who afterwards became the great histologist of Madrid, found himself alone, exciting around him only smiles of incredulity. (quoted in Ramón y Cajal 1901–17: 356)

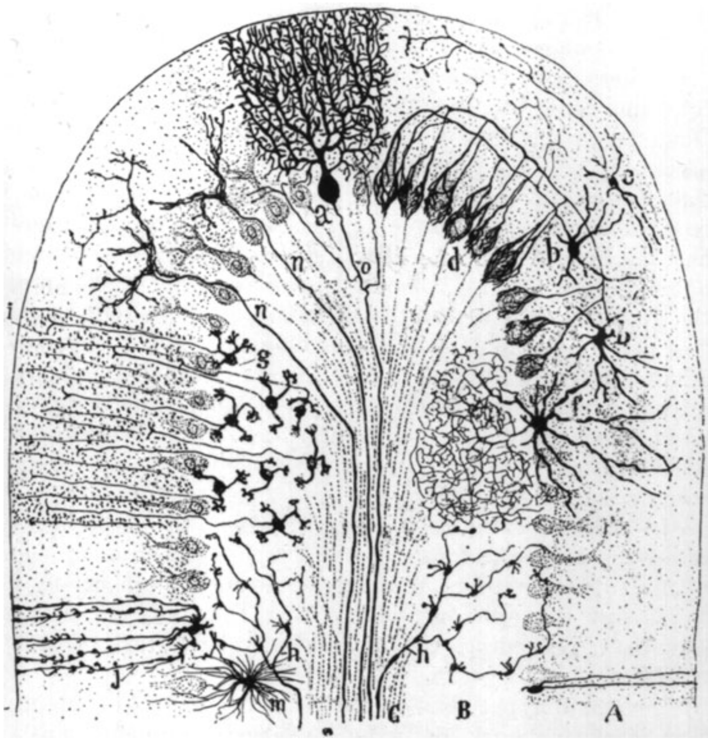


Fig. 2.3. Drawing of neurones, done in 1888 by Ramón y Cajal. Original caption: Semidiagrammatic transverse section of a cerebellar convolution of a mammal. A, molecular layer; B, granular layer; C, layer of white matter; a, Purkinje cell with its dendrites spread out in the plane of section; b, small stellate cells of the molecular layer; d, descending terminal arborizations embracing the cells of Purkinje; e, superficial stellate cell; f, large stellate cell of the granule layer; g, granules with their ascending axons bifurcating at i; h, mossy fibres; j, tufted neuroglia cell; n, climbing fibres; m, neuroglia cell of the granule layer. Reprinted from Ramón y Cajal (1901–17: 333)

The smiles of incredulity soon faded. The same observer also recalled that the microscopic preparations that Ramón y Cajal showed at the Berlin congress were "so decisive" that his claims were soon confirmed by Albrecht Kolliker, "the unquestioned master of German histology".

But to fade isn't necessarily to disappear. The physiological experiments of Sherrington, an early champion of the neurone theory, provided further evidence (see Section viii.d, below). Most people found this evidence compelling. Nevertheless, doubts remained even up to the mid-twentieth century, when disagreements between "neuronists" and "reticularists" ended at last (Changeux 1985, ch. 1).

In the early twentieth century, Ramón y Cajal's microscopic studies showed also that there are many different kinds of neurone, located in different parts of the brain. The structure of the cerebellum, for example, differs from that of the cerebral cortex. And the striate (i.e. striped) cortex contains several distinct layers, whose neurones differ in both shape and spatial arrangement.

Half a century after that, computational neuroscientists would use updated versions of the Zaragozan's pioneering work to try to puzzle out just what the cerebellum is doing, and how (Chapter 14.iv.c–d and viii.b).

d. Integration in the nervous system

In 1894 the champion of the neurone theory was invited to speak to the Royal Society in London. There he met the leading English neurophysiologists, including Sherrington (1857–1952) and his old friend Michael Foster.

Foster was the first professor of physiology at Cambridge, and the founder of the *Journal of Physiology*. (He'd been appointed, in 1870, on Huxley's recommendation.) At the time of Ramón y Cajal's visit, he was acting also as Secretary of the Royal Society. At the banquet held in the Spanish visitor's honour, Foster declared that "thanks to [his] work, the impenetrable forest of the nervous system had been converted into a well laid out and delightful park" (Ramón y Cajal 1901–17: 421).

Soon, Sherrington (at the University of Liverpool from 1895 to 1913, when he moved to Oxford) would show that the park was laid out on a number of interconnected levels, involving subtle hierarchies of control. And in the final years of the century, Sherrington added the new neurone theory to his study of mammalian reflexes. In 1897, while editing a chapter of Foster's *Textbook of Physiology*, he realized that the all-important nerve junction needed a name. He coined the term "synapsis", soon shortened to "synapse", from the Greek word for *to clasp* (Sherrington 1937).

It was already clear that the man-machine was a self-equilibrating system in its motor behaviour, no less than its blood temperature. As early as 1817, Magendie had defined a reflex not as a mere 'string' of events, but as a circular activity initiated at some bodily location, passing via the central nervous system, and returning to the initial point to cause some bodily reaction there. And he and Bell, in their studies of sensory and motor nerves, had identified two arcs of the circle.

But the detailed neural mechanisms were still unknown. Thus in his textbook of 1879 Foster had remarked:

> The spinal cord, and indeed the whole central nervous system, may be regarded as an intricate mechanism in which the direct effects of stimulation or automatic activity are modified and governed by the checks of inhibitory influences; but we have as yet much to learn before we can speak with certainty as to the exact manner in which inhibition is brought about. (quoted in Granit 1966: 41)

In Sherrington's hands, the neurone theory promised to help provide the answers.

Sherrington made precise time measurements, allowing not only for nervous conduction but also for synaptic delay (and refractory periods); and he kept precise records of the forces exerted by antagonistic muscles. Using these experimental data, he was able to explain gross patterns of movement in terms of neural circuits involving inhibitory and excitatory action. Moreover, he showed that inhibition didn't happen only at the nerve–muscle junctions of inhibitory nerve fibres, but also at (nerve–nerve) synapses within the brain.

In 1892, for example, he showed that—despite its very low latency—the knee jerk is indeed a reflex, which can be actively inhibited by electrical stimulation of the antagonist muscle. This was an early example of his lasting interest in the reciprocal innervation of opposing muscle groups—a principle anticipated by Descartes (see Section ii.f). Sherrington described it in several papers in the *Journal of Physiology*, and in the Royal Society's *Philosophical Transactions*, around the turn of the century.

A few years later, he showed that decerebrate cats, in which the cerebrum is separated from the brain stem, respond to a touch on the skin of one paw by an integrated pattern of movements involving all four limbs (Sherrington 1898). These animals suffered a characteristic rigidity (previously observed by Bernard), because the proprioceptive feedback mechanisms regulating their muscle tone had been destroyed. In investigating this, Sherrington pioneered the study of the sensory organs (muscle spindles) involved.

In 1906 he published an influential book citing even more complex automatic movements. For instance, a decerebrate animal will continue to 'step' on a moving belt, walking fast or slowly in perfect adjustment to the speed of the belt. He explained these movements in terms of specific sensori-motor mechanisms (Sherrington 1906). In this case, and others, he tried to relate particular neuronal circuits to the "integrative" sensori-motor action of the organism as a whole. His work on "circular" reflexes and "integration" in the nervous system would later help inspire the cybernetics movement (see Chapter 4.v).

Although Sherrington's theory of 1906 treated the neurone as the analytical unit of the nervous system, he couldn't then record from individual nerve fibres. The electrical signal was too faint. His hypotheses about the functional properties of neurones had to be tested indirectly. Direct confirmation, and an explanation of how these functions actually work, was to come from research done over the next half-century (see below).

e. How do neurones work?

Meanwhile, some sceptics still doubted whether 'mental' functions like associative learning and inhibition could be grounded in *any* physical mechanism. Neurophysiologists assumed that they could—but they weren't able to explain how.

Just before the First World War, the engineer S. Bent Russell set out to prove that this is in principle possible, by building a hydraulic compressed-air model of nervous conduction (Bent Russell 1913). He even suggested that engineers, using mechanical simulation as a general method, might do interdisciplinary research with neurophysiologists and psychologists:

> It is thought that the engineering profession has not contributed greatly to the study of the nervous system . . . *As the cooperation of workers in different fields of knowledge is necessary in these days of specialists* it may be argued that engineers can consistently join in the consideration of a subject of such importance to man. (1913: 21; italics added)

With hindsight, his suggestion looks prophetic. At the time, it looked unconvincing—or anyway, not readily feasible. Bent Russell himself soon abandoned simulation (Cordeschi 2000: 321).

(The early interest in *physical* models of neural learning soon waned: Cordeschi 2002. But it revived in the early 1930s, largely due to Clark Hull's behaviourist 'robot' approach: see Chapter 5.iii.c. It blossomed in the 1940s, thanks to cybernetics: Chapter

4.v–vii. By the 1950s, most simulations of the nervous system were computational rather than physical: Chapters 4.iii–iv and 14.iii.)

By the middle of the twentieth century, Foster's question about *how* conduction and inhibition actually happen had been largely answered. The answer was found by work done at Trinity College, Cambridge—with the help of technical equipment pioneered in the early 1920s by neurophysiologists at Washington University, St Louis (Finger 2000: 245–8). (All the prime movers involved ended up with Nobel Prizes.)

Trinity's Edgar D. Adrian (1889–1977)—later, Lord Adrian—was able to amplify the nerve impulse so that the action potential, or spike, in a single axon could be recorded. He first did this in experiments on muscle (Adrian 1926; Adrian and Zotterman 1926), which confirmed Sherrington's ideas about proprioception due to muscle spindles (see Section viii.d, above). But very soon afterwards he was able to do so by working directly with motor-nerve fibres (Adrian and Bronk 1928). His technique became more widely used when the London anatomist John Z. Young discovered 'giant' axons in the squid, a millimetre across and several centimetres long.

This method vindicated the "all-or-none" principle of nervous conduction, which had been posited—and demonstrated in muscle fibres—by Adrian's Trinity supervisor Keith Lucas (Lucas 1905, 1909) and confirmed *indirectly* by Adrian himself (Adrian 1914). It also provided precise data on the temporal properties of the neurone, such as the refractory period and the timing of bursts of impulses in different stimulus conditions.

Adrian also discovered that neurones are destined to fire spontaneously, without being stimulated to do so by other neurones. As he put it, "The moment at which [firing] occurs can be greatly altered by afferent influences, but it cannot be postponed indefinitely" (Adrian 1934: 1126). He assumed that this unpredictable noise in the nervous system was due to unknown chemical changes at the synapse.

The mechanism of nervous action was further clarified in 1939 by Adrian's fellow Fellows Alan Hodgkin (1914–98) and Andrew Huxley (1917–1998). They suggested, against the all-or-none orthodoxy, that the spike travels down the axon by successive *local* depolarization of the cell membrane, with a swing in potential from inside-negative to inside-positive and back again. However, their hypothesis was too heretical to be accepted—and for the next few years, they were diverted into war work.

By the late 1940s they were again free to study the biophysics of nerve conduction. In 1949 they discovered the "sodium pump", by means of which the electrical impulse travels along the axon. Finally, at mid-century, they published their research in the form of the fundamental 'Hodgkin–Huxley equations' (Hodgkin and Huxley 1952).

And that, for a while, was that. As Huxley later put it:

> When we had completed the work on the [giant] squid fibre that we published in 1952, we could not see what could be done next to take the understanding of the excitation process to a deeper level. Huge advances have been made since, but all have depended on technical improvements or on advances in other branches of biology—notably molecular genetics—that were unforeseeable in 1952. (A. Huxley 1999)

Hodgkin, accordingly, switched his interests to other aspects of nerve physiology. As for Huxley, he turned to study the mechanism of contraction of striped muscles. By the mid-1950s, he was awing Cambridge's medical students with hot-from-the-lab reports of his discoveries (see Preface, ii).

Many questions remained, however, about just which nerve cells do what. Hodgkin had used miniature electrodes to record intracellular changes in the *axon*. John Eccles (1903–97)—who shared the Nobel Prize with Hodgkin and Huxley in 1963—developed their technique to stimulate and record from individual *cell bodies*. This heralded a new era: by the early 1960s, neuroscientists had fallen prey to what one of their number would term "a virtual obsession with unit recording" (see Chapter 14.iv and ix.b). "Obsession", because although a huge amount was learnt through this technique, certain questions were systematically sidelined.

It would be some time yet before neuroscientists focused on the properties of large networks of neurones, or on the detailed functionality of anatomically identifiable areas of the brain. Some of that work—examples of *computational* neuroscience—will be described in Chapter 14. (For a broad summary of post-1960s advances in general neuroscience, including neurochemistry, see Kandel and Squire 2000.)

f. Brains and machines

Eccles's new knowledge of neural mechanisms didn't commit him to mechanism as a philosophical position (see 16.i.a). On the contrary, he argued that, thanks to quantum indeterminacy, human free will can cause physical changes in "critically poised" neurones in the brain (Eccles 1953, ch. 8). He specifically described the cerebral cortex as a "detector" whose sensitivity is of a different kind and order from that of any physical instrument. (Some thirty years later, he was still arguing essentially the same position: see Eccles 1986.)

Most mid-twentieth-century neurophysiologists ignored that speculation. They valued Eccles, rather, as one of the first in a growing line of researchers using single-cell techniques to demystify the brain. His techniques would soon lead to the discovery of feature detectors (Chapter 14.iv). And his own experiments would clarify the detailed structure of the cerebellum—and, ironically, help ground a computational account of the role of the cerebellum in "voluntary" action (14.v.b–d). Even in 1950, those results still lay in the future. But 'man as machine' now had firm neurological underpinnings.

Before Darwin, Bell's robust defence of his practice of "examining the human body as a piece of machinery" had included the argument that the "perfection of the instrument" exemplifies the Creator's "plan universal" and "prospective design" (Bell 1834: 3, 15 ff.). Indeed, Bell's pioneering study of the anatomy and functioning of the hand, including its sensory and motor nerves, was published as one of the eight *Bridgwater Treatises*.

Appearing between 1833 and 1836 (and very soon reprinted), these had been established by the will of the eighth Earl of Bridgwater, who died in 1829. An ordained minister, he was more theologically active after his death than before it, being described as "a noble clergyman who had always neglected his parish assiduously" (Gillispie 1951: 209).

His executors carried no little clout: the Archbishop of Canterbury, the Bishop of London, and the President of the Royal Society. These three gentlemen were required by the terms of the will to choose eight scientific writers supporting the general theme:

"On the Power, Wisdom, and Goodness of God as Manifested in the Creation". (The "Ninth" *Bridgwater Treatise*, as we'll see in Chapter 3.i.b, was unofficially added by Charles Babbage.)

But the will also suggested more specific topics, one of which was "the construction of the hand of man". Having picked up that baton, Bell's preface explicitly condemned:

> the futility of the opinions of those French philosophers and physiologists, who represented life as the mere physical result of certain combinations and actions of parts, by them termed Organization. (Bell 1834, p. ix)

In short, he was just one of many scientists, at that time, who saw the functional organization of living things as a proof, or even as a self-expression, of divine intelligence.

After Darwin, the notions of natural machines and God's design were divorced in many—though not all—biologists' minds. And after Bernard (with his not-so-futile French opinions), and especially Sherrington, some of the most mysterious workings of the body were being compared to inanimate machines. But instead of clocks and fountains, people now cited the steam-engine, the telegraph, and the telephone. The last two of these would feature prominently for the next fifty years, occasionally accompanied by the accordion, harpsichord, or jukebox. (Jukebox, because it seemed, in the late 1920s, that a particular signal from the brain evokes *an entire pattern* of innate or learnt behaviour: see Chapter 14.v.c.)

These ideas didn't spring merely from turn-of-the-century technophilia. For they all exemplified what Gerd Gigerenzer (1991*b*) has called the "tools-to-theories" heuristic. This is a widespread creative strategy in science, of which *man-as-machine*, *mind-as-computer*, and *heart-as-pump* are all special cases. Thinkers guided by it use a pre-existing physical artefact—or perhaps a mathematical calculus, such as statistics (Chapter 7.iv.g)—as the inspiration for a new scientific theory.

The telephone-exchange analogy was undermined in the 1930s, by Adrian's observations of spontaneous neuronal firing ('noise' in the nervous system) and by his and others' work on EEG waves. But it wasn't finally dropped until later. Its demise was partly due to further work on random activity in the brain. "Random", here, didn't mean uncaused: rather, a neurone's firing was not, as had previously been thought, wholly determined by the input—or lack of it—from other neurones (Delisle Burns 1968). (Later still, Horace Barlow would argue that this seeming unreliability of individual neurones had been an illusion: Chapter 14.x.e.)

What replaced the telephone exchange, as we'll see in Chapter 4, was the computer. (Whether the computer itself will some day be replaced by some other machine is discussed in Chapter 16.ix.f.)

By the early to mid-twentieth century, then, the biological version of 'man as machine' had blossomed luxuriantly. Interpreted in the scientific sense, there was a powerful consensus that the body, including the nervous system, is scientifically intelligible. Interpreted in the technological sense, man-made machines were commonly used as analogies in theorizing about the body, including the brain.

Even exciting new toys such as motor boats and aeroplanes were sometimes mentioned, if only to inspire the minds of the young. In 1912 Edwin Brewster's book on physiology, enticingly named *Natural Wonders Every Child Should Know*, confidently declared:

For, *of course*, the body is a machine. It is a vastly complex machine, many, many times more complicated than any machine ever made with hands; but still after all a machine. It has been likened to a steam engine. But that was before we knew as much about the way it works as we know now. It really is a gas engine; like the engine of an automobile, a motor boat, or a flying machine. (quoted in Hodges 1983: 13; italics added)

For all his confidence in mechanism, Brewster didn't pretend to have all the answers. For example, he asked how the "living bricks" of the body "find out when and where to grow fast, and when and where to grow slowly, and when and where not to grow at all"—something, he said, which "nobody has yet made the smallest beginning at finding out".

Brewster's book earns a footnote in history by being the one which, so Turing told his mother, opened Turing's eyes to science (Hodges 1983:11). Indeed, Turing later made "a small beginning" in answering Brewster's question about the living bricks (see 15.iv). Even more to the point, he made a *large* beginning in extending the man-machine analogy from body to mind (Chapters 4.ii and 16.ii). But, as the next two sections explain, that extension was long awaited.

2.ix. Strictly Logical Automata

The wheels and pistons, the switchboards and electric wires, and even the motor boats and jukeboxes were aspects of the *bodily* machine: analogies for the brain, that material stuff inside the skull. If 'man as machine' is taken also to mean 'mind as machine', little had changed. At the beginning of the 1900s there was still scant temptation to think of the mind by analogy with automata.

a. Early gizmos

It may seem strange that no one was suggesting the mind–machine analogy. For would-be logic machines had been attempted in the Middle Ages (Section i.b, above), and primitive calculating machines had existed since the early seventeenth century. Gottfried Schickard, a friend of Johannes Kepler, invented one of these in 1623 (his letters were lost for over 300 years, so very few people have heard of him: Rojas 2002). However, these machines were seen as aids to human thought, not models of it.

So when Blaise Pascal (1623–62) built his cogwheeled adding machine in 1642, he was attempting to embody the laws of mathematics that the mind must respect when calculating, rather than the activities of the mind itself. Nor did he value logic above all other types of thinking. Quite the reverse: a forerunner of existentialism, he praised the leap of faith involved in religious belief (cf. Chapter 8.vi) and insisted that "the heart has reasons that Reason does not know".

Pascal's near-contemporary Leibniz (1646–1716) did believe that every human problem could in principle be settled by logical thought. He dreamt of a universal language in which all properties could be accurately mapped. This, he said, would enable future statesmen and philosophers to settle disputes by formal reasoning, instead of resorting to rhetorical persuasion or even abuse. Disagreements between philosophers or politicians need be no greater than those between accountants:

> For it would suffice for them to take their pencils in their hands, to sit down each to his abacus, and (accompanied if they wished by a friend) to say to each other: "Let's calculate! [*calculemus*]". (Leibniz 1961: 200; my trans.)

This vision had been part-inspired by the writings—and the logical devices—of Lull, 400 years earlier (Section i.b, above). Aged only 20, Leibniz wrote a *Dissertatio de Arte Combinatoria* whose scope and style were clearly inspired by Lull and his followers. In his maturity, he remembered:

> When I was young, I found pleasure in the Lullian art, yet I thought also that I found some defects in it, and I said something about these in a schoolboyish essay called *On the Art of Combinations* . . . I have found something valuable, too, in the art of Lully and [a scholar who] pleased me greatly because he found a way to apply Lully's generalities to useful particular problems. (quoted in Fauvel and Wilson 1994: 55)

Like Lull, Leibniz believed that "the understanding" follows "rules". But his own calculating machines, some of which could multiply and divide as well as add, were concerned only with mathematics—not logic. An early model was shown to the Royal Society in 1672, and several other designs were described later (Leibniz 1685). Surprisingly, perhaps, Leibniz *was not* influenced by Pascal's gizmo:

> When, several years ago [about 1670], I saw for the first time an instrument which, when carried, automatically records the numbers of steps taken by a pedestrian, it occurred to me at once that the entire arithmetic could be subjected to a similar kind of machinery so that not only counting but also addition and subtraction, multiplication and division could be accomplished by a suitably arranged machine easily, promptly, and with sure results.
>
> *The calculating box of Pascal was not known to me at that time. I believe it has not gained sufficient publicity*. (Leibniz 1685: 173; italics added)

If we wanted to produce "a more admirable machine", he said, "things could be arranged in the beginning so that everything should be done by the machine itself" (1685: 178). But he didn't see that as cost-effective, or even of much practical use. All the human operator had to do, after all, was to turn the wheels, or (for multiplication) move the machine from one operator to another.

What's most relevant here is that Leibniz saw his calculators as specialized tools for compiling mathematical tables (for use in commerce, estate management, navigation, and astronomy: p. 180)—not as attempts to mechanize thought in general. He did envisage mechanizing *logic*, using calculating machines based on binary numbers. But he could actually build only (decimal) calculators (see Chapter 4.iii.d).

One of the eighteenth-century machines inspired by Pascal and Leibniz was a calculator built in 1784 by the engineer Johann Müller (1746–1830) (Pratt 1987: 99; Swade 1991: 21–2). Modelled on Leibniz's ingenious stepped-cylinder design (Lindgren 1990), this machine was intended not to do single sums but to perform several calculations in series.

Its central mathematical, and mechanical, principle was to be employed nearly forty years later in Babbage's Difference Engine (see 3.ii). Indeed, the principles embodied in Leibniz's machine were used in almost every subsequent mechanical calculator. But much as the Difference Engine was concerned with arithmetic, not logic (and still less with thought: 3.iv), so Müller's machine was a mathematical device, not a logical one.

The nineteenth century saw the design of two calculating machines enormously more powerful than anything previously available (Chapter 3). One of these—Babbage's Analytical Engine, initiated as early as 1834—was capable in principle of being adapted for logical use. But the Analytical Engine was (and still is) no more than a design. At mid-century, then, there was still no actual mechanical device that would do logic, as opposed to arithmetic.

There was, to be sure, a maverick vision of a logical machine—intended, unlike the Analytical Engine, to "reason" *in the very same way that people do*. This was due to the popular science-writer Smee (1851). Drawing on his ideas about electro-biology, he designed a Relational machine to embody just one idea at a time. ("Relational", because he saw an idea as a set of interrelated properties: see viii.a, above.) Based on George Boole's just-published logic (see below), it would consist of a large metal plate successively divided into two parts by (a hierarchy of) hinges. Their position, open or shut, would represent the presence or absence of the relevant properties. Then, he said, two Relational machines could be combined to make a Differential machine, whose task would be to compare two different ideas.

Smee saw this implementation of Boolean logic as sufficient, in principle, to simulate all human thought. But even he had to admit that what's possible in principle may not be achievable in practice:

> When the vast extent of a machine sufficiently large to include *all words and sequences* is considered, we at once observe *the absolute impossibility* of forming one for practical purposes, inasmuch as it would cover *an area exceeding probably all London*, and the very attempt to move its respective parts upon each other, would *inevitably cause its own destruction*. (Smee 1851: 43; italics added)

By the end of the nineteenth century, things had changed. A device for handling "all words and sequences" was still a pipe-dream. But a few machines now existed for doing simple logic.

One had been designed in 1869 by the economist and logician Stanley Jevons (1835–82). He was part-inspired by Babbage, but relied on electricity instead of cogwheels (Hyman 1982: 255). His device used binary relays to model the logical relations—conjunction, disjunction, if–then, equivalence, and negation—between up to three propositions (Jevons 1870). The state of each relay was signalled by a lamp, all of which would light up to indicate a necessary truth, or tautology.

This device prompted lively correspondence in the 1870s (Mays and Henry 1953). As noted already (Preface, ii.a), it was still influential almost 100 years later. The first modern electrical logic machine was built on Jevons's principles, lamps and all, in 1949 (Mays and Prinz 1950). It could generate logical proofs made up of a sequence of steps. A special store accepted the results of applying one of the five operations to the two propositions in the two lower-level stores. These results (thanks to two auxiliary stores) could then be fed back into the lower level—so modelling a "chain" of inferences.

b. Logic, not psychology

None of those nineteenth-century inventions, however, was thought of as a machine for thinking, or even for simulating thought. Smee's never-built, and self-confessedly impractical, device had been intended as a simulation of reasoning, but these logic machines were not. Rather, they were devices for illustrating/following the formal principles that must be satisfied if thinking is to be logically valid or mathematically correct.

This may seem surprising. For logic was traditionally concerned with argumentation, whether analysed in terms of statements (propositions) or concepts (classes). Lull's *Ars Magna* had supposedly concerned reasoning, knowledge, and truth. The Port-Royal logicians of the seventeenth century (discussed in Chapter 9.iii.c) had described logic as "the art of thinking". And one of the nineteenth century's most distinguished logicians, Boole (1815–64), described logic as "the laws of thought" (Boole 1854).

However, that little word "of" was ambiguous. It could be given *either* of the two interpretations distinguished in the opening paragraph of this subsection. Not until the 1880s was the ambiguity clearly recognized (see below).

Boole is remembered for his development of Boolean algebra, or Boolean logic—which is fundamental to modern computers (Chapters 3.v and 4.iii). These alternative labels mark his demonstrations that algebra can be expressed in terms of a binary notation, which in turn can be mapped onto statements that are either true or false (Boole 1847). By using variables to stand for whole propositions, he showed that the various types of syllogism, familiar since Aristotle, can be expressed in this symbolism and thereby explicitly proved to be valid. (Previously, their validity had been taken as intuitively obvious.) Jevons's logic machine was based on Boole's work.

Babbage, who regarded Boole as "a real *thinker*" (Hyman 1982: 244), noted that there could be a machine for doing logic, but he never designed one. Even if he had, he wouldn't have seen it as a psychological model (see Chapter 3.iv). As for Boole himself, his book title *An Investigation of the Laws of Thought* was misleading: his interest was in the logical foundations of mathematics, not the facts of psychology.

The ambiguous position of logic with respect to psychology was clarified towards the end of the nineteenth century by the mathematician Gottlob Frege (1848–1925). Frege, like Boole, believed that mathematics could be grounded in logic (Frege 1884; Beaney 1997). In trying to prove this, he defined a long-standing logical evil, psychologism—saying that his guiding principle was to avoid it (Frege 1884, Preface).

Psychologism is any approach which confuses formal logic (or *norms* of rational thinking) with empirical facts about how people think. Frege's point was not that people don't—or don't always—think logically. It was that whether they do or not—and how they do, when they do—is of no interest to the logician. The logical should always be distinguished from the psychological, since the (normative) laws of logic are not the (empirical) laws of thought. In modern philosophical jargon, Frege's position was that logic, or rationality, can't be "naturalized". (Kant had said much the same thing, and neo-Kantians today criticize cognitive science accordingly: Chapter 16.vi–viii.)

In his critique of psychologism, Frege introduced new standards of logical rigour, and contributed a host of new ideas to logic and the philosophy of language (Chapter 9.ix.c). For instance:

* He defined the notion of a "truth value".
* He pioneered the truth-functional propositional calculus.
* He provided a formalism, the predicate calculus, that could represent the internal structure of propositions, and deal with quantifiers such as *all* and *some*.
* He defined higher-order functions, or functions made up of functions (an idea that would later feed into LISP: Chapter 10.v.c).
* And he made important distinctions between various meanings of "meaning"—such as *sense* and *reference*. The phrases 'the Morning Star' and 'the Evening Star' have different senses, so would be translated differently; but, as astronomers eventually discovered, they have the same reference: namely, the planet Venus.

As a result of Frege's work, which became widely influential with the publication of Bertrand Russell and Alfred North Whitehead's *Principia Mathematica* in 1910, logic and psychology were pushed even further apart. Not until the mid-twentieth century would people see logic machines as having psychological relevance. Ironically, they'd do so largely because of Frege—via Russell (1872–1970) and Russell's student Rudolf Carnap (1891–1970): see Chapter 4.i–ii.

Still more shocking, cognitive scientists would also suggest the reverse: that psychology is relevant to logic. The philosopher of science Paul Thagard, for instance, suggested a way of "*revising* normative (prescriptive) logical principles in the light of descriptive psychological findings" (1982: 25; italics added). This was based not merely on the outward facts of psychology: people's patterns of error, for example. It was based also on computational principles explaining how it's possible for people to reason at all.

In general, cognitive scientists—even if they didn't seek to "revise" logic—rejected the orthodox assumption that logical norms are ideal for guiding actual thought. One illustration is Herbert Simon's stress on "satisficing", not optimizing (Chapter 6.iii). Others include pleas for a more realistic philosophy of inductive reasoning (e.g. Boden 1980; Stich and Nisbett 1980), and a stress on "fast and frugal" heuristics for problem-solving (7.iv.g). The seeming imperfections ("boundedness") of human rationality, some said, were what made it possible for humans to act rationally at all (7.iv.h).

2.x. Psychology as Mechanism—But Not as Machine

If, 200 years after Pascal and Leibniz, people still weren't ready for 'mind as machine', that's not to say that they weren't ready for a mechanistic psychology. By the end of the nineteenth century, some psychologists had started to study the mind by the methods of science.

a. Visions of a scientific psychology

These fledgling psychologists were grounded in empiricist philosophy, which from its inception in the mid-seventeenth century had always been very close in spirit to the science of the time (see Section iii.b, above). So Locke, for instance, although he *didn't* pick up Hobbes's suggestion that thinking is essentially a form of computation, did think that philosophy must be fully consistent with science. As he put it, it was "ambition enough to be employed as an under-labourer in clearing the ground a little" for scientists such as Boyle and "the incomparable Mr. Newton" (1690, Epistle). Indeed, Locke himself assisted Boyle on various occasions, and saw his *General History of the Air* through the press after his death (Shapin 1994: 398 n.).

In his philosophical writings, Locke discussed many questions which today we'd call psychological. One famous example, posed to him in a letter from William Molyneux, was whether a man blind from birth would be able to recognize things if his sight were suddenly restored:

> Suppose a man born blind, and now adult, and taught by his touch to distinguish between a Cube and a Sphere of the same metal, and nighly of the same bigness, so as to tell, when he felt one and t'other, which is the Cube, which is the Sphere. Suppose then the Cube and Sphere placed on a Table, and the Blind Man to be made to see. *Quaere*, Whether by his sight, before he touch'd them, he could now distinguish, and tell, which is the Globe, which the Cube. (J. Locke 1690: II. ix. 8)

Locke's view (and Molyneux's) was that the man *would not* be able to distinguish them. Shape-learnt-by-touch, he believed, is quite distinct from shape-learnt-by-sight, so the association between a seen cube and a touched one couldn't be inferred (by "reflection"). Instead, it would have to be freshly learnt. The 'Molyneux question' would fascinate philosophers and psychologists for centuries (Morgan 1977), and was largely answered by a cognitive scientist in 1959 (Chapter 6.ii.e).

Another example of a fundamental psychological puzzle that exercised Locke was how we can think about "universals". For instance, how can we understand the concept of "a triangle" in the general case? As he put it, how can a triangle be thought of as "neither oblique nor rectangle, neither equilateral, equicrural, nor scalenon; but all and none of these at once"? (1690: IV. vii. 9). One of the earliest papers in connectionism, some 250 years later, would ask the very same question—and sketch a not-implausible answer (Chapter 12.i.c).

Even in Locke's time, the study of the mind (or, as it was often put, the soul) was still classified as an example of "pneumatology": the philosophy of incorporeal substances—such as angels, and even God Himself. As such, it was associated with theology rather than science (then called natural philosophy) or medicine. By the 1720s, however, it was being termed "psychology"—and in Chambers's *Cyclopaedia* (see Section ii.b above), although it was defined as "a Discourse Concerning the Soul", it was regarded as part of anthropology and linked with Locke's views on physiology (Vidal 1993). In brief, it was moving away from theology and towards science.

Locke had followed Descartes in assuming regular correlations between brain and mind (although, unlike Descartes, he left open the possibility that matter might be inherently active, so capable of thought). But he'd described ideas in atomistic terms. On his view, the primitive ideas of "sensation" were passively received by the sense organs, and combined into complex ideas (in the brain) by "reflection".

Hume carried this approach further. In his vocabulary, the data of the senses were *impressions*, and the copies of them in memory were *ideas*. The mind, then, was made up of a host of impressions and ideas. He described it as a kind of theatre, where several perceptions successively make their appearance; pass, repass, glide away, and mingle in an infinite variety of postures and situations. (Hume 1739: I. iv. 6)

And the script, according to him, was written by Newton—or someone very similar: namely, himself.

Newton had suggested (in Query 31 of the *Opticks)* that if experimental science were to be perfected then "the Bounds of Moral Philosophy [i.e. psychology] will be also enlarged". Hume took up this suggestion in the introduction to his *Treatise of Human Nature*. The book was tellingly subtitled *Being an Attempt to Introduce the Experimental Method of Reasoning into Moral Subjects*, and if he didn't do what we'd now regard as properly controlled experiments, he did base much of what he said on careful observation and introspection. (His assumption that perception is basically passive prevented him from realizing that even introspection isn't pure, but theory-laden: see Chapter 16.iv.e.)

The mental associations he described in the *Treatise* were atomistic, automatic, and based on spatio-temporal contiguity—and on similarity, too. Indeed, he declared mental association to be "a kind of ATTRACTION, which in the mental world will be found to have as extraordinary effects as in the natural" (1739: I. i. 4). He explicitly suggested that the laws of thought were closely analogous to Newton's laws of gravitational attraction (Battersby 1978, 1979).

And the respect due to them was no less: "we may hope to establish . . . a [psychological] science *which will not be inferior in certainty, and will be much superior in utility*, to any other of human comprehension" (Hume 1739: 8; italics added). (Notice the mid-eighteenth-century assumption that physics, for all its intellectual glory, has scant "utility".) Similarly, in his *Enquiry Concerning Human Understanding*, Hume said:

But may we not hope, that philosophy, if cultivated with care, and encouraged by the attention of the public [see ii.c, above], may carry its researches still farther, and discover, at least in some degree, *the secret springs and principles, by which the human mind is actuated in its operations?* (Hume 1748: I. 15; italics added)

Strictly, Hume's wish to model psychology on *physics* can be distinguished from his commitment to *associationism*. In other words, someone might hold (1) that psychology should be modelled on physics, as the most fundamental of the sciences, without holding (2) that psychology should be atomistic and associationistic (that is, 'Newtonian': see Chapter 5.i.a).

Hume himself didn't make that distinction, because in his day physics and Newton were virtually synonymous. But some cognitive scientists today, who also wish to model psychology on physics, stress thermodynamics rather than Newtonian mechanics. Thought and behaviour, they argue, must be understood in terms of state transitions in dynamical systems (14.ix.b, 15.viii.x, ix, and xi, and 16.vii.c). Moreover, they must be understood as essentially *embodied*—so physics is relevant not because it's the fundamental science but because it's the science *of bodies*. Significantly, one leading proponent of this view describes himself as vindicating Hume's dream of a scientific psychology based on mathematical laws like those of physics (van Gelder 1992/1995). The specific "mathematical laws", however, are of course very different.

Newtonian associations, for Hume, weren't the only things to be considered in explaining thought: the emotions were crucial too. Reason, he said, is and ought to be "the slave of the passions"; it "can never pretend to any other office than to serve and obey [our emotions]" (1739: II. iii. 3. 4). He even declared: "'Tis not contrary to reason to prefer the destruction of the whole world to the scratching of my finger" (ibid.). In short, purely intellectual thinking can't tell us what to do, only how to do it.

In the years that followed, many scientific psychologists would forget Hume's stress on emotions. Not until the late twentieth century would it be widely realized that even *the most rational* thinking can't be understood without taking them into account (Chapter 7.i.d–f).

Hume's contemporary David Hartley (1705–57) also took inspiration from Newton—specifically, from his theory of light as involving vibrations, or waves. But whereas Hume had focused on ideas, Hartley (1749) concentrated on what might be going on in the *brain*.

Hartley posited (large and small) vibrations of infinitesimal particles in the nerves and brain, assumed to be "of the same kind with the oscillations of pendulums". These, he said, are "the physiological counterpart of ideas". He was a dualist—"Man consists of two parts, body and mind"—but not an interactionist. He believed, instead, that mind and brain involve parallel laws. So, just as physical waves take some time to die away, the experience of heat persists after the hot object has been removed and the image of a brightly lit window remains after the eyes are shut.

Moreover, mind and brain involve similar sorts of combination. Hartley's explanation of learning held that if only one part of an oft-repeated association recurs, it will be reconstructed as a whole both physiologically and mentally:

> Any sensations A, B, C, etc., by being associated with one another a sufficient Number of Times, get such a Power over the corresponding Ideas, a, b, c, etc., that any one of the Sensations A, when impressed alone, shall be able to excite in the Mind, b, c, etc., the Ideas of the rest. [And, analogously:] Any [nervous] Vibrations A, B, C, etc., by being associated together a sufficient Number of Times, get such a Power over a, b, c, etc., the corresponding Miniature Vibrations, that any of the Vibrations A, when impressed alone, shall be able to excite b, c, etc., the Miniatures of the rest. (Hartley 1749: i. 65, 67, Propositions 10, 11)

It's not surprising that Hartley is now thought of as an early connectionist, for this passage rings distinctly Hebbian bells in modern readers' minds (see Chapters 5.iv.b and 12). It might even be paraphrased as *Cells that fire together, wire together* (today's 'ft/wt' rule).

The eighteenth-century French physician Julien Offray de La Mettrie (1709–51) went even further—and earned a highly scandalous reputation as a result. Indeed, after publishing his book anonymously (in Holland), he had to seek the protection of Frederick the Great when his authorship was discovered—and he was never able to return to his native country (Vartanian 1960). Denying any distinction between mind and matter, he insisted that *matter itself* is active and feeling. He interpreted these terms in a strictly materialist way. As he put it, "the brain has its muscles for thinking, as the legs have muscles for walking". It was one of his followers, Pierre Cabanis (1757–1808), who famously said:

> [The brain is] a special organ whose particular function it is to produce thought just as the stomach and the intestines have the special work of carrying out the digestion, the liver that of filtering the bile, etc. (Cabanis, quoted in Brett 1962: 475)

In a book tellingly entitled *L'Homme machine* ('Man a Machine', 1748), La Mettrie cited Vaucanson, whose automata were touring Europe as he wrote it. He referred to various conditions (including cataract, drunkenness, and fever) wherein people's senses, thinking, and willpower, or self-control, are clearly dependent on the body. While he praised Descartes for being "the first to prove completely that animals are pure machines", he argued that the same applies to human beings:

> Since all the faculties of the soul depend to such a degree on the proper organization of the brain and of the whole body, that apparently they are but this organization itself, the soul is clearly an enlightened machine. For finally, even if man alone had received a share of natural law, would he be any less of a machine for that? A few more wheels, a few more springs than in the most perfect animals . . . any one of a number of causes might always produce this delicate conscience so easily wounded, this remorse which is no more foreign to matter than to thought, and in a word all the differences that are supposed to exist here. (La Mettrie 1748: 48)

> To be a machine, to feel, to think, to know how to distinguish good from bad, as well as blue from yellow, in a word, to be born with an intelligence and a sure moral instinct, and to be but an animal, are therefore characters which are no more contradictory, than to be an ape or a parrot and to be able to give oneself pleasure . . . I believe that thought is so little incompatible with organized matter, that it seems to be one of its properties on a par with electricity, the faculty of motion, impenetrability, extension, etc. (p. 64)

In short, human beings are *nothing but* complex automata, just as Descartes had declared animals to be.

Having cited Vaucanson's duck and flute-player with admiration, La Mettrie declared that "another Prometheus" might make a mechanical man that could talk (1748: 100). And, he added, there's no reason why one shouldn't teach apes to speak, for their physical organs appear to be perfectly suitable. (It took another two centuries for biologists to realize that this isn't true; a host of inter-species differences in the anatomy of the mouth, larynx, and respiratory muscles enable us to speak and prevent other primates from doing so: Lenneberg 1964: 34–51, 75–98.)

La Mettrie, and most of his readers too, believed that he was saying something which Descartes himself could easily have said. The implication was that only theological squeamishness, or what today might be termed "species-ism", had prevented Descartes from extending his doctrine of animal automatism to human beings. But this was a mistake.

The problem was that La Mettrie hadn't realized the special place of language (in particular, its endless generativity) in Descartes's argument that there could never be a plausible mechanical man (see Section iii.c, above). He thought, in effect, that the only problem facing the future Prometheus was to make the flute-player's mobile lips and tongue pronounce phonemes instead of blowing air. But Descartes would have readily conceded that possibility: a flute-player's body, like yours and mine, is indeed a machine—and, as such, might be copied by a superb engineer. His metaphysical problem wasn't with the hypothetical android's articulation of speech, but with its generation of language—something La Mettrie didn't discuss. In short, the seeming continuity between Descartes's *bête-machine* and La Mettrie's *homme machine* is "one which involves the inconsistent adaptation of half-understood views of one's predecessor" (Gunderson 1964*a*: 220).

La Mettrie's theory was comparable to behaviourism: the mind hadn't been explained, so much as explained away. Hardly anyone at the time went that far. Another French physician, the surgeon Claude-Nicolas Le Cat (1700–68)—inventor of the first ventricular shunt for hydrocephalus—apparently did so, even before La Mettrie. Most of his writings are now lost, including his intriguingly named treatise *Description d'un homme automate dans lequel on verra exécuter les principales fonctions de l'économie animale*, but his *Traité des sens* (1744) survives and gives a rigidly mechanistic account of the senses.

However, Le Cat and his fellow physician compatriot were unusual. In particular, they were unlike their philosophical neighbours across the English Channel. The British empiricists were interested in the mind *as distinct from* the body, even though they believed that it, too, followed mechanistic (quasi-Newtonian) laws.

The nineteenth century saw the rise of associationist psychologies aimed at detailing those laws. It also provided novel research programmes (such as psychophysics) aimed at discovering systematic mind–body correlations.

The most influential theories of the association of ideas included those of the philosophers James Mill (1773–1836) and his son John Stuart Mill (1806–73), and the pioneering psychologist Wilhelm Wundt (1832–1920). Wundt's influence was enormous because he founded professional psychology. He established one of the first two laboratories in 1879 (William James started the other), founded the first journal, and wrote extensive textbooks defining the field. In effect, it was defined there as the study of consciousness—so the prime experimental method wasn't the observation of behaviour, but introspection.

Wundt concentrated on sensation and perception, regarding the "higher mental processes", such as thinking, as lying beyond the reach of associationism. (That doesn't mean he didn't discuss them: see Chapter 9.v.a.) However, Hermann Ebbinghaus (1850–1909) generalized associationism to memory, by inventing the experimental method of nonsense syllables. Whereas *dog* has an unknown and uncontrollable number of previous associations, *dob* doesn't. It followed, of course, that Ebbinghaus had got rid of *meaning* too—for which he would eventually be reproached (see Chapters 5.ii.b and 6.iv.c).

Other nineteenth-century research tried to link psychology with a mechanistic neurophysiology—or anyway, with physical mechanism. For example, Ernst Weber (1795–1878) and Gustav Fechner (1801–87) initiated psychophysics. Their aim was to quantify the relations between specific physical stimuli and conscious sensations—something with which Descartes would have been content.

b. Non-empiricist psychologies

Although the empiricist paradigm was the dominant form of experimental psychology in the late 1800s, not all psychologists were drawn to it.

Animal psychologists, such as George Romanes (1848–94) and Conwy Lloyd Morgan (1852–1936), clearly couldn't use an introspective methodology. Indeed, Lloyd Morgan formulated his famous "canon" to reduce anthropomorphism as much as possible:

> In no case may we interpret an action as the outcome of a higher psychical faculty, if it can be interpreted as the outcome of the exercise of one which stands lower in the psychological scale. (1894: 53)

He wasn't forbidding "psychical faculties" outright: in that sense, he and Romanes were more alike than they're commonly thought to be (R. K. Thomas 2001). But neither of them could study their non-human subjects by making use of introspection. Moreover, they were interested in the functions—not just the underlying mechanisms—of animals' behaviour.

Some students of *human* psychology, too, took a different approach. Their criticism was more often directed against associationism than mechanism in the broader sense (one exception is mentioned below).

The introspectionists of the Würzburg School, and their successors the Gestalt psychologists, criticized Wundt's atomism. And William James (1842–1910), despite his general commitment to what we now call connectionism, famously remarked that Wundtian associationism "could hardly have arisen in a country whose natives could be *bored*" (James 1890: 192).

Never one to brush difficult questions under the carpet, James discussed a host of psychological conditions that couldn't be described, never mind explained, in the atomistic terms of empiricism. They included many examples drawn from psychopathology and hypnosis.

These phenomena had been studied (for instance) by the clinician Jean Charcot (1825–93) at the Salpêtrière hospital in Paris. (It was Charcot who identified the puzzling phenomenon of hysterical paralysis: Preface, ii.b.) And, of course, they were studied also by Freud (1856–1939). He'd worked on hypnosis and hysteria with Charcot for one year (1885–6), and with the physiologist Joseph Breuer for many more (from 1882 to 1895)—Ellenberger (1970, ch. 7).

These turn-of-the-century psychologists were less aggressively mechanistic than those in the empiricist tradition. To be sure, bodily mechanisms weren't ignored. But interpretative (hermeneutic, intentionalist) theorizing was also prominent.

The philosophical relation between these two forms of theorizing was controversial—and it still is (see Chapter 16). The two extremes were represented by Freud (1856–1939) and William McDougall (1871–1938).

Perhaps not surprisingly for a pupil of Brücke (who'd taken an oath to counter vitalism: see Section vi.d), Freud insisted that his interpretative theory was compatible with a strictly mechanistic neurophysiology (S. Freud 1895). McDougall, by contrast, explained human action and personality—and much animal behaviour too—partly in terms of psychic energy (*horme*) intrinsically directed to specific purposes, a view he defended at length in his book *Body and Mind: A History and a Defense of Animism* (1911).

McDougall's theories of motivation in everyday social life and clinical psychopathology (for instance, multiple personality disorder) showed great insight into mental architecture (see 5.ii.a, and Boden 1972, chs. 6–8). And his pioneering textbooks on physiological psychology and social psychology were hugely successful (1905, 1908). But his hypothesis of teleological energy was widely spurned. Even psychologists out of sympathy with associationism avoided such a radically anti-mechanistic position.

By the early 1900s, then, a scientific psychology grounded in mechanistic assumptions had got off to a strong start. But that was the only sense in which ‘man as machine’ encompassed the mind. Psychologists at that time *did not* draw on artefacts to find analogies for mental processes.

As for building a specifically psychological automaton, this wasn’t in sight: ‘machine as man’ was still restricted to mimicry of the body. The primitive calculators and logic toys weren’t thought of as psychological models (see Section ix, above), and no nineteenth-century engineer seriously suggested that a man-made device could produce behaviour comparable to the workings of the mind. Even the maverick Babbage didn’t think of his ambitious project in that light (see 3.iv). Still less did people imagine that an artefact might be intelligent *in just the same sense* as we are. Those ideas didn’t arise until later. Specifically, ‘mind as machine’ was first taken seriously in the 1940s (Chapter 4).

3

ANTICIPATORY ENGINES

Men of monumental achievement are often credited with even more than they deserve. That's true of Charles Babbage (1791–1871). Indeed, some readers may have been surprised by the conclusion of the previous chapter: that the mind-as-machine hypothesis hadn't emerged by the turn of the twentieth century. For in 1834 Babbage had designed his Analytical Engine: the first program-controlled, essentially general-purpose, digital computer. Because later examples of such machines were a crucial intellectual source for cognitive science, and also because of a handful of famous (but misinterpreted) quotations, his work is often assumed to be an early step in the field.

That's a mistake. One must resist the temptation to see Babbage as a cognitive version of Jacques de Vaucanson (see Chapter 2.iv), an ingenious engineer whose automata modelled not lips and fingers but thoughts. For Babbage didn't attach any psychological significance to his project.

It's not even clear that he was an important historical influence for the development of electronic computers. His Analytical Engine involved startling anticipations of these modern machines, to be sure. To anticipate, however, isn't necessarily to influence. Babbage's effect on computing technology 100 years later is controversial, being variously described as positive, absent, and even negative.

As for his historical role in the development of *ideas about mentality*, this was zero. I don't mean that he was unjustly neglected. Rather, I mean that he had no thought of modelling minds. Mind-as-machine was first taken seriously in the middle of the twentieth century, not the nineteenth (see Chapter 4).

This chapter, then, is a necessary digression. Before resuming our main theme, we need to understand Babbage's paradoxical relation—superficially close, yet fundamentally irrelevant—to cognitive science.

Babbage's general philosophy is outlined in Section i, and his two remarkable Engines in Sections ii and iii. In Section iv, I explain why, contrary to widespread belief, his work was irrelevant to mind-as-machine.

Section v describes the invention of the modern computer. This process involved two of the 'founding fathers' of cognitive science, and informed its early theories in various ways. Finally, Section vi asks what influence Babbage's research had on computer technology. We'll see that some people regarded his work as over-optimistic techno-hype, with counter-productive effects like those which would blight various types of AI in the 1970s and late 1980s.

3.i. Miracles and Mechanism

Babbage was interested in many other things besides machines, and many other machines besides those for which he's famed today. Moreover, he drew many parallels between machines and other matters—including factory organization, and even theology.

a. Babbage in the round

Born only two years after the French Revolution of 1789, Babbage was widely recognized during his lifetime as one of the most progressive thinkers of his day. He was at the hub of England's intellectual society, constantly interacting with outstanding political, literary, and scientific figures. And he had close contacts with intellectuals in France and, to a lesser extent, Italy. He'd still be regarded, today, as a historically significant figure even if he'd never given a thought to automatic computation.

Babbage's strengths were personal as well as intellectual. As his friend and literary executor Harry Buxton put it: "No man ever enjoyed society with a greater relish than he, and few, indeed, were better calculated to adorn it" (Buxton 1880: 356). (Sadly, this became less true as he grew older, and burdened with disappointments: see below.)

The early feminist Harriet Martineau was one of his many friends. In her autobiography, she remarked on his "exemplary patience" and "genuine good nature" when a "lady examiner" visiting his salon, on being shown his miniature Difference Engine (see Section ii.b), naively asked: "Now, Mr. Babbage, there is only one thing that I want to know. If you put the question in wrong, will the answer come out right?" (quoted in Hyman 1982: 129).

(Such a query isn't always foolish. Babbage's later Engine had a self-correction facility, as we'll see in Section iii.a. Moreover, user-friendly AI interfaces, not to mention spell-checkers, enable today's machines to compensate for users who put the question in wrong: Rich *et al.* 2001; cf. Chapter 13.v.)

Nor was this naive "lady examiner" Babbage's only fan. Martineau, again: "All were eager to go to his glorious soirées; and I always thought he appeared to great advantage as a host." Evidently, the attractions of his soirées weren't only intellectual: the young Charles Darwin, recently disembarked from the *Beagle*, was advised by a friend to attend Babbage's house to meet some "pretty women" (Swade 1996: 44). In short, Babbage was for many years the toast of London's intellectual community, admired by everyone who was anyone.

Yet the title page of his autobiography, published seven years before he died, carried a curious snippet about an oyster-scientist, who "instead of countenance, encouragement, and applause . . . was exposed to calumny and misrepresentation". And in the closing pages he described himself as "a father whose name in his own country has been useless to himself and to his children" (Babbage 1864: 364).

The reason for this bitterness, contrasting with his previous ebullience and sociability, was his failure to win continuing financial support for the Analytical Engine, his prime interest during the last forty years of his life (see Section iii). It may be that an earlier lapse of his usual sociability had been largely to blame. Already frustrated, he behaved very badly—and very stupidly—when meeting the Prime Minister in 1842 to ask for more funds (Swade 1996: 47–8).

Professionally, Babbage was a mathematician, one who—with friends such as George Peacock—contributed to important advances in 'analytical' algebra (Pratt 1987: 93–7). He used his mathematical skills to advance cryptology too, an interest he'd nurtured from his schooldays: he was widely recognized as an authority, and was regularly sent coded messages to decipher (Babbage 1864: 173–9; Buxton 1880: 346–7; Kahn 1996: 204–7). He held the Lucasian Chair of Mathematics at Cambridge for eleven years, a post formerly filled by Isaac Newton—who, unlike Babbage, had actually delivered some lectures. Nevertheless, he scandalized many of his compatriots by championing the 'French' (originally, Leibnizian) version of the differential calculus, rather than Newton's.

He was also a highly accomplished engineer, and—like Vaucanson—a widely consulted expert on machine tools. His work on automatic calculators resulted in significant advances in precision engineering. He was deeply involved in the accelerating industrialization of the time, and played a prominent part in the economic and political debate associated with it.

His fascination with moving gadgets had started early:

> During my boyhood my mother took me to several exhibitions of machinery. I well remember one of them in Hanover Square, by a man who called himself Merlin. [Remarking my great interest, the exhibitor] proposed to my mother to take me up to his workshop, where I should see still more wonderful automata. We accordingly ascended to the attic. There were two uncovered female figures of silver, about twelve inches high.
>
> One of these walked or rather glided along a space of about four feet, when she turned round and went back to her original place. She used an eye-glass occasionally, and bowed frequently, as if recognizing her acquaintances. The motions of her limbs were singularly graceful.
>
> The other silver figure was an admirable *danseuse*, with a bird on the forefinger of her right hand, which wagged its tail, flapped its wings, and opened its beak. This lady attitudinized in a most fascinating manner. Her eyes were full of imagination, and irresistible.
>
> These silver figures were the chef-d'oeuvres of the artist: they had cost him years of unwearied labour, and were not even then finished. (1864: 12)

Many years later, after the wizardly "Merlin" had died, his effects were sold at auction. Babbage bought the lady-with-the-bird. Seeing that "No attempt appears to have been made to finish the automaton; and it seems to have been placed out of the way in an attic uncovered and utterly neglected" (1864: 273), he took the mechanism to pieces, and restored it.

He asked "one or two of my fair friends" to "supply her with robes suitable to her station". But it wasn't only the fair friends who took an interest in her wardrobe: Babbage himself fixed "a single silver spangle" to each of her small pink satin slippers, and "a small silver crescent in the front of her turban". He'd even made the turban, out of pink and light green crepe and "a plaited band of bright auburn hair". Satisfied with her appearance at last, he lodged the Silver Lady in a glass case in his drawing-room (p. 274). In the next room, as we'll see, he placed an even more amazing—not to say miraculous—mechanism (and the juxtaposition intrigued his many friends: Schaffer 1996).

If Babbage respected the social conventions in clothing the Silver Lady so decorously, he didn't do so in his writings, or his politics. In those areas, his radical—and polemically expressed—ideas aroused attention and controversy (Hyman 1989). His polemics were often ill-judged: tact wasn't one of Babbage's strong points. But many of his challenging suggestions, scorned in his lifetime, are now commonplace.

He recommended the introduction of decimal currency, flat-rate postage, and life peerages. He outlined new principles of taxation and life insurance. He suggested that lithography might somehow be used to reproduce facsimiles of out-of-print books. And he argued that science should be included in the general university education, criticizing the aristocracy and industrialists for their ignorance of scientific matters.

But science was one thing, scientists something else. Naming names with abandon, Babbage (1830) publicly scorned the intellectual mediocrity—and, he claimed, the venality—of the Royal Society at that time. (His attack prompted some rethinking in the Society's rooms: from 1847, Fellows were elected solely on the basis of scientific merit.) Naturally, if Babbage was prepared to be slanderous in public he was equally ready to compose calumnies in private. One of his autograph letters (16 June 1854) contained an "epigram" scorning two former Royal Society presidents:

> Methinks I've seen three things look wondrous small:
> A penny loaf in Davies Gilbert's hall;
> A tiny flee upon a lion's hide,
> And Banks' marble block by honoured Newton's side.
>
> (quoted in J. M. Norman 2004: 56)

Davies Gilbert, president in the late 1820s, had been explicitly named and shamed in Babbage's book. He wasn't a scientist, but a littérateur and historian (of Cornwall). Although he'd used his money and his political contacts to support fine scientists such as Sir Humphrey Davy, Babbage had little time for him. As for the aristocratic Joseph Banks (1743–1820), who'd explored the South Pacific with Captain Cook on the *Endeavour*, he was a fine botanist (and founder of Kew Gardens), and a champion of science in general. Hence the marble bust, placed next to Newton's portrait in a Royal Society meeting room by some of his admirers. But he was also vain, arrogant, and ostentatious—all qualities exacerbated by his forty-two-year presidency (from 1778 to his death), and all guaranteed to arouse Babbage's contempt.

When he wrote that mocking squib, Babbage wasn't a disinterested critic. For in 1831, soon after his book appeared, he'd co-founded the British Association for the Advancement of Science. He constantly compared the Royal Society unfavourably with the British Association, not least because of its snobbish exclusivity. The "British Ass" has since gone from strength to strength, and today is hugely effective in bringing science to the public. It's just one example where Babbage helped to bring about important reforms.

In many other cases, however, his provocative ideas gathered dust for decades. Some are still gathering. At the turn of the third millennium, the House of Lords in the British Parliament still retained nearly 100 hereditary peers—and they're not yet ousted, in 2005.

Babbage's most widely read book, which influenced both Karl Marx and John Stuart Mill, was *On the Economy of Machinery and Manufactures* (Babbage 1832/1835; Hyman 1982, ch. 8). This discussed the organization of a factory or other large institution. In the Preface, Babbage said it had grown from the ideas he'd had while designing his "calculating engine", and remarked that it was based on several chapters he'd written for the "mechanical" section of the *Encyclopaedia Metropolitana* of 1829. The thirty-volume encyclopedia may not have been a best-seller, but Babbage's book was. It ran to four editions within three years, and was translated into six foreign languages.

It gave a general recommendation of the division of labour, illustrated (for example) by nine eye-opening pages on the manufacture of pins (Hyman 1989: 132–40). In addition, the book included detailed recommendations on costing and marketing; on industrial relations and profit sharing; on 'clocking-in', for which he invented a suitable machine; and on the application of science in industry. One chapter, later published separately, advised on how to invent machinery, and how to combine machine tools for manufacturing purposes. He foresaw a time when machines would take over the basic repetitive tasks, while the human workers concentrated on "mental" labour.

As if the book weren't already wide-ranging enough, he closed his review of 'The Future Prospects of Manufactures, as Connected with Science' by venturing into theology. Science, he declared, has given us "resistless evidence of immeasurable [divine] design", and reason to believe in extra-terrestrial intelligence:

> [It] would indeed be most unphilosophical to believe that those sister spheres . . . should each be no more than a floating chaos of unformed matter;—or, being all the work of the same Almighty architect, that no living eye should be gladdened by their forms of beauty, that no intellectual being should expand its faculties in decyphering their laws. (Hyman 1989: 200–1)

As these remarks indicate, Babbage was an unorthodox but committed believer, who regarded religion as "the highest calling of man". This didn't prevent his arguing that bishops were educationally unfit to govern in the House of Lords—"My Lord Bishop" wasn't, and still isn't, just an empty phrase (Hyman 1989: 207). Nor did it forbid his remarking that the Athanasian Creed appeared to be "written by a clever, but most unscrupulous person, who did not believe one syllable of the doctrine" (Babbage 1864: 302). But despite these characteristically 'contrary' opinions, his religious faith was secure.

b. Religion and science

Babbage was much concerned by the growing tendency for people to see science as fundamentally opposed to religion—a tendency encouraged by Romanticism (see Chapter 2.vi). He was equally concerned by an influential book arguing that his particular type of science could contribute nothing positive to theology.

William Whewell (1794–1866) had distinguished two sorts of scientific reasoning: deduction and induction (Whewell 1833). A deductivist science, he said, tends to irreligion, but the inductive method doesn't. Whewell was no less keen than Babbage that science should pose no threat to religion. Indeed, his religious scruples would lead him, a quarter-century later, to prevent *On the Origin of Species* from being placed in the library of Trinity College, Cambridge, where he was Master (Browne 2002: 107).

Deduction leaves us no choice, and implies that everything that exists had to be as it is. By contrast, Whewell argued, induction requires a visionary faith in the possibility of deciphering nature so as to glimpse the mind of God. The reason is that scientific laws can show how the physical world is sustained from day to day, but not how it—or its fundamental forces, such as gravity—came to exist in the first place. Biological phenomena, he said, require a distinctive (teleological) type of explanation, not expressible in deductivist terms. He even argued that "deductive" scientists lacked "any authority" in natural theology, and that "we have no reason whatever to expect from their speculations any help, when we ascend to the first cause and supreme ruler of the universe".

Babbage, whose research—on the manufacture of pins, as well as on calculating machines—took a highly deductivist approach, didn't agree. What's of interest for our purposes is the apparently 'modern' nature of his reply, presented in *The Ninth Bridgwater Treatise*. This title cheekily attached his volume to the eight official *Bridgwater Treatises* on natural theology, of which Whewell's had been the first—and Charles Bell's another (see 2.viii.f). (Why there was a need for natural theology in the first place is discussed in Chapter 8.vi.)

Babbage held that the scientific laws we have discovered "converge to some few simple and general principles, by which the whole of the material universe is sustained, and from which its infinitely varied phenomena emerge as the necessary consequences" (Hyman 1989: 209). This comment may have come as no surprise from someone who was on visiting terms with the notorious determinist Pierre Laplace (1749–1827). What distinguished it from a mere post-Laplacian banality was Babbage's view on the origin of those "infinitely varied phenomena".

He argued, in effect, that God is a cosmic programmer whose foresight enabled Him to ensure the emergence of new, and humanly unpredictable, types of natural structure. Examples of such emergence include the appearance of novel species (believed at that time to be sudden), and biological metamorphosis:

> The laws of animal life which regulate the caterpillar, seem totally distinct from those which . . . govern the butterfly . . . [These changes] were equally foreknown by their Author: and the first creation of the egg of the moth . . . involved within its contrivance, as a necessary consequence, the whole of the subsequent transformations of every individual of [its] race. (Hyman 1989: 213)

Read out of context, this passage might seem to be groundless intellectual hand-waving. After all, the physiology of the 1830s couldn't even explain body temperature, never mind butterflies or embryology (2.vii). It's not surprising that Whewell, along with many others, saw such phenomena as requiring a special form of explanation. But Babbage claimed, amazingly, that a machine he'd built himself could do essentially the same sort of thing.

The machine was designed to calculate a series, or table, of numbers according to some arithmetical rule: for example, successive squares, or the iterated addition of 7 to the previous number. In such a device, the eventual appearance of periodic and/or apparently random changes could be built in from the start, by arranging that one rule of progression would automatically be replaced by another when a certain numerical value was exceeded. This type of behaviour is intelligible only deductively, not inductively. Induction alone can be seriously misleading: even if "one unbroken chain of natural numbers [passed] before your eyes, from *one* up to *one hundred million*", the next number, or the one after that, could be totally unexpected. The sudden change in the machine's behaviour would seem utterly mysterious to observers ignorant of the underlying rationale:

In contemplating the operation of laws so uniform during such immense periods, and then changing so completely their apparent nature, whilst the alterations are in fact only the *necessary* consequences of some far higher law, we can scarcely avoid remarking the analogy which they bear to several of the phenomena of nature. (Hyman 1989: 213)

By "several of the phenomena of nature", Babbage meant biological metamorphosis, embryological development, and historical 'leaps' from one set of biological forms (fossils) to another. Indeed, the latter suggestion was taken up by Robert Chambers (1802–71) in his *Vestiges of the Natural History of Creation* (1844: 206–11).

Chambers's book, which created a sensation on its (anonymous) publication in 1844, was a provocative defence of evolution—and, like Darwin's some years later, it had to account for the visible discontinuities in the fossil record. After all, in our experience of flora and fauna, "like produces like". Having quoted Babbage at some length, Chambers continued:

[The] gestation (so to speak) of a whole creation is a matter probably involving enormous spaces of time . . . All, therefore, that we can properly infer from the apparently invariable production of like by like is, that such is the ordinary procedure of nature in the time immediately passing before our eyes. Mr. Babbage's illustration [of how entirely unexpected numbers could be produced by his Difference Engine] powerfully suggests that this ordinary procedure may be subordinate to a higher law, which only *permits* it for a time, and in proper season interrupts and changes it. (Chambers 1844: 211)

(Darwin, of course, would substitute natural selection for Chambers's 'computational' explanation of species change.)

Not content with explaining only biological wonders such as butterflies and fossils, Babbage went even further. He suggested that analogous rule changes could explain religious miracles too. On his view, a miracle isn't some last-minute interference by the hand of God. Rather, it's a divinely preordained singularity falling between two similarly preordained rule changes, the second of which restores the laws of nature that were in effect before the occurrence of the miraculous event.

This logical possibility was mentioned, but without theological comment, in the first published description of Babbage's mathematical machine:

[The] very nature of the table itself may be subject to periodical change, and yet to one which has a regular law . . . [Tables] are produced, following the most extraordinary, and apparently capricious, but still regular laws. Thus a table will be computed, which, to any required extent, shall coincide with a given table, *and which shall deviate from that table for a single term, or for any required number of terms, and then resume its course*, or which shall permanently alter the law of its construction. (Lardner 1834: 95–6; italics added)

The author—not Babbage himself, but a supporter—remarked that it might be impossible for the observer to predict the machine's behaviour inductively, or to discover "any function . . . capable of expressing its general law". In other words, like systems termed chaotic in modern terminology, this machine was deterministic but unpredictable.

3.ii. Differences that Made a Difference

The miraculous machine mentioned in Section i.b was Babbage's Difference Engine. Designed in the early 1820s, this was the first of his two automatic calculators. The second, his Analytical Engine—on which he worked from 1832 until his death—was even more marvellous. It's the Analytical Engine (to be described in Section iii) which anticipated the modern computer.

a. Division of labour, again

The Difference Engine was designed not to do individual sums ($2 + 2 = 4$), but to carry out long series of interconnected calculations. These were necessary for preparing mathematical tables "of infinite extent and variety" for use in astronomy and mathematics, and in practical activities such as navigation and commerce. They included, for instance, "tables of the moon's place for every hour, together with the change of declination for every ten minutes" (Lardner 1834: 55–61).

Such tables existed already. But they were known to be riddled with thousands of errors. Indeed, the published lists of errata would often introduce new, and sometimes worse, errors (Lardner 1834: 61–70).

The basic reason for this wasn't that the calculations were difficult, but that they were repetitive and boring. Indeed, when Babbage had been verifying some tables for the recently founded Royal Astronomical Society in 1820–1, he'd said in exasperation, "I wish to God these calculations had been executed by steam" (Buxton 1880: 46). Moreover, the printers often made mistakes when reading handwritten figures, or when converting them into movable metal type.

About 140 years earlier, Gottfried Leibniz had longed for a machine—not, of course, executed by steam—to compile mathematical tables (Chapter 2.ix.a). And he too had referred to the advantage of their providing "sure results". But he'd designed/built only a highly minimalist version. Indeed, even that description is overly complimentary: what he'd designed was no more than a small hand calculator. Babbage's Difference Engine was quite another kettle of fish.

The mathematical method that Babbage used in the Difference Engine was inspired by the method used by the French mathematician Gaspard de Prony to prepare tables of logarithms (Hyman 1982: 44). De Prony's aim had been to devise a method that could be executed by relatively uneducated clerks. A highly accomplished mathematician would identify the relevant formula (governing changes in the moon's declination, for example), expressing it in terms of mathematical functions that could be numerically calculated by simple steps. Next, a few competent mathematicians would insert the relevant numbers (data) into the equations. Last, a large number of clerks would then do the repetitive arithmetic.

This division of labour, suggested to de Prony by his reading of Adam Smith's *The Wealth of Nations* demanded very little from the clerks. All they had to do was to add and subtract. (Over 100 years later, de Prony's approach was still in use. The untrained people doing the sums for the Los Alamos implosion bomb "were assigned different tasks—adding, multiplying, cubing, and so on—in a kind of reconfigurable arithmetical assembly line": MacKenzie and Spinardi 1995: 225.)

Babbage approved of the division of labour, here as elsewhere. As he put it in his book on factory organization,

> The master manufacturer, by dividing the work to be executed into different processes, each requiring different degrees of skill or of force, can purchase exactly that precise quantity of both which is necessary for each process; whereas, if the whole work were executed by one workman, that person must possess sufficient skill to perform the most difficult and sufficient strength to execute the most laborious, of the operations into which the art is divided. (Babbage 1832/1835: 226)

His Difference Engine was a mechanical embodiment of this manufacturers' maxim. Specifically, his "method of finite differences" used in the Engine was a variation of de Prony's approach. (The idea came full circle, from economics and manufacturing to mathematics and back again: the 1833 edition of *Economy* added a long footnote describing how de Prony's division of labour had been implemented in the machine.)

It's possible that Babbage's method of differences was part-inspired also by Johann Müller, who had employed a similar principle in 1784 (see 2.ix.a). Müller had suggested also that a machine might print its results automatically—which Babbage's machine did. Babbage became aware of Müller's work, some of which was translated for him by his close friend John Herschel, the astronomer. However, it's not known when this translation was done, and it's unclear whether Müller's ideas, as well as de Prony's, influenced Babbage's earliest design (Pratt 1987: 103; Swade 1991: 21–2).

The Difference Engine was basically an adding machine. Used iteratively, it could also do multiplication. It performed a series of additions, where the result of one addition was used as the starting point for the next. The number that was added to the starting point at each step could either remain constant, or change at predetermined points. (Hence the miracles.) The machine checked the accuracy of its results, and printed them as tables or graphs—thus eliminating clerical and printers' errors.

As a practical engineer, Babbage discussed various ways of doing the printing. His first plan involved 30,000 pieces of movable type, to be fed one at a time on the instructions of the calculating part of the engine. Another used type fixed onto the rims of wheels, similar to the printing wheels commonly used today. But the real interest of the machine lay in the principles involved.

Adding machines, of course, weren't new. Besides early forays by Hero of Alexandria and others, many commercial designs had been developed since Blaise Pascal sent his cogwheels summing in 1642 (see Chapter 2.ix.a). But they were less widely used than one might think. Most people wanting help in calculation preferred to use logarithmic tables, slide rules, or Napier's bones as aids (Pratt 1987: 38, 83–5).

Moreover, all 'automatic' calculators prior to Babbage had required the continual intervention of the human operator. For example, separate 'carry wheels' might store information that had to be manually fed into the calculation by the user. Or the result of one calculation would have to be newly entered by a human, so as to function as the input for the next. Using the most recent output as the new input was a crucial aspect of multiplication-by-addition, and of some other mathematical procedures—including the method of differences itself. So removing the human from the iterative loop would save enormously on effort.

The Difference Engine achieved this: it was the first machine that could carry out a sequence of calculations in a fully automatic way. As Babbage put it, it was the first to be "self-acting" (Babbage 1864: 30).

b. Design and disappointment

The calculating part of the Difference Engine was constructed from geared metal wheels mounted on columns. Each number wheel had ten teeth (for the digits 0–9), the wheels for units, tens, hundreds, etc. being placed above one another on the relevant column. The first column stored the starting number, the others the (hierarchy of) numbers to be added. The machine was prepared by setting the wheels to the relevant starting positions by hand, and was operated by turning a handle.

A few wheels took no part in the actual calculations, but provided helpful "memoranda" to the user. An ingenious "anticipatory carry" mechanism enabled the Engine to add 1 to 9,999,999 fairly quickly, without enduring the mechanical equivalent of a nervous breakdown. And bells informed the user when the calculations were completed (much as my new microwave instructs me to OPEN THE DOOR when the food is cooked).

Babbage himself gave a brief explanation of the Difference Engine, imagining how "papa" and "mamma" might comment on a game of marbles (Babbage 1864: 30–50). But the details don't concern us here. A clear discussion, written with Babbage's guidance, can be found in Lardner (1834).

(Readers interested in the engineering aspects can find reproductions of some of Babbage's fine mechanical drawings in Hyman 1982, 1989. For his mechanical notation, an "algebra" of mechanism capable of describing any conceivable machine, see Babbage 1826; Lardner 1834: 99–106; Hyman 1989: 312–17; Buxton 1880, ch. 9. For further details, see Lovelace 1843; Buxton 1880; Bromley 1990, 1991; Swade 1993. For its reception by mathematicians and machinists at the time, see Schaffer 2003*a*: 266 ff.)

Thus far, I've described the Difference Engine as though it were an actual machine. This is misleading in two ways. First, the Difference Engine—and the Analytical Engine too—was in fact a *class* of machines.

The more wheels per column, the larger the numbers that could be represented. And the more columns, the higher the powers (squares, cubes, etc.) that could be computed. Babbage's most ambitious engineering design would accept numbers of up to eighteen digits. But considered as an abstract mathematical device, like the Turing machine described in Chapter 4.i, the Difference Engine could compute *any* polynomial equation.

A further complication is that Babbage's first machine was superseded in 1847–9 by the Difference Engine-2. This was a more powerful, elegant, and much smaller version, whose design had benefited from his work on the Analytical Engine.

Second, the Difference Engine-1—again, like the Analytical Engine—was never actually built. Or rather, the full-scale Engine was never completed. It would have had over 25,000 parts, weighing several tons, and would indeed have needed "steam" to power it.

Babbage did, however, construct a miniature version. Having built a working model illustrating the basic arithmetical principle in 1820–2, he instructed his (superb) toolmaker Joseph Clement to build a small Difference Engine ten years later. This hand-cranked device, constituting about a seventh portion of the full-scale Engine, had nearly 2,000 parts. Its three columns, each with six wheels, could deal with five-digit numbers and square-powers (and cubes up to 9). And it included the anticipatory carry and an automatic printer. This was the machine exhibited, to universal amazement, in Babbage's drawing-room at the "glorious soirées" described by Martineau. (It's now in London's Science Museum—rescued for posterity much as his Silver Lady had been.)

A year later, in 1833, the construction of the Difference Engine was abandoned—to Babbage's lasting distress. When "Babbage's Engine" was demonstrated at London's International Exhibition in 1862, it was the small version completed thirty years earlier that was put on show.

In a just world, it should have been shown at the Great Exhibition of 1851, held in the specially built Crystal Palace in Hyde Park. Indeed, Babbage should have been allowed to accept the organizers' invitation to head the exhibition's Industrial Commission. But the government refused him permission to have anything to do with the event, including showing his machine—probably the finest product of precision engineering to date. (He got his revenge, if not his just deserts, by publishing a vitriolic account of the event and of its organizers—and by including his ideas about how it *should* have been run: Babbage 1851.)

As for calculating machines inspired by Babbage and built by his contemporaries, two small-scale versions of the Difference Engine were manufactured in Sweden by George and Edward Scheutz in the 1850s (Scheutz and Scheutz 1857). These coped with four orders of difference, and up to fifteen digits. Although logically similar to Babbage's Engine, they were mechanically different—and engineered at a much lower level of precision. But they were generously praised by Babbage at the time, and also in his autobiography later (Hyman 1982: 239–40; Babbage 1864: 35). They were featured in the *Illustrated London News*, which suggested that a small dog on a treadmill would be able to turn the handle. And one was bought by the General Register Office in London, to calculate tables for life insurance. Nevertheless, Scheutz's machines weren't easy to use, and weren't a commercial success. (The Register Office soon reverted to manual calculations with logarithms, which they used until switching to mechanical calculators in 1911.) George Scheutz died almost bankrupt in 1873, and his son achieved that dismal fate a few years after him.

Other people—in London, Sweden, and America—also tried to build Difference Engines, and one or two succeeded (Swade 1991: 14–21). But they, too, lost money as a result. Their machines were soon forgotten, to be rediscovered only with the computer-age renaissance of interest in Babbage. Scheutz's first engine, for instance, lay ignored in a Stockholm museum until 1979.

Just why Babbage failed to build his full-scale Difference Engine (or his Analytical Engine, either) is a matter of dispute. Six reasons are commonly suggested.

Arguably, he was betrayed by the politicians—after having received significant financial support, in the hope of improving navigation. In the 1850s the Chancellor of the Exchequer Benjamin Disraeli ruled: "The ultimate success [is] so problematical, the expenditure certainly so large and so utterly incapable of being calculated, that the Government would not be justified in taking upon itself any further liability." Babbage tartly noted, later, that his Difference Engine could have been used to "calculate the millions the ex-Chancellor of the Exchequer squandered" (Babbage 1864: 81).

Conceivably, his ideas were too close to the workbench to be quite respectable. His associate Dionysius Lardner had judged it "more a matter of regret than surprise" that his mechanical notation received little attention: "In this country, science has been generally separated from practical mechanics by a wide chasm" (Lardner 1834: 104). I'm reminded of Plato's assumption, quoted in Chapter 2.i.a, that one wouldn't wish an engineer to marry one's daughter.

Possibly (and thirdly), Babbage's outspokenness, and increasing bitterness, didn't endear him to those who might have been able to help. For instance, his reference to Disraeli's "squandered millions" was followed by his sneer that the novelist–Chancellor's mathematical understanding was surely surpassed by "any junior clerk in his office"—and many similar examples could be cited. Probably, he was let down by Clement's intransigence, and perhaps dishonesty. And apparently, the potential of his engines was unappreciated even by most of the scientific community.

All these five factors, very likely, contributed in preventing the building of his machine. The sixth reason often cited is more questionable: that building a full-scale Engine (as opposed to the mini-versions manufactured by Scheutz, for instance) was then impossible—or, at least, not practically feasible. The British Association may have been right in deciding, a few years after his death, that building a Difference Engine wasn't an affordable project. But we now know that it wasn't absolutely impossible.

A full-scale Difference Engine-2 was built to Babbage's original designs in his bicentenary year, 1991, and the printer was added in 2000 (Swade 1993; 2000: 252–307). The core Engine and the printer were each made of about 4,000 parts. They were constructed using only materials and methods available to Babbage, except that modern techniques were used to make often repeated parts. Even these 'mass-produced' parts showed variations that had to be filed down by hand: otherwise, the twentieth-century Engine wouldn't have worked.

This work of reconstruction was undertaken by London's Science Museum, where the large Difference Engine is now on permanent exhibition. But it wasn't all plain sailing. Like Babbage himself, the scientists responsible for this recent project suffered many vicissitudes. These included the sudden bankruptcy of the engineering company originally contracted, and a crucial deadline met with only minutes to spare (Swade 1993: 67). If one believed in jinxes, the Difference Engine would surely be a candidate.

3.iii. Analytical Engines

Intriguing though the Difference Engine was, it was surpassed by the even more amazing Analytical Engine—the cause of Babbage's greatest pride and bitterest disappointment.

From 1832 onwards, most of his formidable energies were devoted to the invention of this mechanical calculator. He saw it as "wholly independent" of its predecessor, not as a development of it (Lovelace 1843: 275). In his view, the Analytical Engine embodied foresight, whereas the Difference Engine had embodied only memory.

a. From arithmetic to algebra

Babbage's first design for the Analytical Engine dated from 1834, and he improved it throughout the rest of his life. It was highly ambitious as an engineering project, requiring a large number of near-identical parts—for which he experimented with die casting and sheet-metal stamping. Various small portions were built, and he was working on a small trial model when he died. But an entire machine was never completed.

Again, the details needn't concern us (but see Menabrea 1843; Lovelace 1843; Buxton 1880, chs. 7–10). The important point is that, whereas the Difference Engine had been confined to arithmetic, the Analytical Engine could deal with algebra.

It was extremely general, for it could find the value of almost any algebraic function. This generality alone would make it interesting to us, not least in the light of Alan Turing's work on universal machines (see Chapter 4.i). But its interest is greatly increased by the fact that *in essence* it was a general-purpose, program-controlled, digital, symbol-manipulating computer that included many specific features found in modern machines.

Although general-purpose *in essence*, the Analytical Engine was designed to deal only with numbers. Babbage knew, from Boole's work in the 1840s, that algebra can be mapped onto logic (see Chapter 2.ix.b). That is, he realized that the general *class* of possible Analytical Engines included machines capable of dealing with logic as well as mathematics. But he didn't try to design such a machine. As an engineering project, his Analytical Engine was a special-purpose device dedicated for mathematical use.

It was like its predecessor, the Difference Engine, in being constructed from columns of interlocking toothed wheels. Having considered using a binary mechanism, Babbage again chose decimal. However, the data and instructions were entered not by manual wheel setting, but by punched cards. As noted in Chapter 2.iv.c, Joseph Jacquard—following Vaucanson—had used punched cards for weaving fancy textiles. (The technique is described in Menabrea 1843: 254–5; Lovelace 1843: 283–4.) This wasn't a technology to be sneezed at: in Babbage's drawing-room there hung a portrait of Jacquard, woven in silk on a loom controlled by 24,000 cards, each with the capacity for 1,050 holes.

It was the use of sets of punched cards, automatically read one-by-one by the machine, which justifies our describing the Analytical Engine as 'program-controlled', and which often leads people to describe it also as a 'stored-program' machine. Babbage's method enabled an entire program to be provided in one step, as a single batch of cards, to be followed without further human intervention. To make it follow a different program, he didn't have to tinker with the wheel settings, as he'd done for the Difference Engine. Instead, he merely had to exchange one batch of cards for another. But the Engine's program wasn't stored internally, as in modern computers: see Section v.b. Since the card reader operated at much the same speed as all the other components, there wouldn't have been any advantage in this.

The Engine's cards had three main purposes. Some stored the numerical value of a constant (a number of up to fifty digits). Some represented a variable (determining the column on which the number would be placed). And some defined the operations to be performed (addition, division, etc.). Several operational cards could be combined and treated as a unit, allowing for repetitive looping and nested subroutines.

There was a separate "store" and "mill", for numbers being held and numbers being worked on. As in the Difference Engine, there was a facility for making (inoperative) "memoranda" reminding the user of what was going on. And there was even a way of ensuring self-correction if some of the number wheels were set to wrong figures during the calculation (Buxton 1880: 249–50).

b. Programs . . . and bugs

The startlingly modern flavour of the Analytical Engine, including the constraints involved in using it efficiently, is evident from these contemporary descriptions (taken, unless otherwise noted, from Lovelace 1843):

The Analytical Engine is therefore a machine of the most general nature. Whatever formula it is required to develop, the law of its development must be communicated to it by two sets of cards. When these have been placed, the engine is special for that particular formula . . .

Every set of cards made for any formula will at any future time recalculate that formula with whatever constants may be required.

Thus the Analytical Engine will possess a library of its own. (Babbage 1864: 90)

The use of the cards offers a generality equal to that of algebraical formulae, since such a formula simply indicates the nature and order of the operations requisite for arriving at a certain definite result, and similarly the cards merely command the engine to perform those same operations . . . In this light the cards are merely a translation of algebraical formulae, or, to express it better, another form of analytical notation (Menabrea 1843: 264)

. . . the collection of columns of Variables may be regarded as a *store* of numbers, accumulated there by the mill, and which, obeying the orders transmitted to the machine by means of the cards, pass alternately from the mill to the store, and from the store to the mill, that they may undergo the transformations demanded by the nature of the calculation to be performed. (Menabrea 1843: 257)

[We] may, if we please, retain separately and permanently any *intermediate* results . . . which occur in the course of processes [having] an ulterior and more complicated result as their chief and final object . . . (p. 282)

The Operation-cards merely determine the succession of operations in a general manner. They in fact throw all that portion of the mechanism included in the *mill*, into a series of different *states*, which we may call the *adding state*, or the *multiplying state*, etc., respectively. In each of these states the mechanism is ready to act in the way peculiar to that state, on any pair of numbers which may be permitted to come into its sphere of action. Only *one* of these operating states of the mill can exist at a time; and the nature of the mechanism is also such that only *one pair of numbers* can be received and acted on at a time. (p. 281)

There are certain numbers, such as those expressing the ratio of the circumference to the diameter . . . which frequently present themselves in calculations. To avoid the necessity for computing them every time they have to be used, certain cards may be combined specially in order to give these numbers ready made into the mill, whence they afterwards go and place themselves on those columns of the store that are destined for them . . . [By such means] Mr. Babbage believes that he can, by his engine, form the product of two numbers, each containing twenty figures, in *three minutes*. (Menabrea 1843: 264)

The mode of application of the cards, as hitherto used in the art of weaving, was not . . . sufficiently powerful for all the simplifications which it was desirable to attain in such varied and complicated processes as those required [for] an Analytical Engine. A method was devised of what was technically designated *backing* the cards in certain groups according to certain laws. The object of this extension is to secure the possibility of bringing any particular card or set of cards into use *any number of times successively* in the solution of one problem. (p. 283)

It is desirable to arrange the order and combination of the processes with a view to obtain them as much as possible *symmetrically* and in cycles [or even in cycles of cycles: see p. 308], in order that the mechanical advantages of the *backing* system may be applied to the utmost. It is here interesting to observe the manner in which the value of an *analytical* resource is *met* and *enhanced* by an ingenious *mechanical* contrivance. We see in it an instance of one of those mutual *adjustments* between the purely mathematical and the mechanical departments, [which is] a main and essential condition of success in the invention of a calculating machine. (p. 298)

The engine is capable, under certain circumstances, of feeling about to discover which of two or more possible contingencies has occurred, and of then shaping its future course accordingly. (Lovelace, Menabrea 1843: 252 n.)

Figures, the symbols of *numerical magnitude*, are frequently *also* the symbols of *operations*, as when they are the indices of powers. Wherever terms have a shifting meaning, independent sets of considerations are liable to become complicated together, and reasonings and results are frequently falsified. Now in the Analytical Engine the operations which come under the first of the above heads, are ordered and combined by means of a notation and of a train of mechanism which belong exclusively to themselves; and with respect to the second head, whenever numbers meaning *operations* and not *quantities* (such as the indices of powers), are inscribed on any column or set of columns, those columns immediately act in a wholly separate and independent manner, becoming connected with the *operating mechanism* exclusively, and re-acting upon this. They never come into combination with numbers upon any other columns meaning *quantities*; though, of course, if there are numbers meaning *operations* upon *n* columns, these may *combine amongst each other* . . . It might have been arranged that all numbers meaning *operations* should have appeared on some separate portion of the engine from that which presents numerical *quantities*; but the present mode is in some cases more simple, and offers in reality quite as much distinctness when understood. (p. 270)

There are frequently several distinct *sets of effects* going on simultaneously; all in a manner independent of each other, and yet to a greater or less degree exercising a mutual influence. To . . . perceive and trace them out with perfect correctness and success, entails difficulties whose nature partakes to a certain extent of those involved in every question where *conditions* are very numerous and intercomplicated; such as for instance the estimation of the mutual relations amongst *statistical* phenomena, and of those involved in many other classes of facts. (p. 288)

Last, but unfortunately not least:

[It may be objected] that an analysing process must . . . have been performed in order to furnish the Analytical Engine with the necessary *operative* data, and that herein may also lie a possible source of error. Granted that the actual mechanism is unerring in its processes, the *cards* may give it wrong orders. This is unquestionably the case . . . (p. 274)

The vocabulary of computer science wouldn't be coined until over a century later (see Section v). But Babbage's cogwheel design for the Analytical Engine had exemplified a wide range of computational phenomena that are implemented electronically today.

It's clear from the quotations given above that these included programmed control; stored programs (in the sense previously remarked); data and operations; central processor; memory store; addressing; hierarchically nested subroutines; microprogramming; looping; conditionals; and comments.

They even included the programmer's nemesis, bugs. (These are so called by extension: the first bug wasn't a software bug at all, but a moth crushed on a relay switch. It was found in the Harvard Mark I by Grace Hopper, who stuck it into the log book, noting: "First actual case of bug being found". For a photo of this famous animal, see Kurzweil 1990: 179.) True, Babbage didn't foresee that bugs would later be regarded as a "powerful idea", used both in education and in programmed planning (Chapter 10.iii.c and v.g). But he was alive to their possibility.

In sum, Babbage not only designed the first stored-program, general-purpose digital computer. He also anticipated many specific computational functions found in modern machines.

3.iv. Had Wheelwork Been Taught to Think?

In the year of the Great Exhibition, the surgeon Alfred Smee (1818–77)—who'd already argued that "instinct" and "reason" could be "deduced" from electrobiology (Smee 1850)—suggested that a logic machine could be built to operate like the human mind (Smee 1851). He allowed that it would fill an area larger than London (see 2.ix.a), but insisted that it was possible *in principle*. And a close friend of Babbage wrote in the 1870s: "The marvellous pulp and fibre of the brain had been substituted by brass and iron, he had taught wheelwork to think" (quoted in Swade 1993: 64).

Was that Babbage's view? And did he believe that he'd provided evidence for the 'mind as machine' hypothesis? (These are different questions: substitution is neither simulation nor replication.)

Babbage himself said very little on these matters. But what he did say—and also what he didn't say—suggests that the answer to both questions is *No*. In other words, he wasn't a precocious supporter of strong AI, nor of weak AI either (see Chapter 16.v.c). In that sense, he was firmly located in his own century, not the next.

a. For Lovelace read Babbage throughout

Direct evidence from Babbage's own hand is scant. He wrote very little about the general implications of his work, and nothing that one can confidently interpret as addressing the question we're raising here.

In his autobiography, for example, he described "calculating machines" not as devices that can do arithmetic, but as "pieces of mechanism for assisting the human mind in executing the operations of arithmetic" (Babbage 1864: 30). Possibly, he intended there to make the distinction (mentioned in Chapter 2.ix, with respect to Blaise Pascal and Stanley Jevons) between thinking as such and the formal principles that thinking must satisfy if it is to be valid. On other occasions, his language was more ambiguous, as when he called his Difference Engine "a substitute for one of the lowest operations of human intellect" (Hyman 1989: 44).

On most occasions, however, his comments about mind in relation to machine weren't so much ambiguous as non-existent. He published very little about the Analytical Engine, even considered as a mathematical tool or engineering design. When he did, he didn't discuss the mind–machine relation. Nor did he do so in his unpublished notebooks. Moreover, even when—towards the end of his life—he wrote that *the most challenging scientific question* is how inventive thinking works, he mentioned his writings on induction and analogy but said not a word about either of his Engines (Babbage 1864: 321–2).

Occasionally, he did express a passing interest in what a cognitive scientist would call psychological mechanisms. So in his reminiscences he said: "It has often struck me that an analysis of the causes of wit would be a very interesting subject of enquiry. With that view I collected many jest-books . . ." (1864: 272). But "fortunately", he continued, he had the "resolution to abstain from distracting my attention from more important enquiries".

What his analysis of jests might have been like is indicated in the few ensuing remarks. For instance, he recalled one of his friends saying to another, "I am very stupid this morning: my brains are all gone to the dogs"—to which the instant reply was "Poor dogs!" The "wit" here, he said, arose from "sympathy expressed on the wrong side". And he pointed out that "jokes formed upon this principle [*sic*]" depend on the meaning of the words but not on their sound or arrangement, so are "rare" in being translatable into all languages. Puns—which he considered "detestable"—depend, he said, on double meanings of one and the same word, or on similar pronunciation of words that are differently spelt. As an example of "a triple pun", he gave this:

A gentleman calling one morning at the house of a lady whose sister was remarkably beautiful, found her at the writing-table. Putting his hand upon the little bell used for calling the attendant, he enquired of the lady of the house what relationship existed between his walking-stick, her sister, and the instrument under his finger.

$$\text{His walking-stick was} \begin{cases}\text{cane}\\ \\ \text{Cain}\end{cases}\!\!,\ \text{the brother of} \begin{cases}\text{a bell}\\ \text{a belle}\\ \text{Abel.}\end{cases}$$

(Babbage 1864: 273)

Then, instantly, he changed the subject (to talk about his Silver Lady).

It's clear from those examples (and from his use of the word "principle") that he'd have been sympathetic to cognitive analyses of jokes. But nowhere did he suggest that one of his Engines might implement such an analysis. He'd probably have been intrigued by a recent joke-generating program that uses some of the punning principles he identified here (Binsted and Ritchie 1997; Binsted *et al*. 1997; Ritchie 2001, 2003*a*; see Chapter 13.iv.c). But whether he'd think it a model of "the causes of [human] wit" is quite another matter.

This absence of psychological comment on the Engines is highly indicative. Babbage wasn't a blinkered mechanic, blind to the wider implications of his work. He was quick to draw theological morals from his Difference Engine, for instance, as we've seen. Nor did he fear controversy—indeed, he relished it. It's hard to believe that someone prepared to link wheelwork with miracles wouldn't have linked it also with minds, if he'd thought the comparison to be of interest. And if he had, he'd have shouted it from the rooftops.

Something more, however, can be said. If Babbage didn't describe the Analytical Engine in print, his collaborator Ada, Lady Lovelace (1815–52), did. And, much as the spoof history book *1066 and All That* famously advises "For pheasant read peasant throughout", so we can read Lovelace as Babbage—pretty well throughout.

Lovelace, wife of the Earl of Lovelace, was the daughter of the poet Lord Byron (D. L. Moore 1977; D. Stein 1985). It was Byron who was famously described on first meeting—by Lady Caroline Lamb (later, his lover), in her diary of 1812—as being "mad, bad, and dangerous to know". It's a delicious historical irony that this leader of the Romantic movement, with its contempt for order and for science (see Chapter 2.vi.c), fathered Babbage's closest co-worker and confidante—and chose the name that would eventually be given to a widely used programming language, Ada (Chapter 13.i.c).

But Byron's daughter didn't betray her Romantic father so far as to liken minds to machines. On the contrary, in her comments on the general nature and potential of the Analytical Engine she too, like Babbage, avoided drawing any psychological moral.

Lovelace's comments occur in one important document. She translated a paper on the Analytical Engine written by an Italian scientist, Luigi Menabrea—later, Prime Minister of Italy (Menabrea 1843). This paper was based on a lecture given by Babbage himself in Turin, the only occasion on which he spoke at length about his Analytical Engine to fellow scientists. And, significantly, she added lengthy 'Notes', and a few footnotes, giving her own corrections and comments (Lovelace 1843). We may read Menabrea's (translated) paper as being Lovelace at one remove, for her scrupulousness suggests that any claim that she translated without comment was one with which she didn't disagree.

Likewise, we may read Lovelace as being Babbage at one remove—and not just because it was Babbage's lecture that Menabrea was reporting. It's not known just how much Babbage contributed to the first draft of these 'Notes' (nor to the translation of Menabrea's paper), and how much is 'pure' Lovelace. Recent historical research has shown that "most of the technical content and all of the programs in the *Sketch* were Babbage's work" (Campbell-Kelly 1994: 27). But at one point in the 'Notes' (p. 271), Lovelace mentions an idea and says "we do not know" whether Babbage agrees with it: presumably, that passage was drafted by her. However, Babbage did oversee both papers before publication, even if he didn't supply the answer to her implied question. It's therefore most unlikely that he disagreed strongly with anything she said there.

Moreover, the two were very close, both intellectually and personally. They first met, at a party, when she was only 17. He addressed her as "my dear and much admired interpreter", and perhaps called her "the Enchantress of Numbers". I say "perhaps", partly because the context would allow this phrase to mean not Ada but mathematics, or even his beloved Engine (Swade 2000: 165), and partly because her mathematical abilities are actually in doubt. One sour biography ungenerously downplays them (D. Stein 1985: 89–120 *passim*). Another Babbage historian, Bruce Collier, endorses this judgement (1990: [5]), having already complained in exasperation:

> There is one subject ancillary to Babbage on which far too *much* has been written, and that is the contribution of Ada Lovelace. It would be only a slight exaggeration to say that Babbage wrote the 'Notes' to Menabrea's paper, but for reasons of his own encouraged the illusion in the minds of Ada and the public that they were authored by her. It is no exaggeration to say that she was a manic depressive with the most amazing delusions about her own talents, and a rather shallow understanding of both Charles Babbage and the Analytical Engine. . . . Ada was as mad as a hatter, and contributed little more to the 'Notes' than trouble. (Collier 1990: [4])

Declaring "an open mind on whether Ada was crazy because of her substance abuse . . . or despite it", Collier ended tetchily: "I am somewhat sorry I did not debunk her role more vigorously. . . . But, then, I guess *someone* has to be the most overrated figure in the history of computing" (1990: [5]).

Overrated? Well, perhaps. It seems pretty clear, however, that Ada understood the principles of the Analytical Engine. If she didn't grasp every mathematical jot and engineering tittle, she nevertheless appreciated its general nature and implications. It's even possible that she had a better idea of some of these implications than did Babbage himself: non-specialists may be better able to see the wood, even if they can't see all of the trees. His epithet "admired interpreter", unless it was utterly empty flattery, suggests that she did add something to his own view of his work.

However that may be, and even if some of Lovelace's comments wouldn't have come spontaneously from Babbage, it's hardly credible that he disagreed with her on any important point. So we can read Lovelace as Babbage, albeit at one remove. (Even Collier would agree with that: see his remark about the "illusion", quoted above.)

b. What Lovelace said

Lovelace took pains to stress the generality of the Analytical Engine:

The distinctive characteristic of the Analytical Engine, and that which has rendered it possible to endow mechanism with such extensive faculties as bid fair to make this engine the executive right-hand of abstract algebra, is the introduction into it of the principle which Jacquard devised for regulating, by means of punched cards, the most complicated patterns in the fabrication of brocaded stuffs. It is in this that the distinction between the two engines lies. Nothing of the sort exists in the Difference Engine. We may say most aptly that the Analytical Engine *weaves algebraical patterns* just as the Jacquard-loom weaves flowers and leaves. (Lovelace 1843: 272–3)

[Although the Engine was built so as to deal with numerical data and results] it must be easy by means of a few simple provisions and additions in arranging the mechanism [to enable it also to bring out] *symbolical results* [being] the necessary and logical consequences of operations performed upon *symbolical data*. (Lovelace 1843: 271)

By the word *operation*, we mean *any process which alters the mutual relation of two or more things*, be this relation of what kind it may. This is the most general definition, and would include all subjects in the universe. (Lovelace 1843: 269)

She pointed out that there could be a similarly general machine for doing logic. She also remarked that the Analytical Engine could in principle compute all the laws of science, for "Mathematics . . . constitutes the language through which alone we can adequately express the great facts of the natural world . . . ." And she (and Menabrea) foresaw a time when scientists would need to call on mechanical calculation, to deal with otherwise unmanageable amounts of data. (How right she was! Today's scientists regularly depend on computers to do their sums. A few have even used computers to help make scientific discoveries: see 10.iv.c and 13.iv.c.) So, whereas the Difference Engine was to be used for more mundane tasks (helping navigators and arithmeticians), the construction of the Analytical Engine would mark "a glorious epoch in the history of the sciences" (Menabrea 1843: 266). This prophecy was the culmination of Menabrea's paper, and perhaps—who knows?—the triumphant ending of Babbage's lecture.

Even musical composition, she said, might turn out to be explicable by science, and amenable to (programmable for) this machine:

The operating mechanism . . . might act upon other things besides *number*, were objects found whose mutual fundamental relations could be expressed by those of the abstract science of operations, and which should be also susceptible of adaptations to the action of the operating notation and mechanism of the engine. Supposing, for instance, that the fundamental relations of pitched sounds in the science of harmony and of musical composition were susceptible of such expressions and adaptations, the engine might compose elaborate and scientific pieces of music of any degree of complexity or extent. (Lovelace 1843: 270)

However, for Lovelace (and presumably for Babbage), the Engine's generality *didn't* make it capable of thought.

Some evidence for this comes from her comments, or lack of them, on Menabrea's paper. She didn't demur from Menabrea's opening remark, that "mathematical labours", which seem at first sight to be "the exclusive province of intellect", actually comprise two types: "the mechanical [involving] precise and invariable laws, *that are capable of being expressed by the operations of matter*", and "reasoning, [which belongs] to the domain of the understanding" (Menabrea 1843: 246; italics added). Nor did she contradict his judgement that "although [the Analytical Engine] is not itself the being that reflects, it may yet be considered as the being which executes the conceptions of intelligence" (Menabrea 1843: 265). Indeed, she wrote a long note on this very sentence, in which she warned against over-enthusiastic descriptions of the machine:

It is desirable to guard against the possibility of exaggerated ideas that might arise as to the powers of the Analytical Engine. In considering any new subject, there is frequently a tendency, first, to *overrate* what we find to be already interesting or remarkable; and secondly, by a sort of natural reaction, to *undervalue* the true state of the case, when we do discover that our notions have surpassed those that were really tenable. (Lovelace 1843: 300)

One is reminded of the 'hype' sometimes attached to various forms of AI, and the see-sawing public responses to it (see Chapters 9.x.f, 13.iv–vi, 12.iii and vii, and 15.ix). This passage, especially in the context of Menabrea's remark, implies that Lovelace didn't think of the Engine as a "being that reflects".

Further evidence that she saw it merely as a machine, not as anything like a mind (neither simulation nor replication), came from her own pen:

In enabling mechanism to combine together *general* symbols, in successions of unlimited variety and extent, a uniting link is established between the operations of matter and the abstract mental processes of the *most abstract* branch of mathematical science . . . [The] mental and the material . . . are brought into more intimate and effective connexion with each other. We are not aware of its being on record that anything partaking in the nature of what is so well designated the *Analytical* [algebraic] Engine has been hitherto proposed, or even thought of, as a practical possibility *any more than the idea of a thinking or reasoning machine*. (Lovelace 1843: 273; final italics added)

That last sentence seems to mean that, whereas the idea of an algebraic machine is wrongly supposed to be nonsense, the idea of a thinking machine obviously is nonsense. Nor was the notion of a creative machine acceptable to her:

The Analytical Engine has no pretentions whatever to *originate* anything. It can do whatever we *know how to order it* to perform. It can *follow* analysis; but it has no power of *anticipating* any analytical relations or truths. Its province is to assist us in making *available* what we are already acquainted with. (Lovelace 1843: 300)

She would have regarded the music machine, had it ever been built, as an existence proof that "scientific" music can be composed by machine, not as a simulation of how human musicians actually compose ("originate") it.

Possibly—indeed, probably—part of her scepticism here related to the assumption that no mere machine could be conscious. However, she didn't actually say so. Nor did Babbage, when referring to the difficulty in explaining inventive thought (1864: 321–2). Rather, they seemed (to me) to assume that *processes* of some kind are involved in human thinking—but of an utterly mysterious type.

Of course, Lovelace (and Babbage) used psychological language to describe the operations of the Analytical Engine. She even added a footnote describing it as "feeling about" to discover which of several possible events have occurred, in rebuttal of Menabrea's claim that it "must exclude all methods of trial and guess-work, and can only admit the direct processes of calculation" (Menabrea 1843: 252). But she didn't quibble when Menabrea declared, in the very next sentence: "[The] machine is not a thinking being, but simply an automaton which acts according to the laws imposed upon it." Presumably, then, she agreed.

c. Babbage and AI

In his *Ninth Bridgwater Treatise*, Babbage declared: "I was well aware that the mechanical generalizations I had organized contained within them much more than I had leisure to study, and some things which will probably remain unproductive to a far distant day."

Bearing in mind his searing disappointment in failing to complete any full-scale Engine, this remark—with hindsight—may seem poignantly prophetic. In fact, it was more poignant than prophetic.

Babbage was neither a successor of Vaucanson nor a precursor of Allen Newell and Herbert Simon (see Chapters 6.iii, 7.iv.b, and 10.i.b). Unlike them, he had no biological or psychological aims in designing his automata. His machines embodied mathematical principles, not psychological theories.

Given his Boolean assimilation of logic to mathematics (and science), one might say that, *in spirit*, he anticipated Turing's claim that anything computable can be computed by some machine (see 4.i.c). But if his Analytical Engine was the first truly general automatic problem-solver, he didn't see it as such—still less, as a model of human problem solving. He designed a powerful, technologically motivated, calculator. But he got no further along the AI road than to propose a noughts-and-crosses (tic-tac-toe) machine—never built—to raise money for his research (Swade 1991: 32). Even that, had he built it, would have been a technologist's toy, not a psychologist's model. In the terminology of Chapter 1.ii.b, his life's ambition was to do technological, not psychological, AI.

In short, Babbage didn't believe he'd taught wheelwork to think. Nor, despite a still-widespread assumption to the contrary, did he imagine that wheelwork might have anything to teach us about our own thinking. In that sense, he was irrelevant to cognitive science.

Some people would argue, however, that he merits mention in the history of the field in virtue of his role in the invention of computers—without which, cognitive science wouldn't exist. After all, it was he—or so this claim goes—who started us on the road to general-purpose computing. Before we can weigh that argument (in Section vi), we first need to consider briefly how it was that today's machines came on the scene.

3.v. Electronic Babbage

The technological advances in computing that occurred in the mid-twentieth century were crucial for cognitive science. They not only made it potentially possible in practice, but also influenced its theory.

I say "potentially" because the very earliest computers weren't usable by anyone but a few dedicated boffins. Indeed, with one exception they weren't even seen by more than a few hand-picked boffins (plus the servicewomen who 'fed' them, night and day). They were literally a state secret, and remained so for many years. What's more, they were being used to solve problems much more urgent than modelling minds.

Their immediate successors, in the late 1940s, were almost as difficult to use. By the mid-1950s, the machines were somewhat—only somewhat—more manageable. But for several more years, they weren't actually available except within a tiny handful of computing research labs—whose "service clients" were mostly physicists and mathematicians (plus occasional pioneers in machine translation), not psychologists or biologists.

That's why the cognitivist ideas that were exciting a number of professional psychologists in the 1940s and early 1950s still couldn't be implemented as computer models (see Chapters 4.ii, 5.ii–iv, 6.ii.a, and 12.i). Even if—which wasn't always so—the theorists concerned were thinking about psychology in broadly computational terms (perhaps because they'd read a paper in an obscure journal of 1943: see 4.iii.e–f), they had no opportunity to translate their thoughts into computer programs. That could be attempted only by hands-on computer aficionados, who were still very thin on the ground—and most of them shared Babbage's unconcern for matters mental.

Most . . . but not quite all. As it happens, modern computing was largely initiated by two men who, unlike Babbage, *were* interested in core questions about life and mind. Both are now counted among the founding heroes of cognitive science: namely, Turing (1912–54) and John von Neumann (1903–57). Their theoretical ideas will be considered in later chapters (4.i, 10.i.f, 12.i.b–d, 15.iv–v, and 16.ii). Here, the focus is on their practical work on the design of computing machinery. For our purposes, the interest isn't in the engineering details but in the functions, or tasks, which the electronic nuts and bolts enabled mid-century computers to perform.

a. A soulmate in Berlin

The "one exception" to the claim that the earliest computers were known only to a few boffins was a machine ensconced in a family flat in pre-war Germany. In 1935, just over 100 years after Babbage started work on the Analytical Engine, the German inventor Konrad Zuse (1910–95) gave up his job and set up a computer workshop in his parents' flat in Berlin.

Their living-room was near-filled by his machines (the first of which was the size of an eight-place dining table), and the floor littered with the debris from his fretsaw.

Lacking Babbage's independent means, he was financially supported by his father (who went back to work, having been retired for a year), by his sister (who donated much of her wages), and by a dozen friends who provided financial and/or technical help.

For the next six years, until he was called up into the German army, he devoted all his time to designing and building a series of increasingly powerful calculating machines. These had begun even earlier: in 1933–4 (well before Turing's 1936 paper), he'd snatched spare moments from his salaried work to design the first version. They culminated in the first fully automated, program-controlled, computer. Now known as the Z3, this was sketched in 1938 and operational by 1941.

Zuse himself later described the Z3 as "a Babbage machine" (Zuse 1993: 50). That's not to say that it was based on Babbage's work, for it wasn't (see Section vi.a). Nor was it exactly similar in composition or in function. Z3 was built of electrical switches and mechanical parts, not columns and cogwheels, and it used a binary instead of a decimal base. Moreover, it did floating-point arithmetic, and could cope with mathematical matrices as well as series of 'individual' sums. Nevertheless, it was similar in principle to the Analytical Engine. And it was driven by very similar motivations: Babbage and Zuse were soulmates.

Both, for instance, were 'natural' engineers. Zuse had been an inventor of mechanical and electro-mechanical gadgets ever since his schooldays. He repaired no silver ladies. (Not because he lacked aesthetic appreciation: he was an inventive photographer and film-maker, and *art or engineering* was a difficult career choice for him—1993: 9.) But as a schoolboy, he'd managed to mend the broken second gear on his bicycle when the bike-dealer couldn't. And as an engineering student he invented an automatic dispensing machine, which not only accepted different coins and different prices, but provided change. He even built a snow machine for use in theatres, and designed the first split-level (clover leaf) intersection for street crossings. In short, "All types of smaller and larger inventions kept me busy" (p. 16).

Much as Babbage eventually abandoned the Silver Lady, so Zuse eventually allowed "all types" of invention to be overtaken by just one. This happened in his mid-twenties: "If I remember correctly, my thoughts [about the computer] first took concrete shape in 1933" (p. 28), and "In 1935 I decided to become a computer developer" (p. 34). In 1947 he would found Germany's first computer company, with the aim of developing CAD/CAM applications as well as 'mere' calculation. (The company succeeded partly because there was no competition from IBM. He'd suggested a joint effort, but the negotiations came to nothing: IBM wouldn't guarantee that he could continue to work on his computer: p. 114.)

Like Babbage, too, Zuse saw his machine as a *general* computer. Indeed, he'd seen this right from the start. (Babbage hadn't: the Difference Engine wasn't a universal machine, but a dedicated one.) For gadgets weren't Zuse's only passion. He was enthused also by mathematical logic—and the logic was as important as the mathematics. (Surprisingly, he was unaware of the propositional calculus: p. 44.) Indeed, the Z3 was based not on adding wheels, but on logic gates: implementations of *conjunction*, *disjunction*, and *negation*, used to define conditional statements. By 1946 Zuse had even developed a "logical" programming language, the Plankalkul (Chapter 10.v.f). In short, he was well aware that his machines had the potential for computing symbolic expressions such as chess moves and propositions, as well as numbers.

This was revolutionary, despite Babbage's (and/or Ada's) prescient remarks on the same theme and despite the notoriety of *Principia Mathematica* (Russell and Whitehead 1910). And it was resisted accordingly. Even his close collaborators doubted the possibility of non-numerical computing (p. 57).

That was par for the course in the 1940s. When logic gates were independently defined by Warren McCulloch and Walter Pitts two years later (1943), they caused a sensation in a few well-prepared minds but weren't understood by most readers (see 4.iii.e–f and iv). Zuse's lecture on symbolic computing to the Society for Mathematics and Mechanics in 1948 was ignored. And a full twenty years after that, MIT's AI group still felt the need to make a song and dance about the possibility of "semantic" information processing (Minsky 1968; see 10.iii.a).

An added similarity between Zuse and Babbage was their lack of interest in cognitive science. It's true that Zuse's diary for 20 June 1937 declared: "For about a year now I have been considering the concept of a mechanical brain ..." (p. 44), and that "[It] was clear to me that one day there would be computing machines capable of winning international chess matches. I estimated that it would take about fifty years before this would happen" (pp. 49–50). It's also true that by 1944 he'd sketched a version of associative memory, and that he'd considered parallel processing almost from the start. But these projects were more technological than psychological. Zuse's autobiography says nothing about modelling human thought processes as such.

Like his English soulmate, he wasn't afraid of being outrageous. He originated the concept of "the computing universe" (to be described by a digital physics), discussed self-reproducing systems, and analysed attractor cycles and other abstract properties of cellular automata (Zuse 1969/1970)—all topics that are still highly controversial. At the time, he reported that he'd found it "rather difficult to find a publisher" for his work, "which stands somewhat outside the presently accepted method of approach" (1969/1970: 2). That was an understatement: he later admitted that his hypothesis of the computing universe appeared to be "crazy", while hanging on to it nonetheless (1993: 174). So he surely wouldn't have baulked at a 'digital' psychology. Since he didn't claim to have illuminated the mind, it's pretty certain that he wasn't interested in doing so.

The greatest difference between the two men was that Zuse did—eventually—receive recognition in his lifetime. In 1957 he was awarded an honorary doctorate by the Technical University of Berlin. Universities and professional societies in other countries later followed suit (US recognition first occurred in Las Vegas in 1965). His own comment puts the point clearly:

> [As regards professional recognition and honours] I have experienced much of both and was just as happy to receive both. Sometimes I cannot help but think of Babbage, who was denied this as well as the inventor's greatest reward: the successful realization of one's idea. (1993: 166–7)

Why did Zuse have to wait so long for professional recognition? The main reasons why his ideas on symbolic computing weren't taken up earlier were the intellectual isolation and physical devastation caused by the Second World War. Zuse himself was forced to divert his energies onto weapons development (Chapter 11.i.a), and his prototype machines and most of his early papers were destroyed in Allied bombing raids. By the end of the war, he still knew nothing of Claude Shannon or Turing. And from 1947 on, he had to concentrate on running his business, not on advancing or publicizing his theory.

By the time his work became known in Anglo-American circles, the computer industry had already been established there. But whether he'd have received a sympathetic hearing in Allied countries immediately after the war is doubtful in any case.

b. Call me MADM

In 1944 a novel calculating machine was built that was soon hailed in *Nature* as 'Babbage's Dream Come True' (Comrie 1946). That machine was Howard Aiken's (1900–73) Harvard Mark I, an electromechanical (fixed-point) calculator constructed (by IBM, and part-funded by the US Navy) by combining several Hollerith-type statistical machines.

But the salutation was premature. Aiken's machine (originally proposed in 1937) wasn't a general-purpose computer, but a dedicated calculator. Moreover, it lacked the full conditional branching allowed for in the design of the Analytical Engine (I. B. Cohen and Welch 1999; Pratt 1987: 148–50). Aiken himself later said, "If Babbage had lived seventy-five years later, I would have been out of a job" (quoted in Swade 1991: 34). Even so, Babbage's dream was fully realized only by the electronic, stored-program, general-purpose, digital computer.

As we've seen, Zuse's electromechanical Z3 was the first stored-program general computer. But it remained unknown, owing to the chaos of post-war Germany. It had no influence on the Anglo-American developments that led to today's machines.

In that historical context, the 'first' computer was the Manchester Mark I, or MADM (Manchester Automatic Digital Machine). The small-scale prototype (affectionately named "Baby" by the team who built it) was finished in June 1948, and a commercial version was marketed by Ferranti in 1951 (Kilburn and Williams 1953). A simple demonstration computer, called Nimrod, was exhibited by Ferranti at the Festival of Britain in the same year.

MADM's intellectual ancestry was highly distinguished. The design team, led by the electronic engineers Frederick Williams and Thomas Kilburn, was largely inspired by Maxwell Newman (1897–1984)—the recently appointed Professor of Mathematics at Manchester:

> Neither Tom Kilburn nor I knew the first thing about computers when we arrived in Manchester University . . . Newman explained the whole business of how a computer works to us. (Williams, quoted in Copeland and Proudfoot 1998: 6)

Newman had taught Turing in Cambridge in the mid-1930s—indeed, it was his lectures which led Turing to write the seminal paper on the Turing machine (see Chapter 4.i.c). He was familiar with Turing's early construction (in 1937–8) of a simple binary multiplier. And he'd designed and used pioneering computers at Bletchley Park, where he'd headed the cryptanalysis unit (see below).

Turing himself joined Newman at Manchester in 1948, just after the full Mark I had become operational. But he had no direct contact with the MADM engineers until after their first program had been run (Copeland 1999). His one contribution to the hardware of the Manchester machine was in 1949, when he helped to design a random number generator (Hodges 1983: 402).

Williams later described MADM as "pure Babbage", the "sole difference" being that it used subtraction rather than addition as its basic operation (quoted in Randell 1972: 9). Its code allowed for eight functions, including STOP.

Its first program, entered by way of a 5×8 array of push-button switches, was a 'highest factor' mathematical routine, only seventeen instructions long. (A facsimile is shown in Lavington 1975: 11.) The program tested about 130,000 numbers (generated by repeated subtraction), by means of 3.5 million operations, in a run of fifty-two minutes. A quarter-century on, Williams recalled the achievement:

> A program was laboriously inserted and the start switch pressed. Immediately the spots on the display tube entered a mad dance. In early trials it was a dance of death leading to no useful result, and what was even worse, without yielding any clue as to what was wrong. But one day it stopped, and there, shining brightly in the expected place, was the expected answer. It was a moment to remember. This was in June 1948, and nothing was ever the same again. (quoted in Stracey 1997: 17)

The programs for the Manchester machine were written by a number of people, including the mother and father of Tim Berners-Lee, inventor of the World Wide Web (Berners-Lee 2000: 3), and Turing himself. Turing was employed for a while as MADM's software writer—officially, as "deputy director of the Computing Machine Laboratory".

One of his efforts was a programmer's joke: it used random numbers to choose words making up "love-letters" (Chapter 9.x.c). Another, which could have been used for a serious purpose, did long division (it's reproduced in Stracey 1997). A third was a much-improved (faster) version of a prime-number program written by Newman (Lavington 1975: 12). And a long section of Turing's manual discussed how to write code to enable MADM to play tunes on its "hooter". This advice was heeded by Christopher Strachey, one of whose programs—the first ever, of any significant length—ended its activity by playing *God Save the King* (B. J. Copeland, personal communication).

Only a few of these very early programs remain. Indeed, not many were written in the first place. The reason was that MADM's programs, expressed in an unfamiliar notation full of %s and $s and £s and ////s, could be written or understood only by someone fully conversant with the basic structure of the machine itself. Turing's *Programmers' Handbook* was the only aid available, and it sometimes presupposed advanced mathematical knowledge (Hodges 1983: 398–401).

Consequently, this epochal machine could be dealt with directly only by accomplished mathematicians entirely familiar with its construction. That's not to say that others couldn't benefit from it: the Manchester Computing Laboratory offered a consultancy service to both academics and industry from about 1950. But the "others", themselves, couldn't use it.

The first 'usable' stored-program electronic computer was the EDSAC, built by Maurice Wilkes (1913–) and colleagues at the University of Cambridge (M. V. Wilkes 1953). This did employ mercury delay lines (the D stood for 'Delay'), which Turing had wanted for the ACE, the Automatic Computing Engine built in London after the war—see below (Campbell-Kelly 2002). It ran its first program on 6 May

1949, when it calculated a table of squares and printed the result. It provided a regular computing service for members of the University (including CLRU: Preface, ii) from 1950 until 1958, when EDSAC-2 was ready.

One reason why EDSAC was easier to use was that it accepted an alphabetic shorthand instead of binary numbers, converting the one into the other automatically. (Even so, it was still fiendishly difficult: genuine ease of use had to await the development of high-level programming languages: Chapter 10.v.) Moreover, Wilkes's colleagues provided its users with a growing library of commonly used subroutines—for doing division, for example. Similarly, you will remember, Babbage had used a special type of card to express "numbers which frequently present themselves in calculations".

c. Intimations of AI

But where were the "elaborate and scientific pieces of music"? They were nowhere in sight, and almost nowhere in mind. For if these British machines were, in principle, the realization of Babbage's (or Ada's) ambitious dream, that's not to say that most of their users saw them in that way.

They—and their transatlantic cousins the ENIAC and EDVAC—were usually thought of merely as powerful numerical calculating machines, not as general symbol manipulators. (The letter C stands straightforwardly for 'Calculator' in EDSAC, and for the ambiguous term 'Computer' in ENIAC and EDVAC.) Indeed, the ENIAC wasn't a general-purpose machine but was used mainly for calculating bombing tables. (The wartime security surrounding the ENIAC was less fierce than at Bletchley. Even so, when it was in the initial stages of construction for the US Army in 1943, those involved deliberately encouraged the rumour that it was a "white elephant"—J. Norman 2004: 16.)

One person involved with computer design in the mid-1940s has admitted that "in general, the idea of universality of a general purpose digital computer took some grasping" (quoted in Randell 1972: 15). It was obvious that Babbage's algebraic ambitions had been realized, if "algebra" was interpreted in its usual (mathematical) sense. But very few saw these early computers as applying to "other things besides number".

Turing was among the few who did see their more general potential. For instance, we've already seen that he used MADM for non-numerical tasks such as generating "love-letters". He also programmed it to play "music" on its hooter—though not to compose melodies, which is what Lovelace had had in mind. Indeed, he'd already employed the electromechanical Bombes to break the German military codes. And he knew that the Colossus (see below) had been used, from December 1943, for cryptography based on Boolean logic (Hodges 1983, ch. 5).

In 1945 he wrote a report outlining a logical design for a stored-program computer to be built at the National Physical Laboratory (NPL), where he worked for a few years immediately after the war (A. M. Turing 1946; Carpenter and Doran 1977). He called it the Automatic Computing Engine (ACE)—in homage to Babbage. The ACE report was "perhaps the first written discussion of software since A. A. Lovelace" (Carpenter and Doran 1986: 13). Among its many original programming suggestions was a stack allowing nested subroutines (10.v.b), pushed and popped by "BURY–UNBURY" instructions (A. M. Turing 1946: 36, 75–9).

It also included some engineering suggestions, such as the use of mercury for the delay lines. (Sometimes, Turing mischievously recommended gin—M. V. Wilkes 1967: 199.) Turing even attempted to cost the labour that would be required to build it. However, his costings have been described as "amateur" and "unrealistic", and blamed on "the traditional and absurd British academic contempt for engineering" (Carpenter and Doran 1986: 11)—see Chapter 2.i.a. In the event, construction was long delayed. The pilot model worked by 1950, the full version—which differed significantly from the original proposal—by 1957 (Copeland 2005).

ACE was intended from the start as a general-purpose machine (Hodges 1983: 318–24). Indeed, Turing described it as a "practical version" of the universal machine he'd defined in the 1930s (see Chapter 4.i.c) (A. M. Turing 1947*a*: 107). He might have proposed it even earlier, had he not had more pressing matters on his mind: an early history of British computers points out that work on machines based on Turing's 1936 paper was "delayed by the war" (Bowden 1953: 135).

Before its construction was completed, Turing and his NPL colleagues had written a number of "sophisticated" mathematical programs for it (Copeland 2004: 367). And Turing suggested in his report that ACE might be used, for instance, for playing chess, or for information retrieval. He even said:

> the machine should be treated [when programming it] as entirely without intelligence. There are indications however that it is possible to make the machine display intelligence at the risk of its making occasional serious mistakes. By following up this aspect the machine could probably be made to play very good chess. (A. M. Turing 1946: 41)

This suggestion wasn't taken seriously by everyone in the emerging computing community. According to Turing's biographer, the idea of a universal machine—more strictly, of a general-purpose machine with finite storage capacity—"was stoutly resisted well into the 1950s" (Hodges 1997: 28). Probably, Turing's remarks about machine intelligence didn't help, for they—deliberately—raised philosophical hackles. Though only a few people saw the ACE report itself, such provocative remarks soon multiplied in Turing's writing (see Chapters 4.ii.b and 16.ii.a).

Nevertheless, some people—including a group in a Cambridge apple orchard (Preface, ii)—were intrigued, even persuaded. Turing offered some specific guidance, for instance in an early discussion of chess programming (not an actual program) (A. M. Bates *et al.* 1953: 288–95). Within a few years, papers on chess and other board games were being published by the first generation of AI programmers, some of whose letters and publications acknowledged their debt to Turing (see Chapter 10.i.a and b, and Copeland 2004).

d. Turing's invisibility

Turing's practical work with computers, as opposed to his theoretical founding of the field, is less widely known outside his own country than it deserves. Even in the UK, his contribution wasn't known until relatively recently.

It didn't begin with ACE, but dated back to the 1930s. He'd built a gear-wheel calculator in the late 1930s, and an electronic speech scrambler in 1944. He'd designed and used Bletchley Park's electromechanical Bombes, which broke the Nazis' Morse-based

Enigma codes in August 1940. (For an overview of the Enigma project, see Copeland 2004: 217–64; for Turing's description of the machines employed see Turing 1940/2004; an account of the code-breaking written by a Bletchley colleague, and kept top-secret until 1996, is Mahon 1945/2004.) And although he wasn't directly involved with the Colossus machine (which broke the non-Morse Fish codes), some of his methods for automatic cryptanalysis were used in it (Hodges 1983: 230, 266).

Colossus was the world's first large-scale programmable, special-purpose, electronic digital computer. It was outlined by Turing's ex-teacher Newman (who'd joined the Bletchley code-breakers in 1943), and designed and built by the Post Office engineer Thomas Flowers, who'd been recommended to Newman by Turing. Its name was bestowed by the Wrens (WRNS: Women's Royal Naval Service) who operated it: it was the size of a small room, and weighed about a ton.

After building several simple "Heath Robinson" machines (named after the English designer of joke machines, like the USA's "Rube Goldberg machines"), Flowers came up with the Colossus in 1943. It took Flowers and his team at the Post Office Research Station ten months to construct this, "working day and night, pushing themselves until (as Flowers said) their 'eyes dropped out' " (Copeland 2001: 344). Ten examples were eventually built—whose 1,500 valves were supplemented by mechanical pulleys.

Flowers had the revolutionary idea of storing *data* (the Fish key patterns) internally, using electronic valves to do so (Hodges 1983: 267). This was *so* revolutionary, indeed, that Newman and others at Bletchley simply didn't believe that a machine with about 2,000 valves (Colossus had 1,600) could be reliable (Copeland 2001: 360–1). They ignored Flowers's suggestions and concentrated on the Heath Robinson machines instead. Left to himself at the Post Office, Flowers developed the Colossus anyway—at his own expense (his bank account was in the red by the end of the war).

But Flowers hadn't needed the demands of war to originate the idea. Long before the war, he'd explored the possibility of using valves for controlling telephone connections. By 1934 the Post Office had approved his design for an automatic controller (built of 3,000–4,000 valves) for 1,000 telephone lines, and they'd even started using it by 1939. Moreover, in 1938–9 he was already experimenting with a high-speed electronic data store for use in telephone exchanges (Copeland 2001: 352). So the technology he'd intended as an aid to people's everyday chats ended up being used for desperately urgent code breaking.

However, the Colossus *program* wasn't stored internally. To fit Colossus for a new task, the operators had to rewire it using plugs and switches. Internally stored programs would be independently suggested by Turing and von Neumann in 1945.

By the time of the D-Day landings in June 1944, Colossus had been devoted to code breaking for six months. Until as late as 1975, however, its very existence was unknown in the computer community, except by the half-dozen British computer pioneers who'd worked at Bletchley Park. (For writings on this period by Turing and others, see Copeland 2004: 217–352.) Every detail of their wartime activities was covered for thirty years by the government's Official Secrets Act.

Eight of the ten Colossus machines were destroyed at the end of the war, on Winston Churchill's orders. It was revealed over fifty years later that the other two had been secretly installed at the intelligence centre GCHQ, where one was still operating in the early 1960s (Enever 1994: 38; cf. Sengupta 2000).

Thirty years after the war ended, Turing's contribution to the war effort could be acknowledged at last. It's now known that Churchill himself regarded it as so important that he ordered that all Bletchley's requests for equipment and personnel should be met immediately. (That was in response to a letter from Turing and his colleagues, complaining not only that their work was being held up by a lack of typists but that their requests for help had been repeatedly ignored by Whitehall: A. M. Turing *et al.* 1941.) And Turing's chief statistical assistant at Bletchley, I. Jack Good—later, a leader in AI chess—has remarked: "I won't say that what Turing did made us win the war, but I daresay we might have lost it without him" (McCorduck 1979: 53).

Even so, some of the relevant technical information was still secret at the end of 1999. Academic publications on Colossus were being delayed as a result (B. J. Copeland, personal communication). The full details of the Bletchley code-breaking exercise weren't finally released until the new century, when GCHQ's 500-page report on *The History of Newmanry* was made available. That was largely due to Donald Michie, a youngster at Bletchley (and co-author of the report) and later an AI pioneer in the UK (6.iv.e and 11.iv.a). He'd badgered the authorities for years to declassify it. However, a replica of Colossus was constructed in the late 1990s, and is on show at Bletchley—now, a museum.

Quite apart from the thirty-year secrecy blacking out Bletchley and the Colossus, Turing's visionary ACE report written in 1945 remained largely unread for forty years, especially outside the UK. There were only fifty or 100 mimeograph copies of the original report (issued in 1946), and even the designers of the EDSAC never saw it (Carpenter and Doran 1986: 16). A limited edition was printed for Babbage's centennial in 1972, in connection with an NPL Open Day. But it was widely published only in 1986, almost half a century after Turing wrote it.

Moreover, knowing full well (from his Bletchley experience) that working computers could indeed be built, Turing soon lost interest in the practical details of how to improve them. He turned instead to the theoretical problem of self-organization in biology (see Chapter 15.iv).

In short, much of the early British work on computing was either deliberately suppressed or circulated only narrowly. Small wonder, then, that the contribution of Turing and his British colleagues to the invention of the modern computer—as opposed to the notion of formal computation—isn't always recognized, especially outside the UK. As we saw in Chapter 1.iii.f, a US President recently claimed the computer for America—and he's not alone in this illusion.

e. Von Neumann's contribution

By contrast, von Neumann's 'Draft Report on the EDVAC' (von Neumann 1945) was circulated fairly widely, though not officially published, as soon as it was written in June 1945. (Turing was one of the first people outside the USA to see a copy—Hodges 1983: 307.)

Although MADM would be the first functioning example of a stored-program general-purpose electronic computer (EDVAC's debut was in 1951), von Neumann's document was the first 'published' account of such a machine. It was part-inspired by the ideas of McCulloch and Pitts, who in 1943 had described neural nets in logical-computational terms (see Chapter 4.iv.e). And it was largely inspired, too, by Turing's

1936 paper (4.i). Indeed, von Neumann "repeatedly emphasized that the fundamental conception was Turing's" (Copeland and Proudfoot 2005: 114; see also Copeland 2005: 21–7). Even so, "Many books on the history of computing in the U.S. make no mention of Turing," probably because—despite internal evidence that its ideas had been used—there was no *explicit* reference to 'On Computable Numbers' (A. M. Turing 1936) in the EDVAC Report (p. 10).

Von Neumann's paper immediately aroused a good deal of interest. For example, Turing cited it in his discussion of the ACE, written a few months later (A. M. Turing 1946: 21). His own stored-program design, however, *wasn't* inspired by von Neumann—and was very different (Carpenter and Doran 1986: 6). By contrast, Cambridge's EDSAC—although completed earlier than von Neumann's machine—*was* largely inspired by the plans for the EDVAC. Wilkes had discussed these on a visit to Pennsylvania's Moore School of Electrical Engineering in 1946, and he acknowledged his intellectual debt by the similarity in names.

Von Neumann became even more influential after developing the IAS machine at the Institute of Advanced Study, Princeton (Aspray 1990: 52–72, 92–4). This was officially inaugurated in 1952, but was running large programs for Los Alamos in mid-1951. (It would be heavily used for the H-bomb project, in which he was a participant: MacKenzie 1991*a*.) Von Neumann's IAS machine is commonly taken as the prototype of most modern computers. It was indeed the most influential of the early examples—but whether it resembled modern computers more significantly than MADM or ACE did is debatable (B. J. Copeland, personal communication).

How much von Neumann owed to Turing for his ideas on computer design is debatable also. Certainly, he was deeply impressed by Turing's theoretical work of the mid-1930s, and was doing pencil-and-paper experiments with it by 1938 (Aspray 1990: 178). Moreover, he acknowledged that the fundamental conception of modern computing was Turing's (Randell 1972: 10). But it's doubtful whether he was influenced also by Turing's practical experience and designs (Pratt 1987: 169; Randell 1972).

Von Neumann probably first met Turing in 1935, and offered him a job in 1938—which was declined. Some people believe, though this is disputed (B. J. Copeland, personal communication), that he and Turing met again in Princeton during the war (Aspray 1990: 100, 177–8). They had long, and not fully recorded, discussions on practical matters in 1947 (Randell 1972)—but both NPL and IAS were well advanced in significantly different designs by that time.

As for indirect influences, von Neumann became committed to computing on a visit to England in 1943, and wrote his first program (for a tabular calculating machine) at the Nautical Almanac Office there (Aspray 1990: 27, 231). He immediately wrote to friends that he'd "developed an obscene interest in computational techniques", and later told his English host that he'd "received in that period a decisive impulse which determined my interest in computing machines" (quoted in Aspray 1990: 27, 28).

(This "interest" was soon developed, in the spring of 1944, at Los Alamos. Interestingly, his frustrating experiences with their plugboard parallel machine "led him to reject parallel computations in electronic computers and in his design of the single-address instruction code where parallel handling of operands was guaranteed not to occur"—MacKenzie 1991*a*: 113.)

However, there's no evidence that von Neumann met Turing in person on his 1943 visit (and he certainly wouldn't have known of the Bletchley project), although he may have talked to British scientists influenced by him. Nor is there any evidence that he was influenced by Turing's ACE design. Possibly, the two pioneering designs were conceived independently. Turing's biographer sees the British and American initiatives as having only a "tenuous" connection, alongside a "very marked independence" (Hodges 1983: 304). In short, just who owed what ideas to whom may remain forever opaque (but see Copeland 2001).

This particular priority question is intriguing (though not crucial) here, because the protagonists are so important for our general discussion. Both Turing and von Neumann raised basic theoretical issues in cognitive science, and both feature prominently in several later chapters.

By contrast, the historical priorities in computer design *as such* aren't relevant for our purposes. Perhaps this is just as well—for a discussion of them would not only involve many names besides the few mentioned above, but would be highly contentious to boot.

For instance, the design of the EDVAC used ideas also from J. Presper Eckert and John Mauchly, whose wires-and-plugboard electronic calculator ENIAC was functional by November 1945. Indeed, von Neumann was a latecomer to the Moore School's EDVAC team, which already included Eckert and Mauchly (Arthur Burks was also a member of the team: see 15.v.b). A bitter dispute over priorities, and patents, ensued. Even after a court ruling in 1972, which attributed the core idea to someone else entirely (John Atanasoff 1940), priority battles continued for many years (Aspray 1990: 34–8). Other priority arguments may have escaped the law courts, but are hotly disputed nonetheless (Bowden 1953; Randell 1972, 1973; Hodges 1983, chs. 5 and 6; Aspray 1990).

Such complexity, and contention, isn't surprising. As remarked in Chapter 1.iii.f, several people often contribute to a discovery or invention, especially one involving both theoretical and technical developments. And the more complex the origins of the idea, the more difficult it may be to identify what counts as "the" idea under discussion.

Just what, for example, counts as a *computer*? Our attribution of priorities must be influenced by how we answer that question. (Hence the need to distinguish different sorts of machines: analogue or digital, calculators or symbol manipulators, (electro)mechanical or electronic, dedicated or general-purpose, rigidly "wired" or program-controlled, hand-instructed or stored-program.) Moreover, a highly valued and/or commercially successful idea will often be claimed after the fact by many people, some more disingenuous than others.

In the case of computer design, matters are even more complicated than usual. Historical details have been, and many will forever remain, hidden by wartime—and post-war—secrecy in the USA and the UK. Even the development of supercomputers, designed well after the Second World War for the nuclear weapons industry, is still part-veiled in secrecy (MacKenzie 1991*a*).

3.vi. In Grandfather's Footsteps?

Babbage's work has some historical interest for cognitive science, even though he himself wasn't a budding cognitive scientist. It doesn't follow that he actually had any historical *influence* on the field (see Chapter 1.iii.a). If he did, it was in virtue of the part he played in the development of the modern computer. However, that "if" wasn't an empty rhetorical device: there's genuine disagreement over Babbage's role in this matter.

To be sure, his work had no influence on the development of analogue computers. In such machines, quantities are represented not by discrete (digital) states but by continuously varying physical features, such as mechanical rotation or electrical voltage.

Simple analogue calculating devices had been used since the early eighteenth century (Pratt 1987: 139). If any nineteenth-century writer prompted 'modern' developments in analogue computing, it wasn't Babbage but the physicist William Thomson, later Lord Kelvin (1824–1907).

In the mid-1870s Thomson's elder brother James built an ingenious device wherein *any desired fraction* of the motion of a revolving disc could be communicated to a cylinder mounted above it—and he used it to do simple integration (J. Thomson 1876). William realized that (combinations of) machines based on his brother's idea might be applied to more complex problems, such as analysing harmonics or solving differential equations (W. Thomson 1876; Bowles 1996). So he built another analogue machine, intended "to substitute brass for brain in the great mechanical labour of calculating the elementary constituents of the whole tidal rise and fall". However, although his tidal harmonic analyser worked in practice, his design for a differential integrator didn't.

When the first useful differential analysers were built in the USA in the late 1920s some authors, such as MIT's Vannevar Bush, cited the Thomsons' research—but they didn't cite Babbage (Bush 1931). That's not surprising. If Babbage was important in the development of computers, it's digital machines which are at issue. But, as we'll now see, that "if" is genuinely iffy.

a. Conflicting evidence

In 1851 Albert Smee had announced his imaginary machine to be an "absolute impossibility . . . for practical purposes" (2.ix.a). But twenty years before that, Babbage had declared his Analytical Engine to be feasible: it might fill his workshop, but it wouldn't need—to borrow Smee's words—"an area exceeding probably all London".

By the mid-1950s, when functioning modern computers had appeared on the scene, it was clear that Babbage's vision had been vindicated. Late twentieth-century machines incorporate many logical and procedural principles enunciated in the early nineteenth century by Babbage (see Section iii.b). In short, the Analytical Engine and the modern computer are essentially equivalent.

However, as we saw in the case of Vaucanson (Chapter 2.iv), theoretical equivalence isn't the same thing as actual historical influence. Babbage's biographer describes him as "Pioneer of the Computer" (Hyman 1982). In the sense that he was the first to design a symbol-manipulating machine whose fundamental principles are, in essence, those of a general-purpose computer, this appellation is just. Indeed, there's probably no one who'd deny that he had "vision verging on genius" (M. V. Wilkes 1991: 141). But the implication that where he led, others followed (as with the pioneers who opened the American West), is disputed.

Broadly, there are three views on this matter. Some say that Babbage was unknown to or disregarded by computer scientists. Others declare that he was an inspiration to them. Yet others believe that his failure to complete his Analytical Engine, which so embittered his own life, acted as a serious disincentive to comparable work a century later. If that's true, then Babbage's over-confidence delayed computer engineering much as the over-optimism of early machine translation, symbolic AI, and connectionism hindered their later development (see Chapters 9.x.e, 11.iv, and 12.iii).

These very different views on Babbage's influence are illustrated in turn, below. But adjudicating between them isn't easy. I'll be quoting various reminiscences, but one must be wary of taking these at face value. Someone may want—consciously or not—to claim an influence because of the eminence of the historical figure concerned, or to deny it in order to imply their own originality. This caveat will need to be borne in mind in later chapters, too (for example, in Chapter 9.ii–iii).

The first view is that "Babbage was neither influenced by what had gone before nor influential upon what followed him" (Collier 1970, p. v). Collier's not the only historian to have come to that conclusion.

A press notice prepared (by Doron Swade) for the Babbage bicentenary exhibition at the Science Museum in 1991 (see Section ii.b), while acknowledging the closeness of Babbage's ideas to today's computers, said that "he had no effect on the development of large-scale calculating machines". Similarly, Allan Bromley, an expert on Babbage's papers (and a computer collector whose finds are now in Sydney's Powerhouse Museum), judged that "Babbage had effectively no influence on the design of the modern digital computer," and that "Babbage's papers cannot have influenced the design of modern computers in more than the most superficial manner" (Bromley 1991: 9; 1982). That wasn't said from any lack of respect for Babbage: it was Bromley who suggested that London's Science Museum build the Difference Engine for the bicentenary.

These dismissive historical judgements are supported by specific disclaimers from some of the modern computer pioneers themselves. For instance, Wilkes, as a Cambridge mathematician, was well aware of his eminent predecessor. But at the Memorial Meeting in 1971, on the bicentenary of Babbage's death, he remarked: "In writing of Babbage as a computer pioneer one must at once admit that his work, however brilliant and original, was without influence on the modern development of computing" (M. V. Wilkes 1971: 1).

Or again, John Brainerd—the Director of the Moore School, and project manager for the ENIAC—declared in 1965 that "The development of the ENIAC was in total ignorance of Babbage's work . . . Babbage's direct influence was *nil*" (quoted in an unpublished Ph.D. thesis by Alex Arbel: D. Swade, personal communication). And Wilkes has said that "It was not until after the project was completed that [the ENIAC team] heard about the work of Charles Babbage," adding (what is not the same thing: see 1.iii.h) that "indeed it is obvious that the ENIAC shows no influence from that source" (M. V. Wilkes 1982: 55).

Babbage was certainly mentioned in lectures at the Moore School in 1946 (Metropolis and Worlton 1980). By that time, however, ENIAC was complete. And he may well have been discussed by Flowers, who knew of him from Bletchley Park, when Flowers visited the Moore School in 1945 (Copeland 2004). Again, however, ENIAC was by then more than a sparkle in the Moore School's eye: it was fully functional by November of that year.

If we count Zuse as a (even *the*) computer pioneer, we have an entirely clear case of a 'modern' computer being built *in ignorance* of Babbage. For Zuse learnt about his Victorian predecessor only when he applied for a US patent in 1939:

> When I began to build the computer [in 1933], I neither understood anything about computing machines nor had I ever heard of Babbage. It was only many years later, when my constructions and switches were basically set, that an examiner from the American Patent Office showed me Babbage's machines. The otherwise extremely thorough German examiners had not been acquainted with Babbage. (Zuse 1993: 34)

The remark about the German examiners is telling: even they, despite their usual thoroughness, knew nothing of Babbage. (Or if they knew of him, they didn't see the connection.)

Others imply, to the contrary, that Babbage's work was a positive influence in the construction of modern computers. A few say this explicitly. For instance, a friend of some members of the ACE team—who often spoke of Babbage during the mid-1940s—recalled in 1965 that:

> In 1913, I turned back to a boyhood interest in Babbage's 1834 dream of an Analytical Engine, a self-operating, self-recording calculating machine—and during the 1914–18 war I was still thinking in terms of gear wheels . . . In 1934 [I had developed a] plan of [an] electronic computer working in binary, but with octonary (digits 0 to 7) input and output completed to make the human operator's task easier. Babbage's 1834 sleeping beauty had awakened—after the proverbial hundred years. (E. W. Phillips, quoted in Randell 1972: 14)

Most express it less directly. The editor of an early volume on digital computers, which included the first reprint of Lovelace's paper of 100 years before, declared: "This book is devoted to an account of the construction and use of the machines which [Babbage's] vision inspired" (Bowden 1953: 7). Twenty years later, another computer historian wrote: "Thus the saga of Babbage's Analytical Engine came to an end, although its fame lingered on *and inspired several other people to attempt what Babbage had failed to achieve*" (Randell 1973: 12; italics added).

Babbage's Analytical Engine was praised by Turing in 1950 as the first universal digital computer, but without any explicit suggestion that it had inspired his own work (A. M. Turing 1950: 16, 26). However, Turing's biographer points out that he certainly knew of Babbage's "universal" Engine by the mid-1940s, and discussed it on several occasions with a friend (Hodges 1983: 297). Others at Bletchley later described Babbage's ideas *and* his proposed machine as "on occasion a topic of lively mealtime discussion" there (Copeland 2001: 348). And Babbage's biographer endorses the implication: "It seems likely that [Babbage's work] was one of the sources for Alan Turing's [theoretical] Turing Machine" (Hyman 1982: 255).

Babbage's biographer also remarks that: "Amongst the mathematical elite of Cambridge Babbage's work was never forgotten and remained almost synonymous with the idea of mechanizing computation" (Hyman 1989: 327). That elite included not only Turing himself, but also Newman and Wilkes—yet Wilkes insisted, as we've seen, that Babbage's research was "without influence" on the development of modern computers. (Aiken was led to Babbage's work in the late 1930s by seeing the model Difference Engine donated by Babbage's son to Harvard—Swade 1991: 36; however, as noted in Section v.b, his Mark I was a calculator, not a general-purpose computer.)

These remarks suggest the possibility, though hardly the probability, of Babbage's influence. There's no unambiguous acknowledgement here, as had earlier been given by the Spanish engineer Leonardo Torres y Quevedo (1852–1936).

Torres y Quevedo constructed a wide range of (analogue and digital) calculating machines and automata, including the first chess automaton in 1911, which played endgames using only three chessmen and only five possible moves (*Scientific American* 1915). He credited Babbage at length as the inspiration for his own design for an electromechanical "analytical engine", and for his conception of "automatics" in general (Torres y Quevedo 1914: 91, 101–2). However, his work didn't directly influence the design of the electronic computer. Rather, it was rediscovered at mid-century largely as a consequence of that advance.

Yet others (thirdly) see Babbage's legacy as a hindrance, not a help. Evidently, the "calumny and misrepresentation" which he—and his oyster-scientist—endured cast its shadow for 100 years. Wilkes (1971: 1) again:

> More important was the fact that Babbage's projected image became one of failure, with the result that others were discouraged from thinking along similar lines and eventual development of computers was delayed.

This casts a new light on the remark about Cambridge's "mathematical elite" (quoted above), whose intended implication was that Babbage's work played a positive role in the Cambridge achievements. Wilkes suggests, to the contrary, that the widely shared knowledge of Babbage's example was a largely negative influence.

Again, the very man who hailed the Harvard Mark I as the realization of Babbage's dream also said: "This dark age in computing machinery, which lasted one hundred years, was due to the colossal failure of Charles Babbage" (L. J. Comrie, quoted in I. B. Cohen 1988: 180). He himself, an expert on the production of mathematical tables, was specifically discouraged (in the 1930s) by Babbage's example from trying to design a machine for calculating them automatically (Swade 1996: 40–1).

Such commentators argue that Babbage's failure cast its shadow not only over the intellectual optimism of (some) individual scientists, but also over the readiness of funding agencies to support their more ambitious projects.

The earliest recorded example of this effect occurred in the early 1840s. A calculating machine "in certain respects vastly more promising than Babbage's", designed by a Devonshire printer called Thomas Fowler, was refused governmental support, or even consideration. The reason—so Fowler's son wrote—was that they'd already "spent such large sums, with no satisfactory result, on Babbage's [Engine]" (quoted by Swade 1996: 40). And the father's fate was eerily similar to Babbage's. In his son's words, again:

> It is sad to think of the weary days and nights, of the labour of hand and brain, bestowed on this arduous work, the result of which, from adverse circumstances, was loss of money, loss of health, and final disappointment. (quoted in Swade 2000: 311)

Given the drying-up of money, following the early high hopes (of Babbage and of Fowler), one's reminded of two twentieth-century phenomena. Namely, the setbacks in research funding, *and* in intellectual respect from the wider scientific community, that have beset AI as a result of its failure to live up to various promises—some of which had been carelessly made (see Chapters 11.iii–iv and v.b, and 12.iii and vii).

b. So what's the verdict?

The three positions just outlined aren't so starkly inconsistent as they may appear. For example, some social groups—elite or otherwise—may have known about Babbage's example while others didn't. Moreover, familiarity with Babbage's work could have encouraged someone to believe that machines doing highly general computations are in principle possible, while *also* discouraging them from trying to build computers using his approach—or even from building computers at all.

Nevertheless, conflicts in evidence remain. The truth will probably never be clear. Given this situation, the judgement offered by the director of the Science Museum's Difference Engine project seems to me to be fair:

> There is no unbroken line of development between Babbage's work in the nineteenth century and the modern computer. His Analytical Engine was a developmental cul-de-sac. His efforts represented an isolated episode, a startling and magnificent one, but an episode nonetheless. There is a great gap: the movement that led to the modern computer did not resume until the 1940s when pioneers of the electronic age of computing rediscovered many of the principles explored by Babbage, largely in ignorance of his designs.
>
> However, there is more owing to Babbage than a respectful and perhaps awed salute across a barren gulf of time. His exploits and his aims were an integral part of the folklore shared by the small communities of scientists, mathematicians and engineers who throughout remained involved with tabulation and computation. Babbage's failures were failures of practical accomplishment, not of principle, and the legend of his extraordinary engine was the vehicle not only for the vision but for the unquestioned trust that a universal automatic machine was possible. The electronic age of computing was informed by the spirit and tradition of Babbage's work rather than by any deep knowledge of his designs which have attracted detailed attention only in the last few decades. (Swade 1991, pp. ix–x)

Let's give Babbage himself the last word. Nearing the end of his long life, he said—proudly, if sadly:

> If, unwarned by my example, any man shall undertake and shall succeed in really constructing an engine embodying in itself the whole of the executive department of mathematical analysis upon different principles or by simpler means, I have no fear of leaving my reputation in his charge, for he alone will be fully able to appreciate the nature of my efforts and the value of their results. (Babbage 1864: 338)

4

MAYBE MINDS ARE MACHINES TOO

Maybe minds are machines, too! As late as 1930, this shock-horror thought hadn't even attained the status of a heresy. It wasn't a heresy, because no one believed it. Indeed, no one had even suggested it.

The brain, to be sure, had long been compared to machines—in the Roaring Twenties, even to jukeboxes—and nervous conduction had been crudely mimicked by physical models before the First World War (see Chapter 2.viii.f). But *mental* processes lay untouched. Many psychologists avoided all mention of mind, referring to behaviour and/or brain instead (5.i.a). Others granted that mind *as such* is a fit subject for science (2.x). But no one was defending the strong interpretation of 'mind as machine': that *the same type of scientific theory could explain processes in both minds and mindlike artefacts*.

To put the point another way, by 1930 no one had yet argued that mind and/or mental processes, *conceptualized as somehow distinct from matter*, could be understood in *machine-based* terms. Two hundred years earlier, of course, Julien de La Mettrie had provocatively spoken of 'Man, a Machine' (2.x.a). But he'd conflated mind and matter. And by "machine", he meant mechanistic physics in general, not specific types of artefact.

This situation changed in the years around 1940, with the emergence of two new ways of conceptualizing the mind—based on two novel types of machine. The newly minted mind-as-machine hypothesis drew on ideas from logic and/or physiology whose beginnings lay in the late 1800s (see 2.vii–ix). But the logic was now associated with computer science, and the physiology with cybernetics—or "circular systems" (the term *cybernetics* wasn't used until 1947).

By mid-century, and largely because of the invention of the modern digital computer (3.v), the stage was set for a flowering of research on this general theme. That efflorescence had a lasting effect on psychology, anthropology (up to a point), linguistics, AI/A-Life, neuroscience, and philosophy: see Chapters 5.iv.b–f and 6–16.

Some early devotees—notably, Warren McCulloch—drew on formal computation and cybernetics more or less equally. And most of them were sympathetic to both. But the theoretical loyalties, and the sociological groupings, gradually diverged. In general, however, and with the arguable exception of dynamical theorists (see 14.ix.b and 15.viii.c), mind and mental processes were distinguished from their material base, being glossed in abstract (non-physical) terms.

In short: by 1960 things had changed. *Mind as machine* was now a major heresy. ("Heresy", because most people still thought it incredible: see 16.i–ii.) Indeed, McCulloch himself used this very word: "Our adventure is actually a great heresy. We are about to conceive of the knower as a computing machine" (1948: 144). This chapter narrates how that heresy was born, and how it gave rise to two different—sometimes, passionately competing—sects within the field as a whole.

We'll begin by seeing that Alan Turing was the first proponent of this new way of placing mind in nature. Having defined computation formally for the first time (see Section i), he soon argued that minds and computing machines employ *the same general type* of operation—even if they may also employ others (see Section ii). Section iii describes how Turing's ideas were taken up by McCulloch, who suggested that *the same specific computations* are involved. "The whole of psychology", he said, boils down to the definition of particular logical networks. And Section iv shows how McCulloch's work, in turn, influenced John von Neumann (1903–57) to use binary logic in designing his electronic computer.

The cybernetics movement saw the mind as controlled by feedback processes like those in the bodies of living things—and in the machines then being developed by control engineers. The core theoretical idea is explained in Section v, as is the closely related concept of "information". Section vi outlines a cybernetic theory based on the notion of cerebral models, or representations. A wide range of mid-century self-regulating machines is described in Section vii, and two especially influential—and recently resuscitated—examples in Section viii.

Finally, Section ix notes the professional schism that eventually separated the two ways of thinking about mind as machine.

4.i. The Turing Machine

First and foremost, Turing was a mathematician. His ideas about mind (discussed in Section ii) were grounded in his work on the nature of computation *as such*. The famous 'Turing machine' was an abstract mathematical concept, not a clanking or sparking device. And he described other abstract "machines", with no suggestion that they could ever be implemented.

Nevertheless, some of the work discussed in this section influenced the development of modern computing (which *isn't* to say that this wouldn't have developed without it: see Chapters 3.v.a and 16.ix.a). More to the point, it was used by others to help ground a persuasive vision of the possibility of computational psychology and AI (see Section iii below, and Chapter 16.iii.b).

a. Turing the man

As a person, the relatively solitary Turing was very different from the gregarious Charles Babbage (Hodges 1983; Babbage 1864; Hyman 1982). The "lady examiner" who asked Babbage an ignorant question and was treated with gentleness by him (see 3.i.a) would have received a far less gentle response from the prickly Turing.

He didn't bear fools gladly—and, in his eyes, most people were just that. On one famous occasion in 1943, when he was visiting Bell Labs in wartime New York, a sudden lull in conversation in the executive lunchroom enabled all to hear his high-pitched voice saying:

No, I'm not interested in developing a *powerful* brain [in a computer]. All I'm after is just a *mediocre* brain, something like the President of the American Telephone and Telegraph Company. (Hodges 1983: 151)

That's not to say that he was impatient with people whose intellect he did respect. After his death, the zoologist John Z. Young wrote this to his mother:

My impression of your son is of his kindly teddy-bear quality as he tried to make understandable to others, ideas that were still only forming in his own mind. To me, as a non-mathematician, his exposition was often difficult to follow . . . (S. S. Turing 1959: 105–6)

Young's memories here were probably correct, but his reference to Turing's "kindly teddy-bear quality" may have been a diplomatic exaggeration aimed at a grieving mother. For she'd lost her brilliant son when he was only 41. Turing—again, like Babbage—ended his life in unhappiness. Indeed, he endured tragic desperation (Hodges 1983, ch. 8).

He was a homosexual at a time when homosexual activity was illegal in England. (He'd proposed marriage to Joan Clarke, another Bletchley cryptologist, in 1941, and had given her a ring; but he broke off the engagement a few months later—Hodges 1983: 206 ff., 216–17.) In January 1952, on the very day when his BBC debate about mind and machine was repeated on the radio (see 16.ii.a), he made the extraordinarily unworldly mistake of informing the police of a trivial burglary he already suspected might have been committed by a young man he'd invited into his flat (Hodges 1983: 449–55). He was arrested, charged, and tried. There was a whispered word to the judge, to the effect that he'd done great service to his country and should be spared prison accordingly. So he was offered probation—provided that he accepted treatment with female hormones. After a while, the effects on his physique became highly embarrassing.

As though that weren't dispiriting enough, a major spy scandal involving homosexuals (and the Director of MI5, Kim Philby, discovered to be a double agent whose prime loyalty was to the Soviets) prompted the Civil Service in 1952 to embark on positive vetting of everyone who already had security clearance, not just those on the waiting list to get it. Because of his work in the Second World War (see below, and 3.v.c–d), Turing had the highest security clearance possible. Very likely, he would have to suffer the humiliation of having it withdrawn—possibly, with a farewell fanfare in the nastier newspapers. (The decidedly nasty *News of the World* had already featured his court case.)

On 7 June 1954 he committed suicide by taking potassium cyanide.

That, at least, was the verdict of the coroner's court. His mother disagreed. This was perhaps only to be expected, given that suicide was then a crime—and especially as she knew nothing of the background mentioned above. (None of those matters, nor even the already public trial, were mentioned at the inquest—Hodges 1983: 488.) Moreover, she'd often warned him about his carelessness in playing around with poisons (ibid.).

In her memoir of her son, Sara Turing reported not only that neighbours had said he was very cheerful a few hours before he died, but also that he'd been looking ahead:

On his writing table ready for the post were acceptances of invitations for the near future, as well as tickets for the theatre, to which he was to take friends that very week.... [And his close friend Dr Gandy told me]: "When I stayed with him the week-end before Whitsun, he seemed, if anything, happier than usual; we planned to write a joint paper and to meet in Cambridge in July." (S. S. Turing 1959: 118)

Her explanation was that his lifelong habit of doing experiments at home had led to "some unaccountable misadventure". Robin Gandy, on his last visit, had found him surrounded by many such experiments, most dealing with electrolysis. One of these, he told Sara,

almost certainly did produce potassium cyanide, and may have been intended to produce it: but this was not the aim of the experiments as a whole, which was to produce a wide range of chemicals from the simplest and most easily obtainable ingredients [i.e. in "desert island" conditions]. (p. 116)

But, said his mother, "No poison was found in his bedroom. There was just a partly-eaten apple by his bed, for, as a rule, he used to eat an apple at night" (p. 117).

The apple, strangely, was never analysed. If, "as seemed perfectly obvious" (Hodges 1983: 488), it had been dipped in cyanide (two jars of which were found in the house), this was never conclusively proven. Even if it had been, the reasons behind the tragedy would have remained a mystery. Turing's biographer said: "Like Snow White, he ate a poisoned apple, dipped in the witches' brew" (p. 489). "But what", he asked, "were the ingredients of the brew? What would a less artificial inquest [have] made of his last years?" The answer, discussed over his next thirty-eight pages, remains unclear to this day. Whatever the truth of it, Turing was dead at half Babbage's age: Babbage had reached 80.

Let's pass on to happier comparisons.

As mathematical intellects Turing and Babbage were remarkably similar. Both were superb cryptologists, for instance. In the late 1930s, Turing built a very small machine (using electromagnetic relays) to use binary multiplication for relatively secure encoding; and during the war he developed a general theory of cryptanalysis.

More to the point, for our purposes, Turing was the first person "fully able to appreciate the nature of [Babbage's] efforts and the value of their results" (see the closing quotation of Chapter 3).

He did so a good twelve years before modern computers were built (3.v). For he'd proved in the mid-1930s that a machine defined in purely abstract terms could—like the Analytical Engine (which was itself only a *design*: 3.ii.b and iii)—compute anything that's computable (A. M. Turing 1936). This proof is so important for the history of computing that a collector recently paid $19,000 for a copy of the journal in which it first appeared (J. M. Norman 2005). For this abstract machine, soon dubbed a "Turing machine" by Alonzo Church (1937), was essentially identical to a general-purpose digital computer—whether built from "wheelwork" or electronic circuits.

b. Playing the game

Babbage, like everyone else before Turing, had assumed that we know intuitively *what it is* for something to be mathematically deducible (computable). But Turing offered an explicit definition. Indeed, the new "machine" was described specifically in order to make this definition clear.

Before discussing his seminal definition of computation in any detail, let's consider an informal version given by Joseph Weizenbaum (1976: 51 ff.).

Weizenbaum asks us to imagine a game played with a roll of toilet paper, many white stones, five black stones, and an ordinary die. To set up the game, one does this:

1. Roll out the toilet paper on the floor.
2. Put down stones as follows:
   (i) on an arbitrary square, one black stone;
   (ii) on successive squares to the right of the square holding the black stone, as many white stones as you please, one to each square;
   (iii) on successive squares continuing to the right, one black stone, skip one square, one black stone;
   (iv) on successive squares continuing to the right, an arbitrary number of white stones;
   (v) on the square to the right of the last white stone, one black stone;
   (vi) finally, one black stone, the "marker", [placed on the floor] above the square holding the rightmost white stone.
3. Turn the die so that its one-dot side is facing upward, i.e., so that it is showing "1."

Having set up the game, one starts to play it. The rules of the game are called "transformation rules". In general, they work like this:

The marker stone is moved either one square to the left or one to the right on each move. However, before each move, the stone under the marker stone is replaced or removed according to the applicable rule. The die may be turned to a new side after each move.

Each of the eighteen rules mentions a starting position defined in terms of an orientation of the die and a certain number of stones (which may be zero) on the square under the marker. To play the game, the player looks at the die and the toilet roll to see what the existing position actually is, and finds the appropriate rule accordingly. Then, he does what that rule tells him to do:

Each rule says to do three things:

1. Turn the die so that it reads the stated number.
2. Replace the stone under the marker by the kind of stone specified—possibly by no stone at all.
3. Move the marker one square in the indicated direction.

Doing this, of course, creates a new game situation. Accordingly, the rule which is *now* appropriate is found, and obeyed. When a rule tells the player to turn the die to read "0", the game stops.

As for just what Weizenbaum's eighteen rules are, they're defined by the table shown in Table 4.1. Boring, they may be. This game won't rival soccer, or even tiddlywinks. But unclear, they're not. At every move of the game, the player knows precisely what must be done. There's no ambiguity, and no choice—except in setting up the game in the first place. What happens depends entirely on (*a*) the rule being followed, (*b*) the position of the marker, and (*c*) the stones, or lack thereof, at present under the marker.

For anyone who doesn't already know what a Turing machine is—in other words: how, at base, a digital computer program works—this trivial game should help

Table 4.1. The rules of the toilet roll game

IF THE DIE READS	AND THE STONE UNDER THE MARKER IS	THEN TURN THE DIE TO	REPLACE THE STONE BY	MOVE MARKER
1	none	3	white	left
1	black	2	no stone	left
1	white	1	white	left
2	none	2	no stone	left
2	black	3	no stone	left
2	white	5	no stone	right
3	none	3	no stone	left
3	black	4	no stone	right
3	white	5	no stone	right
4	none	4	no stone	right
4	black	1	black	right
4	white	6	white	left
5	none	5	no stone	right
5	black	1	black	right
5	white	1	white	left
6	none	0	no stone	right
6	black	0	black	right
6	white	3	white	left

Source: Weizenbaum (1976: 53)

ease them into some very non-trivial mathematical ideas. For Turing's 1936 paper defined *computation* in terms of transformations (moves, rules) of the same general type. His definition was both mathematically rigorous and relatively easy to grasp. In addition, it was evidently possible to implement computation (so defined) in a *physical* machine—though we'll see in Section ii.a that Turing's informal example featured pencil and paper, not toilet roll and stones.

c. What computation is

Turing's central aim in his 1936 paper was to clarify the intuitive notion of computability. Initially, he thought about this concept purely as a mathematical exercise. He'd been prompted to do so, early in 1935, by a remark of Max Newman's, in his lectures on the foundations of mathematics (Hodges 1983: 90 ff.).

Newman had been discussing one of three fundamental questions about mathematics that he'd heard David Hilbert (1862–1943) raise a few years before, at a conference in 1928. The meeting had been puzzling over the type paradoxes described by Bertrand Russell and Alfred Whitehead in their *Principia Mathematica* (1910). These paradoxes threatened to scupper their goal of basing mathematics on logic—a goal previously recommended by Russell's logical muse, Gottlob Frege (2.ix.b). Hilbert had asked whether one could state in general terms just what could, and what could not, be proved by their theory. In other words: is there some way of deciding, for *any* mathematical statement, whether that statement is provable? (Hilbert himself believed the answer must be *Yes*. "In mathematics", he said, "there is no *ignorabimus*"—that is, there's no "We shall never know".)

This question (known as the *Entscheidungsproblem*) didn't seek the proof of a particular mathematical assertion. Rather, it sought a reliable and precisely definable method for deciding whether or not, for any such assertion, some proof is in principle available. As Newman put it in his lecture: is there a "mechanical procedure" for proving any assertion to be decidable?

Turing (1936) took Newman's expression seriously. But (like Newman himself) he interpreted "mechanical" to mean *precisely definable*, *deterministic*, or even *mindless*—not *physical*. That is, he asked not how such a procedure could be implemented, but how one could define—in abstract terms—just what sort of procedure it would have to be.

Gödel-bells will now be ringing in many readers' minds. For Hilbert's three queries about mathematics had soon been addressed by Kurt Gödel (1906–78), who'd proved a highly counter-intuitive result: that a mathematical statement may be incapable of proof, even though it can be seen to be true (Gödel 1931). (Later, this would be cited in various arguments trying to prove the impossibility of "strong AI", or formal–computational intelligence: see 16.v.a.) This result contradicted Hilbert's hunch that every mathematical statement must be decidable. But it didn't resolve the *Entscheidungsproblem*, for it didn't show whether there's any method of telling just which statements are/aren't decidable—still less, what that method might be.

One might expect that Turing's first thoughts about computability would have been informed by Gödel's theorem. But they weren't. It's not clear that he even knew about it when he wrote his paper in 1935. Certainly, Newman's final lecture had mentioned it—but perhaps Turing hadn't been there? There's good evidence that he was introduced to Gödel's work not by Newman, but by the philosopher Richard Braithwaite (Preface, ii).

Turing's biographer says merely that "He had a few words with Richard Braithwaite at the High Table one day on the subject of Gödel's theorem" (Hodges 1983: 108–9). But Braithwaite once remarked to me on "Turing's complete ignorance of Gödel's work when he wrote his 'computable numbers' paper", adding: "I consider I played some part in drawing T's attention to the relation of his work to Gödel's" (letter from R.B.B. to M.A.B., 21 Oct. 1982). However that may be, Turing's paper made no reference to Gödel. He took the *Entscheidungsproblem* neat, without Gödelian ice.

In his paper, Turing conjectured that a statement is decidable if and only if there exists a definite method for computing it. And "computation" and "definite method" (i.e. algorithm) were, for the first time, precisely defined.

In addition, he proved that some statements *aren't* decidable in this sense, and that some numbers *aren't* computable—even though they can be mathematically defined. (Hilbert's hunch assailed a second time.) In particular, whether or not *any* given statement is decidable (computable) is not itself decidable: the goal motivating the *Entscheidungsproblem* is unattainable. It follows—though he didn't say so, for computers weren't yet on the horizon—that there's no *general* way of knowing, for just any computer program, that it will eventually halt. (*Ignorabimus*, after all.)

Computability (decidability) was defined by Turing in terms of an abstract machine—a fully deterministic system—now called a Turing machine. More accurately: the Turing machine, like the Analytical Engine, was an infinite class of machines. Similarly, there are an infinite number of toilet roll games. That's no accident, of course, for Weizenbaum's game was closely modelled on the ideas in Turing's paper.

In general, a Turing machine consists of: (1) a tape divided into squares, on each of which a symbol can be written, (2) a "scanning-head" that can consider one, and only one, square at a time, and (3) a set of rules (a "table of behaviour") specifying what the machine will do next in this or that circumstance. The machine can read the symbol on the scanned square, and—if the rules so determine—erase it or replace it with another. The tape can be moved one square backwards or forwards, or the machine can stop.

"Moving backwards and forwards" refers to predecessors and successors in some ordered sequence, and is often interpreted in terms of discrete moments in time. For any moment t, the state ("configuration") of the machine at the following moment $(t + 1)$ is fully determined by two things: the state of the machine at time t and the symbol on the square being scanned at time t. The system has a finite set of rules, and a finite set of allowable symbols. However, the tape itself is infinite, so some computable numbers (such as *pi)* are infinite in length.

Turing showed how such abstract machines could be described in a standard logical form, and how they could be used to do elementary computations out of which all standard arithmetical operations could be constructed. He proved—or rather, he claimed—that anything that is computable can be computed by some Turing machine. This is circular, if "computable" is defined as Turing-computable. Turing's claim, rather, was that anything that is computable in the mathematicians' previous (intuitive) sense of the term is also computable in his own, more rigorous, sense.

These were "claims" rather than proofs, since the intuitive sense of computation isn't definable. Indeed, this is why people speak of the Church–Turing "thesis".

The logician Church reached an essentially similar conclusion about the *Entscheidungsproblem* in the same year as Turing, though he defined computability in terms of a novel mathematical formalism, the lambda calculus, not in terms of a potentially physical process. His claim was that intuitive computability boils down to what mathematicians call recursive functions (which would later be implemented by LISP: Chapter 10.v.c). Turing soon proved that these pick out the same set of mathematical operations as Turing-computability does. But because it can't be strictly proved that they map onto the intuitive concept of computability, the claim that they do is known as the Church–Turing *thesis*, not the Church–Turing *proof*. Nevertheless, because this thesis is widely believed to be true, people—including computer scientists—often say that Turing "proved" that anything which is computable is Turing-computable.

Even more significantly, Turing proved (*sic*) that one abstract machine—the "universal" Turing machine—is capable of imitating any other, if the standard description of the second machine (in modern parlance, its program) is written onto the tape. More accurately, there's an infinite class of universal machines, each defined by a different set of rules enabling it to imitate other Turing machines. Any universal machine is capable of computing anything that is computable.

d. Only programs, not computers

You may be muttering that the statement I've just made is—strictly—false, because it ignores the issue of run-time input. A Turing machine that accepts data from the environment while the computation proceeds can compute functions which a universal Turing machine cannot.

True. But this talk of "accepting data from the environment" inevitably suggests a context of *physical* computing machines—for vision, perhaps, or natural language processing. And that's not surprising. For Turing, in his 1936 paper, was (in effect) talking about a computer program: a set of rules defined by software, but implementable in hardware.

Babbage had already had this idea, as we've seen. Moreover, Ada Lovelace had specifically pointed out that numbers could symbolize either data or operations (see 3.iii.b). And her observation that, in the Analytical Engine, "numbers meaning quantities" and "numbers meaning operations" were not—though they could have been—stored on different cards and executed by different columns applied *pari passu* to Turing's device. In a universal Turing machine, data and operations are stored on the same tape, read by the same scanning head, and acted on by the same set of rules.

Indeed, one and the same sequence of tape symbols can be interpreted sometimes as data and sometimes as instructions—a fact which was later crucial for von Neumann's account of self-reproduction (see 15.v). To take a homely analogy, suppose that your sister writes the words "Scratch your left foot" on a piece of paper. Then she asks you to fetch a paintbrush and a tin of red paint, and to copy those words above her front door. In that case, you're treating the written words as *data* (and someone who can read only the Cyrillic alphabet could do the job almost as well as you can). Now, suppose that she asks you, instead, to read the words on the paper and to do what they tell you to do (namely, scratch your left foot). In that case, you're treating the words as *instructions*—specifically, as instructions to carry out the *operation* of foot-scratching. (And the solely Cyrillic reader would be at a loss.)

Unlike Babbage, however, Turing (in his *Entscheidungsproblem* paper) didn't ask how, if at all, the primitive operations could be implemented. Even had he done so, he couldn't have said how to implement an entire Turing machine, because these are defined as having an infinite tape—which no actual machine can have.

He might have asked, instead, how one could build an *approximation* to a Turing machine, or even to a universal machine. The latter would be a general-purpose device, in the sense that it could do many different things, depending on which non-universal Turing machine (which program) was described on its tape at any given time. But, in the 1936 paper, he didn't ask this question either.

According to Newman, he'd been privately speculating about it right from the start (Copeland and Proudfoot 2005: 109 n. 5). But it's not clear that he then thought a quasi-universal computer to be a practical possibility. (I say "quasi-universal", because *in practice* no computer, and no programming language, is actually universal: see Chapter 10.v.) Indeed, he remarked dismissively to a friend that such a machine would have to be as big as the Albert Hall (Copeland 1998*a*). (Even this would be an advance on Alfred Smee, whose notional machine would have exceeded "probably all London": 2.ix.a.)

However, that was before he learnt about electronics. From the mid-1940s, the developments in computer technology outlined in Chapter 3.v were to build on Turing's first paper. He himself, for example, designed the ACE as a "practical version" of his abstract machine of 1936; and MADM—the first stored-program general-purpose electronic digital computer—was based on his theoretical ideas. It was now clear that a special-purpose digital computer is equivalent to some specific Turing machine, and a general-purpose computer approximates a universal Turing machine. In short, if one avoided cogs and wheels (and toilet paper?), programs could be usefully implemented after all.

Turing's initial lack of concern for practical implementation was evident also in his doctoral thesis of 1938 (supervised by Church). There, he defined another abstract machine, called an O-machine: O stands for 'oracle' (Turing 1939; Copeland 1998*a*,*b*). An O-machine is a Turing machine with one or more extra primitives. Each of these is an operation that returns the values of some mathematical function which is *not* Turing-computable. In other words, it "computes the uncomputable"—where the first of these terms is understood intuitively, and the second in the Church–Turing sense (see Copeland and Proudfoot 1999, 2000).

As for how these extra primitives do it, Turing said that—like the Delphic oracle—they work by "some unspecified means", and that we need "not go any further into [their] nature" (Turing 1939: 166–7). He was concerned only with what sorts of mathematical powers such systems would possess, if they existed. (Half a century later, people would start asking in earnest whether, and how, computers going beyond Turing-computation could be built: see 16.ix.a and f.)

4.ii. From Maths Towards Mind

If someone asks, "Did Turing believe the mind is a machine?", the answer is a definite "Yes". If they ask, "Did he believe the mind is *a Turing machine*?", the answer is "Yes, but . . .". However, if they ask whether he already believed this in the mid-1930s, his 1936 paper gives only a hint, not a definitive reply. It wasn't until the 1940s that he made his commitment clear—and even then, it was to theme rather than detail.

a. Computers and computors

The hint in the *Entscheidungsproblem* paper lay in how Turing led up to his abstract definition of computability. He did so by describing, in the simplest possible terms, how a human "computer" does, or could do, arithmetic.

In the quotations below (taken from pp. 135–40 of the 1936 paper), I've explicitly marked the fact that, for Turing—and everyone else—at that time, a "computer" was a human being. For instance, the "computers" in London's Scientific Computing Service, set up in 1936 by Leslie Comrie (1893–1950), were the people who operated the mechanical calculators—not the machines themselves (J. M. Norman 2004: 97).

Initially, the new devices (i.e. small calculators) were sometimes called "computing machines". And that language was carried over to (what we would call) the first real computers: in a meeting held in 1948, von Neumann himself referred repeatedly to computing machines, never to computers (Jeffress 1951: 37 ff.). But "computers" eventually won out—not without some misgivings. Even as late as 1955, most newspapers put the word *computer* in scare quotes, if they used it at all (Ceruzzi 1991).

Indeed, the early electronic computers were sometimes called "computors" (*sic*), to preserve the man–machine distinction. That applied, for instance, to the title of "the founding document in the electronic computer industry" (J. M. Norman 2004: 224), the 1946 business plan co-authored by J. Presper Eckert and John Mauchly: "Outline of Plans for Development of Electronic Computors [*sic*]". The o-spelling has now fallen out of use, but it was routinely favoured by computer scientists in the early days (R. L. Grimsdale, personal communication). And by philosophers, too: at mid-century, Wolfe Mays—or perhaps the editor and/or copy-editor of *Mind*—was writing about "modern digital computors" (*sic*) (Mays and Henry 1953: 484; cf. Mays *et al.* 1951: 262).

Having described how the human arithmetician computes, using pencil and paper in the familiar way, Turing then proposed that we imagine the person being replaced by a machine:

> Computing is normally done by writing certain symbols on paper. We may suppose this paper is divided into squares, like a child's arithmetic book . . . [Turing next pointed out that it needn't be two-dimensional: it could be a one-dimensional paper-tape.]
>
> The behaviour of the computer [by which Turing meant the person] at any moment is determined by the symbols which he is observing, and his "state of mind" at that moment . . .
>
> Let us imagine the operations performed by the computer [i.e. the person] to be split up into "simple operations" which are so elementary that it is not easy to imagine them further divided. Every such operation consists of some change in the physical [*sic*] system consisting of the computer and his tape. . . .
>
> The operation actually performed is determined . . . by the state of mind of the computer [i.e. the person] and the observed symbols. In particular, they determine the state of mind of the computer after the operation is carried out.
>
> We may now construct a machine to do the work of this [human] computer. . . .
>
> We suppose that the computation is carried out on a tape; but we avoid introducing the "state of mind" by considering a more physical and definite counterpart of it. It is always possible for the computer [person] to break off from his work, to go away and forget all about it, and later to come back and go on with it. If he does this he must leave a note of instructions (written in some standard form) explaining how the work is to be continued. This note is the counterpart of the "state of mind". We will suppose that the computer [person] works in such a desultory manner that he never does more than one step at a sitting. The note of instructions must enable him to carry out one step and write the next note. Thus the state of progress of the [person's] computation at any stage is completely determined by the note of instructions and the symbols on the tape.

These quotations are undeniably suggestive. But they don't establish beyond doubt that Turing already accepted 'mind as machine' as his credo. For Babbage, too, had designed computing machines by analogy with human beings (Gaspard de Prony's clerks) doing simple arithmetic. And Turing's "simple operations which are so elementary that it is not easy to imagine them further divided" were comparable to the detailed calculations embodied by Babbage as movements of toothed wheels. Yet Babbage, as explained in Chapter 3.iv, *didn't* see thought in mechanistic terms. Other evidence is needed, then, to show that Turing did.

There's no such claim in Turing's 1936 paper. In Babbage's terminology, the paper dealt with calculation and logic, not reason. Indeed, soon afterwards Turing implied that some thoughts, such as seeing the truth of an assertion Gödel had shown to be unprovable, are *not* computable. (Hence my answer: "Yes, but . . ." above.)

He attributed these thoughts to people's unconscious "intuition", which sometimes results in "correct" judgements—"leaving aside the question what is meant by 'correct' " (Turing 1939: 209–10). Some intuitive judgements later turn out to be provable; but some do not. He evidently believed that intuition, although not a computational process, is sometimes unavoidable:

> In consequence of the impossibility of finding a formal logic which wholly eliminates the necessity of using intuition, we naturally turn to "non-constructive" systems of logic with which not all the steps in a proof are mechanical, some being intuitive . . . [A non-constructive logic should] show quite clearly when a step makes use of intuition, and when it is purely formal. (Turing 1939: 210)

Similarly, his discussion of O-machines left it open whether the mind may be, in part, an O-machine—so able to think thoughts that aren't Turing-computable.

In sum, he certainly thought that much, perhaps even most, of what the mind does could be done by a Turing machine. But he allowed for the possibility that some mental powers may lie beyond such machines.

An anachronistic aside: This historical fact undermines a type of argument that appeared about twenty years later, and that is still frequently directed against cognitive science in general. It goes like this:

* Turing proved that *all* machines are restricted to Turing computation;
* so *any* mind-as-machine approach must be similarly restricted;
* if even one example of non-Turing-computable thought can be found (take your pick . . .),
* then cognitive science must be at best inadequate, at worst fundamentally flawed.

Despite its popularity, this argument misses its target—for the first premiss is false. It's false not because Turing tried but failed to prove that all machines are, in effect, Turing machines, but because he didn't even claim to have proved this (see Copeland 1997).

Moreover, by the end of the century various other definitions of "computation" were—and still are—being explored (16.ix). So those people are mistaken who claim that cognitive science, given its commitment to computational theorizing, *must* see the mind as a Turing machine. (I plead guilty: some years ago, I said that computational psychologists think of the mind "in terms of the computational properties of universal Turing machines"—Boden 1988: 5.) It's more accurate to say, as I did in Chapter 1.ii.a, that cognitive science aims to describe the mind by *concepts drawn from the best theory of what computers do—whatever that theory turns out to be*. (These somewhat cryptic remarks will be clarified in Chapter 16.ix.f.)

b. Commitment to the claim

Soon, however, Turing appears to have decided that the psychological relevance, if any, of O-machines was so limited that it could be ignored. (Presumably, that's why so many people believe that he claimed to have proven that minds are Turing machines.)

His remark (quoted in Chapter 3.v.c) that the ACE might display intelligence, though flawed by "serious mistakes", implies that he'd done so by 1945. And in 1947 he wrote a report in which he referred to man as a machine, and outlined the prospects for "making thinking machinery" (A. M. Turing 1947*b*: 12). This was the first AI manifesto—or rather it would have been, if it had seen the light of day. In fact, it wasn't officially published until 1969.

This paper made no mention of intuition, or uncomputable thoughts. On the contrary, it explicitly rejected the idea that Gödel's proof showed that intelligent computers are impossible. This would be so, said Turing, only if intelligence precluded the possibility of ever making a mistake (A. M. Turing 1947*b*: 4; cf. 1947*a*: 123–4).

In that same paper (A. M. Turing 1947*b*), he defined a different type of machine—what we would now call a connectionist system, or neural network—that could be induced not only to perform, but to learn (see Chapter 12.i.b). Indeed, he showed how this could happen in an initially random, or unorganized, network. And he pointed out that such networks could be simulated by a digital computer. From the historical point of view, this is interesting rather than significant, for the paper remained largely unknown for many years. Written as an internal report for the National Physical Laboratory, it wasn't communicated to the outside world until 1969.

Turing's discussion of neural networks was an aspect of his core argument for the possibility of intelligent computers: that "it is possible to make machinery to imitate any small part of a man". For instance, he said, TV cameras imitate the eye, and unorganized networks imitate (parts of) the brain. In other words, the body is the causal ground of thinking, so body-mimicking artefacts could think. He'd been musing on this topic for some years, having been excited by machine analogies for the body ever since childhood (see 2.viii.f).

Donald Michie, who founded the UK's first university department of "machine intelligence" (6.iv.e), worked in Newman's group at Bletchley in the early 1940s. He recalls that "many of the ideas [about thinking machines and artificial brains] in [Turing's] 1947 essay . . . were vigorous discussion topics during the war. Some of his younger associates were fired by this, although most regarded it as cranky" (quoted in Randell 1972: 6).

Cranky or not, in his assessment of the most promising areas for building intelligent machines, Turing highlighted most of the topics to be forefronted over the next twenty years by early AI—whether symbolic or connectionist.

For instance, he said that "intellectual activity consists mainly of various types of search" (Turing 1947*b*: 23). And he named the following as promising research areas: "Various games, e.g. chess, noughts and crosses, bridge, poker; the learning of languages; translation of languages; cryptography; mathematics" (p. 13).

He also mentioned vision programs, speech analysers, and robots—including a science-fiction version ("allowed to roam the countryside") of the Cog project (see Chapter 15.vii.a). To build that, one would "take a man as a whole and try to replace all the parts of him by machinery [such as] television cameras, microphones, loudspeakers, wheels and 'handling servo-mechanisms,' as well as some sort of 'electronic brain' ". These projects, he said, would be more difficult to achieve than chess, or even translation. For one would need to engineer sensory and motor organs interfacing with the outside world.

In that paper, and in his lecture on ACE to the London Mathematical Society in the same year (Turing 1947*a*), Turing had been asking scientific and practical questions—however visionary his answers may have been. He was considering just which sorts of computer-engineering methods, applied to just which sorts of thinking, would be most likely to afford progress. He identified exhaustive search as a promising computational technique, and implied that it might suffice for all types of thought: "we should not go far wrong for the time being if we assumed that all problems were reducible to this form. It will be time to think again when something turns up which is obviously not of this form" (Turing 1947*b*: 22).

The most plausible candidates for problems "obviously not of this form" were specifically included three years later. In 1950 he published a paper in *Mind*, in which he argued that there's no philosophical reason for denying the possibility of intelligent machinery (A. M. Turing 1950). If a computer artefact were to show performance indistinguishable from that of a human being, he said, we'd have no good reason for denying that it was *really* thinking, and *really* conscious. That argument, involving the so-called Turing Test (which he'd mentioned briefly already: Turing 1947*b*: 23), will be discussed in Chapter 16.ii. We'll see, there, that in his 1950 paper Turing was being part-mischievous—and that his mischief attracted far more philosophical attention than his serious points.

More relevant here, the *Mind* paper explicitly suggested that even the highest flights of human thought—not only reason, but also creative imagination—could be achieved by a computer. A computer might be able, for example, to compose Shakespeare's sonnet 'Shall I compare thee to a summer's day?', and to defend its choice of imagery in terms of rhyme, scansion, and cultural associations—such as seeing an affinity between Mr Pickwick and Christmas Day (Turing 1950: 53).

Mathematicians might have asked—as Roger Penrose (1989) later did—how we can see the truth of Gödel-unprovable statements. But Turing specifically rebutted Gödelian attacks, on the same grounds as in his 1947 paper (making no mention of "intuition", or of O-machines). In short, no sort of thinking was excluded.

c. But what about the details?

However, to say that Turing believed the mind to be a Turing machine isn't to say just which Turing machine he believed it to be.

Even if one assumes (with Turing in 1947) that all thoughts are computable, the mind may not compute them in exactly the same way that a particular program or connectionist network does. As Lovelace had long ago pointed out, there are always different ways of instructing (or designing) a machine to do 'the same' computation, different ways of getting from the same input to the same output. So the fact that a machine has achieved a particular (input–output) computation leaves open the question of precisely how the mind does it.

The psychologist—as opposed to the mathematician, the computer scientist, or the philosopher of mind—will want to know *what actually went on* in the mind (or brain) when someone did something. It's not clear that Turing cared about such questions—or perhaps he thought them premature. As we've seen, he sometimes argued that a certain type of behaviour (learning or vision, for example) could in principle be achieved by the general type of mechanism found in the brain and sense organs (Turing 1947*b*). But he didn't ask, and couldn't say, *just which* detailed computations the brain is executing when a person learns a language, recognizes a visual image, or picks up a coffee cup.

In other words, Turing wasn't doing computational psychology—nor even psychological AI. His use of computers (the Bombes, MADM, and ACE) tried to get the machines to do something reliably (code cracking, long division . . .), not to model the way in which people do it. His remarks about simulating the brain were programmatic, not neurophysiologically detailed. And his philosophically provocative arguments about machine intelligence, including his discussion of the Turing Test (see Chapter 16.ii), said almost nothing *specific* about just how human intelligence works.

In short, this was *mind as machine* only in a very general sense. It was necessary for the cognitive revolution, but not sufficient. Turing was propounding what John Searle (1980) would later call "strong AI", the thesis that a suitable AI system would *actually be* intelligent. But he wasn't seriously doing "weak AI", which uses AI programs to clarify and develop specific psychological theories.

His work was approaching psychology, but hadn't quite reached it. Computational psychology proper had to await the intellectual stimulus of McCulloch (and the empirical slant of Allen Newell and Herbert Simon: see Chapters 6.iii and 7.iv.b).

4.iii. The Logical Neurone

If Turing wasn't a psychologist, McCulloch was. Or rather, psychology—in particular, psychiatry—was one of the many interests he followed in his long life (1889–1969). Another was logic. Yet another, neurophysiology. His place in the history of cognitive science is due mainly to his bringing these three disciplines together.

When McCulloch said, "Everything we learn of organisms leads us to conclude not merely that they are analogous to machines but that they are machines", the *mind* was explicitly included:

> Brains are a very ill-understood variety of computing machines. Cybernetics has helped to pull down the wall between the great world of physics and the ghetto of the mind. (1955: 163)

His first clear statement (in the early 1940s) of *mind as machine* relied heavily on Turing, as well as on cybernetics. But he'd been moving in this direction long before encountering either.

a. McCulloch the Polymath

Initially destined for the Church, McCulloch read mathematics at a theological college before studying philosophy, and also psychology, at Yale. During the First World War he joined Yale's Officers' Training Program for the US Navy—of which, more anon (in Chapter 14.v.a). But in 1919 he began to work "chiefly" on logic (W. S. McCulloch 1961*a*: 2).

Later, he qualified in medicine, specializing in neurology and physiological psychology—and worked for some years with brain-injured people and psychiatric patients. He did experimental as well as clinical work, and was a gifted technician, who designed and made many of his own experimental instruments. In addition, he was a keen reader, and writer, of poetry.

He hadn't abandoned logic and mathematics for the brain, but tried to combine them—as we'll see. He spent a year doing graduate work in mathematical physics, and in 1939 was a member of Nicholas Rashevsky's mathematical biophysics group at the University of Chicago—whose pioneering 'manifesto' had just appeared (Rashevsky 1938).

After eleven years as a psychiatrist at Illinois Medical School, during which time he wrote his two most important papers (with Walter Pitts), he moved (in 1952) to MIT's Electronics Laboratory "to work on the circuit theory of brains" (W. S. McCulloch 1961*a*: 3). He applied his knowledge of the brain to robotics at the end of his life, using recent discoveries about the reticular formation in designing an autonomous Mars robot capable of switching appropriately from one basic behaviour pattern to another (Chapter 14.iv.a).

This rich intellectual background enabled McCulloch to play a highly significant role as an intellectual catalyst, bringing countless disparate ideas and people together. Many novel ideas were sparked off by McCulloch himself, who was nothing if not mentally adventurous. Turing, on meeting him in 1949, thought him a "charlatan" (Hodges 1983: 411)—but Turing wasn't known for generosity of spirit towards intriguing, though still half-baked, ideas. (Also, he may have been annoyed by McCulloch's claim that his neural nets were fully equivalent to Turing machines: in fact, they weren't—Papert 1965, p. xviii.)

Despite his frequent hand-waving and his often purple prose, McCulloch was what William James (1907) had called a "tough-minded" thinker. As such, he was a prime mover in founding both streams of mind-as-machine research: formal–computational on the one hand, and cybernetic–probabilistic on the other (see Section v.b, below).

His intellectual breadth was matched by his sociability. He initiated or encouraged many meetings of minds by his open and generous hospitality. For example, the neurophysiologist Jerome Lettvin and the mathematician Pitts lodged with him when they were impecunious students in the early 1940s. And the influential AI researchers Marvin Minsky and Seymour Papert met (for only the second time) in the early 1960s at a vibrant party in his house, on Papert's return from Jean Piaget's laboratory in Geneva (see 12.iii.a).

Besides his important role behind the scenes, McCulloch co-authored three papers now acknowledged as classics in the history of cognitive science. It's indicative of his intellectual breadth that these fit best into three different chapters of this book.

One, discussed below, was a powerful abstract statement of the mind-as-machine hypothesis (W. S. McCulloch and Pitts 1943). Another, just four years later (Pitts and McCulloch 1947), was a neurophysiologically informed connectionist model of fault-tolerant processing (see Chapters 12.i.c and 14.iii.b). And the third, published in his sixtieth year, identified the first 'feature detector' cells in the frog's retina (Lettvin *et al.* 1959: see 14.iv.a). (To be accurate, the first two authors of this four-author paper were the ones who were primarily responsible for it: McCulloch's name, and Pitts', was added as a "courtesy": C. G. Gross, personal communication.)

A fourth paper, less well known but influential nonetheless, outlined a Mars robot based on brain-stem anatomy (Kilmer *et al.* 1969). Very recently, interest in this long-dormant piece has been revived (see 14.v.a).

b. Experimental epistemology

McCulloch often described his work as "experimental epistemology" (e.g. W. S. McCulloch 1961*a*: 3). This may sound like a contradiction in terms, but it's not.

He *didn't* mean that physiology could provide a normative theory of knowledge (or rationality), which is what philosophers typically mean by epistemology. Whether any scientific theory could ever achieve this is highly controversial: it had been declared impossible by Gottlob Frege (see Chapter 2.ix.b), and is still widely rejected (16.vi–viii). (Indeed, it's a main reason, perhaps *the* main reason, for scepticism about cognitive science among philosophers.) Hence the whiff of contradiction in McCulloch's typically provocative phrasing.

What he did mean was that he sought to understand "the physiological substrate of knowledge". Specifically, he was concerned with the mechanisms that enable us to have concepts, and to use them in thinking about (and acting within) the world, and possible worlds.

He thought about such issues in broadly Kantian terms. That is, he distinguished innate structuring principles from specific environmental stimuli (see 9.ii.c and 14.iii.b). And besides declaring loyalty to Immanuel Kant, he saw himself as engaged in the same general enterprise as Hermann von Helmholtz (2.vii.c).

Two colleagues of the 1930s, too, influenced him deeply because of their general approach to knowledge. Both considered the wood as well as the trees, trying to relate the detailed data to the overall functions of the mind/brain. One was the psychiatrist Eilhard von Domarus, the other the neurophysiologist Joannes Dusser de Barenne (1885–1940), a Dutchman working at Yale.

Von Domarus had been advised by a philosopher (Filmer Northrop) for his doctoral thesis of 1934, which related the "logic" (the language and grammar) of schizophrenia to its neurophysiology. As for de Barenne, he also tried to relate high-level functions to their neurophysiological base. He collaborated with McCulloch on experiments to identify functional pathways in the monkey's brain (Dusser de Barenne and McCulloch 1938).

McCulloch's guiding epistemological theme was well established by the time he was 17. In old age, he recalled:

> In the fall of 1917, I entered Haverford College with two strings to my bow—facility in Latin and a sure foundation in mathematics. I "honored" in the latter and was seduced by it. That winter [the Quaker philosopher] Rufus Jones called me in. "Warren", said he, "what is thee going to be?" And I said "I don't know." "And what is thee going to do?" And again I said, "I have no idea; but there is one question I would like to answer: What is a number, that a man may know it, and a man, that he may know a number?" He smiled and said, "Friend, thee will be busy as long as thee lives." (W. S. McCulloch 1961*a*: 2)

Before this conversation took place, McCulloch had already been enthused by the mathematical logic of Russell and Whitehead (1910). Ironically, given its enormous later influence on logic and computing, the first publication of *Principia Mathematica* had to be subsidized both by the Royal Society and by the authors themselves (Monk 1996: 194). The *Principia* was in part a development of Frege's work (see Chapter 2.ix.b). It was heavy going in more senses than one: there were 1,929 pages, in three volumes (appearing between 1910 and 1913). And initially, as the need for financial subsidy suggests, not everyone could see the point.

But the young McCulloch would. Indeed, he was entranced by it. Nor was he to be the only one: much later, both Pitts and David Marr were bowled over by it while still at high school (see below, and 14.v.b). Moreover, it had an honoured place on Simon's bookshelf—and inspired his Logic Theorist, one of the very first AI programs (10.i.b). Besides all that, it was the logicians' bible for many years, and the foundation of important movements in philosophy. (Not bad, for a privately published tome!) Without the *Principia*, as we'll see, cognitive science would have been very different.

The book wasn't the first mathematical treatment of symbolic logic. That place was taken by Whitehead's earlier work *A Treatise on Universal Algebra* (in 1898). His novel approach to logic had excited the young Russell greatly. Not least, it liberated him from his commitment to Hegelian idealism:

> [Whitehead] said to me once "You think the world is what it seems like in fair weather at noon-day. I think it is what it seems like in the early morning when one first wakes from deep sleep." I thought his remark horrid, but could not see how to prove that my bias [towards clarity] was better than his. At last he showed me how to apply the technique of mathematical logic to his vague and higgledy-piggledy world, and dress it up in Sunday clothes that the mathematician could view without being shocked. (Russell 1956*a*: 41)

McCulloch, here, would have empathized with Russell.

(The source of Whitehead's "horrid" remark was his commitment to process philosophy, of which he was/is perhaps the outstanding proponent: Sibley and Gunter 1978. This sees matter as consisting essentially in self-organizing processes, not inert stuff subject to external influences. Recently, such ideas have been gaining ground in certain areas of cognitive science—but more orthodox colleagues still think them fairly horrid: see 15.viii.c–d and ix, and 16.x.a.)

In particular, as implied by his remark to his Quaker tutor, McCulloch accepted Russell's Fregean definition of a number as "the class of all of those classes that can be put into one-to-one correspondence to it".—But how could this highly abstract and general definition be understood by a finite human brain?

How can a material brain compute numbers, or do formal logic, in the first place? And how can it enable us to understand any particular number (2, 7, 27, etc.)? After all, for any given number we can't actually comprehend all the classes that could be put in one-to-one correspondence with it. It was said that we know "the rule of procedure by which to determine it on any occasion". But that rule of procedure, one-to-one matching, takes for granted our ability to recognize that different things fall into the same class (cats, cups, carpets). How do we manage to do that? And how can we recognize sets containing up to six things by visual perception, while larger numbers require (symbolic) counting?

As his tutor had predicted, this many-sided puzzle kept McCulloch busy for the rest of his life. And all three of his "classics" were part of his solution.

The first, published in the early 1940s, proved that neural networks are capable of computing any number, or logical proof, that a human can compute (see below). Next, in 1947, he offered a neurophysiological theory explaining both "How We Know Universals" (how we assign different, and slightly differing, things to distinct classes) and how the brain can recognize patterns reliably, despite faulty components and noisy data (see Chapter 12.i.c). As he later put it, this paper explained "[what is] a man, made of fallible neurons, that [he] may know [number] infallibly" (W. S. McCulloch 1961*a*: 18). These two papers together covered both deduction and induction, for universals can be projected as "expected regularities of all future experiments, [so the brain] can frame hypotheses in order ultimately to disprove them" (W. S. McCulloch 1948: 142).

Then, in the 1950s, the four-author paper 'What the Frog's Eye Tells the Frog's Brain' (Lettvin *et al.* 1959) showed that single cells, coding specific sensory features, can be involved in various types of ecologically relevant generalization (Chapter 14.iv.a). McCulloch (1965, p. v) later described that paper, co-authored with a mathematician and two neurophysiologists, as "our first major step in experimental epistemology".

c. Enthused by logic

Much as logic came before psychology for the young Jean Piaget (5.ii.c), so it did for McCulloch. His general approach, in his youth, was a psychophysiological version of logical atomism (Russell 1918–19; Wittgenstein 1922).

This philosophical movement, one of whose leading proponents was the early Ludwig Wittgenstein (1889–1951), applied Russell's mathematical logic to language, mind, and metaphysics. It saw natural language and propositional thought as grounded in logic, or as an approximation to the ideal language, logic. As a general position, this wasn't new: it had been held 200 years before by Gottfried Leibniz, for example (see Chapter 2.ix.a). But more powerful logical tools were now available: hence the excitement.

It was assumed that class concepts could be analysed in terms of necessary and sufficient conditions. And psychological verbs were thought to be definable in truth-functional (extensional) terms. As for everyday words such as *and*, *if–then, some*, and *the*, these were reduced to the terms of Russell's propositional and predicate calculus.

There were many prima facie problems here. For one thing, the truth of psychological statements such as *Mary believes that p*, or *John hopes that q* doesn't depend on, and nor does it guarantee, the truth of *p* or *q*. How, then, could such statements be dealt with by this type of logic?

For another, even seemingly boring English words like the four mentioned above weren't so simple as these logicians were suggesting. This had been pointed out soon after the publication of *Principia*, by Russell's colleague George Moore (1873–1958). The logicist view, said Moore (1919), was that the sentence *If Holmes is playing his violin, then Watson is writing about their adventures* means the same as *It is not the case both that Holmes is playing his violin and that Watson is not writing about their adventures*. But this isn't what we normally mean by "if–then": in ordinary English, some sort of meaningful/causal relationship between the two expressions is assumed. Or again, the logicist view that *Some tame tigers do not growl* means the same as *There is at least one tame tiger who does not growl* is problematic (G. E. Moore 1936). For we'd normally expect there to be at least two non-growling tame tigers.

The latter difficulty was easily ignored: *one*, *two*, *three* . . . didn't seem to matter much. But the difference between the "if–then" of logic ("material implication") and the "if–then" of English was more embarrassing. Even today, nearly a century after *Principia*, the relation between these two isn't clear. Nor is the logic of psychological verbs. (These problems set tough challenges for natural language processing, and for logicist AI in general: see 10.iii.e and 13.i.)

Despite such embarrassments, however, logicism flourished in Anglo-Saxon philosophy. This was largely because it was seen as a liberation from the ("horrid") holistic idealism that had been dominant earlier in the century (Ewing 1934; Boucher 1997). Various British versions of the spiritual/religious philosophies sketched in Chapter 2.vi were hugely influential, notably those of Thomas Green (1836–82), Francis Bradley (1846–1924), and John McTaggart (1866–1925). Even Russell, as a young man, had been seduced by the idealist movement.

A corollary of the new logic was that all aspects of meaning were to be made explicit, not left implicit. For instance, Russell claimed that sentences containing definite descriptions, such as "the present king of France", unambiguously assert (not just presuppose) the existence of one and only one thing fitting the description (Russell 1905). And as in language, so in metaphysics. The fundamental structure of the world was seen in terms of atomic facts modelled on logical primitives and related by truth-functional connectives.

Logical atomism was widely welcomed by philosophers with empiricist leanings, who were eager to escape the then prevailing idealism. Influential versions of it still persist today, notably in the empiricism of Willard Quine (1953*a*, 1960) and in the logicist approach to AI and cognitive science: see Chapters 10.iii.e, 13.i, and 16.iv.c–d. The centenary of Russell's 1905 paper was even marked by a special conference, organized by the universities of Padua and Bologna: 'One Hundred Years of "On Denoting"'. (However, the circle turns: idealism is much more fashionable now than it was at mid-century—see 16.vi–viii.)

After the early years, however, its assumption that the structure of world and language mirror the logical structures of *Principia Mathematica* would be increasingly questioned. At mid-century, Moore's doubts about "if–then" were joined by critiques of other specific aspects, including Russell's theory of descriptions (Strawson 1950). Most of these skirmishes were inspired by the later Wittgenstein's (1953) repudiation of the logicist approach in general—including his own previous work—as fundamentally misguided (Chapter 9.x.d). And around 1970, Wittgenstein's ideas were used by Herbert Dreyfus (1965, 1972) as the basis of an attack on the guiding assumptions of AI—and of McCulloch (see Chapters 11.ii.a–c and 16.vii.a).

In the first third of the century, these Wittgensteinian critiques lay far in the future. Even Moore's worries about "if–then" hadn't been published when the young McCulloch first decided to apply logic to language, as well as to number—and to psychology and neurophysiology, too.

In 1920 he started work on supplementing the predicate calculus with a logic of verbs that would cover both knowledge and action (W. S. McCulloch 1964: 391 ff.). But he found it too difficult. He distinguished transitive from intransitive (including reflexive) verbs, verbs of perception from verbs of motion, and verbs of action from verbs of sentiment (what Gilbert Ryle would later call "dispositional" verbs:

16.i.c). These distinctions, however, and their different temporal implications, were too subtle and complex to be easily formalized. Moreover, they weren't reflected in the subject–predicate form of English sentences.

In disgust, McCulloch gave up. Turning from verbs to propositions, he tried to develop a logical atomism of the mind/brain. In order to do this, he sought to define "a simplest psychic act", or "psychon".

A psychon "was to be to psychology what an atom was to chemistry, or a gene to genetics" (W. S. McCulloch 1964: 392). But, unlike atom and gene, it was an event, with a place in time and a temporal history. As a psychic unit, it had to be intrinsically capable of connecting knowledge and action (perception and movement, or even belief and inference)—much as a gene, as a hereditary unit, is intrinsically capable of connecting generations. It had to have a semiotic, or meaningful, aspect: that is, it had to correspond to a proposition. And, for McCulloch, it had to find some equivalent in the brain.

The then recent neurone theory (2.viii.c) offered a suitable candidate:

> In those days the neuronal hypothesis of Ramón y Cajal and the all-or-none law of axonal impulses were relatively novel, but I was overjoyed to find in them some embodiment of psychons. (W. S. McCulloch 1964: 392)

> In 1929 it dawned on me that these events [combinations of psychons] might be regarded as the all-or-none impulses of neurons, combined by convergence upon the next neuron to yield complexes of propositional events. During the nineteen-thirties [influenced by friends including von Domarus, Northrop, and Dusser de Barenne], I began to try to formulate a proper calculus for these events . . . (W. S. McCulloch 1961*a*: 9)

Chains of psychons were essentially comparable to compound propositions, which could in principle be mapped in logical terms. McCulloch tried to put this principle into practice:

> [In teaching physiological psychology] I used symbols for particular neurons, subscripted for the time of their impulse, and joined by implicative characters to express the dependence of that impulse upon receipt of impulses received a moment, or synaptic delay, sooner. (W. S. McCulloch 1964: 393)

Throughout the 1930s, then, McCulloch was already thinking of "the response of any neuron as factually equivalent to a proposition which proposed its adequate stimulus". And he was already trying to express the behaviour of complicated nets in a notation similar to propositional logic (W. S. McCulloch and Pitts 1943: 21).

But his nascent neural calculus couldn't deal with circular (feedback) networks. These were difficult to describe in formal terms, because excitation can continue reverberating around a circuit for an indefinite period of time. Moreover, McCulloch was troubled by the seeming paradox that, if something is negated as it goes around the loop, then an input must be equal to its negation (Arbib 2000: 200).

Circular networks were a key concern of Rashevsky's group. They'd been postulated, for instance, by McCulloch's psychiatrist colleague Lawrence Kubie (1930), when he was working with Charles Sherrington (see Chapter 2.viii.d). He saw them as the neurological basis of disorders of memory, including the "repetitive core" of psychoneuroses (Kubie 1941, 1953). McCulloch was strongly influenced by Kubie's ideas on circular memories (but would later vilify his psychoanalytic explanation of neurosis, as we'll see).

Others had made similar suggestions. For instance, the notion that *normal* memory, also, might depend on "self re-exciting" circuits had been put forward in 1922 by Rafael Lorente de No (1902–90), who gave anatomical and then experimental evidence for them later (Lorente de No 1933*a*, 1934, 1938). And "reverberative" chains of neurones were being discussed in the 1930s, too (e.g. Ranson and Hinsey 1930). In short, talk of looping circuits was prominent among neurophysiologists.

All these suggestions, however, were beyond the scope of McCulloch's calculus at the time. More generally, his logical notation couldn't represent feedback—the core notion of cybernetics and, under other names, of reflexology and Sherringtonian neurophysiology. Circular causality in general required cyclic (looping) nets.

d. The young collaborator

It was Pitts (1923–69) who provided the crucial insight, and the mathematical power, needed to deal with circular nets. The insight was that the seeming paradox could be avoided if *delay* was represented in the theory. And the power was evident in the use of an arcane logical formalism to express it (see below).

McCulloch and Pitts, and assorted guests (including Lettvin), spent "endless evenings sitting around the McCulloch kitchen table trying to sort out how the brain worked, with the McCullochs' daughter Taffy sketching little pictures which later illustrated [the key paper]" (Arbib 2000: 200). Pictures aside, it's highly doubtful whether McCulloch could ever have achieved the theory unaided.

Pitts was a close friend of Lettvin, who lived alongside him for some time in McCulloch's house. Pitts was largely self-educated, and had been accepted as a member of Rashevsky's group when he was only 14. (He was still only 18 when he and McCulloch published their 'Logical Calculus of Ideas'.) He'd already devoured *Principia Mathematica*, which he found in a library stack when he was 12. He'd even been invited to England by Russell, as a result of sending him some criticisms.

This was typical: Pitts acquired detailed knowledge of new domains extraordinarily quickly, and had an exceptional ability to see unsuspected links between—and logical faults in—other people's ideas. McCulloch used to tell a story (apocryphal? embellished?) of how one of these critiques came about:

> Walter Pitts was forced to drop out of high school by his father, who wanted him to go to work and earn money. Rather than do this, young Pitts ran away from home and ended up in Chicago, penniless. The fifteen-year-old boy spent a lot of time in the park, where he met and began to have conversations with an older man he knew only as Bert. When Bert detected the boy's interests, he suggested that young Pitts read a book that had just been published by a professor at the University of Chicago by the name of Rudolf Carnap. Pitts did, and showed up at Carnap's office. "Sir", he said, "there's something on this page which just isn't clear." Carnap was amused, because when *he* said something wasn't clear, what he meant was that it was nonsense. So he opened up his newly published book to where young Pitts was pointing, and sure enough, it wasn't clear; it was nonsense. *Bert turned out to be Bertrand Russell.* (McCorduck 1979: 73–4; final italics added)

Irrespective of the reliability of this particular story, there's no doubt whatever about Pitts' searching intelligence. Norbert Wiener (1894–1984) declared: "He is without question the strongest young scientist whom I have ever met" (quoted in Heims 1991: 40). Given that Wiener himself had been mixing with fine minds ever since his entry to Tufts University at the age of 11, this was no faint praise.

Admittedly, Pitts' strengths didn't include proof-reading. The first edition of Wiener's *Cybernetics* was riddled with errors, because it had been typeset by a small French press: MIT Press had come in at the last minute, insisting on co-publication because Wiener was on 'their' MIT faculty (M. A. Arbib, personal communication). Pitts was supposed to have checked it, but it even included pages printed out of order.

McCulloch's interest, however, was in his ability to cope with proofs of a different kind. Although he was very different from the highly sociable polymath McCulloch, Pitts' wide interests—and exceptional logical acumen—soon made him a valued collaborator.

He was to become a tragic figure, after a personal betrayal in 1952. Wiener suddenly turned violently against McCulloch, and cut himself off from McCulloch's associates too (14.vii.a). Having regarded Wiener as a father figure, the psychologically fragile Pitts ceased his research. He burnt his manuscript (he'd been working on probabilistic and three-dimensional neural networks), eventually succumbed to delirium tremens, and "became a ghost long before he died" (Lettvin 1999).

In later years, McCulloch often declared that Pitts had taught him more than he'd ever taught Pitts. One of the things that Pitts taught McCulloch was Leibniz's work on the mechanization of logic. Leibniz had seen logic and arithmetic as essentially connected, and mechanizable too (2.ix.a). He'd even shown that a calculator using binary numbers (which couldn't be built at the time) was in principle capable of doing logic. Given his prior commitment to Russell's views on logic, McCulloch was highly receptive.

e. Mind as logic machine

Their joint paper 'A Logical Calculus of the Ideas Immanent in Nervous Activity' (W. S. McCulloch and Pitts 1943) integrated three powerful twentieth-century ideas: the propositional calculus, Turing machines, and neuronal synapses. (According to McCulloch, it was Turing's 1936 paper which had initially inspired them— Hodges 1983: 252 n.) This brief text was an abstract manifesto for computational psychology, and—as soon became apparent—for both symbolic and connectionist AI.

The paper described networks of units modelled on what was then known about real neurones. But the units were highly simplified. A McCulloch–Pitts neurone was an all-or-none device, which fired only when its threshold was reached. The threshold was defined in terms of the number of excitatory impulses coming in from other neurones. Several known neural complexities were deliberately ignored. For instance, the activity of any inhibitory synapse in their abstract model absolutely prevented excitation of the (modelled) neurone at that time, whereas real inhibition isn't so simple.

McCulloch and Pitts used a novel notation to define specific types of net. Their formulae didn't denote timeless logical relations, as Russell's did, but events in time (compare: psychons). They called them TPEs, or temporal propositional expressions, and their notation showed the number of synaptic delays, or relays, involved.

They didn't attempt to represent precise time intervals—partly to avoid excessive complication, and also because neurophysiologists knew so little about such matters. Accordingly, their calculus applied only where "the exact time [needed] for impulses to pass through the whole net is not crucial" (W. S. McCulloch and Pitts 1943: 24). This blindness to timing was carried over into the digital computer by von Neumann, and still characterizes most connectionist AI (see 14.ix.g).

This notation wasn't merely a new manner of speaking, but a new *calculus*. The authors used it to prove various theorems about the computational power of their neural nets. For example:

> The behaviour of any non-cyclic net can be expressed in terms of the propositional calculus.
>
> Every logical function of the propositional calculus is realizable by some net.
>
> All nets are recursively definable out of four basic types: precession (comparable to identity), disjunction, conjunction, and negation (see items a,b,c,d in Figure 4.1).
>
> Every net computes a function that is computable by some Turing machine.
>
> And a universal Turing machine can compute anything that is computable by a McCulloch–Pitts net. (The converse isn't true: see below.)

This work was an early instance of what John McCarthy called "meta-epistemology" (see Chapter 10.i.g). Considered purely as mathematical logic, it was imperfect. (For example, we saw above that McCulloch–Pitts networks *are not* fully equivalent in computing power to Turing machines, and other flaws were identified by Stephen Kleene: 1956.) This potentially embarrassing fact was announced in the opening pages of the hugely influential volume on *Automata Studies*—but it hardly affected the authors' reputation. Having *tried* to use logic to address neurological or psychological questions was more significant than managing to produce utterly flawless proofs. Similarly, mathematicians' proofs may be tidied up by other mathematicians, but recognized nonetheless as important achievements—and as we saw in Section i.c, even a *non-proof*, such as the Church–Turing thesis, may be hugely important.

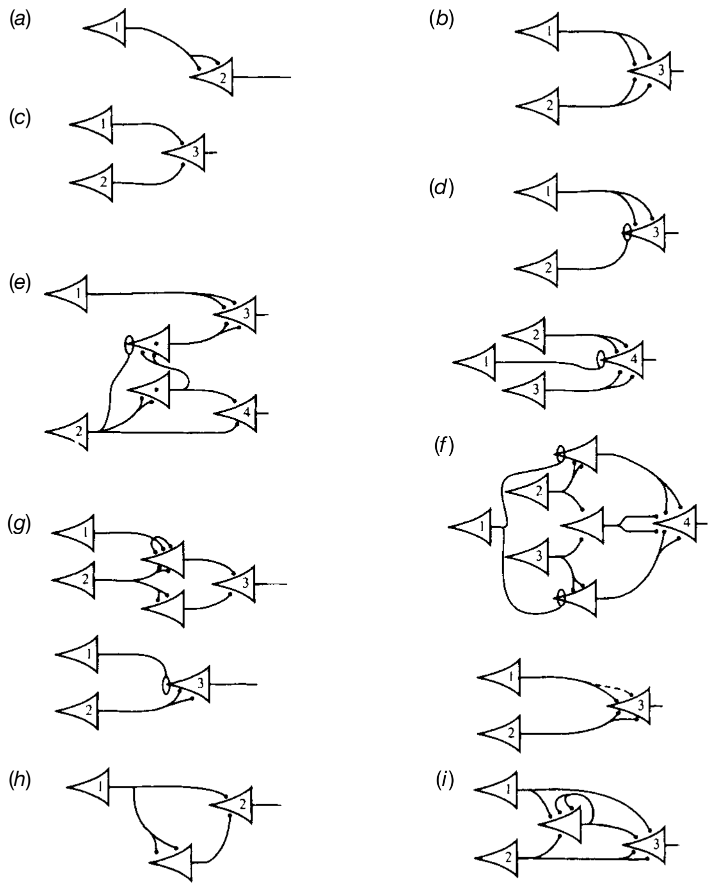
However, networks—unlike universal Turing machines—are finite. A McCulloch–Pitts neurone isn't a purely mathematical abstraction, but a simplified representation of a real thing. So, for any class of logical function, we need to calculate just what type of net could actually compute it. It's not enough to say that we know that *some* net could do so. Indeed, until we know what sorts of net can generate a psychological phenomenon, we don't really know what that phenomenon is: "If our nets are undefined, our facts are undefined" (W. S. McCulloch and Pitts 1943: 37).

The 'Logical Calculus' paper was hugely ambitious. Logic and language were merely part of it. As they put it: "if any number [any function] can be computed by an organism, it is computable by these definitions, and conversely" (p. 37). In other words, the authors' theoretical reach extended across the whole of psychology. For instance:

* They argued that learning could be embodied (in any net) as a threshold change resulting from the simultaneous excitation of two neurones. This idea was a mid-twentieth-century version of suggestions made in the 1740s by David Hartley (see Chapter 2.x.a).

* They proved that, in cyclic nets, threshold change can be mimicked by loops of fixed-threshold neurones.

* They said that purpose was grounded in cyclic networks that reduce the difference between two afferents. This was their way of expressing the cybernetic idea that goal-seeking behaviour uses feedback to reduce the distance from the goal (Rosenblueth *et al.* 1943).



(a) $N_2(t) \,.\equiv.\, N_1(t-1)$

(b) $N_3(t) \,.\equiv.\, N_1(t-1) \vee N_2(t-1)$

(c) $N_3(t) \,.\equiv.\, N_1(t-1) \,.\, N_2(t-1)$

(d) $N_3(t) \,.=.\, N_1(t-1) \,.\sim N_2(t-1)$

(e) $N_3(t) :\equiv: N_1(t-1) \,.\vee.\, N_2(t-3) \,.\sim N_2(t-2)$
$N_4(t) \,.\equiv.\, N_2(t-2) \,.\, N_2(t-1)$

(f) $N_4(t) :\equiv: \sim N_1(t-1) \,.\, N_2(t-1) \vee N_3(t-1) \,.\vee.\, N_1(t-1) \,.\, N_3(t-1) \,.\, N_1(t-1)$
$N_4(t) :\equiv: \sim N_1(t-2) \,.\, N_2(t-2) \vee N_3(t-2) \,.\vee.\, N_1(t-2) \,.\, N_2(t-2) \,.\, N_3(t-2)$

(g) $N_3(t) \,.\equiv.\, N_2(t-2) \,.\sim N_1(t-3)$

(h) $N_2(t) \,.\equiv.\, N_1(t-1) \,.\, N_1(t-2)$

(i) $N_3(t) :\equiv: N_2(t-1) \,.\vee.\, N_1(t-1) \,.\vee.\, N_1(t-1) \,.\, (Ex)t-1 \,.\, N_1(x) \,.\, N_2(x)$

FIG. 4.1. Recursively defined nets for computing propositional functions. The four basic nets are (*a*) precession (identity), (*b*) inclusive disjunction, (*c*) conjunction, and (*d*) conjoined negation. In the diagrams, the threshold is a two-knob input; inhibition is shown as a circle at the tip of the inhibited neurone. In the TPE formulae, the neurone c_i is always marked with the numeral i upon the body of the cell, and the corresponding action is denoted by N with i as subscript. Adapted with permission from McCulloch and Pitts (1943: 36–7)

* And they attributed hallucinations and delusions in psychiatric patients to alterations in someone's normal networks whose causes are other than he/she supposes.

In short, they saw the mind as a Turing machine. And they weren't thinking only of cognition. For *all* psychological processes, "the fundamental relations are those of two-valued logic" (p. 38). Even in psychiatry, they said, "Mind no longer goes 'more ghostly than a ghost'."

This remark was a tacit reference to Sherrington's recent confession, in the 1937–8 Gifford Lectures (see 2.vii.b), that his neurophysiology couldn't explain mind—by which he didn't just mean consciousness:

> Mind, for anything perception can compass, goes therefore in our spatial world more ghostly than a ghost. Invisible, intangible, it is a thing not even of outline; it is not a "thing". It remains without sensual confirmation, and remains without it for ever. All that counts in life. Desire, zest, truth, love, knowledge, "values", and, seeking metaphor to eke out expression, hell's depth and heaven's height. Naked mind . . . It will sit down and watch life acquiescent, or on the other hand take life and squeeze it like an orange. (Sherrington 1940: 266)

All these phenomena, and 'madness' too, were now grist for their Turing-neuronal mill.

f. Initial reception

To say that the 1943 paper is a classic isn't to say that it was widely hailed as important at the time. On the contrary, it initially faced "a hostile or indifferent world" (W. S. McCulloch 1965, p. xvii). The authors themselves weren't surprised: they'd feared that the paper might never be noticed, or even published (Heims 1991: 20).

One reason for this was its fierce logical technicality, unfamiliar to psychologists and physiologists alike. Even the expert mathematician Michael Arbib (1940–) found it "almost impenetrable" (J. A. Anderson and Rosenfeld 1998: 217). Indeed, Arbib abandoned his attempt to check/correct the original proofs, finding it easier to derive new ones instead (Arbib 1961).

The impenetrability was due to the paper's rhetorical form (see Chapter 1.iii.h). This may seem surprising, for anyone who's read McCulloch knows that he was a fine, if flowery, wordsmith. But here, he needed to be a mathsmith too. By most people's standards, he was—but not by Pitts'.

Unfortunately, Pitts persuaded him that they should express their theorems in a rebarbative notation based on that of his ex-teacher the logical positivist Rudolf Carnap (1891–1970) : see Carnap (1934). Technicality was inevitable, of course. But this formalism was unnecessarily difficult, and the fact that it was supplemented by symbols drawn from their beloved *Principia Mathematica* didn't really help. McCulloch later admitted that Rashevsky's group in general was "under the spell" of their Chicago colleague Carnap, and "employed his terminology, although it was not most appropriate to our postulates and hypotheses" (W. S. McCulloch 1964: 393).

The notational difficulty obscured the paper's real-world relevance. Twenty years later, McCulloch complained about highly abstract interpretations of it, protesting that:

> [Our] temporal propositional expressions are events occurring in time and space in a physically real net. The postulated neurons, for all their over-simplifications, are still physical neurons as truly as the chemist's atoms are physical atoms. (W. S. McCulloch 1964: 393)

At the time, however, it was all too easy for people to assume that the new neural calculus was merely a mathematical game.

That's assuming that they even encountered it (Chapter 1.iii.h, again). The paper was published in a relatively obscure journal, Rashevsky's *Bulletin of Mathematical Biophysics*. This didn't achieve a high profile, even though it did raise interest in biophysics, because the Chicago group were not only highly abstract in their interests, but socially isolated too. They rarely got involved with the meetings of the newly founded Mathematical Biophysics Society, and in particular, they didn't interact much with experimenters working on the nervous system—or on metabolism in general (Harold Morowitz, personal communication). It's understandable, then, that most experimental neurophysiologists didn't regard the abstract arguments of the McCulloch–Pitts paper as relevant to their concerns.

Another reason for hostility was the paper's imperialistic claim that "to psychology, however defined, specification of the net would contribute all that could be achieved in that field" (W. S. McCulloch and Pitts 1943: 37). Most experimental psychologists in mid-century America were rampantly behaviourist (see Chapter 6). Since they not only avoided reference to meanings and thought processes, but also treated the nervous system as a black box, they were hardly likely to welcome McCulloch and Pitts' definition of psychology.

As for clinical psychologists and psychiatrists, their therapeutic methods had nothing to do with detailed neural circuitry. 'Talking cures' ignored the brain entirely. And even physical methods, such as lobotomy and electro-convulsive therapy, weren't directed to specific neural networks. On that count, the paper seemed irrelevant to many clinicians—even those who shared McCulloch's general view that psychiatry requires physiological understanding and therapies (W. S. McCulloch 1949).

Moreover, the paper argued that—because of disjunction and circularity—retrospective (as opposed to predictive) description of neural nets is in principle impossible (W. S. McCulloch and Pitts 1943: 35, 38). It followed, the authors said, that historical diagnoses of mental illness are unavailable, and would be therapeutically irrelevant anyway. Clinicians committed to 'child-based' theories wouldn't look kindly on that.

Nor did McCulloch look kindly on most of *them*. Ten years later, he would deliver a vitriolic attack on psychoanalysis (W. S. McCulloch 1953; see also Heims 1991: 115–46). He didn't mince his words. He accused Freudians in general of intellectual futility, dishonesty, and greed.

His cybernetic colleagues were less florid in denigrating Freudian influences in psychiatry. But most of them shared his suspicion, even his contempt. This led to some searing interchanges at interdisciplinary meetings set up to bring physics and psychology together (Dupuy 2000: 85). Even McCulloch's old friend Kubie, for whom Freud had now joined Sherrington as a major influence, received some taxing questions—from McCulloch and others—about how to measure the emotions he was concerned with in his psychoanalytic work (Kubie 1953: 62–72).

What eventually brought the paper to the attention of psychologists capable of appreciating it was its role in the design of the modern computer (which also explains why a copy fetched $6,000 some sixty years later: J. M. Norman 2005). As McCulloch himself admitted, "as far as biology is concerned, it might have remained unknown" had it not been picked up by von Neumann and used to design computers (W. S. McCulloch 1961*a*: 9).

4.iv. The Functionalist Neurone

McCulloch and Pitts' first joint publication made no reference to computer modelling, nor to control engineering either—both to become key influences on cognitive science (see Chapter 1.ii.a). Nevertheless, it was soon seen as the pivotal moment in the foundation of the field. The person who made this possible was von Neumann, wearing his hat as a computer designer.

a. From calculus to computer

The 'Logical Calculus' paper, in effect, had provided an abstract manifesto for computational psychology and AI (both symbolic and connectionist). It was "abstract" in two senses.

First, it didn't mention computer modelling. A few years later, McCulloch would make the declaration quoted at the outset of this chapter: "Our adventure is actually a great heresy. We are about to conceive of the knower as a computing machine." But he followed it by this:

> My problem *differs* from that of the men who build computing machines *only in this*—that I am confronted by the enemy's machine [i.e. the brain]. I have not been told and must learn what it is, what it does, and how it does it. (W. S. McCulloch 1948: 144; italics added)

But his 1943 paper, although it relied on the concept of a Turing machine, made no reference to man-made Turing-equivalent computers.

This is hardly surprising, since no such machines existed at the time—or rather, none was known outside the wartime secrecy of Bletchley Park (3.v.d). McCulloch was familiar with early cybernetic analogies between control in animals and machines. But that approach—rooted in physiology, not logic—didn't focus on language or reasoning as such (although it sometimes referred to systems of belief: see Section v.e, below). The 1943 paper, therefore, *didn't* raise the possibility that a man-made machine might compute in the way it described.

However, the paper's potential for computer design was very soon recognized by von Neumann. He was alerted to it by the cyberneticists Wiener and Julian Bigelow (1906–2003). Both men were members of Rashevsky's biomathematics group, and Bigelow would later be the chief engineer for the Princeton IAS machine (see Chapter 3.v.e).

In particular, von Neumann was impressed by McCulloch and Pitts' use of binary, or Boolean, elements. This contrasted with the early electronic calculator ENIAC, already designed by his colleagues Presper Eckert and John Mauchly (3.v.c and e), which was a decimal device. He was impressed also by the definitions of basic computational mechanisms—conjunction, disjunction, negation—which, he realized, could be embodied electronically as logic gates. (They'd already been embodied electromechanically by Konrad Zuse. But no one outside Germany knew that: see 3.v.a.)

That possibility seems not to have occurred to McCulloch and Pitts. If the logic enthusiast McCulloch was aware of it, he hadn't discussed it in any detail with his young collaborator. This is evident from Wiener's report of a conversation he had with Pitts in 1943:

> At that time Mr. Pitts was already thoroughly acquainted with mathematical logic and neurophysiology, but had not had the chance to make very many engineering contacts. In particular, he was not acquainted with Dr. Shannon's work [on information theory], and he had not had much experience of the possibilities of electronics. He was very much interested when I showed him examples of modern vacuum tubes and explained to him that these were ideal means for realizing in the metal the equivalents of his neuronic circuits and systems. From that time, it became clear to us that the ultra-rapid computing machine, depending as it does on consecutive switching devices, must represent an almost ideal model of the problems arising in the nervous system. The all-or-none character of the discharge of the neurons is precisely analogous to the single choice made in determining a digit on the binary scale, which more than one of us had already contemplated as the most satisfactory basis of computing-machine design. (Wiener 1948: 14)

The "more than one of us" included Wiener himself. In 1940, as he recalled in his introduction to *Cybernetics*, he'd outlined what we'd call a digital computer (though with no stored program). Specifically, he'd sent a letter to Vannevar Bush, the inventor of the differential analyser, enclosing an unpublished 'Memorandum on the Mechanical Solution of Partial Differential Equations' (J. M. Norman 2004: 15).

More to the point, the "more than one of us" also included von Neumann. He had immediately seen the relevance of McCulloch and Pitts' work for the design of computing machinery, and his proposal for the EDVAC cited them explicitly:

> Every digital computing device contains certain relay-like *elements*, with discrete equilibria. Such an element has two or more distinct states in which it can exist indefinitely . . . In existing digital computing devices various mechanical or electrical devices have been used as elements: [wheels, telegraph relays] . . . and finally there exists the plausible and tempting possibility of using vacuum tubes . . . It is worth mentioning, that the neurons of the higher animals are definitely elements in the above sense . . . Following Pitts and McCulloch [i.e. McCulloch and Pitts 1943], we ignore the more complicated aspects of neuron functioning . . . It is easily seen, that these simplified neuron functions can be imitated by telegraph relays or by vacuum tubes. (von Neumann 1945: 359–60)

A few paragraphs later, he pointed out that when vacuum tubes are used by engineers as current valves, or gates, they function as all-or-none devices. And he continued:

> Since these tube arrangements are to handle numbers by means of their digits, it is natural to use a system of arithmetic in which the digits are also two-valued. This suggests the use of the binary system. The analogs of human neurons, discussed [above, in reference to McCulloch and Pitts], are equally all-or-none elements. It will appear that they are quite useful for all preliminary, orienting considerations on vacuum tube systems. It is therefore satisfactory that here too, the natural arithmetical system to handle is the binary one. (von Neumann 1945: 5.1)

The architecture of the most widely used modern computer was designed accordingly. McCulloch and Pitts' brain-inspired definitions of logical functions were embodied in electronic circuitry as and-gates, or-gates, and the like.

(Von Neumann wasn't the only one to adopt a version of McCulloch and Pitts' notation in designing the logic gates for his computer. Turing was already doing so too, in his design for the ACE computer: see 3.v.c. In fact, he extended their original notation significantly—Hartree 1949: 97, 102.)

This engineering development highlighted McCulloch and Pitts' insight that their networks (like Babbage's Analytical Engine) could compute propositions as well as numbers. In short, von Neumann's use of their ideas made it even clearer than it was already (in regard to MADM and the other early computers discussed in Chapter 3.v) that computers aren't mere number-crunchers, but general-purpose symbol-manipulating machines. That's why they seemed so promising, to early cognitive scientists interested in simulating (and explaining) thought.

b. Function, not implementation

The 1943 paper was abstract in a second sense, also. Although determinedly materialist ("Mind no longer goes 'as ghostly as a ghost' "), it ignored the physical details.

McCulloch–Pitts neurones represented real things, as we've seen, and were part-inspired by research on the brain. But those "real things" could be of many different types. The defining properties of the basic computational units had been deliberately chosen so as to avoid commitment with respect to current neurophysiological controversies.

For instance, neuroscientists disagreed about whether the mechanism of inhibition in real brains is direct or indirect. McCulloch and Pitts showed that this was irrelevant for their purposes, because they ignored precise time intervals. Similarly, they argued that the functional effects of experimentally observed facilitation, extinction, and learning could be represented without knowing the underlying electrochemical mechanisms (W. S. McCulloch and Pitts 1943: 20–2).

It followed, though they didn't explicitly say so, that the material embodiment of a McCulloch–Pitts neurone is irrelevant. That is, their computational psychons allow "multiple realizability" (Chapter 16.iii.b).

Von Neumann agreed. Indeed, his EDVAC proposal attracted scepticism from some engineers, precisely because it focused on the logical properties of abstractly defined computing units instead of detailing the hardware issues. Von Neumann explicitly refused to get bogged down in discussions of the switching elements *considered as electronic mechanisms*:

> The ideal procedure would be to treat the elements as what they are intended to be: as vacuum tubes. However, this would necessitate a detailed analysis of specific radio engineering questions at this early stage of the discussion, when too many alternatives are still open, to be treated all exhaustively and in detail. Also, the numerous alternative possibilities for arranging arithmetic procedures, logical control, etc., would superpose on the equally numerous possibilities for the choice of types and sizes of vacuum tubes and other circuit elements from the point of view of practical performance etc. All this would produce an involved and opaque situation in which the preliminary orientation which we are now attempting would be hardly possible.
>
> In order to avoid this we will base our considerations on a hypothetical element, which functions essentially like a vacuum tube—e.g. like a triode element with an appropriate associated RLC-circuit—but which can be discussed as an isolated entity, without going into detailed radio frequency electromagnetic considerations. We re-emphasize: This situation is only temporary, only a transient standpoint, to make the present preliminary discussion possible. (von Neumann 1945: 29–30)

The situation was indeed only temporary. Von Neumann's "preliminary discussion" led to the EDVAC and the JOHNNIAC, up and running in 1952 and 1953 respectively, and to all subsequent 'von Neumann' computers. In the 1950s, and largely due to the availability of this computer technology, McCulloch and Pitts' work prompted early research in both connectionist and symbolic AI (see Chapters 10 and 12) and in computational neuroscience too (Chapter 14).

In sum, the abstractness (in this second sense) of McCulloch and Pitts' networks was significant. It licensed von Neumann to design electronic versions of them. It allowed him, when doing so, to ignore "detailed radio frequency electromagnetic considerations". It permitted computer scientists, including AI workers, who followed him to consider software independently of hardware. It enabled psychologists to focus on mental (computational) processes even while largely ignorant of the brain. And it led to *functionalism* in the philosophy of mind and cognitive science (Chapter 16.iii–iv).

4.v. Cybernetic Circularity: From Steam-Engines to Societies

Cybernetics is the study of "circular causal systems". These are self-regulating systems, in which information about the results of the system's actions is fed back so as to cease, adjust, or prolong the original activity. In the eyes of the early cyberneticians, they ranged from steam-engines to human societies.

Many such systems are highly constrained, having a relatively small number of possible states; but some can achieve self-organization from a random starting point. And whereas some have only one end point (only one state of equilibrium), others have many. There may be an unending succession of different 'circles', as when perceptual input leads to motor action, which leads to a new perception prompting a new bodily action . . . and so on (see Chapter 14.vii and ix.a–b). The cyberneticians of the 1940s, however, focused more on 'circles' leading to single end points.

In any given case, the self-regulation is done in a particular way: for instance, by a metal contraption in a steam-engine, or by chemical metabolites in a cell, or by the nervous system in behaviour. And the early cyberneticians (unlike some AI successors: see Chapters 10 and 12) were seriously interested in the underlying physical processes that made such phenomena possible.

Even so, the focus of cybernetics was on the flow of *information*, as opposed to the matter or energy involved. Because information is an abstract notion, it could be applied to many different types of system—even including minds.

a. Feedback, way back

The central principle—informational feedback—wasn't new, even if the *term* was. It was first mathematically developed in 1868 (see below), and had been quickening within biology since the 1860s (see Chapter 2.vii.a). But engineers had used it long before that, to enable machines to reach the same end in varying conditions.

It had featured, for instance, in some ancient automata (see Chapter 2.i), and in the escapement of a fourteenth-century clock, now in London's Science Museum, that alternated between being slightly fast and slightly slow (J. O. Wisdom 1951: 2–3). It had been exploited also in the early seventeenth century by the chemist–engineer Cornelius Drebbel, whose thermostatic incubator was controlled by the expansion and contraction of alcohol, raising and lowering a rod connected by levers to a damper (Bedini 1964: 41). By the mid-eighteenth century, it was common in water mills, and in the grain distributors of windmills. And it had entered the libraries: feedback devices were described in a book written by an Italian military engineer in the 1580s, and in the windmills chapter of an encyclopedia of 1786 (de Latil 1953: 116 ff.).

However, it was first used widely (and visibly) in the late eighteenth century—notably, as the ingenious "governor" designed by James Watt (1736–1819). This was developed to control the speed of a steam locomotive.

Watt's original device was a pair of heavy metal spheres suspended from an upright rotating rod, whose rotation was caused by cogwheels linked with the main shaft of the engine. Centrifugal force made the suspended spheres move away from the rod: the faster the rod turned, the higher they rose. As they did so, their weight dragged on the main shaft, so slowed the engine down. When the slowed-down rotation of the rod caused them to fall again, the drag on the shaft was decreased and the engine speeded up again. (In later versions, the speed of rotation of the rod controlled the steam pressure more directly, by being linked with the throttle.)

For Watt, this was an elegant practical solution to a specific engineering problem. Indeed, that's how it struck most people. The same was true of the other examples: the windmill gizmo was described as *a way of controlling grain feed*, not as an instance of *feedback*. The French cybernetician Pierre de Latil (1953) put it like this:

> Now, this mechanism [the Watt governor] has been in existence since round about 1780; and, throughout more than a century and a half, *no one* has seen that, far from being merely "ingenious", this classic device contains the makings of a revolution.
>
> In all the various attempts at classification of machinery, the Watt governor has never been rated worthy of any very prominent position. [The author of a well-known *Traité des mécanismes* of 1864] drew up what he called: "a systematic index of mechanical devices" in which he lists governors simply as "accessory apparatus". (de Latil 1953: 47–8; italics added)

He went on to complain about his own contemporaries, many of whom were irritated by the fuss attending this "new science":

> Aware of the fact that cybernetics borrows from their technique and vocabulary, certain electronic engineers discount the evidences of the new science, claiming that they know all about it and have been working along these lines for a very long time. That is as may be!—but are they not merely applying the principles without having grasped the inherent theoretical possibilities, in the same way as Watt and his successors did? For it is a fact that, unwittingly, they held within their grasp the key to problems that had arisen in many other fields, even in metaphysics. (de Latil 1953: 48)

De Latil's references to "metaphysics", here and elsewhere (e.g. pp. 195–204), was less a promise to illuminate the Cartesian mind–body problem than a reminder—and a recommendation—of Kant's distinction between "organism" and "mechanism" (see 2.vi.b).

However, de Latil's "no one" wasn't quite right. James Clerk Maxwell (1831–79) had seen Watt's invention—admittedly, some ninety years after Watt's use of it—as an example of an important general principle. In 1868 he read a paper 'On Governors' to the Royal Society, in which he discussed negative feedback in mathematical terms (J. C. Maxwell 1868). And it was in honour of Maxwell that Wiener named the field in 1947, for the word "cybernetics" has the same Greek root as the word "governor" (Wiener 1948: 12). (André Ampère had coined the term some seventy years earlier, using it in a political sense in his *Essay on the Philosophy of the Sciences* of 1884.)

The mid-twentieth century saw countless novel engineering applications, some driven by the need for weapons and defence in the Second World War. Many of these involved computer—but not Turing-computational—technology. These automated systems monitored various physical parameters of the processes they controlled. Most were analogue devices, such as the Bush differential analyser. Computers were here being used as number-crunchers, not as general-purpose symbol manipulators.

b. Infant interdisciplinarity

Our interest isn't in 'pure' engineering, however, or in analogue computers as such. Cybernetics is relevant to cognitive science because it was a self-consciously interdisciplinary project that studied organisms *as well as* artefacts, and took 'man as machine' as covering mind *as well as* body.

This was made explicit by Wiener, who defined cybernetics (when the US version was already about 10 years old) as the science of "control and communication in the animal and the machine" (Wiener 1948). (In fact, in terms of page-counts, his book was more about animals than about machines.) The British pioneers, too, applied the principles of automatic control—described in terms of physical "models" and/or self-organization—to organisms as much as engineering (see vi and viii below and Chapters 12.ii.c, and 15.i.b and vii).

Moreover, cyberneticians on both sides of the Atlantic were interested in the social/psychological aspects of man–machine systems. Besides the pioneering experimental research done in Cambridge, England (Section vi, below), this topic was highlighted in Wiener's non-technical book *The Human Use of Human Beings* (1950).

It's not surprising, then, that the infant cybernetics was nurtured by an intellectually diverse group—of whom McCulloch was one of the most prominent members. Indeed, he became the first President of the American Cybernetics Society. This was due not only to his own unusually wide-ranging research, but also to his gift for encouraging others to cooperate across disciplinary boundaries. Fired up by the seminal Josiah Macy Foundation meeting on hypnosis and reflexology in 1942, it was he who proposed the post-war Macy seminars on cybernetics—which he organized and chaired from 1946 to 1951.

These hugely influential international mini-meetings—only twenty-to-thirty core members—were held in New York from 1944 to 1953, with published proceedings from 1950 on (von Foerster 1950–5). With hindsight, it's interesting that the very first speaker was von Neumann, who regaled the small audience with a description of the digital computer—but expressed grave doubts about its usefulness for modelling the thoughts generated by human *brains* (see Chapter 12.i.d).

This emphasis on the life sciences was one factor distinguishing cybernetics from the closely related approach of Ludwig von Bertalanffy's (1901–72) general systems theory, developed at much the same time (von Bertalanffy 1933/1962, 1950). Whether von Bertalanffy himself regarded cybernetics as an example of systems theory isn't clear: sometimes he said that it was, but many of his remarks about the nature of "systems" implied that it wasn't (see Dupuy 2000: 129–35). However that may be, his general systems theory studied closed systems as well as open ones. Open systems exchange matter and/or energy with their environment, whereas closed systems don't—and the paradigm case of an open system is a living thing.

Another distinguishing factor was the differing treatment of *purpose*. Like cybernetics, general systems theory focused on self-organization or equilibration. One of von Bertalanffy's core ideas was "wholeness", a property of complex systems where the changes in every element depend on all the others. Another was "equifinality", in which a system would reach some equilibrium state from indefinitely many different initial conditions. He remarked that this principle covered the startling examples of embryological development that Hans Driesch had seen as incontrovertible evidence for vitalism (2.vii.b). So far, so familiar: most cyberneticians sympathized with all that. But von Bertalanffy insisted that embryos—and, by implication, other non-human organisms—*don't* show "true finality, or purposiveness". This, he said, involved foresight of the goal by means of the symbolism of language and concepts (von Bertalanffy 1950). Most cyberneticians, by contrast, underplayed the goal-setting role of language and conceptual thought (see Sections vi and viii, below).

Cybernetics pre-dated digital computers (3.v). Its totem artefacts, instead, were analogue computers and devices used in control engineering. But, like the symbolic computationalists inspired by Turing and by McCulloch and Pitts' 'Logical Calculus' paper, the early cyberneticians saw the mind as controlled by the sorts of processes embodied in (some) machines.

At that time, the two types of researcher—cybernetic and computationalist—were mutually sympathetic. They saw themselves as intellectual blood brothers, interacting at the international conferences of cybernetics and at various specially convened meetings.

There were disagreements, to be sure. For instance, Donald MacKay (1922–87) saw "little merit" in comparing the brain to a digital computer, as opposed to a probabilistic analogue mechanism (D. M. MacKay 1951: 105). But he was keenly interested in the digital hypothesis—and had earlier suggested building a hybrid computer: part-analogue, part-digital (D. M. MacKay 1949/1959). As this example illustrates, the various mid-century proponents of mind as machine were generally convivial. Relations didn't become strained until later.

As late as 1958, for example, a meeting on "the mechanization of thought processes" sponsored by Britain's National Physical Laboratory embraced people from both groups, including MIT's Minsky and McCarthy (Blake and Uttley 1959: see 6.iv.b). The same was true of the Macy conferences, whose participants were even more varied: the 1947 meeting, for instance, included I. A. Richards and Suzanne Langer, invited to talk on human communication and symbols. Torres y Quevedo's electromechanical chess automaton (3.vi.a) was exhibited by his son at the first cybernetics conference (in Paris) in 1951, and Wiener was photographed playing a game with it—which he lost (de Latil 1953: 33, 258). And the two opening talks of the 1945 meeting of the Teleological

Society (Section vi.a, below) were one on digital computers by von Neumann, and another on communication engineering by Wiener.

Even twenty years later, one of the cybernetic pioneers would mention *both* programs *and* feedback devices in a paper tellingly entitled ‘A Discussion of Artificial Intelligence and Self-Organization’ (Pask 1964). It’s not surprising, then, that Minsky’s early bibliography of AI cited a host of cybernetic items (Minsky 1961*a*).

Moreover, many individuals at mid-century—not least, McCulloch and Pitts themselves—had their feet firmly placed in both camps. Wiener, for example, wasn’t concerned only with dynamical feedback. He’d been a pupil of Russell’s. He stressed the role of mathematical logic in cybernetics. He described Leibniz as the cyberneticians’ “patron saint” (Wiener 1948: 12). And at the first Macy conference, he discussed Russell’s paradox and the computer’s oscillatory *true–false–true–false–true–false* response to it.

Similarly, Claude Shannon’s (1916–2001) information theory—his M.Sc. dissertation!—was a core intellectual resource for cybernetics, but was based in Boolean logic (Shannon 1938). When Turing showed Shannon his own paper in 1943, they agreed that their approaches had been essentially alike (Hodges 1983: 250–1). Rashevsky encouraged formal theoretical approaches *in general*, and included both statistical and logical models of the nervous system in his *Bulletin of Mathematical Biophysics*. And von Neumann, whose digital computer made logical programming possible, worked also on probabilistic parallel networks, and studied self-organization in organisms and artefacts (see 12.i–ii and 15.v).

Nevertheless, the two approaches were potentially divergent—and they soon diverged, as we’ll see in Section ix.

c. Biological roots

General physiology had been concerned with self-equilibrating mechanisms since the seminal work of Claude Bernard in the 1860s (2.vii.a). By the 1930s, the living body was recognized as an open system, continuously interchanging matter and energy with its environment while keeping certain vital quantities constant. This was largely due to the Harvard physiologist Walter Cannon (1871–1945), whose recent experiments on “homeostasis” had investigated many biochemical examples of circular causation (Cannon 1926, 1932).

These metabolic mechanisms maintain the internal environment of the body cells, regulating levels of blood temperature, sugar, salt, fat, proteins, calcium, and so on. Cannon showed that homeostatic control operates at many (interconnected) levels, under the regulation of the autonomic nervous system. For example, loss of blood leads not only to constriction of surface blood vessels but also to the release of blood corpuscles from the spleen. The sympathetic and parasympathetic nervous systems were found to have broadly oppositional, or complementary, functions, together maintaining the body’s equilibrium.

Cannon also suggested extending the idea of homeostasis to society. This wasn’t the first time that natural-scientific concepts had been applied to social matters. As we saw in Chapter 2.iii.b, Thomas Hobbes had likened the Commonwealth to “engines that move themselves by springs and wheels as doth a watch” (1651: 19). But Cannon’s exemplar was a self-adjusting biological system, not a piece of inanimate clockwork. His suggestion would be followed by others. Indeed, the official theme of the Macy conferences was ‘Cybernetics, Circular Causality, and Feedback Mechanisms in Biological *and Social* Systems’.

Interest in computer models of social institutions endured even as cybernetics was being overtaken by AI. Examples of the 1950s and early 1960s included work on bureaucracies, international relations, economics, management, and the daily rush hour (S. Beer 1959; Guetzkow 1962). And one of the founders of symbolic AI (namely, Simon) won the Nobel Prize for *Economics* (see 6.iii.a).

However, much as Simon turned towards individual psychology from the mid-1950s, so cognitive science in general became less social in character. By the 1970s, the social dimension had been largely eclipsed (1.ii.a). It would become visible again with Al work on ‘agents’ and the rise of the “new cybernetics”, aka A-Life (Chapters 12.ii.e, 13.iii.d–e, and 15, and J. M. Epstein and Axtell 1996).

Even in the 1930s and 1940s, social homeostasis was a minority taste. The biological versions were far more prominent. One of the founders of cybernetics, the Mexican physiologist Arturo Rosenblueth (1900–70), worked with Cannon at Harvard for eleven years investigating (for example) the regulation of the heartbeat. He did many physiological experiments with Wiener, who dedicated his book *Cybernetics* to him. So the ‘novel’ science of cybernetics was a close descendant of Bernard’s nineteenth-century work.

Neurophysiology—especially reflexology—was prominent in cybernetics from the start. The concept of reflex action took self-regulation beyond mere self-equilibration. Charles Bell and François Magendie, long ago, had described the reflex as a sort of circular causation, capable (for instance) of removing a hand or paw from an irritating stimulus. In the first half of the twentieth century, Charles Sherrington had shown how reflex mechanisms can provide a hierarchy of neural self-regulation. For instance, bodily posture when standing is maintained by a continuous interaction between flexor and extensor muscles, and by continuous sensory feedback from the touch sensors and muscle spindles; these stabilizing feedback circles are modified whenever a body part moves (see Chapter 2.viii.d).

Besides these reflex circularities, neural loops were being found, or hypothesized, within the brain. By the early 1930s, as we have seen, various people were suggesting that memory is grounded in reverberative neural circuits.

Neuro-circularity was assumed also by Lorente de No’s mid-century “law of reciprocity of connections” (Lorente de No 1947). This law stated that any group of neurones in the brain that sends fibres to another group also receives fibres from it (either directly or by means of one internuncial neurone). It was based, for instance, on his studies of the anatomical connections between eye and inner ear (Lorente de No 1933*b*), positing what’s now known as the vestibulo-ocular reflex (14.viii.b). Lorente de No himself was a prominent member of the cybernetics community. He took part in the first cybernetics meeting (organized by Wiener and von Neumann in 1943), and spoke about “the computing machine of the nervous system” at the Macy seminars.

In this neuroscientific context, McCulloch found it natural to describe each individual reflex arc as having a “goal, or aim, or end”: namely, the particular bodily state (for instance, the length of a muscle) which it first alters and then acts to restore (W. S. McCulloch 1948: 151). His teleological language was encouraged also by “goal-seeking” machines developed in England in the early 1940s, and by an influential cybernetic comparison of purpose with the behaviour of anti-ballistic missiles (see Section vi.a).

As for learning, reflexology treated this as the acquisition of conditioned responses by means of positive and negative reinforcement. This approach was originated in mid-nineteenth-century Russia by Ivan Sechenov, and elaborated around the turn of the twentieth century by Ivan Pavlov (see 2.viii.b). By the 1940s, it was flourishing in the USA as behaviourist psychology (5.iii.a–c). Wiener, like McCulloch, restated ideas about conditioning in terms of feedback. And he suggested that artificial synapses in computers might be similarly adjusted by "experience" to enable them to learn (Wiener 1948, chs. 4 and 5).

Psychopathology, too, was included. Tremors, convulsions, rigidities, pathological pain, and even various types of anxiety neurosis were explained in terms of disturbances in neurological feedback. So therapy should aim to 'break the circle' sustaining the pathological behaviour. Preferably, this should be done by neurochemical methods (W. S. McCulloch 1949), but otherwise by some form of psychotherapy (Wiener 1948: 174).

d. Information theory

In discussing this wide variety of neurological circles, cyberneticists often spoke of the "information" they carried. Similarly, cybernetic physiologists explained bodily homeostasis in terms of information flow. And cybernetic engineers glossed the physico-chemical measurements made by their machines, and fed into the control loops, as providing information to the system.

This term was drawn from Shannon's information theory, developed at Bell Labs to measure the reliability or degradation of messages passing down telephone lines (Shannon 1948; Shannon and Weaver 1949). But the "messages" were thought of not as meaningful contents, conveying intelligible information such as that Mary is coming home tomorrow. Rather, they were the more or less predictable physical properties of the sound signal. In Shannon's words:

> Frequently the messages have *meaning;* that is, they refer to or are correlated according to some system with certain physical or conceptual entities. These semantic aspects of communication are irrelevant to the information problem. The significant aspect is that the actual message is one *selected from the set* of possible messages. (Shannon and Weaver 1949: 3)

This statement was fully endorsed by Shannon's colleague Warren Weaver (1894–1978), even though he also expressed the hope that a theory of meaning might one day be expressed in informational terms (Shannon and Weaver 1949: 99–100, 116–17).

In short, "information" wasn't a semantic notion, having to do with meaning or truth. It was a technical term, denoting a statistical measure of predictability. *What* was being predicted was the statistics of a physical signal, *not* the meaning—if any—being carried by the signal.

As a technical term for something measurable, "information" needed a quantitative unit. This new unit was the *bit* (an abbreviation of "binary unit"). One bit of information was defined as the amount of information conveyed by a signal when only two alternatives are possible. (It was Shannon who defined the "bit" in the context of information theory, but the term had been coined in 1946 by the statistician John Tukey, who also thought up "software": Leonhardt 2000.)

Each alternative, of course, may cover many distinct possibilities. For instance, the answer to the *Yes/No* question "Does the missing letter appear in the first half of the alphabet?" conveys only one bit of information. After all, either it does, or it doesn't. However, each possible answer covers thirteen possibilities: the letters A to M (for "Yes") and N to Z (for "No"). If the unit of measurement was unfamiliar, the general principle wasn't: any practised player of the parlour game Twenty Questions was aware that it's foolish to ask "Is it a cat?" before asking "Is it an animal?"

Information theory, then, had nothing to do with semantics. Certainly, Shannon sometimes discussed natural language. Specifically, he used information theory to predict the next word in generating 'English' word sequences. But he used only statistics, not syntax or semantics, to do so (see Chapter 9.vi.c). For Wiener or McCulloch, the signal might be a physical process in a neurone, instead of a telephone wire. Or it might be a written memorandum passed by one person to another. But the fundamental notions of "information", "messages", and "decisions" didn't imply *meaning*. Cybernetic theories didn't specify conceptual or propositional content.

Admittedly, cybernetic *psychologists* assumed meanings to be relevant. For instance, the precise nature of a psychosis would depend on the person's linguistically mediated experiences and beliefs. But its sudden onset was explained by analogy to the breakdown of an overloaded telephone exchange, where the *semantic content* of the many incoming messages is irrelevant. Similarly, cybernetic psychiatrists such as Kubie and McCulloch took it for granted that the feedback mechanisms underlying an anxiety neurosis involve *intelligible* associations. But they didn't specify particular semantic contents (see Wiener 1948, ch. 7—and cf. Chapter 7.i.a).

This flight from meaning and subjectivity has a lot to answer for: it became an important source of the structuralist and deconstructionist assaults on humanism that originated in France in the 1970s (Dupuy 2000). But that was still to come. The main point here is that mid-century cybernetic psychology rarely represented specific meanings in its theories. That's one reason why the symbolic-AI alternative, when it sprouted in the mid-1950s, seemed—to many psychologists—to be more promising.

e. Bateson, Pask, and a sip of Beer

Gregory Bateson (1904–80) was an exception, for he *did* apply the cybernetic approach to psychological meaning. His hugely influential "double-bind" theory of schizophrenia, which became popular in the 1960s, attributed it to the reception of contradictory messages from family members. These messages, often though not always verbal, were supposed to connote permissible or impermissible forms of behaviour (Bateson *et al.* 1956). Similarly, his therapy for alcoholism presupposed a circular semantics involving the patient's self-image (Bateson 1971). He'd picked up the notion of directive ideas, or schemas, from his tutor Frederic Bartlett in the 1920s (5.ii.b and vi.b, below), but the notion of feedback came from cybernetics.

The circularity was important—but, to Bateson's disappointment, the popularized versions of his theory lost sight of it. Most people interpreted the double bind as either a *logical* contradiction between messages, or a situation in which one would be hurt/punished *whatever* one did, as if forced to choose the lesser of two evils. For Bateson, it was more complex: a communicative system was set up which would systematically prevent the 'desired' outcome, owing to paradoxical relations between explicit communications and the background context (Harries-Jones 1995: 134–9).

This focus on multilevel contradictory meanings wasn't new. In his first book, a study of kinship rituals in a New Guinea tribe, Bateson (1936) had described what was going on in terms of complementary conflicts in relationships, defined at three hierarchical levels. These conflicts, he'd said, largely balanced each other out, so that potentially divisive behaviour was contained within a stable cultural system. (He didn't yet have the concept of feedback, but—not surprisingly—immediately got the point when he heard about it from McCulloch a few years later.)

Bateson's concern, in these contexts, with specifically human meanings was belied elsewhere. He frequently claimed that "mind" is present in *every* example of circular causality, even including whirlpools. He even said that all living things and social institutions—"the starfish and the redwood forest, the segmenting egg, and the Senate of the United States—have knowledge" (Bateson 1972; 1979: 12). But the sense in which the starfish "knows how to grow into five-way symmetry" is very different from the sense in which the Senate knows about George Washington, or the child knows what is expected by the mother. So much so, that one might judge the term "knowledge" to be inappropriate (see the discussion of autopoietic biology in Chapters 15.vi–vii and 16.x.c).

Another exception was A. Gordon Pask (1928–97), ensconced in the 1950s in a back room at the Cambridge Language Research Unit (Preface, ii). In the early 1960s he set up his own company, Systems Research Limited. Reached by a set of creaky stairs, this was situated over a launderette in Richmond. But even the creakiest of stairs couldn't keep his admirers away. Both McCulloch and Minsky, for instance, often visited; and the frequent home-based visitors included Stafford Beer, William Ross Ashby, and Richard Gregory.

Pask was a highly imaginative—and highly eccentric—early cybernetician, esteemed by McCulloch as "the genius of self-organizing systems" (W. S. McCulloch 1965: 220). His eccentricities and imaginative experiments had started at an early age:

> [As a young boy] Gordon had been a very determined designer of things electromechanical and chemical. I have it from a contemporary of his at Rhydal School in North Wales that he reputedly designed a special kind of bomb, this would be in 1942 or 1943 when he was aged twelve or thirteen, and my informant recalls Gordon being whisked mysteriously away from school in a military staff car. (Mallen 2005: 86)

Originally trained as a chemist and a psychologist, Pask considered meaning contexts of various sorts.

He was nothing if not ambitious, nothing if not interdisciplinary, and nothing if not prescient. For example:

* He planned a cybernetic theatre, in which audience reaction would influence the unfolding of the plot on stage—an idea that's now being claimed as "new" by enthusiasts for interactive media and virtual reality (Chapter 12.vi.a–c).

* He led a team of scientists, mathematicians, and artists planning Joan Littlewood's "Fun Palace Project", intended to provide a host of challenging diversions not found in ordinary theatres (Ascott 1966/1967: 119).

* He suggested how an architectural space (room, building, public arena . . .) might, in effect, *learn* and *adapt to* some of its users' goals, and their physiological and

psychological reactions (Pask 1969). Some of this learning and adaptation could be automatic (using sensor technology, variable lighting, and movable walls/screens), some would require feedback-driven modifications by human architects. This work anticipated today's ideas on "intelligent buildings", and is regarded by some as one of the "critical writings for the digital era" (Dong 2003; Spiller 2002: 76–7).

* He built a "Colloquy" of mutually adaptive mobiles, descendants of the Musicolour system mentioned in the Preface. They communicated by both sight (colour and pattern) and sound (pitch, rhythm, and simple phrases). They also provided an "aesthetically potent" environment for interactivity, enabling humans to include meaningful interpretation in the loop (Pask 1971). The Colloquy was exhibited at the seminal Cybernetic Serendipity conference, the world's first international exhibition of computer art (Reichardt 1968) (see l.iii.d).

* He concocted simple evolutionary machines, not programmed but implemented as solutions of chemical salts (Pask 1961: 105–8).

* One of his 'craziest' ideas was among the electrochemical models (e.g. Pask 1959), of which the "evolutionary" machine was one. These grew threadlike crystalline structures—dynamically balanced between the deposition and re-solution of ions—supposedly analogous to concepts, since they could discriminate sounds of different pitch (50 or 100 Hertz), or the presence/absence of magnetism, or differences in pH level.

Although the crystalline "ear" wasn't useful *qua* technology, the principle was interesting—even deep. For these chemical threads weren't *designed in order to* discriminate pitch (or magnetism, or pH), but naturally arose in a way that made this discrimination possible. In other words, they were a primitive example of the self-organization of *new* perceptual abilities, creating *new* perceptual dimensions—which is what happens from time to time during phylogenetic evolution. (Mostly, evolution *improves* an ear, or an eye—but sometimes, it *originates* a primitive ear/eye where none had existed beforehand.) That's largely why McCulloch thought so highly of Pask's work. In his Preface to Pask's first book (1961), McCulloch credited him with having illuminated "the central problem of epistemology", namely: where do our concepts *come from*?

McCulloch's accolade didn't prompt Pask's contemporaries to follow in his footsteps. His electrochemical ear was seen as a mere curiosity, and was soon well-nigh forgotten. The world would have to wait for thirty-five years before its theoretical significance was realized (Cariani 1993—and see Chapter 15.vi.d), and for forty-four years before a comparable example occurred (Bird and Layzell 2002).

That post-millennial example involved the evolution of a primitive radio antenna (a "radio wave sensor"), which picked up and modified the background signal emanating from a nearby PC monitor. The researchers involved were using a recently developed technique for evolving circuits in hardware (15.vi.c), in order to evolve an oscillator—not a radio. But, unexpectedly, some of their devices exploited unconsidered an accidental aspects of the physical environment (the aerial-like properties of printed circuit boards, and the proximity of the PC) to produce radio reception. Indeed, the (rewarded) oscillatory behaviour depended largely on accidental factors, such as the order in which the analogue switches had been set, and the fact that the soldering-iron left on a nearby workbench happened to be plugged in at the mains. In short, Pask had been way ahead of his time.

As though all that weren't interesting enough, Pask also discussed the cybernetics of analogy and parable in human learning and creativity (Pask 1963). And he designed a number of pioneering teaching machines that employed feedback to adapt to different individual learners. (True to form, the feedback was initially embodied in dishes of iron-salt crystals—but he soon cobbled simple computers to implement the system instead.)

Pask used this wide variety of adaptive machines to illustrate a theory of "conversation" covering human communication (and brain processes) as well as man–machine interfaces (Pask 1975*b*). However, his theoretical notation was rebarbatively complex. His general theory didn't spread beyond the core cybernetic community, but his machines were more influential. The cooperating mobiles, for example, were exhibited at London's Institute of Contemporary Arts, and the teaching machines inspired further research (Mallen 2005).

Unlike Skinnerian teaching machines, which employed a fixed program and syllabus, Pask's devices used their interaction with the student to alter their own decision rules and data presentation. After his pioneering SAKI, which taught a simple motor skill (see Preface, ii), Pask developed computers enabling human learners (and problem-solvers) to take different routes through a complex knowledge representation (Pask and Scott 1973; Pask 1975*a*, ch. 11). The domain might be probability theory, or an imaginary taxonomy of Martian animals. But the route through it which was proffered to the user by the system depended on the individual's general cognitive style. Specifically, it depended on their preference for semantically "holist" or "serialist" strategies.

The holist learner needs to confront relatively high-level aspects of the unfamiliar domain at the outset, even though these can't be fully understood at that point and may lead to over-generalization. The serialist is put off by such 'premature' presentation of high-order relations, preferring to build the new knowledge step by step, each step being readily intelligible in relation to its predecessor.

In his experiments on human subjects, Pask had identified a further distinction also, between "irredundant" and "redundant" holism (IH and RH, respectively). IH people avoid apparently irrelevant semantic content, and may be confused by it if it crops up. RH thinkers, by contrast, often find it positively helpful. So RH-ers are typically aided by analogies, 'side stories', and parables. But IH-ers (and serialists, too) find them unhelpful, and perhaps even counter-productive.

Pask couldn't explain these differences in cognitive style. (There's still no detailed explanation, but it now seems that RH-ers engage in a type of information processing which is relatively tolerant in estimating 'relevance': it not only *costs* them less to link 'far-flung' ideas, but they're able to get added cognitive *benefits* out of doing so: see Chapter 7.iii.d.) He pointed out, however, that they implied that different communicative styles would be best for reaching different learners. So tutorial programs should ideally be able to present their material in any of the three ways—which a fixed text, such as a book, clearly cannot.

Accordingly, his CASTE machine (Course Assembly System and Tutorial Environment) would engage in a "conversation" with the learner, first identifying and then adapting to the individual's cognitive style. Some students, Pask reported, experienced "a sense of participating in a competition (some say a conversation) with a not dissimilar entity" (Pask 1961: 92). Today's AI work on user-friendly interfaces, and on how people can combine their experience of genuine and 'virtual' reality, explores some of the questions that Pask raised forty years ago (see 13.v–vi).

Pask described the criminal underworld, too, as a cybernetic system. He even saw criminals and the police locked in a communicative symbiosis, which he and his associate George Mallen simulated—at first by role playing, then on a computer—in his laboratory over the launderette. The role playing, which also involved a senior police officer from the Met, was so convincing that it drew a visit from the writers of *Z Cars*, the first police series to be shown on British TV; it also drew a raid by the local constabulary, who'd been alerted by a neighbour overhearing "plans for a bankraid" (G. Mallen, personal communication). Moreover, the SIMPOL computer program (which went through three incarnations) was so convincing that it was used for several years in training at the Bramshill Police College.

However, the particular semantic content was provided by the human beings. For Pask's theory stressed not specific meanings, or propositions, but general notions of feedback in communicative networks. The same was true of other examples of social cybernetics.

For instance, Beer (1926–2002), a close friend of Pask, applied such ideas to industrial organizations at the first meeting of the International Association of Cybernetics in 1956. Unlike Babbage, who'd also compared a factory to a complex machine, and who discussed many detailed logistic and managerial problems (Chapter 3.i.a), Beer developed a general theory of management and operational research that focused on circular causation as such (S. Beer 1959). And unlike the sociologist Max Weber (1864–1920), who'd analysed bureaucracy in terms of rigid administrative hierarchies, Beer described the role of individual decisions and information flow in the organization as a whole. (What he *didn't* do was consider the decision-making processes within individual minds: see Chapter 6.iii.a.)

In 1971 Beer was invited to put his ideas about communication into practice in Chile, for Salvador Allende's government (Beckett forthcoming). He designed a network (called Cybersyn), based on 500 unused telex machines bought and stored away by the previous government, which ran the whole length of the country. Driven by socialist principles, Cybersyn linked mines, factories, local social groups, and government ministers (one of whom was Fernando Flores: 11.ii.g), enabling them to share detailed economic and political information speedily.

Cybersyn had an intellectual thread attaching it to England. For Beer's first computer-based experiments on issues of management had been done with Pask at Richmond. The team at Systems Research had developed "Ecogame", the first management multimedia interactive game (Lambert 2005: 73). Players sat at computer terminals depicting various situations, and made business decisions linked to social costs. Besides random-access 35 mm slide projectors, it utilized realtime displays showing numbers and graphs for each player, updated according to the person's decisions. Ecogame was exhibited at the Computer Graphics 1970 exhibition at Brunel University, and later taken to Davos for a summit of world economic leaders.

(In Chile, however, Beer's Cybersyn was no game, but deadly serious. After Allende's assassination in June 1973, the Pinochet government deliberately destroyed the system because of its egalitarian potential. It had already been used to support resistance to right-wing opposition movements.)

Although a wide range of social scientists were invited by McCulloch to the Macy meetings (Heims 1991, esp. chs. 6 and 8; Dupuy 2000, ch. 3), many Macy participants perceived social cybernetics as speculative and marginal. They respected von Neumann's theory of games (von Neumann and Morgenstern 1944). But they saw the (idealized) psychology of 'rational economic man' as very different from—and theoretically superior to—(empirical) social psychology.

Wiener himself wrote at length about social issues (Wiener 1950). But even he regarded applications of cybernetics to everyday social phenomena as unreliable, because they could initiate circular causation that eliminated previously observed social trends (Wiener 1948: 190). That is, they could provide self-defeating prophecies as well as self-fulfilling ones. And this fact could threaten an entire discipline, some of whose more imaginative practitioners (Bateson and Margaret Mead, for example) were Macy attendees:

> With all respect to the intelligence, skill, and honesty of purpose of my anthropologist friends, I cannot think that any community they have investigated will ever be quite the same afterwards. (Wiener 1948: 190)

Most cyberneticists' attention was devoted not to social systems but to biological organisms—and to artificial self-regulating mechanisms. Some of these artefacts provided ideas that were fed back into theories of the biological "machine", both bodily and mental. And this was happening, simultaneously, on both sides of the Atlantic.

4.vi. Brains as Modelling Machines

Independently of the cybernetics movement in the USA, similar ideas were being developed in England by the psychologist Kenneth Craik (1914–45). His credo was published in 1943, five years before Wiener's book (though near-simultaneously with his co-authored paper on teleology: Rosenblueth *et al.* 1943).

Craik, too, was inspired by the idea that control in analogue machines might be similar to control in the nervous system. But in exploring this theme, he developed a new approach to the brain. He suggested that it's a system which constructs "models" representing the world (and possible worlds, as in fairy tales). Both psychology and neurophysiology would be deeply influenced by his work.

"How is such and such represented in the brain?" seems, today, an obvious question to ask. Even people who deny the existence of internal representations aren't surprised to hear other people ask it, or to hear answers that compare the brain to some kind of computational machine. This wasn't always so. Even as late as 1940, such questions weren't seriously raised—and they wouldn't have been answered in that way in any case.

Certainly, it had been assumed ever since René Descartes that there must be some brain state correlated with the "such and such"—or anyway, with the perception or the thought of it. But correlation isn't representation. What more is needed, over and above correlation (temporal simultaneity), for a brain state to be a representation?

* In what sense—if any—must it match, or correspond to, the external reality, in order to represent it?

* And is correlation really necessary? After all, some brain states, presumably, can represent something even in its absence; and others can represent non-existent things (like unicorns), where no correlations are ever available. How are memories, predictions, hypotheses, illusions, fictions, or hallucinations possible?

* Last but not least, what about the nitty-gritty? Can we say why *this* brain state, rather than some other, should be the one to represent *that* environmental feature? Or are such facts mere contingencies, to be discovered but not understood?

a. A Cambridge cyclist

The questions just listed are a mixture of philosophical and scientific puzzles. On the one hand, they concern what representation *is*, what "representation" means. On the other hand, they concern how, in fact, the human brain manages to represent various things, so as to control behaviour. (Let's assume, here, that it does do this; doubts will be discussed later, in Chapters 13.iii.b–c, 14.viii, and 16.vi–viii.) Craik was well aware of this interdisciplinarity. He described his theory as a new *philosophy*, not just as psychology or neurophysiology.

Before the 1940s, neuroscientists hadn't asked such questions in a rigorous way. If they spoke of representations at all, they did so uncritically, as though the concept were a clear one. And they didn't seriously consider the idea that a certain type of brain state might be *inherently apt* for symbolizing a certain type of thing. That is, they didn't try to match the physiological syntax onto the psychological semantics.

To be sure, the clinical neurologist Henry Head (1861–1940) had spoken of a "postural schema" thirty years earlier (Head and Holmes 1911). This, he suggested, represented one's (continually changing) bodily attitudes. He'd posited some sort of isomorphism between brain states and reality, which supposedly explained how it was possible for the schema to function as a guide for, not just a correlate of, specific bodily actions. He'd even generalized the notion of schema from the body itself to its clothing:

> Anything which participates in the conscious movement of our bodies is added to the model of ourselves and becomes part of those schemata: a woman's power of localization may extend to the feather of her hat. (Head and Holmes 1911)

More recently, Head's student Bartlett (1932)—a leading Gestalt psychologist—had generalized it still further, to cover conceptual memories as well as bodily ones (see Chapter 5.ii.b). But neither man had asked *just what* the schema's neural mechanism might be—for the very good reason that there was no way of knowing. What's more, there seemed to be no way of coming up with fruitful hypotheses.

This changed in the early 1940s, when Bartlett's young colleague (and ex-student) Craik wrote his seminal book *The Nature of Explanation* (1943). This was published, in wartime Cambridge, only two years before Craik's tragic death in a cycling accident on King's Parade, aged only 31. Some say he was knocked off his bicycle by an American jeep on VE Day (V. Stone 1978: 87), others (probably more reliable) that he was thrown under a lorry on the eve of VE day, by someone suddenly opening a car door (Mollon n.d.). Either way, Craik was no longer around to see the effects of his ideas.

He wouldn't have expected a quick payback, in any case. For the book reported no new discoveries. Rather, it set a research agenda. (The closing words: "And so, *Tentare*".)

Craik tried to express ideas about interpretative schemas in precise, and neurologically plausible, terms. He argued that the brain events implementing perceptions and memories are *models* of the things represented, which can be useful in psychological processing because they *work in the same way* as those things. And he used a machine analogy to explain what he meant.

He referred to cybernetic devices such as "an anti-aircraft predictor, Kelvin's tidal predictor", and "the Bush differential analyser" (1943: 51, 60). As analogue machines, it was their *physical* properties which embodied their representational power. The tide predictor, for example, was made of pulleys and levers (so didn't resemble a tide visually), but *it works in the same way in certain essential respects* (namely, "it combines oscillations of various frequencies so as to produce an oscillation which closely resembles in amplitude at each moment the variation in tide level at any place"). The same was true, he suggested, of models in the brain:

By a model we thus mean any physical or chemical system which has a similar relation-structure to that of the process it imitates. By 'relation-structure' I do not mean some obscure non-physical entity which attends the model, but the fact that it is a physical working model which works in the same way as the process it parallels, in the aspects under consideration at any moment. (1943: 51)

I have tried . . . to indicate what I suspect to be *the fundamental feature of neural machinery*—its power to parallel or model external events—and have emphasized the fundamental role of this process of paralleling in calculating machines. (p. 52; italics added)

His fascination with models, and with machines, dated from his schooldays. As a boy, he'd built several pocket-sized steam-engines—one of which he showed to Bartlett on their very first meeting (Bartlett 1946). (Now, they're all in an Edinburgh museum.)

Those toy models were very obviously similar to the full-scale reality. But this needn't always be so—which is just as well, since neuroscientists can't expect to find miniature steam-engines inside our heads. Nor need they fear that the complexity of the model must always match that of the real thing:

[If] the fluid pressure at perhaps half a dozen points within an ocean wave were measured, it might be found that no form of surface other than that of the actual wave would cause the pressure at those points to be just what it is. It is thus conceivable that [in the visual system] a highly complicated pattern of local stimulation may make a fairly simple "label" or symbol for itself, in the form of a pattern of stimulation travelling to a common centre. (1943: 75)

This reference to a pattern of neural stimulation as a "symbol" was deliberate. Craik described his theory as "a symbolic theory of thought" (p. 120). However, he wasn't thinking of the formal symbols used in machines based on Turing-computation, for these didn't yet exist. Nor had McCulloch and Pitts published their formal–symbolic account of psychology (although they'd do so very soon, in the same year that Craik's book appeared). For Craik, symbols symbolized—representations represented—in virtue of their *physical* features:

> Without falling into the trap of attempting a precise definition, we may suggest a theory as to the general nature of symbolism, viz. that it is the ability of processes to parallel or imitate each other, or the fact that they can do so since there are recurrent patterns in reality . . . [The] point is that symbolism does occur, and that we wish to explore its possibilities . . . [We must ask] is there any evidence that our thought processes themselves involve such symbolism, occurring within our brains and nervous systems? (1943: 58–9)

> [This] symbolism is largely of the same kind as that which is familiar to us in mechanical devices which aid thought and calculation. (p. 57)

It followed that there's nothing eerie about memory, prediction, or even imagination. If the cerebral model, considered as a dynamic physical process, works in the same way as the external reality being modelled, it will behave (develop) in the same way. It can therefore represent past and future events—by persisting and by running, respectively.

But it can also model purely hypothetical matters. The results can then be evaluated (as positive/negative reinforcers) by other brain mechanisms, just as veridical models are. If they're evaluated as negative, the animal can avoid carrying out the (hypothetical) actions whose internal models produced them. "In the same way", he said, a tiny model of the *Queen Mary* helps the shipbuilder, and a differential analyser that calculates strains helps the designer of a bridge (pp. 52 and 61). In short, prediction and imagination—in general, thought—are both physiologically possible and psychologically useful.

The general message was that engineering, neuroscience, and psychology—and philosophy too, as we'll see—must be integrated. For the human brain, Craik said, is "the greatest machine of all, imitating within its tiny network events happening in the most distant stars" (p. 99).

The stars in one's eyes, whether literal or idiomatic, may of course be illusory. Craik grasped the nettle of non-veridical representation. He argued that perceptual illusions, errors in abstract thought, false beliefs, and much psychopathology (and religion, too: cf. 8.vi) also depend on adaptive modelling of the external world. But in all these cases, the models are skewed, often rigid, and counter-productive. For example:

> [Hysterical conduct is] a form of adaptation . . . achieved by narrowing and distorting the environment until one's conduct appears adequate to it, rather than by altering one's conduct and enlarging one's knowledge till one can cope with the larger, real environment. Dissociation and schizophrenia and repression are further mechanisms for attaining this state of splendid isolation and pseudo-adjustment and of excluding difficulties and awkward suspicions. (1943: 90)

In general, his approach was to ask how it's possible for *anything* to represent something else. In criticizing the Gestalt psychologists, for focusing on description not explanation, he said:

> The interesting thing in perception, surely, is not just *what* happens, but how and why it happens, and what has failed in the case of illusion or insanity. (1943: 114)

Given that cerebral models aren't inserted piecemeal into our brains by some benevolent deity, how do they arise? They're *in principle* possible, according to Craik, because physical processes in general are so basically similar that any one can be modelled by some other. (Hence his reference, above, to "recurrent patterns in reality".) But which *specific* reality processes are modelled by various species, and *how,* are empirical questions.

So neuropsychologists should ask:

* What physical features does reality-A possess? (Any one of these might function as a stimulus for some species or other.)
* What physical features in the nervous system, *if* they existed there, would be sufficiently similar to enable it to model reality-A?
* And are those features in fact possessed by process-A* found in the brain of this species?

If the answer to that last question is "Yes", then process-A* is a model of reality-A.—Or rather, it *could be*, it is *apt to be*, a model of reality-A. Whether it *is* a model of it depends on other things, as we'll now see.

b. Similarity isn't enough

Whether a brain mechanism that physically resembles some external reality *actually is* a model of it depends on whether it's linked into the animal's sensori-motor processing in such a way as to be useful *to the whole animal.*

In other words (mine, not Craik's), *model*—like *representation*—is an intentional concept. As Minsky would put it some twenty years later, when defending references to models in AI:

> The model relation is inherently ternary. Any attempt to suppress the role of the intentions of the investigator B leads to circular definitions or to ambiguities about "essential features" and the like. (Minsky 1965: 45)

> We use the term "model" in the following sense: To an observer B, an object A* is a model of an object A to the extent that B can use A* to answer questions that interest him about A. (ibid.)

A computer simulation, for the researchers using it, is a model of the real-world phenomenon concerned. With respect to *internal* models, the "investigator" or "observer" is the system itself. If a computer—or an organism—uses some internal mechanism A* to answer questions about A which it wants answered, then the one is an internal model of the other. Let's grant that a computer can't really *want* anything. But an animal can—and, in general, the "questions that interest him about A" involve survival value.

Forty years after Craik's death, neurophysiologists discovered that monkeys have cells in the visual cortex that respond to faces (monkey or human) in particular orientations: full face, profile, looking upwards, or looking downwards—distinctions that are important in the animals' social interactions (Chapter 14.iv.d). Suppose, for a moment, that the monkeys had been found to possess a *continuous* range of eye-aversion detectors, but *without* any added functionality—such as greater discrimination in their social behaviour. Those cells would be *apt* to model many different angles of gaze, but they wouldn't actually *be* models of a continuous gaze range.

Such a discovery is of course unlikely, because such useless—or anyway, unused—cells probably wouldn't have evolved in the first place. Certainly, a neural structure apt for representing this or that might arise accidentally, through random mutation. And, as A-Life work on *neutral* (*sic*) networks suggests, it might survive uselessly for a while and then be exploited by later generations (15.vi.c). But without attracting adaptively relevant connectivities, it would very likely disappear eventually.

Today, the appearance and loss of potentially useful but actually useless neural circuits can be observed close-up, using the techniques of evolutionary robotics (Chapter 15.vi.c, and vii.d below). Mini-circuits may be randomly generated which are apt to represent some aspect of the robot's environment. But if they lack the neural connections that would *put them to use* in the robot's task environment, the evolutionary program (genetic algorithm) will soon prune them. Only if they do increase the robot's 'fitness' will they be retained.

Craik pointed out that selection pressures (survival values) add an extra dimension to the neuroscientist's task. His research strategy outlined above assumes that the *fundamental* requirement is a matter of physical similarities between environment and nervous system. And he believed that to be so. But nervous systems have also been shaped by survival value, which "complicates" matters enormously. "In general", he said, "it is much more illuminating to regard the growth of symbolising power from this aspect of survival value, rather than from the purely physical side of accordance with thermodynamics" (1943: 60). That is, *the whole animal*, and its "interest" in survival, must be considered (see 14.vii and 15.vii–viii).

c. Craik and cognitive science

As for actual examples of neural models, Craik discussed various aspects of (mostly visual) neurophysiology in these terms. Our nervous system, he said, clearly does contain examples of "states of excitation and volleys of impulses which parallel the stimuli which occasioned them" (1943: 60). He suggested several hypotheses about the neural processes that *could* represent this or that environmental feature (see especially pp. 64–77).

These hypotheses were related to (one or more) specific computational functions, and were typically explored with particular machine analogies in mind: from differential analysers and anti-aircraft devices to electron-scanning cameras and temperature-compensated barometers. For instance, in discussing the mechanism underlying vernier acuity (seeing a tiny lateral shift of one part of a nearly straight line), he said:

> We must, then, consider how the nervous system might achieve two essential steps in the process:
>
> (1) The summation of several points of stimulation, lying on a straight line, so as to give rise to a state of neural excitation or trace or pattern having *direction*, which may be called a "vector element"; and
>
> (2) How differentiation occurs, so that a series or line of such vector elements having the same direction possesses little or no perceptual significance, whereas a bend or discontinuity in it has a very marked one. (1943: 68).

He immediately described what he saw as "the easiest mechanical or electrical method" by which to do this—but then pointed out that it underperformed the visual system in various ways. Next, he asked how we should "set about designing a mechanism" to do *those* things. In considering that question, he made careful functional distinctions between superficially similar tasks—suited to the "calculations" (computations) effected by distinct types of physical machine.

Even in a book of only 120 pages, neuroscientists might have hoped to find more than thirteen devoted to nitty-gritty empirical questions, such as these. And indeed, relevant remarks were scattered throughout the volume. But neuroscience wasn't yet able to support many detailed speculations—not even about vision, never mind "hysterical conduct" (pp. 90 ff.).

Moreover, Craik was a psychologist, not a neuroscientist. This didn't prevent him from making prophetic and/or influential (though sometimes mistaken) suggestions about the brain: for instance, that a randomly connected cortex might self-organize with experience, and that the EEG might be a cortical scanner (see Craik 1943: 115, and Chapter 14.iii.a and ix.b). But neuroscience wasn't his prime concern.

In a sense, neither was psychology. His first love—and his first degree, at Edinburgh University—had been philosophy. Moreover, he intended his highly interdisciplinary book as a statement of a new *philosophy.* This fact was declared in the Preface, reflected in the chapter titles (such as '*A Priorism* and Scepticism' and 'On Causality'), and evident on almost every page. As we've seen, the text addressed the conceptual puzzles concerning representation, as well as the empirical ones. Kant was mentioned more often than Sherrington, although Craik gave him a less central role than McCulloch did. And well over half of the forty-item bibliography was devoted to epistemology or philosophy of science—plus the ubiquitous *Principia Mathematica.*

Unlike both McCulloch and Pitts, however, Craik hadn't been seduced in adolescence by this new logic. Or if he had, the love affair was a short one. He described *Principia* as "a garden where all is neat and tidy but bearing little relation to the untidy tangle of experience from which the experimentalist tries to derive his principles" (p. 3). But he did this in the context of a discussion of *epistemology*, not just psychology.

As it turned out, Craik's book had no *direct* influence on philosophy (see Chapter 16.iii.a). Even in psychology and neuroscience, its influence outside his immediate circle was somewhat delayed. Relatively few read it on publication: in 1943, and especially in England, people had other things on their minds. And the transatlantic convoys dodging the U-boats (with Turing's help: 3.v.d) weren't shipping books across the ocean.

Immediately after the war, however, Craik's ideas were much discussed. In the high-profile Waynflete Lectures of 1946, Lord Adrian praised him for (as he saw it) making mentalism redundant: "ideas" were simply organized patterns of electrical activation in the nervous system (Adrian 1947: 94). Bartlett's (1946) obituary stressed his role as a highly creative experimental psychologist. The minutes of the inaugural meeting of the Experimental Psychology Group, founded in Cambridge a year after his death, noted their belief "that the type of work in which the Group will engage would have secured his approval" (Mollon n.d.).

Soon afterwards, the newly formed Ratio Club in London picked up Craik's baton (see Section viii below, and Chapters 12.ii.c and 14.iii.a). Most club members had recent wartime experience of analogue machines like those he'd mentioned. And, spurred by his writings, they leant on that experience in thinking about the mind/brain.

The neurophysiologist Horace Barlow, for instance, was strongly indebted to Craik (Barlow 1959: 542; see also Chapter 14.iii.b). So was Gregory, a Cambridge psychologist close to Barlow and to Bartlett's group. He endorsed Craik's enthusiasm for engineering analogies (14.iii.a), and later developed Craik's approach to visual illusions (6.ii.e). Gregory wasn't a member of the club, but learnt many years later that he'd been about to be elected to it when it closed (personal communication).

His friend and colleague Christopher Longuet-Higgins, who'd later give cognitive science its name (and many of its ideas as well), missed out on the Ratio Club too. But he didn't miss out on Craik. Inspired by his ideas, he built a very simple wheeled model to illustrate the use of internal representations for prediction and gap-filling, as opposed to direct control from the inputs (Gregory 2001: 389).

By the early 1960s, Craik's followers felt themselves to be "at the forefront of a revolution" (Baddeley 2001: 348; see also Zangwill 1980). And those followers were now spread far beyond the magic circles of Cambridge and the Ratio Club. Countless people were drawn to Craik's work by his (posthumous) data-rich papers in the *British Journal of Psychology* (1947–8). These discussed the role of human operators dealing with—or rather, participating in—servo-control systems.

Craik declared, for instance, that:

> As an element in a control system *a man may be regarded as* a chain consisting of the following items:
>
> 1. Sensory devices, which transform a misalinement [*sic*] between sight and target into suitable physiological counterparts, such as patterns of nerve impulses, *just as* a radar receiver transforms misalinement into an error-voltage.
> 2. A computing system which responds . . . by giving a neural response calculated . . . to be appropriate to reduce the misalinement . . .
> 3. An amplifying system—the motor-nerve endings and the muscles—in which a minute amount of energy (the impulses in the motor nerves) controls the liberation of much greater energy in the muscles . . .
> 4. Mechanical linkages (the pivot and lever systems of the limbs) whereby the muscular work produces externally observable effects, such as laying a gun [NB: the second military example]. (Craik 1947–8: ii. 142; italics added)

A clearer statement of man-as-machine would be difficult to find—unless one also mentioned verbal thought (which Craik himself had done in his book, and which symbolic AI would do later).

Craik's two papers hit their mark. They attracted attention partly as pioneering work in applied psychology (he'd been appointed Director of the newly founded Applied Psychology Unit in 1944), and partly as intriguing examples of the new approach of cybernetics.

Cybernetic discussions—and cybernetic machines—were now springing up on both sides of the Atlantic, and Wiener's widely read rallying cry was published in 1948. It was evident from Craik's (posthumous) papers, and from his book, that he'd anticipated the core themes. In brief: although he wasn't part of the cybernetics *movement*, he was an early, and highly significant, *cybernetician.*

d. Might-have-beens

If Craik hadn't died in 1945, he might have broadened his discussion to include digital as well as analogue devices. He might even have joined Ratio member MacKay (1949/1959) in urging the development of hybrid "analytical engines", part-analogue and part-digital. Also, he might have been a strong voice in the development of neural/psychological theories of *emulation* (Grush 2004: see Chapter 14.viii.c).

Despite his previous disparagement of *Principia Mathematica*, he might have been excited by Turing's (1950) paper in *Mind* and by the symbolic AI of the 1950s. After all, he'd already described thought—and mental modelling—as involving three "symbolic" processes:

> (1) "Translation" of external processes into words, numbers, or other symbols.
> (2) Arrival at other symbols by a process of "reasoning", deduction, inference, etc., and
> (3) "Retranslation" of these symbols into external processes (as in building a bridge to design) or at least recognition of the correspondence between these symbols and external events (as in realising that a prediction is fulfilled). (1943: 50)

So, thankful that he'd explicitly avoided giving "a precise definition" of symbolism in 1943, he might have widened his concept to include formal representations as well as physical ones. (From the late 1950s on, cognitive scientists would do precisely this—in one case, marrying Craik's theory to formal semantics: Chapter 7.iv.e.)

However, those are all might-have-beens. What actually happened was that McCulloch and Pitts published their logicist manifesto in the same year as Craik's book.

As we saw in Section iii.f, this didn't sweep the board immediately either. Its technical proofs were taxing, and there were no comfortingly familiar logic machines to back it up. On the contrary, John von Neumann's computer would take the form it did partly because of the ideas in that paper. But by the early 1950s, people in and around MIT were increasingly thinking of digital–formal models of thought as well as analogue ones. The "models" had come from Craik, the "digital–formal" from McCulloch and Pitts.

4.vii. Feedback Machines

Cybernetics would have thrived even in peacetime. After all, it had already been given a good start (biologically) by Bernard and Cannon, and (mathematically) by Maxwell. Moreover, engineers hadn't needed a war to make them work on feedback in clock escapements, or steam-engine governors. But there's no denying that the Second World War provided opportunities—practical challenges as well as money—for mid-century engineers to develop cybernetic machines.

Most cyberneticians were already thinking about biology, as we've seen, and many were looking towards psychology too. It's no surprise, then, that insights and techniques initiated in the military context were soon applied in a wide range of post-war artefacts built with psychology and/or neurophysiology in mind. (Which is *not* to say that no such artefacts had been built before: see Chapters 2.viii.e and 5.iii.c, and Cordeschi 1991, 2002; Valentine 1989.)

a. Purposes of war

The wartime machines of most interest to cyberneticians weren't code-breakers, which in any case were shrouded in secrecy, nor even number-crunching computers (such as ENIAC) used for calculating bombing tables. Rather, they were systems involving self-corrective servomechanisms. In particular, the cybernetics community was concerned with weapons such as radar-guided missiles.

Anti-ballistic missiles were capable of a form of self-equilibration going beyond Bernard's "internal environment" to involve the external environment, too. Anticipating the movement of the tracked target (by extrapolation of its path observed so far), they would aim at the point where the target would be at some time in the future. These continuous predictive adaptations were seen by cyberneticians as a paradigm case of "purposive" or "teleological" behaviour.

In a highly influential paper of 1943, Rosenblueth, Wiener, and Bigelow defined teleological behaviour as "behavior controlled by negative feedback". This was the written version of their talk at the 1942 Macy meeting on hypnosis, and the first published account of the new ideas (see also Rosenblueth and Wiener 1950). Affecting to link hypnosis (and other forms of psychopathology) with missiles, it's no wonder that their audience was excited.

More specifically, they described teleological behaviour as:

> behaviour controlled by the margin of error at which the [behaving] object stands at a given time with reference to a relatively specific goal . . . The signals from the goal are used to restrict outputs which would otherwise go beyond the goal. (Rosenblueth *et al.* 1943: 19)

A clear case of purpose, they said, is a machine designed so as to impinge on a moving luminous or heat-emitting "goal". And they assimilated such machines to human purposes—and thereby to "voluntary activity", since "what we select voluntarily is a specific purpose, not a specific movement". For instance, in discussing the inability of patients with cerebellar disease to bring a glass of water to their lips, they said:

> The analogy with the behaviour of a machine with undamped feed-back is so vivid that we venture to suggest that the main function of the cerebellum is the control of the feed-back nervous mechanisms involved in purposeful motor activity. (Rosenblueth *et al.* 1943: 20)

The notion that purpose is analysable in terms of "difference reduction" (between the current state and the goal) had already entered cybernetic discussions. It was mentioned, for example, in McCulloch and Pitts' logical calculus paper of the same year. But the teleology paper raised the profile of this discussion. Von Neumann was greatly impressed by it, and asked Wiener to arrange for him to meet Rosenblueth (Aspray 1990: 267).

Its influence resulted partly from its definition of *two* categories of teleology. The simplest type is controlled merely by the current distance from the goal state. If the goal happens to be a moving target, the system aims for the place where it now judges it to be. An extrapolative system, by contrast, predicts some future location of the target and aims for that point. Such systems include biological organisms (a cat chasing a mouse) and anti-ballistic missiles, which compute the expected future path of the target in real time.

In 1944, while most information-processing technology was still classified, Wiener, von Neumann, and Aiken (designer of the Harvard Mark I) organized a small gathering of people already largely in the know. This group, which also included McCulloch, Pitts, and Lorente de No, called themselves the Teleological Society. The organizers of the first (and, as it turned out, the only) meeting declared:

> Teleology is the study of purpose or conduct, and it seems that a large part of our interests are devoted on the one hand to the study of how purpose is realized in human and animal conduct and on the other hand how purpose can be imitated by mechanical and electrical means. (quoted in Aspray 1990: 183)

Whether the "mechanical and electrical means" best suited for imitating purpose are cybernetic or computational, involving dynamical or logical-symbolic systems respectively, wasn't then much debated. As remarked in Section v.b, this distinction wasn't yet clearly made.

To be sure, von Bertalanffy distinguished equilibration from "true finality" involving symbolic foresight (see Section v.b). But most cyberneticians seemed to see no fundamental difference between pure self-equilibration (as in homeostasis), purposive behaviour directed to some observable object (as in guided missiles), and goal-seeking directed to some intentional end (as in human deliberation and planning).

In particular, the concept of mediation by *symbolic* representations wasn't widely current. As we've seen, Craik had suggested that behaviour involves internal models of the world, which could be paralleled in artefacts. But he saw these models as analogue physical mechanisms inside the brain, not as symbolic structures embodied in neural networks. And he focused primarily on sensori-motor control, not abstract problem solving or purposive behaviour directed to still-imaginary ends.

Similarly, the cyberneticians in general relied on quantitative measures, and on equations describing correlations between continuously varying system parameters. They didn't, and couldn't, describe complex hierarchical structures or detailed discontinuous changes in system structure, such as are involved in much human thinking, and language.

Problem solving and planning would come into centre-stage only in the mid- to late 1950s. Then, Newell and Simon attributed them to *symbolic* representation of the end state—the key notion of difference reduction being retained (see 6.iii.c). At that time too, Noam Chomsky's (1957) new theory of generative linguistics explained hierarchically structured behaviour in terms of internal models conceptualized as *formal rules* (6.i.e and 9.vi). So by 1960, the general concept of internal models, or representations, had been hijacked by the symbolists. (As we'll see in Chapters 12 and 14.viii, other types of representation are also possible.)

Meanwhile, the early cyberneticians had been modelling purposive behaviour in either analogue–dynamical or, less commonly, reflex terms.

b. Post-war projects

Anti-ballistic missiles had been built for practical, and deadly, use. (So, for that matter, had ENIAC: see Chapter 3.vi.a.) But some self-regulating artefacts at mid-century were developed to throw light on psychological and biological phenomena.

Unlike machines (from Pascal's onwards) focused on logic or mathematics, these didn't attempt abstract problem solving. Admittedly, Shannon, like Turing, discussed the possibility of chess programs, and argued that these should use symbolic concepts like those humans employ to avoid considering every possible move (Shannon 1950*a*,*b*). But his own chess machine (Caissac), which played a variety of endgames by comparing the advantages of various moves, was a special-purpose gizmo, not a program (McCorduck 1979: 103).

In general, the focus of cybernetic devices—most of which were analogue, not digital—was on adaptive behaviour. Some were intended to illustrate fundamental organizational principles of life in general and nervous systems in particular.

Shannon built a mechanical maze-runner called Rat, which he took along to the eighth Macy conference in 1951. It used an electrical contact "finger" to detect the walls of a twenty-five-square maze. Two electrical motors enabled the fingers to move either north–south or east–west: if the finger touched a wall, Rat started again from the centre of the relevant square. Exploring the maze in ignorance, this machine would store its errors (those choices which led to a blind alley) in its memory, and avoid making them on its second run (Shannon 1951). However, if the maze wasn't solved within a given number of moves, the memory was erased, so enabling the maze-runner to escape from a repetitive cycle.

Early British devices, some of which were exhibited at the Festival of Britain in 1951, included a maze-learning "rat" designed independently of Shannon's by I. P. Howard (1953). This electromechanical rat, equipped with three whiskers ("feelers"), could learn any maze, of reasonable size and suitable lane widths.

Always trying the right turn first at any new choice point, it marked its errors on a memory wheel used to set the switches for its subsequent runs. Unknown to Howard, an essentially similar memory wheel had been used in a maze-solving machine built many years earlier (T. Ross 1938). But Howard's work was of more general interest. His theory didn't require that the creature always turn right first, nor that there be only two possible paths. Moreover, it could be adjusted to cover probabilistic behaviour (Howard 1953: 56).

Howard's rat wasn't lonely: comparable artefacts were legion. In the 1940s and 1950s, so many machine models of life and mind were developed that Pask (1964) soon counted over 350 instances. Besides those of Pask himself, interesting examples—interesting, that is, to cognitive scientists—were built by MacKay (1956*a*), Albert Uttley (1956, 1959*a*,*b*), Tony Deutsch (1954), and John Andreae (1963) (see Chapter 12.ii.b–c, iv.a, and c). Their machines ranged from adaptive robots, through ball-dropping hoppers that learnt by reinforcement, to abstract models of adaptation in neural networks. (Many examples were described in Blake and Uttley 1959; for reviews, see George 1959, 1961, and Cordeschi 2002.)

We can't consider them all here—and we don't need to. For their basic principles were illustrated in two 'families' of device constructed in England from the late 1940s on. Together with McCulloch and Pitts' theoretical papers of 1943 and 1947, and Craik's book, these artefacts inspired most of the cybernetic machine makers of the 1950s.

Both were the brainchildren of neurologists who were also amateur engineers. If they hadn't been, they couldn't have put mind-as-machine into practical form, nor kept so close (*despite* the huge oversimplifications) to the biology. In so far as neurological data could be exploited at that time, these machines were driven by neurological interests, and knowledge. And as the next section (and Chapters 12 and 14–15) will show, they were in many ways far ahead of their time.

4.viii. Of Tortoises and Homeostats

The cybernetic families of Tortoises and Homeostats were constructed by William Grey Walter (1910–77) and William Ross Ashby (1903–72), respectively. (Neither of them used the "William".) Both men were early members of the interdisciplinary Ratio Club (Husbands forthcoming).

This was a small dining club that met several times a year from 1949 to 1955, with a nostalgic final meeting in 1958, at London's National Hospital for Neurological Diseases. The founder–secretary was the neurosurgeon John Bates, who had worked (alongside Craik) on servomechanisms for gun turrets during the war.

His archive shows that the letter inviting membership spoke of "people who had Wiener's ideas before Wiener's book appeared" (P. Husbands, personal communication). Indeed, its founders had considered calling it the Craik Club, in memory of Craik's work—not least, his stress on "synthetic" models of psychological theories. In short, the club was the nucleus of a thriving British tradition of cybernetics, started independently of the transatlantic version.

The Ratio members—about twenty at any given time—were a very carefully chosen group. Several of them had been involved in wartime signals research or intelligence work at Bletchley Park (3.v.d). They were drawn from clinical psychiatry and neurology, physiology, neuroanatomy, mathematics/statistics, physics, astrophysics, and the new areas of control engineering and computer science. (For a discussion of the neuroscientists and psychologists involved, see Chapter 12.ii.c.)

The aim was to discuss novel ideas: their own, and those of guests—such as McCulloch, who was their very first speaker in December 1949. (Bates and MacKay, who'd hatched the idea of the club on a train journey after visiting Grey Walter, knew that McCulloch was due to visit England and timed the first meeting accordingly.) Turing gave a guest talk on 'Educating a Digital Computer' exactly a year later, and soon became a member. (His other talk to the club was on morphogenesis.) Professors were barred, to protect the openness of speculative discussion. So J. Z. Young (who'd discovered the squid's giant neurones, and later suggested the "selective" account of learning: 2.viii.e and 13.ix.d) couldn't join the club, but gave a talk as a guest.

The club's archives contain a list of thirty possible discussion topics drawn up by Ashby (Owen Holland, personal communication). Virtually all of these are still current. Indeed, if one ignores the details, they can't be better answered now than they could in those days.

These wide-ranging meetings were enormously influential, making intellectual waves that are still spreading in various areas of cognitive science. Barlow (personal communication) now sees them as crucial for his own intellectual development, in leading him to think about the nervous system in terms of information theory (see Chapter 12.ii). And Giles Brindley (another important neuroscientist: 14.iii.c), who was brought along as a guest by Barlow before joining for a short time, also remembers them as hugely exciting occasions (P. Husbands, personal communication).

Our specific interest here, however, is in the machines built by two members of the Ratio Club: Grey Walter's tortoises and Ashby's Homeostat. As befitted the diverse interests of the club, these were very different in style. One was intended to model purpose and learning, the other to approximate general features of life. And they were very different as physical objects, too. One was an intriguing gadget that attracted enormous publicity (frowned on by some Ratio members), while the other was a laboratory contraption predictably ignored by the newspapers. Their theoretical interest, however, was significant in both cases.

More "interest", perhaps, than immediate influence. With hindsight we can now see how hugely insightful they were. But that 20–20 vision wasn't available to their contemporaries. Some people got the point, to be sure. De Latil (1953), for instance, regarded both as "revolutionary", and wrote about their wider scientific—and philosophical—implications at length. (His book was soon translated into English by a relation of Grey Walter's boss, the neurologist Frederick Golla.) However, the primitive state of electronic technology didn't enable those implications to be explored in practice.

That wouldn't be possible until the late 1980s, with the development of behaviour-based (situated) robotics and dynamical theory: see Chapters 14.vii and ix, and 15.vii–viii and xi.

a. Robots at the festival

Grey Walter was the first Director of Physiology (from 1939) at Golla's newly founded Burden Neurological Institute, Bristol. He was a highly influential electroencephalographer. For instance, he discovered the delta and theta rhythms, and designed several pioneering EEG-measuring instruments. In addition, he founded the EEG Society (in 1943), organized the first EEG Congress (1947), and started the *EEG Journal* (also 1947). With his EEG expert's hat on, he focused on the overall effects of large populations of neurones rather than on specific cell connections. But his robots, as we'll see, used as few 'neurones' as possible.

His interest in the EEG dated from his time at Cambridge in the early 1930s, when he worked on muscle contraction with Edgar D. Adrian. Ever since the first paper on EEG (by Hans Berger) in 1929, Adrian was a key pioneer in the field. He discovered body mappings—of the limbs, for instance—in both cerebellar and cerebral cortex. And he predicted that improved brain-monitoring technologies would one day (fifty years later, as it turned out: 14.x.c) enable neuroscientists to study the cerebral changes associated with thinking (Adrian 1936: 199).

In his youth, Grey Walter also studied conditioning with a team of Pavlov's students visiting from St Petersburg. Indeed, he met Pavlov briefly. But his prime neurological interest was in the activity of the brain as a whole.

Besides these psychophysiological skills, he was a skilled speaker and writer in several languages. He was much in demand to talk on both professional and political issues. Initially a communist, he later veered towards anarchism: that is, the rejection of top-down control. But he was so full of ideas that, as his son Nicolas remembers, "he found it difficult to produce more sustained work, and both of his two books were actually written by his father from his notes and conversations" (N. Walter 1990).

From 1949 onwards, Grey Walter built several intriguing cybernetic machines. These were intended to throw light on the behaviour of biological organisms—although he did point out that they could be adapted for use as "a better 'self-directing missile' " (Grey Walter 1950*a*,*b*, 1953; see also O. Holland 1997, 2002). Unlike actual missiles, however, his machines displayed a range of different behaviours.

He'd been inspired, in part, by a wartime conversation with Craik. Craik was then working on scanning and gun aiming, and visited Grey Walter at the Burden Institute to use some of his state-of-the-art electronic equipment. During his visit he suggested that the EEG might be a cortical scanner, affected by sensory stimuli. This idea became influential in neuroscientific circles (13.ii.a). And it was later modelled by Grey Walter as a rotating photoelectric cell, whose "scanning" stopped when its robot carrier locked onto a light source.

Wiener's influence on him was less effective. Thus in a letter to Adrian written in June 1947 (after Craik's early death), Grey Walter said:

> We had a visit yesterday from a Professor Wiener, from Boston. I met him over there last winter and found his views somewhat difficult to absorb, but he represents quite a large group in the States . . . These people are thinking on very much the same lines as Kenneth Craik did, but with much less sparkle and humour. (O. Holland 2002: 36)

Wiener himself was more generous—or perhaps just more polite. In a letter thanking Grey Walter for his hospitality during this brief visit, he wrote, "I got a great deal out of our trip, and am certain that it will be possible to renew our contact at some future date" (Wiener 1947).

In particular, Grey Walter sought to model goal seeking and, later, learning. But he did so as economically as he could—in both the financial and the theoretical sense. Not only did he want to save money (the creatures were cobbled together from war-surplus items and bits of old alarm clocks), but he was determined to wield Occam's razor. That is, he aimed to posit as simple a mechanism as possible to explain apparently complex behaviour.

And simple, here, meant simple. His wheeled robots, or "tortoises", had two valves, two relays, two motors, two condensers, and one sensor (for light or for touch). In effect, then, a Grey Walter tortoise had only two neurones. For, crucially, the tortoises weren't mere toys but models of (very simple) nervous systems.

Robot toys with simple tropisms were already common at the time, at public exhibitions if not in the toyshops. For instance, a French-made "Philidog" at the 1929 International Radio Exhibition in Paris would follow the light from an electric torch—until it was brought too near to its nose, when it started to bark (de Latil 1953: 240–1). Ten years later, visitors to the New York World Fair in 1939 were sadly robbed of their chance to be enchanted by another robot dog. It had committed suicide a few days earlier:

> [The "electrical dog"] was to be sensitive to heat and was to have attacked visitors and bitten their calves, but just before the opening of the exhibition it died, the victim of its own sensitivity. Through an open door it perceived the lights of a passing car and rushed headlong towards it and was run over, despite the efforts of the driver to avoid it. (de Latil 1953: 241)

Great fun—except perhaps for the dog! But nothing to do with neuroscience. Grey Walter, by contrast, was pioneering biologically based robotics, an activity taken up many years later by (among others) Arbib, Valentino Braitenberg, Rodney Brooks, Randall Beer, and Barbara Webb (13.iii.b, 14.vii.c, and 15.vii).

One of his tortoises, the *Machina speculatrix*, showed surprisingly lifelike behaviour. "Lifelike" rather than (human) mindlike—the Latin word meant exploration, not speculation. But Grey Walter clearly had his sights on psychology as well as physiology. This was the first step in a research programme aimed at building a model having

> these or some measure of these attributes: exploration, curiosity, free-will in the sense of unpredictability, goal-seeking, self-regulation, avoidance of dilemmas, foresight, memory, learning, forgetting, association of ideas, form recognition, and the elements of social accommodation. (Grey Walter 1953: 120–1)

"Avoidance of dilemmas" and "free-will" were supposedly modelled by the tortoise's ability to choose between two equally attractive light sources (O. Holland 2002: 44–5). Unlike Buridan's ass, forever poised between two identical bundles of hay, the tortoise would unknowingly exploit its scanning mechanism to notice, and so to follow, one light before the other. "Learning", "association of ideas", and "social accommodation" came later (see below). Meanwhile, the Latin tag being too much of a mouthful, this early tortoise was quickly named ELSIE (from Electro-mechanical robot, Light-Sensitive with Internal and External stability). The prototype, which was very similar, was dubbed ELMER: ELectro-MEchanical Robot.

ELSIE soon became something of a celebrity. Much as Jacques de Vaucanson's flute-player had delighted visitors to London's Haymarket 200 years before (2.iv.a), so ELSIE amazed visitors to the Festival of Britain held—only a couple of miles away—in 1951. A few years later, it caused great amusement at a meeting of Babbage's brainchild, the British Association for the Advancement of Science. For it displayed an unseemly fascination with women's legs, presumably because of the light reflected from their nylon stockings (Hayward 2001*a*). (Much later, it was resurrected at the Science Museum for the Millennium Exhibition, and for the centenary of the British Psychological Society in 2001.)

Members of the public unable to reach the Festival site were soon able to read about ELSIE in a potboiler entitled *The Robots Are Amongst Us* (Stehl 1955). A more serious account—but including a photograph of ELSIE and her infant human 'brother' in Grey Walter's living-room—had already come off the press in France, and appeared in English in 1956 (de Latil 1953). Grey Walter himself became something of a celebrity too. He was often invited to speak by the BBC, sometimes appearing as a panellist on the popular radio programme *The Brains Trust*.

b. Of wheels and whiskers

The Festival robot ELSIE explored its environment. It used a scanning photoelectric cell (coupled to the steering wheels) to seek light and to guide the wheels towards the illumination. If the light wasn't too bright, the tortoise would stay in front of it, oscillating very slightly to left and right. But a strong light would cause the creature to continue scanning. In that case, its attention—and its movement—might be attracted by another, perhaps a weaker, light. As Grey Walter put it (in deliberately biological language), the tortoise showed a positive tropism to moderate light, but negative tropisms with respect to both strong lights and darkness.

Besides these basic tropisms, Grey Walter's robot displayed simple forms of approach and avoidance. Its (slightly movable) "shell" acted as a three-dimensional whisker, or pressure sensor: it closed an electrical circuit whenever it encountered a mechanical obstacle, causing the creature to back away from walls, furniture, or people's fingers. Since the robot also carried a pilot light, it would approach its own image in a mirror, or another light-bearing tortoise. On touching the mirror, or the mate, it would automatically move away—only to be drawn back again by the other's pilot light. The mechanical minuet that resulted was, for a while, fascinating to watch.

Another cybernetic device designed (in 1950) by Grey Walter was an electrical learning circuit named CORA (COnditioned Reflex Analogue), now kept in London's Science Museum. Largely 'cannibalized' from ELSIE, this functioned only as a static box on the workbench. It wasn't incorporated by Grey Walter himself into a mobile tortoise, although there's some evidence that he'd intended to do so. That may explain why "in spite of his efforts, CORA had only a small fraction of the impact of the tortoises, and made little lasting impression" (O. Holland 2002: 46). This shouldn't have mattered, at least outside the exhibition halls. For, quite apart from its theoretical interest, Grey Walter did *connect* it to the circuitry of a moving tortoise, so presumably was able to demonstrate learning in action. But the rhetorical effect was less dramatic (see 1.iii.h), and even the scientists failed to see the point.

The "point", here, was Pavlovian. CORA was based in neurophysiology, being a development of an earlier circuit named NERISSA (Nerve Excitation, Inhibition, and Synaptic Analogue). It was intended as a model of the sort of conditioning that Pavlov had reported in his bell-salivating dogs (2.viii.b)—and which had fascinated Grey Walter in his Cambridge days.

If CORA was repeatedly presented with two stimuli in quick succession, only the second of which 'naturally' caused a particular response, it would eventually produce that response even when the first stimulus occurred alone. Like Pavlov's dogs, CORA needed occasional reinforcement (wherein the first stimulus is again followed by the second) in order to maintain the conditioning. It was based on a probabilistic theory, and may have owed something to Grey Walter's fellow Ratio member Uttley (see Chapters 12.ii.c and 13.ii.b). In short, CORA reflected current views on neural communication and learning—and it was described by Grey Walter in his account of the *living* brain (Grey Walter 1953: 203–7).

Grey Walter pointed out that CORA could be combined with *Machina speculatrix* to produce a robot capable of learning: *Machina docilis*. A tortoise equipped with CORA and an auditory sensor would learn to approach at the call of a whistle, if the whistle was repeatedly blown just before a flash of light. Similarly, it would learn to move away from its current position when 'whistled', if the whistle had often been blown just before it touched an obstacle. One follower of Grey Walter built tortoises, called *Machina reproducatrix*, sensitive to various combinations of lamp, flute, and whistle (Angyan 1959).

Significantly, Grey Walter noted that his model of associative learning could "respond to a part of the significant association as if the whole were present" (Grey Walter 1956: 368). This, he said, was "essentially the same process" as pattern recognition—what Pitts and McCulloch (1947) had called knowledge of universals (Chapter 12.i.c). He didn't say, because he couldn't know, that part-to-whole generalization would be hailed thirty years later as one of the triumphs of parallel distributed processing (12.vi).

Grey Walter's last reported device, built in about 1953 and shown to the Ratio Club in 1955, was called IRMA: Innate Releasing Mechanism Analogue (Grey Walter 1956: 367–8). As the name implies, this was designed to model the ethologists' notion of an IRM: an innate propensity to respond in a specific way to a specific stimulus (see 5.ii.c). In developing IRMA, Grey Walter was especially interested in stimuli originating in the action of some other robot, so that the activity of two (or more) creatures could be coordinated in adaptive ways. Again, this robotic research would be largely forgotten, only to be taken up several decades later (13.iii.e).

If IRMA was the last device to be built by Grey Walter, it wasn't the last to be envisaged by him. Near the end of his working life (his research ceased when he was seriously injured in 1970), he remarked on "a new era" made possible by transistor technology:

> We are now envisaging the construction of a creature which instead of looking as the original did, like a rather large and clumsy tortoise, resembles more closely a small eager, active and rather intelligent beetle.
>
> There seems to be no limit to which this miniaturisation could go. Already designers are thinking in terms of circuits in which the actual scale of the active elements will not be much larger, perhaps even smaller, than the nerve cells of the living brain itself. This opens a truly fantastic vista of exploration and high adventure . . . (Grey Walter *c.*1968: 7)

Grey Walter's intriguing tortoises, despite their valve technology and clumsiness, were early versions of what would later be called Vehicles (Braitenberg 1984), autonomous agents, situated robots, or animats. They illustrated the emergence of relatively complex motor behaviour—analogous to positive and negative tropisms, goal seeking, perception, learning, and even sociability—out of simple responses guided and stabilized by negative feedback.

The "tropisms" of *Machina speculatrix*, for instance, emerged from a few core rules linking the speeds of the two motors to the level of illumination. In the dark, the drive motor would run at half-speed and the steering motor at full speed. In a moderate light, the drive motor would run at full speed while the steering motor was switched off. And in strong illumination, the drive motor and steering motor would run at full and half-speeds, respectively. These simple mechanisms gave rise to a wide range of observable behaviour.

Even more to the point, in an unpublished manuscript of about 1961, Grey Walter described a complete behaviour—finding the way past an obstacle to reach a light source—as being achieved by four reflex "behaviour patterns", some of which were "prepotent" over others (O. Holland, personal communication). The four basic patterns envisaged were exploration, positive and negative phototropisms, and obstacle avoidance. His analysis, in this case, was equivalent to that used in the 'subsumption architecture' of modern behaviour-based robotics.

During Grey Walter's lifetime, and for nearly thirty years afterwards, his tortoises—like Vaucanson's flute-player—were commonly dismissed by professional scientists as mere robotic "toys". The general verdict was that they were superficially engaging, but of little theoretical interest.

This largely negative reception was due partly to the vulgar publicity they'd attracted in the mass media. The brouhaha surrounding the tortoises put off even some of Grey Walter's fellow Ratio members, who were better placed than anyone to appreciate their significance. He wasn't the last to suffer the counter-productive effects of publicity. The connectionist Frank Rosenblatt would do so too, only a few years later, and symbolic AI would suffer similarly in the early 1970s (Chapters 12.iii and 11.iv, respectively).

But excessive media attention wasn't the only obstacle. As remarked above, Grey Walter himself never provided an extended account of his tortoises' theoretical implications. Having his father draft his books from his notes and conversations simply wasn't enough.

c. Less sexy, more surprising

The "mere toy" criticism was less often made of another interesting cybernetic gadget of the 1940s, Ashby's "Homeostat". For this *was* explicitly situated within a comprehensive theoretical context by its designer. Whereas Grey Walter kept most of his neuropsychological speculations to himself, Ashby didn't.

In addition, the Homeostat was less sexy, less media-friendly, than the tortoises. (Rhetorical considerations, again.) It didn't explore its environment, approach other Homeostats, or learn to come at the call of a whistle. Indeed, Grey Walter sarcastically dubbed it *Machina sopora* (sleeping machine), or "a machine designed to do nothing" (O. Holland, personal communication, and de Latil 1953: 308). Nor did it look like any sort of animal. It was intended not to replicate superficially lifelike behaviour, but to explore the most general features of life and mind: feedback and self-organized adaptation.

The Homeostat aroused interest, and scepticism too, when it was described at the ninth Macy meeting in 1952. Interest, and even stunned incredulity, because its behaviour wasn't carefully predesigned like ELMER's but emerged from a random starting point (see below). Scepticism, because Ashby insisted that it was to be understood as a purely *physical* mechanism. Even the hard-headed engineer Bigelow was nonplussed by this, declaring that whereas Shannon's Rat—which stored "memories" of its encounters with the maze—could be said to learn, the Homeostat could not. One might as well say, he complained, that a ball-bearing that falls through a hole in the bottom of a shaken box has "learned to find the exit" (Dupuy 2000: 150).

Despite the initial scepticism, the Homeostat would later inspire important research in both connectionism and A-Life. In the 1950s, however, it was too far ahead of its time to be followed up (improved on). Indeed, it was too radical even to be properly appreciated by most people.

Ashby's definition of cybernetics was even more inclusive than Wiener's:

> [Cybernetics] takes as its subject-matter the domain of "all possible machines", and is only secondarily interested if informed that some of them have been made, either by Man or by Nature. What cybernetics offers is the framework on which all individual machines may be ordered, related, and understood. (Ashby 1956: 2)

In principle, then, any physical system whatever is grist to the cybernetics mill. But Ashby, like Grey Walter, was a neurologist (and he too worked at the Burden Institute, becoming Director for a brief period at the end of the 1950s). As such, he was most interested in biological examples.

He was interested in everyday homeostasis, as all the cyberneticists were. But he was especially intrigued by the startling reorganizations implied by spontaneous adaptation to inverting spectacles, which had been reported by George Stratton in the 1890s (see 14.viii.b). (A recent computer model of such reorganizations, explicitly based on Ashby's work, will be described in Chapter 15.xi.a.) Similarly, he was intrigued by Roger Sperry's (1947) then recent discovery that animals could recover from the surgical 'cross-wiring' of antagonistic muscles.

From the outset of the 1940s, and independently of Wiener, he conceptualized biological adaptation in terms of feedback-based equilibrium. Adapted behaviour is "the behaviour of a system in equilibrium", which can be described in mathematically rigorous terms (Ashby 1947). But whereas Wiener treated feedback as a fundamental concept, Ashby defined it in terms of difference and change in dynamical systems:

> The most fundamental concept in cybernetics is that of "difference", either that two things are recognisably different or that one thing has changed with time . . . All the changes that may occur with time are naturally included, for when plants grow and plants age and machines move some change from one state to another is implicit . . . (Ashby 1956: 9)

> That a whole machine should be built of parts of given behaviour is not sufficient to determine its behaviour as a whole: only when the details of coupling are added does the whole's behaviour become determinate . . .
>
> Cybernetics is . . . specially interested in the case where each [of two parts] affects the other . . . When this circularity of action exists between the parts of a dynamic system, *feedback* may be said to be present. (Ashby 1956: 53)

> . . . the concept of "feedback", so simple and natural in certain elementary cases, becomes artificial and of little use when the interconnexions between the parts become more complex. When there are only two parts joined so that each affects the other, the properties of the feedback give important and useful information about the properties of the whole. But when the parts rise to even as few as four, if every one affects the other three, then . . . knowing the properties of all the twenty [possible] circuits does *not* give complete information about the system. Such complex systems cannot be treated as an interlaced set of more or less independent feedback circuits, but only as a whole.
>
> For understanding the general principles of dynamic systems, therefore, the concept of feedback is inadequate in itself. What is important is that complex systems, richly cross-connected internally, have complex behaviours, and that these behaviours can be goal seeking in complex patterns. (Ashby 1956: 54)

Ashby specifically included the neural control of behaviour as an example of adaptation (Ashby 1940, 1947). As he put it:

> The brain is a physical machine . . . Adaptive behaviour is that which maintains it in physical equilibrium with its environment. (Ashby 1947: 58)

> "Adaptive" behaviour is equivalent to the behaviour of a stable system, the region of the stability being the region of the phase-space in which all the essential variables lie within their normal limits. (Ashby 1952: 64)

The kitten who learns to avoid a hot coal on the carpet, he said, does so by a neural mechanism embodying feedback, whereby its brain adapts to the unwelcome stimulation. Its trial and error learning is not merely (as the early behaviourists implied) a blind effort at success, but a way of gaining specific information about the environment and how to adapt to it. And errors aren't valueless failures, but essential parts of the information-gathering process.

That process could be thought of as information gathering or as self-equilibration. Similarly, physiological processes as such could be thought of in either way. Talk of "information" was the more likely when *mind*, or behaviour, was in question. But life and mind were essentially similar, comprising many dynamical systems coupled to, and nested within, each other. In the words of Ashby's French admirer:

> As soon as we introduce the idea of functional activity [as opposed to "event"-driven causality], we are able to enter into a realm of thought where all is relative and the concepts themselves are not absolute. Within this realm each of the various systems seeks to attain its own particular state of equilibrium; but all are involved in ever vaster systems which again are themselves in search of equilibrium. Such a complex in a continuous process of equilibratory activity is the central nervous system. (de Latil 1953: 353)

In his books *Design for a Brain* (1952) and *An Introduction to Cybernetics* (1956), Ashby applied his view of "The Organism as Machine" to behaviour as well as to homeostasis. In doing so, he defined a form of negative feedback ("ultrastability") that could account for self-organization from *random* starting points.

He'd illustrated this principle some years earlier, in his analogue machine, the Homeostat—first demonstrated at the meeting of the Electroencephalographic Society on May Day 1948. There was no crawling, no bumping and turning away, no coming at the call of a whistle—in short, nothing to delight the kiddies. This was a machine for theorists.

d. How the Homeostat worked

The Homeostat consisted of four square boxes, each with an induction coil and a pivoting magnetic needle on top (Ashby 1948: 380 ff.). Current changes in a coil would cause its needle to move. But, because of a feedback system, these movements would cause further changes in the current. Each needle carried a wire dipping into a water trough, and the needle movements were affected by the electrical potential of the water at the dipping point. Electrodes were placed at the ends of each trough, providing a potential gradient in the water. And these gradients were influenced by the current generated by the induction coil.

So far, so boring: four magnet–trough–coil units, each embodying feedback. The interest of the Homeostat arose from the fact that these four feedback units were interconnected in a flexible way so that it could maintain the set of needles in a stable equilibrium.

Whenever any one (or more) was deflected, a coordinated activity would return them all so as to point to the centre of the four-box device. This was possible because each box sent its output to the other three (and so received input from them also): "As soon as the system is switched on, the magnets are moved by the currents from the other units, but these movements change the currents, which modify the movements, and so on" (Ashby 1952: 96).

The four main variables of the Homeostat were the positions of the four magnetic needles. Its "essential variable", corresponding to the viable range of blood temperature or blood sugar (for instance), was a maximum value of the current being output by any box. If the outputs of all four boxes fell below this maximum, each would be pointing to the centre.

The feedback links between the boxes could be continually altered by a stepping switch, or "uniselector". If—and only if—the maximum allowable output current from some box was exceeded, the switch would close so as to move the box's uniselector to a different position. This changed the quantities defining the relevant feedback link, so diverted the machine onto a different path through the space of its possible states.

One might even say, instead, that the uniselector *changed one machine into another*, defined by a different feedback function. Ashby himself pointed out that, where there are various levels of dynamical coupling, what we decide to call "one" system is largely a matter of convenience (Chapters 13.vii.b–c and 16.vii.d).

If the parameter values of a single-level Homeostat were to be set inappropriately, it would embark on an uncontrolled "runaway" until the magnets hit the ends of the troughs (Ashby 1952: 96). But in the double-level Homeostat, where second-order feedback ensured that the first-level controls were reset so as to achieve stability, this didn't happen. The parameter values (represented by the positions of the uniselectors) could be randomized, and the Homeostat would still achieve stability. Ashby even outdid Sperry, by crossing the wires from the mains switches so that input became output, and vice versa: again, the machine reached equilibrium.

Since each box's uniselector could take up twenty-five different positions, the system could in principle reach its end point from any one of 390,625 distinct starting conditions. Or rather, it could reach one of the many acceptable end points: the stable state defined as "four needles pointing to the centre" could be implemented by many different states defined in detailed physical terms. However, the Homeostat didn't always equilibrate quickly. In practice, the convergence would sometimes take a very long time. And it couldn't learn. Having reached an acceptable position by chance it stayed there, but its past achievements didn't improve its future performance.

Ashby saw this principle of double feedback as essential to the dynamics of physiological equilibrium and (the brain states underlying) learning and purposive behaviour. And he developed a mathematical theory of ultrastability, pointing out that the Homeostat illustrated only one example of a wide range of possible forms, many of which are found in biological systems (Ashby 1952, ch. 9).

Strictly, he said, a living organism isn't an ultrastable system: rather, it is multistable. In other words, it's a complex system made up of many ultrastable subsystems whose main variables are dynamically linked to each other (Ashby 1952, ch. 16). Ashby distinguished various types of multistability differing (for instance) in the degree and frequency of these subsystem interactions. The body's subsystems, he said, adapt to each other "in exactly the same way as animal adapts to environment" (Ashby 1952: 174).

The biologically typical form of multistability—where subsystems affect each other only weakly, or occasionally, or indirectly—was illustrated by a particular version of the Homeostat. Ashby showed that stability would be achieved even when the number of units in active combination varied from time to time. And he considered the question of just how long it would take for various types of multistable system to adapt successfully to changes in their (internal or external) environment.

In brief, Ashby originated a systematic formal theory of adaptive self-organization in dynamical systems (see 12.ii and 15.vii–viii and xi). As for its explanatory potential, he made sweeping—not to say grandiose—claims about the capabilities of future versions of the Homeostat (Ashby 1948: 383). But he himself acknowledged that they might, in effect, turn out to be black boxes, their behaviour being "too complex and subtle for the designer's understanding" (ibid.).

His more down-to-earth contemporaries were sceptical. Some complained that his stress on communication within the body, and especially the brain (Ashby 1960: 218–24), couldn't immediately be applied to neurology. So the psychophysiologist Karl Lashley, also a Macy participant, pointed out that Ashby's (and McCulloch's) account of the brain involved "a very great oversimplification of the problem", and declared that doing research on the brain itself should take priority over "indulging in far-fetched physical analogies" (Lashley 1951*b*: 70, and quoted in F. A. Beach *et al.* 1960, p. xix).

Detailed application of cybernetic and computational theories to brain function would come only later. This required powerful computers, in place of simple devices cobbled together on a hobbyist's workbench, and much-improved knowledge of the brain (see Chapters 14 and 15.vii).

4.ix. Schism

By the end of the 1940s, there were two ways of thinking about mind as machine. One was symbolic–computational, the other cybernetic–dynamical. (Early connectionism lay on both sides of the fence: see Chapters 12.i–ii and 14.iii.) One might even say that the former focused on *computer and mind*, the latter on *computer and brain.* For as we've seen, the cyberneticists—besides being more interested in the brain than the committed symbolists were—had little or nothing to say about specific mental contents. Adaptation, self-organization, feedback, purpose, reverberating circuits . . . took precedence over (for instance) doing logic or using natural language.

* The two approaches focused on different types of control, favoured different types of mathematics (discrete or continuous–statistical), and took different wartime computers as their artificial exemplars.

* Their technological fruits differed. One led to (digital) symbolic AI, the other to (analogue) control engineering and dynamical forms of connectionism.

* And their historical sources were distinct, as we've seen (see also M. W. Wheeler *et al.* forthcoming). The symbolic approach was nurtured by propositional logic and logicist philosophy of language. Cybernetics was grounded in control engineering, general physiology, reflexology, and information theory—which drew, in turn, on the mathematics of probability.

Nevertheless, the scientists concerned were collaborators agreeing to differ, rather than rivals—still less, enemies. Wiener himself, the high priest of cybernetics, had outlined a digital computer in 1940 (see iv.a, above). A few individuals explored both approaches in detail: McCulloch, for instance, defined neural networks both logically and statistically. And most people communicated willingly, feeling themselves to be involved in the same general project.

This cooperative spirit was still evident in the 1958 meeting on 'The Mechanization of Thought Processes' at the UK's National Physical Laboratory, or NPL (6.iv.b). Indeed, it survived for a few years after that: "Through the early 1960s, all the researchers concerned with mechanistic approaches to mental functions knew about each other's work and attended the same conferences" (Newell 1983: 201).

That happy state of affairs didn't last. The initially convivial mind-as-machine community split into different, even opposing, camps. There were two engines driving this split. The first was fuelled by some of the less admirable features of Everyman, the second by genuine theoretical differences.

a. All too human

By the mid-1960s, two distinct groups had emerged, namely formal computationalists and (what are now called) cyberneticians. For many years afterwards, there was little love lost between them. In other words, the Legend was tarnished, again (1.iii.b).

This splitting was due partly to the availability of two methodologies, involving different types of skill: 'logical' programming, and building or simulating dynamical or probabilistic systems. Both were used to study circular causation, or feedback. And they could be seen as theoretically equivalent at some very fundamental level, for both could be described by Boolean logic and modelled by a Turing machine (or rather, by a Turing machine with time specifications added). But in practical terms they were different.

The importance of tacit practical skills in science is too often forgotten, as though theoretical knowledge alone were enough (but see Polanyi 1958, 1967). The historian Donald MacKenzie, for instance, has criticized the over-quick assumption that atomic weapons can't be "un-invented": a large part of the Manhattan Project involved practical engineering rather than scientific theory, and the former isn't fully preserved in the library books (MacKenzie and Spinardi 1995). Indeed, some critics argue that the importance of hands-on manipulation and other kinds of tacit knowledge is an insuperable obstacle to human-level AI (see 13.ii.b).

In any event, and quite apart from specific theoretical disagreements and different target topics (such as adaptation versus meaning: see below), people who feel comfortable with different methodologies typically feel uncomfortable with each other. They won't even share—or understand—the same private jokes. It's not surprising, then, that the two computational communities grew increasingly distinct in sociological terms too.

The natural territoriality of Everyman, the desire to carve out and defend one's own patch, did nothing to prevent the schism. Far from it. In the mid- to late 1960s Minsky contrasted 'Artificial Intelligence and its Cybernetic Background' like this:

> While work on artificial intelligence draws upon methods from other fields, this is *not* a significantly interdisciplinary area: it has *its own concepts*, *techniques*, and *jargon*, and these are slowly growing to form an intricate, organized *specialty*. (1968: 6; italics added)

He even went on, cheekily, to define Newell-and-Simon *out* of what he was calling "AI":

> The [digital] computer made it practical to be much more ambitious [than the cyberneticians had been]. As a result, cybernetics divided, in my view, into three chief avenues.
>
> The first was the continuation of the search for simple basic principles—most clearly exemplified by the precise analyses of Ashby . . .

> The second important avenue was an attempt to build working models of human behavior incorporating, or developing as needed, specific psychological theories . . . Work in this area—Simulation of Human Thought—has focussed rather sharply at the Carnegie Institute of Technology . . . in the group led by Newell and Simon.
>
> The third approach, the one we call *Artificial Intelligence*, was an attempt to build intelligent machines without any prejudice toward making the system simple, biological, or humanoid . . . Much of the earlier work on artificial intelligence was done by the group at MIT . . . (pp. 7–8)

Eventually, mere difference led to mutual ignorance, not to say emotionally loaded schism. The enmity didn't involve "blood-boiling animosity", as disputes over Noam Chomsky's linguistics later did (9.viii.a). And the leaders didn't "hate each other's guts", as some feuding connectionists would (12.v.b). Nevertheless, the atmosphere was far from congenial.

In technological contexts (such as industrial research laboratories, university engineering schools, and equipment manufacturers) the schism started early. In the late 1940s and 1950s, these institutions constituted "a major source of resistance to the emerging digital paradigm, especially when it came to using the new machines for purposes other than mathematical calculation" (Edwards 1996: 68). One servo-engineer recalled later that analogue computer experts felt "threatened" by digital computers, and that (partly because of the huge post-war demand for control engineers) "only a few" of his peers were able to make the switch. (Besides professional jealousy, part of the problem was the genuine doubt "that a machine containing vast numbers of vacuum tubes could ever function for more than a few minutes at a time without breaking down": Edwards 1996: 69.)

Within cognitive science, where such practicalities could more easily be ignored, the group rivalries and jealousies didn't emerge until the late 1960s. On my visits to the apple orchard in the late 1950s (Preface, ii), and even at early 1960s Harvard, I heard plenty of healthy disagreement—but never (what I often encountered later) champions of one theoretical style mocking, even vilifying, those of the other. At mid-century, there was one heterogeneous research group rather than two homogeneous ones.

The invective appeared later, especially after symbolic AI achieved public prominence—and generous research funding—in the early 1970s. The merciless attack on connectionism by two of the high priests of symbolic AI didn't help (Minsky and Papert 1969). Early drafts of this attack, described by one insider as dripping with "vitriol", had been circulating for several years before the (toned-down) final version was published (see Chapter 12.iii). So there was plenty of time for resentment to grow, followed by a twenty-year funding famine to make it fester.

When the worm finally turned in the 1980s (12.vii), and the funding—and the glamour—was reversed, the invective didn't disappear. Too often, resentment on the part of the 'cyberneticists' gave way to ill-considered triumphalism.

Moreover, the excessive media hype previously attached to symbolic AI was now transferred—more accurately: transferred back again (Chapter 12.ii.g and iii.a)—to the connectionist variety. At each swing of this journalistic pendulum, some group of researchers suffered chronic irritation that often spilt over into personal abuse of those in the other group. The explosion of A-Life in the 1990s also soured the intellectual atmosphere with ungenerous denigration of opponents, not to mention yet more media hype guaranteed to exasperate the 'enemy' (15.ix.a).

One indication of this sorry state of affairs can be found in a recent number of the newsletter of the UK/Europe's oldest AI society. The editorial bemoaned the "many cliques" in the field, and the "frankly insulting" names often used by researchers for approaches different from their own. As the editor put it: "The lack of tolerance [between different research programmes] is rarely positive, often absurd, and sometimes fanatical" (Whitby 2002*b*). Outside academia, he continued, where theory is of less concern than practical efficacy, "different approaches to AI cohabit not only in the same office, but often in the same program" (see Chapter 12.ix.b and d). (For example, the Clementine commercial data-mining system includes *both* the ID3 machine learning program *and* a Kohonen network, *and* some evolutionary computing too: Whitby 2004; Khabaza and Shearer 1995.) These self-serving attacks on other forms of AI have even contributed to today's widespread, but mistaken, view that AI has "failed" (13.vii.b).

Granted, people recommending new (or newly revived) ideas typically overplay their hands. It's a good way of getting attention. But it's liable to backfire. It can cause fruitless squabbling, and unnecessary blindness to what "the others" have seen.

The Harvard sensory psychologist Edwin Boring complained about such behaviour in chastising the New Look psychologists (Chapter 6.ii)—*and* in recalling "Titchener's in-group" of many years before. He'd observed the same "digs" against opposing schools in psychology, the same "unsophisticated and unscholarly and biased . . . aggressions about what [the speaker] did not care to understand" because it didn't come from the relevant in-group (letter to J. S. Bruner, in Bruner 1983: 91). Certainly, he said, in-groups can work "integratively" as well as "disruptively". But reaching the truth would require cooler heads, and less self-serving rhetoric.

The same applies here. If, as I claimed in Chapter 1.ii.a, there are *two* fruitful pathways through the many meadows of cognitive science, then we shouldn't be barred from either of them.

b. Adaptation or meaning?

Besides the all-too-human reasons for schism described above, there were substantive differences too. The two methodologies seemed to line up with two different areas of interest: adaptation (and life) on the one hand, and meaning (and mind) on the other.

To put it another way, the main controversy concerned the interpretation of *purpose*, and the place—if any—for *meaning*. Could Ashby's theory, or even Rosenblueth's, really explain purpose? Perhaps—as Craik had suggested—the modelling of purposive behaviour required internal representations of the goal, understood as symbolic structures in the artefact concerned (10.i.d)? More generally, there was discontent over information theory's flight from meaning (see Section v.d, above). Surely, psychologists must often pay attention to specific semantic content, whether of purposes or beliefs?

For example, Ashby was soon criticized by MacKay (1956*b*) for ignoring "goal-guided" behaviour, which involves some sensitivity to the required direction of action. Ashby's exemplary kitten and the Homeostat were indeed acquiring information that enabled adaptive change, but their actions ("trials") occurred at random. In the Homeostat, for instance, the difference reduction (between actual and permissible levels of output current) was achieved by chance. By contrast, difference reduction in guided missiles offered some teleological guidance as to the most promising line of action.

And hierarchical means–end modelling, soon to be developed in symbolic AI, would provide even more. (Today, half a century later, it's still disputed whether any form of dynamical systems theory can suffice to explain purposive behaviour: see 16.vii.c.)

The division in the mind-as-machine community that was brewing throughout the 1950s was (and still is) due also to differing views on the relation between *life* and *mind.* The cyberneticists assumed that broadly similar sorts of explanation (based in circular causation) are required for biology and psychology. Not only is the mind a machine, but it is essentially the same sort of machine as the living body is.

The computationalists, by contrast, focused mainly on logical–linguistic thinking, problem solving, and purposeful planning. When they studied non-linguistic capacities, such as vision and locomotion, they assimilated them to logical thought (Chapter 10.iv.b). But they didn't assume that *metabolic* systems work by logic. On their view, then, the human mind is a machine that differs significantly from the biological machine in which it's embodied.

For the next four decades, most—though not all—theorists who took *mind as machine* as their guiding theme concentrated on the non-biological issues. Their ambition was to further AI and computational psychology, not A-Life or neurophysiology. The early cybernetic work was either ignored or assimilated to connectionist AI.

Even when Turing and von Neumann themselves turned (in the early 1950s) to discuss life, few mind-as-machine researchers listened. Partly, this was because the lack of computer power didn't enable their ideas to be explored much further (see 15.iv–v). But the prime reason was that such researchers were interested in more evidently *psychological* (including linguistic) problems, which they addressed by way of symbolic and/or connectionist AI.

Connectionism, in turn, soon faltered, not rallying until the 1980s (Chapter 12). Meanwhile, in the 1960s–1970s, symbolic AI held court (Chapters 7 and 10). Only in the 1990s would ideas of *life* and *mind* be prominently drawn together again (15.ix and 16.x).

In sum, the "logical" side of the mind-as-machine divide gained the ascendancy around 1960 because it was better fitted to deal with goal-directed action and propositional meaning. The newly available digital computers could symbolize an indefinite variety of thoughts. (Whether these were *real* "meanings" wasn't, then, much discussed: see 16.v.c.) In psychology, linguistics, AI, and philosophy, the 1960s were largely logicist. Even neurophysiology was emphasizing matters, and models, more logical than dynamical. Explicitly logicist definitions of 'cognitive science' flourished (Chapter 16.iv).

But the dynamical stream was still flowing, underground. It would resurface in the last twenty years of the century, first with connectionism (Chapters 12 and 14) and then with A-Life (Chapter 15). The dual theoretical heritage of cognitive science, and the see-sawing ascendancy of one side or the other, is a central theme in its history.

5

MOVEMENTS BENEATH THE MANTLE

Sir Isaac Newton was not a nice man. His personal unhappiness (Manuel 1968, ch. 3) often fuelled intemperate attacks on others, including his social inferiors. One such was his Royal Society colleague Robert Hooke. Although Hooke was a scientific rival (with a competing theory of light), and the first to suggest (in 1679) that the planets move under some influence inversely proportional to the square of their distance from the sun, he was menially employed as a technician (Pumfrey 1991; Shapin 1994: 395–403). Newton rarely minced his words in criticizing him, and many others of his acquaintance too.

Yet Newton is now widely regarded as a model of magnanimity, because of his oft-quoted remark: "If I have seen further it is by standing on the shoulders of Giants." He said this in a letter to Hooke, one of several written at that time in which he fulsomely complimented his long-time adversary (Manuel 1968, ch. 7).

These compliments weren't intended seriously, however. Indeed, Newton's apparent modesty conveyed a venomous personal insult. His remark was a commonplace, dating back five centuries to John of Salisbury—for whom it was already second-hand:

> Bernard of Chartres used to compare us to [puny] dwarfs perched on the shoulders of giants. He pointed out that we see more and farther than our predecessors, not because we have keener vision or greater height, but because we are lifted up and borne aloft on their gigantic stature. (John of Salisbury 1159: 167)

John's words had often been quoted—for instance by the translators of the 1611 King James Bible, defending their reuse of previous translations if these weren't clearly wrong (McGrath 2001: 176). So Newton's contemporaries, including Hooke himself, would inevitably be reminded of puniness and dwarfs.

—And the punchline? The unfortunate Hooke was a tiny hunchback, described by an acquaintance as "[physically] but despicable, being very crooked . . . [and] but low of Stature, tho' by his limbs he shou'd have been moderately tall" (Manuel 1968: 137; cf. L. Jardine 2003). Magnanimity, this was not.

But if Newton was no *moral* exemplar, perhaps he exemplified good *science*?

David Hume certainly thought so: he explicitly modelled his 'scientific' psychology on Newton's laws of physics (see Chapter 2.x.a). The nineteenth-century associationists apparently agreed. So, too, did the twentieth-century behaviourists, who dominated American academic psychology for over forty years.

Behaviourism acted as a rough-spun mantle, thrown hastily over the body of psychology (and neurophysiology) and hiding it from view.—*Very* hastily: the 35-year-old John Watson was elected President of the American Psychological Association (APA) only two years after publishing the first behaviourist paper (Watson 1913). To be sure, the mantle eventually became less rough-spun. By the 1950s, behaviourism was much more sophisticated. But it was still obscuring other psychological ideas.

Admittedly, that wasn't true of Europe: one European psychologist, recalling the 1960s, described behaviourism as "completely absent from my horizon" (see 6.iv.d). But in the USA, where most professional psychologists were based, things were very different. George Miller, interviewed a quarter-century later, said: "The power, the honors, the authority, the textbooks, the money, everything in psychology was owned by the behaviorist school" (G. A. Miller 1986: 203).

Cognitive science arose when the mantle was shrugged off. If there was a cognitive "revolution" in the late 1950s, it consisted in the overthrow of the core assumptions of behaviourism—listed in Section i, below.

That overthrow was due in part to new ideas. These came from mid-century psychoneurology, and from late 1950s AI, linguistics, and informational psychology (see Chapter 6). But it also involved older ideas, already moving more strongly beneath the mantle as mid-century approached. These psychological concepts, highly unfashionable during behaviourism's heyday, would later—in some cases, only much later—play a positive role in cognitive science.

Those first-half-century approaches are outlined here. So Section ii discusses relevant theories of personality, cognition, and biological aspects of behaviour. Some anticipatory "softening" of the behaviourist orthodoxy is described in Section iii. Finally, the entry of neurology is reviewed in Section iv.

In short, this chapter concerns pre-computational work. The only person discussed at length, namely Donald Hebb (in Section iv.b–f), would later be hugely important for three areas of cognitive science: psychology, neuroscience, and connectionist AI/A-Life. But he wasn't a computationalist. So, apart from a quick peep over the parapet in Section iv.f, man-as-machine won't raise its head until Chapter 6—which describes the excitement that ensued when a new choreography was offered to describe the movements going on beneath the mantle.

5.i. Newtonianism

The cognitive revolution can't be understood, or evaluated either, unless one's aware of what it was reacting against. In one word, this was behaviourism. But more than one word is needed to explain just what it was about behaviourism which was challenged by cognitive science.

a. The six assumptions

Behaviourism was 'Newtonian' in six ways:

First, it was atomistic. The key theoretical categories, *stimulus* and *response*, were thought of as separately identifiable and unstructured, like a point mass.

Next, it was associationist. Stimulus and response were linked by contiguity, not structure. (Think billiard balls.)

Third, it was universalist. Where Newton had assimilated falling apples to orbiting planets, behaviourism posited psychological laws applying across all domains and many species. So people were said to learn language in much the same way as laboratory pigeons learn to play ping-pong—and, a fortiori, in much the same way as they learn table manners (see 8.vi.b).

Fourth, it was externalist. Everything the creature does, apart from a few inbuilt reflexes—like showing fear at loud sounds, as baby Albert B. famously did (J. B. Watson and Rayner 1920), was attributed to environmental influences. Left to itself, nothing would change—like a physical mass with no external force to act on it. Nativism (the claim that some psychological abilities are innate) and self-organization, then, were alike anathema.

Fifth, it was meaning-less. Much as point masses have nothing to do with meaning, so "stimulus" and "response" were operationally defined as *non-intentional* (non-mentalist) categories.

And last, it avoided hypotheses about hidden causes. Newton famously refused to speculate on the cause of gravity: "Hypotheses non fingo." The previous "mechanical" philosophy had seen physics in terms of causation by contact (see Chapter 2.ii, preamble). Similarly, behaviourists treated the organism as a black box. (For the record, some scholars believe Newton meant not "I offer no hypotheses", but "I'm not inventing fictions": P. M. Williams, personal communication.)

The behaviourists didn't doubt that neural mechanisms cause observable behaviour. Indeed, Ivan Pavlov and his fellow reflexologists were adopted as patron saints. But they themselves said almost nothing about the brain. Very little was known about it, in any case (see Chapter 2.viii).

They also avoided talk of mental causes. This was due less to philosophical worries, such as those expressed by Princess Elizabeth (2.iii.b), than to the practical difficulties—already familiar by the turn of the century—in getting reliable introspective reports from laboratory subjects. And dumb animals, of course, couldn't offer such reports anyway. The notion that there may be non-introspectible (non-conscious) mental processes was given even shorter shrift. As for meaning, purpose, and intentionality, these were mentalistic notions to be avoided at all costs. Minds were out, brains were ignored, behaviour was all.

As a result, even perception—surely, or so one might think, a mentalistic concept—was studied in fully objective, non-cognitive terms. And thinking was *hors de combat*. Jerome Bruner remembers the mainstream "high status" textbooks thus:

> When I was a graduate student, Woodworth's *Experimental Psychology* [1939] was *the* book . . . . It boasts 823 pages of text. By a generous count, the topic of thinking is treated in two brief chapters . . . (including animal studies) . . . All told, 77 pages. When the prestigious *Handbook of Experimental Psychology* appeared in 1951 . . . it had 1,362 pages. This time, the topic was disposed of in a chapter called "Cognitive Processes": 27 pages. Add a few to that, for George Miller had a chapter on "Speech and Language", the last few pages of which were devoted to the relation of thought and language. The mind was not doing well in psychology. The eye, ear, nose and throat fared far better: nine chapters, about four hundred pages. (Bruner 1983: 106–7)

It will be helpful (I hope!) to bear my sixfold sketch of behaviourism in mind over all of the following chapters. But it's no more than that: a sketch. It isn't "the whole truth".

It isn't even "the truth and nothing but the truth". For the behaviourists didn't always follow their own diktats, as we'll see in Section iii.b below. The Harvard psychologist Burrhus Skinner (1904–90) came closest. His departmental colleague Edwin Boring (1886–1968), in his magisterial *History of Experimental Psychology*, described him as dealing with "the empty organism", because his theory was empty of neurones and almost empty of "intervening variables" (Boring 1957: 650). But even Skinner was covertly non-Newtonian in some ways (iii.a below, and Chapter 9.vii.b).

All six behaviourist assumptions were questioned by the cognitive revolution. Computational psychology did offer causal hypotheses. These were couched in mental/computational terms. And even in the 1940s, some were also interpreted as brain processes, broadly defined (see Chapters 4.iii.e and 12.i.c, and Section iv below). Eventually, more detailed theories of neural computation would emerge (Chapter 14).

b. What sort of revolution was it?

One might say that the cognitive revolution was made "official" in 1960, when the President of the APA used the term in the title of his Presidential Address (Hebb 1960). But what sort of revolution was it?

One shouldn't describe this anti-Newtonian rebellion as a *Kuhnian* revolution, though people sometimes do (O'Donohue 1993). The strongest point in their defence is that, by the 1950s, the most influential behaviourist theories were seemingly lost in untestable speculations about hidden Ss and Rs (see iii.b, below). In other words, behaviourism was running into trouble *even in the eyes of its sympathizers*—a state of affairs which Thomas Kuhn (1922–96) saw as the characteristic prelude to a "paradigm shift" (Kuhn 1962).

Perhaps that's why, as one psychologist remembers:

> It was just a tremendously exciting time [in 1968]. The basic assumption was that things were boiling over, and of course there was going to be a lot of argumentation and debate, but a new day was coming. And of course, everybody toted around their little copy of Kuhn's *The Structure of Scientific Revolutions*. (Jenkins 1986: 249)

However, the "new day" wasn't bringing wholly new ideas. Many of the ideas central to cognitive science had been around for years in continental Europe and the UK (see Section ii). They'd been spurned by the behaviourists—but, as George Mandler (1924–) has pointed out:

> American psychologists sometimes fail to understand that behaviorism was a *very* parochial event . . . [The] reaction of the Europeans during the '30s, '40s, and early '50s when [Americans] suffered through behaviorist orthodoxy, was "What is going on over there?" They paid very little attention to it. (G. Mandler 1986: 259)

In other words, the "revolution" could almost be seen as a *counter-revolution*. Mandler, himself a refugee from the old world, was struck less by the newness of these ideas than by their familiarity: "The so-called 'cognitive revolution' . . . was not so much a revolution as a return to an *ancien regime*" (2002*c*: 256).

Just what Kuhn meant by a scientific paradigm is rarely considered with care. (And we can't do so here: there were no fewer than twenty-one senses in his book, not all mutually compatible—Masterman 1970.) Broadly, however, it refers to some unquestioned base of assumptions (and preferred methodologies), accepted—rather than argued—by everyone in the field concerned.

So, as Mandler pointed out, behaviourism wasn't a paradigm for mid-century European psychologists. Cognitive science isn't a paradigm either. Even *within* cognitive science, the "argumentation and debate" hasn't finished. There's still fundamental disagreement—which is why, in Chapter 1, we couldn't simply launch straight into our story. As for professional psychology as a whole, many researchers still start from assumptions very different from those of cognitive science (see 7, preamble, and 7.vii.e).

Early computational psychology reconnoitred *both* of the two mind-as-machine footpaths (Chapters 1.ii.a and 4.ix). In the late 1950s, a single volume of the *Psychological Review* carried seminal papers scouting each route (Newell *et al.* 1958*a*; Rosenblatt 1958). Initially, the logical footpath was explored more fully. This was partly because it seemed more apt for representing meaning (Chapters 4.ix.b and 16.v.c), and partly because the cybernetic scout soon met a major obstacle on the way (Chapter 12.iii). Later, as psychological theories of self-organization blossomed, the cybernetic pathway became well-trodden too.

So the passage from behaviourism to cognitive science *wasn't* a shift from one Kuhnian paradigm to another. (Psychologists aren't the only ones over-ready to use the popular concept of paradigm: I'm reminded of the newspaper cartoon showing a newly hatched chick crying "Wow! Paradigm shift!")

The term *revolution*, however, is more apt—and it's widely used (e.g. de Mey 1982; Bruner 1983: 277; H. Gardner 1985; Baars 1986; D. M. Johnson and Erneling 1997). To be sure, one of the prime revolutionaries—Miller, whose frustrations with behaviourism were quoted above—insists that it wasn't really a revolution, but an "accretion" of old ideas to newer ones (Miller 1986: 210). This is much the same point as I made in speaking of a counter-revolution. For that there was *significant change* is incontestable.

Besides the accretion of new (computational) ideas, the empiricist psychological assumptions that had guided Hume and his intellectual successors for over 200 years were abandoned *by some of those most sympathetic to them*.

That's not to say that all six core assumptions were dropped at once. Universalist associationism is still robust today—though recently dubbed "the phlogiston of psychology" (14.ix.g). But many empirically oriented people adopted a new way of doing psychological science.

5.ii. Psychology's House

In the late 1950s, when cognitive scientists first entered the house set aside for Psychology, there were already many tenants lodging there. The most comfortable suites held the behaviourists—often unaware of the other inhabitants ("For a while in the 1920s it seemed as if all America had gone behaviorist"—Boring 1957: 645). But the less-favoured rooms were long-occupied too.

The longest-established sitting tenants were the Freudians, Gestalt psychologists, Piagetians, and ethologists. More recent entrants included the first Gibsonian psychologists, and the Third Force personality theorists. The once-fashionable William McDougall had been banished to the attic, but his grumbling could still be faintly heard.

Some early cognitive scientists took the sitting tenants seriously from the start. Others didn't. Eventually, most came to recognize the insights of these older traditions.

The more we consider mid-century *American* researchers, the truer this is. For behaviourism (as already remarked) was an American malady, not a European one. Mandler remembers being "asked, in all seriousness", by a senior British psychologist in 1965 "whether anybody in America really believed any of the behaviorist credo" (2002*a*: 344). And he adds:

> The important aspect of European psychology of the time was that not only was Europe *essentially unaffected and uninfluenced by behaviorism*, but also that *the developments in Europe became part of the American mainstream* after the decline of behaviorism. (pp. 343–4; italics added).

Long before the late 1950s, when American behaviourism first came under concerted fire, ideas which today would be called 'cognitive' were flourishing—indeed, dominant—in Britain, Germany, France, and Canada. That's why psychologists from those countries feature prominently in this chapter.

a. Sitting tenants with personality

When the early cognitive scientists were first given house room, they had no wish to invite the behaviourists to tea. For behaviourism was defined as "the enemy" (see Chapter 6.iv.d). But many were also loath to invite the other inhabitants.

For instance, in the 1960s–1970s neither Gestalt psychology nor Piagetian theory was taught in the Experimental Psychology group at the University of Sussex—at that time, the best psychology department in the UK. The group was headed by N. Stuart Sutherland (1927–98), one of the first British psychologists to embrace cognitive science. There was no question, however, of his embracing earlier thinkers whom he regarded (rightly) as less than rigorous. So he chided me on my arrival in 1965 for "wasting my time" on McDougall, and said much the same twelve years later when I was writing on Jean Piaget (Boden 1972, 1979).

As for Sigmund Freud, his name was mud. Sutherland wrote a passionate, part-autobiographical, attack on Freudian analysis as a clinical tool (1976). This attracted huge attention: it was extracted in the Sunday newspapers, went into several revised editions over the next twenty years, and was even made the basis of a West End (and off-Broadway) play.

One may share (as I do) most of Sutherland's scepticism about Freudian theory and therapy. Not least, one may deplore Freud's outrageously unscientific "evidence" for the Oedipus complex. This was based largely on leading questions about a little boy's dreams about giraffes—which weren't even posed to the child by Freud himself, but fed to him through the father (S. Freud 1909). Nevertheless, five aspects of Freud's work are highly relevant for cognitive science:

* First, he focused on particular intentional phenomena, or propositional attitudes: highly specific meanings, purposes, beliefs, anxieties, hopes . . . and so on.

* Second, in doing this he indicated the hugely subtle complexity of the semantic networks and associative/inferential principles in human minds.
* Third, he integrated cognition with motivation and emotion, suggesting how beliefs affect our purposes and anxieties—*and vice versa*.
* Fourth, he taught that one can't understand adult cognition without knowing how it developed. (Note that this is different from saying that "how it developed" is interesting in itself.)
* And fifth, he tried to specify the mental structures and processes involved.

For examples of mental structure, consider his views of memory and motivation. In *The Interpretation of Dreams* (1900, esp. ch. 7) and *The Psychopathology of Everyday Life* (1901), he presented memory not as myriad associative strings of meaningless beads, but as a richly complex structure. It was organized, he assumed, in terms of semantic and phonetic dimensions (think puns—and see 13.iv.c), as well as of contingent associations specific to the individual (think madeleines). As for motivation, his distinction between id, ego, and superego was a broadly sketched account of what cognitive scientists would later call the architecture of the mind. Indeed, Marvin Minsky, among others, has drawn on Freud's ideas in formulating his computational theory of mental structure (Chapters 7.i.e and 12.iii.d).

Examples of mental processes included the unconscious "mechanisms of defence", first mentioned by Freud in the mid-1890s and elaborated later. These were hypothetical transformations of one belief into another. Which one would be brought into play at a given time depended on the emotional circumstances: the level of anxiety caused by the superego. They might be evidenced as verbal reports and/or 'free associations' by the patient. Or they might show up (for instance) as stuttering and/or hesitations in speech (see 7.ii.c), obsessional hand washing, or hysterical paralysis (see Preface, ii).

Just how many defence mechanisms Freud believed there to be isn't clear. (There's even disagreement about the relation between "defence" and "repression"—Madison 1961, esp. 3–30.) His daughter Anna listed ten, giving clear examples of each: repression, regression, reaction formation, isolation, undoing, projection, introjection, turning against the self, reversal, and sublimation (A. Freud 1937, ch. 4). Eight more, not specifically listed by her, are often cited: denial, displacement, splitting, fixation, condemnation, neutralization, intellectualization, and rationalization (Rycroft 1968).

Most of these concepts have entered into common parlance, and many examples will spring to mind. What's less likely to spring to mind, because it's not widely realized even within cognitive science, is that some of the earliest "computer simulations" modelled the selection and application of Freudian defence mechanisms (see Chapter 7.i.a and c).

The defence mechanisms were supposed to describe *how the mind works*. Freud was quite clear about this. He saw them as "the corner-stone on which the whole structure of psychoanalysis rests" (S. Freud 1914: 16). And he claimed, in turn, that "Psychoanalysis is a part of psychology; not of medical psychology . . . but simply of psychology" (1926, postscript). In other words, his account of *personality* had deep implications for *general psychology*.

Admittedly, there were strong whiffs of homunculism. For id, ego, and superego were treated as little minds, whose functioning was taken for granted in a way that's impossible today. But Freud did attempt to specify some of the purpose-directed processes involved in memory, belief fixation, and emotional equilibrium.

What's relevant here is that he tried. Whether what he said about repression was true is another, hotly contested, question. (So, too, is whether it was original. The debts Freud owed to Friedrich Nietzsche, in particular, were many: Ellenberger 1970, ch. 7.)

Some critics argue that Freud's use of 'repression' was unfalsifiable (Cioffi 1970), others that there's empirical evidence both pro and con (e.g. Kline 1972; Grunbaum 1984, 1986; Farrell 1981). Yet others hold that genuine "evidence" simply isn't obtainable (K. M. Colby and Stoller 1988, chs. 5–6). And some practising analysts reject Freud's bizarre "hydraulic" concept of mental energy, but retain his views on mental structure (Colby 1955). So those cognitive scientists today who draw inspiration from Freud's work certainly aren't committed to an uncritical acceptance of his theories.

Another theorist of personality who was seeking underlying psychological causes was McDougall (2.x.b). He was hugely influential for many years, because of his pioneering books on physiological and social psychology (1905, 1908), and on the philosophy of psychology too (1911). His work on purposive behaviour in animals would inspire some core theoretical concepts formulated by the ethologist Konrad Lorenz (see ii.c below). And he was well known also for his part in the first systematic cross-cultural study, the 1898 anthropological expedition to the Torres Strait (Herle and Rouse 1998, esp. ch. 6).

Clearly, then, he was a man of parts—and a significant thinker. Eventually, he was headhunted from across the seas. He moved from Oxford to accept the Chair of Psychology at Harvard in 1920.

Unfortunately, that was just a few years too late. The youngsters there, behaviourist to a man (*sic*) since Watson's hijacking of American psychology in 1913, didn't respect him. Nor he, them: he dismissed behaviourism as "a most misshapen and beggarly dwarf" (1923, p. ix). In addition, he made enemies because of his obstinacy and arrogance, and got into hot water as a result of "incautiousness in lectures on public affairs" (Eysenck *et al.* 1975: 637).

Disaffected, he soon moved to Duke University, where he founded the parapsychology laboratory, with Joseph Rhine as his first assistant. Later, he taught the young Bruner. Indeed, Bruner left Duke with his mentor's warning against the "corruptions" of behaviourist Harvard ringing loudly in his ears (see Preface.ii). "Only Oxford was worse, he said" (Bruner 1983: 31). The warning worked: Bruner never succumbed to the American orthodoxy (see Chapter 6.ii).

Despite the scorn heaped on him by the behaviourists, McDougall's theory of personality was evaluated at mid-century as

> a systematic treatment of conation and affection [i.e. motivation and emotion] that, in completeness and thoroughness, is without a rival, and in penetration is second only to the work of Freud. (Flugel 1951: 277)

No serious critic was prepared to accept his views on psychic energy, which were even more bizarre than Freud's (2.x.b). But his account of mental architecture (both normal and abnormal), and of the dimensions that distinguish one individual personality from another, was highly suggestive.

McDougall described the mind as a "hierarchy", or "colony", of *sentiments*. These were ultimately based in the dozen or so "instincts" common to all human beings, and were controlled—in normal personalities, at least—by the "master sentiment of self-regard" (McDougall 1926; Boden 1972, chs. 6–7). The cognitive and motivational unity of the mind depended not on some mysterious Cartesian "self", but on the control exercised by the master sentiment. By the same token, mental *dissociation* occurred when one or more of the lower-level sentiments became to some degree autonomous.

Minor cases of dissociation, he said, express themselves merely as temporary confusion or absent-mindedness. But deeper and/or longer-lasting dissociations give rise to pathological states of various kinds. These ranged from fleeting fugues to persisting personality disorders.

For instance, if the master sentiment temporarily loses control of some member of the mental colony, the result may be a clinical automatism (where the person has *no* consciousness of their strange behaviour). Alternatively, he said, if the master sentiment fails to develop properly, we may see what's called multiple personality. In such cases, there appear to be two or more persons inhabiting one human body, with different interests and beliefs—and, sometimes, non-reciprocal co-consciousness between them.

On the Cartesian view, multiple personality is simply impossible. Irrespective of *why* it arises, that it can occur at all is profoundly mysterious (N. Humphrey and Dennett 1989; Boden 1994*d*). Indeed, postmodernists today use the inner diversity of every 'normal' self, as well as its pathological extremes, as a stick with which to beat Cartesianism in general (see 13.vi.e). But while they acknowledge the diversity of the self, they don't explain it—still less, offer a scientific account of it, which is what McDougall was trying to do. With hindsight, his explanation can be seen as an anticipation of Ernest Hilgard's (1977/1986) neo-dissociation theory, and even of current computational views on this clinical syndrome (7.i.h).

A sentiment, he explained, is "a system in which a cognitive disposition is linked with one or more emotional or affective conative dispositions to form a structural unit that functions as one whole system" (1908: 437). Each sentiment is centred around some intentional object, whether concrete or abstract. For example: "the sentiment of love for a child, of love for children in general, of love for justice or virtue" (1908: 140). And that cognitive core could grow into "an extensive system of abilities (a system of knowledge or 'ideas' concerning that object)" (1932: 223). Most sentiments reflect the person's place in a particular social group or culture. This is especially true for the political and religious sentiments (see Chapter 8.vi).

In sum, the idiosyncratic purposes, life goals, cultural interests, and personal dilemmas of Tom, Dick, and Harry are generated by organized emotional/cognitive structures. These develop, grow, and sometimes decline or decay, as the individual life progresses. Abnormal psychological states—of which McDougall had seen a great many, as a psychiatrist in and after the First World War—are due to the unusual content and/or organization of these everyday mental structures.

From the point of view of cognitive science, it's interesting that McDougall saw the "structure" of the mind—what's now called its computational architecture—as being abstract/functional, not physical. It's comparable, he said, to the "structure" of a poem or of a society; and he described the internal processes of control and communication in non-physical terms. Specifically, he spoke of "telepathy" between homuncular "monads" comprising the "society" of mind. Predictably, his more tough-minded readers were put off, not to say appalled. But since the vocabulary of information-processing and/or programming wasn't yet available, these dubious concepts and Leibnizian language (Leibniz 1714) were as near as he could get (see Boden 1972: 248–55; 1994*d*).

Surprising as it may seem, given his contempt for behaviourism, McDougall was the first person to define psychology as the study of behaviour (1908: 13). Clearly, however, he was no Newtonian. For him, behaviour was *by definition* purposive. Even in his account of perception, he stressed the organism's activity, goal-directedness, and (largely innate) organization. And his theory of personality, like Freud's, posited hidden causes galore—all integrated within a complex psychological structure.

In the early 1960s, the most recent occupants of the wing devoted to personality theory were the Third Force psychologists. The most prominent were Abraham Maslow, Gordon Allport, Ronald Laing, Carl Rogers, and Rollo May. They had varied (mostly neo-Kantian) philosophical roots: existentialism, phenomenology, Gestalt, organismic, and humanist psychologies. Nevertheless, they comprised an increasingly "total, single, comprehensive system of psychology" (Maslow 1962, p. vi).

Their central theoretical commitment was to human freedom (see 7.i.g). Their interest was not to support a quasi-mystical picture of human beings, but to insist that conscious choices *matter*: free will *makes a difference*. Accordingly, they all favoured "proactive" rather than "reactive" approaches in psychology—that is, theories stressing spontaneous, self-directed, and future-oriented behaviour (G. W. Allport 1960). And they all rejected Freud, whom they saw as backward-looking and reductionist, and behaviourism too. (Hence the "Third" Force.)

They were much closer in spirit to McDougall. For they saw personal growth into the future as an important theoretical dimension, and they emphasized the role of purposes, hopes, aspirations, and creativity in the personal life. Every human being, they said, is a self-determining subject with an idiosyncratic experience of him/herself and of the lived-in world. We need to appreciate this if we're to understand people's lives, and especially if we're involved (on either 'side') in clinical therapy.

While computational psychology was still no more than a hope or aspiration, the Third Force was highly fashionable in clinical psychology—and in the world outside. Laing (1927–89), in particular, became a guru for the young and radical worldwide. From his relatively sober critique of orthodox psychiatry (R. D. Laing 1960), to his later—and far more questionable—recommendations of schizophrenia as a way of getting *closer* to reality (e.g. R. D. Laing 1967; Laing and Esterson 1964), his maverick approach was hugely popular within the counter-culture of the 1960s and 1970s.

The uncritical acceptance of his ideas sometimes led to tragedy, as when a husband—advised by Laing himself—refused to yield to his suicidal wife's pleas for electro-convulsive therapy, a method which (though admittedly invasive and ill-understood) had helped her in the past (D. Reed 1976). And, over the years, Laing's increasingly radical politics would frighten many people off. Nevertheless, the psychological importance of the self-image, and of the recursive layers of perception linking family members, was now evident. And it could even be captured in an ingenious multilevel questionnaire (R. D. Laing *et al.* 1966).

(Gregory Bateson, of course, had been thinking about such issues since his days at the Macy conferences: 4.v.e. But he'd had less influence than Laing, largely because his writings were less intelligible: see Harries-Jones 1995, esp. chs. 2 and 8.)

For the Third Force, it was no more false to think of people as machines than to think of them as biological systems. Laing himself, for instance, said, "I am not here objecting to the use of mechanical or biological analogies as such, *nor indeed to the intentional act of seeing man as a complex machine* or as an animal" (1960: 21; italics added). But they regarded machine analogies as likely to be even more misleading than biological theories, because most people assume that machines can offer no purchase for everyday psychological concepts—especially the concept of *freedom*.

If by "machines" one means cars or jet engines, that's true. Whether it's true for computers is discussed in Chapters 7.i.g and 16. But it's worth noting here that the first computationalists to enter Psychology's House included several who *did* believe that this new technology could help us understand personal life (see Chapter 7.i.a–c). It's worth noting, too, that the person who first named "cognitive psychology", then describing it in computational terms, was an ex-colleague of Maslow (see 6.v.b).

In short, and *despite* the counter-culture's love affair with the Third Force, this movement wasn't so deeply incompatible with cognitive science as most people assume.

b. Sitting tenants with knowledge

The other long-time lodgers in Psychology's House weren't much interested in personality. They were more concerned with the nature and/or development of knowledge—sometimes, with a strong leaning towards biology (see subsection c). (So a *cognitive* psychology doesn't have to be *computational*.) But they, too, were non-Newtonian in that they posited internal processes and structures, and emphasized meaning.

The reason why the Gestalt psychologists stressed structure, holism, and meaning is that they were hugely influenced by neo-Kantian philosophy (see 2.vi). The influences went in both directions: Maurice Merleau-Ponty's philosophy of mind relied heavily on their research (16.vii.a). But where the Naturphilosophen had merely theorized, they reported a host of intriguing experiments.

They showed, for example, that how people perceive the parts of a visual stimulus may depend on the structural organization of the whole. So in 'figure–ground' reversals, a stimulus part can be seen either as a chin or as the indentation at the base of a moulded vase (see Figure 5.1). In other cases of "restructuring", a stimulus part previously seen as a young woman's chin is seen as an old hag's nose (see Figure 5.2).

Structure, and restructuring, affected problem solving too. For instance, Max Wertheimer (1880–1943) asked children to find the area of the parallelogram, not by applying some pre-learnt—and perhaps ill-understood—formula but by their own creative ("productive") thought. He paid attention not just to the final results but also to their remarks along the way. And he explained their thinking in an explicitly non-Humean (i.e. non-Newtonian) way (1945: 9). It depended not on "habit" or "past experience", but on

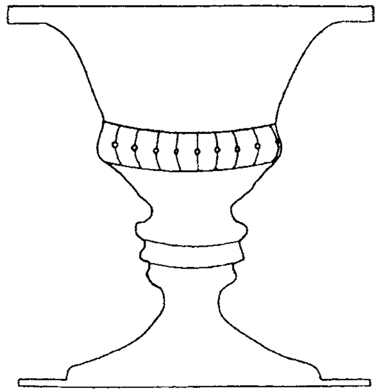


Fig. 5.1. Figure-ground ambiguity: one vase or two faces? Reprinted with permission from Gregory (1966: 11)

Fig. 5.2. E. G. Boring's object-ambiguous figure. Reprinted with permission from Gregory (1970: 39)

[spatial] *grouping*, *reorganization*, *structurization*, operations of dividing into sub-wholes and still seeing these sub-wholes together, with clear reference to the whole figure and in view of the specific problem at issue. (Wertheimer 1945: 9, 41)

(If this quotation reminds you of early AI work on problem solving, that's no accident: see 6.iii.) These Gestalt principles were applied to several historically famous problem solvers. One was Albert Einstein—with whom Wertheimer had spoken, from 1916 on, "for hours and hours" and "in great detail" about "the concrete events in his thought" (1945: 168).

Wertheimer's student Karl Duncker (1903–40) didn't try to emulate the interviews with Einstein. He excused himself, saying: "[Although] a thunderstorm is the most striking example of electrical discharge, its laws are better investigated in little sparks within the laboratory" (1945, p. v). So, in the early 1930s, he asked people to solve this mundane practical problem:

> Given a human being with an inoperable stomach tumor, and rays which destroy organic tissue at sufficient intensity, by what procedure can one free him of the tumor by these rays and at the same time avoid destroying the healthy tissue which surrounds it? (1945: 1)

He recorded the detailed "protocols" of his subjects, asking them to "Think aloud" and "emphatically [warning them] not to leave unspoken even the most fleeting or foolish idea".

Taking seriously every detail of these protocols, Duncker analysed the problem-solving process as a whole. He asked how his subjects managed to come up with a variety of tentative solutions; how they recognized impracticalities and dead ends; what made them switch to other alternatives; and how they finally arrived at the solution—which was to focus two or more *weak* rays on the tumour. (So, as he pointed out, the phrase "at sufficient intensity" in his statement of the problem was a huge clue.)

Duncker identified a number of general "heuristics", or solution procedures. These included adapting the solution to some similar problem solved in the past, and asking "'*Just why doesn't it work?*' or '*What is the ground of the trouble (the conflict)?*'" (1945: 21). Heuristic methods, he said, aren't properties of the solution itself, but "ways" to find it: "They ask: 'How shall I find the solution', not: 'How shall I attain the goal?'" (1945: 24). Moreover, he analysed the subject's thinking as a tree (with tree diagram) of goals and sub-goals, and compared the "family trees of two solutions" (1945: I. 6 and 13). He also marked different "functions" (operations) for achieving different things. For instance, a *lens* can be used to *focus* rays, and *swallowing* can *convey something to the stomach*.

All of this would eventually be reflected in early AI. What John Haugeland (1985: 112) has dubbed "Good Old-Fashioned AI", or GOFAI, not only adopted a particular form of computer modelling: sequential, symbolic, programming. Also, it included the psychological claim that our thinking *is* symbol manipulation of this general type. As we'll see in Chapter 7.iii, GOFAI was partly inspired by the findings and theories of the Gestalt psychologists.

One anti-associationist implication of Gestalt psychology was that the perceiver, as well as the problem solver, is *active*. The phenomenal percept is constructed by the mind, according to its own principles of organization, or "good form"—such as proximity, similarity, symmetry, and closure. I say "by the mind", for the Gestaltists mostly spoke in psychological terms. However, they believed that isomorphic physiological processes—continuous field-effects—were going on in the brain. (That theory was eventually disproved by inserting gold foil in the brain, to disrupt electric fields: Lashley *et al.* 1951.)

Another anti-associationist implication was the stress on *meaning*. They showed that ecologically important meanings can be constructed from highly artificial stimuli.

One example—described in 1912 by Wertheimer—was apparent movement: if two spots at nearby points are shown with a very brief time interval, they're perceived as *one spot moving*. Another was the perception of cause. Albert Michotte (1881–1965) found that if one spot moves to touch another, which then moves, people (given certain timing relations) see this as the first *causing* the second to move (Michotte 1963). A third involved social perception: Fritz Heider (1896–1988) showed that cartoons of moving triangles, squares, and circles were sometimes perceived as agents chasing, fleeing, threatening, fighting, and embracing each other (Heider and Simmel 1944). Similar effects, he said, occur in everyday life: we don't *infer* that someone snarling and clenching their fists is angry, but *see them as* angry (Heider 1958).

The "effort after meaning" was a prime theme of the British Gestaltist Frederic Bartlett (1866–1969), Kenneth Craik's mentor at Cambridge. A student of the neurologist Henry Head (Chapter 4.vi), he generalized Head's concept of the "postural schema" to *conceptual* memories. This work was immediately influential in British psychology (Oldfield and Zangwill 1942–3; Broadbent 1970*a*,*b*). Nevertheless, even his fellow countrymen later came to shun its vagueness. In 1978 a youngster at Sheffield University complained to an older colleague, an ex-student of Bartlett's, that "*Your* generation has virtually *betrayed* Bartlett. We know nothing about him"—and his senior had to admit that he was right (V. Stone 1978: 87). (The youngster clearly hadn't read the New Look psychologists, or anyway hadn't followed up their frequent references to Bartlett: see Chapter 6.ii.)

Like the other Gestaltists, Bartlett reported some remarkable experimental data. Following a suggestion from his "close friend" Norbert Wiener (Bartlett 1958: 144), he adapted the American parlour game Russian Scandal—in England, Chinese Whispers—to study memory. (It turned out that a French psychologist had done this first, in 1897—Bartlett 1932: 63.) He used inkblots, drawings, and stories, and got comparable results from all three.

For instance, he got people to read a story, and later asked them to recall it, again and again. He chose a folk tale ('The War of the Ghosts') from a native American culture, whose beliefs about the supernatural were unfamiliar to English people. (In other words, they differed from English people's notions of the supernatural: see 8.vi.d.)

What he found was striking. First, his subjects remembered only the gist of the story, not its details—still less, its words. They seemed to be reconstructing it rather than resurrecting it. Second, they subtly altered it (unconsciously) so that the unfamiliar concepts were assimilated to more familiar ones. That wouldn't have surprised Wilhelm von Humboldt, who'd taught that every culture has its own concepts, not fully intelligible to people from other societies (9.iv.b). Indeed, he'd have said that the original story was unknown even to its translator, the anthropologist Franz Boas. However, both findings surprised Bartlett's colleagues.

These things happened, according to Bartlett, because memories are stored as hierarchically organized, meaningful, *schemas*. "I strongly dislike the term 'schema,'" he confessed, adding that "pattern" and "organized setting" were better (1932: 200–1). But Head's term was already familiar—and Bartlett may have felt that its 'hard-headed' neurological provenance added respectability. (Originally, it had been Immanuel Kant's term, not Head's: it's no accident that the psychologists who first revived the concept had Kantian sympathies: see Chapter 2.vi.a.)

In recall—i.e. reconstruction—the higher levels, expressing the gist, determine the details at lower levels. This activity can't be explained as the re-collection of associationist strings:

> "Schema" refers to *an active organization* of past reactions . . . which must always be supposed to be operating in any well-adapted organic response. That is, whenever there is any order or regularity of behaviour, a particular response is possible *only because it is related to other similar responses which have been serially organized*, yet which operate, *not simply as individual members coming one after another*, but as a unitary [holistic] mass. (1932: 201; italics added)

The implication was that experiments on memory using Hermann Ebbinghaus's method of nonsense syllables (Chapter 2.x.a) weren't dealing with typical memory at all. Moreover, the fact that people learning meaningless lists usually make up some mnemonic to help them—a fact noted later by Donald Hebb, and later still by Miller (iv.d below and 6.iv.c, respectively)—is entirely understandable accordingly.

Such feats of memory were special cases of the mind's *effort after meaning*, which characterizes "every human cognitive reaction—perceiving, imaging, remembering, thinking and reasoning" (1932: 44). (And, one might add, musical performance: see 13.iv.b.) It may involve psychological processes "of very great complexity", even though they appear, to the person concerned, to be "of the utmost simplicity". In a word, these processes are unconscious: not repressed (à la Freud), but hidden from introspection.

The Gestaltists, then, offered many hypotheses about holistic and/or central mechanisms. As though that weren't shocking enough to behaviourist ears, their hypotheses were always vague and sometimes (when describing the brain) implausible. Karl Lashley even said to Wolfgang Kohler (1887–1967), commenting on his studies of "insight" in chimps (W. Kohler 1925): "Excellent work—but don't you have religion up your sleeve?" (Heims 1991: 235). (He wouldn't have said this to the neo-Gestaltist Craik, who *did* try to describe interpretative schemas—"models"—in precise and/or neurologically plausible ways: Chapter 4.vi.)

The Gestaltists, other than Kohler (whose work had been translated in 1925), were largely unknown in America until the late 1930s. By that time, however, a number of them had fled the Nazis to the USA.

In 1933, when Adolf Hitler became Chancellor, the German Society for Psychology rapidly capitulated to the new regime in various ways—well before this was required by law, or even prudence (G. Mandler 2002*b*: 193). For instance, psychological journals instantly dropped Jewish members from their editorial boards; and the 1933 Congress featured several talks on racial purity, as well as many tributes to Hitler and his political project. Indeed, Gestalt psychology itself was appropriated by Nazi sympathizers to its intellectual cousin Ganzheitpsychologie, which emphasized "the unity of experience, transcending immediate or personal preoccupations and consistent with the notion of the new German *Volk* community" (G. Mandler 2002*b*: 192).

Psychologists who were Jewish, or married to Jews, resigned from the Board and/or lost their jobs over the next few years—and some emigrated. One who didn't was Otto Selz, whose work on thinking processes would eventually inspire Herbert Simon: he died in Auschwitz in 1943. (Another non-emigrator was the Aryan ethologist Lorenz. His address at the 1938 Congress warned against social types "whose dangerous virulent propagation threatens to invade the body of the nation", and urged further genetic research "to discover the facts that solidify 'our holiest racial, *völkisch*, and human heritage' "—G. Mandler 2002*b*: 198.)

The Gestaltists slowly gained a hearing in their newly adopted country. That's not to say that their foreign ideas were widely welcomed, for they weren't. It would have been amazing if that hadn't been so, given the predominance of behaviourism in the USA. They had to make do with spaces grudgingly made available on the periphery of American academia (J. M. Mandler and Mandler 1968). It didn't help that much of their work remained untranslated until the late 1930s, when a new "source book" made it more widely known (Ellis 1938). Even then, Gestalt psychology was a minority taste. However, it would eventually inspire important work in cognitive science (see Chapters 6.iii, 7.i.c, and 10.i.c).

To some extent, American appetites for Gestalt approaches had already been whetted by Norman Maier's intriguing work on "functional fixedness" in problem solving (Maier 1929, 1930, 1931, 1933). Maier showed that someone may persist pig-headedly in an unsuccessful strategy, despite the presence of (apparently 'invisible') clues.

For instance, he showed his subjects into a room with two strings hanging from the ceiling, and asked them to tie them together. Naturally, people would take hold of the end of one string and walk towards the other—only to find that the string they were holding was too short to reach it. The only way to solve this problem was to tie a heavy object to one string and then use it as a pendulum, swinging back and forth while the subject walked over to the 'empty' string and waited for the other one to arrive. The only things in the room which were suitable as pendulum bobs were scissors or pliers.

Maier found that: (1) many subjects couldn't immediately solve the problem; (2) failure was even more likely if, before the experiment (apparently) started, he had sat at his desk using the scissors or pliers for their normal purpose while chatting to the subject; (3) some previously frustrated subjects were able to solve the problem if he walked past one of the strings and 'accidentally' brushed it with his arm, so making it move slightly; (4) some of those subjects sincerely claimed that the idea of making a pendulum had occurred to them spontaneously, 'out of the blue'; and (5) some people who had the idea of the pendulum, spontaneously or otherwise, didn't think of using the scissors or pliers as a bob. In short, they were "fixated" on the usual function of these tools: that they might be used for a different purpose appeared to be inconceivable.

Maier's work had been well known since the early 1930s. By 1940, the exodus of German Gestaltists had taken place, and Ellis's *Source Book* had appeared. And by 1950, Wertheimer's and Duncker's research on problem solving had been translated. The movements beneath the mantle were becoming more robust.

c. Sitting tenants with biology

The sitting tenants with biology were the Piagetians and the ethologists. The former had moved into their rooms in 1923 (on publication of *Le Langage et la pensée chez l'enfant*), the latter in the mid-1930s. Piaget's people could communicate with all of their neighbours (although the behaviourists weren't interested), for four of his books were translated within ten years (1926, 1928, 1929, 1932). But the ethologists' occupancy was hardly noticed for twenty years by readers limited to English.

Piaget (1896–1980) is normally thought of as a psychologist. In his own mind, however, he was a biologist (Boden 1979, chs. 1 and 6). His first publication, at the awesome age of 11, reported his sighting of an albino sparrow; and he received his doctorate in 1918 for a thesis on molluscs. But this isn't a merely chronological point: throughout his life, his first intellectual loyalty was to biology—and his second, to philosophy. He took up *psychology*—in around 1919—only in order to integrate those two approaches, planning to spend no more than five years on it (Piaget 1952*a*: 255).

Accordingly, he called himself a "genetic epistemologist", not a psychologist. The label was borrowed from the child psychologist James Baldwin (1861–1934), now remembered by theorists of learning and A-Life through the 'Baldwin effect' (Baldwin 1915; Ackley and Littman 1992). And his epistemology/psychology was 'non-Newtonian' in various ways.

Piaget saw the child's mind not as a small-scale version of the adult's (just lacking a few facts and habits, so to speak), but as a fundamentally different system. Indeed, he said, children don't even have *concepts* until 4 years of age. The 3-year-old's words are "notions" attached to the first verbal signs, and "remain midway between the generality of the concept and the individuality of the elements composing it" (1950: 128).

The baby's mind (he said) differs from the 2-year-old's, the infant's from the 7-year-old's, and the 7-year-old's from the adolescent's—at long last, a mini-adult. In short, he posited four major "stages" of mental development (with up to six sub-stages within them): sensori-motor, pre-operational, concrete operational, and formal operational. Each one is necessary for the next, but the changes take place through holistic transformation rather than piecemeal addition. One of his key terms was "equilibration": a change that happens when a dynamical system is in conflict, ending in a more stable state.

Initially observing his own children, from the cradle onwards, he reported many startling facts. For example, a very young baby will lose interest in a toy once it's hidden under a cushion: as Piaget put it, it's as though the object has ceased to exist. An older baby will look for it under cushion-A *even though* he or she has just seen someone removing it from cushion-A and putting it under cushion-B (this is known in the trade as the A-not-B error). Or again, a 4- or 5-year-old will insist that there are "more" beads in a widely spread five-bead row than in a closely spread one, *even though* the longer row was visibly produced by someone's moving the beads further away from each other. Conservation of number—and, when playing with water-in-jars, of volume—isn't yet recognized. Similarly, apparently basic concepts describing the physical world, such as *movement* and *speed*—what AI workers would later call "naive physics" (13.i.b)—aren't developmentally basic at all, for they take time to emerge (Piaget 1970).

Piaget found also that it takes many years for the child to master apparently simple skills, such as "seriation": putting a set of items in order, according to some perceptible criterion. So a 2-year-old can't build a 'staircase' out of seven blocks. He or she may build one or two short sequences, but can't integrate them. Later, trial and error can be used to produce a perfect sevenfold staircase (and later still, an eighth block can be cleanly inserted into an already completed staircase). Eventually, the child constructs the staircase rationally, starting with the shortest or longest block and successively adding the correct neighbour. At adolescence, the child/adult can also express the general principle involved, and understand that seriating blocks is in essence *just the same thing* as seriating dolls, or pencils . . . or even numbers.

These qualitatively (observably) different stages of seriation, Piaget argued, are structurally discontinuous at the deeper psychological level. And they don't follow each other by magic, nor by inner determinism. The child's practical experience (clenching and unclenching the fists, shaking a rattle, playing with boxes and blocks, pouring water from one container to another . . .) is crucial in enabling stage transformations to take place. Even formal logic and mathematics—and language, too—can develop (he said) *only* from the seed of bodily action in the real world (Inhelder and Piaget 1958, 1964).

The word he used for this was "epigenesis", an ancient term for the unfolding of the embryo but used at that time by—and borrowed from—the biologist Conrad Waddington (Chapter 15.iii.b and Boden 1979: 93–103). Waddington's key claim, and Piaget's too, was that development—in embryo and/or behaviour—is neither a deterministic unfolding of a preformed 'germ' nor merely a response to the environment (whether internal, uterine, or external world). As Piaget put it, it's a self-regulatory "dialectic" between organism and environment, brain and behaviour.

Piaget's hypotheses were not only unfashionably non-Newtonian, but undeniably vague—hence Sutherland's disdain (see subsection a, above). (They were also overwhelming in quantity: he admitted he "could not think without writing"—1952*a*: 241.)

The vagueness remained even after the rise of cybernetics had put some scientific flesh onto dialectic bones. The imprecision, in some cases near-emptiness, of his core concepts was criticized even by his admirers. For example, Bruner—of all people—dismissed his concept of equilibrium as "surplus baggage". It contributed nothing to theory or experimental design, he said, save some "confusing imagery", and served merely to give Piaget "a comforting sense of continuity with his early biological apprenticeship" (Bruner 1959: 365).

Partly because of the vagueness, and partly the non-Newtonianism, Piaget wasn't taken seriously by American experimental psychologists (as opposed to educationists) for many years. Harvard gave him an honorary doctorate when he was 40, but he wasn't honoured by the APA until he was 72.

This also reflected the low status, at that time, of developmental psychology in the academic pecking order. It was seen as something one does if one loves babies—which more or less excludes the higher-status half of the human race. The notion that one might need to consider children in order to understand adults hadn't yet got across (see Chapter 7.vi.g). Granted, the first English textbook on Piaget (Flavell 1962) was *immediately* assigned for Bruner's graduate seminar. Indeed, Bruner had been aware of Piaget's work for over twenty years. But Bruner was no behaviourist, as we'll see (6.ii.a–c).

It would be misleading to describe Piaget as a non-Newtonian and leave it at that. For he went only half-way in rejecting the fourth tenet. Yes, he believed in self-organization. But—like the behaviourists—he *didn't* believe in innate knowledge of the external world. If he'd been told (what was discovered in the 1980s: Chapter 14.ix.c) that newborns can already recognize their mother's voice and the stress patterns of her language, he'd have been surprised—but not theoretically challenged. For the explanation is that some external sounds can reach the foetus while it's still in the womb. Other 1980s discoveries, showing that the neonate brain is prefigured to pay attention to specific aspects of the physical and social world, would have been far more embarrassing (see 7.vi.g–h).

In other words, he was equally drawn to the empiricist and neo-Kantian traditions, often describing his own theory as a ''middle way''. He agreed with the Gestaltists that we actively organize our perceptions in terms of categories such as *cause* and *object*. But he disagreed with them (and with Kant: see 9.ii.c) in arguing that these internal schemas aren't innate, but epigenetically constructed through the real-world activities of the baby/infant/child.

In the final decades of the century, after his death in 1980, epigenesis would gain ground in both psychology and A-Life (Boden 1994*a*, introd.). Indeed, the concept would become highly fashionable. Meanwhile, and with the important exception of Seymour Papert (Chapter 10.v.f), the Piagetians had been speaking more to each other than to the computationalists (Chapter 7.vi.g).

That's true of the ethologists, too. By mid-century, when cognitive scientists started knocking at the door of Psychology's House, the ethologists had taken up lodging there. But the newcomers had little to say to them. And the long-established behaviourists, even less. The early ethologists weren't so shocking as to speak of animal minds (or of animals' theories of mind: Chapter 7.vi.b and f). But they had a healthy respect for inherited capacities, and an unfashionable disrespect for laboratory experiments and universal laws.

Ethology began in continental Europe, thanks to Baron Jakob von Uexküll (1864–1944), Lorenz (1903–89), and Niko Tinbergen (1907–88). Tinbergen moved from Holland to Oxford in 1949, and collaborated with William Thorpe at Cambridge to establish ethology in England. He published an English-language textbook very soon (Tinbergen 1951), but his early work—and Lorenz's—wasn't translated until 1957 (Lorenz 1935–9; Lorenz and Tinbergen 1938; Tinbergen and Künen 1939).

In 1899 the Estonian biologist von Uexküll had co-authored a paper calling for a strictly objective vocabulary in neurophysiology (Boring 1957: 625). At that point, he was concentrating on the physiology of muscle. And with some success: his tough-minded muscular efforts won him an honorary doctorate from the University of Heidelberg, in 1907. But only two years after collecting that honour, he wrote a book on a very different topic: the inner world (*Umwelt und Innenwelt*) of animals.

A version of this appeared in English, much later, as a textbook on *Theoretical Biology* (1926). Despite being published in a highly respected series, alongside the hugely influential work on semiotics *The Meaning of Meaning* (Ogden and Richards 1923), it didn't make a splash. Today, von Uexküll is recognized as a pioneer of biosemiotics (his major paper was reprinted in the journal *Semiotica* in 1992), and his work has appeared in at least nine languages. However, that recognition came later. Thomas Sebeok, the American comparative psychologist who founded ''zoosemiotics'', read his book as a teenager when visiting England in the 1930s, but found the translation so poor that he got nothing from it (Paul Cobley, personal communication).

Soon afterwards, von Uexküll wrote a paper describing 'A Stroll Through the Worlds of Animals and Men: A Picture Book of Invisible Worlds' (von Uexküll 1934). That, too, remained untranslated for almost a quarter of a century—until it was included in a collection on instinct (C. H. Schiller 1957). It was this paper which eventually attracted most of von Uexküll's English-speaking admirers. (''Eventually'', because among psychologists, as opposed to ethologists, "instinct" was a highly unfashionable concept in those behaviourist days.)

From "objectivity" to "invisible worlds": what on earth was going on? The difference was more apparent than real. This *wasn't* an exercise in neo-Kantian idealism, even though it's sometimes glossed in that way (see Chapters 15.viii.b and 16.vii and x.c). The point von Uexküll was making in his "picture book" was that different species have different perceptual and motor abilities, closely integrated in each case. So the animal's environment ("world") or *Umwelt* isn't best thought of as what we'd normally call the real world, but rather *that subset of it* to which the creature can respond and which it can affect.

To the members of one species, the *Umwelten* of other species are invisible—and, of course, unlivable. The best one can do is to rely on the charming, and unforgettable, pictures in von Uexküll's paper, portraying a living room as seen by a human being, a dog, or a fly. Different species, then, live in different worlds—or, as it's sometimes put, different "life worlds". Seventy years later, a leading MIT roboticist would cite von Uexküll in describing the "perceptual worlds" of his new-style robots (R. A. Brooks 1991*a*: sect. 3; see 15.viii.a). And as we'll see in Chapter 8, human beings from different cultures live in significantly different worlds, too.

However, the unavailability of good translations wasn't the only reason why von Uexküll was less famous than his fellow ethologists. For unlike them, he wasn't a field scientist. The first ethologists to base general theories on systematic study of animals in their natural habitats were Tinbergen and Lorenz.

They first met in London in 1936 (two years before that infamous 1938 Congress address: see subsection b). As a result, Lorenz invited Tinbergen to spend a few months at his family estate in Altenberg, near Vienna. (Today, it's the home of the Konrad Lorenz Institute for Theoretical Biology.) Together, they formulated theoretical concepts such as *imprinting*, *fixed action pattern* (FAP), and the *innate releasing mechanism*, or IRM (modelled in one of William Grey Walter's tortoises: see Chapter 4.viii.b).

One example of an IRM is the hawklike cross that causes a greylag gosling to react as if in fear (Tinbergen 1948). Not any cross will do: the crossbar, representing the predator's wings, needs to be slightly shorter than the axis-bar, and placed nearer to one end than the other (i.e. nearer the 'head'). The gosling's reaction, namely crouching, is the FAP that goes with this IRM. The concept of fixed action patterns was linked by Lorenz with that of "action-specific energy"—an idea taken over from McDougall, whom Lorenz acknowledged repeatedly (Hendriks-Jansen 1996: 208 ff.). But Lorenz took care to separate it from the notion of goal-directedness, and above all not to posit specifically *psychic* energies, as McDougall had done (2.x.b).

In general, the ethologist followers of Tinbergen and Lorenz explained animal behaviour in terms of interactions of simple reflex rules, not of inferences run on internal representations. In that sense, they were faithful "Newtonians".

They were non-Newtonian, however, in favouring nativism (the IRM and FAP) and rejecting universalism. Not only do various species behave differently, but animals of a given species conduct themselves differently in the laboratory and in the wild. The fact that cats in a behaviourist's puzzle box eventually learn to escape as a result of random thrashing around doesn't prove, they said, that cats always learn in that way (cf. Thorndike 1898).

We'll see later that some modern neurologists agree, even arguing that there's *no* universal learning mechanism but only "[many] different problem-specific learning mechanisms [which we may call] 'instincts to learn' " (Gallistel 1997: 82; see 14.ix.g). We'll see also that a new form of ethology, namely computational neuro-ethology (CNE), is now part of cognitive science (Chapters 14.vii.a and 15.vii). CNE helps explain species-specific adaptive behaviour, by modelling the neural mechanisms involved.

In the 1950s and early 1960s, however, the ethologists weren't invited to the cognitive tea parties. The first computational psychologists had happily dropped several behaviourist assumptions, as we'll see in Chapter 6.i–iii. But they weren't ready—yet—for nativism.

A very recent (1940s) entrant to the biological wing of Psychology's House was James Gibson (1904–79). Like the ethologists, he was much concerned with animals' overall lifestyle: how their sensory and motor repertoires fitted together. But he focused much more closely on the detailed *physics* of the environment, asking how it affects perception and behaviour. His approach wasn't popular, since it challenged Gestaltists and behaviourists alike. Nor did it become popular when, soon afterwards, cognitive science came on the scene. Having no time for "internal representations", Gibson had none for "computations" either.

Eventually, battles royal would be engaged between Gibsonians and Marrians (Chapter 7.v.e–f), and psychologists (and philosophers) interested in *embodiment* would recognize Gibson as a patron saint. Meanwhile, he and his disciples worked busily inside their self-contained flat.

5.iii. Soft Centres

The sitting tenants discussed in Section ii were all looking towards the centre of the mind/brain. (Even the ethologists were positing IRMs.) They believed in structural organization and/or central mechanisms integrating perception and action. However, they could say very little about *just what* these central influences were, or *just how* they worked. In other words, their theories had soft centres. (Sutherland's contempt for McDougall and Piaget was due to their softness, whereas I'd been intrigued by their centres.) The cognitive revolution would eventually revive important aspects of these older views, by turning soft centres into harder ones.

Hardness, however, can be overdone—as it was in orthodox behaviourism. If the ideas outlined in Section ii were to be salvaged, meaning and purpose would have to be salvaged too.

a. Mentalism goes underground

In behaviourism, as in most areas of life, propaganda and practice diverged. The propaganda was "Newtonian" (in the six ways listed above). But the practice wasn't. However—again, as in most areas of life—the divergence was mostly covert, more rarely overt. Although most behaviourists believed themselves to be avoiding mentalistic concepts entirely, they weren't. In a nutshell, the key notions of *stimulus* and *response* weren't as atomistic as they claimed.

John Watson (1878–1958) had borrowed these concepts from the reflexologists (Chapter 2.viii.b). In his first statement of behaviourism—*the* first statement of behaviourism—he concentrated on spurning "consciousness" and other mentalist terms (J. B. Watson 1913). Although he spoke of "the range of stimuli to which [the animal] ordinarily responds", he didn't explicitly define psychology as the search for S–R laws. By the mid-1920s, however, such definitions were common. Watson, again:

> We use the term *stimulus* in psychology as it is used in physiology. Only in psychology we have to extend somewhat the usage of the term . . . In a similar way we employ in psychology the physiological term "response", but again we slightly extend its use. (J. B. Watson 1924: 10–11)

> By response we mean anything the animal does—such as turning toward or away from a light, jumping at a sound, and more highly organized activities such as building a skyscraper, drawing plans, having babies, writing books, and the like. (J. B. Watson 1925: 7)

Though not all S–R psychologists were so crass as to call architecture or authorship a "response", they followed Watson in his atomistic assumptions concerning the two key terms.

This is true even of the arch-behaviourist Skinner (1938), who—strictly speaking—wasn't an S–R theorist. He focused not on classical conditioning (described by S–R laws) but on operant conditioning, described in terms of "contingencies of reinforcement". (It's often thought that he discovered operant conditioning, but that's not so—C. G. Gross 2002: 89. Ironically, it was first described by the Polish neurophysiologist Jerzy Konorski, whose highly anti-Newtonian ideas about "gnostic" units primed the discovery of brain cells for detecting a monkey's hand: see 14.iii.b.)

In classical conditioning, a response stably elicited by stimulus A becomes attached also to stimulus B, given repeated trials in which A and B occur simultaneously. Higher-order conditioning can "chain" reflexes, as yesterday's B is used as today's A . . . and so on. (That's true for the higher animals; it's much more difficult to achieve when working with animals having relatively simple brains.)

In operant conditioning, the experimenter waits for the animal to do something—why it does it, doesn't matter—and immediately rewards (or punishes) it. This raises (or lowers) the probability of that response being emitted again. The conditioned response can be "shaped" to a desired behaviour. For pigeons, perhaps batting a ping-pong ball; for babbling infants, saying "Mummy". This is done gradually, by rewarding only behaviour that's more like the desired pattern than yesterday's was. (Similarly, in evolutionary robotics one starts by selecting *any robot which moves*, next only robots which move *broadly* in the desired direction . . . and so on: see 15.vi.c.) But if Skinner didn't formulate S–R laws as such, he did speak of stimulus and response.

However, both the "S" and the "R" of the behaviourists were more soft-centred than they appeared. This point was made in the early days by the philosopher John Dewey (1859–1952). Indeed, Dewey wasn't early so much as anticipatory. Some twenty years before Watson's revolutionary paper, he gave a fundamental critique of the concept of the reflex arc (Dewey 1896).

He was glad that the reflexologists weren't focusing on "consciousness". That, he said, was a crucial advance. But he wanted them to appreciate—and study—the *teleological unity* in experience/behaviour. He was stressing what the cyberneticists, fifty years later, would call circular causation. For he complained of reflexology's "failure to see that the arc of which it talks is virtually a circuit, a continual reconstitution", and its offering us, instead, "nothing but a series of jerks, the origin of each jerk to be sought outside experience itself" (1896: 360). He hoped to persuade the new physiologists to his point of view by showing that they were endorsing it *covertly* already.

The reflexologists believed that their language of stimulus and response was associationist. But Dewey demurred. As he put it, the distinction between stimulus and response is a "teleological distinction of interpretation" with reference to an assumed *end*, not a "distinction of existence". (In modern terminology, stimulus and response are concepts used within the intentional stance, not the physical stance: Chapter 16.iv.b.) The reflexologists, like Hume and the two Mills before them (2.x.a), were ignoring the central—intentional—structure that generates the reflex and is served by it:

> [For the reflexologists, the] sensory stimulus is one thing, the central activity, standing for the idea [thought], is another thing, and the motor discharge, standing for the act proper, is a third. As a result, *the reflex is not a comprehensive, or organic unity, but a patchwork of disjoined parts, a mechanical conjunction of unallied processes* . . . (Dewey 1896: 358)

Instead of looking for "sensation-followed-by-idea-followed-by-movement", he said, physiologists should focus on the "organized coordinations" that unite them.

For over half a century, his pleas fell on deaf ears. Teleologically structured "coordinations" were shunned in favour of atomistic "connections". Lashley would challenge this non-structural orthodoxy in the 1940s, as we'll see (subsection iv.a). But not until the 1960s would neuroscientists start thinking in terms of the (teleological) *interests* of the whole animal (Chapter 14.iv and vii).

In short, Dewey held that reflexology seemed plausible only because it was covertly non-Newtonian. Six decades later, much the same point was made by Noam Chomsky, and by the philosopher Charles Taylor too.

Taylor (1931–) focused on the general philosophical points stressed by Dewey. But he also provided many detailed critiques of behaviourist experiments and S–R theories (C. M. Taylor 1964*a*). In addition, he argued that no *causal* account could possibly explain behaviour, which involves some non-causal "press of events" in the "essential nature" of behaving organisms (p. 24). Hence, he said, a mechanical dog, programmed to behave like a real one, could be *described* as "goal-directed", but not *explained* in that way (p. 20). Taylor's book was hugely successful in Anglo-American philosophical circles. But although his critiques impressed the philosophers, who felt that he'd obviously done his homework, they didn't impress the more knowledgeable Sutherland, who rebutted them forcefully (N. S. Sutherland 1965); and Taylor's recourse to the "essential nature" of organisms didn't impress me (Boden 1972: 119–37).

Chomsky (1928–) was more concerned with specific—in his view, outrageous—behaviourist claims about language. His savage review of Skinner's *Verbal Behavior* argued that Skinner, despite his anti-mentalist rhetoric, relied heavily on (covertly) intentional concepts, and on an intuitive recognition of linguistic structure (Chomsky 1959*b*). Chomsky's review is discussed at length in Chapter 9.vii.b. Here, the point is that it showed Skinner to be less Newtonian than he claimed.

In sum: although Bruner would complain of the behaviourists' "impeccable peripheralism" (Bruner *et al.* 1956, p. vii), their peripheralism was less impeccable than they believed. Even Skinner was covertly flirting with mentalism, meaning, and structure.

b. Behaviourism softens

If one were to ask "When is a behaviourist not a behaviourist?" the answer would seem to be "Always!" The previous subsection showed that, in a sense, *no* behaviourist was really a behaviourist. However, some diverged from the party line more openly. Ralph Perry, Edward Tolman, and Clark Hull all ignored the sixth tenet, and Perry and Tolman—self-styled "purposive behaviourists"—bravely flouted the first, second, and fifth as well.

Perry (1876–1957) was perhaps less brave than Tolman, for he wasn't a card-carrying psychologist. Rather, he was a philosopher. However, as a close friend and ex-student of William James, he was deeply interested in psychology. He described himself as a behaviourist, but had scant sympathy for the behaviourist propaganda.

Drawing on the writings of McDougall, who'd just been invited to Harvard, Perry argued around 1920 that *purpose* is an essential concept for psychology (R. B. Perry 1918, 1921*a*). He pointed out—what Continental philosophers would call the hermeneutic circle, and what Daniel Dennett would include within "the intentional stance" (16.iv.b)—that one can't identify a given purpose without assuming some specific belief, and vice versa (J. B. Perry 1921*b*). To infer, for instance, that someone carrying an umbrella *wants* to keep dry, is to assume they *believe* that umbrellas provide protection from rain. It followed, said Perry, that behaviour can't be described by the language of stimulus and response unless this is understood in intentional terms: "behaviour is incapable of being translated into simple relations correlated severally [i.e. atomistically] with external events" (J. B. Perry 1921*a*: 102).

Perry wasn't an experimentalist, so his views could safely be ignored by the more orthodox behaviourists. Tolman (1886–1959) was quite another matter. Working at UC-Berkeley from 1918 until his death (despite risking dismissal in the 1950s by refusing to take Senator Joseph McCarthy's loyalty oath), he reported many surprising experimental results. These cried out for attention even from those who rejected his explanations of them.

Many did reject Tolman's explanations, which sprang from his commitment (following Perry) to teleological structure. Even in the early 1920s, he called himself a "purposive" behaviourist (Tolman 1922, 1925*a*,*b*, 1932). Tolman had no use for atomistic "S" and "R": "I rejected the extreme peripheralism and muscle-twitch-ism of Watson" (1959: 94). He believed in "instincts", defined not as reflex movements but as inborn goals/interests (1920, 1923). And he posited many central organizing mechanisms, which he saw as "intervening variables" between episodes of observable behaviour (1935).

These included innate or acquired "exploratory impulses", or hunches, and meaningful "expectancies" linking different stimuli (which he often termed "Gestalts"). They also covered mechanisms of "means–end-readiness" linking perception to action, and "belief-value matrices" organizing the creature's choices. In the late 1940s, he even added "cognitive maps" and "hypotheses" (1948).

All these mentalistic concepts were attributed to rats in the first instance, and later to humans (1958). (I say "later", but Tolman's theoretical terms had mostly been drawn from common-sense psychology in the first place, as he himself admitted—1959: 98 n.)

Clearly, Tolman was no Newtonian. He preferred a very different approach: "I don't enjoy trying to use my mind in too analytical a way" (1959: 93), and welcomed very different ideas: "I was tremendously influenced by Perry . . . [and] much influenced by gestalt psychology" (1959: 94, 95). He'd interacted with the Gestaltist Kurt Koffka (1886–1941) while on an early visit to Germany, and later collaborated with Egon Brunswik (1903–55), who left Vienna for the USA in the 1930s (e.g. Tolman and Brunswik 1935).

In the 1920s–1930s in particular, Tolman's views were hugely unfashionable. To be sure, the young Simon, puzzling over human decision making, was primarily inspired by Tolman and William James (Simon 1947*a*, ch. 5). But Simon was then an economist. Professional psychologists were wary, not to say contemptuous, of Tolman's theories. But they couldn't ignore him—and in 1937, they even elected him President of the American Psychological Association—because of the startling data he reported.

Many of these concerned "latent" learning, in which rats appeared to learn mazes without any reinforcement. Allowed to explore a maze *without* any food in it, they later learned the maze more quickly than usual. Tolman explained this in terms of the "confirmation of expectancies": if the expected situation is reached, the probability of the action is increased. (Some psychologists posited a drive of curiosity, to provide hidden reinforcements; but this move was embarrassing for mainstream behaviourists.)

Later, Tolman described rats who seemed to have learnt internal models/schemas of spatial orientation, used to organize their behaviour. But instead of talking about the chaining of reflexes/responses, he spoke of the organization of stimuli (Gestalts) in "goal/sub-goal hierarchies", or "cognitive maps". Cognitive maps troubled the orthodox, for the first word was mentalistic and the second, structural. The other 'folk-psychological' terms troubled them too.

One S–R psychologist who tried to accommodate Tolman's data without using his explanations was Hull (1884–1952). Initially trained as a mining engineer, he did most of his psychological research at Yale. One of the people attending his seminars in the early 1930s was Warren McCulloch, and it wasn't only McCulloch whose attention was captured. Throughout the 1940s and early 1950s, Hull (not Skinner) was the most professionally influential—though not the most popularly famous—of all the behaviourists.

Hull was more firmly wedded to empiricism than Tolman was. He saw purposive explanations as illegitimate because, he argued, they assumed queer backward-working causes (a common misunderstanding which McDougall, for one, had countered clearly). Purposes, said Hull, are just "habit mechanisms" (1930), which are reducible to "colorless movements and mere receptive impulses" (1943: 25). He was an S–R theorist par excellence—except that the S and the R could happen *inside* the organism.

Moreover, Hull's philosophy of science was austere. He shared the logical positivists' view of scientific method (memorably mocked by Skinner: 1959). And he favoured their ambitious programme of the unification of science (see Chapter 9.v.a), even to the extent of axiomatizing his own theory (C. L. Hull 1937, 1943; Hull *et al.* 1940). Tolman, by contrast, confessed: "Apparently I have no scientific superego which urges me to be mathematical, deductive, and axiomatic . . . [My] system is based on hunches and on common-sense knowledge. It is certainly not 'hypothetico-deductive' " (1959: 97, 150).

So why mention Hull here? Surely, *he* was a behaviourist if anyone was?—Well, yes. But he was more open-minded than one might expect. His doctoral thesis (1920) had explored the topic of concept formation; he was interested in hypnosis, which he studied experimentally in some detail (C. L. Hull 1933); and he even invited Koffka to Wisconsin as visiting professor for a year. More to the point, he ignored the sixth tenet: his psychology bristled with intervening variables.

To be sure, these weren't overtly mentalistic. At worst (at best?), they were covertly mentalistic, in the sense explained in the previous subsection. But he attributed a complex functionality to the mind/brain—and in doing so, flouted the sixth tenet with abandon. As a computationalist critic would later put it: "stimulus–response theorists themselves are inventing hypothetical mechanisms with vigor and enthusiasm and only faint twinges of conscience" (Neisser 1967: 5).

The hypothetical mechanisms posited by Hull (1934) included pure stimulus acts (aka attention), persistent stimulus components (aka representations), reaction potentials, anticipatory responses, and habit-family hierarchies. The last of these was used to explain what McDougall had called "variation of means", and Tolman "means–end readinesses".

For Hull, each goal, and each of its anticipatory sub-goals, is associated with a habit-family, a set of alternative responses which share a common anticipatory response. The latter mediates the transfer of reinforcement from one family member to others. (Like human families, habit-families have members who vary in influence: a behaviour-based equation was provided for calculating "habit-strength".) Hull's chains of "fractional anticipatory goal responses" served much the same theoretical role as Tolman's "expectancies". So Hull felt that he'd allowed for the three crucial features of purposive behaviour: that it may involve a wide range of responses, that these may be aimed at a variety of sub-goals (and sub-sub-goals . . .), and that—up to a point—it persists until the goal has been achieved.

The remark (quoted above) about "only faint twinges of conscience" implied that *testing* Hullian theory was easier said than done. Axioms and deductions were all very well, but the increasingly baroque logic of Hull's hidden Ss and Rs seemed to bear little relation to specific experimental evidence. (At least Skinner reported observable phenomena, namely contingencies of reinforcement—which were reliable enough to be used to train animals for circuses and cinema films.) By the late 1950s, then, the leading psychological journals were publishing many critiques of Hull's (and similar) theories.

What such critiques didn't do, however, was to draw the explicit conclusion that *behaviourism in general* was doomed. That was left to Lashley in 1951 (see iv.a below), to Chomsky in 1959 (9.vii.b), and to the cognitive science manifesto of 1960 (6.iv.c).

c. Behaviourist machines

Hull quite often suggested neurophysiological underpinnings for the psychological functions he described. (These weren't always in the brain: the anticipatory goal responses were assumed to lie largely in the muscles and endocrine glands.) Tolman didn't do this, but he was well aware that his theories must allow for neural implementation.

Moreover, neither man had any philosophical qualms about artefacts being used to model their ideas. But when they discussed this, their approach was theory-to-machine, not machine-to-theory. In other words, they *didn't* use concepts that had been originated for describing machines to help them express their own psychological theories.

Tolman (1941), for instance, outlined a "schematic sowbug", a hypothetical creature broadly comparable to Valentino Braitenberg's "Vehicles" (15.vii). As for the ex-engineer Hull, he built a physical model of the conditioned reflex (having already built a logic machine and a device for calculating correlations in aptitude tests). He described his conditioning machine briefly in the pages of *Science* in 1929, and more fully soon afterwards (Baernstein and Hull 1931).

He wasn't alone: the psychological journals around 1930 reported several attempts to model basic behaviourist principles in machines (e.g. Stephens 1929; A. Walton 1930: 110–11; Ross 1938). In one of these, a doll would "naturally" raise her arms to approach a toy rabbit entering her field of view, and "naturally" tremble at a loud noise. But the wiring diagram of the machine (Figure 5.3) would ensure Pavlovian conditioning:

> If the loud noise be made first . . . and then the rabbit is presented, no conditioning takes place. Even if the doll is still trembling when the rabbit approaches, the trembling is somewhat reduced (probably by reason of the "distraction" involved), but there has been no conditioning. If the rabbit be again presented alone, the old approach reaction is made just as before. If, however, while the approach response is being called out, the loud sound be made [by banging together two blocks of wood], the arms fall to the side and trembling ensues. The rabbit being withdrawn and presented again now elicits no "approach response," but does bring on the fear reaction of trembling. Fortunately, unconditioning can be effected simply by pressing a push-button. (A. Walton 1930: 110)

This affecting vignette of the doll-and-rabbit was surprisingly engaging. But the existence of such machines wasn't surprising at all. We saw in Chapter 2.vii.e, for instance, that S. Bent Russell, who'd built a hydraulic model of nervous conduction as early as 1913,

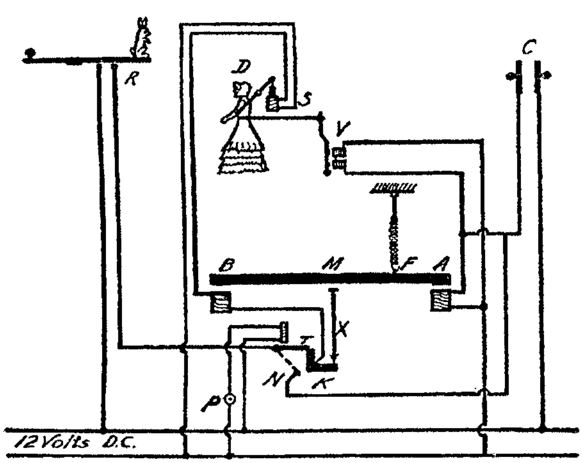


FIG. 5.3. Wiring diagram for a doll-and-rabbit conditioned reflex machine. Reprinted with permission from A. Walton (1930: 110)

had suggested then that *psychological* principles might be simulated too (see also Bent Russell 1917*a*,*b*). By Hull's time, quite a few people were playing this game (Boring 1946; Valentine 1989; Cordeschi 1991, 2002).

Boring approved. Indeed, he assumed that to be an S–R theorist *just is* to think of organisms as "robots" (Boring 1946: 177). He'd come to this conclusion after being "defied" by Wiener (in a letter of 1944) "to describe a capacity of the human brain which he [Wiener] could not duplicate with electronic devices". In his response, delivered as the Presidential Address to the Eastern Psychological Association, Boring described Tolman's schematic sowbug and said:

> The advantage of playing this kind of game lies *solely* in the fact that, if you talk about machines, you are more certain to leave out the subjective, anthropomorphic hocus-pocus of mentalism. There would be nothing wrong with mentalism if it used rigorous definitions of terms, but usually it does not. Hence the mentalistic concepts need first objective analysis into functions, and then a further test, the test of thinking about them as pertaining to a machine. (Boring 1946: 191; italics added)

He went on to discuss the design and construction of *actual* robots, like Hull's. He even mentioned the possibility of using electronic computers, if the result were thought worth the trouble and expense. And he cited a futuristic paper by Vannevar Bush (1945), full of hype about computers and how they might be used (and how, eventually, they *were* used: 10.i.h and 13.v–vi).

But this was 1946. MADM had yet to make her debut, the 'logical' neurone was still confined to the pages of the *Bulletin of Mathematical Biophysics*, and cybernetic robots were still hidden in infancy (see Chapters 3.v and 4.iii–v). Boring's word "solely" is a give-away: his assimilation of S–R organisms to robots *wasn't* an example of mind-as-machine. To be sure, he admitted that, challenged in Wiener's letter "to describe a capacity of the human brain which he could not duplicate with electronic devices", he couldn't do so (Boring 1946: 178). Nevertheless, lacking an "inventory" of psychological functions, he "could not be sure that there was not [at least one] which a nervous system could perform and an electronic system could not".

Similarly, Tolman and Hull weren't cognitive scientists, for they'd turned to consider machines only *after* formulating their theories. The artefacts could show whether their theories seemed to 'work'. But there was no suggestion that thinking about *the machines themselves* might generate psychologically interesting ideas. Computational psychology was still (just) below the horizon.

5.iv. Neurology Creeps In

Psychologists before mid-century had mostly ignored neurophysiology, not least because little was then known about it. McDougall was an exception, but by 1920 he'd fallen out of fashion.

The behaviourists, in particular, paid only lip-service to the brain—hence Boring's talk of "the empty organism" (see Section i.a). To be sure, they'd been inspired by Pavlov, and had little doubt that their psychological findings would one day be explained in neurological terms. But such explanations weren't their concern. Indeed, Skinner (1938) had specifically warned psychologists not to digress into neurological speculation (subsection b, below).

The most important exceptions in the 1940s were Craik (4.vi), Lashley (1890–1958), and Lashley's student Hebb (1904–85). However, Craik's work was still relatively unknown outside the UK. The combination of Lashley and, especially, Hebb would encourage psychologists on both sides of the Atlantic to think about possible neural mechanisms.

Some people impressed by early cybernetics were encouraged to model them also (4.viii). Hebb's work soon set IBM's computers buzzing (subsection f, below). Plausible neurological models, however, wouldn't be available for twenty years or more (see Chapters 12, 14, and 15.vii). Meanwhile, most psychologists' talk about brain mechanisms was—like Tolman's theory—'soft' in the sense that it was *talk*, not *implementation*.

a. Hierarchies in the brain

The person who mounted the first *really* worrying attack on behaviourism, from the behaviourists' point of view, was initially one of their own: Lashley. To be sure, he'd never been one for *hypotheses non fingo*: as a psychophysiologist, his prime aim was to discover neural causes. But he'd given a behaviourist analysis of consciousness (Lashley 1923), and had been trained—by none other than Watson himself—in the canons of S–R behaviourism and reflexology.

Eventually, however, he undermined both. On the one hand, he questioned the *neural reality* of the reflex arc. On the other, he denied its *theoretical adequacy* as an explanation of behaviour.

By 1920, Lashley was already beginning to doubt 'pure' behaviourism. Before moving to Harvard in 1935 (and to the Yerkes primate laboratory in 1942), he'd commenced an epochal series of experiments showing that rats, after damage to their cortical cells and/or cuts in their brain circuitry, could still learn to negotiate mazes (1929*a*,*b*, 1931). They needed more practice after more extensive damage, and for the more difficult mazes—but they *could* still learn.

His conclusion was shocking:

> It is very doubtful that the same neurons or synapses are involved even in two similar reactions for the same stimulus . . . The results are incompatible with theories of learning *by changes in synaptic structure*, or with any theories which assume that particular neural integrations are *dependent upon definite anatomical paths specialized for them* . . . The mechanisms of integration are to be sought in the dynamic relations among the parts of the nervous system rather than in details of structural difference among them. (Lashley 1929*a*: 3; italics added)

There'd been a monumental failure to locate memory circuits in the brain. Conditioned reflexes were real enough, considered as learnt behaviours. But how they were embodied was suddenly a mystery. Twenty years later, he was still shocked:

> The series of experiments has yielded a good bit of information about what and where the memory trace is not. It has discovered nothing directly of the real nature of the engram. I sometimes feel, on reviewing the evidence on the localization of the memory trace, that the

necessary conclusion is that learning just is not possible. *It is difficult to conceive of a mechanism* which can satisfy the conditions set for it. (Lashley 1950: 477–8; italics added)

(In Chapters 12 and 14 we'll see how research in connectionist AI and computational neuroscience would eventually demystify this problem, at least up to a point.)

Even more relevant, here, is Lashley's lecture on "serial order" in behaviour (Lashley 1951*a*). This was delivered at Caltech's Hixon symposium, held in Pasadena in 1948. It caused a mini-sensation. (There were so many appreciative comments that he admitted: "I have been rather embarrassed by some of the flattering remarks made today"—Jeffress 1951: 144.) For Lashley explicitly rejected four of the six behaviourist assumptions: atomism, associationism, externalism, and black-boxery. Black-boxery, of course, was no great loss for people interested in the brain. But the other three were.

In Lashley's opinion, externalism was the major casualty. At the start of his lecture, he declared: "My principal thesis today will be that the input is never into a quiescent or static system, but always into a system which is already actively excited and organized" (1951*a*: 112). He ended with the same point:

Attempts to express cerebral function in terms of the concepts of the reflex arc, or of associated chains of neurons, seem to me doomed to failure *because they start with the assumption of a static nervous system*. (p. 136; italics added)

In addition, Lashley's paper posited top-down structural organization (so the first two tenets of behaviourism bit the dust). This, in a nutshell, was Lashley's answer to "the problem of serial order in behavior".

The phenomenon that was puzzling him was temporal order, or sequence. What controls the production of phonemes in speaking a word, or the movements of the fingers in playing an arpeggio? The orthodox answer was: a chain of S–R reflexes. This implied two things. First, that what happens can depend only on what's already happened, not on any future links in the (uncompleted) chain. Second, that each link is controlled separately, by sensory messages reaching the brain and triggering motor messages to the muscles. Lashley showed that neither implication fitted the evidence.

In rebutting the first, he started with seven pages discussing language: mainly speech, but also typing. Many everyday phenomena, he said, make it clear that what happens in speech *is* affected by what's going to happen later. Common linguistic errors such as 'spoonerisms', and anticipatory slips of the tongue (or typing-fingers) in general, show that speech (and typing) is actively organized by syntax, not merely describable by it. And the same applies, he said, to the *comprehension* of language (as in the 'garden-path' example given below).

By "syntax", he didn't mean Chomsky's NP–VP (which hadn't yet been defined: Chapter 9.vi), nor even the grammar-school child's *subject*, *predicate*, *noun*, *adverb* . . . and so on, but something much wider. He spoke of the "generality of the problem of syntax", by which he meant that all skilled action ("almost all cerebral activity"), whether in animals or in humans, is hierarchically organized. Examples of skilled action include speaking, piano playing, walking, jumping . . . even 'simply' reaching and grasping. Moreover, action errors comparable to those found in speech can be seen in other skills too—for instance, as Lashley pointed out, the many non-linguistic action errors described in Freud's *Psychopathology of Everyday Life*.

Hierarchical structure is most easily recognized in language. We all know that language involves several levels of organization: sounds, syllables, words, phrases, sentences . . . Moreover, the higher levels affect what happens below them. Lashley instanced the *spoken* sentence: "Rapid righting with his uninjured hand saved from loss the contents of the capsized canoe." Most people hear this as "Rapid writing with his uninjured hand . . ." until they hear the phrase "capsized canoe". At that point, they do a double-take and reinterpret the initial verb. (Garden-path sentences, such as this one, are discussed in Chapter 7.ii.b.)

In rebutting the second implication of orthodoxy, Lashley used not only qualitative arguments (concerning "syntax" and hierarchy) but quantitative ones as well. For instance, "whip-snapping" hand movements can be accurately controlled to less than an eighth of a second—yet that's the *minimal* reaction time for the arm to respond to touch or kinaesthesis. Moreover, the speed of nervous conduction doesn't allow enough time for messages to pass up to the brain and back again, so as to control every detail of the hand's movement. The same applies in playing music vivace, or in batting a cricket ball. It's clear, said Lashley, that "a series of movements is not a chain of sensory-motor reactions". On the contrary, there must be some central mechanism, working "in independence of any sensory controls", controlling the relevant muscles *as an organized whole*.

What might that mechanism be? Lashley had only the vaguest idea. Speech, for instance, involves a huge number of muscles and many different body parts, all subtly integrated whenever we utter a word. (A fascinating account of the anatomical coordinations involved, which draws heavily on Lashley's paper, is in Lenneberg 1967, chs. 1–3.) He posited "general schemata of action which determine the sequence of specific acts", defined as "elaborate systems of inter-related neurons capable of imposing certain types of integration upon a large number of widely-spaced effector elements". But the real problem was to discover their nature, "and to this problem", he confessed, "I have no answer". (Soft centres, again.)

Given the soft centres, one might expect that Lashley's ideas would instantly be taken up in a cognitivist spirit. They weren't. The mini-sensation didn't become a genuine sensation until about 1960, when his lead was acknowledged by Miller and Chomsky—and, afterwards, Eric Lenneberg (Chapter 6.iv.c). Indeed, the same applies to the 1948 Hixon symposium in general, which was a good ten years before its time (D. Bruce 1994).

Part of the reason was that, in criticizing behaviourism in this way, Lashley himself wasn't embracing the nascent cognitive science. He said, for example:

> The brain has been compared to a digital computer because the neuron, like a switch or valve, either does or does not complete a circuit. But at that point the similarity ends. [Among other differences, the] number of neurons involved in any action runs into millions so that the influence of any one is negligible . . . Any cell in the system can be dispensed with . . . The brain is an analogical machine, not digital. Analysis of its integrative activities will probably have to be in statistical terms. (Lashley 1958: 539)

Nevertheless, Lashley was a precursor of cognitive science, not just a predecessor of it (see 9.ii). For he sketched a physiological research programme to test von Neumann's ideas on cellular automata (Chapter 15.v.a; Aspray 1990: 182). And he spoke of "plans" and "structures": concepts that his student Karl Pribram would help to highlight in the cognitive science manifesto (6.iv.c). He even identified many of the questions addressed in that manifesto, and—in a very broad sense—anticipated some of the same answers.

b. Connectionism named

If Lashley's 'Serial Order' paper wasn't immediately recognized as important by all and sundry, neither was Hebb's book *The Organization of Behavior*. This was published in 1949, the year after the Hixon meeting. It galvanized some, to be sure—but not all. Mandler recalls:

> [As] a major alternative to mainstream behaviorism, [Hebb's book] was in part ignored to the eventual embarrassment of the conventional wisdom, but found enough support to become the core of a small counterrevolutionary movement. (G. Mandler 1996: 19)

However, it came into its own a few years later. Its rise to prominence was aided in 1958 by Hebb's hugely successful *Textbook of Psychology* (still in use today, in the fourth edition of 1987), and by the sudden arousal of interest in cognitive psychology to be described in Chapter 6. Thus Mandler, again:

> In the United States, it was not until another half-dozen years had gone by before the rejection of behaviorist dicta hit full stride. (G. Mandler 1996: 19)

> *A burst of activity rarely seen before* turned the field around between 1955 and 1960 and established a firm basis for the "new" cognitive psychology. (p. 20; italics added)

Shortly after that, in 1960, Hebb was elected President of the APA. (His Presidential Address promised that his non-behaviourist form of connectionism would throw light, at last, on higher-level cognition: Hebb 1960.) And in 1965, he was nominated for a Nobel Prize.

(Which, by the way, he didn't get. Perhaps the Committee felt that although the name was a new invention, the core idea wasn't?—see subsection e, below. And perhaps it was premature in any case. A quarter-century later, maybe he'd have got it—see 12.vi–vii, and 14.v–vi and ix.)

His 1949 book would turn out to be a crucial spur to cognitive science, in two ways. On the one hand, it soon encouraged work in connectionist modelling—already starting, thanks largely to McCulloch (4.iii and 12.i–ii). On the other, it encouraged a 'non-Newtonian' mentalism: specifically, a concern with *concepts* (verbal and non-verbal), and how they're learnt. "Mentalism", here, means the use of intentional vocabulary. Mentalism in the Cartesian sense was implicitly denied: "the task of the psychologist [is] understanding behavior and reducing the vagaries of human thought to a mechanical process of cause and effect" (Hebb 1949, p. xi). (He would reject it explicitly later, in his book on the mind–body problem: 1980.)

Hebb got away with this, in those predominantly behaviourist times, partly because—unlike Chomsky, ten years later (9.vii.b)—he didn't go out of his way to scorn the behaviourists. Quite the contrary: an ex-student has recalled that "Hebb always stated with complete conviction that he regarded B. F. Skinner as the greatest psychologist of the century" (Harnad 1985).

Mainly, however, he got away with it because although he was (from 1946) chairman of McGill's *Psychology* Department, he presented mentalism in the respectable form of a theory of *neural mechanisms*. Indeed, he pleaded that psychologists and neurophysiologists should cooperate more than they were then wont to do (1949, pp. xii, xix, 12).

That apparently unexceptionable plea was immediately spurned by his hero, who didn't see Hebb's theory as "respectable" at all. According to Skinner (1950), psychologists should stick to their last: describing changes in the probability of behaviour. They shouldn't meddle with neurophysiology, he protested, because studying actual structures and biochemical processes in the brain—including "synaptic connections [being] made or broken"—requires very different observational methods. Mere speculations aren't good enough.

Nor should they fog matters, as he'd tartly put it some ten years earlier (in criticizing Hull's theory), by treating the CNS (the common shorthand term for the central nervous system) as the "conceptual nervous system" (Skinner 1938: 421). Quasi-neurophysiological theories that treated the brain merely as "a system with a certain dynamic output", and postulated unobservables such as "concepts" and "expectancy", were useless. Nevertheless, he complained, "Theories of this sort are multiplying fast." He didn't mention Hebb by name, but the target was clear. (Hebb soon took up Skinner's challenge, with respect not only to "concepts" but also to "drives": 1955.)

As for what Hebb said about the nervous system, his position was broadly similar to David Hartley's two centuries earlier (Chapter 2.x.a):

> *The general idea is an old one*, that any two cells or systems of cells that are repeatedly active at the same time will tend to become "associated", so that activity in one facilitates activity in the other. The details of speculation [*sic*—and *pace* Skinner] that follow are intended to show *how this old idea might be put to work again*, with the *equally old idea* of a lowered synaptic "resistance", *under the eye of a different neurophysiology* from that which engendered them. (1949: 70; italics added)

In short, simultaneity of firing is the key: cells that fire together, wire together. A few pages before, he'd given a somewhat different version of this 'ft/wt' principle:

> Let us assume then that the persistence or repetition of a reverberatory activity (or "trace") tends to induce lasting cellular changes that add to its stability. The assumption can be precisely [*sic*] stated as follows: *When an axon of cell A is near enough to excite a cell B and repeatedly or persistently takes part in firing it, some growth process or metabolic change takes place in one or both cells, such that A's efficiency, as one of the cells firing B, is increased.* (1949: 62)

In fact, Hebb's two formulations of the ft/wt rule were neither "precise" nor equivalent. But that wasn't realized until a few years later (see subsection f, below).

This general idea, that synaptic resistance can be changed by activity, is nowadays 'common sense'. We've come a long way since Hartley spoke of unseen "Vibrations", and since Sherrington showed that synapses do actually exist. *Just how* synaptic change happens, at the neurochemical level, may still be controversial. But *that it happens at all* is taken for granted. In Hebb's time, that wasn't so.

The opening page of his chapter on the 'Growth of the Assembly' acknowledged as much:

> The first step in this neural schematizing is *a bald assumption* about the structural changes that make lasting memory possible. The assumption has repeatedly been made before, in one way or

another, and repeatedly found unsatisfactory by the critics of learning theory. I believe it is still necessary. As a result, I must show that in another context, of added anatomical and physiological knowledge, it becomes more defensible *and more fertile* than in the past.

The assumption, in brief, is that *a growth process accompanying synaptic activity makes the synapse more readily traversed*. This hypothesis of synaptic resistance, however, is different from earlier ones in [various] respects . . . (1949: 60–1; italics added)

He spoke of "a bald assumption" because no one knew just how synaptic resistance could be changed. (Lashley had stressed this fact, in his attack on theories based on the assumption: 1924.) The change might take place, Hebb suggested, through the growth of synaptic knobs, which would increase the area of contact between the two neurones concerned (1949: 62–6). But whatever happened at the level of (what we'd now call) the wetware, the need for an adequate higher-level, functional, explanation made the assumption "necessary".

The term "connectionism" is Hebb's coinage. But if someone had asked him whether he was a connectionist, he'd have dithered. He was certainly a connectionist as opposed to a field theorist:

Two kinds of formula have been used [to explain what goes on within the cortex], leading at two extremes to (1) switchboard theory, and sensori-motor connections; and (2) field theory . . . [The first claims that direct sensori-motor] Connections rigidly determine what [the] animal or human being does, and their acquisition constitutes learning . . . [The second] denies that learning depends on connections at all, and attempts to utilize instead the field conception that physics has found so useful. (1949, p. xvii)

(That's to say, Hebb accepted the second tenet of 'Newtonianism'.) However, he wasn't the then usual kind of connectionist, because the sixth tenet was being dropped:

[My] theory is evidently a form of connectionism, one of the switchboard variety, though it does not deal in direct connections between afferent and efferent pathways: not an "S–R" psychology, if R means a *muscular* response. The connections serve rather to establish *autonomous central activities*, which then are the basis of further learning . . . It does not, further, make any single nerve cell or pathway essential to any habit or perception. (1949, p. xix; first and third italics added)

The "no single cell" disclaimer was an obeisance to Lashley, whose search for the engram had suggested that memories aren't stored at specific locations.

A further ground for dithering was that, for Hebb, connections weren't the whole story. Timing was crucial too:

In a single system, and with a constant set of connections between neurons in the system, the direction in which an entering excitation will be conducted *may be completely dependent on the timing* of other excitations. *Connections are necessary*, *but may not be decisive in themselves*; in a complex system, especially, time factors *must always* influence the direction of conduction. (1949: 10–11; italics added)

Hebb admired Skinner greatly, as we've seen. But he also regarded Gestalt psychology as hugely important (1949: 23). His own approach was "based at least as much on *Gestalt* as on [behaviourist] learning theory" (p. 59). His major disagreement with both of them was that, in their very different ways, they each assumed "a sensory dominance of behavior" (p. 3). (This was more true of the early Gestaltists, such as Kohler, than of Bartlett.) By contrast, Hebb would emphasize largely autonomous *central* (cortical) processes. For Hebb's connectionism was defined in terms not of the linear single-cell circuitry of "switchboards", but of cyclical activity in *populations* of neurones—or "cell assemblies".

c. The cell assembly

Like the authors of the future cognitive science manifesto (6.iv.c), Hebb was concentrating not on the "S" and the "R" but on the hyphen between them. But whereas they would spell out the hyphen in computational terms, Hebb spelt it out in terms of (notional) neurophysiology.

Craik had done this before him, of course (Chapter 4.vi). But besides being unknown in the post-war USA, his account of "models" in the cortex was even sketchier than Hebb's. Indeed, Hebb's theory of cell assemblies could be used to put (speculative) flesh onto Craikian bones. So psychologists in the UK, already primed by Craik, were no less excited by *The Organization of Behavior* than their colleagues across the seas.

Because of the widespread mid-century doubts about the very possibility of learning-by-changes-in-synaptic-resistance (see subsection b, above), Hebb had to develop an unorthodox neurophysiology. Previous brain scientists (including McCulloch in 1943, though not in 1947) had tended to concentrate on single-cell circuitry. But Hebb focused on interacting *groups*, of neurones, or cell assemblies.

He proposed "two radical modifications" of earlier ideas about synaptic transmission. Transmission isn't simply linear, he said, but "always involves some closed or recurrent circuits". And a single impulse can't normally cross a synapse: "two or more must act simultaneously, and two or more afferent fibers must therefore be active in order to excite a third to which they lead" (1949: 10).

This led to a third neurophysiological idea with "revolutionary implications for psychology"—namely, the cell-assembly hypothesis:

> It is proposed first that a repeated stimulation of specific receptors will lead slowly to the formation of an "assembly" of association-area cells which can act briefly as a closed system after stimulation has ceased; this prolongs the time during which the structural changes of learning can occur and constitutes *the simplest instance of a representative process* (image or idea) . . . (1949: 60; italics added)

Considered as a physical structure, a cell assembly is "a closed solid cage-work, or three-dimensional lattice" (p. 72). It has "no regular structure", and connections are possible "from any one intersection to any other".

The implication was that only "stimulation of receptors" engendered significant structure in the cortex. Without it, cortical activity would be largely random. Whether such stimulation needed to be continued in order to *maintain* this structure was then an open question, although Hebb's belief was that it did. Eventually, it led to countless studies of "sensory deprivation" (Zubek 1969), starting with Woodburn Heron's hiring of college students to do virtually nothing for twenty-four hours a day (Heron 1957). (Heron also did the "brilliant" experiments on stabilized images which, Hebb said later (1980: 98), "abolished" his doubts about cell-assembly theory: Pritchard *et al.* 1960.)

In today's terminology, Hebb was describing a form of "unsupervised" learning (see 12.ii.b). There's no 'tutorial' feedback, to make the pattern of activity in the cortex become correlated with the input pattern. Mere temporal simultaneity/contiguity does the job.

A cell assembly is an example of what's now called distributed memory (Chapter 12.v–vi). The representation, or 'engram', isn't stored in any one cell; and the individual neurones making up an assembly needn't be spatially near one another. Nor need they all be firing when the assembly as a whole is activated: to that extent, a cell assembly is a "statistical" concept (1949: 77).

Moreover, because of the continuous (reverberating) self-excitation of the cell assembly, a *partial* stimulus can activate the assembly as a whole. (This includes cases where an object that stimulates several senses can eventually be recognized by one sense alone: hearing someone's voice, for example, brings many other aspects of that person to mind.) For the same reason, slightly *dissimilar* stimuli can activate one and the same assembly, thus allowing for stimulus generalization (McCulloch's "knowledge of universals": Chapter 12.i.c). And cell assemblies are noise-tolerant in another sense too, for they can continue to function even if some component cells are damaged.

It followed, Hebb said, that a concept isn't something cut-and-dried: "Its content may vary from one time to another, except for a central core whose activity may dominate in arousing the system as a whole" (p. 133). This important idea wasn't picked up by experimental psychologists until much later—and even then, it wasn't picked up from Hebb (see Chapter 8.i.b). Meanwhile, most psychologists continued to think of concepts as sets of necessary and sufficient conditions (6.ii.b–c).

(Hebb continued by saying: "To this dominant core, in man, a verbal tag can be attached; but the tag is not essential." This idea, too, would be resurrected later: see 12.x.g.)

Much the same was true of the way concepts were being envisaged in philosophy. Although Ludwig Wittgenstein had by then developed his notion of "family resemblances", his later work wasn't yet officially published. Only a tiny handful of the people doing empirical studies of language were taking any notice—and they, perhaps surprisingly, were working on machine translation (9.x.d).

Even horse-racing officials took the (common-sense) cut-and-dried view. So the UK's Jockey Club laid down supposedly strict rules for deciding automatically (by photography) which horse had won the race. However, Alan Turing himself possessed a print of a photograph showing 6 inches of spittle spewing out of a horse's mouth, which had forced the officials to reconsider the rules: was the spittle to be counted as part of the horse's head, or not? (Schaffer 2003*b*). He showed the photograph to his Manchester colleague Michael Polanyi, who delightedly used it as ammunition in his lectures defending "tacit" knowledge (see 13.ii.b). But Polanyi was a philosophical maverick. Most people, in or out of the Jockey Club, assumed that concepts were, or anyway should be—and therefore *could* be—defined (represented) strictly. Hebb's cell assemblies cast doubt on that assumption.

It also followed from Hebb's theory of cell assemblies that "there are two kinds of learning" (1949: 111). One is the learning of the newborn baby, or of the adult reared in darkness but now presented with light. This, said Hebb, is very slow—especially in primates. Indeed, the higher up the phylogenetic scale, the slower infantile learning is. (He cited evidence from experiments on insects and various mammals, and from 'restored-sight' patients such as those studied later by Richard Gregory: see 6.ii.e.)

That was an unorthodox claim: "We are not used to thinking of a simple perception as slowly and painfully learned" (p. 77). The reason was this:

A triangle then is a complex entity in perception, not primitive. As a whole, it becomes distinctive and recognizable *only after a prolonged learning period* in which there is a good deal of receptor adjustment—head-and-eye movement . . . [These] changes of visual fixation and some locomotion occur freely. The problem is to show how the variable stimulation which results from such movements can have a single effect, the perception of a single, determinate pattern. (p. 84; italics added)

Each grossly different pattern of stimulation, as the object is seen from one side or another, requires the establishment of a *separate* set of cell-assemblies . . . [The] various sets of assemblies would *gradually* acquire an interfacilitation—*if* sight of the object from one angle is often followed by sight of it from another. Arousing one would then mean arousing the others, and essentially the same total activity would be aroused in each case. (p. 91; italics added)

Only *Homo sapiens* can achieve this with full generality. A rat, for instance, can learn to perceive a triangle. But he fails to recognize it if it's rotated by 60 degrees, or if the familiar white-on-black is switched to black-on-white. The difference lies in the difference between the brains:

The possession of large association areas [in the cortex] is an explanation both of the astonishing inefficiency of man's first learning, as far as immediate results are concerned, and his equally astonishing efficiency at maturity. (p. 126)

The "equally astonishing efficiency" referred to the second type of learning, which is faster—even instantaneous. A human adult, said Hebb, can glance once at a face and remember it indefinitely. Yet a face is a highly complex structure, whose neurophysiological representation must code many different relationships.

Learning such complexity so quickly is far beyond the ability of a rat. Even human babies can't recognize individual faces until they're several months old. (Hebb assumed that faces are learnt *purely* by experience, since the newborn's cortex is randomly connected—see e.g. pp. 68–9, 71. Much later, it was discovered that innate neural mechanisms aid the baby's face learning, and seed the abilities of the adult—see 14.ix.c.) The adult human, however, has built up a host of mutually facilitative cell assemblies. Their associative structure is already so rich that the person can 'instantly' learn complex stimulus patterns: "The prompt learning of maturity is not an establishing of new connections but a selective reinforcement of connections already capable of functioning" (p. 132).

This aspect of Hebb's work defused some of the psychological disputes raging at mid-century. These had opposed "incremental" to "single-trial" learning, and learning to "insight". His distinction between early and mature learning integrated much of the data supporting these competing theories (pp. 109–20).

So Lashley's experiments on memory and learning were respected, and to an extent explained. Indeed, they were explained—or so Hebb claimed (Lashley disagreed: see subsection e, below)—in a way already intimated by the great experimenter himself:

[Lashley (1929*b*)] himself has pointed out [that his] evidence is consistent with the idea that the trace is structural but *diffuse*, involving, that is, a large number of cells widely spaced in the cortex, *physiologically but not anatomically unified*. (Hebb 1949: 13; italics added)

The physiological 'unification', according to Hebb (p. 41), was a matter of "connections and specialized conduction paths", not—as both Kohler and Lashley had thought—of neural "gradients and fields". The perceptual psychologist's classic problem of *stimulus generalization* was solved too—not by innate stimulus equivalence (favoured by Kohler and Lashley) but by learning.

d. Beyond perceptual learning

Hebb's vision for psychology went far beyond perception and sensori-motor learning. It covered thinking, attention, expectancy (set), purpose, emotion, and even psychopathology.

Psychopathology was one of his long-standing interests. He'd read James and Freud (and Watson) before enrolling as a psychology student. And, having worked with Wilder Penfield in Montreal after leaving Lashley and Harvard, he'd gained clinical experience of various types of brain damage and mental illness. Three chapters of his book were devoted to these issues—including a discussion of 'Mental Illness in Chimpanzees', such as "neurotic" and "psychotic episodes" and "depression" (1949: 245–50).

Mental illness? And in *chimpanzees*? Behaviourists' minds boggled. But the cell assembly was supposed to show how all of this is possible—hence Hebb's promise that his form of connectionism would be "more fertile than in the past" (p. 60).

He suggested, for instance, that temporally organized *thinking* is the sequential activation of a number of cell assemblies:

> Any frequently repeated, particular stimulation will lead to the slow development of a "cell-assembly" . . . capable of acting briefly as a closed system, *delivering facilitation to other such systems* and usually having a specific motor facilitation. *A series of such events* constitutes a "phase sequence"—the thought process. Each assembly action may be aroused by a preceding assembly, by a sensory event, or—normally—by both. The central facilitation from one of these activities on the next is the prototype of "attention". (1949, p. xix; italics added)

Besides temporal sequence, Hebb's theory allowed for structural complexity. Phases and phase cycles, like Lashley's motor skills and Bartlett's schemata, were assumed to be hierarchical. (Hebb often used the term "schema" as a near-synonym for "phase" and "phase sequence": e.g. p. 121.) It followed that "two ideas or concepts to be associated might have, not only phases, but one or more subsystems in common" (p. 131). This makes memory, and therefore thought, easier than in the case of "a simpler perception without [complexity or] meaning". Indeed, said Hebb (p. 132), that's why people trying to remember a list of unrelated things normally construct a meaningful mnemonic to help them do so (see ii.b above, and Chapter 6.iv.c).

Phase sequences—which, largely because of their hierarchical structure, can include alternative pathways—can be learnt from experience, just as individual cell assemblies can. So thinking can improve with practice. It can also deteriorate with practice: a maladaptive sequence, such as a neurotic's obsessional idea, will become more entrenched with repetition.

Like McCulloch before him (Chapter 4.v.c and d) and Kenneth Colby after him (7.i.a), Hebb was interested in neurosis. But like McCulloch (and unlike Colby) he didn't focus on the specific semantic content of individual neuroses. He was more interested in how *neurophysiological* factors could explain their origin and/or functioning:

It is assumed that the assembly depends completely on a very delicate timing which might be disturbed by metabolic changes *as well as by* sensory events that do not accord with the pre-existent central process. When this is transient, it is called emotional disturbance; when chronic, neurosis or psychosis. (p. xix; italics added)

He admitted that he couldn't fully explain the chimpanzees' mental illness—nor human neuroses either (p. 250). But the answer must lie in cell-assembly country:

Freudian theory has the credit of recognizing the existence of a kind of learning that causes, apparently, no immediate emotional disturbance and yet may contribute to one much later. Such learning undoubtedly occurs . . . [but] we must find some way of incorporating it into other learning theory—not as an *ad hoc* assumption specially made to deal with mental illness. (p. 250)

He suggested that emotional disturbances can prompt long-lasting "adaptive" responses of fear or aggression, associated with the thought patterns of the individual. The phase sequences concerned are assembled (learnt) differently, according to personal circumstances. So a child brought up in a punitive authoritarian regime learns that anger directed at the parents is ineffective, and becomes an adult whose anger is diverted onto those whom he/she believes can be influenced in some way and/or onto those who show (forbidden) insubordination (pp. 257–8).

In general:

. . . mental illness consists either of a chronic disturbance of time relations in the cerebrum, or a lasting distortion of the thought process from such a disturbance at an earlier time . . . [The illness may be "adaptive" in that it consists in] changes in thought (new phase sequences) that are not characteristic of the majority of the population but which avoid some conflict that makes for major disruption of [the] assembly or phase sequence . . . neurosis or psychosis is a product neither of experience nor of [neurophysiological] constitution, but a joint product of both. (p. 259)

It followed, as a matter of logic, that no mental illness can be caused *simply* by one's childhood experiences.

Although Hebb didn't use the term, nor belabour the point, this was an example of "epigenesis" (see Chapter 7.vi.g). Epigenetic development doesn't fit the nature/nurture dichotomy, because what happens at any given moment depends on *both* the genetic constitution (and past history) *and* the current environment—including the bodily tissues.

Where brain development is concerned, "environmental" influences are of several different kinds. They obviously include (1) sensory stimulation from the outside world after birth. Less obviously, they include (2) sensory stimulation from the external world before birth (newborn babies can recognize the intonation patterns of their mother's language). They also cover (3) sensory stimulation in the womb (as the foetus moves its limbs or sucks its thumb). And (4) non-sensory internal factors qualify too—for instance, random 'noise' as the unborn baby's neurones fire spontaneously.

Hebb didn't have specific behavioural (or histological) evidence of pre-natal learning. But he explicitly allowed for it, when discussing the lowering of synaptic resistance by the growth of synaptic knobs: "[this] is learning in a very general sense, which must certainly have begun long before birth" (p. 66). Similarly, he said that "the intrinsic organization of cortical activity" that's present at birth is (in that sense) "innate", but it "may or may not be unlearned": it could have been " 'learned'—established *in utero* as a result of the neural activity itself" (p. 121 n.).

That insight was relegated to a footnote. Half a century later, spontaneous self-organization in the cortex would be observed and modelled (Chapter 14.vi.b and ix.c). And innateness would be "rethought" much as Hebb had suggested (7.vi.g).

In short, Hebb's position on the development of mental illness was all-of-a-piece with his physiological psychology. As for whether psychoanalysis works better as a therapy than therapies based on conditioning, he doubted it. But no one knew. Moreover, "electroshock" and lobotomies were options too (pp. 271–4). The one sure point was that questions about therapeutic effectiveness were empirical questions—requiring experiments, not dogma (p. 260).

e. Hebb's originality?

Just how original was Hebb's theory? Connectionism, to be sure, was a new word. But it was a very old idea, and had already appeared in a number of neurophysiological guises. So what about Hebb's way of expressing it, in terms of cell assemblies?

According to Hebb himself (1949: 10, 61), the cell assembly was a development of Rafael Lorente de No's 1930s work on self re-exciting, or reverberatory, circuits (1933*a*,*b*, 1934, 1938). He'd found out about this, he said, from the new textbook on learning theory (Hilgard and Marquis 1940). Thirty years later, Hebb was still acknowledging his debt to Lorente do No:

> [The cell assembly theory] certainly looked improbable to its author—me—when it was first conceived [because it makes the ease of perception of common objects the result of a long process of learning].
>
> The problem of perception remained intractable for about five years (1939 to 1944) and as a result I made no progress in my attempt to understand concepts and thought. It seemed obvious that concepts, like images, must derive from perception, and I could think of no mechanism of perception that corresponded to my . . . preconceptions. *In fact, by 1944 I had given up trying to solve the problem.* What happened then was that I became aware of some recent work of Lorente de No in conjunction with some observations of Hilgard and Marquis (1940) [which led me to think about the problem] from a different point of view. . . .
>
> The essential basis of an alternative view was provided by Lorente de No, who showed that the cortex is throughout its extent largely composed of enormously complex closed or re-entrant paths, rather than linear connections only between more distant points. . . . When an excitation reaches the cortex, instead of having to be transmitted at once to a motor path, or else die out, it may travel round and round in these closed paths and may continue to do so after the original sensory stimulation has ceased. (Hebb 1980: 83 ff., 87; italics added)

That's surprising, since (as he noted—1949: 10) Lorente de No's "revolutionary" neural-feedback ideas were "well enough known by now to need no elaborate review". The cyberneticists had picked them up in the 1930s (see Chapter 4.iii.c). And Lashley had been writing about them for some years (Lashley 1938, 1942).

Half a century later, indeed, Jack Orbach (1998) would argue at length that Hebb owed a huge, and unacknowledged, debt to Lashley. Specifically, Hebb used four of

Lashley's ideas in defining the cell assembly: "non-sensory control of behavior, the central autonomous process, mechanisms of attention, and the importance of Lorente de No's reverberatory circuit" (Orbach 1998, p. xii). What's more, he adopted Lashley's focus on neurone groups—the assembly being his name for what Lashley called "neural lattices". (Hebb sometimes described the cell assembly as "a three-dimensional lattice", as we've seen.)

The notion that neural networks can be *learnt*, Orbach admitted, was indeed original to Hebb:

> Probably the most enduring idea embodied in the 1949 monograph, and for which the monograph is justly famous, is the *empirically assembled* nerve net that Hebb dubbed the "cell assembly." I would venture the opinion that the conception that functional nerve nets can be assembled *by experience* is one of the more important ideas in neuropsychological theory of the twentieth century. *Lashley never developed that idea, nor did he ever acknowledge it in print*. We have to credit the 1949 monograph for its dissemination. (Orbach 1998: 71; italics added)

Orbach's "controversial and startling if not downright outrageous" claim (1999) attracted significant peer commentary, most of it highly critical. For example, Peter Milner (1999)—who back in the 1950s had improved Hebb's learning theory by including inhibition (see below)—pointed out that Lashley's work on reverberatory circuits was very different from Hebb's. Whereas the former posited *innate mechanisms of stimulus generalization in perception*, the latter focused on *the learning of neural mechanisms representing concepts*.

Orbach knew that, of course. He'd even noted that on the only occasion when Lashley referred to Hebb in public after 1949, he disagreed with him. (The reason for Lashley's near-silence on Hebb, according to Orbach, was that the key ideas listed above had long been discussed in his own publications.)

In his last lecture, given in 1957 and never published before being summarized in Orbach's Prologue, Lashley criticized Hebb's account of stimulus equivalence as fundamentally behaviouristic: "[of all current learning theories, it's] the most in accord with conditioned reflex theory". He also dismissed it as mistakenly externalist, being "ruled out by a mass of evidence for innate [*sic*] discriminations and equivalencies". Moreover, in his four Vanuxem Lectures, given at Princeton in 1952 (and printed for the first time in Part II of Orbach's volume), he developed his long-standing view that learning *can't* be explained in terms of lowered synaptic resistance after cell firing—if only because unactivated neurones appear to learn too (Lashley 1924, 1952). (In fact, Hebb had explained this: neurones could be activated indirectly, through their membership of assemblies linked to the prime candidate.)

Orbach noted also that Lashley had been invited by Hebb to co-author *The Organization of Behavior*, but had declined. On scrutinizing the comments that Lashley gave to Hebb in early 1946, Orbach inferred that Lashley had read only the first ninety-six pages of the 300+-page draft. (These pages, however, contained the most sustained *neurophysiological* discussion.) He found that Hebb had made no changes as a result of Lashley's comments, not even when they concerned his descriptions of Lashley's own work.

This led Orbach to suggest that Hebb had asked Lashley to look at the manuscript not because (as he told others at the time) he thought the book would be more widely read with Lashley's name on the title page, but because he wanted to ensure that Lashley wouldn't claim the ideas as his own. Given that he evidently had no intention of making changes as a result of the comments, said Orbach, why else would he have asked for them?

This intriguing personal snippet doesn't prove much, however. Probably, Hebb had expected that some of Lashley's comments would be (from his point of view) worth taking on board. Possibly, he felt that Lashley—as his mentor—would be offended if he wasn't asked to comment. What's more, the story could be glossed *against* Orbach's main claim. For Lashley's failure to protest that his ideas were being either stolen or misused (as opposed to misdescribed) in Hebb's manuscript suggests that he didn't recognize them there.

That's hardly surprising. The two men's theories were very different—as Milner would point out later and as Lashley's own comments of 1957 (quoted above) confirmed. Moreover, by the 1940s no one connected with the cybernetics community—centred at MIT and Harvard, Lashley's base camp—could seriously have imagined that reverberatory mechanisms were "his" idea alone.

In sum, Orbach's ascription of priority to Lashley is, in my view, mistaken—albeit not "outrageous". Hebb is rightly credited with the cell assembly, even though the idea of neuronal reverberation (and neural lattice) was already being used by others.

For sure, his contemporaries saw him as original—and it was the cell assembly which excited them. His work had an enormous influence on experimental psychology, firing an explosion of research on early learning and/or sensory deprivation over the next decades.

Some people, for instance, asked how the development of vision depends on active, as opposed to passive, movement (Held and Hein 1963). Others observed the effects of rearing kittens in the dark, or in unnatural environments—such as having only vertical stripes to look at (Hirsch 1972). A few asked whether kittens could learn to overcome fundamental changes in the position of an eye (D. E. Mitchell *et al.* 1976). Yet others probed the effects of sensory 'silence' on mature human beings (Zubek 1969).

These studies weren't all purely behavioural: sometimes, brains were involved too. After the discovery of feature detectors and ocular "columns" in visual cortex (Chapter 14.iv), neurophysiologists soon asked whether these anatomical features could be modified by changes in early visual experience (C. Blakemore and Cooper 1970). In short, psychology was now being complemented by real neurophysiology, just as Hebb had hoped.

What's more, it was being complemented by computer modelling too.

f. Loosening the mantle

Hebb's book didn't achieve its phenomenal success with cognitive psychologists immediately (see the Mandler quotation, above). That's so *despite* the fact that parts of it had been read before publication by two of the manifesto writers featured in Chapter 6.iv.c: Karl Pribram, a fellow student of Lashley's, and Miller (Hebb 1949, p. vii). Not until the mid- to late 1950s would computer modelling become successful enough, or anyway promising enough, to shrug off the behaviourist mantle. For all that, Hebb had *loosened* the mantle, by offering a clear neurophysiological theory in support of mentalism.

However, there's clear—and then there's *clear*. Despite Hebb's claim to have stated the ft/wt (fire together, wire together) principle "precisely", there were three important unclarities. The first was that he supposed his two formulations (given at the beginning of subsection b, above) to be equivalent—but they weren't. Second, the earliest computer implementation of his work ran into unsuspected difficulties, forcing a redefinition of his learning rule. And third, when others came to define it later, they found that there were many different ways of doing so.

The first computer model of Hebb's rule was produced at IBM's research laboratory in Poughkeepsie, by a group led by Nathaniel Rochester, IBM's manager of information research (Rochester *et al.* 1956). It was begun soon after the appearance of *The Organization of Behavior*, and benefited from regular consultation with Hebb himself. It also built on network models being produced by Belmont Farley and Wesley Clark at MIT, and pioneered by Raymond Beurle at Imperial College, London (see Chapter 12.ii.b).

The Hebb simulation was one of the earliest examples of computer modelling being used to test/improve a specific psychological theory. And the effort was well spent, for Rochester's group discovered several previously unsuspected problems.

The first was that if Hebb's learning rule was implemented (as it was, initially) by McCulloch–Pitts on–off threshold units, a runaway positive feedback ensued—so that the strengths of all the synapses increased to saturation. To counter this, they introduced a "normalization" rule, according to which the sum of all the synaptic strengths remained constant. Strengthening *here* would be compensated by weakening *there*. They also mimicked neuronal fatigue, so that a unit that had just fired would be unlikely to fire again immediately.

Even so, the network (of 69 to 99 units) didn't develop recognizable cell assemblies. So the IBM group tried again. Now using a network of 512 units, they followed Peter Milner's (unpublished) suggestion that Hebb's theory needed to include *inhibition*. (This wasn't cheating. Sherrington had shown that the brain must contain inhibitory neurones, although at mid-century their manner of function still wasn't understood: see Chapters 2.viii.d and 4.iv.b.) The idea, here, was that cell assemblies compete. That is, they're not only self-exciting (reverberatory) but also other-inhibiting.

The units, or "neurones", were made less simple accordingly. Instead of being binary, unit-activity was now graded from 1 to 15. A mathematical measure of unit-activity (the ancestor of several such measures used today) took into account both synaptic strength and the average frequency of past firing. And Hebb's ft/wt rule was modified too. If the frequencies of the pre-synaptic and post-synaptic units were correlated then the synapse would be strengthened, and if one unit fired and the other one didn't then it would be weakened. That helped: Rochester was now able to say, "there is no doubt that cell assemblies did form, [even though the model] still needs improvements".

Rochester's paper had begun by making a historical point about the need for interdisciplinarity:

> As the neurophysiologist considers more and more complicated structures of neurons he gets into problems that are less and less related to his normal way of thinking. Curiously, however, some of these problems do not begin to resemble parts of psychology. *What is happening is* that the neurophysiologist is beginning to think about information handling machines that are too complex to be understood without the specialized knowledge of other disciplines. These other

disciplines are *information theory*, *computer theory*, *and mathematics*. People in these other fields need to augment the work of the neurophysiologists and psychologists before the brain can be properly understood. (Rochester *et al*. 1956: 80; italics added)

Across the pond, Horace Barlow, Giles Brindley, and the young David Marr were thinking along broadly similar lines (14.iii and v.b–e). In short, neurophysiology was beginning its long metamorphosis into computational neuroscience.

As for connectionist modelling, many people followed Rochester's example. Over the next forty years they found that there are many "Hebbian" rules, which give rise to different learning profiles (Chapters 12 and 14.v–vi).

Hebb himself hadn't pursued this methodology. When he was writing his 1949 book, it wasn't yet technologically possible (Chapter 3.v). The very first connectionist systems were wire-and-solder contraptions, not computer programs (see 12.ii.a–b). And even computers, in those days, could be used only by the technically minded. Hebb had originally aspired to be a novelist, settled sensibly for schoolteaching, and studied psychology part-time. He got his Ph.D., at 32, without having had time to learn engineering too.

It's not even clear that he was tempted towards modelling, whether material or mathematical. He hailed the work of McCulloch, and other members of Nicholas Rashevsky's "Mathematical Biophysics" group, as interesting. Indeed, he did this on the very first page of his book. But he valued McCulloch's neuro-anatomical research (cited several times in the text) at least as much as his mathematical studies: neither the 1943 nor the 1947 paper was mentioned, or listed in the fifteen-page bibliography. Indeed, he may not have known about the "Universals" paper:

For the present, Kohler and Lashley are the only ones who have attempted to say *where* and *how* perceptual generalization takes place. (1949: 38)

This was a mistake, for the 1947 Pitts–McCulloch paper had attempted to do just that (Chapter 14.iii.a).

Moreover, Hebb was suspicious of idealizations. Endorsing a recent review by the Washington University neurophysiologist George Bishop (1946), he complained that Rashevsky's group had been "obliged to simplify the psychological problem almost out of existence" (1949, p. xi).

It's intriguing, therefore, that one of the four people thanked for their "painstaking and detailed criticism" of his 1949 manuscript was the MIT (and Bolt, Beranek, & Newman) psychologist Joseph Licklider (1915–90). Licklider (1951, 1959) was already building simple connectionist models of perception, some of which implemented temporal correlations—stressed by Hebb, but largely neglected by computer modellers for many years afterwards (14.ix.g).

Looking back in the 1990s, he recalled: "I was one of the very few people, at that time, who had been sitting at a computer console four or five hours a day" (Edwards 1996: 264). This wasn't purely theoretical research. For in 1950 Licklider had outlined the interface design for the ambitious SAGE computer system, the US Department of Defense's (DOD's) early forerunner of the infamous "Star Wars" project of the 1980s (Chapter 11.i).

A few years later, Licklider would become a hugely important force in the development of computing, and of AI (Edwards 1996: 262–71). In 1962 he was appointed by ARPA, the DOD's Advanced Research Programs Agency (renamed DARPA in 1972: "D" for Defense), to found their information-processing section. In that role, he would enable—and influence—AI research for a quarter-century (Chapter 10.ii.a and 11.i.b).

(He sometimes acted also as a latter-day Gabriel Naudé, who had pioneered library science over 300 years earlier: see Preface, preamble. Licklider addressed advanced versions of Naudé's questions about librarianship by using new ideas about information technology, including timesharing, and by redescribing a library as a "thinking center": Licklider 1965.)

Apparently, Licklider didn't notice the unclarities in Hebb's formulation of ft/wt. If so, he wasn't alone. In the few years after 1949 it was the general approach which excited psychologists, not the nitty-gritty detail of just how ft/wt should be expressed. Or rather, they thought that he'd *provided* the nitty-gritty. The high standard of clarity enforced by computer modelling was then appreciated only by a tiny handful of aficionados. These, of course, included Licklider. But unless Hebb simply ignored his advice, as he did Lashley's, Licklider didn't pick up on the ambiguities either.

Rochester did. And the concluding paragraph of his paper drew a general moral:

> This kind of investigation cannot prove how the brain works. It can, however, *show that some models are unworkable* and provide clues as to how to *revise the models to make them work*. Brain theory has progressed to the point where *it is not an elementary problem to determine whether or not a model is workable*. Then, when a workable model is achieved, it may be that a definitive *experiment can be devised to test* whether or not the workable model corresponds to a detail of the brain. (Rochester *et al.* 1956: 88; italics added)

In short, computer modelling could help in the mind-as-machine project already taxiing along the runway.

A couple of years later, the project finally took off—fuelled partly by GOFAI and partly by cybernetics, including Hebb (Chapter 6.iv.b). The jet wash was strong. It blew away the behaviourist mantle, bringing all the non-Newtonian ideas sketched in this chapter into view.

6

COGNITIVE SCIENCE COMES TOGETHER

The 1950s saw the emergence of a self-consciously interdisciplinary cognitive science—though not, yet, under that name. Both the interdisciplinarity and the ideas had been prefigured in the cybernetics movement of the 1940s.

As for the interdisciplinarity, Warren McCulloch alone had combined no fewer than five areas: logic, computer science, philosophy, neurophysiology, and psychology—including clinical psychiatry (4.iii). His fellow cyberneticists had each integrated two or more of those, sometimes also weaving in anthropology, educational technology, or management/sociology (4.v.e). And, of course, biology and engineering had defined the overall frame.

As for the ideas, these came from both sides of the future divide in cybernetics (4.ix). Initially, the strongest influences in 1950s cognitive science were Claude Shannon's information theory and the concept of self-control by feedback. But thanks to the two epochal papers by McCulloch and Walter Pitts (1943 and 1947), plus the development of usable computers (3.v), there was an ever-growing interest in both symbolic and connectionist computation.

By the end of the 1950s, research using these ideas was more widespread and more confident. The confidence was expressed in 1960 in two highly visible ways: by the founding of an interdisciplinary centre, and by the publication of what turned out to be the manifesto of cognitive science. Crucial theoretical concepts were diffusing from one area to another.

The cognitive revolution began with psychology. Indeed, quite a few psychologists had opposed behaviourism well before 1950, and both Karl Lashley and Donald Hebb had recently hurled anti-behaviourist rocks into the water (see Chapter 5). Now, in the early 1950s, computational accounts of perception, concepts, and human decision making appeared on both sides of the Atlantic.

The other disciplines, at that time, were less easily visible. When the revolution picked up steam in the late 1950s, linguistics would provide much of its motive power (Section i.e, below). But only one linguist's work was important in this regard, and in the early 1950s he was still almost unknown. Neurophysiology was just beginning to feature informational/computational ideas (14.iii–iv), but connectionist AI was still in early infancy (5.iv.f and 12.ii). As for symbolic AI, this—apart from work in machine translation (9.x)—had hardly begun (10.i).

That being so, this chapter must begin by discussing psychology. How mind-as-machine first began to drive psychological experiments is outlined in Sections i–ii. These describe the development of informational psychology and the New Look, and sketch the first psychological work inspired by Noam Chomsky. Section iii recounts the electrifying effect of Herbert Simon's encounter with Allen Newell early in 1952, which prompted the first experimentally guided computer models of thinking.

The rest of the chapter relates how the various disciplines came together in the mid- to late 1950s, and how that led to the establishment of the first official research groups. It highlights the cooperation that's needed for science in general and interdisciplinarity in particular (2.ii.b–c). The three seminal meetings of cognitive science, the manifesto, and the early research groups are all described in Section iv. Finally, Section v says a little about how the word was spread in the following years.

(A note about nomenclature: This chapter is about what cognitive scientists were doing in the 1950s and early 1960s, and how they saw themselves at the time. As for what they *called* themselves, that varied—but it wasn't "cognitive scientists". The term "Cognitive Studies" was coined in 1960, and "cognitive science/s" had to await the early 1970s: see Chapter 1.ii.a. Even "cognitive psychology" wasn't named until 1967. So the current phrase is used here only for convenience: the people concerned weren't thinking of themselves under that label.)

6.i. Pointers to the Promised Land

The immediate forerunners of computational psychology—or, if broadly interpreted, its earliest examples—were based on information theory. At first, this was welcomed by psychologists because it offered quantitative measurements. Soon, its theoretical concepts were appreciated too. For it supported centralism without falling into homunculism: positing mysterious 'little men' in the mind/brain. (The soft centres were hardening: see Chapter 5.iii–iv.)

Information-processing psychology used ideas about machines (telephone systems) to conceptualize minds. But it didn't stress computer modelling, nor even programs. (The occasional exceptions included an early implementation of formal grammars—G.A. Miller and Chomsky 1963: 464–82.) The first person to use information theory to describe *the mind as a whole* was Donald Broadbent (see subsection c). But George Miller (1920–) was the first to apply it within (a more limited area of) psychology.

Miller was the first, too, to persuade psychologists that Chomsky's linguistic theory was relevant for them. Chomsky himself had deliberately remained silent on this point in the mid-1950s, for fear of frightening the horses (9.vi.e). In 1959, however, he published his notorious attack on behaviourism (9.vii.b). (His yet more provocative defence of nativism would be delayed even longer: see 7.vi.a and 9.vii.c–d.) Miller's interest in Chomsky was a key point in his own turn from "information" to "computation".

a. Informed by information

The mid-twentieth century saw an explosion of numerical/formal methods for describing mental processes, known collectively as "mathematical" psychology.

Some were developments of behaviourism, rendered axiomatic (by Clark Hull) or probabilistic (by Edward Tolman and Egon Brunswik): see Chapter 5.iii.b. For instance, William Estes (1919–), a founder of the Society for Mathematical Psychology, proposed a statistical theory of learning (1950, 1959). Eventually, he would develop mathematical models of many aspects of cognition.

Instead of treating the stimulus as an indivisible Newtonian atom (see Chapter 5.i.a), Estes saw it as *a set of stimulus elements*, only some of which will be "sampled" by the animal on any given trial. The conditioning that occurs involves those elements alone. So if the animal happens to sample a different subset on the next trial, the 'learnt' response won't ensue. Learning actually takes place in a single step; but it *seems* to be gradual, because time is needed for a response to be conditioned to all possible samples of the stimulus concerned.

Most mathematical psychologists were inspired by the cybernetic ideas outlined in Chapter 4.v–vii. Their new theories included John von Neumann's theory of games (von Neumann and Morgenstern 1944); signal-detection approaches (Wald 1950); Robert Luce's axiomatic theory of choice (Luce 1959); and Simon's (1957) account of decision making in social groups.

By the end of the decade, Chomsky's work on formal grammars was included too. Fully 40 per cent of the second volume of the *Handbook of Mathematical Psychology* (1963) was devoted to papers written or co-authored by Chomsky. As for the first volume, this carried a lengthy chapter on mindlike artefacts, ranging from the Homeostat, through GOFAI, to perceptrons (Newell and Simon 1963).

As well as new theories, mathematical psychology provided new methods of *measurement*. For instance, perceptual psychologists were offered a "geometrical" measure of similarity between structured stimuli (Eckart and Young 1936); each stimulus was located at a particular point within a multidimensional hyperspace (for an explanation of hyperspaces, see Chapter 14.viii.b). And psychologists in general were given an analysis of different types of measurement scale: some fully arithmetical, some dealing only with ratios, and some limited to the ordering of values (S. S. Stevens 1946). If psychology couldn't match the numerical precision of physics, it could use mathematical descriptions whose parameters were specified more vaguely.

So mathematical psychology was a broad church. But of all the new ideas fermenting in the 1940s, Shannon's were the most intoxicating. They were also the most important *as a lead-in to computational psychology*, for they dealt with the coding, or transformation, of information as well as its transmission. Psychologists theorizing about internal representations could look to Shannon in asking how—and why—these are constructed. Moreover, information theory, internal models, and computers were then considered all part of the same bag (Chapter 4.ix). Anyone interested in one was probably interested in them all.

Although information theory offered both explanatory concepts and numerical description, the initial feeding frenzy was due to psychologists' hunger for the latter. Measurement was widely believed to be essential for a psychological science.

In practice, not everyone agreed. Programs and generative grammars—both pioneered in this period—were formal systems having nothing to do with numbers. So were McCulloch and Pitts' logical networks, or TPEs (4.iii.e). (Indeed, science in general is best thought of as an empirically constrained search for *systematic structural possibilities*, of which numerical laws are a special case—Sloman 1978: chs. 2–3; see Chapter 7.iii.d.) But the initial attraction of information theory was its ability to provide numbers—that is, measurement in bits.

At base, Shannon's theory—like behaviourism—was a beads-on-strings affair, for it concerned sequences of events conceptualized as Markov processes. (It was therefore vulnerable to Chomsky's criticisms of Markovian theories: Chapter 9.vi.c.) But the informational psychologists used a non-Newtonian concept of the stimulus.

Instead of finding complexity inside the stimulus, as Estes had done, they found it outside. That is, a stimulus was no longer definable in isolation. Much as John Dewey and Ralph Perry had seen *stimulus* as a covertly purposive term (5.iii.a), so the information theorists saw it as covertly probabilistic. To be sure, information can be measured. However, a given input (or response) carries *this much* information if it's one of only two possible alternatives, but *that much* if it's one of three, or four . . . or more. In other words, *what the stimulus is* depends on what had previously been called environmental variance.

These psychologists asked how many bits a given sensory channel can carry. They looked to see whether some senses are more nicely discriminatory than others. And they studied how much (*sic*) it helps to have input from two or more senses simultaneously. Also, they asked how specific redundancies in the environment (in the signal) can improve the reliability of the message communicated.

For instance, Fred Attneave (1919–91) argued that perception relies heavily on redundancies (1959). A straight line, or a uniform curve, is highly redundant, highly predictable. But a sudden change of direction, a corner, or a break isn't. The perceiver should therefore pay more attention to those parts of the scene, or stimulus, which are low in redundancy. (A few years later, a remarkable Russian study of eye-movements would show that this is indeed what happens: Yarbus 1967.)

Eventually, psychologists woke up to the *theory*, not just the *measurements*. For example, the Oxford professor R. Carolus Oldfield (1954) used Shannon's concepts—plus ideas about computers—to formulate a theory of memory. He imagined a computer that codes redundancies, and stores stimuli in two parts: the core-schema and the deviances. Such a system, in principle, could represent facts like *All birds fly—but emus don't* (compare "frames" and "defaults" in GOFAI: 10.iii.a and 13.i.a). Oldfield envisaged hierarchies of coding schemas, much as Miller would later (see below).

As for problem solving, the typical information-theoretic approach was to remind their readers of the parlour game Twenty Questions (see 4.v.d). Only an idiot would ask *Is it a Manx cat?* before asking *Is it a cat?*, and some even wider questions—*Is it a mammal? Is it a domesticated animal?*—should come earlier still. The efficient problem-solver aims to halve the number of possibilities at each successive step. This assumes that all possibilities are equally likely: if they aren't, then other strategies must be calculated (Attneave 1959: 5–9). The core idea, that *some rational strategy or other* must be used, would later be picked up by the New Look researchers (Section ii.b, below).

Questions about the efficiency of different methods of coding were applied to neurophysiology too. By the end of the 1950s, Albert Uttley and (most notably) Horace Barlow were thinking about specific neural mechanisms in terms of economical coding (Chapter 14.iii.b).

Mathematical psychology didn't suit all tastes. Tolman, for instance, commented:

> Psychology today seems to me to be carried away (because, perhaps, of feelings of "insecurity") into a flight into too much statistics and too great a mathematization . . . [To] me, the journals seem to be full of oversophisticated mathematical treatments of data which are in themselves of little intrinsic interest and of silly little findings which, by a high-powered statistics, can be proved to contradict the null hypothesis. (1959: 150)

However, he was talking primarily about behaviourist curve fitting. One could share Tolman's impatience with obsessively precise mathematical *descriptions* while being excited by formal–mathematical *explanations*. Among the first to take the explanatory concepts of information theory to heart was Miller.

b. Miller and magic

The Harvard luminary Miller was the first information-theoretic psychologist. (I say "Harvard", but for four key years he was at MIT: 1951–5. He'd been appointed by Joseph Licklider, who'd set up MIT's Psychology Department a few years earlier.) He was initially trained by the psychophysicist Stanley (Smitty) Stevens (1906–73), who ran Harvard's Psycho-Acoustic Laboratory.

This was "the largest university-based program of wartime psychological research" in the USA, and it continued to thrive in the post-war years (Edwards 1996: 212). Much of the Lab's effort went on studying communication in noisy conditions, and on investigating the psychological side of what Licklider called "man–computer symbiosis" (Chapter 11.i.a–b). In other words, it was the American equivalent of the MRC's Applied Psychology Unit at Cambridge (see 4.vi, and subsections c–d below).

Miller was always more interested in speech and language than in pure psychoacoustics. As a youngster intending to go into clinical psychology, he'd worked as a speech therapist at the University of Alabama in 1942 (G. A. Miller 1986: 201). The acoustics was added after the US entered the war, when he spent two years on a top-secret speech-jamming project for the Signal Corps.

In the mid-1940s, when he worked on auditory "masking" (see i.c, below), he realized that, in effect, he could turn this wartime research upside down in order to understand how speech is recognized in normally noisy conditions. (Military concerns were still relevant. Indeed, Miller's then colleague Licklider would later enable research on HEARSAY and on DARPA's "intelligent pilot's assistant", intended to interpret speech inside a jet's cockpit: 9.xi.g and 11.i.c.)

Eventually, the speech conquered the acoustics. Miller chose the young Chomsky as his assistant for his Stanford summer seminar on mathematical psychology in 1957, and they soon co-published on formal grammars (Chomsky and Miller 1958; see Chapter 9.vi.a and c).

Even at that point, Miller was already asking questions about the "psychological reality" of syntax (1986: 208). In 1960 he introduced Chomsky to psychologists in general. He did this both in his cognitive science manifesto (Section iv.c, below) and in an "elegant lecture on the psychological implications of Chomsky's grammar, given at many major universities, [which] became famous" (Baars 1986: 199). Later still, he'd make major contributions to psycholinguistics, concentrating more on vocabulary than on syntax (7.ii.d).

As for his interest in computers, this dated from the 1940s when he read about cybernetics and the McCulloch–Pitts neurone (G. A. Miller 1986: 205). But it wasn't until the late 1950s that computational concepts became central for him (Section iv.c).

Meanwhile, he'd made his name as the leading light of information-theoretic psychology. He'd pioneered this even before the seminal Shannon–Weaver book was published. By 1948 he was grilling his new graduate students on their knowledge—typically non-existent—of information theory and hill-climbing algorithms (McGill 1988: 8). And by 1949, when the Shannon–Weaver volume appeared, he was already teaching a course on phonemes, Shannon, and the redundancy of English (Bechtel *et al.* 1998: 45). Ulric Neisser (1986: 274) remembers this as being based on the manuscript of Miller's first book (1951), and "all full of 'bits' and 'phonemes' and the like" (Neisser 1986: 274).

Miller soon applied information theory in designing experiments. In the early to mid-1950s, he did a number of studies showing that redundancy (i.e. familiar context) helps people to recognize and/or remember (G. A. Miller 1951; Miller and Selfridge 1950; Miller *et al.* 1954). The stimuli he used were Shannon-inspired letter strings and word strings that approximated English more or less closely (see Chapter 9.vi.c).

The 1954 experiment helped kick-start the New Look (Section ii.a). And in 1956 his work suddenly leapt onto centre-stage for a wide range of psychologists (see below). Before then, he'd been seen as an austere specialist—some of whose papers were so symbol-ridden as to be unreadable by those not mathematically inclined (e.g. G. A. Miller 1952).

In fact, it was Miller who'd introduced information theory to psychologists in the first place. Almost immediately after Shannon's publication of it, he'd co-authored a paper with Frederick Frick in the *Psychological Review* (G. A. Miller and Frick 1949). This provided a method for *quantifying* the organization, or patterning, in sequences of events (including responses) where only a few alternatives are possible. Two years later, he and Frick would apply their analysis to the behaviour of rats in Skinner boxes (Frick and Miller 1951).

Miller's enthusiasm for information theory wasn't instantly persuasive. The psychophysicist Wendell Garner recalls that at first, even many *mathematical* psychologists (including Stevens) were sceptical, seeing Shannon's work as providing just another descriptive statistic. But that reaction didn't last long:

> The change in attitude occurred as soon as psychologists saw that information theory was much more than just another statistic and that *there was a fundamental theory behind the statistics, with a set of useful new ideas and concepts*. In retrospect, I would say that the wait-and-see attitude lasted for little more than half a year, and after that it was accepted [by psychophysicists] at what can only be called a phenomenal rate. (Garner 1988: 22; italics added)

Miller (1953) soon wrote a beginner's guide for psychologists in general, published in the main journal of the American Psychological Association (APA). Looking back, Garner now sees Miller and Frick's paper as "the birth date of cognitive psychology" (p. 32). The concepts it introduced (borrowed, of course, from Shannon)—*information*, *redundancy*, *channel capacity*, *coding*—are now part of the daily vocabulary of cognitive psychologists.

Miller's high visibility to psychologists *in general*, however, arose in 1956. In that year, he published 'The Magical Number Seven, Plus or Minus Two'—probably still the most widely read paper in his œuvre. A bibliographic study in the mid-1970s identified it as the most-cited paper in the whole of cognitive psychology (E. Garfield 1975). Although that may no longer be true, it continues to be cited today. Moreover, even when the "seven" is challenged, the core theoretical point is accepted (Cowan 2001).

This paper didn't report any new data, but surveyed a huge range of—individually, pretty boring—experiments in the literature. In so doing, it lit a blazing fire.

Miller opened with a strange confession: "My problem is that I have been persecuted by an integer." And he went on to suggest—on the basis of many different experiments, in distinct domains and involving various senses (and dating back to William James)—that, as in fairy tales, there really is something special about the number seven:

> For seven[!] years this number has followed me around, has intruded in my most private data, and has assaulted me from the pages of our most public journals. This number assumes a variety of disguises, being sometimes a little larger and sometimes a little smaller than usual, but never changing so much as to be unrecognizable. The persistence with which this number plagues me is far more than a random accident. There is design behind it, some pattern governing its appearances. (G. A. Miller 1956*a*: 81)

That "pattern", as declared in the paper's subtitle, concerned 'Some Limits on our Capacity for Processing Information'. Considered as an information-processing channel, the human mind is limited to about seven items, or 2.6 bits. That's why telephone numbers ("my most private data") typically have no more than seven digits, and why one-dimensional psychophysical discriminations (reported in "our most public journals") and arbitrary scales (found in *Woman's Own* as well as in journals of psychometrics) normally run between five and ten.

Of course, some telephone numbers have as many as fifteen digits. But they're grouped into a small number of "chunks" (international code, country code, area code, city code, individual number), none of which has more than eight elements.

Similarly, we can remember sentences of many more than nine words (and very many more than nine phonemes: a word is itself a chunk). But this holds only if they're genuine sentences, hierarchically structured (chunked) as noun phrase, verb phrase, etc. Asked to remember lists of randomly chosen words, we're constrained by the magical number seven again. The only way of overcoming this constraint is to use some mnemonic to add structure to the random signal.

In this way, the total amount of information remembered can be hugely increased. For it's the *number* of chunks on a given structural level, not their information content, which matters. (Given the usual size of an English-speaker's vocabulary, one English word carries about ten bits: G. A. Miller 1956*b*.) Another way of putting this is to say that information theory alone can't describe memory, although it can describe absolute judgement.

(Occasionally, we may appear to recall many more than seven items, as in reciting the alphabet. But no one learns the alphabet in the first place as an unstructured twenty-six-letter string. And even an adult, who can gabble the whole alphabet without taking breath, is usually aware of some sort of chunking of the letters.)

In short, said Miller, "There seems to be some limitation built into us either by learning or by the design of our nervous systems, a limit that keeps our channel capacities in this general range." (Conceivably, work in comparative and/or computational psychology might one day be able to explain why the magic number was seven—not ten, or four: Chapter 7.iv.h.)

He didn't jump to all the seemingly obvious conclusions. The seven wonders of the world, the seven seas, the seven deadly sins . . . and so on *may not* depend on this fact. The appearance of the magical number there, as well as in the psychophysicist's laboratory, *may be* "only a pernicious, Pythagorean coincidence" (1956*a*: 97). Nevertheless, the limits on human channel capacity underlie some of our most distinctively human properties. For hierarchical chunking, or recoding, which enables us to escape this basic limitation, is the general principle underlying schemas of all kinds.

Moreover, it was information theory we had to thank. "Recoding", Miller pointed out, was part of "the jargon of communication theory". The experiments he'd reported "would not have been carried out if information theory had not appeared on the psychological scene, and [so] the results are analyzed in terms of the concepts of information theory".

He'd already emphasized recoding-by-chunking as a memory mechanism some years earlier (S. L. Smith and Miller 1952). But that was in a private research report from MIT to the US Air Force—and, like most of his early papers, was highly technical. Now, he was expressing his ideas more intelligibly and circulating them more widely. Psychologists *in general* would prick up their ears when he said: "In particular, the kind of linguistic recoding that people do seems to me to be *the very lifeblood of the thought processes*" (1956*a*: 96; italics added).

c. Going with the flow

The "lifeblood" of psychology, or anyway its central problem, is often held to be consciousness. Miller himself said as much in later years: "I consider consciousness to be the constitutive problem for psychology" (1978: 233).

In the 1950s, such a remark—at least in America (see iv.d, below)—would have been too shocking, too out of kilter with the temper of the times. Whereas James had devoted several chapters to human consciousness (including attention) in his *Principles of Psychology* (1890), Stevens's *Handbook of Experimental Psychology* (1951) ignored it. Nevertheless, one of the formative texts of informational psychology was primarily concerned with consciousness. Not with *how consciousness is possible at all* (the theme of Chapter 14.x–xi, below), but with *the conditions in which it does or doesn't occur*—and with *why it's limited*. Even then, however, the theme wasn't identified as "consciousness", but as the more anodyne "attention".

The author of that text was Broadbent (1926–93). He was an English psychologist then working with Frederic Bartlett, and later—like both Bartlett and Kenneth Craik before him—Director of the MRC's Applied Psychology Unit (APU) at Cambridge. (Later still, he was the MRC Professor of Experimental Psychology at Oxford.)

His main interests were attention and perceptual vigilance. In particular, he was interested in their effects on how people—from airline pilots, through car drivers, to factory workers—interact with machines. Today, decisions about *what* information should be presented to the human operator, and *when*, and *how*, are often made (in real time) by computer programs. That applies to many medical monitoring systems, and to NASA's ground control for the space shuttle (see Chapter 13.iii.d). Fifty years ago, it wasn't even clear just what questions should be asked about this. Broadbent tried to find out.

In this, he was following in the footsteps of Craik (Chapter 4.vi.c). Alan Baddeley, a student at the APU in the early 1960s, remembers:

> There was little pressure to publish. There was, however, a feeling of real intellectual excitement. We felt that we were at the forefront of a revolution—a revolution inspired by Kenneth Craik's insights, developed into a broad information-processing approach to the human mind as reflected by Donald Broadbent . . . in his highly influential book *Perception and Communication*. (Baddeley 2001: 348)

Many people were enthused by Broadbent's ingenious and hard-headed—and practically important—research. In 1968 it earned him election to the Royal Society; for many years, he was the only psychologist so honoured. (William McDougall had been an FRS too, but he was elected for his work in the relatively high-status *physiological* psychology: 1897, 1901, 1902–6, 1904, 1905.)

As a young man, Broadbent developed a number of ideas drawn from Uttley's early connectionist models (12.ii.c–d). Like Uttley, he'd been impressed by Shannon's then recent account of information theory. He applied this not only to the nervous system but also to perceptual masking—including the interference between different speech signals presented to the two ears simultaneously (Broadbent 1952*a*,*b*).

Real-life masking/unmasking phenomena include the "cocktail-party effect", in which one suddenly hears one's own name above the general buzz of conversation (Cherry 1953; cf. Treisman 1960). They also cover the many distractions of noisy work environments—where "noise" means *any irrelevant information* (from loud sounds, to visual signals on a multi-dial dashboard or flight deck). Sometimes, of course, one *wants* to be distracted, because the so-called noise is an important warning signal. But how is it possible for such a signal to be consciously noticed, among so many others competing for one's attention?

In such cases, the *selective* nature of consciousness is evident. Broadbent argued that this depends on the information-processing properties of the mind/brain, and especially on the channel capacity of the perceptual system. That system, he said, involves several input pathways working in parallel, sensory buffers, memory stores, and a central processor. His ideas dominated research on attention (aka consciousness) for over twenty years.

The twelve general principles announced at the close of his book included several which rang magic-number bells, such as: "(A) A nervous system acts to some extent as a single communication channel, so that it is meaningful to regard it as having a limited capacity" (1958: 297). Others recalled the New Look's interest (see Section ii) in how motivation and emotion affect perception:

> (E) States of the organism which increase the probability of selection of classes of events are those normally described by animal psychologists as 'drives'. When an organism is in a drive state it is more likely to select those events which are usually described as primary

reinforcements for that drive. Thus food has a high probability of being selected if the animal has been deprived of food for 24 hrs . . . (p. 298)

(K) There is [or rather, may be] a minimum time during which information from one class of event is sampled before any action is taken about it.

(L) This minimum time is [or rather, may be] shorter in persons who are extroverted, by Eysenck's operational definition of that word. (p. 299)

Broadbent imagined someone complaining that *we already knew* that attention is limited, that noises distract us . . . and so on: "What gain is there from putting these everyday experiences into this stilted language?" (p. 300). He replied that previous psychologists hadn't expressed these matters adequately, even if they'd sometimes stated them. The precision of information theory was a significant advance.

Moreover, this new theoretical approach had some non-obvious implications, including: "The interference between two tasks will increase as the probability of the stimuli in each decreases: two highly probable stimuli will hardly interfere with one another" (p. 300). In short, the common-sense notion that "noise distracts" is correct, but it doesn't capture *just when*, *just how*, or (above all) *just why*.

One of Broadbent's most influential contributions was to use flow diagrams to express psychological theories. They'd been developed in the early 1950s for designing computer programs. But very few people knew anything about that. So the first example given in Broadbent's book had to be carefully explained: today, it seems laughably simple (see Figure 6.1).

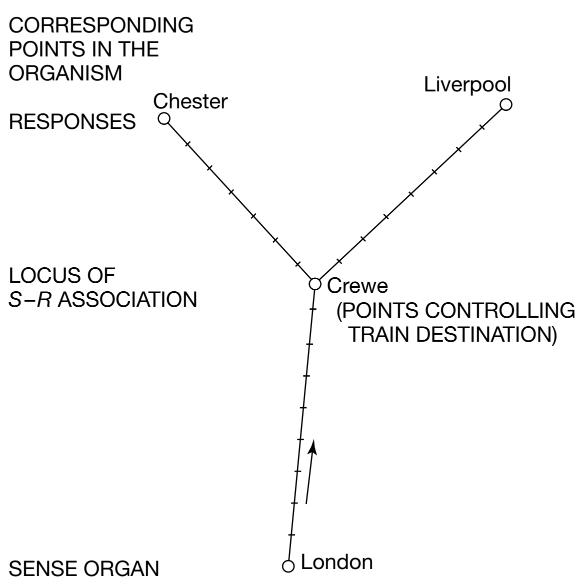


Fig. 6.1. Broadbent's introductory flowchart. Original caption: A railway system analogous to the flow of information through the nervous system in conditioning. The locus of 'inhibition' may be before that of S–R association: it is usually assumed that it is after it, for no good reason. Redrawn with permission from Broadbent (1958: 188)

Both flow diagrams and concepts derived from computer design were soon widely used, to describe not only perceptual information processing but also learning and memory:

* For instance, Earl Hunt's account of concept learning was profusely illustrated by flow diagrams and decision trees (see Figures 10.10–10.12).
* Memory stores for pronouncing or recognizing words were distinguished in John Morton's flow-diagrammed "logogen" model (1969, 1970). (For a recent development of that model, see Coltheart *et al.* 2001.)
* Thanks to the programming analogy, short-term memory was presented as an active "working" memory, not just a temporary data store, by Broadbent's student Baddeley (Baddeley and Hitch 1974).
* Also, it was found that recognition and (especially) recall are improved when their environmental conditions match those at the time of the initial "coding" (Tulving and Thompson 1973).

A more ambitious example of a flow diagram was given at the close of Broadbent's book (see Figure 6.2). Broadbent claimed that this single diagram included contemporary views on immediate memory, learning, anticipation, refractoriness, noise, multi-channel listening, and prolonged performance (p. 299). In other words, the organism was here being presented as an integrated system. And this, in a sense, was the whole story: "Nervous systems", he declared, "are networks of the type shown in Figure [6.2], *and of no other type*" (p. 304; italics added).

For all his hard-headedness, Broadbent was no behaviourist. Besides allowing one to emphasize "the relationship between the stimulus now present and the others which might have been present but are not", information theory fostered many hypotheses about internal mechanisms. But this wasn't mystery mongering:

> [Mine] is a non-positivistic approach, and attempts to find out what happens inside the organism . . . [Statements] about unobservables are not necessarily mystical or scientifically useless . . . [We] have tried to make our hypothetical constructs of such a kind that they could be

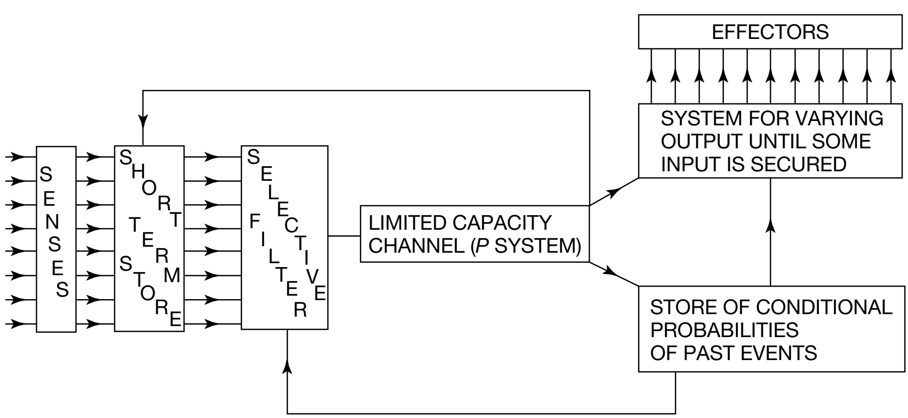


Fig. 6.2. Broadbent's "tentative information-flow diagram for the organism". Redrawn with permission from Broadbent (1958: 299)

recognized if it were possible to observe them directly: a filter or a short-term store might take different physiological forms, but it could be decided with reasonable ease whether any particular physiological structure was or was not describable by these terms. (1958: 302–3)

The physiology must always be borne in mind, and the psychological theory eventually explained by it. For instance, Broadbent cited the recent discovery that cells in a cat's auditory cortex are 'distracted' from responding to meaningless clicks if a mouse enters the cat's visual field (Galambos *et al.* 1956). But he pointed out—what will be emphasized throughout Chapter 14—that "it may often be preferable to explain a *physiological* fact by reference to its role in a well-understood *psychological* function" (p. 305; italics added).

Further, he said, there's room for purely psychological theorizing while remaining silent on the physiology. (The Gestaltists hadn't realized this: in the event, their mistaken ideas about the brain led people to downgrade their psychological work.)

In sum: "information theory is desirable as allowing future contact with physiology but never assuming physiological detail" (p. 306). Or as Jerry (Jerrold) Fodor (1968) later put it, psychology is an autonomous ("special") science, not reducible to neurophysiology even though mental processes are implemented in the brain (see Chapter 16.iii.b).

d. Information and computation

During his lifetime, Broadbent contrasted his information-theoretic approach with three alternative views: behaviourist, humanist, and computational psychology. He had only limited respect for the first, and none at all for the second. However, he was sympathetic to the third. In his eyes, it wasn't wrong-headed so much as premature.

In the late 1950s, he was defending himself against attack by the behaviourists, as we've seen. Twenty years later, he would defend himself against a very different enemy: humanism.

Humanism, in this sense, was the neo-Kantian view that psychology is—or should be—an interpretative discipline, forever trapped within the hermeneutic circle (Chapters 2.vi and 16.vi–viii). Moreover, since interpretation focuses on single texts, not on regularities, the subject matter of psychology was seen by the neo-Kantians as a patchwork of individual phenomena, or "stories". Comparisons could doubtless be made, much as *Hamlet* can be compared with *The Tempest*. But the 'Newtonian' assumption that psychology should look for general principles of explanation was roundly rejected.

This view hadn't imbued the Anglo-American Zeitgeist of the 1950s. In the late 1960s, however, it became fashionable, forming part of the general reaction against modernism (Roszak 1969; Toulmin 1999).

Many disciplines were affected. Terry Winograd, an AI researcher whose work had interested many hard-headed psychologists, was just one of those who turned away from empiricism (see 11.ii.g). Social psychology, in certain quarters, was renamed ethnomethodology—and concerned itself with highly detailed, non-generalizable, accounts of everyday behaviour (Garfinkel 1967). Anthropology took an overwhelmingly "narrative" turn, and literary/historical studies were in turmoil too (8.ii.a–c). And the turmoil often involved bad-tempered invective from both sides of the fence.

Within professional psychology, this counter-cultural movement spawned many—often vitriolic—attacks on orthodox scientific approaches. Liam Hudson, for instance, attacked the so-called "cult of the fact" (1972), and described psychologists as "prime sufferers of the infatuation with scientism" (1975: 17). "Evidence", he said, should give way to "interpretation", and we should accept "the daring assumption that psychologists should study the people around them".

Hudson was by no means the only one to rebel against the experimental orthodoxy. Even Boston University's Sigmund Koch, for ten years "a dauntless and virile rat-runner", joined in on the enemy's side (S. Koch 1961; 1974: 3). The "enemy" he was referring to was "humanistic" psychology in general, including those new forms of social psychology which saw it as descriptive/historical rather than scientific (e.g. Gergen 1973). The *Journal of Humanistic Psychology* was founded in the late 1960s to counter this approach (and behaviourism and Freudian psychoanalysis too), and many papers and fast-selling books of the 1970s opposed it.

Psychologists whose views gained some notoriety—and influence—in this period included John Shotter (1970), Amedeo Giorgi (1970), Robert Joynson (1970, 1974), Rom Harré and Paul Secord (1972), Kenneth Gergen (1973), and Alan Gauld (Gauld and Shotter 1977). And one psychologist who was hugely famous already, namely Jerome Bruner, suffered a humiliating ordeal at the hands of his Oxford colleagues when—at the height of this controversy—he gave a public lecture criticizing their more orthodox scientific approach (see Section ii.c, below).

If the notoriety has now faded, the conviction usually hasn't. For recent examples, see Harré's unorthodox account of what cognitive science should be like (1994, 2002), Shotter's Derridean views on social psychology (Parker and Shotter 1990), and Bruner's characterization of psychology as "narrative" (2002). An exceptionally clear statement of the core claim is Giorgi's unapologetic insistence that

> a complete break from the natural science conception of psychology would be profitable at this time, and only after psychology as a human science has had significant development, should the dialogue with psychology as a natural science be pursued. (Giorgi 2000)

Today, the humanists and "scientific" psychologists quietly follow their own paths, rarely asking how they might fruitfully meet (Giorgi 2005). But in the 1970s humanist psychology was perceived as an aspect of the counter-culture, and fervently favoured by many people for political as well as purely theoretical reasons (see Chapter 1.iii.c). As such, it represented a real threat to the more scientific forms of psychology.

Whereas the American Anthropological Association (as we'll see in Chapter 8.ii.b) was happy to embrace the counter-culture, the British Psychological Society (BPS) wasn't. Broadbent himself was an influential BPS figure, of course, but there were many other experimental psychologists with scant sympathy for the new movement. The BPS became so concerned by this division into warring camps that it convened a special conference in 1979 to discuss it (A. J. Chapman and Jones 1980).

Broadbent was unmoved. He refused to take refuge in the easy excuse that social psychology, from which most of these attacks had come, is humanist whereas cognitive psychology is scientific. ('Humanist' is a useful shorthand here: many in the opposing camp, namely those with postmodernist leanings, were almost as critical of classical humanism as of science.) In a book-length "defence of empirical psychology" written soon after these attacks had surfaced, he declared: "We are, in fact, studying one subject, not many" (1973: 141). (Whether that's really so is still controversial: see Chapters 14.x–xi and 16.)

Citing the raging language wars of post-Chomskyan linguistics (9.ix.a), Broadbent derided any approach to psychology "through the armchair, by the exercise of fallible human reason, intuition and imagination" (1970*c*: 96). Empirical, not interpretative, methods were required:

> [In] dealing with human beings, a proper sensitivity to other men demands that we should take an interest in what they actually do rather than what we think they do. The empirical method is a way of reconciling differences. If one rejects it, the only way of dealing with a disagreement is by emotional polemic . . . If we refuse to use experiment and observation on other human beings, we start to regard them as wicked or foolish. I think this is a serious danger, and I have no doubt whatever that the methods of empirical psychology are socially more hygienic, or to use the older and more robust phrase, morally better. (Broadbent 1970*c*: 95–6)

This wasn't mere rhetoric. In a book already in press, Broadbent (1971) had shown why bad decisions often result from stress, in the form of noise, time-pressures, and/or over-abundant input information (Broadbent 1971). To regard them as bad (mistaken) decisions is justifiable, but to damn them as "foolish" is not. Even 'pilot error' is tendentious, since it implies that full responsibility for the pilot's mistake rests with the pilot, not with (for instance) the designer of the dashboard in the cockpit.

At the same BPS conference, Broadbent expressed his sympathy for a third way of doing psychology: computer modelling. He was closer to this approach than to humanistic psychology or behaviourism, but remained sceptical nonetheless:

> The great merit of models which can be implemented on a computer . . . is that they avoid many . . . ambiguities. I would firmly believe that in the long run any adequate account of human beings will have to be capable of computer implementation. The problem at the moment is rather that such models are not adequate empirically; none of them behaves quite like a person behaves, and this tends to be obscured when one discusses this without looking at concrete data. (1980: 114)

> The models advanced in artificial intelligence . . . are not satisfactory in detail; but they have made the important contribution of showing that in principle a [formal–computational] system could show the interesting, creative, and purposive features of human behaviour without resorting to a little man at the centre of the model. (1980: 126)

In other words, the Church–Turing thesis (Chapter 4.i.c) was being tacitly granted, since "in principle" an AI system could model even creativity and purpose. The problem, rather, lay in the practicalities. Computational psychology *understood as computer modelling* was just too hard. The informational approach was—as a matter of fact, not principle—more effective.

Shortly before his death, Broadbent organized a series of eight public lectures on 'The Simulation of Human Intelligence'. His own lecture declared that AI models had much to offer to psychology (Broadbent 1993). But he still wasn't a computer modeller, and nor did he treat programs as a rich source of theoretical concepts. What 'flowed' between his diagram boxes was information, not control.

The switch from information to control would be made by Miller, soon after Broadbent's seminal book appeared (see Section iv.c). This switch was not a schism.

It was seen by many, including Miller himself, as an *improvement* rather than an *alternative*.

Walter Reitman, for example, who wrote one of the more interesting early GO-FAI simulations (7.i.b), refused to distinguish between 'information-processing' and 'computer-modelling' theories and concepts:

> The term [information processing] is not meant to be synonymous with "data processing" or "computer". It is a label for a general approach to the study of psychological activity . . . [So] information-processing theories examine the representations and processes involved in cognitive activity. They emphasize the functional properties of thought and the things it achieves. (Reitman 1965: 1)

The information-processing approach, he said, encourages us—and enables us—to focus on the details of cognitive structures and processes, and on the higher-order strategies involved in thinking. Computer models are especially helpful, precisely *because* a programmed computer can do only what we've ordered it to do (1965: 15). But the basic theoretical advance is to theorize in terms of information-processing (including computational) concepts.

Despite such commonalities, the *methodology* of the informational and computational psychologists differed. And the theoretical emphasis was different too, on data and control respectively. These were two intellectual communities, not one.

Nevertheless, the sympathies and assumptions were very close. Broadbent's interest in computational psychology was always evident, in his private conversation as in many of his writings. Some of his disciples were less broad-minded, more concerned to protect their patch against rivals. In retrospect, however, it's clear that the two streams were part of a common intellectual enterprise, springing from cybernetics (broadly defined)—and with behaviourism and, of course, hermeneutic "humanism" as the common enemies.

e. Chomsky comes on the scene

Chomsky's first—and still his most well-established—theoretical contributions concerned different types of *computation* (see Chapter 9.vi.a). In 1956 he published a classification of computer languages that's still central to computer science today; and 1958 saw a paper on finite state languages co-authored by him and Miller. Indeed, it was largely because of Chomsky that Miller replaced 'information' by 'computation' as the core concept of psychology.

Very few of Miller's psychological colleagues were enthused by Chomsky's 1956 paper. Their attention was captured, rather, by his little book *Syntactic Structures*. When this hit the scene in 1957, it wasn't only linguists who were interested. Psychologists of language were interested too—though *not*, yet, psychologists in general. Chomsky had deliberately 'censored' his views on innate knowledge of language: to mention that would have been far too provocative (9.vi.e). Nor had he mentioned behaviourism *as such*, although he had rejected statistical/Markovian theories of language. Most psychologists would be introduced to Chomsky, whether to agree or disagree, by his assault on Burrhus Skinner two years later (9.vii).

But if the fourth tenet of behaviourism remained unchallenged, the first, second, and sixth did not (see 5.i.a). In rejecting them, Chomsky's work made psycholinguistics possible. Or rather, it made *what we now think of as* psycholinguistics possible. Quite a few people besides Skinner were already asking questions about the psychology of language (aka verbal behaviour). But they were doing so in the context of Bloomfieldian theory (9.v.b).

For instance, James Jenkins (who, unlike Skinner, was happy to posit intervening variables) had been seeking experimental evidence for structuralist linguistics. He first learnt of Chomsky's work from Miller, when they were both visiting the Stanford Center in 1958–9. Six years later, again at Stanford, he attended a sentence-by-sentence tutorial on *Syntactic Structures* given by Fodor and Sol Saporta, and co-published with them as a result (Fodor *et al*. 1967).

It wasn't easy for him to make the switch. In 1964 (a full seven years after *Syntactic Structures)* Jenkins recommended Chomsky to his verbal-behaviour colleagues, telling them: "You know, we've got to work on generative behavior . . . rule-governed behavior" (Jenkins 1986: 247). And their response?—"Everybody in the meeting jumped on me."

Besides being "jumped on" by his behaviourist colleagues, he was soon dismayed by the master himself. For Chomsky, in his second book, moved the goalposts:

> Then Chomsky pulled the rug out from under us . . . [We] were very busy trying to provide the apparatus for a theory of linguistics that at that moment was being discredited. It's a very disappointing position to be in . . . [By] the time we could supply the right kind of theory, the nature of what language was believed to be had changed. The whole theory was no longer appropriate. Very grim, very grim. (Jenkins 1986: 243)

What had excited Jenkins (and other refugees from behaviourism), and what persisted even after the "grim" rug-pulling, boiled down to three things. These were (1) Chomsky's view that language—or anyway, syntax—could be formally described as a generative system; (2) his claim that sentences must be represented on more than one level; and (3) his talk of grammatical transformations.

"Generative", "representation", and "transformation" can all be understood mathematically and/or psychologically. Chomsky had intended them to denote timeless relations between abstractly defined linguistic structures (see 9.vi.a–d). But psychologists naturally interpreted them as hypotheses about actual mental processes. Indeed, Miller's cognitive science 'manifesto' in 1960 encouraged them to do just that (Section iv.c, below).

An explosion of experimental work ensued, investigating the psychological reality of Chomsky's ideas. For example, researchers asked whether sentences are remembered more easily if fewer transformations are involved. They tried to find out whether listeners assign syntactic structure to a sentence word by word, or only after the whole sentence has been heard. And they asked whether people can understand sentences with many levels of grammatical nesting, or recursion.

Very early in the game, Miller—with Chomsky himself—formulated the derivational theory of complexity (G. A. Miller and Chomsky 1963). This proposed that people actually perform (unconscious) transformations when they produce, understand, or remember sentences. For a while, the experimental evidence seemed to support them. But trouble lay ahead. It soon became clear that derivational complexity *didn't* necessarily predict difficulty in understanding or remembering sentences (Fodor and

Garrett 1966). By the early 1970s this theory had bitten the dust, its obituary written by Chomsky's ardent champion (Fodor *et al*. 1974).

Chomsky had changed his mind about transformations by then (9.viii.b). So Miller reacted in much the same way as Jenkins. Remembering the early days, he wryly recalls: "[We] were working before Chomsky's 1965 book; so, as we were trying to test the psychological reality of syntax, the syntactic theory we were testing was moving out from under us." As a result, he said, "About '65–'66 I had given up on syntax" (G. A. Miller 1986: 209, 216). (However, he hadn't given up on language: see 7.ii.a and d.)

What matters, here, isn't whether Chomsky's œuvre was consistent, or whether any version was correct (considered either as linguistics or as psychology). These questions will be discussed in Chapter 9. The important point is that his early approach, part-mediated by Miller, encouraged psychologists to think about language in computational terms.

Even though most of them didn't turn to computer modelling, they began to think of language as a precisely describable generative system. Chomsky didn't try to measure information, nor to locate it on a flow chart, nor to incorporate it in a program. But he asked how it was *structured*, how it could be *generated* and *transformed*, and what (very abstractly defined) types of *computational* system would be able to represent this. That's why he was an important voice in the rise of computational psychology.

6.ii. The New Look

The New Look in haute couture was a heady liberation. The austere fashions of wartime gave way in 1947 to the glorious full, long, skirts of Christian Dior's A-line. And, with fabric rationing ended, the new dresses soon migrated joyfully from catwalk to high street.

Much the same was true of the 'New Look' in perception, launched around the same time. The major couturier was Bruner (1915–). The British stylist Richard Gregory (1923–) was important too, doing even more than Bruner to make *mind-as-machine* fashionable on the high street. But his New Look research was begun after Bruner's, and was narrower in scope, as we'll see. It was largely thanks to Bruner's imaginative cutting and stitching that American psychologists by 1960 were free at last to study full-skirted perception (instead of the narrower 'sensory discrimination'), to recognize joy (or anyway, values), and even to champion models—not on the catwalk, but in the mind/brain.

New Lookers reacted against the 'passive reception' view of perception that had been inherited from classical empiricism (Chapter 2.x.a). They saw the human mind as "proactive" rather than "reactive"—a position stressed also, in relation to personality theory, by Bruner's colleague Gordon Allport (1897–1967): see 5.ii.a and G. W. Allport (1960). Accordingly, they focused on the selective and constructive aspects of perception. They were interested less in *how much* input information can pass into/through the mind than in *how it's selected and represented* by the person (*sic*) concerned. They saw perception (and concept learning) as being guided by conscious and unconscious *expectancies*, *hypotheses*, or *models*, and *tested* by more or less reliable *cues*.

In short, everyday cognition was thought of as an informal version of scientific inference or reasoning, though with social/personal factors playing a larger role than they do in science. This 'reasoning' can trigger predispositions to act in specific ways, so our perceptual expectancies, said Bruner (1961), are similar to what the ethologists had called IRMs (5.ii.c). But they're mostly learnt, not innate. And, as we'll see, they depend on the person's values as well as their beliefs.

These psychologists didn't do computer modelling, but their theoretical approach became increasingly computational over the years. This reflected the influence not only of Miller but also of Broadbent, who (with Gregory) was one of Bruner's closest friends during his visit to England in 1955–6. In its fully developed form, the New Look wasn't a mere forerunner of computational psychology but an early version of it.

Moreover, it attracted the attention of the very early GOFAI researchers. Marvin Minsky's seminal 'Steps Toward Artificial Intelligence', for instance, mentioned Bruner's work as highly relevant to the programme of AI (Minsky 1961*b*: 450; cf. 10.i.g). That's partly why a publication by Bruner was one of the six events which defined the *annus mirabilis* of cognitive science—namely, 1956 (see Section iv.a, below).

a. Coins and cards

The first liberating New Look challenge to the austerities of behaviourism was the decision to study perception properly so-called (i.e. not just sensory discrimination), or what the world actually *looks* like. The learning theorists at the time, and the sensory psychophysicists too, regarded this question as "mentalistic, phenomenological, essentially European" (Bruner 1983: 67). A main achievement of the New Look was to make perception respectable (in America) again.

The second liberating challenge arose as what people *wanted*—and what they wanted to avoid—came to the fore. This included not only what they wanted (what they were trying to achieve) at a particular time, but also what they were generally disposed to want (or to fear), given their individual personality-structure:

> [Given] the interdependence of all aspects of personality . . . [within] the dynamical system that constitutes the person, [the problem is] to understand how the process of perception is affected by other concurrent mental functions and how these functions, in their turn, are affected by the operation of perceptual processes. (Bruner and Goodman 1947: 33)

Sigmund Freud (1925), in his essay on the "mystic writing pad", had described perception as selective, balanced between motivated hallucination and realistic ("ego-driven") response. But he'd cited anecdotes rather than experiments.

Immediately after the war, Bruner—with his "inseparable" friend Leo Postman (Bruner 1983: 75) and his undergraduate student Cecile Goodman—did a number of experiments exploring this general theme (e.g. Postman and Bruner 1946; Bruner and Postman 1947*a*,*b*; Bruner and Goodman 1947). Even they, however, had been impelled to do so by an anecdote:

> J.B.: [One needs] crazy ideas that connect things up. I think it was in Faulkner's *Intruder in the dust*, or one of the Faulkner novels, where the kid named Benji or something reaches his hand in his pocket and finds a half-dollar. And he says that it felt as though it could fill his whole palm.

B.S.: Is that where the idea for your little experiment came from?

J.B.: That's where the idea of the experiment came from . . . And I even remember that I happened to be having dinner that night with [my close friend the Gide scholar Albert Gérard]. Albert said, "Oh. That's fantastic! That's a wonderful passage." And he starts giving me other examples where people have come to him with the same sort of thing. Something looking brighter or bigger because it had gotten important. So I thought, "Marvelous experiment."

(Shore 2004: 15)

They found that perception can indeed be influenced by emotion and motivation. It wasn't only basic drives such as hunger which could do this (R. Levine *et al.* 1942), but culturally acquired needs too. In fact, one effect of the New Look was to bring social and cognitive psychologists closer together. This happened largely because Miller and Bruner were close personal friends. Despite being located in different Harvard departments, due to an administrative split in 1946 (Bruner 1983: 63), they were highly cooperative colleagues (see Section iv.d).

It seemed that the greater the social value of an object, and/or the greater the individual need for it, the more susceptible it was to perceptual biases from 'dynamic' factors. For example, Bruner and Goodman reported that children's perception of such an apparently objective matter as the size of a coin was affected both by their knowledge of the coin's face value and by their need/desire for money. The size of high-value coins was overestimated, and the poorer children overestimated more than the richer ones did. (The effect didn't work for half-dollars: the experimenters suggested that the 10-year-olds saw a half-dollar as less "real" than the more familiar quarter.) Bruner and Goodman posited a range of unconscious perceptual "hypotheses" to explain the bias, or selectivity, they'd observed.

These results were an eye-opener because previous experiments on the effect of social influence had dealt only with highly ambiguous stimuli, where objective information wasn't available. These concerned the 'autokinetic' effect, in which a spot of light in a darkened room is mistakenly seen as *moving*.

Muzafer Sherif (1935: 17–22) had shown that the extent to which the spot apparently moves can be altered by the statements of other people in the room. These "other people" were in fact the experimenter's stooges, instructed to describe the spot to the victim in a particular way. But the victim's perception might be explained as a case of rationality: if everyone else says they see the spot as moving *thus-and-so*, it's only reasonable to think/believe that it's moving in that way. And if perception involves judgement, as René Descartes had argued centuries before (Chapter 2.iii.d), then it's understandable that the subject should *perceive* it in that way too. This is a far cry from perceiving some 'objective' feature in a subjectively biased manner.

The coins paper, Bruner recalled later, "turned out to be the catalyst, the cloud seeder. It produced the New Look, and before that was done, it rained about a thousand articles and books" (1983: 69). An APA citation study done three years after the coins publication showed Bruner and Postman as second only to Freud in the number of bibliographical references made to their work (Bruner 1980: 107).

For instance, Solomon Asch (1956) caused a sensation by claiming that people's perception of relative line lengths (an objective matter, unlike the illusory movement of the autokinetic effect) could be affected by social pressures. He'd found—on debriefing—that although some subjects had merely conformed verbally (i.e. they'd said the same thing about the line as the other subjects were saying), others insisted that they *really had seen* the shorter line as longer. However, this was tricky. Sceptics objected that Asch's key subjects might be lying during the debriefing, not wanting to admit that they'd said something they didn't believe merely because everyone else was saying it. (Follow-up studies showed that people were less likely to conform if the judgement involved politics—presumably, because disagreement is common there.) Various criticisms were made of the methodology of the original coin studies too. In short, enthusiasm and critique both flourished.

Soon after the coin experiments, Bruner (1951) discussed a range of contemporary research indicating that different personality types show systematic biases in perception. For example, some people are biased towards tolerance or intolerance of ambiguity, others towards optimism or pessimism. (For the distinction between optimism as perceptual/affective bias and optimism as reasoned belief, see Boden 1966.)

That research included post-war studies of "the authoritarian personality", undertaken in an effort to explain the popular acceptance of fascism in Nazi Germany (Frenkel-Brunswik 1948, 1949; Adorno *et al.* 1950). Both Bruner and these researchers (whose methodology, for the record, was gravely flawed: Christie and Jahoda 1954; McKinney 1973) saw Freudian repression as a determinant of perception, as well as belief.

This conviction underlay Bruner's claim that "intrapunitive" individuals have a disposition (or "set") to evaluate ambiguous information as confirming their own guilt:

> The more marked the degree of intrapunitiveness, the less the appropriate information necessary to confirm self-guilt. As the [often unconscious] hypothesis attains greater and greater strength, intrapunitiveness attains neurotic proportions, which is to say that self-guilt hypotheses are confirmed by information judged by society to be grossly inappropriate or ambiguous. (Bruner 1951: 104)

Experimental studies of such matters, however, had been very thin on the ground, "in spite of the fact that Freud early referred to one aspect of the ego as 'perceptual consciousness' and despite the title of [Anna Freud's] first chapter ('The Ego as the Seat of Observation')" (Bruner 1951: 108). Bruner decided to provide some.

As an ex-student of McDougall (5.ii.a), his interests in personality theory and social psychology—and in their close relations with cognition—were only to be expected (cf. Bruner *et al.* 1956: 16; Bruner 1983: 59). Indeed, before the 1946/1947 papers (co-authored with Postman and Goodman) cited above, he'd already published some twenty articles on social and personal psychology. Several of these had had a 'cognitive' air, for they showed that people's preconceived political opinions acted as (often unconscious) "filters" of incoming news-items and propaganda (e.g. Bruner 1944).

By the late 1940s Bruner was already referring (as in the quotation, above) to perceptual selectivity as the result of "hypotheses", and to motivation as "strengthening hypotheses". In his defence, he quoted Bartlett's opening remark in a lecture he'd given in 1951:

> Whenever anybody interprets evidence from any source, and his interpretation contains *characteristics that cannot be referred wholly to direct sensory observation or perception*, this person thinks. The bother is that nobody has ever been able to find any case of the human use of evidence

which does not include characters that run beyond what is directly observed by the senses. So, according to this, people think *whenever they do anything at all with evidence*. (Bartlett, quoted in Bruner 1957*a*: 219; italics added)

To posit internal hypotheses was highly non-Newtonian, of course. But to some extent Bruner was protected from his fellow professionals' scorn by the fact that "hypothesis" was a term derived from the language of science. (He now sees it as equivalent to "intentionality", a philosophers' term that was "unheard of" in those behaviourist-dominated days—interviewed in Shore 2004: 180.)

Where Bartlett had spoken of schemas (Chapter 5.ii.b), Bruner spoke of hypotheses. These could be made stronger or weaker (i.e. more or less salient, or "accessible") by motivational factors. But they arose, directly or indirectly, from past experience: the more probable a cue category was found to be, the more likely that it would be applied. (By the early 1950s, he was expressing this in terms of the recognition of informational redundancy: G. A. Miller *et al*. 1954.)

Bruner was especially interested in Craik's question of how people use concepts to predict and anticipate the future (Bruner *et al*. 1956: 14; Bruner 1957*a*). In addition, he wanted to know how they test, or validate, their perceptual/conceptual hunches.

This assimilation of perception to scientific reasoning, in the active construction of percepts, became the third criterion of the New Look. As Bruner put it, "A theory of perception . . . needs a mechanism capable of inference and categorizing as much as one is needed in a theory of cognition" (1957*b*: 123). By "mechanism", here, he meant a system defined in psychological terms. But he didn't forget that there must be some (broadly Hebbian) neurophysiological mechanism too: see, for instance, the section on 'Mechanisms Mediating Perceptual Readiness' in Bruner (1957*b*).

Bruner used anomalous or blurred stimuli to show how stubbornly we protect our perceptual hypotheses despite conflicting evidence (Bruner and Postman 1949*a*; Bruner *et al*. 1951; Bruner and Potter 1964). For instance, he reported that people often see (briefly presented) anomalous playing-cards, bearing black hearts or red spades, as normal ones.

The delay in recognizing the colour red could be a whole order of magnitude greater than when the redness was expected, on the basis of the heart/diamond shape. (So much for the associationists' passively received red sense datum.) Sometimes, there was a perceptual 'compromise'—as when red spades are seen as "brownish" or "purple". Only very rarely was the strange playing-card immediately seen as it really is.

This experiment had required a certain amount of do-it-yourself ingenuity on Bruner's part. And the reason why that was so was a further confirmation of the psychological truth the experiments were taken to show:

J.B.: I tried to get an American playing card company to manufacture special cards which altered the colors for the suits. I had written on Harvard stationery so they wouldn't tell me "I'm going to get the cops on you. What kind of scam are you trying to pull off?" [But they wouldn't make them for me, either.]

B.S.: This resistance that they had to doing that was just as strong an evidence of the importance of category classification as the experiment was.

J.B.: Exactly. I only realized that years later. I mostly thought they were being a pain in the ass. And they were. So I went into an art shop on Beacon street with Mrs. Ware Eliot, T. S. Eliot's

> sister-in-law, with whom I had taken some drawing lessons. We brought in some playing cards and tried out different things until we found the right thing to paint them with.
>
> (Shore 2004: 92–3)

In the case of playing cards, we're confident that we know the card "as it really is". Even if it's 'really' a black card, painted red with Mrs Eliot's help, we're confident that—now—it's *really* red. This is largely because of intersubjectivity: in normal conditions, we reach agreement with ease. (That's why Bruner and Postman had to use a tachistoscope, to show the cards only very briefly.) In the case of describing personalities, we aren't so sure.

People are always complex, and they sometimes dissemble. Any action, considered on its own, can be given many different interpretations (the hermeneutic circle, again: Chapter 5.iii.b). It's hardly surprising, then, that 'first impressions' are so important. The New Look approach implied that they should be both influential (in biasing later action interpretations) and difficult to change. For the more disputable each individual cue, the less chance of the original hypothesis being abandoned—and personality categories, unlike red/black, involve highly disputable cues.

This made sense of some intriguing mid-century experiments, done by Hull's student Carl Hovland (1912–61) and others. These showed that once a person had been categorized as (for instance) honest/dishonest, their later behaviour would be seen as reticence/evasiveness, and integrity/trickiness, accordingly (Haire and Grunes 1950; Hovland 1957).

The playing-card experiments (Bruner and Postman 1949*a*) interested not only psychologists but philosophers too. Thomas Kuhn, for instance, cited them in his account of "normal" science, much as he drew on Gestalt psychology in describing "revolutionary" science (Kuhn 1962: 63–4, 110–14). Nearly twenty years later, Paul Churchland (1979: 1–45) would draw heavily on the by-then-familiar fact of concept-based perceptual plasticity in his first statement of eliminative materialism (Chapter 16.iv.e).

In short, the New Look (and subsequent work) had convinced psychologists that our perception is deeply imbued with concepts (Bruner 1957*b*). Many philosophers, from Descartes to Karl Popper (1935), had already said this, of course. Now, there was added empirical support.

One shouldn't assume, however, that *all* philosophers were favourably impressed by the New Look. Wittgensteinians later objected strongly to its talk of non-conscious hypotheses and inferences. They didn't contest the experimental data. But they argued that, when seeking to explain perception, one shouldn't adopt—nor even adapt—language normally used to describe deliberate conscious thought. Cognitive scientists in general, and Gregory in particular, were rapped over the knuckles by two of the master's closest disciples: Norman Malcolm (1971) and Elizabeth Anscombe (1974). (Gregory defended himself with some spirit. In my view, he won that battle hands down: see Chapter 16.v.f.)

Nor should one assume that all *psychologists* welcomed the New Look. The sensory psychophysiologists caricatured the New Looker as someone "who tends to use a stimulus input of a relatively complex kind, without being able to specify very rigorously its nature, and then proceeds to vary a great many other conditions that have little directly to do with conventional descriptions of perception" (Bruner and Klein

1960: 121). And the New Look in general was felt by some critics to be unpersuasive, because of its reliance on the suspect concepts of *unconscious bias* and neo-Freudian *defence*.

Bruner and Postman had posited "perceptual defense" in 'normal' subjects as well as neurotics, to explain why people often fail to perceive emotionally threatening stimuli (Bruner and Postman 1949*b*; Bruner 1957*b*). Their subjects might *register* it at some unconscious level (measured by galvanic skin response, for example), but conscious *perception* was delayed or even prevented. Such data were new, and undeniably interesting (and prompted an explosion of work on 'subliminal' perception). But Floyd Allport (Gordon's brother), for one, was unconvinced by explanations in terms of defence or unconscious perceptual expectancies (1955, chs. 13–15). Quite apart from their air of paradox, he said, these didn't allow precise predictions. Bruner had an answer:

> To ask [the New Looker] to rule [defensive processes and other personality variables] out of his research in the interest of cleaning up his experiments is as silly as asking the psychophysicist at every turn to replicate his experiments under conditions of inattention or imperious need states, given that his interest is in sensory receptivity under optimal conditions.
>
> In the end, what we are saying is that full explanation of any phenomenon—be it perception or anything else—requires both a close study of the context in which the phenomenon occurs, and also of the intrinsic nature of the phenomenon itself under idealized conditions. The New Lookers have tended to do the former, the researcher raised in psychophysics and sensory physiology the latter. (Bruner and Klein 1960: 122)

He admitted, in retrospect, that the New Look in its early days had been "naive and inept and confusing", and like a "noisy and brawling adolescent . . . [had] often proceeded without enough attention to the lessons learned by its [elders]" (Bruner and Klein 1960: 119). However, the critics wouldn't have bothered if the ideas hadn't been so provocative. Bruner again:

> [The] New look has had an activating effect, a disturbing effect; it has created some useful models; it has got part way through some research that shows signs of being better done; and it has been bold enough to look at problems . . . [It] has at least had the virtue of not taking much for granted . . . (Bruner and Klein 1960: 119)

b. A study of thinking

The New Look culminated in *A Study of Thinking* (1956), by Bruner, Jacqueline Goodnow, and George Austin (BGA). This was both the last stage of the New Look and a first stage of computational psychology.

Bearing in mind the many criticisms of the "noisy and brawling adolescent", this mature version of the New Look involved painstaking and systematic experiments—and said almost nothing about issues of emotion and personality (but see pp. 79, 228). Less 'sexy' than the need-biased coins and the dodgy playing-cards, it wouldn't reach the newspapers. In theoretical psychology, however, it made even more waves than they had done.

The title itself was a challenge. No one 'respectable' (in S–R terms) was studying *thinking*—or if they were, they called it 'learning'. Only ten years earlier, Wolfgang

Kohler had lamented: "It is to be regretted that few psychologists take an active part in the investigation of thinking" (1945, p. iii). Even Hebb, with his unorthodox talk of "concepts", had merely outlined a theoretical approach to thinking (Chapter 5.iv.c).

BGA not only tackled this unfashionable topic, but disarmingly declared: "To the reader conversant with contemporary American psychology, [our] book will appear *singularly lacking in the more familiar forms of theoretical discourse*" (p. 23; italics added). That was true—and the book caused a revolution. (One textbook later described it as "the" classic work on adult intellectual behaviour—P. H. Lindsay and Norman 1972: 498.) It led to a lasting revival of interest in *cognitive* psychology, and helped lay the foundations for *computational* psychology—and for cognitive science as such.

The central topic was concept learning—or, as Pitts and McCulloch (1947) had put it, 'How We Know Universals' (see Chapter 12.i.c). But where Pitts and McCulloch had concentrated on the sensory aspects, BGA—in typical New Look style—assimilated perception to scientific reasoning. Moreover, they pointed out at the start (p. 10) that concept learning has a very wide scope. Besides being important in its own right, it *includes* crucial aspects of perception and *is included in* reasoning, memory, and creativity. In effect, then, it's the pathway that leads into cognitive psychology in general: research on what used to be called the "higher mental processes".

Some of the behaviourists had studied concept learning: Hull (1920), for example. But their subjects had been asked to learn the names of certain patterns, not to discover their defining features. A New Look psychologist had recently discovered that if specifically bidden to do the latter, people's success in identifying those features rose hugely (H. B. Reed 1946). Moreover, the informational aspects of the stimuli hadn't been considered or controlled. If a stimulus part was there, it was there: *how many alternatives* were also present wasn't considered relevant. And results that depended on the order of presentation weren't interpreted by Hull in terms of *how the subject had decided to tackle the task*.

BGA paid homage to Tolman, rather than Hull (5.iii.b–c). They described their work as a study of Tolman's intervening variables, or cognitive maps (p. vii). For they reported systematic experiments on the (largely unconscious) "strategies" that people use in learning a new concept. And this term, they said, wasn't meant metaphorically:

> A strategy refers to a pattern of decisions in *the acquisition*, *retention*, *and utilization of information* that serves to meet certain objectives . . . [including, though not limited to,] the following:
>
> a. To insure that the concept will be attained after the minimum number of encounters with relevant instances.
> b. To insure that a concept will be attained with certainty, regardless of the number of instances one must test *en route* to attainment.
> c. To minimize the amount of strain on inference and memory capacity while at the same time insuring that a concept will be attained.
> d. To minimize the number of wrong categorizations prior to attaining a concept. (Bruner *et al.* 1956: 54; first italics added)

Instead of everyday stimuli such as coins, BGA used artificial ones. This was partly to minimize social/personal factors, but mostly to provide neatly defined concepts (more tractable than real concepts are: see 5.iv.c, 8.i.b, 9.x.a, and 12.x.b).

So they showed their subjects cards bearing crosses, squares, and circles in various numbers and colours, and with or without borders. In terms of these dimensions, they defined concepts of several kinds:

* conjunctive (A + B + . . .),
* disjunctive (A or B or . . .),
* relational (e.g. having more As than Bs), and
* probabilistic (if such-and-such conditions are satisfied, then it's probably an X).

And instead of focusing on isolated responses, like someone's estimation of the size of a coin, they looked at the behavioural patterns within *sequences* of responses.

Their experimental rationale was explained in terms of examples of (neuro)scientific reasoning (pp. 81, 246). So, they said, much as a neuroscientist investigating the role of six brain regions can choose the order, and combinations, in which to ablate them, so BGA's subjects could normally choose which card to ask about at any given time. (Occasionally, the cards were presented in an order chosen by BGA.) In each case, they'd be told whether it was or wasn't an example of the concept being learnt. In the vocabulary of Chapter 12.ii.b, this was supervised learning; in BGA's vocabulary, it was concept attainment, as opposed to concept formation (pp. 232–3).

The information (logical implications) available from *this* card, in *this* position in the sequence, for *this* type of concept, was known to the experimenters. (Such matters had recently been analysed by Yale's Hovland: 1952.) They defined four ideal strategies—"ideal" meaning logically pure, not best: simultaneous scanning, successive scanning, conservative focusing, and focus gambling. These had complementary advantages and disadvantages in terms of ease of remembering, informational content, and cognitive risk.

The overall question was whether people would follow identifiable search plans—and if so, whether these would match/approximate the ideal ones. (Accordingly, BGA saw their work as description, not explanation; in particular, they were agnostic on how strategies come about: pp. 241–2.)

BGA's subjects could distinguish exemplars from non-exemplars before being able to define the features on which their judgements were actually based. That wasn't new: Hull's subjects, nearly forty years before, had done so too. But BGA (unlike Hull) asked how people actively go about learning new categories.

They observed several different strategies, which varied according to the informational demands of the task (including the time allowed). For instance, some subjects started by using random sampling—an inefficient plan normally soon abandoned for more structured sequences of choice. Others waited till they found a positive instance of the concept, and then searched for a stimulus like it in all ways but one. If this was *not* an example, the 'missing' feature must be a relevant one. The procedure would then be repeated for each of the other features. As BGA pointed out, this strategy puts little strain on the memory but is time-consuming. When subjects were hurried, they abandoned it for more chancy strategies. These often failed, as people lost track of the implications of what they'd learnt. In that case (sometimes, even from the start), they'd use a more risky method, gambling that *several* dimensions were relevant and trying to test them all at once. With good luck, this could yield success quickly. With bad luck, the person was left floundering.

By and large, and especially in relaxed conditions (with no time pressure), people chose a sensible strategy—if not always the best. They tended to follow each strategy faithfully, though they sometimes switched from one to another. The experimenters concluded that they were constructing internal hypotheses, or representations, which guided their choices and were continually modified by the results.

Moreover, it appeared that certain sorts of hypothesis were difficult to represent and/or to modify. Conjunctive concepts were learnt more easily than disjunctive or relational ones. Probabilistic concepts caused difficulty. And the information available from positive instances (*this is an X*) was used more efficiently than that from negatives (*this is not an X*).

In addition, there was an important distinction between "defining" and "criterial" attributes. The former is a logically necessary condition, whereas the latter is a pragmatically useful feature (pp. 32–41). BGA gave the example of psychiatrists screening conscripts during the war: "Did you wet your bed as a child?" was quickly asked and answered, and was assumed to be a useful—but not a defining—indicator of maladjustment. Bruner's team drew here on the work of Tolman and Brunswik (1935), whose concept of "the causal structure of the environment" they interpreted in terms of informational redundancy and contingent probability.

This reference to real-world probabilities shouldn't be taken too literally. BGA themselves pointed out (p. 204) that easily available cues may be relied on *even when they're not realistic*. For instance, Bruner's Harvard colleague Gordon Allport had done important work on prejudice, showing that racial (and other) stereotyping depends on quick judgements based on easily available cues (G. W. Allport 1954). The general conservatism of perceptual hypotheses mentioned in subsection a (above) then protected the prejudiced first impression from change.

With hindsight, BGA's discussion of one experimental result was especially interesting. In situations of stress and/or cognitive overload (caused in various ways: p. 235), their subjects relied more than usual on criterial attributes. Easily available cues were used in preference to logically reliable ones.

What's significant about that is that BGA didn't reject this strategy as an error, nor as a lazy prejudice that should be avoided. Rather, they valued it as rational—or anyway, sensible (238 ff.). In other words, this was an early example wherein 'satisficing' was valued above logic. Later work in cognitive science would stress the adaptiveness of such thinking—and, in so doing, would challenge the philosophers' notion of rationality itself (see Chapter 7.iv).

c. Computational couture

Bruner's post-war tailoring hadn't been cut from computational cloth. In the mid-1940s, personality theory had contributed more threads than information theory. As for computers, these were still a dream—except in top-secret Bletchley Park (Chapter 3.v.d). By the early 1950s, however, things had changed. To be sure, Bruner didn't start writing programs. Nevertheless, the New Look had now acquired a clearly computational style.

For instance, the notion that perceptual input would be tested against internal models, or hypotheses, owed something to Craik as well as to Bartlett. More to the point, the guiding questions in BGA's 1956 book couldn't have been asked without the spur of information theory.

In fact, they'd been inspired by John von Neumann himself. In a group meeting at Princeton's Institute of Advanced Study, during Bruner's stay there in 1951, von Neumann had argued that any efficient information-seeking system would have a "strategy" that specified selectivity *not only of what information would be taken up, but also of how that information would be searched for*. This, Bruner said many years later, was "the germ of the idea that started us off on the experiments that went into *A Study of Thinking*" (1980: 112).

As for the New Look in general, Bruner described its main contribution as relating perception to various other forms of mental processing, including psychodynamics and personality traits, "by which information [*sic*] is acquired, retained, and transformed for future use". This non-homuncular language drew the problematic sting from talk of perceptual (or even neurotic) defence (Erdelyi 1974). Twenty years later, many judged the focus on information processing, not the attention to psychodynamics, to be the New Look's most important contribution (Greenwald 1992; Bruner 1992). (There are still some, however, who unfashionably resist *all* talk of unconscious perceptions: Holender and Duscherer 2004.)

The computing metaphor had been important too, he said. He spoke of "metaphor" because virtually no one at the time was actually *modelling* mental processes. His 1957*b* paper mentioned several early machines in passing, due to Oliver Selfridge and Uttley (12.ii.c–d and 14.iii.b), and cited a model sketched by Donald MacKay (1956*b*). It also referred to a toy system that could discriminate a small number of phonemes (Fry and Denes 1953). But these were very simple, and didn't deal with high-level thought. (An interesting AI program for game playing ran in 1949, and an impressive theorem prover in the autumn of 1956: see 10.i.b and Section iv.b, below.)

However, even merely thinking "metaphorically" about the mind in terms of computers helped psychologists to think clearly. It made them consider, for every identifiable point in the reasoning/learning process, *just what* information is being used, and *how*. When Bruner spoke of an inferential "mechanism" underlying perception, he was thinking computationally if not *program*-atically. (Programs based on Bruner's work were very soon written by others, however: see Chapter 10.iii.d.)

In addition, the computer metaphor eliminated the mystery that had formerly attended theories of perceptual selectivity, or "filtering". Whether the filters were ascribed to learnt expectancies or to motivations, they'd had an unsettling air of paradox. For, it seemed, the stimulus had to be perceived before being not-perceived. Now, the paradox was dissolved:

> Many of the forms of filtering required by a selective system could now be reformulated as ordinary operations of ordinary computers—*and man surely was one of the most high-powered versions of a computer, whatever else he might also be*. The old objections against filtering in perception as implying a Judas Eye that first had to have "a tiny look" before deciding what to exclude were at last being undermined. (Bruner 1983: 277; italics added)

By the same token, studies of perceptual masking, such as Miller's (1947*a*,*b*) and Broadbent's (1952*a*,*b*), were now less mysterious. In cases of 'meta-contrast', for instance, one stimulus masks (blots out), or sometimes modifies, another one. So visual stimulus

A may make stimulus B invisible. What's so surprising, and had previously been deeply mysterious, is that stimulus A is presented *after* stimulus B. How can an input that's presented later prevent (or even modify) conscious awareness of an input that was presented earlier? Once one realizes that a percept isn't a passively triggered sense datum, but a mental structure that takes time—and several information-processing steps—to be actively constructed, one can see that such things are *possible*. (Explaining them in detail is another matter; interesting work on meta-contrast was done by Paul Kolers, a visitor at Bruner's Center for Cognitive Studies in 1962–4: Kolers 1968, 1972.)

Bruner first engaged with computational ideas in the early 1950s. He'd come across psychologically oriented pioneers on both sides of the Atlantic. Most were based on his patch: Harvard–MIT.

One local colleague (at MIT's Lincoln Laboratory) was Selfridge, whose work on Pandemonium Bruner later recalled as "one of the first instances in which the new computing metaphor began to affect my way of thinking" (1980: 109; cf. Chapter 12.ii.d). Another was Miller, whose research on linguistic redundancies chimed with Bruner's interest in "how people constructed 'theories' or 'models' ". Chomsky's attempt to formalize language, and to see it as a *generative* system, was relevant also (Section i.e above, and Chapter 9.vi). (The influence was mutual: Chomsky later told Bruner that the New Look had prompted his own notion of a "Language Acquisition Device" generating hypotheses about well-formed utterances—Bruner 1980: 81.)

Craik, too, "was beginning to make an impression" (Bruner 1980: 110). Bruner entered Craik's (posthumous) circle of influence when he spent 1955–6 in Cambridge, England. There, Bartlett's student Gregory became one of his two "closest companions"—the other being Broadbent (1980: 101). Gregory was a perceptual psychologist, a follower of Craik, and a fan of engineering/computational models (see subsection e below, and Chapter 14.iii.a). Such models were being hotly discussed, for instance by the early connectionist Uttley, whose 1958 meeting in London—at which Gregory gave a paper—caused much excitement (Section iv.b, below).

The New Lookers had started out with an anti-Newtonian aim: "to liberate psychology from the domination of sense-data theory, the notion that meaning is an overlay on a sensory core" (Bruner 1983: 103). But—in Bruner's view (and mine)—they merely "readied the ground" for new theories of perception.

The real change was effected by "the cognitive revolution . . . [and] particularly the respected metaphor of information-processing automata—computers" (p. 104). By exploiting that metaphor, the New Lookers' hopes could be fulfilled. Computational psychology could integrate perception with other aspects of cognition: not only concepts and inferences, but also needs, wishes, styles of mind, and patterns of personality. And this was so "thanks, ironically, to *the liberating effect of the computer on the psychologist's image of what is humanly possible*" (p. 104; italics added).

By 1960, then, Bruner had used these new ideas to rescue perception from the psychophysicists' grasp, and even from 'pure' cognitivism. And although he himself didn't do computer modelling, BGA had given those who did some reasonably rigorous ideas to play with.

Indeed, *A Study of Thinking* "served as the basis for a lot of Alan Kay's work with Apple Computer" (J. S. Bruner, interviewed in Shore 2004: 43; cf. Chapter 13.v.c–d). And Douglas Engelbart's pioneering vision of personal computing, though it didn't cite

BGA by name, had explicitly remarked that understanding how people process different types of concept structure would be essential for improving interface technology (1962: 85 ff.; cf. 10.i.h).

Bruner's interests, already broad, now broadened further. Having started with personality and motivation, he passed on (in 1957) to development and education. His developmental psychology stressed the growth of increasingly powerful *systems of representation* in the child's mind: enactive, iconic, and verbal–symbolic (e.g. Bruner 1964, 1966*a*,*b*). This work, also, was taken up in the design of the Apple computer. His ideas on education (which had a huge influence in the USA) drew on that theory, and his widely read book *The Process of Education* (1960)—which appeared in over twenty languages—stressed neo-Craikian "models in the head". This focus on how knowledge is actively structured by the mind was evident, too, in his intriguing discussions of creativity and myth (1962).

Bruner had a huge influence on education in the USA, and was invited to become the policy top-dog in the field—an invitation which was declined:

> Mac Bundy wanted to know whether I would be available [for the post of head of the US Office of Education]. And I thought about it real hard and I said no, because the fact of the matter is I really think I do better not upholding official policy. I do better in the role of the oppositional [or rather, dialectical] critic. (Shore 2004: 36)

His approach to education is interesting, here, not least because it put pedagogy into a comparative biological context. In his wide-ranging paper 'The Nature and Uses of Immaturity' (1970), he argued that play is adaptive because it allows the young child (or mammal) to experiment with actions whose consequences might be costly, even disastrous, if done 'for real'. Having introduced biological comparisons into his account of education, he soon moved on to quasi-ethological studies of mother–infant communication.

Based in Oxford in the 1970s (having sailed his small yacht across the Atlantic in 1972 to take up the new Watts Professorship of Psychology), he and his student Michael Scaife pioneered what was to become a hugely influential experimental method for studying very young babies (see 7.vi.h)—and, eventually, for modelling social interaction in humanoid robots (15.viii.a).

The eye movements of adults had already been used as indicators of their internal information processing, namely, their perceptual strategies (Yarbus 1967). Now, Scaife and Bruner (1975) worked out a way of using babies' eye movements to discover what they're interested in, and in particular of tracking the joint attention of mother and baby. Mothers, they discovered, are able to get their babies to look at something by looking at it themselves—and the baby's gaze will follow.

They did this because they thought that "very early on, starting at three or four months, there had to be some primitive form of intersubjectivity" (J. S. Bruner, in Shore 2004: 82). And that, indeed, they found to be the case. Since interest is correlated with novelty (see Chapter 8.iv.b), these experiments also showed what babies of various ages could and couldn't recognize, or discriminate.

The results were an eye-opener (no pun intended!). Babies can discriminate a good deal more than had previously been thought—and what they attend to is greatly influenced by the direction of the mother's gaze. In that sense, 'the baby's looking' is *a joint action* of mother and baby combined. Social cognition had been found to be a key aspect of cognitive development. Later work, including that done by Bruner's Oxford students Colwyn Trevarthen (1931–) and George Butterworth (1946–2000), reiterated the importance of social "scaffolding" in what had previously been assumed to be 'pure' cognitive development, or 'individual' learning (see C. B. Trevarthen 1979; Butterworth 1991; and Chapters 7.vi.h and 13.iii.e).

As well as his concern for the mother–baby social dyad, Bruner was interested in the psychological effects of culture as such. (He was one of those who had always seen anthropology as a crucial aspect of cognitive science: see 8.i.a and 8.ii.a.) Culture, he said, is crucial to cognition: to its *inner nature*, not just the particular subject matter it happens to be concerned with. For he argued that cultural artefacts, especially widely shared representational systems—such as drawing, language, and numerals—provide various "cognitive technologies" (his term). These shape, and empower, the mind itself (e.g. Greenfield and Bruner 1969; Cole and Bruner 1971).

Bruner's work on language as a cognitive technology owed a great deal to the Soviet psychologists Alexander Luria (1902–77) and, especially, Lev Vygotsky (1896–1934). Both men were often mentioned in his seminars at the Harvard Center in the early 1960s. Indeed, he was largely responsible for the first publication in English of Vygotsky's posthumous book *Thought and Language* (1962), for which he wrote an appreciative Introduction (pp. v–x). (He also wrote a preface for the 1987 edition of Luria's *The Mind of a Mnemonist*, first translated in 1968.) Although Vygotsky became *persona non grata* with the Soviet authorities, largely because of his interest in the non-Marxist topic of consciousness (Kozulin 1990), his developmental psychology reflected the Soviet respect for social, communal, influences. Where Jean Piaget saw the origin of language in the sensori-motor behaviour of the individual child, Vygotsky saw it as the gradual internalization of external signs learnt from the child's social environment (cf. Chapter 12.x.g).

During his seven-year stay in Oxford in the 1970s, Bruner did further important work on cognitive technologies. For instance, he and Michael Scaife did pioneering research on the development of cooperative attention and turn-taking in mother–infant communication, showing how these social activities "scaffold" the growth of language—the most important cultural tool of all (e.g. Scaife and Bruner 1975).

In short, he anticipated some of the insights of the anthropologist Edwin Hutchins and the philosopher Andy Clark, who would write about such matters in highly provocative terms in the late 1990s (Chapters 8.iii and 16.vii.d; see also 13.iii.d). And he influenced Kay yet again, by implying that successful human–computer interfaces would draw on *several* cognitive technologies, not just one (13.v.b and d).

d. Costume change

Eventually, Bruner moved on still further, to cultural matters in general—and to "interpretation" rather than "explanation". The latter move ousted him from his position as a universally respected guru of developmental psychology and cognitive science. For it was seen by some of his previous admirers as an intellectual betrayal.

One can compare his move to the apostasy of Hilary Putnam, who—at much the same time—abandoned functionalism for a neo-Kantian philosophy of mind

(Chapter 16.vi). But in the eyes of Bruner's bitterly disappointed followers, it wasn't just comparable: it was worse. Because of the nature of philosophy, philosophers are relatively tolerant of people changing, even fundamentally overturning, their previous views. But experimental scientists, besides having intellectual convictions as deep as anyone's, have hard-learnt technical skills and expensive laboratory equipment, neither of which can be changed overnight. So the suggestion that their entire project has been fundamentally wrongheaded is experienced as even more of a threat.

That helps to explain why Bruner's sojourn in Oxford was brought to a premature end in the mid-1970s by opposition from his colleagues in the Experimental Psychology Department. They felt that in his public pronouncements, if not in his experimental work, he was bringing psychology into disrepute. In particular, his high-profile Herbert Spencer Lecture in 1976, on 'Intentionality and the Image of Man', caused a scandal.

In it, Bruner criticized scientific (including computational) psychology for ignoring meaning, intention, and responsibility. In doing that, he said, psychology was inevitably distancing itself from the human and cultural sciences—such as his old love anthropology, and jurisprudence and sociology too. As he put it later: "a psychology that is negligent or inattentive to the other social sciences and to philosophy will inevitably be bland, particular *and even trivial*" (1983: 280; italics added).

Retribution was swift. It was the sole occasion, says Bruner, on which the overly compartmentalized department ("I have rarely seen an unhappier one"—1983: 264) pulled together. And it was brutal:

> [A] weekly departmental seminar was organized; my friends referred to it merrily as the "Bruner-bashing seminar". It was in the high English academic abrasive tradition. I never was much good at it. But at least the issues were aired. (Bruner 1983: 265)

Week by week, his views were not only rebutted, but ridiculed—by the very people who'd invited him to Oxford in the first place.

This wasn't a mere personal vendetta. Nor was it grounded merely in intellectual, albeit passionate, disagreement. For the experimentalists' hackles had previously been raised by the counter-cultural turn in social psychology, and the general public's sympathy for it (Section i.d, above). The popular, not to say populist, political/intellectual attacks on their way of doing psychology were at their height. Had that not been the case, the Herbert Spencer Lecture might have been seen as a mark of eccentricity (highly valued in England), not professional betrayal.

In short, Bruner's departmental colleagues were already under siege. So this, from one of their own, was a bitter blow. Punishment, indeed *public* punishment, was called for. In other words, this exercise in "the high English academic abrasive tradition" was in fact a "ritual degradation" (Shotter 2001). It even fitted the classic criteria of a status degradation ceremony, whereby someone perceived as a deviant is expelled from a social group (Garfinkel 1956). Far from being a purely personal matter, it was a skirmish in the (still-continuing) war between opposing philosophical conceptions of mind and psychology (see 16.vi–viii).

Bruner left Oxford soon afterwards. Much as he loved the place, and much as he savoured its many eccentricities, the atmosphere surrounding him at the Psychology Laboratory had become insufferable.

But he was (and still is) unstoppable. In his mid-seventies he became adjunct Professor of Law at the New York University Law School, because of his interest in legal reasoning—which combines "coherent system" with "messiness" (Shore 2004: 40). He sees law as largely grounded not in formal rules but in case-based reasoning, where the "cases" concerned are *stories*, whose narrative structure has to be mapped onto legal concepts such as "negligence" or "attractive nuisance" (Bruner 2002; see 13.ii.c).

Clearly, then, he wasn't taken over by computing and information theory, even though he'd been heavily—and fruitfully—influenced by them. Some forty years after his New Look phase, he confessed:

> I think I am suspicious of "formal" models of human behavior—theories couched exclusively in mathematical terms or in abstract "flow diagrams". I have always been sympathetic to the metaphors of computation and information processing, but resistant to getting trapped in their necessary measurement constraints. Perhaps I feel that such systems of measurement trap you on their flypaper while you are still wanting to fly . . .
>
> I did not, in consequence, get deeply drawn into the Harvard–MIT "cognitive sciences" network—the remarkable group of people who were pursuing formal ideas about information processing and computing and decision-making. George Miller and Oliver Selfridge kept me abreast enough, I thought. I regret now not getting more involved earlier. (1983: 99)

However, if in 1983 Bruner still "regretted" not having done more specifically computational research, he felt differently fourteen years later. By then, he believed that computational metaphors and methods *had* trapped psychologists on the flypaper. He was still "a well-wisher", he said. But he felt that "computational models are not, on the whole, much concerned with my principal interest . . . [namely] how human beings *achieve* meanings and do so in a fashion that makes *human culture* possible and effective" (1997: 281; last italics added).

As a description of what computationalists have actually done, this was fair. Whether they're *necessarily* trapped on the non-cultural flypaper, as Bruner suggested (1997: 281–9), will be discussed later (Chapters 8 and 16.vi–viii).

e. Will seeing machines have illusions?

If Bruner was the leading New Look psychologist in the USA, his counterpart in the UK was Gregory. He was more explicitly committed to 'mind-as-machine' than Bruner was, and even more important in spreading this idea far and wide.

His huge influence in the 1960s was partly due to his provocative critique of the way in which brain-ablation studies were commonly interpreted at the time (see Chapter 14.iii.a). As he said, an engineer who applied the same logic in diagnosing faults in a radio set would be laughed out of court. But his influence was mostly due to his ingenious experiments, described in his unusually accessible book *Eye and Brain*, first published in 1966 and now in its fifth edition (1998).

By means of this publication, Gregory did more than anyone else to introduce the general public, and many generations of psychology students too, to the idea that sight involves active interpretation by the brain. The book skilfully integrated work in computational psychology, neuroscience, and philosophy to tell a fascinating story about vision.

One of the stories within the story concerned Gregory's experience with a man who'd been blind from 10 months of age, whom he called S.B. He'd first heard of him in January 1959. At that time, neuroscientists were suggesting—to huge excitement—that some visual interpretation is carried out by dedicated mechanisms in visual cortex (Chapter 14.iv). But as Bruner's playing-cards had shown ten years earlier, the higher brain centres can play a role too. In addition, Hebb had argued that early visual experience would have a lasting effect on the cell assemblies involved, and Hebb-inspired experiments on animals reared in darkness had confirmed this (Riesen 1947, 1958; see also Chapter 14.vi.b). So when Gregory read in his daily newspaper that a blind man had "immediately recovered his sight" after corneal replacements, he smelt a rat (Gregory and Wallace 1963: 15).

He sought permission to work with the man, predicting (correctly) that he would *not*, at first, be able to see—unless, through touch, he already 'knew' what to expect and could somehow transfer information from one sense to the other.

The work with S.B. addressed the question famously posed to John Locke by his friend William Molyneux (Chapter 2.x.a). Indeed, Gregory had first encountered it in Locke's own writing. On leaving school at 16 he was too young to go to university, and joined the RAF's Voluntary Reserve instead. He was called into the RAF proper a couple of days after his seventeenth birthday, and worked on the development of airborne radar (R. L. Gregory, personal communication). But in 1947 he was free to go to Cambridge, where he studied Moral Sciences (alias philosophy)—with Bertrand Russell as one of his teachers, and Locke as one of the set texts.

In 1949 he turned to experimental psychology, moving a mile along the Cam to become one of Bartlett's last students. Coming from the same Cambridge stable as Craik, whose engineering ideas were "dominating" the department at the time (Gregory 2001: 382), Gregory was well versed in the notion of anticipatory internal models. He still regards Craik's concept of physical representations in the brain as "surely the most important single idea in cognitive psychology" (p. 383).

As a youngster, he saw it as an exciting new version of a key insight due to his intellectual hero, Hermann von Helmholtz: namely, that "unconscious inferences" are necessary for perception (2.vii.c). But inferences may be faulty. Even if a perceptual inference or hypothesis is reliable for 99.9 per cent of the time, there's the other 0.1 per cent too. Gregory took this fact to heart, in deciding to study visual illusions. That was a "deeply unfashionable" topic in the 1950s, although one that had interested Helmholtz, among others, 100 years earlier (Gregory 2001: 388). Illusions, then, fitted his vision of the eye–brain as a (biologically evolved) *machine*, whose 'pre-programmed' functions might sometimes lead us astray.

Much as Craik had argued for close relations between internal models and highly general features of the external world, so did Gregory. Many illusions, for example, depend on shadows (1966: 184–7). That's to say, they depend on our expectations, whether learnt or inbuilt, that there's a single sun located above us (pp. 236–7).

When such fundamental expectations are false, as may happen in space travel, unprecedented errors of judgement result (pp. 229–37). In the early 1960s, Gregory (personal communication) was invited to give two lectures at NASA's Houston Space Centre, after the 'inexplicable' failure (twice) of their astronauts to dock in space—a simple enough manoeuvre, they'd thought. The problem, Gregory told them, was the highly unusual visual environment. Shadows weren't helpful, because of the anomalous position of the sun; and familiar distance cues were missing, because there were no physical objects to be seen, apart from the space dock itself. (The stars weren't visible as three-dimensional objects, but as mere spots of light.) After further training with these psychological matters in mind, the astronauts on the next voyage managed to dock successfully.

Largely because of his wartime engineering background, the mind-as-machine approach was more evident in Gregory's work than in his friend Bruner's. For example, he soon argued (1967) that *any* effective visual system, including a seeing *machine*, will be prone to illusions just as we are. Illusions aren't mere human fallibility. Rather, they're an inevitable result of a system's using internal representations to go beyond the information given (one of Bruner's favourite phrases: 1957*a*). By the same token, they're a promising way of finding out what those representations (hypotheses) are. Gregory's hugely ingenious demonstrations of newly discovered visual illusions became justly famous. But the point, of course, was to understand *normal* vision.

Gregory and Bruner communicated often during Bruner's visit to Cambridge in 1956. But Gregory didn't share his American colleague's interest in the perceptual effects of psychodynamics. He was much closer to psychophysics and neurology (Gregory 1966). (Which isn't to say that Bruner totally ignored neurology: see Bruner and Klein 1960.)

He was closer to AI and computational modelling, too. Although most of his research focused on human subjects, Gregory did some work in robot vision, while at Edinburgh's pioneering Department of Machine Intelligence and Perception—which he'd helped to found in 1967 (Section iv.e, below). The first edition of his hugely popular *Eye and Brain* ended thus:

> We think of the brain as a computer, and we believe that perceiving the world involves a series of computer-like tricks, which we should be able to duplicate, but some of the tricks remain to be discovered and, until they are, we cannot build a machine that will see or fully understand our own eyes and brains. (1966: 237)

Nine years later, he put it in a nutshell: "As man teaches machines to see, machines teach us *what it is to be able to see*" (1975: 627; italics added).

In the third edition of his best-seller, Gregory added a final chapter on 'Eye and Brain Machines'. And there, he expressed this Craikian view:

> The problem is to build in knowledge and allow machines to learn about the structure of the world, so that they can develop internal representations adequate for finding solutions within themselves. When machines have knowledge they can be imaginative. When they have imagination they, like us, may live with reality by acting on their postulated alternatives rather than as slaves to signalled events. (1977: 236)

He'd already built a machine with visual hypotheses, if not with "imagination", in the 1960s. He'd done so with his then assistant Stephen Salter (later, the inventor of the wave-power 'duck'), whose technical ingenuity was to be invaluable to him for many years (see Chapter 1.iii.f). First outlined in *Nature* in 1964, this early hypothesizing machine wasn't a digital computer. Nor was it programmed, as the

Edinburgh robot was. Rather, it was a specially adapted camera (Gregory 1964/1974; 1970: 170 ff.).

The camera was designed for use with earth-based telescopes, since it could compensate for potentially confusing effects in the image caused by atmospheric disturbances. It worked on broadly Craikian principles, in that it first built up an *average* image of the relevant portion of the night sky and then used this as a model against which future inputs were judged. When the difference between the input and the model was at its lowest (measured automatically, as electric currents), a high-speed photograph would be taken. Several such high-speed images would be superimposed. The result was always much clearer than the model itself, which was very blurred because it had been constructed by taking a long-exposure photograph.

Gregory didn't claim that this model was similar to the visual models in our brains. But he did see it as an example of a general (Craikian) psychological principle, that "sensory information is used to build up symbolic models of the world in the brain" (1970: 170). His extensive research on visual illusions aimed to identify some of those "symbolic models", and to distinguish those due to individual experience from those due (thanks to evolution and/or post-natal development) to the physiology of the eye–brain.

One familiar example is the Müller-Lyer illusion, in which lines of equal length *look* as though they're different (see Figure 6.3). Gregory reported that all human subjects so far studied—and even pigeons and fish—suffer this illusion to some degree (1966: 160 ff.). Nevertheless, people in whose environments there's not a straight line to be seen are much less susceptible to it. So rural Zulus—who live in round houses with round doors, keep their food in round pots, and plough their furrows in curves—are only slightly affected. (Later, it was found that Zulu children who visit the town for their schooling are more susceptible than their rural parents, but less than city-living Zulus and Westerners.) Evidently, said Gregory, this illusion is largely due to learnt expectations, being strongest in people surrounded by straight-sided artefacts such as boxes and buildings.

He didn't conclude that all illusions depend on learning. Although some are entirely absent in non-Western environments, others affect every cultural group so far studied—and some non-human species too. Presumably, the latter involve 'expectations',

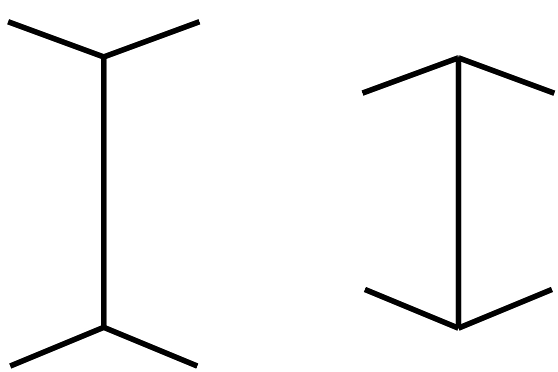


Fig. 6.3. The Müller-Lyer illusion. Adapted with permission from Gregory (1966: 136)

or 'hypotheses', built into the biology of the visual system as such—including visual cortex, but not the higher centres of the brain. But just what are they? In this early book and in much of his later research, Gregory suggested/rejected a host of detailed processing explanations for the phenomenological facts.

Gregory had encountered anti-Craikian views as a student, when he was "saturated in [James Gibson's] direct theory of vision" (see Chapters 5.ii.c and 7.v.e–f). But, thanks to his grounding in Helmholtz, he "never believed it" (Gregory 2001: 387–8).

His disbelief was buttressed by some early experiments he did at Farnborough air base, using their human centrifuge (Gregory 1974*a*: 295). These showed that the change of size in after-images, when the subject moves forwards and backwards (a phenomenon that was already familiar) may sometimes be set by the subject's *hypothesis* of distance, and of how he is moving through space. "It was these observations", Gregory recalled later, "which gradually made my thinking deviate sharply from the position held, and argued so well, by [James and Eleanor Gibson]" (Gregory *et al.* 1959/1974: 296).

In sum, the mind-as-machine hypothesis spread far and wide in the mid-1960s, partly as a result of Gregory's research and writings. Like Bruner, he'd warmed to the notion that perception involves predictive hypotheses partly because "it suggests links between [perception and] the methods whereby science gains knowledge" (2001: 388). In other words, as his friend Horace Barlow was then saying in a different context (14.iii.b), the very same psychological mechanisms probably underlie the heights of human thought and the perceptual capacities we share with many other animals.

6.iii. From Heuristics to Computers

The mid-century encounter between Newell (1927–92) and Simon (1916–2001) led to the most fruitful partnership in cognitive science. They caused a sensation at Dartmouth College (New Hampshire) in 1956, where they reported almost the first functioning program in psychological AI (Chapter 10.i.b). Indeed, it's thanks largely to their contribution that the Dartmouth meeting was one of the formative events of cognitive science (see Section iv.a, below).

After their Dartmouth bombshell, they spent the rest of their lives developing an increasingly powerful intellectual artillery, their guns finally silenced only by death. But the reverberations still persist—in AI, in certain areas of A-Life, in psychology, and in philosophy. In short, their echoes are everywhere.

Their mature psychological work, done at Carnegie Mellon, will be outlined in Chapter 7.iv.b. Here, the focus is on their role in getting cognitive science off the ground.

A warning, before we begin: They—and everyone else—naturally spoke of AI systems as searching, knowing, recognizing, trying, remembering, choosing, and the like. Such psychological language is a practical necessity, if one wants to describe what programs do. Even John Searle agrees with that (Pagels *et al.* 1984: 160). Whether it can properly be interpreted *literally* (which Searle robustly denies) is a deep and difficult philosophical question, to be discussed throughout Chapter 16. Newell and Simon eventually argued that it can (16.ix.b).

Many cognitive scientists are content to leave that issue open. But all are committed, at least, to "weak AI" (16.v.c). That is, they hold that computer programs can help in clarifying, testing, and developing psychological theories. This section shows how Newell and Simon's early work helped that view to arise.

a. The economics of thought

The two men had very different intellectual backgrounds: physics and mathematics for Newell, economics and political science for Simon. But both were interested from the start in *human behaviour*.

In Simon's case, the behaviour in question was economic and administrative decision making. His painstaking empirical research as a graduate student and tyro-academic was sociological rather than psychological. But it was underlain by intuitive psychological insights, which would eventually seed his work in cognitive science. Years later, he declared in his autobiography:

> I would not object to having my whole scientific output described as a gloss—a rather elaborate gloss, to be sure—on the pages of *Administrative Behavior* [his Ph.D. thesis, Simon 1947*a*]. (Simon 1991: 88)

That book, *Administrative Behavior*, was—and still is—widely read for its own sake by students of management, business economics, and administration. Besides its discussions of decision making, it devoted several chapters to social-psychological topics such as incentives, organizational loyalty, status, and authority. It has remained in print ever since 1947, the fourth edition appearing a few years before he died; and it has been translated eight or more times. Indeed, it was specifically mentioned in the official notification of his Nobel Prize in 1978. Our interest in it here, however, is as the forerunner of his research in (cognitive) psychology.

To begin with, Simon was more concerned with what goes on inside an organization than with what goes on inside a mind. For instance, in his first book he pointed out that *administrative organization* influences decision making. ("Decision making", not "problem solving" or "information processing": that terminology came later, in 1950 and 1952 respectively.)

The different divisions in a company start from different premisses. They employ different heuristics, or intuitive rules of thumb. (The word "heuristic" has the same root as Archimedes' famed "Eureka!"; Simon focused on problem-solving rules of thumb, but the term also covers default assumptions used in reasoning.) And they follow different goals—for the company as a whole, *sub*-goals. Indeed, the concepts of purpose, goals, and goal hierarchies were set out at the opening of the book as the root of everything in the following 250 pages (1947*a*: 4–8); and means–end analysis was highlighted too (pp. 62–6).

It follows, said Simon, that the various divisions make decisions—about budgets, for example—in different ways. There's no magical guarantee that their decisions will mesh sensibly. For instance, the marketing department may strive to produce highly creative advertising, largely irrespective of its effectiveness or expense. The management's task, then, is to ensure that the divisions are not only individually efficient, but mutually supportive.

Moreover, said Simon, individual employees have *bounded rationality*: they have incomplete knowledge and limited reasoning ability. The term "bounded rationality" didn't appear in the first edition, where Simon put the core idea like this:

> Rationality, then, does not determine behavior.... Instead, behavior is determined by the irrational and non-rational elements that *bound* the area of rationality. (Simon 1947*a*: 241; italics added)

So companies and administrative units should aim for satisfactory solutions, not perfect ones. Likewise, economists should drop their unrealistic vision of "a preposterously omniscient rationality" (1976, p. xxvi), and focus on *satisficing*, not optimizing (1955).

Much later, some of the "bounds" would be identified. For instance, it was found that people play competitive games at only one or two levels of strategic depth (Colman 2003), much as they employ only one or two levels of grammatical embedding (see 7.ii.b). In the 1940s, however, the concepts needed to consider such matters weren't available.

Nevertheless, Simon was already trying to find formal methods for talking about decisions.

* As a graduate student, for instance, he'd attended "with more diligence than usual" Rudolf Carnap's courses on logic and the axiomatic philosophy of science, and found them "particularly important" (Simon 1991: 53; cf. Chapter 9.v.a).
* In the late 1930s he'd co-authored some twenty papers on how to *measure* success in city administration.
* He'd been intrigued by the theory of games even before the key book (von Neumann and Morgenstern 1944) was published, and devoted Christmas 1944 to reading it night and day so as to write "the very first review"—for the *American Journal of Sociology* (1991: 108). Indeed, on glimpsing the title *Theory of Games and Economic Behavior* in a journal advertisement under someone's arm in a concert queue, he'd felt "a flush of envy so great he could remember it vividly thirty years later" (McCorduck 1979: 147).
* He'd described workers in organizations as, in effect, machines for doing logic: given *this* set of premisses and goals, *those* decisions would result. (He himself has identified this as a notion which would later "transform itself" into the mind-as-program view—McCorduck 1979: 127.)
* And when teaching law to engineers in the 1940s, he'd sketched Supreme Court decisions as wiring diagrams for electric circuits, the switches representing the yes-or-no choices of the court (Simon 1991: 96).

He'd even used "prehistoric" (punched-card) machines in the late 1930s—but for doing statistics, not modelling. Although they'd intrigued him greatly, he hadn't realized that similar machines might be used to study thinking:

> Of course, what the calculating punch could do was very limited . . . But the seed of an idea had been planted in my mind, and from that time on I was alert to any scraps of news I encountered about the progress of calculating machines. *I had no idea that I would find a use for them*; they simply fascinated me. (1991: 70; italics added)

By the late 1940s he was adept at wiring computer boards: hence his wired-justice law lectures. But those lectures formalized the *results* of the Supreme Court's ongoing decisions, not the mental processes involved in generating them.

It's all of a piece, then, that when he helped to found Carnegie Mellon's Graduate School of Industrial Administration (GSIA) in 1949, neither he nor anyone else expected to become involved in the computer simulation of psychology. He did expect that "we were going to experiment and see where these new ideas—operations research, or management science, as we preferred to call it, and organization theory—where they led" (interview in McCorduck 1979: 116). But no simulations were foreseen: they "came into GSIA by accident, so to speak".

Around 1950 Simon wrote several papers inspired by cybernetics and automata theory. One of these was a review of Nicholas Rashevsky's *The Mathematical Biology of Social Behavior* (Simon 1951). Rashevsky's seminars on mathematical biophysics had inspired McCulloch (Chapter 4.iii), and they worked their formalist magic on Simon too. But his enchantment wasn't unmixed:

> Rashevsky had a marvellous talent for building simple assumptions into models of biological systems ... [But in] spite of his skills in building mathematical models, *Rashevsky was rather cavalier in his attitude toward data*. He never acquired a deep command of the biological phenomena he was treating and, as a consequence, was generally ignored by biologists. They could have learned a great deal from him, and a few of them [such as McCulloch] did, but the mutual respect was largely lacking on which effective interdisciplinary communication must build. (Simon 1991: 51–2; italics added)

He himself avoided a "cavalier" attitude to data, as we'll see. But he still couldn't express his theory of bounded rationality in formal terms.

Nor could he—or anyone else in the 1940s—conceptualize the *details* of decision making. Stafford Beer, for example, was embarking on a lifetime's work on management efficiency—but he thought about it in cybernetic terms, in which 'internal' decision making didn't feature (see Chapter 4.v.e). So although Simon indicated some of the heuristics used by administrators to compensate for bounded rationality, he did so at a very coarse-grained level. He didn't ask *just why* and *just where* rationality is bounded, merely pointing out that human minds are finite. Similarly, he took it for granted that people can apply heuristics, identify sub-goals, compare alternatives, infer from premisses, and solve problems satisfactorily. *Just how* they manage to do this wasn't a prime concern.

That all changed when he encountered the 24-year-old Newell, early in 1952. This was the "accident" which eventually led to GSIA becoming a hub for computational psychology.

b. A meeting of minds

Why Newell? After all, psychologists had been studying problem solving for decades (5.ii.b). Simon's attention was captured by Newell because the younger man—since 1950 a full-time researcher at the RAND corporation, Santa Monica—was describing human problem solving in *formal* terms.

Like Simon, he had a thorough grounding in game theory. Indeed, he'd worked as a research associate for Oskar Morgenstern for a while, as a student at Princeton. But he wasn't as enthusiastic about it as Simon had been. Although he'd initially applied to RAND because he'd heard that game theory was being studied there (McCorduck 1979: 118), he'd soon got involved in a very different approach. By the time he and Simon met, he was already speaking in terms of "information processing", and thinking about how to program computers to play chess (Newell 1955).

(He *wasn't*, however, thinking about simulating human thought on a computer. The idea of doing that would come to both men together in summer 1954, in a conversation started in a parking lot—McCorduck 1979: 132.)

Moreover, Newell was familiar with the notion of heuristics. He'd taken undergraduate courses with the mathematician George Polya (1887–1985), whose imaginative 'tricks' for arriving at proofs would eventually influence early AI (Polya 1945). (That had been at Stanford; before then, still in Europe at Zurich's Institute of Technology, Polya had also taught von Neumann.) Although Polya wasn't explicitly cited in Newell's early work (on the Logic Theory Machine and General Problem Solver, or GPS), he later acknowledged that Polya had implicitly affected his thinking for many years to come (Laird and Rosenbloom 1992: 20).

RAND saw Newell's work as technology: trying to make computers solve "complex tasks". The psychology was added by Newell, largely inspired by Selfridge's work on pattern recognition, described at a talk at RAND late in 1954 (see below). The crucial insight was that computers could become "truly non-numerical processors" (Newell and Simon 1972: 882). Even before Selfridge's talk Newell was already using 'numerical' machines (card-programmed calculators) to model non-numerical matters, namely, the blips on a radar screen. If pattern recognition, why not chess—or (less difficult) logic? And why not the *psychology* of chess or logic?

Predictably, Simon was sympathetic. For despite his disdain for "excessive formalism and shallow mathematical pyrotechnics" (1991: 249), he'd long sought formal theories of behaviour. He'd been happy to represent Supreme Court decisions as electrical circuits. And he'd very recently started speaking of "problem solving" as well as "decision making" (1991: 163). If he wasn't yet using computers to explore psychological questions, he was on the verge of doing so.

Newell enabled him to take the crucial step. Indeed, Simon soon showed the enthusiasm typical of new converts. In 1952, when he (and Newell) attended von Neumann's RAND seminar on the obstacles facing chess programmers, he felt that the master had "overestimated the difficulties substantially" (1991: 166). (Five years later, he made his notorious prediction that a computer would be the world champion in chess within another ten years: Chapter 10.i.f.)

To cap it all, Simon's "emphasis was on human simulation rather than chess prowess". (He would soon publish formal theories of "rational choice" in leading journals of economics *and psychology*: Simon 1955, 1956). In late summer 1954, they hatched the notion, together, of using computers to simulate human problem solving (McCorduck 1979: 132).—But how? The clue to that came in November, when Selfridge visited RAND to talk about his work (with Gerald Dinneen) on the infant Pandemonium (Selfridge 1955; Dinneen 1955; cf. Selfridge 1959).

As Newell recalled later (and as described in Chapter 12.ii.d), this system didn't merely recognize patterns, but "actually carried out transformations and had several levels of logic to it". For him, it was an epiphany:

> I didn't know Oliver at the time at all. We just sat in an office with five or six other people while he talked about this system they were programming, just in order to keep us people at RAND up on it. *And that just fell on completely fertile ground. I hate to use the phrase, but it really was a case of the prepared mind.* It made such an impact on me that I walked out after a couple of hours and walked into somebody's office—I don't remember whose—and gave them an hour's lecture on this thing. And then I went home that night and designed another system like it, for working on the air-defense center. (Newell, interviewed in McCorduck 1979: 133; italics added)

> I can remember sort of thinking to myself, you know, we're there. Those guys, Oliver and Jerry, had developed a mechanism that was so much richer than any other mechanism that I'd been exposed to that we'd entered another world as far as our ability to conceptualize. And that turned my life. I mean that was a point at which I started working on artificial intelligence. Very clear—it all happened in one afternoon. (p. 134)

Newell soon subjected Simon to more than an hour's lecture on the thing. The older man got the point immediately.

It had been Simon's prepared mind (i.e. formalist and computer-fascinated) which had enabled him to appreciate what Newell was trying to do even at their first meeting. The intellectual attraction was mutual:

> In our first five minutes of conversation, Al and I discovered our intellectual affinity. We launched at once into an animated discussion, recognizing that though our vocabularies were different, we both viewed the human mind as a symbol-manipulating (my term) or information-processing (his term) system. (Simon 1991: 168)

Newell soon enrolled for a Ph.D. under Simon in Carnegie Mellon's Business School (GSIA), and in 1961 joined him as a Professor of Psychology. Simon was already energetically building CMU's Computer Science department, to which—in his eyes—psychology was crucial (later, he was named Professor of *Computer Science and Psychology*). He took pains from the start to insist that they were equal partners, despite the difference in age and status. For instance, their joint publications listed their names alphabetically—although, due to Simon's fame (capped in 1978 by the Nobel Prize for Economics), people often mistakenly reversed the order. In addition, they alternated in accepting talk invitations (McCorduck 1979: 136).

Why they met in the first place was that both were involved in RAND's study of the behaviour in an air-defence facility in Tacoma, Washington. (As explained in Chapter 11.i.a, RAND was a military organization, set up to study techniques of air warfare.) The group was noting the detailed communications between radar operators and air controllers, to discover how these operatives made their decisions and how the stress involved could be reduced. This involved tape-recording and analysing all the phone calls between crew members: an early example of what Newell and Simon would later term "protocol analysis".

In practical terms, they succeeded—so much so that the US Air Force asked RAND to train its air-defence personnel. But the theoretical dividends were initially sparse. Whereas Broadbent, across the Atlantic, was investigating similar matters by considering information flow (Section i.c–d, above), Newell and Simon wanted to get inside the heads of the decision-makers concerned, to discover *what they were doing* with the information, and *how*.

At first, they had no luck. They toyed with geometry, in which the nature of the task seemed more clear. But they soon discovered that they couldn't reliably represent diagrams in the computer (McCorduck 1979: 138).

Then they decided to tackle logical problem solving. Logic was admittedly difficult for their human subjects, but that was deliberate. For Newell, on first arriving at RAND, had done some experiments in which he'd discovered that intelligent people solve simple problems by thinking about them silently and then announcing the answer: good for the subjects' egos, perhaps, but frustrating for the experimenter (McCorduck 1979: 126–7). With the first inklings of the Logic Theory Machine (see below), they felt they were beginning to get somewhere:

> [Despite the profusion of empirical data] Al and I suffered from continuing frustration in trying to write formal descriptions of the process. Somehow, *we lacked the necessary language and technology* to describe thinking people as information processors . . .
>
> [This] frustration . . . had major consequences, the first of which [was the foundation of AI]. In simplest terms, *it determined the rest of my life*. It put me in a maze I have never escaped from—or wanted to. (Simon 1991: 168; italics added)

Their mid-1950s work, which electrified many in the Dartmouth audience, had set the mould for all their later research. In the Logic Theory Machine (also called the Logic Theorist), computers and psychology were inextricably mixed: buffs on both sides were equally excited. This interdisciplinary mix endured. As they put it, some twenty years later:

> [Newell's] initial approach also established the precedents, followed in all of the subsequent work of our project, that *artificial intelligence was to borrow ideas from psychology and psychology from artificial intelligence* . . . (Newell and Simon 1972: 883; italics added)

c. A new dawn

The Logic Theory Machine (LT) was a new dawn in the psychology of reasoning. It solved problems by structuring them in terms of goal/sub-goals, and then searching the space of possibilities in sensible, though fallible, ways (Newell and Simon 1956*b*; Newell *et al.* 1957). The same was true of its more powerful sibling GPS, briefly reported in *Computers and Automation* in 1959, and described more fully two years later (Newell *et al.* 1959; Newell and Simon 1961; see also Ernst and Newell 1967).

If this sounds familiar, that's no surprise. For these two programs—which are described *as programs* in Chapter 10.i.d and v.b—drew on Simon's views on administrative decision making. They involved bounded rationality, satisficing, heuristics, the definition and achievement of distinct sub-goals, and efficient internal ordering and the identification and reduction of 'goal differences', an idea grounded in cybernetics (Chapter 4.vii.a). In other words, unlike McCulloch and Pitts' a priori mapping of propositional logic onto Turing machines (4.iii.e–f), they reflected problem solving *in practice*.

For the psychologists at Dartmouth, the Logic Theorist's triumphant debut was doubly exciting. Not only was it shockingly anti-behaviourist (transgressing all but the third and fourth of the six tenets: see 5.i.a), but it promised new experimental vistas. For instead of rat mazes and rote memories, its topic was logical reasoning—and of a significant kind, namely, proving the first fifty-two theorems of *Principia Mathematica* (Russell and Whitehead 1910).

Logic was chosen as the theme because of the research being directed at Pittsburgh by O. K. Moore (his initials represented the Persian poet Omar Khayyam), who'd asked his experimental subjects to think aloud while solving their problems (Moore and Anderson 1954*a*,*b*). That thinking aloud was the seed of Newell and Simon's lifelong methodology of "protocol analysis". As for the Russell–Whitehead book, this was chosen because it was already in Simon's personal library.

Some ten years earlier, Alan Turing had suggested using programmed search to prove *Principia* theorems. However, he didn't write such a program; and his suggestion remained unpublished outside the National Physical Laboratory (NPL) until 1969 (A. M. Turing 1947*b*; see 4.ii.b), although a briefer version had appeared in *Mind* (Turing 1950; see 16.ii.a). In fact, prior to the reprinting of Turing's 1950 essay in the *Computers and Thought* collection, his work had very little direct influence on NewFAI. It had enthused McCulloch and Pitts, to be sure. But Simon later recalled that Turing had "no particular influence on his work", and Minsky said much the same thing (McCorduck 1979: 95 n.).

Starting from the five axioms and three rules of inference given in *Principia*, Newell and Simon's Logic Theorist managed to prove thirty-eight of the Russell–Whitehead theorems for the propositional calculus. It even found a more elegant proof of one of them (no. 2.85) than that given by the original authors. Bertrand Russell was delighted when he was told this. But the *Journal of Symbolic Logic* refused to publish a paper with the Logic Theorist listed as an author (McCorduck 1979: 142). The editor's attitude was that the *Principia* theorems were old hat—and his readers were interested only in theorems, not in who/what had proved them.

Despite the LT team's unfashionable focus on internal processes, they couldn't be accused of woolly-mindedness, nor—despite the debt to Gestalt psychology—of "having religion up their sleeve" (Chapter 5.ii.b). For after all, it worked. And so did GPS, three years later. GPS could even solve the tricky missionaries-and-cannibals puzzle, which requires one to go backwards in order to go forwards. (Three missionaries and three cannibals on one side of a river; a boat big enough for two people; how can everyone cross the river, without cannibals ever outnumbering missionaries?) Purpose, thinking, and mental representations: all these seemed within reach at last.

It's little wonder, then, that Miller was energized to write a manifesto promising myriad successes for cognitive science (Section iv.b, below). There wasn't an equivalently broad-ranging manifesto for AI as such. Minsky's paper 'Steps Toward Artificial Intelligence', written in 1956, wasn't aimed at a wide audience—and it had been available only as an MIT Technical Report for five years, until it appeared in a journal in 1961. As for what *could* have been the first AI manifesto, this was still languishing unpublished at London's National Physical Laboratory (A. M. Turing 1947*b*; see 4.ii.b)—although it had been briefly outlined in *Mind* (A. M. Turing 1950; see 16.ii.a).

In one sense, of course, these “new vistas” weren’t new at all. The Gestaltists, too, had studied structure, directedness, and heuristics in problem solving. Indeed, Simon had read their work with great appreciation. He’d read Otto Selz (1881–1943) with especial interest, for Selz had focused less on the structure of thoughts than on the process of thinking. Later, after defining *production systems* (Chapter 7.iv.b), Simon even credited Selz with “the idea of condition–action pairs”, quoting Selz’s remark:

> Intellectual processes . . . [like bodily reflexes] are a system of specific reactions in which there is as a rule an unambiguous relation between specific conditions of elicitation and both general and special intellectual operations. (Selz, quoted by Simon 1991: 226–7)

But it was one thing to make such a remark, quite another to spell it out. That, Selz hadn’t done. Nor could Karl Duncker and Max Wertheimer provide chapter and verse for their ideas about problem solving (Chapter 5.ii.b). With LT and GPS, Simon and Newell had provided a methodology for explaining *how* the Gestaltists’ vague and (then) suspiciously mentalistic processes might be formalized and tested.

This “testing” took two forms, technological and psychological. On the one hand, the program must work. On the other hand, it must work in a human-like way, since (for Newell and Simon) it was equivalent to a psychological theory. Just what counts as ‘human-like’ and ‘equivalent’ were intuitively understood: these questions would be explored at length later (see 7.iii and 16.ix.b). Meanwhile, Newell and Simon (1961: 121 ff.) listed some of the relevant dimensions, and compared people with programs as closely as was then feasible.

Such comparisons required psychological knowledge, preferably based on experimental work as well as everyday introspection and common sense. Here, Simon’s healthy respect for empirical data, evident ever since his earliest work on municipal administration, paid dividends. Having studied the detailed experimental reports of Wertheimer and Duncker, he and Newell had already begun their own experimental programme. The careful observation of human subjects, alongside theoretical interpretation and/or computer modelling, would become a lifelong project for each of them.

Their seminal *Psychological Review* paper was co-authored with the computer scientist J. Clifford Shaw (1922–91), a colleague of Newell’s at RAND who did most of the initial programming. (He’d already helped Newell develop the radar simulator, and had written the assembly language for the JOHNNIAC—the machine on which the Logic Theorist was first run.) The paper didn’t attempt detailed mind–machine parallels, although it compared the Logic Theory Machine with human thinking in general terms (Newell *et al.* 1958*a*). Simon describes this paper as “the first explicit and deliberate exposition of information-processing psychology, but without using that or any other trademark name” (1991: 221). (“Information psychology”, of course, had already started, and Broadbent’s key book was published in the very same year; but that was concerned more with information flow than with information processing: see Sections i.a–c, above.)

In 1958, however, GPS was already in preparation, explicitly intended as a psychological theory. Over two-thirds of the first GPS paper was devoted to human/program comparisons (Newell and Simon 1961). Besides, Newell and Simon were already running their own experiments, in which they asked people to talk aloud while solving a problem and then compared the results with the program trace.

At first, they used the type of logic problem that had been tackled by their Dartmouth program (the missionaries and cannibals came later). For instance, they gave an engineering student, ignorant of symbolic logic, twelve rules for transforming one logical expression into another. (These were 'rewrite' rules, as Chomsky's $S \rightarrow \mathrm{NP} + \mathrm{VP}$ and $\mathrm{NP} \rightarrow \mathrm{det} + \mathrm{N}$ were too.) Rule 1, for example, stated that A.B can be transformed into B.A, and that $A \vee B$ can be replaced by $B \vee A$. They told the subject that the logical symbols $\supset$ (horseshoe), $\vee$ (wedge), $\sim$ (tilde), and . (dot) stand respectively for "implies", "or", "not", and "and". Then they asked him to find a way of going from the left-hand expression, below, to the right-hand one:

$$(R \supset \sim P).(\sim R \supset Q) \quad \Big| \quad \sim (\sim Q . P)$$

(The wedge didn't appear in the statement of this problem, but featured in some of the rules used in solving it.)

The young man's protocols included these four (taken from Newell and Simon 1961: 282):

> Well, looking at the left-hand side of the equation, first we want to eliminate one of the sides by using rule 8.
>
> Now—no,—no, I can't do that because I will be eliminating either the Q or the P in that total expression. I won't do that at first.
>
> I can almost apply rule 7, but one R needs a tilde. [S]o I'll have to look for another rule. I'm going to see if I can change that R to a tilde R.
>
> As a matter of fact, I should have used rule 6 on only the left-hand side of the equation. So use 6, but only on the left-hand side.

Whereas the first three of these protocols matched 'equivalent' actions of GPS, the fourth didn't. Nothing in the program trace corresponded to this self-correction, which involved the realization that applying rule 6 at one point would *undo* its application at a previous point. It would be another ten years before AI would be able to implement such self-criticism (Chapter 10.iii.d).

That's not to say that GPS couldn't provide *any* self-corrections, or changes of mind. For example, Newell and Simon saw a "good agreement" between its performance and this human report:

> Now I'll apply rule 7 as it is expressed. Both—excuse me, excuse me, it can't be done because of the horseshoe. So—now I'm looking—scanning the rules here for a second, and seeing if I can change the R to ~R in the second equation, but I don't see any way of doing it. (Sigh.) I'm just sort of lost for a second. (Newell and Simon 1961: 282)

Where others would later model program redirections on people's emotional hang-ups (7.ii.c), the GPS designers modelled them on the inner processes of rational thought. And the details of the subject's imperfect behaviour were taken seriously.

The authors admitted various instances where trace and protocol diverged. But they didn't despair of AI as a result. To the contrary, they tried to explain these divergences in terms of general features of problem solving (such as self-correction due to hindsight: see above) which GPS lacked, but which some future program might possess. That is, they showed—by Popperian "conjectures and refutations"—how an AI program can be used not only to *model* a psychological theory (so testing its coherence), but also to *criticize* it on empirical grounds, and to indicate ways in which it might be *improved*. The Gestaltists had been vindicated, but the vindicators had gone way beyond them.

That's not to say that behaviourism had been rejected entirely. Besides claiming descent from *both* Gestalt and behaviourist psychology (see Chapter 7.iv.a), Simon did some early modelling work that was closer to the latter. With Edward Feigenbaum, about to become famous as co-editor of *Computers and Thought* (and later more famous still, as the populist champion of expert systems: Chapter 11.v.a–c), he developed a simulation of rote learning (Feigenbaum 1961; Feigenbaum and Simon 1962; Simon and Feigenbaum 1964).

This skill had been studied fifty years before by Hermann Ebbinghaus (2.x.a), whose atomistic, objectivist, and meaning-less approach was highly congenial to the behaviourists. Indeed, they'd adapted his nineteenth-century methodology for their own experiments on "verbal learning". Simon and Feigenbaum's EPAM model (Elementary Perceiver And Memorizer) aimed to specify the information processes that make rote learning possible, and to explain some of the positional effects first reported by Ebbinghaus. Indeed, EPAM aimed—with some success—to model people's mistakes in memory tasks as well as their achievements (see 10.iii.d).

The moral was said to be that behaviourism was inadequate. Information processing must provide the theoretical terms for psychology:

It is asserted that there are certain elementary information processes which an individual must perform if he is to discriminate, memorize and associate verbal items, and that these information processes participate in the cognitive activity of all individuals. (Feigenbaum 1961: 122)

Despite its 'mentalism', however, EPAM could be read as buttressing behaviourism, not rejecting it. For its authors apparently assumed that mastering lists of nonsense syllables is appropriately described as *verbal* learning, and even as the learning of *language*. Some years before EPAM was fully implemented, both Chomsky (1957, 1959*b*) and the manifesto authors (see iv.b, below) had declared that view a fundamental mistake.

The Logic Theory Machine was a new dawn in *philosophy* too, for it offered new philosophical vistas as much as psychological ones. Simon's interest in philosophy, expressed by his fascination with Carnap's lectures and by early papers on the philosophy of science (Simon 1947*b*, 1952), had been channelled into a 'functionalist' view of mind. Indeed, he himself saw the LT less as a new dawn in philosophy than as a final closing of the philosophical curtains:

[We] invented a computer program capable of thinking non-numerically, *and thereby solved the venerable mind/body problem*, explaining how a system composed of matter can have the properties of mind. (Simon 1991: 190; italics added)

However, as we'll see in Chapter 16, philosophical problems aren't solved so quickly. Over the next three decades, both Newell and Simon would devote a good deal of time to justifying that claim—for instance, by articulating the theory of "Physical Symbol Systems" (16.ix.b). Many, including John Searle (1980), would remain unconvinced (16.v.c). In brief, it's still highly controversial whether they'd solved the mind–body problem—and even whether the 'problem' was a real one in the first place (16.vi–viii).

6.iv. The Early Church

A new church needs more besides a handful of theological leaders. It also needs consciousness-raising meetings, perhaps a sacred text, some mission stations, and working missionaries. Much the same is true of a new field of scientific research. Miller, Chomsky, Bruner, Selfridge, Simon, and Newell: all these high priests were crucial. But without enthusiastic disciples and organized church activities, their message wouldn't have spread as it did.

The first mission statement of cognitive science—hardly a sacred text, but hugely important as a manifesto nonetheless—appeared in 1960. It would have been less sympathetically received had it not been for the interest already aroused by three inspirational interdisciplinary meetings of the late 1950s.

The first mission station was established in New England, at much the same time—although an important precursor had existed in the same place for ten years. Before another ten years had passed, other early missions were starting to form on both sides of the Atlantic.

a. Consciousness raising

It's no accident that people still remember the three mid-1950s meetings that launched cognitive science—and also those which launched PDP connectionism in 1979, and A-Life ten years after that (Chapters 12.v.b and 15.x.a). For each of these occasions involved consciousness raising of a high order.

Consciousness raising isn't the same as making new converts. The former typically involves people who are *already* sympathetic to some challenging new idea, or anyway are on the verge of accepting it. The latter spreads the idea to people who previously had no inkling of it. So although consciousness raising may produce enormous, even infectious, enthusiasm, those who are infected are members/hangers-on of the same club. Outsiders aren't involved, or may remain immune. As we'll see, this applies to the three meetings considered here. Even the most famous, which took place at Dartmouth College, made very few new disciples: Simon later recalled that "Dartmouth got only half a dozen people active that weren't before" (quoted in Crevier 1993: 49).

The more interdisciplinary a new field is, the more important face-to-face gatherings are likely to be. The different specialists need to enthuse each other, to convince each other that their particular field *is* potentially relevant even though this may not be immediately obvious. Only then can substantive interdisciplinary research be done.

Conversations over the coffee cups are more helpful, here, than podium talks. Few professional academics have the confidence, or indeed the desire, to speak informally from the platform—excepting 'Panel' sessions. In any event, they may not know what interests, or bemuses, the other-disciplinary members of their audience. Far better to face them in person, where both individuals can speak in words of one syllable (unfamiliar jargon being explicitly questioned), and where one person's enthusiasm can excite the other's.

When this works well, the interchange quickly ratchets up to an intellectually provocative, and mutually enjoyable, level. Moreover, in small 'invitees-only' groups people accept that every other member is probably worth listening to, even if they've never even heard of them before. They're probably imaginative too, or they wouldn't have been invited in the first place. The social costs of saying something ignorant, or apparently foolish, are vastly reduced.

McCulloch had organized the interdisciplinary Macy seminars on this principle (4.v.b). There were fewer than thirty regular participants, and never more than five invited guests. The meetings were highly informal, and recorded by minuted notes of discussion rather than prose texts. No official *Proceedings* appeared until six years after the first meeting (held in 1944), and even these offered verbatim transcripts and discussion notes rather than polished essays.

Turning intellectual provocation into satisfaction, of course, requires not coffee cups but test tubes, or the equivalent. In other words, meetings and manifestos are all very well, but hard work is needed too. One of the Macy participants, the neurophysiologist Ralph Gerard, described both the excitement and the danger of coffee conferencing:

> It seems to me . . . that we started our discussions and sessions in the "as if" spirit. Everyone was delighted to express any idea that came into his mind, whether it seemed silly or certain or merely a stimulating guess that would affect someone else. We explored possibilities for all sorts of "ifs". Then, rather sharply it seemed to me, we began to talk in an "is" idiom. We were saying much the same things, but now saying them as if they were so. (von Foerster 1950–5, seventh conference, p. 11)

The Macy meetings had caused much excitement among physiologists and engineers. But they'd been less provocative to psychologists. Admittedly, mathematical psychology arose, thanks to information theory. But the contents of the mind were all but ignored: *meaning* was hardly mentioned. Only a few mavericks, such as Gordon Pask (a 'lodger' at the interdisciplinary Cambridge Language Research Unit, or CLRU), used cybernetic ideas to discuss specific concepts or beliefs (Chapter 4.v.d–e). Even machine translation (MT), which had started in a number of places by the late 1940s (Chapter 9.x), wasn't being done in a psychological spirit. Nor did MT take *sentences* seriously: Lashley was still almost alone in focusing on hierarchical structure in behaviour.

Computational psychology would need more than cybernetics and MT, more than Macy and the Hixon symposium, and even more than the meaning-rich New Look, to get off the ground.

Some might argue that it got off the ground in 1943. For that year saw the introduction of the McCulloch–Pitts neurone, Craik's book on cerebral models, and the first publication of Arturo Rosenblueth's (and Norbert Wiener's) ideas on purpose. However, the scientific community, albeit intrigued by the last, wasn't instantly enthused by any of them (see Chapter 4.iii.f and vi.d). A few insiders got the point, but it's only with hindsight that this year seems so significant.

It wasn't until 1956 that the fog obscuring mind-as-machine from wider view began to clear. One year later, Chomsky's *Syntactic Structures* (1957) brought language—that is, sentences—publicly into the formalist fold. And one year after that, seminal ideas in connectionism and in computational neuroscience suddenly became a talking point. In short, it was only in the mid- to late 1950s that the new—and shocking—approach to mind captured the imagination of anyone but a few cognoscenti.

Asked to name the *annus mirabilis* for cognitive science, I'd choose 1956. That year saw a sudden growth in the invisible college, as six events made people aware that something new, and exciting, was happening. Four were publications, and two were interdisciplinary meetings. A third such meeting, held two years later, would turn out to be equally important (see below).

The publications were BGA's *A Study of Thinking*, Miller's 'Magical Number Seven', Nathaniel Rochester's paper on Hebbian theory (Chapter 5.iv.f), and—across the seas—Ullin Place's 'Is Consciousness a Brain Process?' Place's paper is the outlier here, for his mind–brain identity theory wasn't a contribution to cognitive science *as such*: it said nothing about mind as machine. Nevertheless, it was eagerly welcomed by scientifically minded readers. Moreover, its materialist spirit—though not its reductionist letter—was retained when mind-as-machine "functionalism" replaced it four years later (Chapter 16.i.d and iii).

The first—and longer—of the two 1956 meetings was the Summer Research Project on AI, held on the beautiful campus of Dartmouth College, New Hampshire. The second was the IEEE's three-day Symposium on Information Theory, convened at MIT in mid-September. (The "EEE" stands for Electronics and Electrical Engineers.) The 1958 meeting was held at the NPL in London—a resonant venue, given its post-war connection with Turing (3.v.c).

This was the mid-1950s, and the intellectual schism described in Chapter 4.ix hadn't yet happened. Each of the three gatherings featured people on both sides of the future divide, and welcomed both styles of contribution. For example, the MIT symposium saw signal detection theory being applied to psychophysics (Tanner and Swets 1954; D. M. Green and Swets 1966) and GOFAI to human problem solving.

The Dartmouth forum is widely regarded as the kick-off point for AI—considered as a communal endeavour, not just a few lone researchers (or BBC talks: Copeland 1999). Indeed, a "Fiftieth Anniversary of AI" meeting is now being planned for 2006 by the AAAI (American Association for AI), perhaps to be followed by a celebratory jaunt to Dartmouth.

The 1956 meeting at MIT, despite its being sponsored by the IEEE, was more significant for psychologists and linguists. But, occurring within such a short time-span, they had a joint effect in launching cognitive science. Hard on their heels in 1958, the London meeting combined AI and psychology with neurophysiology. At that point, cognitive science was truly on its way.

b. A trio of meetings

The beauty of the campus wasn't the prime reason why Dartmouth College was a fitting venue for AI's first meeting. The chairman of the Mathematics Department, from 1955, was the computer pioneer John Kemeny (1915–2001), co-inventor of the programming language BASIC. It was thanks to him that Dartmouth would soon be one of the first places in which students' computer literacy was taken almost as seriously as their 'real' literacy. Kemeny knew intellectual quality when he saw it, having worked on the

Manhattan Project under Richard Feynman, and having been employed for a year while still a graduate student to help Albert Einstein with his maths. (Yes, truly!) Accordingly, it was thanks to him too that, as soon as he had the power to make appointments, the young John McCarthy (1927–) was brought in to teach mathematics there.

And thereby hangs the tale, for the memorable Dartmouth event was initiated by McCarthy—with his ex-Princeton friend Minsky (1927–). The planning was done by them together with Rochester and Shannon. With hindsight (again!), this foursome is impressive.

Shannon was hugely famous already. The young Minsky and McCarthy knew him personally, for he'd given them summer jobs at Bell Labs in 1953. That's why they dared approach him to give respectability to their application for $13,500 from the Rockefeller Foundation. (Soon, all three would be colleagues at MIT: Shannon moved there from Bell Labs in 1958, McCarthy from Dartmouth in 1957, and Minsky from MIT's offshoot Lincoln Laboratory in 1958.)

Shannon's fame was due to his development of information theory, dating from the late 1930s (Chapter 4.v.d). But his role in the Dartmouth planning was grounded in his work on chess and maze-running, and on his insight—which he'd already made explicit (Shannon 1950*b*, 1953)—that digital computers could be used *non*-numerically, as general symbol processors. Despite Shannon's imprimatur, that idea still hadn't filtered through to most people, even those professionally concerned with computing. (Fully thirteen years later, Minsky would still feel it necessary to give a book on AI the title '*Semantic* Information Processing': Chapter 10.iii.a.)

As for Rochester, who'd also employed McCarthy in a summer job, he was familiar to computer scientists for co-designing the first mass-produced general-purpose computer (the IBM-701). He was about to become known to psychologists too, for his model of Hebbian learning—described at the Summer Project as well as in print (see Chapter 5.iv.f). His own particular hope for AI, as expressed in the proposal for funding, was to study "the process of invention or discovery" by asking "How can I make a machine which will exhibit originality in its solution of problems?" (McCarthy *et al.* 1955: 49). (His wide interests endured: much later, he'd encourage IBM to engage in the Media Lab at MIT—Brand 1988: 6; see 13.v.a.)

The two younger men, Minsky and McCarthy, would later be great names in cognitive science, and especially in AI (Chapters 7.i.e, 10, 12.iii). Indeed, McCarthy was already developing the ideas for his seminal paper on 'Programs with Common Sense' (1959), which he'd deliver in London two years later (see below, and 10.i.f). And Minsky distributed drafts of his influential 'Steps Toward Artificial Intelligence' to various Dartmouth visitors (Minsky 1961*b*: 10.i.g).

Dartmouth turned out to be the naming-party for AI. However, in contrast with Christopher Langton's 1987 party for A-Life (15.x.a), the new name wasn't welcomed by every guest there. The event had been announced as "The Dartmouth Summer Research Project on Artificial Intelligence". But there was no little disagreement at the meeting about which term should be adopted for the field: *cybernetics*, *automata studies*, *complex information processing* (the Newell–Simon favourite), *machine intelligence*, or (McCarthy's suggestion) *artificial intelligence*. (The phrase *knowledge-based systems* was dreamt up a quarter-century later, in the mistaken belief that this raises fewer philosophical problems: 11.v.c.)

McCarthy had insisted on using "artificial intelligence" in the meeting's official label partly because the book he'd just co-edited with Shannon, and which the senior author had insisted (against McCarthy's pleading) be called *Automata Studies* (1956), had attracted only highly mathematical papers. There was little or no mention of intelligence, language, game playing, or other psychological issues (McCorduck 1979: 96). His ploy was only partly successful.

On the one hand, the "AI" label did encourage people with interests in computing to think about computational *psychology*—whether human/animal intelligence or (McCarthy's concern) intelligence in general. On the other hand, it raised hackles unnecessarily, implying that AI *must* be seen as what Searle would later call "strong" AI (16.v.c). The seeming philosophical absurdity (to many others besides Searle) of that position deflected attention from the empirical question of just what tasks could, or couldn't, be achieved by computers, and how. So Minsky, asked in 1979 whether he'd had a sense of being at a historical gathering, replied:

> Well, yes and no. There was a false sense [at the Dartmouth meeting] that people were beginning to understand theories of symbolic manipulation and theories of cybernetics which dealt with concepts rather than simple feedback, and that things were going to be understood around the world on a wide scale. I think we had the feeling that these ideas were beginning to become popular, and maybe that's a historic event. It wasn't really true. *It took another ten years* before people could tolerate the idea of AI without thinking that it was funny and impossible. (interview in McCorduck 1979: 98; italics added)

Even then, of course, not everyone could tolerate it: almost exactly ten years after Dartmouth, Hubert Dreyfus (1965) published his first squib attacking the very idea of artificial intelligence (11.ii.a–b and 16.vii.a).

Unlike most scientific meetings, the Dartmouth affair was a long-extended conversation: a two-month Summer School. McCarthy (1989) still prefers to call it a Summer Project, since there was no separation into "lecturers" and "students", and no defined courses. It's often called a Summer School nonetheless, not least because most of those who turned up went there in order to learn something.

No invitation was needed, and many people dropped in and out. Indeed, McCarthy later expressed "great disappointment" that, partly because of this dropping-in-and-out, there was not "as far as I could see, any real exchange of ideas" (McCorduck 1979: 95). To the contrary, he said, "Anybody who was there was pretty stubborn about pursuing the ideas that he had before he came." Consequently, "[The] distance between what I had hoped to accomplish and what we did accomplish . . . was pretty large" (p. 99).

That "anybody" was too strong. Others, such as Bernard Widrow (1929–), remember Dartmouth as a major turning point in their lives (see below). But certainly, the ten core researchers, who were funded to be present throughout (though two played hookey), spent their time describing their own work—and their hopes for its future development.

Two of those core researchers were Newell and Simon, then at RAND Santa Monica and Carnegie Institute of Technology, respectively. Their names hadn't been put on the list by the local organizer, McCarthy (then teaching mathematics at Dartmouth): "I'd never heard of them before. I had no idea that anyone was doing logic with computers" (personal communication). McCarthy's ignorance was shared by others supposedly in the know. McCulloch, for instance, had recently summarized the "Points of Agreement" at the 1955 Macy conference without even mentioning digital computers (von Foerster 1950–5). Although Newell and Simon weren't named in the original "Proposal", they were featured in an Appendix written a few months later, early in 1956 (J. McCarthy, personal communication).

In the event, the two mould-breakers played truant for all but one week of the Dartmouth meeting. They weren't going fishing, but desperately trying to finish programming their Logic Theory Machine. Or rather, they were trying to convert the outline program (verbal instructions written on index cards), which had already been 'acted out' by friends and family members for about a year (Newquist 1994: 53; Simon 1991: 206–7), into a form that would actually run on a computer.

But the truancy paid off. The Logic Theorist, completed at the last minute, provided the only printout of a functioning AI program to be exhibited at Dartmouth (Newell and Simon 1956*b*; Newell *et al.* 1957). Compared with human thought, it was limited in many ways. But it *worked*. (Whether it was, as is often claimed, the *first* working AI program is another matter: see 10.i.b. And whether it had, as its programmers proudly proclaimed, "solved the venerable mind/body problem" is yet another—Simon 1991: 190; see Chapter 16.)

Other core members at Dartmouth included MIT's Ray Solomonoff (9.x.b, 10.i.g), and the AI pioneers Arthur Samuel (10.i.e), Alex Bernstein, and Selfridge—who was already a major influence on Bruner and on Newell and Simon, and also on the neurophysiologist Jerome Lettvin (see Bruner 1983: 99, Section iii.b above, and 12.ii.d). IBM staff were prominent: besides Rochester himself, Samuel and Bernstein were both employed by IBM. Among the visitors were Belmont Farley (12.i.b) and Widrow, each of whom was enthused by the general air of optimism. Widrow resolved on the spot "to dedicate the rest of my life" to AI (12.v.a).

Having been asked for $13,500, the Rockefeller Foundation provided $7,500 (McCarthy 1989). But it was money well spent. Besides the general consciousness raising, specific proposals emerged too.

For instance, Minsky—having seen the Logic Theorist—outlined a possible "geometry machine" (Minsky 1956*b*). While still at the meeting, he got Rochester to persuade an ex-physicist colleague, Herbert Gelernter, to undertake it (J. McCarthy, personal communication). With the advice—and programming skills—of several IBM staff, plus the technical resources of their Poughkeepsie office, it took him three years (Gelernter 1959). In part, this was because he had to design a new programming language, FLPL, in order to do so (10.i.c). This program is seen today as a seminal move in AI problem solving (13.iii.a).

The consciousness raising worked. Only five years after Dartmouth, Minsky would publish a "selected" bibliography on AI covering sixteen large pages—seventy, in the widely read reprint that appeared two years later (Minsky 1961*a*). Admittedly, many items dated from the early cybernetic era. But others were more recent. In short, the effect of this meeting on the budding field of AI, despite McCarthy's "great disappointment", was highly positive.

More to the point, here, Dartmouth also implied the possibility of a computational *psychology*. All four men on the planning panel were mathematicians and/or computer scientists, although Minsky's Junior Fellowship at Harvard was in "Mathematics *and Neurology*". However, they'd already embarked on some psychologically relevant projects (Minsky's is described in Chapter 12.ii.a), and Shannon's work had influenced psychology in various ways, as we've seen. Moreover, their overall agenda, outlined in their application for funding, had a markedly psychological air. They proposed research on machine models of language; goal seeking; intelligence; adaptation to the environment; problem solving; musical composition (eventually); sensori-motor control; learning; imaginativeness and originality; and neural processes (McCarthy *et al.* 1955).

Naturally, psychologists would prick up their ears at this. To be sure, their interest was rather different. Where the mathematicians wanted to show how phenomena *such as* human thought are possible, the psychologists wanted to understand human thought *as such*. In other words, the AI people didn't care if their machines didn't match the human details, whereas the psychologists did. Newell and Simon straddled this fence: besides making a huge contribution to technological AI, they did care about the psychological details. Even in the 1950s they were outlining a theory of *human* problem solving. (Later, they added multifarious empirical data: see Chapter 7.iv.b.)

The difference of emphasis, however, didn't prevent eager communication. At that time, even broad similarities between mind and machine were novel enough to be exciting. Faithful matching could wait. (And *completely* faithful matching is impossible anyway: 7.iii.d.) So Dartmouth, and tales thereof, excited psychologists too.

For psychology, however, the MIT symposium was even more fruitful than Dartmouth. Miller delivered his 'Magical Number Seven' paper there (for the second time: the first had been in April 1955, to the Eastern Psychological Association—Hirst 1988*a*: 71). And he experienced an intellectual epiphany that moved him from mathematical to computational psychology. He soon initiated a spirited declaration of intent for what's now called cognitive science (see subsection c).

In an autobiographical talk given more than twenty years later, he named the second day of the MIT meeting as the time when—for him—things suddenly came together. On that day, talks were given on the Logic Theorist by Newell and Simon and on formal grammars by Chomsky. The first, delivered at Dartmouth only shortly before, showed that a computer can prove theorems in logic. The second showed that language—considered as structured sentences, not just word strings—can be formally described. Miller instantly put the two together:

> I went away from the Symposium with a strong conviction, more intuitive than rational, that human experimental psychology, theoretical linguistics, and computer simulation of cognitive processes were all pieces of a larger whole, and that the future would see progressive elaboration and coordination of their shared concerns. (H. Gardner 1985: 29)

This larger whole, he continued, was cognitive science—in which he'd begun work "before I knew what to call it".

No one else knew what to call it either. At that time, what's now referred to as cognitive science was more likely to be dubbed "the simulation of cognitive processes" (as in RAND's summer 1958 "Research Institute", where Miller, Minsky, Newell, and

Simon all worked together) or "the mechanization of thought processes". Indeed, the latter phrase was chosen as the title of another hugely influential meeting, held two years later.

In November 1958 Uttley (1906–85) organized a select four-day seminar at London's NPL. He himself had worked for some years at the UK's Radar Research Establishment, but his interests went way beyond the technicalities of radar. He'd done pioneering work in symbolic AI (a logic program, for instance: Uttley 1951), in information-theoretic neurology (1954), and in connectionist modelling (1956). So he was eminently well suited to host a seminar that promised to set many intellectual hares running.

And that, it certainly did. What the participants lacked in quantity, they more than made up in quality. The event turned out to be important for both connectionist and symbolic AI, and for computational neuroscience too. Indeed, the list of papers given at this small meeting reads almost like a core course on cognitive science. For among NPL's many memorable moments were these:

* Selfridge first presented Pandemonium (Chapter 12.ii.d).
* Frank Rosenblatt introduced perceptrons (12.ii.e).
* Barlow announced his 'coding' theory of perception (14.iii.b).
* Gregory initiated his cat-among-pigeons critique of brain-ablation studies (14.iii.a).
* MacKay made public his ideas about building 'hybrid' (analogue–digital) machines, previously discussed only privately—with the Ratio members, for instance (4.v.b and 12.ix.b).
* McCarthy proposed that programs could be given "common sense" by expressing everyday knowledge in Russell's predicate calculus, using a programming language later developed as LISP (10.i.f, iii.e, and v.c).
* Minsky talked about heuristic programming, and told the soon-to-be-garbled story of the 'computer-generated' geometry proof (10.i.c). This was the first officially published version of core ideas in his 'Steps' paper (10.i.g), already handed out in draft at Dartmouth.
* Pask described his electrochemical model of a dynamically developing concept (4.v.e).
* And, as a dissenting voice, Yehoshua Bar-Hillel argued that McCarthy's ideas were "half-baked" and philosophically jejune, and subject to what was later called the frame problem (10.i.f and iv.iii).

Not surprisingly, then, this get-together was immediately recognized as an important step in developing theories of mind-as-machine. Instead of falling into a black hole, as so many conference Proceedings do, the two volumes of record—verbatim discussions included—were made generally available very soon (Blake and Uttley 1959).

The atmosphere at the meeting itself was electric (R. L. Gregory, personal communication). It was clear that something exciting was happening. McCulloch, in particular, made a great impression on the meeting as a whole. And Bartlett took a very active part. Colin Cherry "as usual, spoke at literally twice any normal speed, and managed to say practically twice as much as would normally be possible". The Pandemonium paper "created a visible stir". J. Z. Young was "very interesting", and McCarthy added colour to the event, wearing "hyper-Californian clothes with a necklace of big beads". (The young McCarthy was a member of the counter-culture, not least because his father was a Marxist union leader and both he and his brother were communists—Crevier 1993: 37.) As Gregory remembers it, no one realized just how difficult McCarthy's plan to give computers common sense would turn out to be.

Selfridge, too, recalls the NPL meeting as an event where "the general mood was excitement". He says this led him, and no doubt others, to make "preposterous predictions" (personal communication).

The small band of participants included no fewer than twenty-two people featured in various chapters of this book. Besides the thirteen just mentioned, were Ross Ashby, William Grey Walter, Wilfred Taylor, Stafford Beer, Richard Richens, Grace Hopper, N. Stuart Sutherland, Lucien Mehl, and A. J. Angyan.

Several were members of the influential Ratio Club—hardly surprising, since Uttley himself had been a founder member (Chapter 4.viii). Others—such as Young—had spoken there. Young was the first general anatomist to adopt a computational approach (which he would defend, years later, in the Gifford Lectures on natural religion: J. Z. Young 1964, 1978). Indeed, he'd already taken part in discussions with Turing on mind and computers (see 16.ii.a). And Gregory, for whom this was his "first significant meeting", only narrowly missed becoming a member just before it closed down (4.vi.c).

In short, this 1958 meeting brought together many of the older luminaries of the field, and several youngsters destined to become luminaries in their turn. Like its predecessors at Dartmouth and MIT, it was a memorable catalyst for the growth of an intellectual community.

c. The manifesto

If Miller didn't know what to call the new field in the late 1950s, he knew how to envision it. He did so in a hugely influential book, a rousing declaration of intent, or mission statement, for cognitive science: *Plans and the Structure of Behavior* (Miller *et al*. 1960). A good way of judging, today, how far cognitive science has—or hasn't—succeeded is to compare current theories with the hopes expressed there.

The book was written with Eugene Galanter (1924–) and Karl Pribram (1919–), after the three men (MGP for short) had spent 1959–60 together at Stanford's Center for Advanced Study in the Behavioral Sciences. They already knew each other before the trip to Palo Alto, for all three had worked at Stevens's Psycho-Acoustic Laboratory—as had Postman, Garner, and Licklider too. (Pribram had worked also with Lashley.)

Together with another half-dozen visiting psychologists, and faculty dropping in from the Stanford campus, MGP's general remit had been to develop "an experimental and theoretical capability to study natural language and communication" (Galanter 1988: 37). But mind-as-machine was a constant dimension. In the summer months before his arrival, Miller had been at RAND, working on what was then called "cognitive simulation" alongside Minsky, Newell, and Simon. Indeed, when the book appeared its declared goal was to discover "whether the cybernetic ideas have any relevance for psychology" (p. 3).

The idea of writing such a book arose at a conference organized by Miller at the Stanford Center. This occasion has been vividly recalled by Galanter:

> It was a wild affair. The people who participated were mathematical psychologists and empirically oriented experimental psychologists who tore into each other with a viciousness I had rarely witnessed. (1988: 38)

The experimentalists accused the mathematical psychologists of producing mere descriptive statistics, empty trivia seducing bright young graduate students away from real behaviour. The mathematicians, besides saying "this is only the beginning; tomorrow our theories, like Heinz catsup, will be thick and rich", suggested that even human behaviour isn't nearly as complex as it seems: what looked like complexity was merely randomness.

Galanter's—and Miller's—sympathies lay mid-stream. Yes, behavioural complexity was real, and not captured by mathematical psychology. But *theory*, not yet-more-data, was needed. What the theorists there were failing to see was that "there might be other logical and internally consistent modes of theory construction besides the analytical and mathematical" (Galanter 1988: 39)—specifically, *computational* theories. The point of writing *Plans*, then, was to enlarge the theorists' conceptual armoury while doing justice to the richness of behaviour.

Galanter had recently co-published with Stevens on scales of measurement (Stevens and Galanter 1957), and was co-editing the huge *Handbook of Mathematical Psychology* (Luce *et al.* 1963–5). But he wasn't interested only in numbers. Familiar with Turing machines and (tutored by Nelson Goodman) with computational logic, he already suspected that symbolic computation might be a way of describing the structure of behaviour. In addition, he'd recently published a plea for a "synthesis" of S–R and model-based (cognitive) theories in psychology (Galanter and Gerstenhaber 1956).

As for Pribram, he was a controversial neuroscientist who'd recently suggested (see 14.ii.b) that vision involved dedicated brain areas higher than striate cortex (and who would later offer a holographic account of memory: 12.v.c). He'd argued in 1957 that extreme behaviourism needed leavening by concepts derived from introspection (G. A. Miller *et al.* 1960: 212).

So both those men had something to contribute. But Miller was given most of the credit. I remember a quip going the rounds of Harvard's Emerson Hall in 1962: "Miller thought of it, Galanter wrote it, and Pribram believed it."

This was the Chinese-whispered version of George Mandler's remark "Miller wrote it, Galanter takes credit for it, and Pribram believes it." ("Others", says Mandler, "have claimed this invention as their own and I want, once and for all, to establish copyright and priority rights"—Mandler 2002*c*: 170.) Both versions, you'll have noticed, had Pribram believing it. This was amusing partly because Pribram held highly maverick views on other matters, and had attracted mockery accordingly (see 12.v.c and 14.ii.b, and cf. 16.x.a). Indeed, he was described as "crazy Karl Pribram, the *prima donna* of physiological psychology" (Walter Weimer, interview in Baars 1986: 309). Mostly, however, it was amusing because *no one* could seriously believe it.

The book was vague, simplistic, often careless. It had to be taken not with a pinch of salt, but barrelfuls. It presented the mind, *all* aspects of *every* mind, as a tote bag of TOTE units (see below). Psychology was assimilated to GOFAI, with parallelism acknowledged in two footnotes (pp. 50 and 198). And it was hopefully—or hopelessly?—ambitious. Animals and humans; instinct and learning; language and memory; habit and motor skill; chess and choice; values and facts; self-image and social role; knowledge and affect; intention and desire; hope and morality; personality and hypnosis; normal life and psychopathology (Miller's initial interest as a student) . . . *everything* was included. Even during my exhilarating ten-minute introduction to it (see Preface, ii), I was almost deafened by the sound of hand-waving.

The draft manuscript had been even more provocative:

> We sent our first draft, complete with no citations, to our friends and colleagues. They hated it. Where were the footnotes? What were the references to X's (for X read "my") papers? Where were the data in support of such dream work? It was depressing as, each day, a new fusillade of criticism and derision arrived. Karl and I were for damning the torpedoes and publishing at once. George (thankfully) displayed a cooler hand.
>
> "We will get the footnotes", he said.
>
> We moaned and went about the task. This period of grace also allowed us to clean up some[!] of the more egregious proposals and to lay in some more experimental-sounding text . . . [It was a] dark journey of the soul. (Galanter 1988: 42)

As remarked in Chapter 1.iii.g, the "X" above includes Newell and Simon. Alongside Hovland, Roger Shepard, and Edward Feigenbaum, they'd spent much of the summer of 1958 doing computer simulation with Miller, at RAND. But they felt that they'd taught him more than they really had. Miller recalls the unpleasantness thus:

> We showed [our draft] to Newell and Simon, who hated it. So I rewrote it, toned it down, and put some scholarship into it . . .
>
> Newell and Simon felt that we had stolen their ideas and not gotten them right. It was a very emotional thing . . . I had to put the scholarship into the book, so they would no longer claim that those were their ideas. As far as I was concerned they were old familiar ideas; the fact that they had thought of it for themselves didn't mean that nobody ever thought of it before. (G. A. Miller 1986: 213)

This fits in, of course, with the suggestion in Chapter 5.i.b, that the cognitive revolution was in fact a counter-revolution. The ideas beneath the mantle were being resuscitated.

Despite all that, the book was a revelation. It was unremittingly non-behaviourist. Its opposition was up-front and explicit:

> As for the stimulus–response business, it seemed the right approach for quite a long time. The limits were not really explicit until you got to Miller, Galanter, and Pribram. They made it explicit that stimulus–response really wasn't the right unit. (Ernest Hilgard, interviewed in Baars 1986: 298)

In addition, *Plans* brought *purpose* and *meaning* into the heart of psychology, in a way that promised (*sic*) precise formulation and rigorous testing.

Where S–R psychology had focused on the S and the R, *Plans* focused rather on the hyphen. It took Dewey, Tolman, Bartlett, Craik, and Lashley very seriously. Freud, the Gestaltists, and the ethologists were acknowledged too. It drew from Turing, Shannon, and McCulloch and Pitts—saluting the schematic sowbug and its mechanical cousins along the way (5.iii.c). Among MGP's contemporaries, it highlighted Bruner, Newell and Simon, and Chomsky (both his generative grammar and his critique of Skinner). And it offered a full chapter of neurophysiological speculation.

The core ideas—feedback, internal model, programs, hierarchy, and the flow of control—were drawn from both sides of the emerging cybernetic/GOFAI divide. A few brief quotes will convey the flavour of MGP's approach:

A human being—and probably other animals as well—builds up an internal representation, a model of the universe, a schema, a simulacrum, a cognitive map, an image. (p. 7)

[Our book tries] to describe how actions are controlled by an organism's internal representation of its universe. (p. 12)

The Image is all the accumulated, organized knowledge that the organism has about itself and its world. (p. 17)

A Plan is any hierarchical process in the organism that can control the order in which a sequence of operations is to be performed.

A plan is, for an organism, essentially the same as a program for a computer, especially if the program has [a] hierarchical character . . . (p. 16).

The organizing, or planning, operations in memorization are perhaps easier to see and recognize when the material to be memorized consists of *meaningful* discourse, rather than nonsense syllables, mazes, etc. (p. 133; italics added)

A fact not much recognized at the time—nor later, for that matter (13.iii.b)—was that MGP allowed that one might sometimes want to think of the Plan as partly contained in the environment, rather than residing entirely within the individual organism. That would be so when the observed behaviour seems to be integrated by an appropriate succession of environmental stimuli. An excellent example, though not one quoted by them, had recently been discovered—namely the courtship and nesting behaviour of ring doves (Lehrman 1955, 1958*a*,*b*). Examples in human beings included behaviour prompted by semi-permanent cultural signs (such as traffic lights: see Chapter 8.i.a) or transient jottings on pieces of paper.

But although they acknowledged that this approach wasn't unreasonable, MGP chose not to adopt it:

It is almost as if the Plan were not in the organism alone, *but in the total constellation of organism and environment together*. How far one is willing to extend the concept of a Plan beyond the boundaries of an organism seems to be *a matter of metaphysical predilections*. We shall try to confine our use of the term to Plans that either are, have been, or could be *known to the organism*, so that we shall not speak of [environmentally] concatenated behavior as part of a Plan even when it is highly adaptive. (p. 78; italics added)

One way in which the book was "careless" was that this metaphysical predilection seemed to confine Plans to *Homo sapiens*. Yet MGP had also applied it, helpfully, to instinctive behaviour in animals—speaking of inborn Plans, such as the IRMs and FAPs of the ethologists (5.ii.c).

(If they'd chosen the alternative meaning given above, the history of cognitive science might have been very different. For late-century work on situationism, and on cognitive technologies, would make a point of rejecting MGP-friendly psychology and philosophy as overly rationalist, individualist, and internalist: see Chapters 7.iv.a and g, 13.iii.b–e, and 16.vii.d.)

Another example of MGP's carelessness was that the core concept of Image, introduced (with its dignifying capital letter) in the opening sentence, was largely ignored.

Internal models, or representations, yes—but not the Image as a whole. It was declared to be 75 per cent of the human mind (the other 25 per cent being Plans), and to include not only our background knowledge but our values, so all our hopes and fears. Yet, in MGP's pages, values (motives, affect, and culture) were taken for granted: how they arose, or how they might be changed, wasn't considered. As Miller admitted:

> [The Image concept] was a sort of intellectual wastebasket for us into which (it seemed to me) we tossed all the problems that did not fit easily into the hierarchical plan structure . . . But as soon as you want to *do* anything with our ideas, of course, you must decide what a "test" is, and that can only be done if you know what part of the Image is to be tested against this input. Two ideas about it that I wish we had made more explicit in the book (but perhaps we didn't understand them then!) are: (1) some "operate" units serve merely to modify the Image and so to change what the "test" will find acceptable, and (2) it must be possible to *generate* parts of the Image as needed, since it is too detailed to have been all stored away in memory in advance. This opens up a huge spectrum of speculations, of course. (letter from G.A.M. to M.A.B., 27 April 1964)

Memory received a whole chapter in *Plans*. MGP regarded Ebbinghaus's deliberately *meaning-less* experimental paradigm of nonsense syllables (Chapter 2.x.a) as irrelevant to memory in real life. Bartlett had done so too, but he'd said very little about *how* memory schemas are formed. MGP discussed subjects' spontaneous use of mnemonics in terms of Miller's "Magical Number Seven". Such memory aids, they said, effectively shorten the length of the list to be memorized, by converting it into a hierarchical structure having only a few information-rich chunks on each level (pp. 130–6). Short-term memory was compared to a computer's input buffer, and retrieval from long-term memory was described as heavily dependent on language. And the notion of "working memory" was highlighted for the first time, implying a distinction between long-term *storage* and short-term *processing*.

Language too, "the very lifeblood of the thought processes" (Section i.b, above), was discussed at length. The central topic wasn't vocabulary, nor even MT, but Chomsky's grammar—here introduced to most of MGP's readers for the first time.

As for *the* central theoretical concept, the TOTE unit, this had been provided by Miller (Galanter 1988: 40). It was similar to Broadbent's flow diagrams, except that what 'flowed' between the boxes was not information but control. Miller explicitly intended the TOTE unit as a replacement for the S–R reflex:

> [The] fundamental building block of the nervous system is the feedback loop. (pp. 26–7)

> Obviously, the reflex is not the unit we should use as the element of behavior: the unit should be the feedback loop itself. [This can be defined as] the Test-Operate-Test-Exit unit—for convenience, we shall call it a TOTE unit . . . (p. 27)

> The TOTE represents the basic pattern in which our Plans are cast, the test phase of the TOTE involves the specification of whatever knowledge is necessary for the comparison that is to be made, and the operational phase represents what the organism does about it—and what the organism does may often [though it need not] involve overt, observable actions. (p. 31)

The most important difference between TOTEs and S–R reflexes was that TOTEs can be hierarchically structured:

> A central notion [of our book] is that the operational components of TOTE units may themselves be TOTE units . . . Thus the operational phase of a higher-order TOTE might itself consist of a

string of other TOTE units, and each of these, in turn, may contain still other strings of TOTEs, and so on. (p. 32)

MGP gave the example of hammering a nail: a *sequence* of observable actions, whose inner structure is defined—and controlled—by a holistic *hierarchical* Plan. This involves Tests such as looking at (or feeling) the nail, to see whether it's flush with the wood, and Operations such as raising and lowering the hammer (see Figure 6.4).

How we hammer nails, of course, isn't an enthralling topic. But MGP generalized the many-levelled hierarchy of TOTEs to *all* thinking and behaviour. Although not all behaviour is (as we say) planned, it *is* (as they said) all Planned. Even a 'reflex' knee jerk is generated and controlled by a (very simple) Plan.

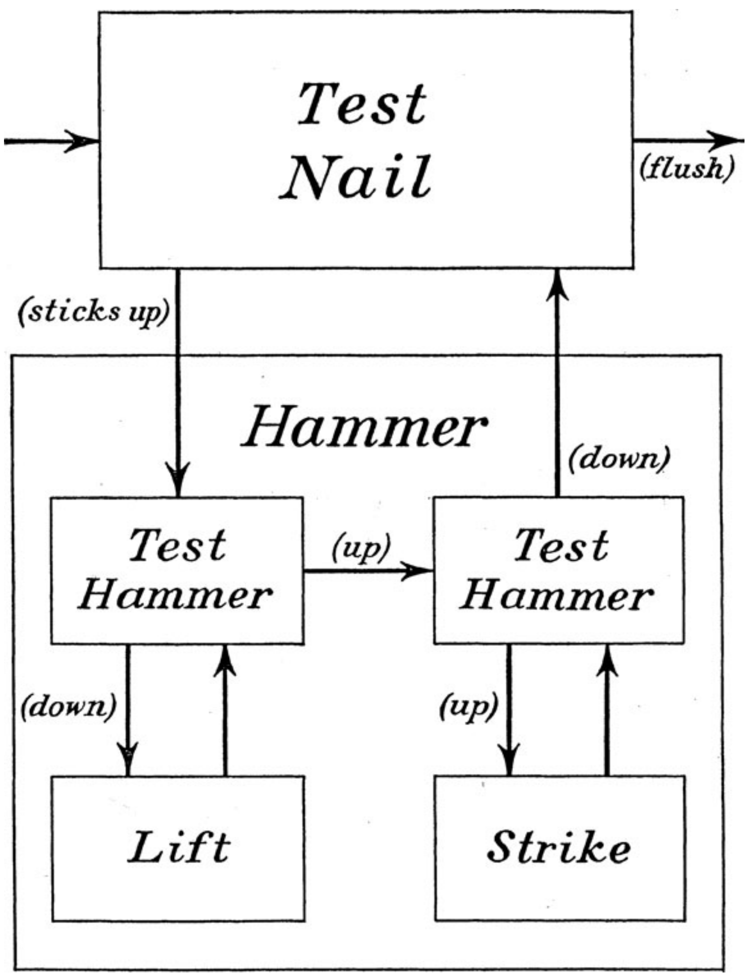


FIG. 6.4. The hierarchical Plan, or TOTE unit, for hammering nails. Reprinted with permission from G. A. Miller *et al.* (1960: 36)

Hypnosis, they suggested, involves "relinquishing" one's own Plans and adopting those of someone else (1960, ch. 8). Many personal idiosyncrasies depend not on different knowledge or motives, but on differences in the way individuals organize and manage their Plans (ch. 9). Various sorts of psychopathology, including neurosis and depression, result from a faulty integration of one person's Plans, whereas cooperation requires the socially integrated sharing of Plans (chs. 7–8). Memory and language involve hierarchical schemas, or rules, and so does problem solving (chs. 10 and 12–15). And whereas some Plans are inherited (instincts), others—such as habits, motor skills, and social roles—are learnt (chs. 5–6, 4–15).

All of this, MGP insisted, applies to human beings in general. Even very basic cultural differences, such as are sometimes described by anthropologists, didn't undermine their theory:

> Planning is a function we have seen span whatever boundaries separate Americans from the digger wasp and the computing machine. It is not the fact that Americans plan that makes them different from their fellow men. It is the way Americans plan . . . that distinguish[es] them from men who live in other cultures. *In its broader aspects, we would argue, planning is not an American idiosyncrasy. It is an indispensable aspect of the human mind.* (102; italics added)

Plans aroused great interest, earning praise from Hebb in his high-visibility address as President of the APA: "[MGP have provided] a fundamentally important line of analysis . . . [and] promise a new approach to some major problems of human behavior" (Hebb 1960: 744). It helped, of course, that Miller was already highly respected. The jokey quips withal, his book couldn't simply be shrugged off.

Or rather, it couldn't be shrugged off by anyone who understood what it was getting at. In 1960 not everyone did. For instance, the ecological psychologist/philosopher Walter Weimer, despite his growing scepticism about behaviourism, dismissed it as unintelligible:

> If you go back to 1960 there's the infamous Miller, Galanter, and Pribram book, *Plans and the Structure of Behavior*. A funny book. A lot of us bought it, looked at it, and said, What's this? We couldn't make head or tail of it. So we threw it on the shelf and never came back to it until *after* the revolution. (interview in Baars 1986: 309)

It didn't help matters that, as Weimer records, all three authors were seen as "oddballs" in psychology. Even Miller, well-respected because of 'The Magical Number Seven', was "a little suspect among the experimentalists. . . . basically a good guy, but sort of way out" (ibid.).

Another reason for being interested was that various thinkers from the past, even including Dewey, had been resurrected. Yet another, that many readers were intrigued by the new research cited by MGP: Chomsky's grammar and Newell and Simon's GOFAI planning. Further, people already sympathetic to cybernetics and/or GOFAI (remember: this was pre-schism) would at least be interested in, even if they weren't fully convinced by, any serious attempt to apply feedback and programs to *the entire range* of psychology.

Despite their talk of programs, however, MGP didn't realize just how useful computer modelling could be for psychology. Galanter, again:

> What we clearly failed to recognize at the time was the over-riding importance of the computer as an experimental device. We did not catch its signal potential contribution to cognitive science. This machine, with its ability to control complicated input–output relations, would open the way to new and complex kinds of behavioral studies. (1988: 42)

In 1960, of course, ''complicated input–output relations'' couldn't yet be modelled. There were fewer than a dozen AI programs of specifically *psychological* interest. Although MGP's visionary hand-waving could have been more careful, it couldn't—at that time—have been much more precise. In sum: for all its faults, *Plans* was the right book at the right time.

d. The first mission station

Simultaneously with the appearance of *Plans*, cognitive science got its first dedicated research centre, at Harvard. (It grew out of an enterprise that had already been bustling along for ten years, as we'll see.)

Granted, the first AI labs had been started a few years earlier at Carnegie Mellon and MIT (see Chapter 10.ii.a). And the CMU version, thanks to Newell and Simon, was markedly psychological in spirit. But the potential scope of cognitive science as a whole was better represented at Harvard. Most unusually, the invisible Chinese walls that normally separate academic disciplines were being deliberately ignored.

One of the two people responsible was the senior partner in the MGP trio. The other was Bruner. On Miller's return to Harvard from the Stanford think-tank, he and Bruner applied to the Carnegie Foundation for ten-year funding to set up the Center for Cognitive Studies.

Carnegie obliged, and the Harvard centre was established in 1960. A separate grant proposal soon earned them an in-house PDP-4 computer, perhaps only the second computer ever to be set aside for psychologists' use (D. R. Norman and Levelt 1988: 104). Intriguingly, the house they were assigned was one that had been lived in by the family of William James (G. A. Miller 1986: 214): whether this was more inspiring than inhibiting, who knows? In retrospect, Miller wrote:

> I think Jerry and I made a good pair . . . We shared a vision of cognitive psychology, but our intellectual playmates were very different. I gave him access to ideas growing out of communication theory, computation, and linguistics, whereas he gave me access to ideas from social psychology, developmental psychology, and anthropology. He broadened my view of cognition; I hope I helped to sharpen his. (quoted in Bruner 1983: 123)

The Center was a Solomon's House (2.ii.b) whose inhabitants and visitors were drawn from many fields. These included various areas of psychology, from 'respectable' psychophysics to developmental psychology—then still far from fashionable. Bruner himself had been deeply influenced by Piaget, who featured prominently on his reading lists—and in his own developmental writings, which helped to 'cognitivize' developmental psychology around the world (Bruner 1964, 1966*a*; cf. Chapter 5.ii.c).

The other fields were primarily linguistics and anthropology, though AI and philosophy were included too. History, art history, and sociology could be found in the visitor fringe. The common interest was in "the distinctively human forms of gaining, storing, transforming, and using knowledge of all sorts—what makes humans human" (Bruner 1983: 122).

These "cognitive processes", Bruner recalled later, "were certainly being neglected—particularly at Harvard". Quite a few psychologists felt thoroughly uneasy with the terminology. In fact, cognition—so Mandler remembers—was "a dirty word" among his own American colleagues. Cognitive psychologists were seen as "fuzzy, hand-waving, imprecise people who really never did anything that was testable" (G. Mandler 1986: 254).

The key principle in inviting visitors was interdisciplinarity:

> What [George Miller and I] had decided was that *psychology was too complicated a field to be left to the psychologists*, and that what we needed was an alliance with colleagues in other disciplines who were, each in his or her own context, concerned about how humans used and acquired knowledge. (Bruner 1988: 92; italics added)

The visitors invited to the Center were imaginatively chosen. For example, those I worked with while I was a graduate student there included Kolers, Goodnow, Roger Brown, Brendan Maher, Barbel Inhelder, Margaret Donaldson, Philip Stone, Ernst Gombrich, and Eric Hobsbawm—the last two, prominent scholars in art history and social–political history, respectively.

Others who were sometimes around (though I didn't have the opportunity to work with them) included the philosopher Nelson Goodman (1906–98). He was a research fellow at the Center in 1962–3, preparing the published version of his 1962 John Locke Lectures on cognition and aesthetics (Goodman 1968). Goodman's presence exemplified the reciprocal successes of interdisciplinarity that Bruner and Miller were trying to foster. For his influence on Bruner's thinking was considerable—and, apparently, vice versa:

> J.B.: *[One of the things] I'd like to be remembered for* is as another one of the guys among a long series of them [notably Bartlett: see 5.ii.b] who made an effort to reintroduce meaning into psychology. I intend meaning-making as a process. [I called my recent] book *Acts of Meaning*, because it made meaning into something that you have to get. This is something I found very sympathetically expressed first by Nelson Goodman.
>
> B.S.: What's your genealogy in terms of this focus on meaning-making. Where did it come from?
>
> J.B.: I think from Nelson. And oddly enough, when he was interviewed about his influences recently [i.e. in the 1990s], Nelson Goodman gave me as one of the important factors that led him along this line. (J. S. Bruner, in Shore 2004: 100; italics added)

Even Bruner and Miller themselves were often seen as coming from different disciplines. For they were located in two recently separated departments, with little love lost between them: Social Relations and Experimental Psychology. The former covered social and clinical psychology, anthropology, and sociology; the latter, psychophysics and behaviourism.

Indeed, the interdisciplinarity went even further than that. For many 'self-invited' visitors turned up at the Center's weekly colloquium, including physicists, historians (such as Stuart Hughes and John Fairbank), and lawyers (such as Paul Freund). Bruner saw this as hugely important for psychology as a discipline, not just at Harvard but in the intellectual world as such:

> Up to that point (after the James days) psychology had been (and [in 2004] it still is in many places) an oddly isolated field. I think the Center (with its emphasis of "knowledge acquisition and organization") had the effect of remaking psychology into a part of the university at large—and of the American university world at large. (J. S. Bruner, personal communication)

Bearing in mind the non-humanism, not to say anti-humanism, of the behaviourists who dominated academic psychology at the time, this isn't too surprising. However, as we saw in Section i.d, intellectual counter-currents would soon form which would undermine attempts to foster an experimentally based humanism.

One might say that the Center was born already 8 years old. For it had been anticipated by Bruner's Cognition Project, founded in 1952. The Project had set its stamp on the nascent cognitive science: not just psychology, but linguistics and philosophy too. Chomsky had been a visitor there for a year, and Jerry Fodor had attended many seminars—indeed, both were deeply influenced by it. Chomsky would soon borrow Bruner's ideas about perceptual/conceptual hypotheses, in his account of language acquisition (Chapter 9.vii.d). As for Fodor, he would later defend non-behaviourist psychology, and draw on Bruner's study of concept learning in his notorious *Language of Thought* (Fodor 1968, 1975; see Chapter 16.iii.b).

Alongside the Project, considered as a research centre, Bruner and Miller had been doing subversive undergraduate teaching for several years:

> [We] collaborated in teaching the (in)famous Psychology 148—The Cognitive Processes. It lured an astonishing number of bright kids into the field. (J. S. Bruner, personal communication)

Their collaboration didn't stop there. Bruner said later, "I have no doubt (1) that there would have been no Center without George Miller" (1988: 90).

However, he immediately added his conviction "(2) that George and the other actors in the events surrounding its establishment were corks on the waves of history". And the most powerful roller of them all, according to Donald Norman (an early visitor), was anti-Newtonianism:

> [The] Center at Harvard was not set up to be *for* anything in particular; it was set up to be *against* things. What was important was what it was not: not psychophysics (at the time, a major, mainstream activity in psychology), not animal studies, certainly not Skinner's operant psychology (whose world headquarters were just down the street). Basically, not contemporary American 1950s psychology. Late 1800s or early 1900s psychology, perhaps, but certainly not the contemporary American psychology of the 1950s. The enemy was the present. (Norman and Levelt 1988: 101)

In Europe, "the present" was of course very different. (We've already noted Mandler's remark, that behaviourism was "a *very* parochial event": 5.i.b.) Willem Levelt, on his arrival at the Center from the Netherlands (and studying with Albert Michotte), was familiar with the sitting tenants discussed in Chapter 5.ii—which his American peers were not. But, he recalled later, "behaviorism had been so completely absent from my horizon that I didn't even know the difference between classical and operant conditioning" (Norman and Levelt 1988: 102).

Similarly in Britain. When Miller gave his anti-behaviourist lecture on a visit to the UK in 1963, his host said, "What was all that about behaviorism? You know, there are only three behaviorists in England, and none of them were here today!" (G. A. Miller 1986: 212). That was a joke, of course. But it had a firm grounding:

> The one road which [when behaviourist ideas were overwhelming psychology in the USA] no British psychologist of standing took was the road of behaviourism . . . Most British philosophers and psychologists between the wars would have agreed with C. D. Broad's judgment that behaviourism was simply a "silly" theory with no claim to be taken seriously. (Hearnshaw 1964: 210)
>
> [Above] all British psychology was largely shaped by McDougall . . . (p. 212)
>
> From among contemporary foreign schools of thought a good deal was borrowed from psychoanalysis and something from Gestalt psychology. . . . The more extreme movements, American behaviourism on the one hand and German phenomenological schools on the other, made little headway in Great Britain. (p. 213)
>
> By 1940 [there was] a wide divergence between British and American psychology. (p. 215)
>
> In America the importance of the conditioned reflex was widely appreciated, and it became one of the main planks of behaviourism; in Britain it was not regarded as much more than a minor curiosity. (p. 216)

To be sure, by the late 1950s the conditioned reflex was no longer "a minor curiosity" in Cambridge: it was sufficiently prominent to dissuade me from my plan to study psychology there (see Preface, ii). Even so, the new Center was more shocking—and more courageous—in Cambridge Mass. than it would have been in Cambridge England.

That "would have been" is important. Although many, even most, European psychologists were *already* cognitivists, the Bruner–Miller enterprise had no 'twin' outside the USA. The nearest equivalent was the apple-orchard CLRU, the most visible the MRC's Applied Psychology Unit—both in Cambridge (Preface, ii, and Section ii, above).

What was distinctive about the Center wasn't only that it was cognitivist, but that it was deliberately interdisciplinary. As a result of their experiences during the war, many denizens of the CLRU and the APU were accustomed to interdisciplinarity. But that was primarily for practical reasons: if one was interested in how pilots or gunners use their high-tech instruments, one had better consult some communications engineers as well as psychologists. The Harvard Center, by contrast, valued interdisciplinarity in (all) the human sciences for its own sake. For without it, the human mind would never be properly understood.

The attack on the behaviourist "enemy" was reflected in the Center's name. Miller has remarked:

> To me, even as late as 1960, using "cognitive" was an act of defiance. It was less outrageous for Jerry, of course; social psychologists were never swept away by behaviorism the way experimental psychologists had been. But for someone raised to respect reductionist science, "cognitive psychology" made a definite statement. It meant that I was interested in the mind—I came out of the closet. (quoted in Hirst 1988*a*: 89)

So the "cognitive" was a defiant rejection of behaviourism. Very soon, Miller's introductory book *Psychology* (1962) barred the door against the behaviourists even more firmly. It was subtitled *The Science of Mental Life* (a phrase taken from the first sentence of William James 1890), and—shock, horror!—it paid homage to several of the sitting tenants mentioned in Chapter 5.ii.

Almost twenty years later, Miller would found a second interdisciplinary centre, the Cognitive Neuroscience Institute at Rockefeller University. Although he'd been interested in neuroscience in the 1950s (hence the P in MGP), he'd had little knowledge of clinical neurology. It wasn't until the late 1970s that he realized the importance of detailed clinical data for psychological theory in general (14.x.b).

By that time, the Harvard Center no longer existed. Not because its task was completed, or unimportant, but because of a lack of imagination and an excess of red tape. In Bruner's words:

In the end, we closed shop. The deans had never been very keen about a Center after McGeorge Bundy [who'd enabled us to found the Center] left. We were self-supporting, *but a center such as ours is messy to a good administrator*. It duplicates the power of departments. Bundy, like John Gardner, our first Medici, had been out to reduce the power of departments. After we [presumably Bundy and Bruner, though maybe Miller and Bruner] left, the Center ceased to exist. Nobody ever asked whether that was a good idea. It just happened. The dean said it should. (Bruner 1983: 126; italics added)

The Chinese walls had got their revenge. (A quarter-century later, much the same happened, for much the same reasons, to the School of Cognitive Sciences at Sussex: see below.)

If the Center hadn't closed down, its emphasis might have changed. For both founders eventually altered their intellectual priorities. Miller turned to neuroscience, founding the first institute of cognitive neuroscience in the late 1970s (Chapter 14.x.b). And Bruner, as we've seen, moved away from computation to interpretation, and from pure psychology to psychological aspects of law and anthropology.

Nevertheless, the original vision hadn't been abandoned by either of them. Miller still acknowledged his "enormous debt to people like Simon, Newell, and Minsky" (1983*a*: 57). As for Bruner, he still allows that "the study of man" requires psychologists, anthropologists, neurophysiologists, and "computational scientists" too (1996, p. xiii). And he still sees Miller, whose current interests are very different from his own, as a hugely important colleague:

George Miller is [somebody who's close by my desk, in that I always ask myself, "What would he think about that?"]. George is particularly valuable because we know that I know that he knows that I know that we disagree very deeply, but if I can make my point to him and he can say, "I understand what you are saying but I disagree", then that's okay. (interviewed in Shore 2004: 6)

Moreover, he sees an essential continuity between his late 1950s formal (computational) work and his current emphasis on narrative, as the core of humanistic interpretation:

B.S.: The old classical theory of categories [assumed that] distinctive features converge into a well-formed category based on a kind of magnetic attraction.

J.B.: Yes. That's the old Bruner–Goodnow–Austin view in *A Study of Thinking*. At least it characterizes the first half of the book, but the second half had to do with what we called thematic categories. You should see the amount of waffling we did. I mean, it is shameful. I re-read that section twenty-five years later and realized the extent to which I simply didn't pay attention or have the capacity or the courage to pursue what it meant that categories formed

> thematically. Out of what kind of themes, Bruner? So I come back twenty-five years later and I re-discover the obvious, that most people think in terms of stories about what happened and blah blah blah. And so they form their categories. (Shore 2004: 147)

(Part of that "blah blah blah" is carried by the "themes" and "scripts", and the insights into the general structure of motivation, intention, and cooperation, that were analysed formally—though crudely—by Robert Abelson in the late 1960s and early 1970s: see Chapter 7.i.c.) In light of the charge of intellectual "betrayal" mentioned in Section ii.c (above), this insistence on continuity is interesting.

e. Missionary outposts

On the other side of the Atlantic, the pioneering CLRU still continued. This had been founded in the mid-1950s, but it wasn't *psychologically* oriented (see Preface, ii, and Chapter 9.x.a). Except for Gordon Pask, its 'take' was linguistic and/or technological.

A halfway-house between CLRU and Bruner's Center was formed in the 1960s at the University of Edinburgh. Whereas the Harvard group, its precious PDP-4 notwithstanding, was focusing above all on psychology, the group in Scotland was at least equally concerned with AI.

Donald Michie (1923–), then Reader in Surgical Science, had been speculating about what would later be called AI since the early 1940s. As a cryptographer at Bletchley during the war (see 3.v.d), he—with fellow code-breakers Turing and Jack Good—had formed "a sort of discussion club focused around Turing's astonishing 'child machine' concept", that is, "a teachable intelligent machine" (Michie 2002). "It gripped me," he now recalls, and "I resolved to make artificial intelligence my life as soon as it became feasible."

Feasibility, of course, was some years in the making. In 1948, still in close contact with Turing, he was writing a paper-and-pencil chess program in his spare time, while spending most of his time on genetics. These split intellectual loyalties continued through the 1950s.

Michie's hopes for AI were well known by his Edinburgh colleagues, most of whom were pretty sceptical. In 1959, when word was spreading about Uttley's epochal NPL meeting, one of them told Michie that learning machines were "impossible"—and challenged him to prove him wrong. That was a red rag to a bull. Since Edinburgh, to Michie's disgust, still lacked an equivalent of ACE or MADM or EDSAC (3.v.b), he built "a contraption of matchboxes and glass beads" called MENACE (the Matchbox Educable Noughts And Crosses Engine). MENACE not only won the bet, but also won him an invitation to Stanford—where he wrote a reinforcement-learning program based on a task that Widrow (12.ii.g) had set to one of his Adaline students (Michie and Chambers 1968).

On his return from the USA, he "lobbied everyone in sight" to overcome "the national computer-blindness" (2002: 3). Even the Minister of Science and Education knew nothing of the Enigma-busting triumphs of Colossus (3.v.d), and thought that "computing" meant "desk calculators". Michie, of course, couldn't enlighten him about the still-secret Colossus. But, as he often admitted, he still carried the Bletchley spirit of *Get the job done, and damn the bureaucrats!* So ignorance, even opposition, from those on high was less of an obstacle to him than one might imagine. During this relentless lobbying, Michie persuaded the Royal Society to provide "a few hundred pounds" to enable him (with Bernard Meltzer)—to set up a small research group in 1963.

This was made official as Edinburgh's Experimental Programming Unit in 1965. In 1966 it became the Department of Machine Intelligence and Perception, whose intellectual focus was much wider. It was devoted to research on "integrated cognitive systems", whose "intellectual" attributes were grounded in "sensorimotor and reflex capabilities" (Michie 1970: 75). (Meltzer now headed the Metamathematics Unit, specializing in the automation of maths—whose students would include Patrick Hayes and Robert Kowalski.)

The "and Perception" in the Department's title was due to Gregory, who left Cambridge to be one of Michie's leading triumvirate at Edinburgh. He headed their new Bionics Group, where he worked on machine vision for robotics as well as on visual illusions (Section ii.d). The robotics caused a hiccup in the Department's search for accommodation:

> Initially we were offered a deconsecrated Church of Scotland church for our laboratory and offices; but when they heard we were going to build a robot, it was withdrawn! (Gregory 2001: 389)

(This experience put them in the same honourable league as Jacques de Vaucanson, thrown out of his seminary for building automatic flying angels: Chapter 2.iv.a.)

The third member of the triumvirate was H. Christopher Longuet-Higgins (1923–2004). He was a highly distinguished theoretical chemist (many colleagues feel that he was unlucky not to have got the Nobel Prize) who had recently turned to psychology. (He ended up being almost as distinguished there: see Chapters 2.iv.c, 7.v.d, and 12.v.c.) He, too, had left Cambridge for Edinburgh.

At one level, Michie's Department was a resounding success:

* It pioneered symbolic AI in the UK (connectionist AI had started even earlier: see Chapter 12.ii).
* Robin Popplestone, who became the fourth staff member in 1965, wrote a new-style programming language (POP2) that was hugely important for British AI research (10.v.c).
* The Edinburgh hand–eye robot FREDDY, designed to assemble a toy boat and car from a few pieces dumped together on a table, raised some important issues with regard to vision and robotics in general (10.ii.a, 11.iv.a).
* And Michie's intellectual breadth encouraged AI ventures outside the usual limits, such as James Doran's pioneering work on AI agents (Doran 1968*a*) and his applications of AI to archaeology (Doran 1968*b*, 1970, 1977; Doran and Hodson 1975; cf. Doran and Palmer 1995).
* Psychologically relevant early research included Longuet-Higgins's analyses of associative memories (12.v.c)
* and musical perception (Longuet-Higgins and Steedman 1971);
* Gregory's (1967) argument that seeing-machines would necessarily suffer illusions (Section ii.d, above);
* and Sylvia Weir's neo-Gestaltist study of the perception of 'meaningless' geometrical figures in social/personal terms (Weir 1974; Weir *et al.* 1975).

More generally, Michie's group raised the visibility of computer modelling. On the one hand, he founded the influential—and still continuing—Machine Intelligence Workshops in 1965. These drew participants from across the world. On the other hand, his Department trained many year-long visitors in AI techniques. I was invited to go there in the late 1960s for that purpose, but a new baby took priority. A few years later, my Sussex-philosophy colleague Aaron Sloman did so, and soon became a highly imaginative leader of AI in the UK (see Chapters 7.i.f, 10.iv.b, 12.v.h, and 16.ix.c). Other people who cut their teeth at Edinburgh in the early days included Longuet-Higgins's student Geoffrey Hinton (12.v.h).

Without Michie's energy and commitment, none of that would have happened. However, he wasn't an easy man to work with. The triumvirate soon splintered, as it became clear that three vibrant egos couldn't exist in the same small unit. In 1970 Gregory moved to Bristol, where he founded the Brain and Perception Laboratory, and in 1974 Longuet-Higgins (soon followed by Hinton) went to Sussex as a Royal Society Research Professor, working alongside Sutherland and Uttley. Arguably, this split—although painful at the time—helped the development of British cognitive science. For it dispersed a combination of psychological interest and AI expertise around the country.

The effect in Edinburgh, however, was to reduce the prominence of psychological AI. Increasingly, Michie's group—administratively restructured several times—moved away from psychology towards technology. (Its work on speech perception was an exception: although the prime aim was to produce useful tools, this required extensive attention to *real* speech.)

Ironically, Michie—having founded AI research in the UK—eventually led to the worst assault it has suffered over the years (Chapter 11.iv and v.c). His over-optimistic predictions about AI applications, and his often abrasive personality, irritated many of his scientific colleagues. In 1971 his main sponsors, hearing of "the high level of discord" at Edinburgh (Howe 1994), commissioned Sir James Lighthill to investigate AI in general and the Edinburgh group in particular. The result was a disaster, and led to the UK's "AI Winter"—which pre-dated the American one, and lasted for over ten years.

The mid-1960s also saw the beginnings of cognitive science at the recently founded University of Sussex. Sutherland, like Michie, had been enthused by visits to MIT and CMU in the early 1960s. He lectured on AI within the Experimental Psychology course from its inception, and invited various AI experts from outside to give additional, more technical, lectures. Indeed, he was already negotiating with potential funders to establish a (psychologically oriented) AI Department in 1966, when Michie was doing the same—with more immediate success.

The earliest researchers at Sussex were various members of Sutherland's group (notably Uttley and Longuet-Higgins), and myself (Preface, ii). After the arrival of the AI vision pioneer Max Clowes, and Sloman's visit to Michie's AI empire, Sussex established the Cognitive Studies Programme (COGS) in 1975 to offer interdisciplinary degrees in cognitive science. Students took some combination of philosophy, psychology, linguistics, and AI. (The AI courses were offered also to anthropologists, but the students' anthropology tutors soon started advising them against it: see Chapter 8.ii.c.) An AI subject-group was born within the Programme a few years later. In 1987 the

Programme metamorphosed into a School, and in 2003 into the Centre for Cognitive Science (closely associated with the new Department of Informatics).

In the late 1960s, the San Diego psychologists—also newly established, with Mandler as the first Chairman—were building a lively research group. This was a West Coast version of the New England mission station. Indeed, three of the founding members, Mandler himself (in the late 1950s) and his appointees Norman and William McGill, had been close to Miller and/or Bruner at Harvard—and Mandler was about to accept an invitation to Bruner's Center when San Diego approached him.

McGill, a psycho-acoustician trained by Miller, had the least influence on the group: he stayed at San Diego for only three years. But Norman and Mandler remained active there for much longer. Norman's background was in electrical and computer engineering, so he was well placed to combine computation with psychology. Mandler was less explicitly computational, but he was a committed cognitivist. As a refugee from Nazi Germany, he brought 'non-Newtonian' assumptions from the European tradition—"to some extent German and French but to a large extent British" (2002*c*: 175). So the ideas of Craik and Bartlett, one long dead and the other a nonagenarian half a world away, were now working their magic in California.

The San Diego group, and its offshoots such as CHIP (the Center for Human Information Processing), would soon have a major influence on the field. Its members wrote the first widely used textbook of cognitive science (Section v.a, below), and an early computational text on associative memory (John R. Anderson and Bower 1973). They helped found the Cognitive Science Society (v.c, below). And they were later crucial in the renaissance of connectionism (Chapter 12.vi–vii).

In addition, anthropology was represented at San Diego by Roy D'Andrade, a student of Miller and Bruner at Harvard in the mid-1950s. And, last but not least, San Diego philosophers would provide a counter-intuitive philosophy of mind. This was based in part on the insights of New Look psychology, and (after their arrival in San Diego) it was integrated with connectionist ideas (16.iv.e). Eventually, San Diego neuroscientists would join in too (14.ii).

So, by 1970, mission stations had been established on both sides of the pond. Although the Harvard Center finally succumbed to strangulation by red tape, its mission lived on.

f. The sine qua non

Every church needs its donation boxes, and Descartes's wealthy philanthropists were as crucial as ever. They were available in more abundance than usual in late 1950s and 1960s USA: "We were lucky historically. Those were the days of post-Sputnik academic expansion" (Bruner 1983: 124).

Sputnik, the first man-made satellite, was put into orbit by the USSR in October 1957. Only a few weeks later, Sputnik-II was launched. It was not only heavier, but carried a live dog, Laika. (She died from heat exhaustion, having spent two days on the launch-pad and five hours in space.) The USA, too, had been working for some years on rocket technology. Indeed, in the final days of the Second World War the US Army had recruited the Nazi scientist Werner von Braun, designer of the V2 rockets that devastated London in 1945. For many months before the appearance of Sputnik, he had been trying—without success—to persuade the army to think beyond earthbound rockets, and to fund a test launch of his planned satellite. The worldwide sensation caused by the two Sputniks was a bitter pill to swallow, not just for him but for the US government. The money-taps for space technology were suddenly turned on, enabling von Braun's device (Explorer-I) to be sent into orbit some four months after Sputnik. What's relevant here, however, is that the money-taps were also turned on for science, and scientific education, in general.

That political background helped raise the funding made available through the USA's national research councils. That was partly because many cognitive science projects arguably had some military potential. Indeed, the US Office of Naval Research was already funding MT in the USA and UK, and the USA's Department of Defense would be crucial for the development of AI and some types of cognitive psychology (see Chapters 9.x.a. and 11.i).

Mandler remembers, for instance, that as the Chairman of the San Diego psychology group in 1966, he was "in the midst of raising federal money for our building and thanks to Sputnik [launched by the USSR in October 1957], there was lots of it around" (2002*c*: 192). "Under the aegis of national defense", money was poured into higher education, so that he was granted \$1.3 million only a year after his new group had been founded.

We've seen that Bruner's Center for Cognitive Studies was supported by the Carnegie Foundation. One of the "luxuries" of the place, he recalls, was that the Carnegie grant (and its runner-up of \$250,000 five years later) was unrestricted money:

> Even *small* unrestricted grants help . . . Every cent of unticketed money is a fortune . . . Do what you will with it but make it interesting. (Bruner 1983: 125)

"Every cent" is correct: the size of the grant isn't all-important. The Dartmouth meeting, for instance, showed that even so small a sum as \$7,500 can be very effective if it's used imaginatively.

The sponsorship of cognitive science by the Alfred P. Sloan Foundation was especially generous. In the late 1970s, they promised \$15 million (later rising to \$20 million) for a "particular program in cognitive science". Admittedly, even this munificent gift horse was looked in the mouth. The guiding Report aroused such "virulent opposition" from people disagreeing on the definition of *cognitive science* that it wasn't published until five years later (H. Gardner 1985: 35–8; State of the Art Committee 1978).

Besides huge donations to several US universities, this programme included money for numerous conferences, workshops, and foreign visitors—and even for the first history of the field (H. Gardner 1985, p. xiii). (I was one of the many non-US citizens who benefited, being invited by Yale's AI group as a "Sloan visiting scientist" for June 1979.) The sponsorship was intended to support six different disciplines (State of the Art Committee 1978). However, we'll see in Chapter 8 that one of the six virtually disappeared from view.

The highly interdisciplinary Media Lab at MIT (13.v.a), founded in 1986, received its funding from a remarkable diversity of sources (Brand 1988: 12–13). Besides the usual suspects (governmental and philanthropic bodies, plus computer manufacturers), these included several Hollywood film companies, half a dozen TV channels, the LEGO toymakers, Polaroid, and Nippon Telephone and Telegraph. Clearly, these benefactors were driven by the practical/commercial potential of cognitive science, not its theoretical/scientific interest.

In the mid-1980s, AI research in the USA and UK was boosted by extra governmental funds made available because of the Fifth Generation scare—and the "Star Wars" scare too (11.i.c and v.b). Although most of this work was technological, some was psychological.

That's not to say that governmental funds were always easy to access. The Lighthill Report (SRC 1973) decimated AI funding in the UK, and US funding had been hit a couple of years earlier. In 1970 the Mansfield Amendment of the Defense Appropriations Bill required that all ARPA research (henceforth, DARPA: D for Defense) have some direct military application. The money didn't dry up. But blue-sky projects, or even perfectly feasible projects which looked non-military, were sidelined.

To be sure, "military" didn't have to mean missiles. For instance, AI's early work on virtual reality (VR), involving speech and telepresence, was funded by DARPA in the 1970s and 1980s (Brand 1988: 163). But others, besides DARPA committee members, had their own views about what research it was reasonable to fund with taxpayers' money.

In 1975 Senator William Proxmire of Wisconsin made himself a household name in the USA by announcing (in a newsletter sent to over 100,000 people) that he'd instituted the Golden Fleece awards for "unnecessary expenditures in the Federal Government". (Had he read *Gulliver's Travels*, he might have called them the "Lagado Academy" awards instead: Chapter 2.i.b.)

He started off worrying, as well he might, about the US Air Force spending $2,000 for each toilet seat put into a bomber. But he soon turned his attention to the research grants provided by the NSF (National Science Foundation) and NIH (National Institutes of Health). And there, his bluff common sense often let him down.

He had a lot of fun, for instance, mocking a grant project entitled "The Sexual Behavior of the Screw-Worm Fly". Unfortunately, he rarely got beyond the titles. His judgements (or rather, his ill-educated guesses) about the scientific, and even the practical, potential of candidate projects was so unreliable that the scientists he named as Golden Fleece recipients took to boasting about it on the Johnny Carson TV show. One NIH project scorned by Proxmire, because it involved a study of "Polish pigs", helped lead to a blood-pressure medicine used by millions of his compatriots. And the Media Lab's Aspen Movie-Map, built with $300,000 from DARPA and also castigated by Proxmire, led on to today's VR (Chapter 13.v.a and vi). Only rarely did he hit the nail on the head, as when he suggested that the money spent by NASA on SETI (the Search for Extra-Terrestrial Intelligence) might be better spent on searching for intelligent life in Washington DC.

With hindsight, science-policy researchers see the Senator as having done significant damage to the advancement of science in the USA (Guston 2000). Fortunately, the damage was temporary. Proxmire eventually moderated his views to some extent (R. C. Atkinson 1999: 416). Between 1976 and 1980, no NSF grant received a Golden Fleece award. Indeed, he even admitted, at a 1980 seminar on biological methods of pest control, that the work on the screw-worm fly had actually been important. Nevertheless, for a while (until the awards ceased in 1988) he did have significant influence. For example, he managed—by giving the dreaded Golden Fleece—to block funding of a

SETL project (Search for Extra-Terrestrial Life) proposed by NASA that had already been endorsed by a Congressional subcommittee (Dick 1993).

As for "artificial intelligence", that was a red rag to a bull. AI was declared a "racket" by Proxmire, on national TV (Raskin 1996: 4; V. Raskin, personal communication).

All the more reason, then, to agree with Bruner's observation (above) that even small amounts of money given with no strings are hugely valuable. There's no need to tell the funders that you plan to study the amatory antics of the screw-worm fly, still less to try to justify it.

6.v. Spreading the Word

A new church needs to spread the word, largely by epistles sent to the four corners of the earth. Similarly, science couldn't progress if scientists hid their lights under bushels. We saw in Chapter 2.ii.c how important publications are, both in forming the invisible college and in turning it into something more established. The history of cognitive science therefore involves its formative textbooks and journals, and the setting-up of relevant societies and conference series.

At the same time, the initiates must learn the church's rituals. For these not only help to make the new ideas familiar, but also bring them alive. In the case of cognitive science, this meant introducing people to computer programming *in practice*.

a. Training sessions

That last remark doesn't merely mean that if someone wants to build a computer simulation they need a computer to build it on. (Although that's true: the early history of cognitive science was hugely affected by who got access to computers. Rosenblatt, for example, was able to develop the perceptron, at a time when Cornell's Psychology Department had no suitable machine, only because he persuaded his aeronautical neighbours to let him work on theirs: 12.ii.e.) It also means that *using* computers gives one an added dimension of understanding, which can then naturally flow into one's thinking about other things—such as psychology.

This is a special case of what Bruner called "cognitive technology", and of what Gerd Gigerenzer (1991*b*, 1994) calls the "tools to theories" heuristic in scientific creativity. As one grows skilled in using a tool, it's experienced as available, or ready to hand, in a way it wasn't before. One aspect of that is that its use can now be attempted outside the context in which it was first encountered. Gigerenzer originally described this heuristic by reference to statistical tools, newly used in the context of experimental (including cognitive) psychology. But it applies also to cognitive science. The importation of heavy-duty mathematical statistics into connectionism, for example, wouldn't have surprised him (see 12.vi.f). And, what's relevant here, the heuristic applies to computational tools as well as statistical ones.

If the new gospel was to spread, therefore, people needed to get their hands on a computer—not just to read about them. Accordingly, in the summer of 1958 Newell and Simon ran a Research Training Institute on the Simulation of Cognitive Processes at RAND. As well as lectures and seminars, and demonstrations of three working simulations (the Logic Theorist, GPS, and EPAM), the Institute gave participants a chance to do some programming on the RAND machine.

They did this in IPL-IV, a programming language in which it's almost impossible for a novice—or anyone else, for that matter—to do anything of interest (see 10.v.b). But that didn't matter. Nor was it a disaster, although it was a drawback, that the eager novitiates might not be able to access a machine after returning home. For they would have learnt a hugely important lesson at RAND, one which could have been learnt in no other way.

Even the minimal programming that my fellow graduate students and I did at Harvard–MIT in 1962 (Preface, ii) awakened us to the clarity of thought, and the unforgiving precision, that's needed to make a program do what one expects. In that sense, there's more difference between someone who's never programmed at all and someone who's done only Mickey Mouse programming, than there is between the latter person and a world-class AI wizard.

So if the Institute did nothing else, it provided attendees with a wholly new vision of theoretical rigour. Hand-waving was still often unavoidable in psychology (as it is today). But it could no longer be complacently mistaken for accuracy.

However, the Institute did do something else: it gave people hands-on experience of specific programming/theoretical concepts, such as *instruction*, *subroutine*, *goal–sub-goal hierarchy*, *list*, *search-space*, *search*, *iteration*, *transfer of control*, and so on. Under Newell and Simon's expert guidance, people learned to use these new tools and in so doing sensed their potential power. Even though what they could do on the RAND computer was hugely limited, the relevant concepts had got inside their heads and could then be used (*sic*) to think about psychological processes in newly creative ways.

Among those attending this pioneering summer Institute were Miller himself, the social psychologist Robert Abelson, the cognitive psychologist Roger Shepard, and the NewFAI researcher Bert Green (Gigerenzer and Sturm 2004: 25). Abelson would soon embark on highly influential work on the representation of plans, intentions, social roles, and emotional relationships (see 7.i.c). Shepard would later revolutionize the study of mental imagery (7.v.a). And Green, with Carol Chomsky and others, would soon add impetus to natural language processing by writing the BASEBALL program (9.xi.b and 10.iii.a). Evidently, the summer was well spent.

However, summer schools weren't enough. The new gospel couldn't spread really widely until most research universities had their own computer, and encouraged their psychologists—not just their aeronautical engineers—to use it. This would take many years (Aaronson *et al.* 1976).

The Harvard Center itself, as we've seen, didn't actually *do* much computer modelling—even though it possessed a computer. The reason wasn't just that Bruner wasn't computer-minded. After all, his co-founder Miller was enthusiastic about programming, and had enrolled for the RAND summer school. The major problem was the difficulty of getting the computer to work. On an average week in 1965–6, the Center's machine (a PDP-4C) saw eighty-four hours of use—but fifty-six of these were spent on debugging and maintenance. The Annual Reports for 1963–9 contained "several remarks of the type 'It is difficult to program computers . . . . Getting a program to work may take months' " (Gigerenzer and Sturm 2004: 26). The title of a 1966 technical report issued by the Center said it all: *Programship, or How to Be One-Up on a Computer Without Actually Ripping Out its Wires*. (This mid-1960s title owed its resonance to

Stephen Potter's best-selling, and hilarious, manuals on the rituals of *One-Upmanship* and *The Theory and Practice of Gamesmanship, or The Art of Winning Games Without Actually Cheating*.)

Given such problems, people who couldn't yet do anything else—such as graduate students—might be willing to persevere in learning the new technique. But established professional psychologists, with other (professionally respected) investigative skills at their command, would be tempted to give up—or not to bother even to try. So hearing about the computer modelling approach, and even playing around with it for a time on a (necessarily) primitive machine, didn't always lead to a conversion experience.

For many years, then, the take-up of computing by psychologists was patchy. (Some men of my acquaintance refused even to try, because they couldn't type—having always relied on their female secretaries to do that job for them.) In 1972, fourteen years after the summer Institute, Newell and Simon's epochal book *Human Problem Solving* appeared (7.iv.b). It could function both as a sermon, or pep talk, and as a training manual. But even then, the majority of researchers *in their own Department* remained reluctant to use the new tool (Gigerenzer and Sturm 2004: 27).

If this was true of CMU, it was inevitably true elsewhere. Across the USA and UK, the social scientists' take-up of the computational approach was slow. For example, when Feigenbaum and Julian Feldman tried to set up an AI group at the Berkeley Business School in the early 1960s they got little support from their fellow faculty members—despite having just published their hugely exciting *Computers and Thought* (see below). That's why Feigenbaum left Berkeley for the more welcoming Stanford in 1965 (Crevier 1993: 148). Psychologists, too, were slow to respond to the clarion call. It's true that in 1969, even before the publication of the Newell-and-Simon tome, the APA gave Simon their award for a Distinguished Scientific Contribution: clearly, then, some professionally influential psychologists valued this new approach. Nevertheless, it was as late as 1983 when a cognitive psychologist told me that computational psychology was still "too specialized" to be discussed by the newly founded BPS sub-group on the History and Philosophy of Psychology (see 7.vii.d).

A quarter-century after the brief training session at RAND, another seminal tutorial was run at CMU (see 12.vi.a). The motives were the same, but the tool-for-training was different. The 1986 summer school was focused on the relatively novel form of computer modelling called PDP (parallel distributed processing) connectionism. Well aware of the endemic professional inertia remarked above, the organizers were happy to welcome interested colleagues but (like the Jesuits?) even happier to welcome the youngsters. Indeed, we'll see that they went to great lengths to make their training manual affordable to impecunious graduate students.

By that time, the need for training sessions had long been recognized. RAND was the first of several devoted to NewFAI methods—from the early 1970s, including production systems (10.v.e and 7.iv.b). Little by little, the rituals had been spreading.

b. Library tickets

Early computational work was scattered over many different journals, of which only a few were psychological (Simmons and Simmons 1962). If psychologists were to be made aware of the new approach, *collections* of papers would be needed. And if students were to be attracted, accessible textbooks would eventually be required.

A slim volume on *Simulation in Social Science* appeared in 1962 (Guetzkow 1962). This contained contributions from Newell, Hovland, and Robert Abelson—but most of the topics were drawn from sociology, economics, or management studies. The same year saw Harold Borko's collection *Computer Applications in the Behavioral Sciences*, a textbook based on a course he'd been teaching at the University of Southern California (Borko 1962, preface). However, only a few chapters dealt with psychological themes (binary choice, language, and diplomacy). Borko's book wasn't going to set the world alight.

What did set the world alight, just one year later, was the ground-breaking collection *Computers and Thought* (Feigenbaum and Feldman 1963). This had been put together using some of the money from a $70,000 grant from the Carnegie Corporation, awarded to the two young editors to look into "the potential of artificial intelligence" (Newquist 1994: 176).

The editors, then at Berkeley, were both ex-students from CMU. Feigenbaum was the one who'd raised the Carnegie money. He was a psychologist in the process of moving over to computer science, and had already written a pioneering program under Simon's tutelage: a model of rote memory (Feigenbaum 1961). But it was the book which thrust him onto the international stage. It sold so well that some of the royalties were eventually set aside to fund a biennial Computers and Thought Award. The first recipient was Winograd in 1971, at the second meeting of IJCAI (the International Joint Conference on Artificial Intelligence), in London (10.iv.a).

The psychological models described in the collection included five that would be repeatedly cited later. These were Newell and Simon's Logic Theorist and General Problem Solver; Selfridge's Pandemonium and a 'learning' version thereof (Chapter 12.ii.d); Earl Hunt and Hovland's model of concept learning (1961; cf. Hunt 1962; and see 13.iii.f); and Feigenbaum's EPAM (1961).

The next important collection was ten years later: *Computer Models of Thought and Language* (Schank and Colby 1973). This provided several new papers by leading researchers, including one by the wunderkind Winograd (9.xi.b). In the opening chapter, Newell (1973*b*: 25) boldly declared that AI might as well be called "theoretical psychology". (He'd already said as much in the 920-page Newell-and-Simon book on problem solving, but that had a more restricted audience.)

In 1975 the label "cognitive science" was used in two more collections: the San Diego psychologists' *Explorations in Cognition* (D. A. Norman and Rumelhart 1975) and the AI-based *Representation and Understanding* (Bobrow and Collins 1975). Both referred to their field of interest as *cognitive science*. The latter did so up front: in its subtitle and in the Preface, which referred to "a new field we call *cognitive science*" (p. ix). The former saved this unfamiliar term for the very last page (and avoided using it on the publisher's mail-order leaflet, which I still have). On that final page, the San Diego pair declared: "The concerted efforts of a number of people from the related disciplines of linguistics, artificial intelligence, and psychology may be creating a new field: *cognitive science*" (p. 409). These were the first *books* to identify their theme in that way. However, the plural version—cognitive *sciences*—had been used for some time by Michie's group, and had appeared in print in Longuet-Higgins's response to the infamous Lighthill Report (SRC 1973: 37; see Chapter 11.iv.b).

"Cognitive psychology" had been labelled some years earlier. In 1967 Neisser (1928–) had published the first monograph explicitly devoted to this area. "I got letters", he recalled later, "from people saying that they were glad that I had given it a name, because they were interested in all the topics I considered, but the area had not had a theoretical identity" (1986: 278).

The Gestaltists—who, in the person of Kohler, had influenced Neisser during his postgraduate course at Swarthmore—were cognitive psychologists too, though not under that label. In other words, not all cognitive psychology is part of *cognitive science*. But these terms are often assumed to be equivalent, partly because Neisser, when he defined the new term, was a computationalist.

An ex-student of Miller's, he had now—like Miller himself—passed beyond " 'bits' and 'phonemes' and the like" (see i.b, above). He'd already declared an interest in computer modelling some years before—together with a healthy scepticism about the extent to which (then current) simulations could match human thinking (Neisser 1963). For instance, he'd complained that "When a program is purposive, it is too purposive," and that programs aren't emotional—because they don't "get tangled up in conflicting motives", as humans often do. Nevertheless, his book followed MGP in declaring that "Although information *measurement* may be of little value to the cognitive psychologist, another branch of the information sciences, computer *programming*, has much more to offer" (1967: 7–8; first italics added). This meant GOFAI—but GOFAI, of course, included the parallelist Pandemonium. Indeed, Neisser was greatly impressed by Pandemonium, and had already written two commentaries on it (Neisser 1959; Selfridge and Neisser 1960).

Compared with MGP's catholic manifesto, however, Neisser's book was narrow in compass. It focused on experimental research, and especially on the problems of coding and representation involved in cognition. Bartlett, the New Look, and Chomsky all loomed large. As a result, it was much more influential than Reitman's earlier *Cognition and Thought* (1965) which, although largely 'methodological', *did* tackle motivational conflict and emotion (see Chapter 7.i.b)—and creativity as well.

Admittedly, cognition—the term used with such defiance by Miller and Bruner (and Reitman too) only a few years earlier—was said to be "involved in *everything a human being might possibly do*", so that "*every* psychological phenomenon is a cognitive phenomenon" (1967: 4; italics added). This followed from Neisser's definition of it as "all the processes by which the sensory input is transformed, reduced, elaborated, stored, recovered, and used". Dreams, schizophrenic hallucinations, and hypnosis were briefly mentioned in the chapter on "visual memory". Freud's primary process thinking was described, following Pandemonium, as "a shouting horde of demons"; and Freudian slips were attributed to the parallel processing of learnt associations (pp. 298–9).

These personal matters were included, if only fleetingly, in part because Neisser had been influenced "quite a bit" by the Third Force psychologist Abraham Maslow (5.ii.a). Maslow had been his chairman during his first job, at Brandeis (Neisser 1986: 275). Indeed, he was "pleased with the book" because it had "a systematic scientific feel to it . . . [yet] also had a definite humanistic quality" (p. 279). And both "clinical" and "humanistic" colleagues approved of it (p. 280).

Nevertheless, his closing words admitted that he'd ignored purpose and motives, even though cognition and motivation—and personality, too—were probably "inseparable" (p. 304). His final sentence ran: "The study of cognition is only one fraction of psychology, and it cannot stand alone" (p. 305).

Clearly, then, library tickets would eventually be needed for more wide-ranging volumes, reaching as far as personality and hypnosis. As it turned out, those tickets wouldn't be usable for many years to come. (More accurately, they were usable already—but only for a handful of relatively unilluminating texts: see Chapter 7.i.a–c.) Meanwhile, the developing cognitive science was more cognitive than either Neisser or MGP had intended.

Neisser's text was computational through and through. The S–R psychologists, he said, were loath to make hypotheses about mental processes because they were "afraid that a separate executive would return psychology to the soul, the will, and the *homunculus*; it would be equivalent to explaining behavior in terms of a 'little man in the head' " (p. 295). But, recently, things had changed:

> It now seems possible that there is an escape from the regress that formerly seemed infinite. As recently as a generation ago, processes of control had to be thought of as *homunculi*, because man was the only known model of an executive agent. Today, the stored-program computer has provided us with an alternative possibility, in the form of the *executive routine*. This is a concept which may be of considerable use to psychology. (Neisser 1967: 295)

> A program is not a device for measuring information, but a recipe for selecting, storing, recovering, combining, outputting, and generally manipulating it . . . [This] means that programs have much in common with theories of cognition. Both are descriptions of the vicissitudes of input information. (Neisser 1967: 8)

In other words, the sixth tenet of behaviourism could be dropped.

It didn't follow, said Neisser, that computational psychology implies "a commitment to computer 'simulation' of psychological processes" (p. 9). Computer models couldn't do "even remote justice to the complexity of human [minds]". But this didn't matter. The crucial point was to realize that psychology is as much about "processes" as "contents" (the distinction that had eluded me in the Cambridge apple orchard, but which was made crystal clear by MGP: Preface, ii).

That done, questions could be asked in a new way. For example, Neisser's "analysis by synthesis" theory of speech perception suggested that the hearer actively constructs an internal model that matches the attended speech signal, whereas unattended signals are perceived more passively (1967: 193–8). (In the 1970s, Neisser's analysis by synthesis would influence some of the AI vision work done at the University of Edinburgh: A. Sloman, personal communication.)

For the record, Neisser's commitment to computationalism didn't last. He soon turned to ecological psychology, dedicating his next book (1976) to James and Eleanor Gibson, whom he'd encountered on moving to Cornell in the late 1960s. Quite apart from the Gibsonians' general suspicion of 'computation' (see 7.v.e–f), real-life situations are even less amenable to computer modelling than laboratory tasks are. Neisser now accused cognitive psychology of being "indifferent to culture", and feared that it "could become a narrow and uninteresting specialist field" (1976: 7; see Chapter 8). In particular, he said:

> The villains of the piece are the mechanistic information-processing models, which treat the mind as a fixed-capacity device for converting discrete and meaningless inputs into conscious percepts. (Neisser 1976: 10)

He didn't change his mind back again. In his late-century retrospective (Neisser 1997), he gave computational theories much less credit than he'd given them thirty years before.

Neisser's 1967 book convinced many professional psychologists that computational theories were needed. But it didn't score highly with first-year undergraduates seeking a user-friendly introduction. Gregory's 'New Look' *Eye and Brain* (1966) had already done that, as we've seen. But it had concentrated on vision, not (like Neisser's volume) on cognitive psychology as a whole. The ice-breaker appeared in 1972: Peter Lindsay and Donald Norman's *Human Information Processing*.

This was the first of several hugely influential textbooks to come from the San Diego stable, the most famous being the PDP 'bible' of 1986 (12.vi.a). The 1972 volume had been six years in the making, dating from when Norman (fresh from Bruner's Harvard Center) and Lindsay helped set up UCSD's first degree programme in psychology. After some teething troubles, "The course took hold, the enrollment climbed. But we were hampered by the lack of a suitable text. The only way to get one seemed to be by writing it ourselves" (Lindsay and Norman 1972, p. xvii).

Lindsay and Norman boldly subtitled their book *An Introduction to Psychology*: not cognitive psychology, but psychology *tout court*. Admittedly, fifteen of the seventeen chapters dealt with perception, memory, language, learning, problem solving, and choice. And although amnesia was considered (and aphasia mentioned in passing), there was no discussion of personality, whether normal or abnormal. Nevertheless, social psychology was given one full chapter, and motivation (including stress, conflict, and emotions) another.

Among the book's most memorable features were its pictures, especially Leanne Hinton's impressions of Pandemonium. Several chapters included demonic diagrams as entertaining as they were educational (see Figures 6.5 and 6.6). And the paperback cover bore thumbnail sketches of the two authors, clean-shaven and bearded respectively: each had horns, cloven feet, and a demon's tail—no favouritism here. It also carried a cogs-and-pulleys depiction of illumination—the pun was clearly intended. There was even a private joke for the readers of the MGP manifesto: a lone demon, carefully hammering a nail into the "H" of the title (Figure 6.7).

These engaging illustrations reminded readers that Norman and Lindsay's approach was *computational*, even though they didn't claim that *computer modelling* was necessary. Their amusingly demon-ridden volume soon made computational psychology into a familiar undergraduate experience. The pages bristled with introductory bibliographies and other helpful advice. It's a telling comment on how much things had changed since the 1950s that their advice included this:

> Do not shun the older issues [of the journals]. Because of the peculiar history of psychology, the most fascinating papers seem to have been published in the years around 1890 through 1910. (Lindsay and Norman 1972: 686)

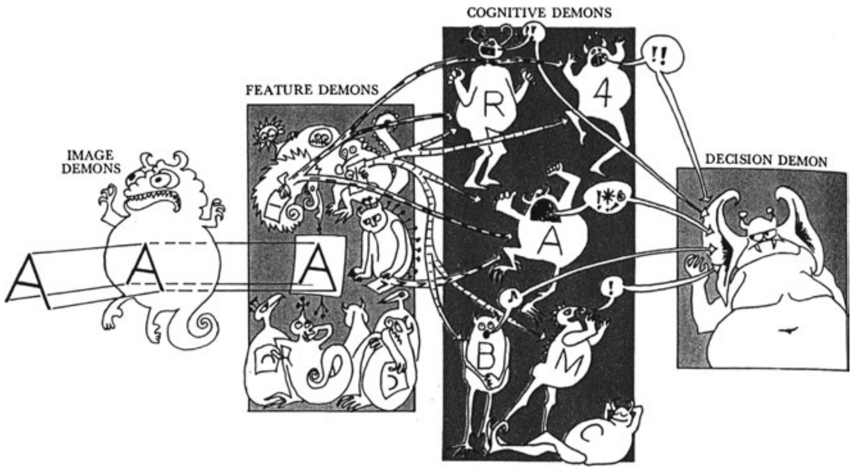


FIG. 6.5. Artist's impression of Pandemonium. Reprinted with permission from Lindsay and Norman (1972: 116)

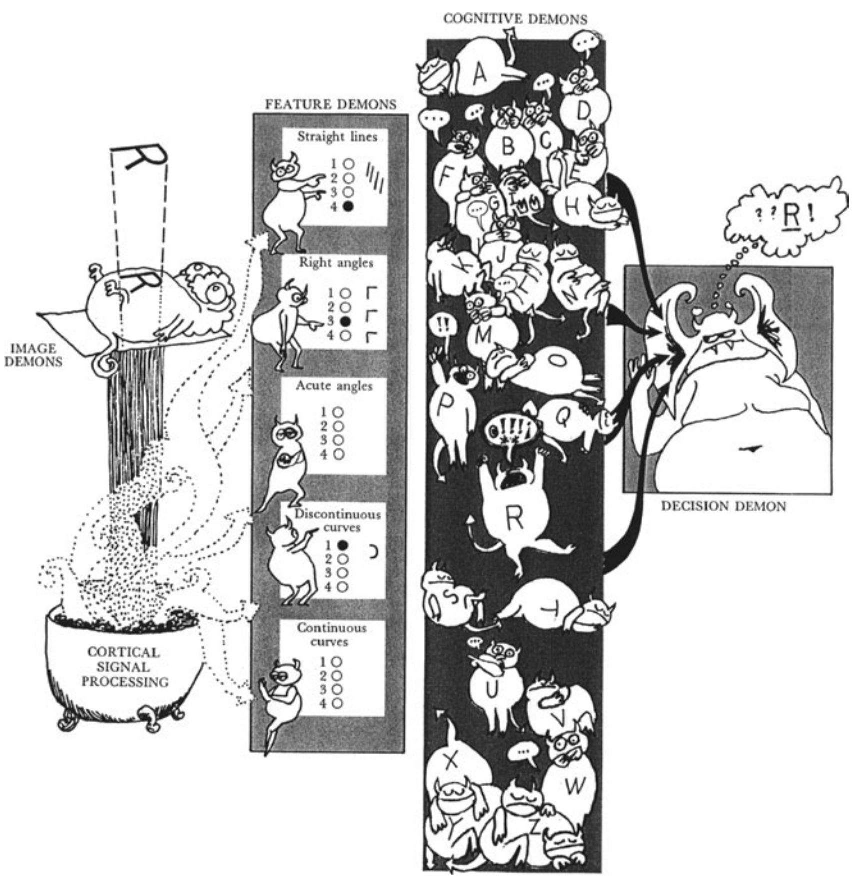


FIG. 6.6. Pandemonium at work. Reprinted with permission from Lindsay and Norman (1972: 125)

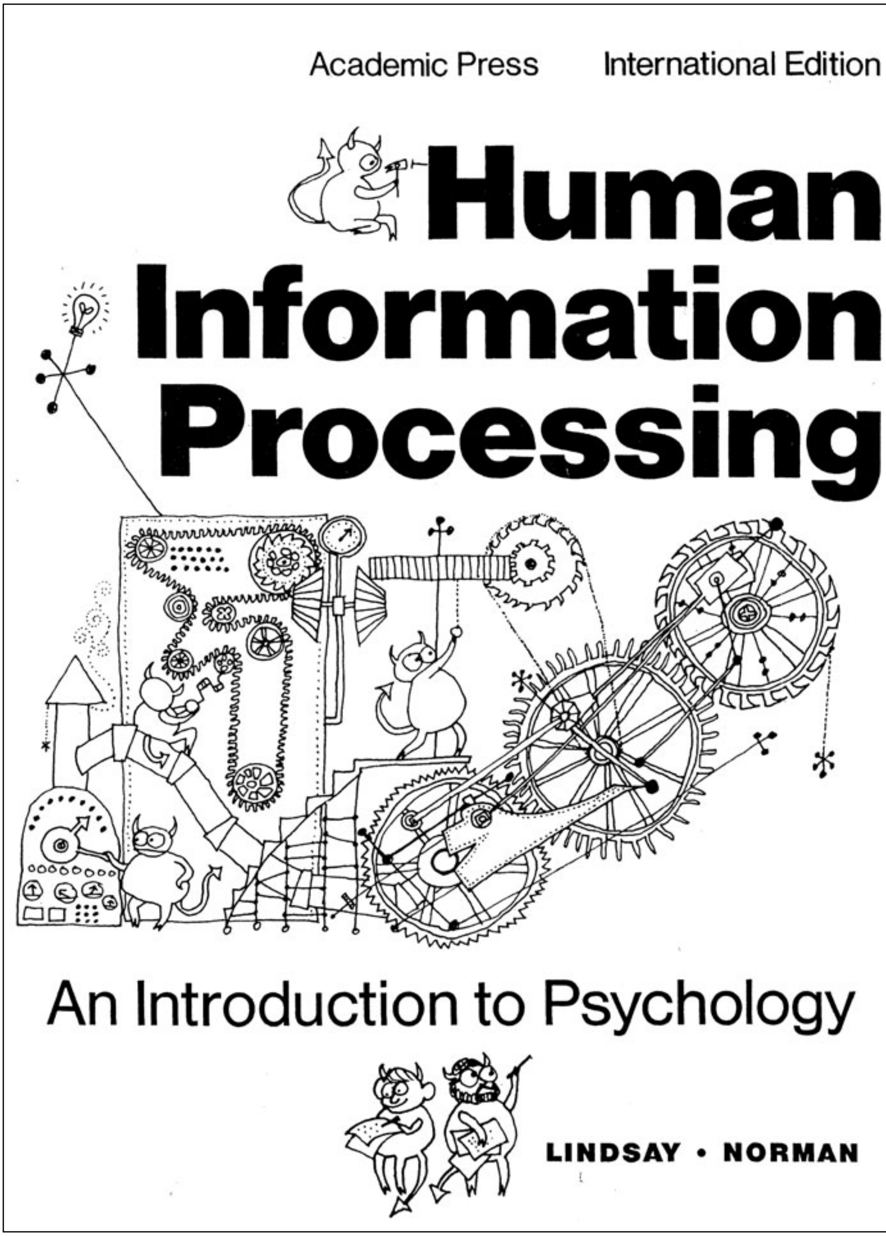


FIG. 6.7. Front cover of Lindsay and Norman (1972). Reprinted with permission

In other words, the elderly sitting tenants (Chapter 5.ii) should be invited to tea after all. (I can't resist backing up their claim by recommending George Stratton's paper of 1896: you'll rarely find more "fascinating" reading matter.)

Lindsay and Norman couldn't mention many programs other than Pandemonium, for only a few had then been written. (One of these was their colleague John R.

Anderson's HAM: see Chapter 7.iv.c.) By 1977, when the UK's Open University launched its first course in Cognitive Psychology, there were many more. One of the two set books chosen for the OU's course was my *Artificial Intelligence and Natural Man* (Boden 1977), soon adopted for other psychology (and AI) courses in the UK and USA. "Natural Man" was what the book was really about: it explored a wide range of psychologically relevant AI programs, detailing not only their successes but also their many failures, and asking how these might be overcome. The philosophical (and social) implications of AI and computational psychology were discussed too. In short, it was an exercise in *interdisciplinary* cognitive science.

Another disciplinary mix appeared a few years afterwards, a collection on *Mind Design* edited by the philosopher John Haugeland (1981*a*). His volume included papers by AI researchers, computational psychologists, and philosophers—and it forefronted some of the radical philosophical criticisms that were being made of the field. As a way of introducing cognitive science to outsiders it was highly successful, and was reprinted several times. (A revised edition appeared recently, now including papers on connectionism and dynamical systems theory: Haugeland 1997.)

In 1982 "cognitive science" featured in the final volume of *The Handbook of Artificial Intelligence*. Chapter 11 focused on GPS, EPAM, and semantic networks (10.iii.a); on a model of humans' "opportunistic" planning (B. Hayes-Roth 1980; B. Hayes-Roth and Hayes-Roth 1978); and on the psychologists' studies of belief systems and reasoning to be discussed in Chapter 7.i.a, i.c, and iv.c. The chapter was specifically described as "an introduction to cognitive science" (P. R. Cohen and Feigenbaum 1982, pp. xiv, 4).

Yet another interdisciplinary volume was supported by the Sloan Foundation itself. They sponsored Howard Gardner's history, *The Mind's New Science* (1985). This accessibly written book raised the profile of cognitive science, and doubtless drew many people into it as a result. Just as important, it helped those *within* the field to appreciate the links between the various disciplines.

By the mid-1980s, then, the epistles had multiplied. Their enthusiasm and interdisciplinarity had spread the gospel far and wide. Cognitive science might not be to everyone's taste. But at least it was visible.

c. Journal-ism

The eponymous journal *Cognitive Science* was launched in 1977 by three American researchers in "psychological" AI: Roger Schank, Eugene Charniak, and Allan Collins. This soon became the official journal of the Cognitive Science Society, established in 1979. The founding conference was in San Diego—in the same place and year, and thanks to some of the very same people, as the seminal interdisciplinary meeting on PDP connectionism (Chapter 12.v.b). The invited papers appeared in the journal, and also as a stand-alone volume (Norman 1981).

Even before that, the quarterly *Cognitive Psychology* had been founded in 1970, by Reitman in the USA. Noam Chomsky was on the Editorial Board, and so was Hunt, who later edited it for twelve years and is still on the Board as I write. And in 1977 Martin Ringle (at Vassar) and Michael Arbib (at UMass, Amherst) had started *Cognition and Brain Theory*, covering "philosophy, psychology, linguistics, AI, and neuroscience". (In 1982 this swallowed up Arbib's *Brain Theory Newsletter*, which had failed owing to neuroscientists' suspicion—in those days—of computational modelling: Chapter 14.vi.c.)

One journal which looked as though it was new wasn't new at all, except in its approach. The *Journal of Verbal Learning and Verbal Behavior* had been started in 1962, when stringent criticisms of S–R accounts of language were already being heard—from psychologists as well as Chomsky. From 1985 (volume 24), however, it appeared as the *Journal of Memory and Language*. The old title was too reminiscent of the behaviourist approach, which had still infused its pages in the early days but was now an embarrassment.

In mid-1960s England, the British Computer Society established a Study Group on Artificial Intelligence and the Simulation of Behaviour (AISB). Started by Clowes in 1964, within a decade this had become the AISB Society, under the interdisciplinary leadership of researchers at the universities of Edinburgh, Sussex, and Essex. The name was a psychological pun on Clowes's part: "A is B" captured the New Look ideas of visual interpretation and ambiguity, the focus of Sussex's AI research at that time (see Chapters 10.iv.b and 12.v.h).

The informal *AISB Newsletter*, now called the *AISB Quarterly* (and recently joined by the peer-reviewed *AISB Journal*), was started in the late 1960s. This low-profile serial carried psychological as well as technological items, and some telling spoofs too. One of these, by "Sir Grogram Darkvale FRS", featured the Lighthill Report (and appeared alongside a more serious critique: P. J. Hayes 1973; see 11.iv.b). Many others were credited to the pseudonymous Father Hacker, whose real identity was a closely guarded secret (*Now it can be told!*: it was Clowes). Father Hacker remains alive and well today, penned by various authors (his tart 'Guide for the Young AI-Researcher' is quoted in Chapter 13.vii.a). In other ways, however, AISB has changed. Over the years, the "AI" gradually swamped the "SB", so AISB's publications are now of more interest for AI scientists than for computational psychologists.

Across the English Channel, the high-profile journal *Cognition* was founded by Bruner's associate Jacques Mehler in 1971. It joined *Communication and Cognition*, founded in 1967 (with Piaget and Leo Apostel, among others, as advisory editors) by a research group at the University of Ghent. Both of these publications welcomed theoretical and methodological papers as well as experimental ones.

Perhaps the most important journal of all was Stevan Harnad's (1945–) interdisciplinary *Behavioral and Brain Sciences*, or *BBS*. And perhaps there's no "perhaps" about it: a quarter-century later, in 2004, *BBS* was the leader of *all* the behavioural science journals, according to the ISI citation index.

Harnad was a philosopher and psychologist, with strong interests in both computing and neuroscience. So he was exceptionally well suited to edit a wide-ranging journal. AI and computational psychology were featured in *BBS* right from the start. The first number carried Zenon Pylyshyn's (1978) paper on 'Computational Models and Empirical Constraints', and the first volume also included Haugeland's (1978) critique of "cognitivism". Volume 3 devoted a whole number to *The Foundations of Cognitive Science*, comprised of three hugely influential papers (Fodor 1980*a*; Pylyshyn 1980; Searle 1980).

One might even say that "Pylyshyn's paper" had *over thirty* authors, nearly all leading names in cognitive science (twenty-one are mentioned in this book). And much the same could be said about each of the other papers just cited. For *BBS* was a journal like no other—except *Current Anthropology*, which had pioneered the genre. It published up to four dozen peer reviews (plus the author's Reply) alongside each target article. That, itself, would have been chosen by a ''substantial'' number of referees as presenting ''a controversial viewpoint worthy of argument and discussion from various subdiscipline perspectives'' (Harnad 1978: 1). So publication in *BBS* not only started a debate. It provided commentary drawn widely from cognitive science as a whole.

Much later, Harnad would seek an even wider audience on the World Wide Web. He was one of the first scholars/editors to champion electronic publishing (Harnad 1995*a*,*b*; Fuller 1995). Indeed, he was the founder editor in 1990 of *Psycoloquy*, an international and peer-reviewed publication sponsored by the APA, which—like *BBS* before it—sends out widespread calls for comments on target papers. Thanks to the Web, interdisciplinary discussion is easier now than it used to be. But the *trust* that's needed for one scientist to build on another's work (2.ii.b) tends (in 2005) to accrue more strongly to the printed word, even if the electronic journal is peer-reviewed too. So *BBS* continues, albeit in both printed and electronic forms, and still plays a pivotal role.

Later, of course, further relevant journals would appear. Many were forbiddingly specialist. Others applied cognitive science to practical fields, such as law, medicine, or education. And some reflected unforeseen theoretical changes within cognitive science as such. For instance, one post-millennial arrival was *Phenomenology and the Cognitive Sciences*, a title that would have been unthinkable in the early days. But those I've mentioned here were the first to place cognitive science on librarians' shelves—and to help the invisible college become visible *to itself*.

7

THE RISE OF COMPUTATIONAL PSYCHOLOGY

Although the computational psychology of the late 1950s filled its few proponents with a messianic zeal (Chapter 6.iv), it wasn't yet an identifiable *movement*. The gospel had hardly begun to spread (6.v). So it wasn't mentioned by the President of the American Psychological Association (APA) when, in 1957, he looked down from a great height and identified "the two disciplines of scientific psychology" (Cronbach 1957).

These, Lee Cronbach (1916–2001) said, were the *correlational* and the *experimental*. The first focused on individual differences. It was exemplified by Sir Cyril Burt's work on IQ, and by comparisons between different ages and species, or different classes and cultures. The second, aiming at general laws/theories, included Sir Frederic Bartlett's studies of memory and the behaviourists' of learning. But for Cronbach it was methods, not topics, which defined a discipline:

> A discipline is a method of asking questions and of testing answers to determine whether they are sound . . . [It is our methods of enquiry] which qualify us as scientists rather than philosophers or artists. (Cronbach 1957: 671)

The "philosophers or artists" he had in mind were *other* professional psychologists. Cronbach bemoaned the fragmentation of psychology, but he was uninterested in many of the fragments. So the Freudians and Third Force movement, for instance, weren't mentioned (5.ii.i). They were tacitly included among "the agile paper-readers swinging high above us [like circus acrobats on a trapeze] in the theoretical blue, saved from disaster by only a few gossamer threads of fact" (p. 671). (Little did he know that those gossamer threads would cause a major scandal some fifteen years later: see 6.i.d and ii.d.) It was *scientific* psychology which concerned him.

Even those psychologists who shared a commitment to science, Cronbach complained, had for many years had little or nothing to say to each other:

> The personality, social, and child psychologists went one way, the perception and learning psychologists went the other; and *the country between turned into desert*. (Cronbach 1957: 673; italics added)

That was true. Indeed, any glimpse of an oasis was dismissively declared a mirage. For the two sides offered differing, often mutually contemptuous, accounts of psychological *explanation*: "nomothetic" for the generalizers, "idiographic" for the individual-oriented psychologists (see iii, preamble, below). Indeed, the invective sometimes reached fever pitch. Cronbach was hopeful, however: people were starting to see that one and the same topic—ego involvement, for instance—could be addressed from both sides (p. 682).

At that very moment, a third scientific discipline—namely, computational psychology—was emerging. For Cronbach's Presidential Address came halfway between the hugely exciting meetings of 1956 and 1958 (Chapter 6.iv.b). Methods based on information theory and computer modelling were being applied to perception, language, and memory by George Miller, Donald Broadbent, Jerome Bruner, Richard Gregory, Oliver Selfridge, and Frank Rosenblatt, and to problem solving by Allen Newell and Herbert Simon. And these researchers were now seeing themselves as joint pioneers in a new scientific project—with a new notion of psychological explanation.

It was by no means accepted by psychologists in general, however, that this approach might be fruitful in answering their questions. Indeed, over twenty years later a leading journal in cognitive psychology published a paper defending the—clearly, still heretical—view that AI concepts and methods offered "a new theoretical psychology" (Longuet-Higgins 1981).

By that time (the early 1980s), there were already more heretics around. For the first quarter-century of computational research in psychology had raised many new questions, and found some new answers. The second quarter-century raised the stakes still further. In those fifty years, the new approach was applied to countless different topics. (Even ego involvement: Section i, below.)

That being so, the story of how computational psychology matured can't be intelligibly condensed into a single narrative. Instead of trying to do that, I've selected six topics and followed each one chronologically. That is, the time line starts anew (from about 1960) with each of the relevant sections.

The first topic is personal psychology, including emotion. The next is language. Changing views on the nature of psychological explanation are featured in Section iii. Sections iv and v deal with reasoning and vision, respectively. And Section vi recounts the changing status—and interpretation—of nativism (the claim that some psychological powers are inborn). Finally, Section vii offers an overview of the development of computational psychology considered *as a whole*. It ends by comparing the state of psychology today with what Cronbach was describing in 1957.

A warning, before we begin: several important psychological topics have already been dropped like hot potatoes, and won't be picked up yet:

* I'll say almost nothing about natural-language processing (NLP) or neuroscience; these are tackled in Chapters 9.x–xi and 14, respectively.
* Similarly, learning is hardly mentioned: it was introduced in Chapter 5.iv.b–f, and is considered at length in Chapters 10.iii.d, 12, 13.iii.e, and 14.v–vi and ix.
* The nature of concepts, already raised in Chapters 5.iv.c and 6.ii.a–c, is merely touched on below (in Section ii.d), but is taken up again in Chapters 8.i.b and 12.x.
* Knowledge representation *as such* (e.g. logic, semantic nets, neural networks) is discussed at length in the AI-related chapters (10.iii.a and e, 12.iii–x, and 13.i); much of the research discussed in this chapter was deeply influenced by that work.

* As for memory, this is closely related to language use, learning, and knowledge representation; so it's touched on in Sections ii.d and iv.b–e, and in several other chapters too (2.x.a, 5.ii.b and iv.c, 6.i–ii, 8.i.b, 10.iii.a, and 12—especially 12.v.c–f, vi, and viii–x).
* Creativity isn't discussed here, but in Chapters 9.iv.f and 13.iv.
* I say very little (except in Section v.b–d) about connectionism: not only were most early (and some recent) models based on GOFAI, but connectionism will be discussed at length in Chapter 12. Many psychological topics are included.
* And evolutionary psychology, touched on in Sections iv.g and vi.d–f, is discussed at greater length in Chapter 8.iv–v.

As that list shows, psychological topics are discussed in virtually every chapter. (So Section vii's "overview" also contains multiple cross-references, and will be fully intelligible only after reading the whole book.) That's inevitable. It follows from the definition of cognitive science (1.ii.a) that although AI, in its various forms, is the *theoretical* heart of the field, computational psychology (and neuroscience) is its *thematic* heart.

7.i. The Personal Touch

Meetings and manifestos are all very well, but—as remarked in Chapter 6.iv.a—hard work is needed too. From the 1960s on, there was a lot of that. However, the *problems* were hard as well. Indeed, some pioneering efforts in computational psychology were hugely premature—namely, those aimed at the computer modelling of personality and emotion.

These themes had been of interest to several cyberneticists, including psychiatrists Warren McCulloch and Lawrence Kubie—who'd almost come to blows over the value of Freudian psychoanalysis (Chapter 4.iii.f). But because cybernetics had had little room for meaning, the *specific content* of anxiety neuroses and other personal phenomena was usually ignored. Even when it was addressed, by Gregory Bateson for instance, it was still considered in relatively general terms (4.v.d–e).

Symbolic computing seemed to offer a solution. Accordingly, GOFAI programs focused on personality, and personal belief systems appeared as early as the late 1950s. In 1962 they even enjoyed a dedicated conference at Princeton (Tomkins and Messick 1963). The main organizer was the psychiatrist Silvan Tomkins (1911–91), whose theory of "positive" and "negative" emotions—despite its subtle descriptions of phenomenology—owed as much to cybernetics as to Sigmund Freud (Tomkins 1963; cf. Sedgwick and Frank 1995).

The early birds, however, didn't catch the worm. Even considered as deliberate idealizations (inclined planes for psychologists), these systems were grossly oversimplified—although they did indicate some of the issues that would eventually need to be faced. By the time that Ulric Neisser defined cognitive psychology, albeit with "a definite humanistic quality", the initial interest in modelling personal matters had abated. With only a handful of exceptions, the practice was put on ice for a quarter-century. (However, a *cognitive*, though not explicitly computational, approach to psychoses and anxiety disorders would soon arise, and become increasingly influential: e.g. Beck 1976; Morrison 2002; D. M. Clark 1996.)

By the millennium, the ice had thawed. The computer modelling of emotions was now hotly debated even in the media. This was due in part to the 2001 Spielberg–Kubrick film *AI*, whose child-android David was an (unworthy) successor to HAL of *2001*. More importantly, it was due to popular writings by neurologists—whose general moral was that the human personality *can't* be compared with computers in any significant way. But that moral was disputed: the turn of the century saw computational theories of grief and mourning, and of hypnosis and clinical psychopathologies too. Cognitive science had come full circle, returning to topics discussed in its earliest days.

In brief, this section justifies the claim made in Chapter 1.ii: that "cognitive science" isn't merely "the science of cognition". And in comparing the early examples (described in subsections a–c below) with today's efforts (subsections d–i) it shows how far computational theorizing about personal matters has advanced in fifty years.

a. The return of the repressed

Psychiatry at mid-century relied heavily on the consulting-room couch, not only for diagnosis but also for therapy. Kenneth Colby (1920–2001), a psychiatrist and Freudian analyst as well as a GOFAI pioneer, tried to move the action from couch to computer.

In the late 1950s, at Stanford, he started work on a 'neurotic' program, which he improved over almost ten years (K. M. Colby 1963, 1964, 1967; Colby and Gilbert 1964). Unlike Ulrich Moser's group in Zurich, whose Freudian simulations would focus on 'energy-flow' (Moser *et al.* 1968, 1969), Colby tried to tackle *meanings*.

The program's "beliefs" were based on one of Colby's long-term psychoanalytic patients: a woman unable to admit to her unconscious hatred of her father, whom she believed to have abandoned her. Aiming to clarify Freudian ideas about repression, he modelled eight of the defence mechanisms listed in Chapter 5.ii.a. (Rationalization wasn't one of them, for reasons explained in subsection c, below.) Each simulated belief carried a number, reflecting its emotional importance (cathexis). Both *I must love father* (a prime diktat of the superego) and *I hate father*, for instance, had high emotional import. Each defence mechanism was selected in a particular class of anxiety-arousing circumstances, and each transformed the troubling belief in a different way. For example, applying Projection to *I hate father*, required merely that subject (*Self*) and object be switched, giving *Father hates me*. For Displacement, the program had to find an analogue of the father to be the new Object, and also weaken the verb (perhaps giving *I see faults in Raymond*).

Many psychological questions were raised, and some clarified, by Colby's early work (for details, see Boden 1977: 22–63). But few, if any, were answered. Even the best-developed version of the neurotic program, despite being complex for its time, was far too simple to advance understanding much.

Its major drawbacks were its crudeness in modelling anxiety and analogy. Anxiety can't be captured by semantic-clash-plus-numbers. It arises from a complex computational architecture that wasn't even minimally modelled until the end of the century (see subsection f). As for analogy, Colby's program—by means of the FINDANALOG procedure—treated this simply as *sharing x properties*, where *x* is a number that varied with circumstances. But even to explain intuitively obvious instances of neurotic displacement would require a much better theory of analogy than this. And psychoanalysts are able to see analogies where most of us cannot (even if they sometimes overdo it).

In the late 1960s, Colby switched from neurosis to paranoia. Although there was 85 per cent agreement between psychiatrists in diagnosing paranoia, there was less unanimity on its explanation. Colby drew partly on Freudian theory, but even more heavily on Tomkins's (1963) analysis of the emotions. The central idea was that paranoia is rooted in defence mechanisms whose goals are to protect the self against shame, the core negative emotion. This (so he said) accounted for common phenomena that otherwise seem anomalous, such as the occurrence of paranoid reactions after false arrest or on the birth of a deformed child (K. M. Colby 1977).

Colby saw his theory of paranoia as a significant "reconceptualization" of the syndrome. He viewed it "as a mode of processing symbols", where the patient's remarks "are produced by an underlying organized structure of rules and not by a variety of random and unconnected mechanical failures". That underlying structure consists of "an algorithm, an organization of symbol-processing strategies or procedures". In order to change it (to 'cure' the paranoia), "its procedures must be accessible to reprogramming in the higher-level language of the algorithm". In general, he said, "other types of psychopathologies might be viewed from a symbol-processing standpoint" (K. M. Colby 1975: 99–100). So paranoia was just an example: mental illness as such was the ultimate explanatory target.

To illustrate his theory, he wrote a program called PARRY (K. M. Colby *et al.* 1971; Colby 1975, 1981). This was a language-using system describable as ELIZA-with-attitude (10.iii.a), where the attitude was systematically grounded in its "anxieties" and "beliefs". It responded in a paranoid fashion to various keywords related to its particular danger themes. Again, these recalled one of Colby's patients: a man whose delusions featured dishonest bookmakers who claimed that he owed them money, and set the Mafia on him when he refused to pay.

PARRY soon became notorious for "passing the Turing test". Strictly speaking, this was inaccurate—as Colby realized (see 16.ii.c). But it's true that PARRY's responses were often diagnosed as "paranoid" by doctors unaware that they were dealing with a program.

Colby's aim in writing PARRY had been practical as well as theoretical. He hoped it might help in making therapeutic decisions, and also in training student therapists before they were let loose on real patients (K. M. Colby 1976). In effect, then, he was designing a virtual-reality system for training in psychotherapy, much as today's VR engineers are designing simulated 3D brains for training brain surgeons (see 13.vi.b). He'd had similar practically oriented hopes for PARRY's neurotic ancestor (K. M. Colby *et al.* 1966).

After moving to UCLA in 1974, he started using an ELIZA-like program to conduct initial diagnostic interviews at the Los Angeles Veterans' Hospital. The transcript would be discussed by the patient with Colby himself. Many people, he discovered (personal communication), found it easier to express their anxieties to a non-judgemental computer program than to an unknown psychiatrist (these were *initial* interviews).

In 1989 he retired to found a company marketing therapeutic software. One program was designed to help people decide whether they might be clinically depressed, and to advise them on what to do (e.g. to go to see a human doctor). Its seven "Lessons" dealt, for instance, with negative self-comparisons; mood and value; ideal standards; reprogramming oneself; and suicide and antidepressant drugs (K. M. Colby and Colby 1990).

The Manual opened with these three questions: "Are you sad for days or weeks at a time? Do you have a low opinion of yourself? Do you feel hopeless and helpless?" This was casting a wide net (fully 25 per cent of the US population, so readers were informed, are depressed at some time in their lives). And the Introduction closed with this seductive reassurance:

> The dialogue mode offers the world's first-ever computer therapeutic learning program for depression which allows you to express yourself freely in your own words *and which responds accordingly in natural language*. The program's dialogue responses are designed . . . to facilitate therapeutic learning through the emotional arousal stimulated by *real-life conversation*. (K. M. Colby and Colby 1990: 2; italics added)

If you know anything about ELIZA (10.iii.a), you'll know that this was a highly tendentious, not to say deceptive, way of describing the matter. True, the users could express themselves freely. And true, doing that—and reflecting on it (partly due to the promptings of the program itself)—might help to increase their self-knowledge and/or self-confidence. But a "real-life conversation" this was not. The program's responses were all essentially empty—and not just because *all* NLP responses are essentially empty (see 16.v.c). They were empty *even compared with other NLP systems*, for by the time this product was marketed there were many programs whose interlinking of verbal concepts was very much richer (see 9.x–xi).

The OVERCOMING DEPRESSION program was used on the mainland and in Hawaii, by the US Navy and the Department of Veterans Affairs (Chang 1993). In addition, from 1992 it was sold on the High Street (as "DEPRESSION 2.0")—to individuals who would be using it (on an IBM PC) without supervision from a psychiatrist. As one would imagine, the press had a field-day. Challenged by one journalist, Colby retorted provocatively that programs could be better than human therapists because "After all, the computer doesn't burn out, look down on you, or try to have sex with you" (quoted in Turkle 1995: 115).

This was just the sort of thing which the computer scientist Joseph Weizenbaum, the author of ELIZA, had denounced as "obscene" (see 11.ii.e). There was little love lost between him and Colby. Indeed, I witnessed a heated public interchange between them at the third international AI conference, held at Stanford in 1973. I also had many private conversations with Colby (and one with Weizenbaum) in 1972 and 1973. Each was bitter in his comments about the other.

This antagonism was partly grounded in a priority dispute concerning ELIZA. In the early 1960s Colby had written a similar program called DOCTOR. (DOCTOR was soon supplanted by a more interesting "dialogue" program, reported by Colby at the very first international conference on AI: K. M. Colby and Smith 1969. That system was less empty-headed than either DOCTOR or ELIZA: like PARRY, it had an artificial belief system on which it drew in responding to the human's remarks.) The brief publication that introduced DOCTOR to the world appeared shortly *before* Weizenbaum published on ELIZA (K. M. Colby *et al.* 1966; Weizenbaum 1966). It's true that they'd discussed these matters together before either of those publications appeared. But Colby certainly felt that the priority was his (personal communication). However, it was ELIZA who/which became famous.

Mostly, however, Weizenbaum's antipathy was due to his worries about "dehumanization". When he learnt of Colby's plans to develop therapeutic programs, he was appalled. Such uses, he said, would offer patients a "profoundly humiliating" self-image (1972; 1976, esp. 268–70).

Colby's response was that it's "dehumanizing" to herd thousands of patients into understaffed mental hospitals where they will hardly ever see a doctor (K. M. Colby 1967: 253). For him, the proof of the pudding was in the eating. By the 1980s, when computers had actually been used in psychiatric contexts, he could look at the clinical facts. Investigating computer-assisted therapy for stress, he found clear advantages and no obvious bad side effects (K. M. Colby *et al.* 1989).

Whether there were *non-obvious* bad side effects is another matter. The Third Force psychiatrist Rollo May had already reported that his patients' ability for self-control had been damaged by science's tendency "to make man over into the image of the machine" (May 1961: 20). Science in general, and behaviourist psychology in particular, had no room for concepts such as freedom, deliberation, purpose, and choice. Given the high cultural status of science (which would soon be resisted by the counter-culture: 1.iii.c–d), the result was that these concepts were insidiously downgraded—not to say denied. And, said May, if people didn't believe themselves to be capable of autonomous choice then they were unlikely to try to engage in it—with the result that they fatalistically accepted their current life situation instead of trying to change it. This "sapping of willing and decision" was bad news, since—like all the Third Force psychologists—he took people's conscious decisions to be crucial if therapy was to succeed (see 5.ii.a).

Colby's AI clinician may have had a similar effect. Some unsupervised users, perhaps with a tenuous grip on reality in the first place, may have imagined that there was genuine understanding there. And/or they may have tacitly inferred that they themselves were "no more than a machine"—where their concept of "machine" *did not* include the subtleties of cybernetics or computation. If so, their self-image and sense of personal responsibility could have been undermined. (This is true even though many cognitive scientists see their approach as *supporting* ascriptions of "freedom" to human beings: see subsection g, below.)

Whether this would actually happen or not would depend to some degree, of course, on the prevailing Zeitgeist. In the early 1970s, when Colby's interview program was first used in a real-life clinical setting, computers were still hugely unfamiliar and the counter-culture was at its height. But the Zeitgeist can shift. Sherry Turkle (1948–), another psychoanalyst (and a colleague of Weizenbaum's at MIT), noticed over fifteen years that there was a change in the attitudes of MIT students invited to discuss computer psychotherapy.

At first, there was heated disagreement with the project: almost all the students were highly sceptical. By 1984, the Colby fans were still a minority, although there were twice as many. But by 1990 the youngsters "saw nothing to debate" (Turkle 1995: 115). They now regarded therapy programs as like self-help books (some of which are actually helpful, some not), but better—and very much cheaper, and less threatening—than a human therapist. Turkle reports that this was part of a widespread disillusion with psychotherapy in general and Freudian theory in particular: "In the main, [MIT] students in the 1990s did not consider a relationship with a psychotherapist a key element in getting help" (p. 116).

MIT students, of course, aren't a representative cross-section of society. It would be interesting to know what the general public, today, would think of Colby's clinical aims. (Or, for that matter, how they would react to current attempts to use "social" robots to help autistic children, not least to mediate communication between the child and other human beings: see Chapter 13.vi.d.) As for Colby's theoretical aims, they were greatly premature—but, as we'll see, not intrinsically absurd.

b. Argus with 100 eyes

Walter Reitman (1932–) started modelling the mind at much the same time as Colby, but his approach was very different (Reitman 1963; 1965, esp. chs. 8–9; Reitman *et al.* 1964). He saw personality as "a problem-solving coalition" (1963: 81–6), incorporating multiple "distinct and perhaps conflicting needs and motives". And instead of relying on pure GOFAI sequentialism, he constructed a simple version of what would now be called a hybrid system (Chapter 12.ix.b).

His Argus program addressed Neisser's (1963) complaint that current problem-solving computers were too single-minded, non-distractable, and unemotional. This complaint, said Reitman, is relevant because "most theories of personality . . . allow for the possibility of several independently originated activities simultaneously under way" (1963: 77). The system as a whole must organize those simultaneous motives and activities sensibly. If it can't, various types of psychopathology will ensue.

The task for the simulator, then, is to describe the "intrapsychic communication" involved:

> [The theorist must specify on his code sheets] the manner and form in which information, commands, and requests at one level in the system are transmitted elsewhere . . . [His difficulties are increased if he has to consider] a system in which sub-systems are able to do such things as induce concealment or refuse access to information which other systems require to achieve their aims. (Reitman 1963: 73, 85)

These things must be specified because the computer modeller can't assume that the right hand knows what the left hand is doing: the right hand must be told. Today, this is a banality (though no less important for that). In Reitman's time, it wasn't.

Reitman, in the early 1960s, was a colleague of Newell and Simon at the Carnegie Institute of Technology. He was greatly influenced by them, not least by their interest in trying to match the tiniest details of behaviour (1965: 32–3). But Argus, despite being written in their new language IPL-V (10.v.b), wasn't another GPS.

Named in honour of the mythical Argus with 100 eyes, this GOFAI program (like Pandemonium) was conceptualized as a parallel system involving many simultaneously active elements. Reitman compared these to Donald Hebb's cell assemblies, but confessed that he hadn't been able "to imagine any way in which a system consisting entirely of Hebbian cell-assemblies might be made to [account for goal-directed thinking]" (1965: 208). (A similar failure of imagination still besets us today: Chapter 12.viii–ix.) So he implemented a "mixed" system, "linking a limited sequential control to an underlying structure of active [Hebbian] elements" (p. 209).

Moreover, Argus wasn't sequentially single-minded, like GPS. While it was following goal A, it could be distracted onto goal B by some content (idea) that had just arisen in its processing. That distraction could be temporary or permanent. At each point, it had to decide which of the competing alternatives it would follow.

Reitman's aim was to model how people recover from interruptions of various kinds. For instance, someone who's been distracted by an anxiety-ridden idea can usually pick up where they left off. Sometimes, the idea can be ignored altogether—but not if it's urgent and/or important (see subsection f below), nor if one's already in a highly emotional state. Again, something being taken for granted may turn out be false, so that one must reorganize one's thoughts about how to reach the goal (see 13.i.a). These self-organizing recoveries, Reitman argued, require top-down influences: what's needed is "a sequential executive able to some extent to modulate and focus the changes taking place in the cognitive structure" (1965: 203).

Argus illustrated what he meant. One of its first tasks was to complete very simple analogies. For instance, given *hot:cold::tall:(wall, short, wet, hold)*, the program would choose *short*. Its basic strategy is shown in Figure 7.1.

As the caption indicates, the activation states of the "Hebbian" elements (implemented as list structures) varied throughout the run (1965: 212 ff.). The numbers representing activation and inhibition weren't summed: a single element, or concept, could be represented as high on *both*. The executive would recognize this as a conflict, and decide (according to the current strategy and activation patterns) whether it should be resolved immediately, postponed, or tolerated. The elements could be affected by non-firing too: long-quiescent concepts wouldn't fire spontaneously, although they could be activated directly by some other element. These continuous state changes meant that a problem might be addressed differently at different times.

Many analogy problems, however, are more difficult than this. Reitman pointed out that, faced with *Sampson[sic]:hair::Achilles:(strength, shield, heel, tent)*, someone could get the right answer in several different ways (1965: 227–8). These include reliance on word association (Achilles' heel), superordinate category (hair and heel as body parts), and the concept of vulnerability (not straightforward: Samson's hair was his strength, Achilles' heel his weakness). In other words, several strategies are available for completing this analogy correctly. Which one will be chosen, and why? And will it be followed through to the bitter end, or abandoned (temporarily/permanently) in mid-stream?

Of course, Argus could have been set up beforehand to solve this problem just as it solved the *hot:cold::tall:?* puzzle. But Reitman's point was that, at least for the third strategy, humans have to think about it. To enable Argus to do that would require (among other things) giving it a mass of general knowledge that's not tagged for any particular goal, and wide-ranging ways of recognizing the relevance of specific items in it. Accordingly, he hoped to develop Argus so as to study cognitive dissonance and language comprehension (1965: 230–53), and to explore individual differences—between people, and between one person's thinking at different times.

Argus didn't go unnoticed. For instance, Neisser (1967: 299–300) praised it as a rare attempt to combine parallel and sequential processing (see Chapter 12.ix.b). And I referred to it in outlining a computational version of William McDougall's personality theory, which was broadly consonant with Reitman's (Boden 1965: 14, 16; 1972: 227).

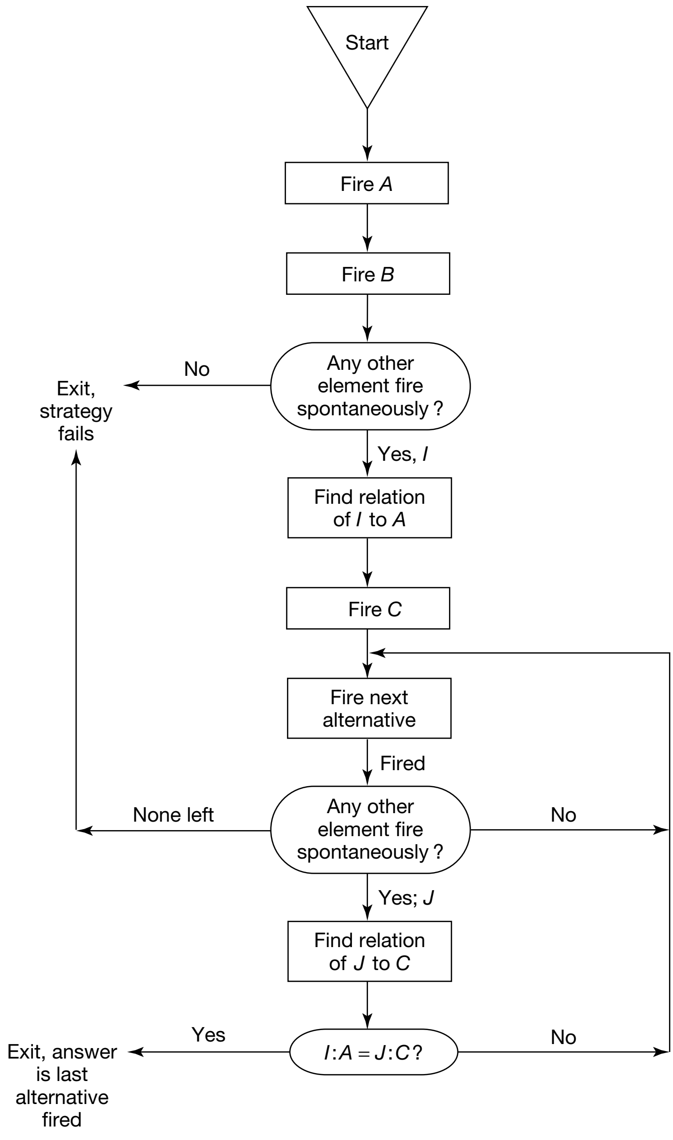


FIG. 7.1. Rudimentary strategy (slightly simplified) for analogy problems of the form A:B::C: (W, X, Y, Z). When an element is *fired*, activation and inhibition increments are added to related elements, altering their states. Redrawn with permission from Reitman (1965: 211)

However, the program was soon near-forgotten. It was ignored in the ice-breaking textbook of cognitive science (Lindsay and Norman 1972), despite the authors' enthusiasm for the parallelist Pandemonium. This was partly because it wasn't tied closely to experimental evidence. More to the point, Reitman's hopes for future versions of his program hadn't been fulfilled—or even approached (1965, ch. 9). The problems were far too difficult.

c. From scripts to scripts

Yet another early bird was Yale's Robert Abelson (1928–). He did more than anyone else to bring (not just personal but) interpersonal issues into computational theory.

A few others, to be sure, were modelling interpersonal issues. In anthropology, for instance, Anthony Wallace (1965) was applying TOTE units to socially organized behaviours such as driving to work (Chapter 8.i.a). In psychology, the best-known were John and Jeanne Gullahorn (at Berkeley's School of Business Administration), whose HOMUNCULUS program was featured in the widely read *Computers and Thought* (Gullahorn and Gullahorn 1963, 1964, 1965).

Their approach was very different from Abelson's, being based on George Homans's positivistic social psychology. Homans relied on five "axioms", including (2) *The more often within a given period of time a man's activity rewards the activity of another, the more often the other will emit the activity*, and (5) *The more to a man's disadvantage the rule of distributive justice fails of realization, the more likely he is to display the emotional behavior we call anger* (Homans 1961: 64, 75). No mental causation, no psycho-logic . . . just behaviourism, pure if not simple. In short, the Gullahorns offered a computer model of psychology but *not* an example of computational psychology.

Abelson himself was a convert to computationalism. But this wasn't a Damascene conversion. It happened gradually, as he became increasingly dissatisfied by the constraints of "Newtonian" theories (Chapter 5.i.a).

By the late 1950s, he was already acclaimed as a leader of operationalist social psychology. With Carl Hovland and others, he'd been studying the *coherence* of beliefs—assessed not by logic but by "psycho-logic", in which values and emotions play a major role (Rosenberg *et al.* 1960). So *anger*, for instance, might render a certain interpersonal response psycho-logical, if not strictly logical.

Psycho-logic was also central to Abelson's then popular theory of "cognitive balance". Inspired by the Gestaltist Fritz Heider (see 5.ii.b), this concerned how attitudes are changed by value clashes (Heider 1958; R. P. Abelson and Rosenberg 1958). Suppose a highly valued person says something you disagree with: how does this alter your attitude to them and/or to the proposition they've asserted? To cut a long story short, Abelson held that the high value is lowered, and the low value raised. *Just how much* depended on numerical calculations of the "balance" between positive and negative (lots of positivistic brownie points there!).

Attitude change—and resistance to it—was a key topic for 1950s social psychology. The central discussion was Leo Festinger's theory of cognitive dissonance (Festinger 1957; Brehm and Cohen 1962). This taught, for instance, that someone who has bought an expensive item will actively seek information that confirms its quality, while avoiding comparisons that favour alternatives. By the mid-1960s, Festinger's ideas were being described as "probably the most influential ideas in social psychology" (R. Brown 1965: 584).

Dissonance theory had swept the popular press too. For besides illuminating the power of advertising, it seemed to explain certain aspects of the strange behaviour of members of eschatological cults—and Christianity.

The newspapers had reported that a cult leader in the USA's mid-West was prophesying her hometown's destruction by flood. Her followers were abandoning their jobs, to enjoy life while they could. Festinger predicted that when the prophecy failed, the group's commitment would be *increased*, and 'reasons' found to 'explain' the unexpected non-event.

He was right. Having infiltrated the cult with two of his colleagues, he was present when the leader reported "a message from the Guardians on the planet Clarion" promising a midnight rescue by flying saucer some days later. The saucer's non-arrival prompted frozen shock (and frantic time checking), followed by fearful disappointment. But this didn't last. Before dawn, another "message" was reported (couched in suitably portentous quasi-biblical language—Festinger *et al.* 1956: 168 ff.). The flood, it announced, had been averted by the faith of the cult members. Immediately, they started spreading the happy news—even to the media, which initially they'd shunned.

For Abelson, however, even such telling anecdotes as this one weren't enough. Other psychologists had raised various 'factual' problems facing Festinger's theory, noting that we sometimes *favour* (a low level of) dissonance: think of Robert Herrick's poem 'A sweet disorder in the dress' . . . But Abelson was more interested in a theoretical problem, namely, that Festinger's approach predicted behaviour without really explaining it. (Similarly, Homans had predicted the "behavior we call anger" without really explaining it.) *How* someone comes up with this reason, or that one, wasn't considered. (Why say that faith had prevented the flood? Why not say that the Guardians had been mistaken? Or teasing?) The same was true of Abelson's own brainchild, balance theory. This said nothing about *how* evaluations can be changed, or *how* this can trigger a host of implications in the person's mind.

In the early 1960s, then, Abelson—shockingly—abandoned operationalism for mental processes. The focus of his address at the Princeton meeting was on inner information processing, aka thought. He focused on everyday cases of "hot" (value-laden) cognition whose emotional temperature is lower than it is in neurosis. And he described a program that applied *rationalization* and *denial* to input beliefs whose valuation clashed with pre-existing attitudes (R. P. Abelson 1963; Abelson and Carroll 1965).

Colby's neurotic program had modelled denial, but not rationalization—and that's no accident. For rationalization is more taxing. One must generate a belief with a different subject matter, not merely a logical/grammatical transform of the original. And the beliefs must be linked intelligibly. Abelson's "hot cognizer" selected one of three rationalizing subroutines: REINTERPRET FINAL GOAL, ACCIDENTAL BY-PRODUCT, and FIND THE PRIME MOVER. As these labels suggest (though over-optimistically: see 11.iii.a), this involved reasoning about cause and personal responsibility. His concern with these everyday linkages between beliefs would inform all of Abelson's later work.

Soon after Princeton, he developed an example he'd briefly mentioned there: the effect of different political attitudes on the acceptance of new information. Besides being a committed political campaigner for the Democrats (personal communication), Abelson was professionally interested in how politics works. For instance, he'd helped build a computer simulation of the strategies informing the recent US presidential campaigns (Da Sola Pool *et al.* 1965). Now, his IDEOLOGY MACHINE modelled the internal coherence of the thought processes of the Democrats' arch-opponent, Senator Barry Goldwater (R. P. Abelson and Reich 1969; Abelson 1973: 287–93).

Putting this in the terms that had recently been used in Bruner's *A Study of Thinking*, Abelson was investigating the categories, heuristics, and mental strategies employed by people with differing political viewpoints. Putting it (anachronistically) in the terms that would later be used by Philip Johnson-Laird, he was trying to define distinct mental models (iv.d–e, below).

What's especially relevant here is that he was beginning to see these categories/heuristics/models as *sharing* some underlying computational structure. Goldwater and his Democrat opponents favoured very different political dress. But according to Abelson, the contrasting fabrics were cut with similar computational patterns. Much as an anthropologist may seek universal structures and processes underlying a profusion of cross-cultural particularities (see 8.vi), so Abelson was doing this within a small part of his own culture.

When Goldwater ran for President, Democrat car stickers proclaimed, "In your guts you know he's nuts." (This was a riposte to the Republicans' "In your heart, you know he's right.") However, Abelson wasn't trying to ridicule him. On the contrary:

> The tendency to caricature and trivialize the motives and character of the enemy and to glorify—but also trivialize—the motives and character of one's own side has often been remarked by students of the human condition. My purpose is to try to anchor the phenomenon of ideological oversimplification in general psychological theory and data. (1963: 287)

Abelson's new model of attitude change was designed to generate politically plausible answers to these questions: *Is the news-report of event E credible? If and when E, what will happen? And what should political actor A do? When E happened, what should A have done? How come E? (That is, what caused E, or what is E meant to accomplish?) Sir, would you please comment on E?* (R. P. Abelson 1973: 290). For instance, how would Goldwater assimilate a news item such as "The East Germans have erected the Berlin wall" or "The USA has attacked neutral Vietnam"?—And how would the Communist leader Nikita Khrushchev respond to the very same items?

The program generated its answers by means of an information-processing mechanism called a *master script*. Abelson likened this to the Gestalt ideas of "good form" or "cognitive balance" (p. 289). The Goldwater version contained an internally coherent master script saying (among other things) that *if the Free world really uses its power then Communist schemes will surely fail* (p. 291). The seven core concepts of this master script—and, on Abelson's view, of Goldwater's political thinking—were: *Fuzzy liberal thinking*; *Our call to action*; *Free World paralysis*; *Use of Free World Power*; *Communist schemes*; *Free World Victory; Communist Victory*. Khrushchev's master script would contain partly different concepts, linked in somewhat different ways. Both statesmen, however, would accept the schematic belief that *The enemy attacks and subverts, and we defend, but never the other way round* (p. 290). Hence the problem, for Goldwater though not for Khrushchev, in assimilating and explaining the news item about the US attack on Vietnam.

To make this model work, Abelson had had to consider 'cold' cognition. He'd been forced to ask (again) how a belief can imply attributions of personal responsibility, or predictions of causal consequences. In 1970, then, his research shifted to embrace belief systems *in general*. He studied their internal structure, and how to integrate new information without compromising the system's core.

(Abelson had already dipped his toe into these waters, in his research on "implicational molecules": R. P. Abelson and Reich 1969. This work followed Heider's studies of 'personal' perception, mentioned in Chapter 5.ii.b. It was an early example of attribution theory, which became a prime topic in 1970s social psychology: e.g. Jones *et al.* 1972; Schmidt 1976.)

Very soon, he started to consider the internal structure of *intentions*, *plans*, and *roles* (1973). For he'd found that when his IDEOLOGY MACHINE generated implausible responses this was often due not to its factual ignorance but to its unstructured representation of these basic concepts. Much influenced by the cognitive science manifesto (6.iv.c), he outlined the ways in which two actors, each with their own agenda (plan), can intervene in the plans of others, whether to assist or to obstruct them. In other words, he explored the psychological structure of cooperation (and sabotage).

But he gave up, for a while, on computer programming. Like the manifesto authors, he now offered a schematic computational approach rather than a functioning model. That was no accident: the problems involved were way beyond the state of the art. It wasn't until the 1980s–1990s that AI researchers were able to model cooperation (and the lack of it) between autonomous mindlike agents (13.iii.d). When they did, they had to consider the problems of mutual plan-alignment that Abelson had sketched in this early essay.

Based on his analysis of plan-structures, Abelson defined a wide range of interpersonal concepts called *themes*. Examples included cooperation, betrayal, humiliation, victory, love (of which, more in subsection f), and many more (1973: 315–31). These weren't defined by dictionary entries, nor even by conceptual analysis. Rather, they were defined systematically, by placing what seemed to be the most appropriate ordinary-language words in the various locations available within a formal matrix. The dimensions of the matrix concerned actors' interests in, and abilities to influence, other actors' plans (see Figure 7.2).

	Influence of Actors		
Sentiments Toward Other	Neither Influences Other	One Influences Other	Both Influence Other
Some Positive, No Negative	Admiration	(T_1) Devotion (T_2) Appreciation	(T_3) Cooperation (T_4) Love
One Actor Negative	(T_5) Alienation (also, Freedom)	(T_6) Betrayal (T_7) Victory (also, Humilation) (T_8) Dominance	(T_9) Rebellion
Both Actors Negative	(T_{10}) Mutual Antagonism	(T_{11}) Oppression (also, Law and Order)	(T_{12}) Conflict

FIG. 7.2. A taxonomy of themes. Adapted with permission from R. P. Abelson (1973: 320)

In other words, he was exploring *a structured space of computational possibilities*. These interpersonal themes, and the "scripts" defined in terms of them (see below), identified crucial aspects of a wide range of intelligible/plausible human stories. (That's why I said, at the close of Chapter 6.iv.d, that what Bruner terms the "blah blah blah" of human narratives includes the matters addressed by Abelson.)

For example, the box labelled *betrayal* defined a theme in which "actor F, having apparently agreed to serve as E's agent for action A_j, is for some reason so negatively disposed toward that role that he undertakes instead to subvert the action, preventing E from attaining his purpose" (p. 323). This abstract schema covered many varieties of betrayal, definable by varying the relative power of F and E and/or the importance (to E) of the thwarted plans. So abandonment is a specially nasty form of betrayal, since only the strong can abandon and only the weak can be abandoned; by contrast, anyone can let down, or be let down (Boden 1977: 80–6).

Abelson's 1973 paper ended by defining a number of empirically familiar sequences-of-themes, called *scripts*. These were less specific than the "master scripts" used by the IDEOLOGY MACHINE. Instead of fears about Communist domination, they included everyday notions such as *rescue*, *blossoming*, *turncoat*, *end of the honeymoon*, *revolution*, *romantic triangle*, *alliance*, and *the worm turns*. The relevance to personal (and even political) matters is intuitively obvious. But Abelson defined these scripts as coherent successions of formally specified themes. One theme leads "naturally" to the next as the relations (of power, success, and/or sympathy) between the two actors change. He suggested that whereas themes are probably human universals, anthropology might show some scripts to be culturally specific (see Chapter 8).

From the mid-1970s on, Abelson's work became increasingly influential in computational psychology and GOFAI. I've found, however, that his name is less well known than his ideas, having been largely eclipsed by that of Roger Schank (1946–). Abelson had used Schank's Conceptual Dependency (CD) theory in his paper on belief systems, to formalize the notion of intention and to express various causal/teleological implications (1973: 293–310: see Schank 1973). Soon afterwards, when Schank moved from Stanford to Yale, the two men began a long-lasting collaboration.

They soon published a highly influential book, *Scripts, Plans, Goals, and Understanding* (Schank and Abelson 1977). These "scripts" weren't the same as the scripts of Abelson's 1973 paper (rescue, and the like), or as the master scripts of the IDEOLOGY MACHINE. Rather, they were defined as *stereotyped ways of behaving*—in a hamburger restaurant, for example. That is, they were closer to the concept of *role*.

The social psychologist Roger Barker (1963, 1968) had recently rejected roles *considered as internal mechanisms generating an agent's behaviour* in favour of automatic triggering by "situational" cues. So he spoke of the behaviour-triggering power of clothes, furnishings, buildings, and ritualistic behaviour—such as greeting. Simon himself had taken a similar position, in his 1969 fable of the ant (Section iv.a, below). However, most computational psychologists at the time ignored Simon's ant. (They ignored Barker too, because as a *non*-computationalist he took it for granted that behavioural triggers can function, without asking precisely how.) Even those who did take Simon's fable seriously tried to find 'antlike' ways of accommodating scripts (see Section iv.b–c).

Both psycholinguistics and NLP were hugely influenced by Schank and Abelson, despite CD theory's many shortcomings (Chapter 9.xi.d–e). Their ideas helped fuel experiments on psychosemantics, asking what implications people spontaneously draw on hearing a sentence (R. P. Abelson 1976, 1981*a*; see also Schank and Langer 1994). In a roundabout way, philosophy was lastingly affected too: it was the script-driven Yale programs which were mocked by John Searle in his notorious Chinese Room argument (16.v.c).

Within professional social psychology, where Abelson had already been a 'name' in the late 1950s, recognition was more patchy. He received Distinguished Scientist awards for Experimental (*sic*) Social Psychology in 1990 and for Political Psychology in 1996. But social psychologists in general were less ready to accept computational ideas than their 'cognitive' peers. (Even Bruner reneged eventually: see 6.ii.d and 8.ii.a.) Moreover, we saw in Chapter 6.i.d that many in the 1970s argued that their field lay within history or hermeneutics, not science. It was at that very time that Abelson published his accounts of scripts (in all three senses). In brief, he was following an intellectual stream very different from either of the then fashionable approaches to social psychology—namely, operationalism and hermeneutics.

Nevertheless, it was largely thanks to his own early influence that he was able to say, in 1973:

> The modern trend in both experimental and social psychology is away from a behavioristic emphasis upon stimuli and responses toward a Gestaltist focus on cognitive capacities and performances, with experimental psychologists talking of information processing and social psychologists of cognitive consistency and causal attribution processes. (R. P. Abelson 1973: 287–8)

The Newtonian ramparts listed in Chapter 5.i.a were rapidly crumbling.

d. Emotional intelligence

One of the high priests of GOFAI, namely Simon (1967), acknowledged early in the game that emotion is integral to intelligence. (He was responding to Neisser's 1963 criticisms of GPS: Chapter 6.v.b.) It's needed, he said, to set our priorities when choosing which goals to follow. Purely 'intellectual' reasoning tells us what can be done, and how, and what might happen next . . . but not *what to do*. (Compare David Hume's " 'Tis not contrary to reason to prefer the destruction of the whole world to the scratching of my finger": Chapter 2.x.a.)

However, Simon didn't follow this up. In both GPS and production systems (Section iv.b below), top-level goals were taken for granted and sub-goals chosen by reference to them. Emotion wasn't featured. In his massive book on problem solving, it was mentioned only four times—one of which was a historical reference to Abelson (Newell and Simon 1972: 887).

This wasn't a mere oversight, but a deliberate strategy. Since emotion operates "through the lens of the cognitive system", he said, "a plausible scientific strategy is to put our cognitive models in order before moving to the other phenomena" (Newell and Simon 1972: 8). A theory of "normal" thinking can ignore emotion, to a first approximation (p. 866). Indeed, *only one* of the 321 protocols observed in his study of mental arithmetic was a "clear example of the injection of emotionally toned behavior" (p. 206; cf. p. 166). Even his later SOAR models didn't prioritize emotions, although he still recognized their importance (Simon 1994*a*).

This attitude was typical. Emotion, having been fairly prominent in 1960s cognitive science, had fallen out of sight. (Within psychology in general, of course, it was still visible. It was being studied in cognitive as well as psychodynamic contexts, and in tiny babies as well as in adult subjects: see Section v.e, below.) It was superficially represented in Abelson-influenced NLP programs (e.g. Dyer 1983), because most stories contain emotion words. But (with a few exceptions, discussed below) the role of emotions in mental processing *in general* wasn't addressed.

Some philosophers saw this as no accident, believing that GOFAI-based cognitive science is *in principle* unable to deal with all aspects of emotion. One of the first to say this was Keith Gunderson (1935–). Gunderson wasn't opposed to GOFAI-based cognitive science, nor even to computational models of emotion, in so far as these dealt with aspects *other than* the conscious feelings involved. But he drew a distinction between "program receptive" and "program resistant" features (1963, 1968, 1971). Problem solving was said to be an example of the former, emotion of the latter.

The program-resistant features of the mind were its *qualitative* aspects, of which consciously experienced feelings of emotion were a special case. Gunderson saw these as lying outside the type of computational psychology favoured by Simon, because—so he said—they are features of the underlying neural mechanism in which our mental functions are implemented. Possibly, he believed, such features might be *simulated* in computers. However, the simulation problem wasn't a matter of writing programmed routines, but of "somehow imparting to the machine analogues of basic capacities *presupposed* by possible software strategies" (1981: 538; italics added). This, he said, might be in principle impossible, because of subjectivity (see Chapters 14.xi and 16.iv–v).

As for Colby's "conceptual" representation of feelings of shame in PARRY, this—said Gunderson—was no better than

> drawing a pineal gland, making a dot in it, and saying "This," pointing to the dot, "represents the human soul as it is joined to the human body". Well, of course it does, but that tells us nothing illuminating about the human mind, but rather something about how easygoing and unilluminating certain forms of representation can be. (Gunderson 1981: 538)

He added, however, that Colby might have made an important contribution to the "taxonomy" of paranoia, for one doesn't need to solve the mind/brain problem to do that. (Whether functionalism in general is defeated by conscious experience will be discussed in Chapters 14.xi and 16.iv–v.)

The philosopher Hubert Dreyfus went much further: even problem solving, on his view, couldn't be understood in AI terms (see Chapter 11.ii.a–d). He said hardly anything about emotions as such. But he argued that human *needs* were being treated by AI researchers as pragmatic "values", conceptualized as *additional properties* to be computed alongside others (Dreyfus 1972: 184–90). Rather, he said, our needs belong "to the structure of the *field* of experience, not the objects in it" (p. 186).

It followed (although Dreyfus didn't explicitly say so) that one should reject any account of emotion, such as Simon's, which saw its role as prioritizing values during planning. In addition, Dreyfus stressed the role of the *body* in our psychology, and complained that GOFAI ignored this (1967; 1972, ch. 7). Again, it followed (though he didn't say so) that if emotions were thought of—as they generally were—as being somehow closer to the body than rational thinking is, they would inevitably be played down by a GOFAI-inspired cognitive science. Dreyfus's critique is considered in Chapters 11.ii.a–d and 16.vii.a. Here, the point to note is that he *wasn't* the leading philosophical critic of *computational accounts of emotion*, because he said so little about it. Rather, the main critics were Gunderson and (especially) John Haugeland (1945–).

Haugeland's doubts about cognitive science's ability to model affect drew more attention from the field than Gunderson's earlier criticisms had done. That was partly because his critique appeared as a target article in the new—and groundbreaking—journal *Behavioral and Brain Sciences,* or *BBS* (see 6.v.c).

In the 1970s Haugeland was excited by the promise of "cognitivism", by which he meant "roughly the position that intelligent behavior can (only) be explained by appeal to internal 'cognitive processes,' that is, rational thought in a very broad sense" (1978: 215). Later, he would cross the philosophical divide and opt for neo-Kantianism, becoming an enthusiastic advocate for Dreyfus (Chapter 16.vii.c). In fact, the acknowledgements of his *BBS* paper already mentioned "more of a debt than I can properly express to the inspiration and constant guiding criticism of H. L. Dreyfus". But even when he was still prepared to give cognitive science house room, he had misgivings about emotions and moods (1978, sect. 7). "I cannot prove that Cognitivist accounts of these phenomena are impossible," he said, but he thought their possibility was "dubious".

For Haugeland, cognitivist accounts were even less likely to succeed for moods than for emotions. He argued that emotions might be no more problematic for cognitivism than the "pre-cognitive" senses are. He was happy to allow that the bodily basis of conscious feelings and sensory images might be delivered as representational "input" to the information-processing mind, which would then focus on the cognitive content of the emotion (gratitude, for instance). But moods are different, he said.

On the one hand, they're "pervasive and all-encompassing" in a way that feelings aren't:

> The change from being cheerful to being melancholy is much more thorough and far-reaching than that from having a painless foot to having a foot that hurts. Not only does your foot seem different, but everything you encounter seems different . . . greyer, duller, less livable. (Haugeland 1978: 223)

On the other hand, moods are neither quasi-linguistic (representational) nor rational. This was a puzzle, even a paradox. Since they "permeate and affect all kinds of cognitive states and processes", they can't be theoretically segregated from cognition, as conscious experiences (arguably) can. Nevertheless, "they don't seem at all cognitive themselves".

One didn't have to be a professional philosopher to believe that affect and computers don't mix. Young children in the late 1970s who'd had access to MIT's computers for some years, in school and/or at home, had no qualms about saying that computers are intelligent, and can think. But they insisted that they don't have feelings, that they don't "care"—in short, that they don't have emotions (see 16.ii.c).

However, they weren't saying that computers *aren't as intelligent as they might be*, because they lack emotions. Nor was Haugeland saying that a computer's intelligence is compromised by its lack of moods. Affect was one thing, intelligence another. They were even opposed, in the sense that emotions were assumed to cause irrationality: love, after all, is blind. In short, "emotional intelligence" was seemingly a contradiction in terms.

By the late 1990s, that had changed: emotional intelligence had become a focus of research across the whole of cognitive science. (George Mandler sees the 1990s "tidal wave" of emotion studies as having begun with a "watershed conference" in Stockholm in 1972, at which the link with *cognition* was given due prominence for the first time: 2002*c*: 231–2.)

The new century saw several interdisciplinary conferences on it, including one organized in 2001 by the Royal Institute of Philosophy (D. Evans and Cruse 2004). For the philosophers, having largely ignored emotion for years, now started paying attention (e.g. D. Evans 2001; Prinz 2004*a*,*b*). Many psychologists got involved. For example, Paul Ekman linked the facial expression of emotions, which he'd been studying since the early 1970s (e.g. Ekman 1979), with the cognitive phenomenon of lying (Ekman 1985/2001, 1992, 1998; Ekman and Davidson 1994). AI workers got in on the act too, as we'll see (e.g. Picard 1997; cf. Sloman 1999; Picard 1999). Last but not least, the neurologists had plenty to say.

The topic even became trendy with the public at large, because of several trade books written by neurologists. Antonio Damasio's account of *Descartes' Error* (1994), for example, was so popular that it had appeared in twenty-three languages by 2002, and his 3-year-old book on feeling (1999) had already reached eighteen. Other widely read neurological accounts of emotion and/or the self came from Joseph LeDoux (1996) and Gerald Edelman (1992, chs. 17–18).

This neuroscientific research fed on, and fed, the 1990s upsurge of interest in the tricky topic of consciousness (discussed in Chapter 14.x–xi). But the psychological *function* of emotion was also at stake, and that's our concern here. "Descartes' error", said Damasio, had been to ignore the role that emotions (and the body) play in reason.

One case study was much cited: Damasio's patient "Elliot". Prefrontal lobe damage had left this man's intellectual intelligence intact. He had the normal stock of knowledge, including the social/personal concepts and scripts described by Abelson, and was aware of moral principles too. Moreover, he could compute sub-goals, compare possible consequences, construct contingency plans, and even perform individual sub-tasks with no trouble at all—often, better than most of us. What he couldn't do was choose sensibly between alternative goals, stick with a plan once he'd chosen it, or assess other people's motives and personality effectively. As a result, his personal and professional life, and his financial affairs, disintegrated in a series of bizarre disasters.

As Damasio put it:

> The tragedy . . . was that [Elliot] was neither stupid nor ignorant, and yet he acted often as if he were. The machinery for his decision making was so flawed that he could no longer be an effective social being. (1994: 38)

> After [many] tests, Elliot emerged as a man with a normal intellect who was unable to decide properly, especially when the decision involved *personal or social* matters. (p. 43; italics added)

Elliot was unable to choose effectively, or he might not choose at all, or choose badly . . . As we are confronted by a task, a number of options open themselves in front of us and we must select our path correctly, time after time, if we are to keep on target. Elliot could no longer select that path. Why he could not is what we needed to discover. (p. 50)

The germ of the answer was already available:

Elliot was able to recount the tragedy of his life with a detachment that was out of step with the magnitude of the events . . . Nowhere was there a sense of his own suffering . . . He was calm. He was relaxed . . . He was not inhibiting the expression of internal emotional resonance or hushing inner turmoil. He simply did not have any turmoil to hush. (p. 44)

I never saw a tinge of emotion in my many hours of conversation with him: no sadness, no impatience, no frustration . . .

Later, and quite spontaneously, I would obtain directly from him the evidence I needed. [On being shown pictures of ghastly events] he told me without equivocation that his own feelings had changed from before his illness. He could sense how topics that once had evoked a strong emotion no longer caused any reaction, positive or negative. (p. 45)

In short, Elliot's disabling predicament was "*to know but not to feel*" (p. 45).

The case histories of twelve other brain-damaged patients, said Damasio (p. 54), provided further evidence for (or illustration of: see below) the fact that intellect isn't rationality. As Abelson might have put it, all-beliefs-and-no-attitudes doesn't make a rational system. Our powers of reason depend on emotional valuation to set (and to retain) overall priorities, and to guide decisions at choice points in the plans that structure our behaviour. In short, MGP shouldn't have neglected the *Image* (Chapter 6.iv.c).

Damasio suggested that various regions of the brain underlie some or all emotions. And he outlined a theory of "somatic-markers": complex physiological responses that act as clues to the likely outcome—advantageous or not—of specific actions (1994, ch. 8). In short, "high reason" (which René Descartes had divorced from the body) was replaced by body-based judgement.

Because of its stress on emotion and the body, Damasio's work was widely regarded—not least by the general public, always ready to hear that the mind/brain *is not* like a computer—as a knock-out blow to computational theories of mind. Simon's long-standing argument that emotion is needed for goal setting was forgotten.

That was understandable, since Simon had seemingly forgotten it (or anyway, ignored it) too. But whether Damasio's—or anyone's—recent neurological research really was a knock-out blow is another matter. When his readers dismissed computational psychology, with a sigh of triumph or relief, they did so without knowing that important relevant work *had already been done*.

e. Architect-in-waiting

During the last quarter of the century, and despite the widespread neglect of emotion in cognitive science at that time, a few computationalists were working steadily on emotion and personal phenomena. They were doing so in the context of *mental architecture* in general.

Mental architecture is the overall structure of the mind (and/or possible minds), considered as a *virtual* machine—including the potential for various types of processing within and between the component parts. These theorists painted the mind with a broad brush, for claims about mental architecture need not involve hypotheses about specific processing details (although some do, such as Newell's SOAR: see Section iv.b, below). So they asked, for example, what general types of computation are involved in emotion, and how emotions fit into the overall computational system. As we'll see, however, their work wasn't being taken seriously by (most of) their peers.

The foremost examples of architectural theorists focusing on emotion and personality were Marvin Minsky (1927–) at MIT, and Aaron Sloman (1936–) at Sussex—and after 1991 at Birmingham. (Minsky was still communicating with Seymour Papert, but Papert's own research was now even more focused on educational matters: see 10.vi.) Their work is discussed, respectively, in this subsection and the next.

In addition, Daniel Dennett, who was much influenced by Minsky, sketched a computational account of the construction of the self, seen as "a center of narrative gravity" that unifies and directs thought and action (1991, ch. 13; and see subsection g, below). And the neuroscientist Michael Arbib, who described mental architecture in terms of *schemas* (Chapter 14.vii.c), claimed to have shown "how the hundreds of thousands of schemas in a single brain may cohere to constitute a single person" (1985, p. viii; cf. Arbib and Hesse 1986, ch. 7). But he did this only in the sketchiest terms. Minsky and Sloman, by contrast, tried to specify some relevant schemas and their interactions.

Minsky was already considering architectural questions by the late 1950s. He argued, for instance, that a creature capable of answering a question about a hypothetical experiment without actually performing it must, as Kenneth Craik had said (Chapter 4.vi.b), possess knowledge in the form of symbolic models of the world (Minsky 1965, sect. 2). Moreover, any creature capable of answering questions about itself *considered as an intelligent system* (so excluding questions like *How tall am I?* but including *What are my current goals and resources?*) would need to construct a model of itself—again, considered as an intelligent system, or mind. This self-model would omit some of the detail present at the level being modelled (hence giving rise to the empty illusion of 'free will': sect. 8), and would distinguish to some extent between the creature's physical body and its modelling activities (hence encouraging mind–body dualism: sect. 5).

The same—he said—would apply to successful AI systems, when they were eventually constructed:

> We should not be surprised to find [intelligent machines] as confused and as stubborn as men in their convictions about mind–matter, consciousness, free will, and the like. For all such questions are pointed at explaining *the [invisible] complicated interactions between parts of the self-model*. A man's or a machine's strength of conviction about such things tells us nothing about the man or about the machine except what it tells us about his model of himself. (Minsky 1965, sect. 9; italics added)

As for what those "complicated interactions" might be, Minsky couldn't say. Thankfully, they're not introspectible.—"Thankfully", because (as Patrick Hayes later put it) if they were, we'd all be "cast in the roles of something like servants of our former selves, running around inside our own heads attending to the mental machinery which currently is so conveniently hidden from our view, leaving us time to attend to more important matters". As Minsky remarked in response to Hayes, consciousness is marvellous "not because it tells us so much [about how we manage to think of a name, for instance], but for protecting us from such tedious stuff!" (Minsky in preparation, sect. 4-3).

The downside is that *if* we want to know how we manage to think of a name, hammer a nail, or write a love letter, it's very difficult to find out. In the mid-1960s, it seemed to Minsky that a creature's knowledge is recursively contained in its model-of-its-knowledge (if it has one)—and vice versa. This suggested:

> first, that the notion "contained in" is not sufficiently sophisticated to describe the kinds of relations between parts of programlike processes and second that the intuitive notion of "model" used herein is likewise too unsophisticated to support developing the theory in technical detail . . . *An adequate analysis will need much more advanced ideas about symbolic representation of information-processing structures*. (Minsky 1965, sect. 3; italics added)

Much of his later work would be an attempt to develop that "adequate analysis", using approaches from both connectionism and GOFAI (see 12.iii).

By the late 1970s, he was developing an architectural theory of the human mind as a whole. Emotions were briefly discussed in his trade book *The Society of Mind* (Minsky 1985), and at the turn of the century became the prime focus of *The Emotion Machine.* (As we'll see in Chapter 12.iii.d, the draft of *The Emotion Machine* has been in continuous change for nearly ten years; the quotations given in this section date from April 2002.)

In both books, Minsky paid homage to Freud. In 2002 he said: "few researchers in 'cognitive science' yet appreciate Freud's idea: that 'thinking' is a collection of schemes for appeasing both instincts and ideals" (in preparation, sect. 1-5). In Minsky's terminology, he assimilated the superego to "learned values, censors, and self-ideals", the ego to "conflict-resolving processes", and the id to "innate, instinctive wishes and drives". And this terminology, in turn, was interpreted by reference to a wide range of specific computational mechanisms, drawn from nearly fifty years of AI research. Among other things, these helped to make explicit the complex interactions within and between the various 'levels' of the mind.

Minsky was (is) nothing if not ambitious: in April 2002, the opening section of *The Emotion Machine* was called 'Falling in Love'. He didn't attempt a conceptual analysis of *love* (cf. Fisher 1990, and subsection f, below). But he did point out that personal love (of a parent for a child, or a child for its parent, between friends or lifelong companions, or for a group and/or its leader . . .) differs from other things called love (a patriot's allegiance to country, a convert's adherence to doctrine, a scientist's passion for finding new truth . . .) (Minsky in preparation, sect. 1-2). And he followed Sloman (see below) in listing several—very different—psychological features normally associated with personal love, including: grieving for a lost child, excited anticipation of a loved one's arrival, and jealousy of someone favoured by the person you adore.

Minsky didn't offer a conceptual analysis of *emotion*, either. But he pointed out that dictionary definitions differ, and that English has hundreds of 'emotion' words naming a wide variety of phenomena—from admiration, agony, and alarm; through curiosity, dismay, and disgust; to hope, impatience, and infatuation (sect. 1-8). Alarm and hope are intuitively (and computationally) very different, as are agony and infatuation—yet these two are also linked. Pride and shame are 'opposites'—yet both can help us learn to evaluate our goals in a new way and to generate new goals appropriately (sect. 2-1).

We can't go into the (fascinating) details here. The crucial point is that Minsky's aim was to outline the computational architecture—an integration of detailed aspects of cognition, affect, and motivation—that *generates* emotions, and that *enables them to perform their psychological functions*. Some of these functions involve self-models (e.g. those associated with pride and shame). Others don't (e.g. fear).

There are two reasons why one might call Minsky an architect-in-waiting. The first is that, as he'd readily admit, he hasn't finished the job. Indeed, "finishing" the job may be too much to ask, even by the *next* millennium. It would be enough (for example) to have outlined the computational structures and processes enabling love to be expressed in the varied ways familiar to novelists and psychologists alike (even within a single culture). Minsky has made a start, but there's a long way to go.

The second reason is that Minsky's ideas on personality still (in 2003) aren't widely known. Why is this? With respect to *The Emotion Machine*, the answer's easy: it isn't officially published. But why was *Society of Mind* largely ignored? After all, Minsky was already hugely famous, and the book had been eagerly awaited for many years. The prime obstacles were its (deceptively simple) rhetorical style, and the lack of programming (see Chapter 12.iii.d).

Few of Minsky's AI colleagues were impressed, and some were highly scornful (12.iii.d). Most psychologists weren't impressed either. Minsky's text alluded to many AI results without identifying them, so people lacking a wide knowledge of AI didn't find it easy to understand. Moreover, its message was largely tacit. Readers were left to make important connections for themselves—and many couldn't. Some psychologists, no doubt, were put off also by the lack of experimental evidence, quantitative measurement, and testable claims. (They may have been wrong to dismiss the book on these grounds: see subsection f, below.) So Minsky is an architect-in-waiting: waiting for his contribution to be given its due.

Probably more people became (superficially) aware of Minsky's ideas by reading Dennett than by reading Minsky himself. For in his runaway best-seller *Consciousness Explained* (1991), Dennett included a chapter on the self that was deeply influenced by Minsky. This was welcomed, among others, by postmodernists who'd already rejected the unity/stability of the self (e.g. Turkle 1995: 261; see 13.vi.e). But it didn't produce a visible upsurge of interest in Minsky's own writings.

Dennett's discussion had appeared in a volume devoted to consciousness. Minsky, by contrast, was trying to work out how the various psychological functions (of which consciousness and self-consciousness are but examples) cooperate within the mind as a whole. For most of Dennett's readers, that seemed less interesting. They didn't realize that the various meanings of the word "consciousness" can't be properly understood without using *architectural* ideas. (Indeed, this is still widely doubted: 14.x–xi.)

f. Of nursemaids and grief

Sloman, too, focused on the computational architecture of the mind as a whole. His core interest, ever since the mid-1970s, has been in what he calls "the space of possible minds", of which existing (animal and human) minds are just examples (Sloman

1978, ch. 6; cf. Dennett 1996). Even his early research on the POPEYE vision program discussed visual psychology in light of architectural principles (and, in so doing, clarified some aspects of familiar concepts such as *interest*, *conscious*, and *experience).* In that sense, it was way ahead of its time (see Chapters 10.iv.b and 12.v.h).

His mature work on mental architecture is broadly like Minsky's, but instead of Minsky's "society of mind" he speaks of the "ecosystem of the mind" (Sloman 2003*a*). Mental mechanisms, he argues, are even more closely interdependent than the members of a society, as they're co-evolved sub-organisms. It's no wonder, then, that vision is intimately involved in reasoning (see Section iv.d, below), and that rational and emotional processes are intertwined too.

Where personal psychology is concerned, Sloman was willing to take the bull by the horns. One of his papers was subtitled 'What Sorts of Machine Can Love?' (That paper, Sloman 2000, is the most accessible statement of his mature approach. The best technical introduction is Sloman 2001; the latest developments can be found on his web site: Sloman n.d.)

One doesn't have to be a computationalist to give a broadly functionalist analysis of love. Mark Fisher (1990) has shown that the essence of personal love is a deep commitment to the goals and motives of the loved one, even to the extent of preferring them over one's own. Such a commitment involves many different cognitive and emotional phenomena, working together to discover and advance the interests of the other person. As a 'pure' philosopher, Fisher was content to take the *possibility* of these phenomena for granted. Sloman, by contrast, aimed also to outline the computational resources required to generate them. (Both Fisher and Sloman were considering "love" as it's conceptualized in Western traditions. Near-equivalents in other societies, such as the Samoan *alofa*, would have to be analysed somewhat differently: see Chapter 8.i.d.)

Like Fisher, Sloman was an accomplished analytic philosopher (though originally trained in maths and physics). He was greatly influenced by Gilbert Ryle and John Austin, who showed that a structured folk psychology is tacit in our everyday language (e.g. Sloman 1978, ch. 4).

Austin, the quintessential 'ordinary-language philosopher', had argued that although ordinary language isn't the last word for theoretical psychology, it is a helpful first word. Its many subtle psychological distinctions have been "honed for centuries against the intricacies of real life under pressure of real needs and therefore give deep hints about the human mind" (Sloman 1987*a*: 217). But they're no more than hints, for everyday language is often ambiguous—or worse. For example, the concept of *pain*, commonly assumed to be the epitome of clarity/intelligibility, is in fact hugely confused and even contradictory (Dennett 1978*a*).

Moreover, the grammar of emotions isn't easily uncovered. As Sloman put it, "our ability to articulate the [psychological] distinctions we use and grasp intuitively is as limited as our ability to recite rules of English syntax" (1987*a*: 217). When we try to articulate these distinctions in expressing some psychological (or philosophical) theory, we typically lose sight of the fine print.

A telling example of this was described long ago by Austin (1957). He showed that the language used by an uneducated hospital assistant giving evidence in a law court may make clear just what went on in his mind (though not, of course, *how).* One can see just what it was which led to the unfortunate event in question (the severe scalding of a patient being given a bath). By contrast, said Austin, the legal terminology used by the judge in summarizing it may obscure many crucial details. And the same is true, he said, of philosophers' jargon and theories, such as their talk of "voluntary/involuntary" actions, or of "free will" (see subsection g, below).

Austin's strictures can be applied also to psychological theories of personal matters, including most computational theories of emotion. The psychologist should aim to recognize, and to explain, *at least* those distinctions which are intuitively drawn in everyday language. These will certainly have to be supplemented, and often even corrected. But they shouldn't be ignored, nor shoehorned into ludicrously over-simple theoretical categories. Sloman's work is an attempt to avoid such shoehorning.

Sloman defined emotions as datable *episodes*, closely related to attitudes—which are what Ryle (1949) called *dispositions* (see 16.i.c). (Thanks to his study of grief, mentioned below, he now defines them as "dispositions which can trigger episodes": personal communication) This wasn't a report of, nor a recommendation for, everyday usage. Rather, it was a theoretical clarification enabling the psychologist to distinguish emotions from other mental phenomena—which may or may not be described as emotions in ordinary conversation.

So Sloman argues (personal communication) that Damasio's case studies don't provide *empirical evidence* for a link between emotion and intelligence, as Damasio claims. Rather, they *confirm*—in a startling and memorable fashion—the relation that's already implicit in these familiar concepts. (Hence my suggestions, above, that Damasio's work offers illustration rather than evidence, and that asking "how one could test" Minsky's society theory may not be appropriate.)

What the computational psychologist should do, then, is (first) make explicit the distinctions tacit in ordinary-language terms as carefully as possible, and (second) "extend colloquial language with theoretically [i.e. computationally] grounded terminology that can be used to mark distinctions and describe possibilities not normally discerned by the populace" (Sloman 1987*a*: 217).

Accordingly, Sloman didn't merely unpack the implications of ordinary language, as Fisher did. He also exploited his experience of software design, and his deep understanding of various types of computation (Chapter 16.ix.c). He drew on these skills in discussing a wide range of *possible* mental architectures, with special emphasis on the generation and control of human motives, emotions, and attitudes.

To do that, he didn't need to do any programming, nor even decide just what types of virtual machine—e.g. GOFAI, localist-net, PDP, GasNet . . . or some form of computation yet to be discovered—might be most appropriate for particular functions. Similarly, Minsky didn't need to make those decisions when asking broad architectural questions. The price they both paid was to lay themselves open to the implementation hunger of their AI colleagues, many of whom ignored—or even scorned—their work for years because it wasn't immediately implementable (see 12.iii.d).

But times change. In the late 1980s a publisher's reviewer advised me to drop Sloman's (1987*a*) paper from a collection I was preparing on the philosophy of AI (Boden 1990*b*). He/she said it was unrepresentative (true) and irrelevant (words fail me!). I insisted that it stay in. Today, there wouldn't be a peep of protest.

Largely because of the revival of interest in emotional intelligence, Sloman's long-standing work on mental architecture has now achieved international recognition.

Indeed, in 2002 DARPA invited him to participate in a small workshop related to their new cognitive systems initiative, where Rodney Brooks (15.viii.a) discussed Sloman's work in one of the introductory papers. This was more than a straw in the wind: the significance of DARPA's support within cognitive science is illustrated in Chapter 12.iii.e and vii.b. Two years later, the AAAI held a cross-disciplinary symposium on 'Architectures for Modelling Emotion'.

Some of Sloman's current recognition is undeserved, however, as he's the first to admit:

> I've even had someone from a US government-funded research centre in California phone me a couple of months ago [i.e. mid-2002] about the possibility of modelling emotional processes in terrorists. I told him it was beyond the state of the art.
>
> He told me I was the first person to say that: everyone else he contacted claimed to know how to do it (presumably hoping to attract research contracts). (Sloman, personal communication)

It's difficult enough to limn the mind of a cricket (15.vii.c). To depict the complex architecture of the human personality, whether terrorists or just plain folk, is orders of magnitude harder.

That's why, nearly forty years ago, Reitman had to give up on Argus. He'd have been well aware, for instance, that a nursemaid dealing with several hungry, active, and attention-seeking babies (with an open door leading onto a water-filled ditch) is subject to countless perceptual and emotional interrupts, and consequent changes of plan. She has to distinguish between important and trivial goals, and decide on urgency and postponement. (Feeding a baby is important but not highly urgent, whereas preventing it from falling into a ditch is both.) She'd welcome Argus' 100 eyes, and 100 hands besides. But she has only two of each, so must schedule her limited resources effectively—which is what emotion, on Sloman's view, is basically about. Reitman knew this. But he couldn't express it, still less model it, in computational terms.

Thanks to thirty years' progress in AI, Sloman's architectural account of such matters is a huge advance on Reitman's. What's more, a small part of his theory was implemented in the 1990s, in the nursemaid-and-babies model mentioned in Chapter 1.i.b (Wright and Sloman 1997; see also Beaudoin 1994; Wright 1997). The model is still being continually improved. (My account here ignores many details, including the differences between successive versions.)

Sloman's simulated nursemaid coped with many fewer problems than a real nursemaid does. "She" merely had to feed the "babies" (i.e. recharge them), try to prevent them from falling into ditches, and—if they'd already fallen in—deliver them to a first-aid station. (That's a euphemism: a baby in a ditch was in effect dead, and had to be delivered to the exit for dead babies.) She didn't have to worry about cuddling them, bathing them, changing them, singing to them . . . and there were no live electric plugs to be avoided. And she herself was a pretty simple creature, for she had only seven motives to follow:

* feeding a baby (if she believed it to be hungry);
* putting a baby behind a protective fence (if it's already been built);
* moving a baby (if it's been damaged by falling into a ditch) to a first-aid station;
* patrolling the ditch, to see if any babies have fallen in;
* building a protective fence;

* moving a baby to a safe distance from the ditch;
* and wandering around the nursery (if no other motive is currently activated).

Even these few motives, however, could conflict. (What if she was feeding a very hungry baby, and another crawled near the ditch?) Further, they could arise unexpectedly as the result of environmental contingencies. For each baby was an autonomous agent, whose crawling and hunger crying was independent of other babies, and of the nursemaid's actions and motives.

By and large, Sloman's program enabled the carer to act with (minimal) intelligence in the circumstances. If she was focusing on feeding a baby, she'd instantly put it down if she realized that another baby was approaching the ditch. But she'd remember that the first baby still needed feeding, and would do so as soon as possible. Moreover, the hungrier babies would get fed first. If there were *two* babies approaching the ditch, the nursemaid would judge which was the nearer so as to prioritize her currently active motives: *feed baby a*, *rescue baby b*, *rescue baby c.* (Given baby a … to baby z, however, there'd be trouble: the virtual nursemaid's performance deteriorated rapidly as the number of babies increased—as a real nurse's does, too.) Further flexibility sprang from the program's ability to adjust the filter that allowed a pre-attentive motive to enter attention.

There were many obvious weaknesses. For example, the system didn't implement visual processing, but merely simulated it. The carer's vision was assumed to be perfect, up to a certain distance—but human nursemaids may have to cope with dusk, or with vision blurred by tears or smoke. And babies behind obstacles were fully "visible" (i.e. their location was known by the nursemaid), because occlusion wasn't simulated.

Other weaknesses were less obvious, unless one's aware of the many psychological distinctions within Sloman's theory. He listed several important limitations, and admitted that the simulated anxieties ("proto-perturbances") are but a pale shadow of a real nursemaid's perturbances (Wright and Sloman 1997, sects. 3.7.2, 4.3). He also pointed out that "perturbances" are only one of several architecturally distinguishable forms of anxiety. In short, *anxiety* is really *anxieties*—but Sloman had provided clear computational distinctions between them.

However, the weaknesses weren't terminal. For this wasn't a one-off tinkered program, but the first in a theory-based series. Sloman had long taken on board Drew McDermott's advice on how AI should be pursued (see 11.iii.a). That's why his computational "toolkit", developed for designing autonomous agents in general, was made available on the Internet for others to experiment with (Sloman and Poli 1995; Sloman 1995). The 1997 version of the nursemaid program was called MINDER1. Eventually, MINDER2 and MINDER3 … will follow, as Sloman's implementations approach his architectural theory more closely.

Sloman's theory of emotions—like Simon's and Minsky's too—isn't simply a phenomenological (descriptive) classification, as Tomkins's was. It provides an architecture-based *explanation* of the differences between what Damasio terms "primary" and "secondary" emotions (Sloman 1999). And it makes many fruitful distinctions.

For instance, emotions differ in terms of three main architectural "levels", which involve reactive, deliberative, and meta-management mechanisms. A cricket's mind is mostly reactive, depending on learnt or innate reflexes (Chapter 15.vii.c). As such, it's capable only of "proto-emotions": inflexible reactions that have much the same adaptive function as (for example) fear in higher animals. A chimpanzee's mind is largely deliberative, capable of representing and comparing past (and possible future) actions or events. So backward-looking and forward-looking emotions can now arise: non-linguistic versions of anxiety and hope, for instance.

In human adults, the deliberations can include conscious planning and reasoning, generating more precisely directed anxieties accordingly. (Remember Colby's neurotic and paranoid patients.) In general, language makes possible emotions with propositional content—unknown to crickets, or even to chimps. An adult human mind also has a rich store of reflexive meta-management mechanisms, which monitor and guide behaviour. (McDougall's "master sentiment of self-regard" and Dennett's "center of narrative gravity" are examples, though much less well specified.) So emotions centred on the self—such as vainglory and embarrassment—are now possible.

One might call this theory a generative grammar of emotions (cf. Sloman 1982), provided one remembers that Sloman (unlike Noam Chomsky) was interested in the underlying processes, not just the abstract structure of the phenomenology (see Section iii.a, below). Better, it's a generative grammar of *the mind as a whole.* For Sloman—like Minsky—offered many promising ideas about how the various types of emotion are functionally related to other mental features, including the processes involved in intelligent action.

For example, his computational analysis of grief (on the death of someone truly loved) shows why it's inevitable, why it lasts a long time, why it eventually disappears—replaced, perhaps, by an enduring sadness—and why it's *cognitively* debilitating in many specific ways (Wright *et al.* 1996).

In a word, to mourn is to dismantle. What's dismantled is much of the cognitive–motivational core of the personality concerned. In Fisher's terms, this is a complex self-denying (yet also self-enhancing) commitment to another, now departed; in McDougall's, it's a powerful sentiment of love, focused on the deceased person and closely integrated with the master sentiment of self-regard. Dismantling that can be neither easy nor pleasant. It involves both cognitive and emotional effects, some—but not all—of which are introspectible/observable episodes, as opposed to dormant dispositions. And for sure, it's not the work of a moment (see 1.i.b).

These effects will differ across cultures, because concepts of love, personal obligation, and so on vary. For instance, perhaps I should have said (above), "why it's inevitable *in privileged Western cultures*". For the medical anthropologist Nancy Scheper-Hughes has shown in distressing detail how some cultures protect themselves from what would otherwise be an intolerable excess of grief. Her book on Brazil has a chapter whose subtitle is 'The Social Production of Indifference to Child Death'—1992: 268–339. But that "indifference" is rooted in pre-existing schemata of parental love that are different from ours in culturally relevant ways. It's those schemata which mould whatever grieving goes on.

According to another anthropologist, Renato Rosaldo, headhunters often decapitate their fellow men out of rage, born of grief (see Chapter 8.ii.b). Whether their grief is—as Rosaldo (1989) claims—closely comparable to his own, on mourning his wife, is quite another matter. Sloman's analysis would imply that, beyond a common human core, it is very different. (However, that common core may be what leads virtually all religions to pay ritual attention to the bodies of the dead, especially of people whom the mourners had loved: 8.vi.d.)

Lasting personal attachments falling short of personal/romantic love also require computational dismantling to some degree. A funeral service for a friend, or a memorial service for a much-admired person, are each distressing in their own way. But the grief involved is different, because the mourner's cognitive/emotional schemas linking them to the deceased are different.

A final word: Sloman's theory of grief could doubtless be improved in detail, or perhaps replaced by an alternative. But anyone who's sceptical about *the very idea* of giving a computational analysis of grief should know that the journal editor who published it is a practising psychiatrist (K. William Fulford, of Oxford's Radcliffe Infirmary). As such, he's more familiar than most of us with the multifaceted cognitive–emotional ravages of mourning. So one should beware of dismissing computational treatments of grief simply because of prejudices about the emotional irrelevance of computers.

In particular, one should set aside the Spielberg–Kubrick puzzle over whether computers could "really" experience emotions (see Chapters 14.xi and 16.v.b). Cognitive science aims to help us understand *biological* minds, by showing (for instance) how such humanly significant phenomena as personal love, grief, mourning, and funeral rituals are *possible* (cf. Section iii.d, below). It may also aim to help us understand the range of *all possible* minds. Even so, if its philosophical implications about the "reality" of the mental lives of robots are unclear, that needn't compromise its power as a theory of human and animal psychology.

g. Free to be free

One of the ways in which the Third Force personality theorists differed from the behaviourists was their emphasis on freedom (see 5.ii.a). Whereas Skinner (1971) famously scorned this notion, as not only illusory but socially pernicious, the Third Force saw freedom of choice as the crucial aspect of human minds. Most 'laymen' agree with them.

It's certainly true, as remarked in Chapter 1.i.b, that an adult human being is free in a way in which a dog, or a cricket, is not. In other words, the range and flexibility of their behaviour is markedly different. So much is undeniable. What's controversial is the *philosophical analysis* and/or *psychological explanation* of that difference.

Before the rise of cognitive science, this issue was normally put in terms of determinism versus indeterminism. So the neuroscientist John Eccles, for example, posited mysterious quantum phenomena acting on "critically poised neurones" in the brain (see Chapters 2.viii.f and 16.i.a). Why such phenomena are absent from the brains of dogs and crickets—or, for that matter, of human babies (who *aren't* yet free agents: see Section vi.f below)—wasn't discussed. (Nor is this discussed when similar arguments are put forward today: see 14.x.d.)

Eccles's neurological hypothesis found few followers. But even if they found his "explanation" implausible, many people agreed with him that it's obvious that free choice is incompatible with determinism. The trouble was that the opposite seemed just as "obvious" to others. Compatibilists argued persuasively that indeterminism at the origin of action would *destroy* human freedom, because it would make it impossible to ascribe moral responsibility to people (e.g. Hobart 1934).

Cognitive science showed a way out of the impasse, by sidelining the determinism/indeterminism issue and focusing instead on the huge cognitive and motivational differences between humans, dogs, and crickets.

It's the computational *architecture* of the human mind/brain (including, crucially, its ability to support language: see 12.x.g) which generates deliberation, self-reflection, and multifaceted choice—wherein the values inherent in MGP's Image are used in comparing the many possible Plans. And it's our (largely tacit) understanding of those features which underlies our ascriptions of freedom, and of blame, to everyday actions (cf. Austin 1957). The subtle distinctions between different actions that are so often marked in gossip are *real* distinctions, made possible by the psychological abilities possessed by persons.

This view was hinted at in almost the first paper on GPS:

> [The key feature of sophisticated problem-solving programs] is . . . that they finally reveal with great clarity that the *free* behaviour of a reasonably intelligent human can be understood as the product of a complex but finite and determinate set of laws. (Newell and Simon 1961: 124; italics added)

Early versions of it were argued at greater length by a number of writers, including Minsky (1965), Dennett (1971), Sloman (1974), and myself (Boden 1972: 327–33; 1978, 1981*c*). Later, Dennett developed it in his essay 'On Giving Libertarians What They Say They Want' (1978*b*: 286–99), and especially in his book *Elbow Room: The Varieties of Free Will Worth Wanting* (1984*a*).

This book was a philosophical analysis, deepened by a cognitive scientist's appreciation of computational architecture. (Many years later, Dennett would return to the topic—now, with a focus on the *evolution* of the relevant architecture: Dennett 2003*a*.) It provided a host of examples illustrating the psychological processes that underlie our freedom of action, and various (normal or pathological) restrictions on it. It made clear that only creatures with a certain type of cognitive–motivational complexity are capable of choosing freely. Or, rather, to choose freely *just is* to exercise those architectural features.

In other words, determinism/indeterminism is largely a red herring. Although (Dennett argued) there must be *some* element of indeterminism, this can't occur at the point of decision—for that would give us a type of "free will" that's *not* one worth wanting. The indeterminism, then, affects the considerations that arise during deliberation. The person may or may not think of *x*, or be reminded (perhaps by some environmental contingency) of *y*—where *x* and *y* include both facts and values. The deliberation itself, by contrast, involves deterministic processes of selection, weighting, and choice. (Deterministic, but not universalist: moral principles, cultural conventions, and individual preferences are all involved.)

Elbow Room, in my opinion, didn't get the recognition it deserved. Analytic philosophers mostly dismissed it, despite Dennett's already high reputation and the book's origin as the 1983 John Locke Lectures in Oxford (see 16.iv.b). Besides being hung-up on the determinism/indeterminism issue, they saw Dennett's approach as "psychologism"—defined by Gottlob Frege as philosophy's unforgivable sin (2.ix.b). And the neo-Kantian philosophers, of course, would reject *any* naturalistic, science-based, account of freedom (16.vi–viii).

Some neuroscientists, too, analysed freedom in architectural terms. I don't mean Benjamin Libet, who showed in the early 1980s that the "intention" appears in consciousness *after* the relevant messages have been sent to the muscles—and who was highlighted in Dennett's 1991 book on consciousness, accordingly (14.x.b). Rather, I'm thinking of the schema theorist Arbib (14.vii.c), and of the clinical neurologist Timothy Shallice (1940–).

In his Gifford Lectures, written with the philosopher Mary Hesse, Arbib applied his neuropsychological theory of schemas (Chapter 14.vii.c) to cultural attitudes and personality—and devoted an entire chapter to freedom. Human behavioural flexibility was explained in architectural (schematic) terms, much as it was by Minsky, Sloman, and Dennett. Indeed, Dennett was praised for having made the best sense of the free-will supporters' position (Arbib and Hesse 1986: 96). But given the religious context of the Gifford Lectures (2.vii.b), Dennett's analysis was considered in light of its implications for theology as well as psychology.

In those terms, Dennett's arguments were said to "still leave everything to play for" (p. 98). Indeed, that applied to *any* scientific explanation of freedom. Whether humans (or robots) really have freedom is at base an "evaluative" question, not a scientific one (p. 99). That is, it's a question about *human relations*—and, for the theist, also about God–human relations. To have personal freedom, on this view, is to possess the type of mental architecture described by cognitive science *and* to participate in a community whose communications are imbued with trust, consideration, and respect.

That's not to say that Arbib's neuropsychology was essentially religious, for it wasn't. On the one hand, he accepted the *fact/value* distinction, as we've seen. So no scientific psychology can entail (or forbid) religious commitment. On the other hand, his theory applied to theists and atheists alike (Arbib and Hesse 1986, chs. 10–12). Theists believe in (possess central schemas positing) a God–human relation. Secularists don't. As educated persons, they possess the God schema; but they use it only in scare quotes. (In McDougall's terminology, it's not a *sentiment*—or if it is, it has a very different, perhaps even negative, emotional contribution.)

As for Shallice, his clinical work on the psychopathology of action made him all too familiar with how brain damage can constrain our freedom. That is, how brain damage can constrain the subtle complexities of our cognitive and emotional processing. Countless examples are described in his book (1988*a*), and in his research (with Donald Norman) on the classification of errors (see Chapter 12.ix.b).

With respect to deliberate conscious control, including what William James had called "the slow dead heave of the will" (1890: ii. 534), Shallice outlined how a "supervisory attentional system" can control action, given certain types of information about the various action schemas potentially involved (Norman and Shallice 1980/1986). He showed that the normal exercise of "the will" doesn't merely trigger action, but involves ongoing error correction too—not to mention the intelligent anticipation of consequences. His theory of control distinguished between schema-driven *contention scheduling*, which covers habitual actions done unthinkingly (and clinical cases of "automatism": see Preface, ii), and *executive control* in which conscious monitoring is involved (see 12.ix.b). In short: "The phenomenology of attention can be understood through a theory of mechanism" (Shallice 1988*a*: 388).

To "understand through a theory of mechanism", of course, needn't mean to do computer modelling. What's more, computer modelling needn't be seen as instantiation. So whether a computer could "really" have free choice is irrelevant here, just as the Spielberg–Kubrick question is irrelevant when evaluating computational theories of emotion. The question, rather, is *whether we can use computational concepts to help us understand human personality and freedom.*

(A caveat: I said, above, that cognitive science "showed a way out of the impasse". But not everyone agrees. In fact, the analysis sketched above is still a minority view. For instance, several people used quantum physics to bring indeterminism in again: R. Penrose 1994*a*; D. Hodgson 1991, 2005. Their arguments, in turn, were opposed: see the peer commentaries to D. Hodgson 2005. Like many other philosophical questions, this one will doubtless run and run. But, for what it's worth, my own view is that it has been answered—unlike the questions of conscious experience or realism/anti-realism: see 14.xi and 16.vi–viii.)

h. Some hypnotic suggestions

If cognitive science really can explain the possibility of freedom, it ought also to be able to explain the seeming *loss*, *surrender*, or *compromise* of freedom that's seen in hypnosis. Loss, because the hypnotized person may feel impelled to do something even though they don't consciously want to do it. Surrender, because they seem to be driven by the hypnotist's intentions rather than their own. And compromise because, even though they're able to perform complicated bodily actions just as effectively as in normal voluntary behaviour, they don't consciously want to do so.

Notoriously, someone under hypnosis may not only do strange things, such as bursting into song whenever the hypnotist coughs, but may see or not-see strange things too: even counting books wrongly, having been told by the hypnotist—falsely—that there are no books by Jane Austen on that shelf. Notoriously too, some people are easier to hypnotize than others. All that is common knowledge—not least, to stage hypnotists. It's also common knowledge that surgical anaesthesia can sometimes be effected by hypnosis rather than drugs, the pain either disappearing or not being experienced as "real" pain. Even if the concept of *pain* is by no means as clear, or coherent, as we normally think it is (Dennett 1978*a*), that fact can't be denied.

Experimentalists have studied these familiar, but highly mysterious, phenomena in some detail. So they've asked (for example) what sorts of task can/can't be carried out by hypnotized subjects; how individuals differ in their susceptibility to hypnosis; and what mental or environmental conditions raise/lower the likelihood of the hypnotist's success. And they've discovered countless intriguing data.

For instance, some sorts of hypnotic suggestion are easier to communicate, or instil, than others (Perry *et al.* 1992). The easiest are "motor" examples, such as *Your arm is becoming so light it is rising in the air*. Next come "challenge" examples, such as *Your arm is rigid* (from which it follows, in successful cases, that the person won't be able to bend their arm when asked to do so by the hypnotist). Negative cognitive demands are more difficult still: *Whenever you count, you'll forget the number four*. And the most difficult of all are positive hallucinations. But even here, experiments show systematic differences. It's easier to make someone hallucinate as required to the suggestions *You* *can taste something sweet* or *You can hear/feel a mosquito* (50 per cent of people are fooled) than to *You can hear a voice speaking* (only 10 per cent).

In short, a huge amount is now known about the behaviour, and the accompanying phenomenology (or rather, the lack of it), characteristic of hypnosis. What's been very much less clear is the explanation for these things.

As MGP put it in 1960, confessing to "an embarrassing lack of firsthand experience" due to the general mistrust of anyone who'd worked with hypnosis:

> One of the seven wonders of psychology is that so striking a phenomenon as hypnosis has been neglected. Some psychologists literally do not believe in it, but consider it a hoax, an act put on by a cooperative stooge . . .
>
> [. . . Our] conception of hypnosis is quite simple. It is based on a naive faith that the subject means what he says when he tells us that he surrenders his will to the hypnotist. Is there any reason to doubt him? It is just as good a theory as any of the others that have been proposed.
>
> The trouble with such a theory, of course, is that *no one knows what the will is*, so we are scarcely any better off than before. (G. A. Miller *et al.* 1960: 103–4; italics added)

Their remark that "some psychologists literally do not believe in it" presumably referred, though they didn't say so, to Theodore Sarbin and/or to Martin Orne. Sarbin (1950) had argued that hypnosis is a matter of two people playing out familiar *roles*. A role, as Abelson would make clear (subsection b, above), involves two (or more) people with interlocking goals, intentions, and choice points. Sarbin's claim was that the role of hypnotic subject is part of people's general knowledge, so it can be played out when there's someone available (the hypnotist) to take the complementary role. In short, hypnosis is more a matter for the social psychologist than for the clinician.

When MGP were writing their book, Orne's views were even more in the air than Sarbin's. His notorious claim—which I remember filling the newspapers at the time—was that some cases of hypnosis were actually a species of suggestion, and that hypnotists themselves were often deceived accordingly (Orne 1959; cf. O'Connell *et al.* 1970). Although (unlike Sarbin) Orne allowed that high-hypnotizable people do pass into a genuine hypnotic state, he argued that low-hypnotizables could *simulate* compliance with the hypnotist's expectations (remember MGP's "hoax" and "cooperative stooge"). He also suggested ways of identifying the deceivers, although experiments showed this to be more difficult than he'd expected. (Many years later, some would claim that *all*, or virtually all, cases of hypnosis are mere social "compliance", as opposed to a special mental state: Wagstaff 1981.)

Orne implied (what was later confirmed) that easily hypnotizable subjects are, in general, more suggestible in other ways. As he put it, they're more likely to accept, and comply with, the "demand characteristics" of the situation laid down by the hypnotist. But this didn't solve the mystery. His theory allowed that some cases of hypnosis were genuine, without explaining *how it's possible* for someone to accept the suggestion to do X, but to do it without any awareness of the delicate monitoring that seems to be necessary when X is done of one's own free will (*sic*).

MGP described hypnosis in terms of someone's accepting the hypnotist's "Plans" as their own. They acknowledged, however, that

> Obviously, a hypnotized subject does not relinquish all his capacity for planning. It is still necessary for him to supply *tactics for executing* the strategy laid down by the hypnotist. If

his cessation of planning were to extend to the point where he could not even work out the step-by-step details of the hypnotist's Plans, then we would expect to find him in a state of stupor . . . (p. 108; italics added)

Discussing the various levels of hypnosis, they spoke of "the kind of planning by inner speech that sets the voluntary strategy for so much that we do". But even if that level of planning is "captured" by the hypnotist, "there must be still other planning functions that are performed more or less automatically as a kind of 'housekeeping' ".

In sum, MGP had many insightful things to say about hypnosis. These included their distinction between "thoroughly habitual" behaviour, such as speaking grammatically, and the "spontaneous inner speech" that guides normal voluntary behaviour (p. 111). Even so, their suggestive remarks didn't amount to an adequate theory.

Talk of "dissociation" soon seemed more promising—and in my opinion it still does (for a critical review, see Kirsch and Lynn 1998). This old idea (Chapter 5.ii.a) was revived in the 1970s by Stanford's one-time behaviourist Ernest Hilgard (1904–2001).

His pioneering experiments on hypnotic susceptibility in the early 1960s had already provided a plethora of new data—as well as a standardized measure of susceptibility to hypnosis (Hilgard 1965). (Hypnosis was so unfashionable in those behaviourist times that Hilgard had been the only person to apply for money set aside by the Ford Foundation for research in this area—Baars 1986: 296.) Now, he turned to dissociation as an explanatory construct (1973, 1977/1986).

Hilgard wasn't a card-carrying computationalist, although in the 1950s he'd been fairly close to Bruner (Hilgard 1974). It was at Bruner's invitation that he'd visited Cambridge, England, to attend a small meeting on cognitive processes—a maverick topic at that time, at least in the USA (6.iv.d). Among the other mavericks he met there were Broadbent and two-thirds of MGP: namely, Miller and Karl Pribram. (He also took the opportunity to meet Anna Freud, at Freud's house in Hampstead.)

His theory of "neo-dissociation", which he was still developing in his eighties (Hilgard 1992), posited a hierarchy of relatively autonomous subsystems, headed by a central controller (the "executive ego") capable of planning and monitoring action. So much wasn't new (5.ii.a). What was new—besides the greater (post-cybernetic) understanding of the notion of *control*—was his attempt to use dissociation (i.e. varying degrees of breakdown in intra-systemic communication) to explain not only abnormal phenomena such as hypnosis but a host of everyday details concerning *attention*, and the lack of it.

Hilgard intended his theory of attention to explain the nature of consciousness, and of the ways in which it can be compromised while behaviour carries on nevertheless. One could also say that it was a study of *freedom*, and the ways in which it can be adulterated. (Predictably, many were sceptical. Neisser, for instance, argued that psychology wasn't yet "ready" for consciousness: Neisser 1979.)

Of the explanations of hypnosis offered by today's computational psychologists, perhaps the most persuasive is that of Zoltán Dienes and Josef Perner (forthcoming; cf. D. A. Oakley 1999). They see their theory as closely related to Hilgard's but, modulo certain ambiguities in Hilgard's writing, different in one important respect (see below). They outline computational mechanisms whereby hypnosis of varying types can occur, and explain the ranked difficulty-differences mentioned above.

For example, they argue that the more computational effort goes into performing a task the harder it is to suppress higher-order thoughts (HOTs) of intention. This may explain "why challenge suggestions are more difficult than simple motor suggestions" (p. 16). Similarly, positive hallucinations may be especially difficult to induce because they involve more cognitive effort, of a type that would normally involve HOTs (pp. 15–16). In each of these cases, and others too, they outline experiments that could distinguish between various computational possibilities. In addition, their theory generates predictions (as I write, not yet fully tested) about how "high-" and "low-hypnotizable subjects" will differ in several tasks where hypnosis *isn't* involved.

Dienes and Perner, a cognitive and a developmental psychologist respectively, base their account on six main roots:

* One is the Norman–Shallice theory mentioned in the previous subsection (and described more fully in Chapter 12.ix.b).
* The next two are Perner's own long-standing research on the nature of representations of various types/levels (Section vi.f below), and his more recent account of a hierarchy of voluntary action (Perner 2003).
* The fourth is Annette Karmiloff-Smith's developmental psychology, in particular her theory of representational redescription (Section vi.h, below; and see Dienes and Perner 1999: 748–9).
* Next, is their previous joint work distinguishing various levels of "explicitness" and "implicitness" in representations—a nineteen-page *BBS* paper that prompted thirty-five pages of peer commentary (Dienes and Perner 1999).
* And last, is the CUNY philosopher David Rosenthal's (1986, 2000, 2002) concept of Higher Order Thought—HOT, for short. (HOT is intended as a theoretical analysis of conscious awareness. It's primarily because Hilgard helped himself to the notion of conscious awareness without analysing it that his theory differs from this one.)

In a nutshell, Dienes and Perner explain hypnosis in terms of "cold control". By *control*, they mean what Norman and Shallice called executive control—as opposed to contention scheduling (which underlies what MGP termed "thoroughly habitual" behaviour). By *cold*, they mean *absence of HOT*. That is, there's no higher-order thought.

More precisely, there's no explicit (self-reflexive) representation of the fact that the hypnotized person has the intention to do X. (As MGP put it, there's no spontaneous inner speech expressing the Plan to do X.) Nevertheless, the person does have that intention, communicated to them by the hypnotist. Moreover, they can execute it flexibly by means of detailed perceptual monitoring and motor adjustment. For that to be possible, the intention needs to be (in a particular sense explained by the authors) *explicit*. That is, it indicates some feature or state of affairs "directly" (2004: 4). But it's *not* represented at the higher level of explicitness that's required for conscious thought and verbal report. In other words, the person doesn't *know* that he/she has the intention to do X.

Dienes and Perner added the "cold" to the "control" because they needed to be able to explain *unconscious but planned* behaviour. Executive control as described by Norman and Shallice is conscious by definition. It applies to paradigm cases of planned, monitored, voluntary action—as opposed to absent-mindedness or pathological error

(12.ix.b). Much hypnotic behaviour is also planned and carefully monitored, as MGP had pointed out—but it's not guided by conscious intent or deliberation.

As for when and why the relevant HOT gets turned off, or inhibited from forming in the first place, this depends on a wide range of factors discussed in some detail by Dienes and Perner. They include internal cognitive and motivational conditions, and external social conditions. Norman and Shallice's "supervisory attention system" enables some action schemata to be more heavily weighted than others (12.ix.b), and Dienes and Perner adapt this idea in discussing how attentional mechanisms can aid/inhibit someone's falling prey to the hypnotist's suggestions.

Their theory, which takes account of computational work in many different areas (perception, memory, sensori-motor control . . .), indicates what types of turn-off/inhibition are *possible.* But experimental data are needed to discover which possibilities are realized, and with what frequencies. Sometimes, the data (such as the differential frequencies mentioned above) can be explained by the two authors in terms of the computational architecture they posit.

Dienes and Perner relate their theory to the notion of "multiple selves" (24 ff., 31–2). By implication, it can help us to understand the nature, and to some extent the aetiology, of multiple personality disorder.

As remarked in Chapter 5.ii.a (and in Preface, ii), the very possibility of multiple personality is a puzzle. Postmodernists insist that each of us 'possesses' a diversity of selves (and they welcome virtual reality avatars, as a way of playing with them: see 13.vi.e). But they don't explain how such diversity, or its extreme pathological version, is possible in the first place. The computational approach in general can demystify this strange phenomenon (Humphrey and Dennett 1989; Boden 1994*d*; and cf. Flanagan 1994). And Dienes and Perner's specific version of it helps us to distinguish competing accounts of *just what* may be going on. (Brain-scanning studies, given a good psychological/computational theory to help us to interpret the data, may help too: Reinders *et al.* 2003.)

In addition, though they don't say so, certain aspects of schizophrenia can be glossed in these terms. Hallucinations of inner voices, or delusions of alien control of one's own body, apparently involve planned speech or action *without* the normal HOTs. (Similarly, neurosis involves non-HOT beliefs and intentions, which guide thought and behaviour unknown to the person concerned: see subsection a, above.)

What causes the absence of HOTs in schizophrenia is another matter. Some recent neuroscientific studies suggest that a prime cause may be delay to, or inhibition of, reafferent motor signals—so that the person *does not* attribute responsibility to the self (Cahill *et al.* 1996; Silbersweig *et al.* 1996; C. D. Frith *et al.* 2000; S.-J. Blakemore *et al.* 2003). As for *which* person, imaginary or otherwise, is deemed responsible, that will naturally depend on rationalizations, or confabulations, guided by the idiosyncratic belief structure and anxieties in the person's mind (subsection a, again).

Cold control theory, then, fulfils some of the more startling promises made by MGP nearly half a century ago (Chapter 6.iv.c). Strictly, "fulfils" isn't quite the right word. For the theory has gaps. It doesn't explain, for instance, why "only about a third of people pass amnesia suggestions compared to 50% of people who pass the mosquito hallucination" (p. 17). Nevertheless, it appears to be very much on the right path. (Dienes and Perner themselves regard their theory "more as a means of theoretically orienting in the right direction rather than as a final explanation of hypnosis'': p. 33.)

i. An alien appendage

Given a cognitive system capable both of Shallice's ''executive control'' and of all the layers of representation specified by Dienes and Perner, the possibility naturally (*sic*) arises of the first being activated *without* any guiding representation at the very highest, self-reflexive, level. If this happens, the person naturally (*sic*) won't be able to report any such non-existent representation—whether to themselves in introspection, or to others.

I say ''naturally'' here, partly because this computational possibility is *inherent* in any such system (cf. Section iii.d, below). But in addition, it's a significant fact about *Homo sapiens* that this possibility is often realized—and that when it is, other members of the species sit up and take notice.

I'm not thinking primarily of ordinary hypnotism, nor of the always eager audiences for the stage variety. Rather, I'm thinking of religious experiences, and their effects on religious believers.

As James (1902) noted long ago, there's a wide variety of ''religious'' experiences, ranging from inchoate feelings of some numinous presence, through visions and voices, to states of ''possession'' and/or alien bodily control like those mentioned above (Bourguignon 1976; Ferrari 2002). Sometimes, these come unexpectedly. At other times, they're more or less deliberately sought after.

Typically, they're interpreted by the individual concerned (and often by their fellows, too) in terms of some alien power. This, in turn, is glossed in terms provided by the religious tradition of the person concerned. Perhaps a fire god or an ancestral soul, perhaps a saint, perhaps some utterly transcendent Being . . . or perhaps even a nine-day-wonder belief in the Guardians on the planet Clarion (subsection c, above).

Besides drawing on those traditions, these anomalous experiences strengthen them further. The near-universal forms of religious culture discussed in Chapter 8.vi are typically buttressed by the religious experiences of priests, shamans, or just plain folk—and, in some cases, of schizophrenics too. Indeed, we'll see that one common explanation for the origin of religion refers specifically to those experiences (item 2 on the list given in 8.vi.b).

Given that the core religious *concepts* arise naturally for other reasons (8.vi.d), they can be maintained—and the power of the priests protected—partly because of visions and states of possession (and partly by mechanisms avoiding cognitive dissonance: subsection c, above). These involuntary states are attributed to the influence of gods or spirits largely *because* their controlling causes aren't introspectively accessible to those undergoing them. (That is, there are no HOTs.)

From a computational point of view, it's no surprise that the specific *content* of religious experiences can be communicated, or ''suggested'', by the culture concerned—and especially by charismatic shamans. Much as Senator Goldwater's belief structure led him to react in predictable ways to news items about the Berlin Wall (subsection c, above), so someone committed to a particular structure of religious belief will react in broadly predictable ways to alien—that is, involuntary and otherwise inexplicable—experiences.

In many societies, religious rituals have developed which encourage the occurrence of such anomalous psychological states, and which lead the persons concerned to interpret them in culturally preordained terms. They can be induced not just by charismatic suggestion, but by anything from monotonous Gregorian chants to psychotropic drugs such as mescaline. Once induced, the ritual fixes their interpretation in a culturally appropriate manner.

Dienes and Perner themselves point out that "spirit possession" can be seen as a species of hypnosis (2004: 30). Indeed, this suggestion is a very old one. The novel insight, here, is that such phenomena *are only to be expected* in minds with a computational architecture of the type described above. The specific *content* of religious belief may be very surprising, especially if we focus on the many cultural variations rather than the near-universal aspects (Chapter 8.vi). But *that religious experiences occur at all* is not.

MGP had pointed out in their manifesto that the overall "Image", or value system, differs in different cultures (1960: 122–3). Indeed, a culture *just is* an Image that's widely shared within a particular community, with various ritual and spontaneous Plans springing from it. However, they said very little about the Image in general terms. And they said almost nothing about cultural differences, except to remark that human societies adopt differing views about the importance of time, and of attempts to anticipate the future (p. 123).

If they'd tried to say more, their hand-waving would have been even more evident than when they discussed hammering. When Wallace (1965) applied their ideas in anthropology, his hands didn't stay firmly in his pockets either (Chapter 8.i.a). Even today, forty years later, computational analyses of cultural matters are still largely speculative, and usually based on verbal theories rather than computer models. But, as the example of 'auto-hypnotic' possession indicates (other examples are given in Chapter 8), our improved grasp of the nature of human minds can help us come to grips with phenomena that were previously mysterious in the extreme.

In sum, cognitive scientists have always believed that their approach could help us understand personality, psychopathology, and even culture. MGP were explicit about that. But the early efforts were grossly over-ambitious, and had to be dropped (subsections a–c, above). It was many years before they could be taken up again. Now, it's clear that MGP were right.

For instance, Dennett's work, and Shallice's, helps us understand human freedom. Dienes and Perner's throws light not only on hypnosis, but to some extent on Colby's concerns (neurosis and paranoia) too. An account of "relevance", to be described in Section iii.d below, throws light on the communicative processes involved in what Abelson called attitude change, and on what he termed structured belief systems. And architectural theories such as Minsky's or Sloman's have put nourishing meat onto the bare bones of emotion and personality sketched by Reitman (and Colby and Abelson) in the 1960s.

All these areas of research need to be developed further. To be honest—as Sloman was, in responding to the ludicrous query about simulating terrorists—the computational birds *still* can't catch the personal worm. (Sloman intends to work on his approach "for the next 300 years or so"—2003*b*: 7.) But in the new millennium the worm is, at last, in sight (see Chapter 17.iv).

7.ii. The Spoken Word

If 1960s computational psychology wasn't ready for personality, it was ready for certain aspects of language. As outlined in Chapter 6.i.e, language—thanks to Chomsky—was now being seen in a new way. Specifically, it was now regarded as a generative system whose rules (conceptualized in GOFAI terms) play a role in psychological processing—and may be implemented in computer models. This approach prompted research into NLP (described at length in Chapter 9.x–xi). In addition, it spawned a new branch of experimental psychology.

Because of Chomsky's (1957) influence, most of the very earliest experimental work focused on syntax. Soon, however, psychologists also studied meaning—in vocabulary, sentences, and connected texts.

The most important computationalist influence here wasn't Chomsky, but M. Ross Quillian. His 1960s theory—and modelling method—of "semantic networks" (see 10.iii.a) was widely used in AI and computational psychology alike. That had been his intention all along:

> The purpose of [my research] is both to develop a theory of the structure of human long-term memory, and to embody this theory in a computer model such that the machine can utilize it to perform complex, memory-dependent tasks. (Quillian 1967: 410)

Besides his own experimental work on language and memory (e.g. A. M. Collins and Quillian 1972), many others used theories based on semantic networks to ask psycholinguistic questions. One such was Schank, whose account of conceptual dependencies will be featured in Chapter 9.xi.d. Some other examples of the (voluminous) research involved are described in this section.

Three areas of psycholinguistic research, however, will be postponed to later sections. The early work on "procedural semantics" is described in Section iv.d–e. And two hugely controversial topics will be explored in Section vi.

One of these is the question of non-human languages. Chomsky's *Syntactic Structures* had already implied that to attribute language to any species is to say that it has not only meaningful communication but also syntax; and after the publication of the nativist *Aspects* in 1965, Chomsky fans predicted that no non-human species would qualify. The other is how language develops in human children. Chomsky's emphasis on syntax had raised new questions about this ancient puzzle. It will be discussed later, in the context of nativism in general. So the relative brevity of this section on 'The Spoken Word' shouldn't mislead you. Psycholinguistics has been a thriving area of cognitive science ever since the 1960s.

If psycholinguistics eventually burgeoned beyond Chomsky, so did (some) linguistics as such (see Chapter 9.ix). Today, a new field has been named: *cognitive linguistics.* Two examples, briefly mentioned in Chapter 12.x.g, are situation semantics (Barsalou 1999*a*) and blending theory (Fauconnier and Turner 2002).

Cognitive linguistics has abandoned Chomsky's purist vision of language-in-the-abstract (and the logician's model-theoretic view of meaning: 9.ix.c). Instead, it focuses on the cognitive processes involved in language use. Its researchers may be less likely than psycholinguists to get involved in laboratory experimentation or neuroscientific studies, but they're just as ready to use the empirical results in their work. In short, the line between psycholinguistics and cognitive linguistics is a fine one, more a matter of background emphasis and preferred methodology than of theoretical principle.

a. Psychosyntax

We saw in Chapter 6.i.e that psycholinguistics was born around 1960, thanks to Chomsky—and to George Miller's championing of him. Their "derivational theory of complexity" prompted an explosion of studies on the psychological reality—or, as it turned out, unreality—of transformations.

This Chomsky-inspired experimentation included examples concerning the effect of grammatical knowledge on perception. That knowledge can affect perception top-down was by this time 'old hat', having been supported by Gestalt psychology and the New Look—and by early GOFAI too (see Chapters 6.ii and 10.iv.b). Now, psychologists asked whether knowledge *of syntactic structure* could do so.

Among the experiments designed to find out were some in which random clicks were conveyed to one ear, while spoken sentences were being fed into the other ear (Fodor and Bever 1965; Bever *et al.* 1969). To cut a long story short, the clicks were heard as being at, or near, the phrase boundaries. So *That he was [click] happy was evident from the way he smiled* might be heard as *That he was happy [click] was evident from the way he smiled.*

Even here, there were many complications. For instance, it seemed that it was the boundaries in the *deep* structure which were important. The subject's production of the sentence in speech or writing, when informing the experimenters of the position of the click, could have influenced the result. And semantic influences couldn't always be ruled out. Nevertheless, and unlike the contemporaneous work on the psychological reality of transformations, these findings did hold up over the years.

As for *why* the clicks were 'displaced' in this way, the reason was believed to be one which Donald Broadbent had shown to be generally important. Namely, it was assumed that the moments at which clicks are relatively difficult to perceive are moments when many other information-processing tasks (involved in parsing the sentence) are being tackled. But which tasks are these, exactly? Some psycholinguists used ATNs (augmented transition networks: Chapter 9.xi.b) in answering that question (e.g. Wanner and Maratsos 1978).

(These experiments, like other work on auditory and visual masking, implied that a conscious percept *takes time* to form. This fact would later be highlighted in various discussions of consciousness: 14.x.a–b.)

Once Chomsky started talking also about *semantics* (9.viii.c), experimenters asked—for instance—whether parsing is done independently of meaning (as it seems to be for *'Twas brillig, and the slithy toves | Did gyre and gimble in the wabe*), or whether syntax and semantics are assigned to words simultaneously. In addition, when he confessed to his long-standing nativism—positing "innate ideas" concerning grammar—developmental psycholinguistics suddenly became a hot topic (see Section vi.a). In brief, it's impossible to overestimate the role that Chomsky played in getting modern psycholinguistics off the ground.

However, to say that psycholinguists were hugely excited by Chomsky isn't to say that they took his writings as gospel. This was just as well, for he pulled the theoretical rug out from under them many times over the years (see 6.i.e and 9.viii.b–c). After the very earliest days, most psycholinguists discreetly ignored his current theory. (A few explicitly rejected it, offering their own alternative: 9.ix.b.)

In any event, they were interested in many questions of little concern to Chomsky—specifically, how people *actually* speak.

b. Up the garden path

One intriguing aspect of actual language use is the occurrence of spoken sentences that are unproblematic from the linguist's point of view, but which cause problems in practice.

This isn't a matter of length, for sentences of indefinitely many words may be readily understood. Think of a page-long sentence by Marcel Proust, or—better—of a seventy-four-word example that even young children can delight in: *This is the farmer sowing his corn, that owned the cock that crowed in the morn . . . that lay in the house that Jack built*. But multiply-embedded sentences, which are perfectly legal according to the rules of grammar, are well-nigh unintelligible. Consider, for instance, *The rat the cat the dog the man the woman the car injured married stroked bit chased squeaked*. The only way to get one's head round this is to use paper and pencil to link the right noun with the right verb. Indeed, Miller found that even shorter nested sentences than this one will elicit errors in comprehension and recall: three levels of embedding are enough to cause trouble (Miller and Isard 1964). In other words, grammar as such allows for indefinite complexity, but there's a limit to the grammatical complexity that people can easily handle.

Moreover, there's the strange phenomenon of "garden path" sentences, in which the final words require one to reparse the whole of the preceding string. Karl Lashley gave one of these in his 'Serial Order' paper, as we saw in Chapter 5.iv.a: "Rapid righting with his uninjured hand saved from loss the contents of the capsized canoe". This is normally heard as "Rapid writing with his uninjured hand . . . ", until the occurrence of "capsized canoe". Or consider a much shorter example: *The horse raced past the barn fell*.

Many people find this sentence baffling, even after thinking about it for a few moments. If you're one of them, compare it with *The horse stabled in the barn neighed*. But why should anyone need such a clue? How can seven little words cause such difficulty?

Most Chomskyans didn't care. (They'd retreated to the mathematics.) But one, MIT's Mitchell Marcus (1950–), did. In the late 1970s he wrote a program called PARSIFAL—because what it did was to *parse* incoming sentences (M. P. Marcus 1979, 1980). This was one of the very few computer models to be based on Chomsky's grammar.

(Chomsky's grammar, but not *transformational* grammar. Hardly any models had been based on this, because comparing two tree structures—namely, deep and surface forms—is computationally expensive. Many psychologists assumed that what's computationally expensive is psychologically implausible, so that realistic NLP systems should aim for computational economy. Some *linguists* assumed this too, eschewing transformations as a result: Chapter 9.ix.d–f. Marcus used a post-rug-pulling version known as "trace" theory. In this variant, transformations were fewer and less complex, and sentences were represented by *annotated surface structures*, in which there were *traces* explicitly reflecting the deep structure and derivation.)

Miller's 'Magical Number Seven' had implied that there's a limit to the short-term memory load one can carry when parsing a sentence, and Marcus wondered whether this sort of thing might account for the garden-path phenomenon. More generally, he wanted to discover what type of parsing procedure would necessarily result in language's having certain "universal" features posited by Chomsky (specifically, the "complex noun phrase", "subjacency", and "specified subject" constraints).

That is, he wanted to know how the processing properties of the *mind* might have affected the abstract nature of *language*. Other types of language might be theoretically conceivable, but if their sentences couldn't actually be parsed by human minds then they wouldn't exist. Chomsky himself had suggested that highly abstract features of syntax result from properties of the "mental organ" responsible for language. But, apart from vague references to "memory limitations" for instance, he hadn't said just what those properties might be. Marcus aimed to find out.

His key hypothesis was that the parser inside human heads is *deterministic*. That is, it allows only one choice at every point during the parsing process: namely (in nearly all cases), the right one. This would account, he said, for certain grammatical rules and "universals" that Chomsky had described but couldn't explain. In addition, and especially interesting to psychologists, it would explain the introspective ease and speed of speech understanding.

Here, Marcus was swimming against the tide. Other NLP programs at that time were *non-deterministic*. ATNs, for example, allowed many choices at a given node, and sometimes all but one had to be traversed—and their computations at least partly undone—before the correct parsing was found; the same applied to Terry Winograd's SHRDLU, too (Chapter 9.xi.b). This was computationally expensive, and therefore—so Marcus felt—psychologically implausible.

But deterministic parsing, it seemed, was implausible too. On the one hand, there were the garden-path sentences, where the *wrong* choice is made early on, and this isn't realized (if it's realized at all) until the end of the sentence. On the other hand, there were sentence pairs such as "Have the girls taken the exam today?" and "Have the girls take the exam today!" We can't know whether the initial word "Have" should be parsed as an interrogative (auxiliary) or an imperative (main) verb until we've encountered the fourth word—"taken" or "take", respectively. Yet these two sentences, even without intonation cues, cause us no difficulty. How, then, could parsing possibly be deterministic?

Marcus dealt with the 'take/taken' objection by saying that a deterministic parser can't rely on a simple left-to-right pass through the sentence. It requires some degree of look-ahead, enabling it to inspect the future context. Clearly, if the parser could always inspect the whole sentence, the determinism claim would be vacuous. It's a substantive (and testable) hypothesis only if the size of the look-ahead is specified, and strictly limited. As for the puzzling garden-path sentences, if determinism-with-look-ahead could explain and/or predict *them*, that would be further evidence in its favour.

The crucial questions, then, concerned the size of the look-ahead and how it worked. The answers lay in PARSIFAL's "constituent buffer", described by Marcus as the "heart" of the program. (More accurately, the answers lay in *just how* the buffer was used by the program; those details are ignored here, but see Boden 1988: 110–14.)

The constituent buffer was a left-to-right sequence of three 'boxes', each of which is either empty or holds only one item. Each item is a syntactic category seeking a higher-level attachment—where S is higher than NP, NP is higher then Det, and Det is higher than "the" (see 9.vi). So "item" here means *any grammatical constituent*, including not only words but also syntactic substructures that the parser has already built—such as the NP ⟨ the big black cat ⟩.

How do the boxes get filled/emptied? Items can enter the buffer either from the right (if they're words) or from the left (if they're partially parsed constituents). However, words aren't read in to fill all the empty buffer places. If they were, the buffer would soon overflow. A word can enter the buffer only if some parsing rule has asked to see the contents of a buffer place which happens to be empty. Once an item has entered the buffer, it can be pushed rightwards, if there's an empty box to receive it. But it can't be pushed out of the buffer at the rightmost end. An item can leave the buffer only at the left end, and only after it has been attached to some higher-level constituent.

Never mind the details. The core idea embodied in the buffer was that the parser can look at up to three items *but no more*. So Marcus was claiming that a three-item look-ahead will suffice to parse *all* the sentences, no matter how long they may be, which people can understand without conscious difficulty. The farmer sowing his corn can be easily linked with the house that Jack built, despite the many intermediate clauses (cock, priest, man, maiden . . .), by a succession of automatic bottom-up processes filling, emptying, and refilling . . . only three buffer boxes.

Garden-path sentences, however, do cause difficulty. The reason, said Marcus, is that they need *more* than three boxes. When he made up new sentences that would require four boxes, he found that twenty out of forty people did indeed find them difficult to understand.

What about the other twenty? Well, some of them may have picked the right choice initially by chance. Some may have picked it because of intonation cues. Some may have been less conscious of backtracking (to undo wrong choices) than others. And some—said Marcus—may have learnt to extend their buffer so as to allow four, or even five, boxes.

However, that last suggestion went against the spirit of his theory, which assumed *inbuilt* processing constraints. Possibly, some people may be genetically anomalous and have four boxes (as some have six fingers). But in that case, "learning" isn't in question. Or perhaps some people have learnt to use compensatory processing techniques, which make it appear as though there were a larger buffer when in fact there isn't. In other words, perhaps they've changed the virtual machine involved in parsing, even though the genetically grounded hardware remains the same.

Psychologists could try to address all of those questions. Moreover, they didn't have to accept Chomsky's trace theory in order to do so, even though Marcus himself had hoped to find support for it. Their interest in his work was that he'd given them a way of raising, and thinking about, detailed psychological questions about the effortlessness of everyday speech—and why it sometimes flounders.

c. You know, uh, well . . .

In speech, as opposed to writing, grammatical mistakes abound: the linguist's law-abiding NP–VP is an idealization. In the language of Section iii.a below, real speech is (flawed) performance whereas grammar is (ideal) competence. Some mistakes in performance involve phonetics, not syntax (e.g. spoonerisms such as "shoving leopard" instead of "loving shepherd"). Yet others involve an unhappy choice of vocabulary (think of Richard Sheridan's Mrs Malaprop). Psycholinguists interested in actual language use studied a wide variety of common speech errors, and explained many of them in cognitive terms (Kempen 1977; Cutler 1982).

However, Freud had pointed out in *The Psychopathology of Everyday Life* that some speech errors are due to affect—notably, to anxiety. This may be repressed, in the Freudian sense, or open to introspection. All-too-conscious worries probably underlay some of the grammatical infelicities in the Watergate tapes, for example. (And, of course, the "expletives deleted" too.)

John Clippinger, for his University of Pennsylvania Ph.D. in the early 1970s (Clippinger 1977), tried to show how such real-world details may be sculpted by processes integrating many different aspects of mind. His prime concern, here, wasn't speech as such. Rather, he saw anxiety-based speech errors as a window onto the computational architecture of the mind as a whole.

He'd been alerted to this topic by the anthropologist Claude Lévi-Strauss, and was influenced also by Freud and—especially—Bateson (Clippinger 1977, pp. xv, 158–70). Above all, he was inspired by Winograd's work (and his seminars) on NLP, which gave him "a handle on how to think about and represent cognitive and linguistic processes" (p. xv).

As data, Clippinger used the transcripts of four years of taped therapy sessions of a depressed woman undergoing psychoanalysis. He analysed her speech errors, and tried to explain them in terms of general principles of cognition and emotion.

Besides certain types of grammatical error (e.g. restarting a sentence, or rephrasing a clause in mid-sentence), he noted factors such as hesitation, grunting (*uh*), qualification, euphemism, and the use of contentless expressions such as *you know* or *well*. Each of these occurs in this fragment of the woman's speech:

> You know, for some reason, I, uh, just thought about, uh, about the bill and about payment again. That (pause 2 seconds), that, uh (pause 4 seconds), I was, uh, thinking that I—of asking you whether it wouldn't be all right for you (pause 2 seconds), you know, not to give me a bill. That is, uh—I would (hesitates), since I usually by—well, I immediately thought of the objections to this, but my idea was that I would simply count up the number of hours and give you a check at the end of the month. [And on she goes, and on . . .] But maybe because of it would—uh (pause 2 seconds), it would reduce some—I—the amount of, uh (pause 2 seconds), I—exchange or, uh (pause 2 seconds), I was going to say interchange, but that's not right. And then I thought of, uh (slight laugh) (hesitates), intercourse between us. (Clippinger 1977: 29–30)

Supported by an analysis of similar passages in the body of transcripts, Clippinger suggested that what underlay this whole speech episode was the patient's initially wanting to request intercourse with the therapist—meaning not simply "sex" (forbidden by the superego) but also/instead "a warm, affective, and intimate relationship" (p. 114).

She'd quickly recognized, however, that even this wasn't possible, because of the financial/contractual nature of the transaction.

In listening to such troubled speech, the psychoanalyst (and the sensitive layman, too) can often pick up what's bothering the speaker. But Clippinger wanted to know *just how* the mind's resources can be marshalled to modify the verbal expression of anxiety-ridden topics. His theory of mental processing was much more detailed than Freud's, because he had nearly fifteen years of cognitive science (including Winograd) to draw from. He expressed it as a computer program called ERMA, designed to generate something like the transcript quoted above.

Much as Colby had seen the level and semantic content of anxiety as selecting a particular defence mechanism, so Clippinger saw these factors as determining the types of speech error. ERMA monitored its verbal expressions while they were being generated, and continually adjusted its previous plans accordingly.

A top-level SPEAK-UP program was designed to initiate possible topics for expression. After a topic had been selected, it was passed on for processing organized in five sub-programs, each of which was a complex system in itself. One, named LEIBNITZ (*sic*), contained the system's general knowledge, and was accessed by all the others. Another (CALVIN) dealt with the initiation *and censoring* of the content of thoughts, and of verbal expressions. The third, MACHIAVELLI, found ways of structuring complex content that had already been approved by CALVIN, so that the fourth (CICERO) could then express these concepts in words. As for FREUD, this sub-program tried to work out *why* certain concepts had been thought or expressed. In certain circumstances, it could pass its findings on to MACHIAVELLI for eventual expression in speech (see Clippinger 1977, ch. 7).

These five aspects of processing were called "contexts", not stages. For ERMA wasn't conceptualized as a rigidly sequential program, even though it was implemented by GOFAI techniques. Continual monitoring led to continual interruptions, as the various sub-programs diverted the control of processing away from the current goal. (This was an example of what Winograd had called "heterarchical" processing: Chapter 10.iv.a.) The results of such interruptions were reflected, directly or indirectly, in ERMA's output.

Hand simulations of the program (in which the topic was provided and/or the rules partly applied by Clippinger himself, using paper and pencil) produced non-Chomskyan strings such as: "That, that I was thinking that I—of asking you whether it wouldn't be all right for you, you know, not to give me a bill" (p. 118) and "You know, I was just thinking about, uh . . . well, whatever it was isn't important" (p. 202). In the latter case, of course, the primary goal—to express a desire for "intercourse" with the therapist—hadn't been modified, but completely suppressed.

As for the wholly computer-generated output, this included the following extract (sentences that were realized by ERMA during processing, but didn't appear in the 'spoken' output, appear in parentheses):

> You know for some reason I just thought about the bill and about payment again. (You shouldn't give me a bill.) ⟨Uh⟩ I was thinking that I (shouldn't be given a bill) of asking you whether it wouldn't be all right for you not to give me a bill. That is, I usually by (the end of the month know the amount of the bill), well, I immediately thought of the objections to this, but my idea was that I would simply count up the number of hours and give you a check at the end of the month. (p. 146)

If you compare this with the woman's own words quoted above, you'll see a great similarity—marred, for instance, by her use of one more *you know* and several more *uhs*. Clippinger remarked (p. 146) that the extra *uhs* would be more difficult to model than the *you know*, because they come at points where the speaker interrupts her thoughts to evaluate the impact of what she's going to say. Additional procedures would be needed in both CALVIN and CICERO to make—and make use of—such evaluations. His remark indicates, even without going into the details of the program (which, for its time, were considerable), that ERMA wasn't a mere cheat. It wasn't simply ELIZA-with-knobs-on.

As regards the psychological plausibility of ERMA's output in general, quasi-Turing Tests similar to those carried out by Colby (16.ii.c) showed that psychotherapists found it all too familiar. Moreover, they usually ascribed the same anxiety topics to the 'patient' as had actually been used by the program (pp. 196–210).

Clippinger hoped that his theory might aid practising psychiatrists, in both diagnosis and therapy. The crucial concept here was debugging, since "debugging a computer program and 'curing' a patient are similar processes, both of which require teleological descriptions to proceed" (p. 191).

Expanding on Gerald Sussman's pioneering AI work on self-debugging (10.iii.c), Clippinger pointed out that such descriptions (bug identifications) are often difficult to achieve. That's especially true if the manifest problem is due to more than one bug. For instance, suppose that a patient diverts attention from the original goal by formulating one or more less anxiety-ridden substitute goals. It may be difficult for her to know what her original goal was—particularly if (as could happen in ERMA) the substitute goals weren't *sub-goals*, but were produced by independent computations. It's difficult for the therapist too, but experience (and disinterestedness) helps.

'Cognitive' behaviour—dream reports, free association, and discourse on the couch—can provide clues as to just which thought structures and frustrated goals are involved. And if the Third Force are to be believed, ego-driven discourse can help suggest which alternative structures and goals might be most fruitful. But only the superego (which was responsible for the patient's problems in the first place) can effect a cure, by constructing alternative processing mechanisms. If that's done, the anxiety-fuelled speech errors (and other behavioural problems) will disappear, because they'll no longer be generated.

The bad news is that this self-debugging—like the computational deconstruction/reconstruction involved in grief and mourning (Section i.f, above)—will inevitably take a long time. Clippinger suggested experiments to discover whether training patients to think in terms of simple forms of debugging would help them to resolve their life problems. (I've been unable to discover whether he ever followed up that suggestion. But "cognitive therapy" in general often involves analogous exercises: D. M. Clark and Fairburn 1996.)

Clippinger gave two talks at an invitation-only TINLAP workshop (Theoretical Issues in Natural Language Processing) in Cambridge, Massachusetts, in 1975. One was on ERMA, the other on impersonal aspects of NLP. Despite this airing, his work on pathological speech fell into a black hole, and his hopes (217–18) for continuing interdisciplinary research into the effects of affect on cognition weren't realized.

Partly, this was because he left academia to set up a commercial NLP company. As an outsider to the scientific research community (Chapter 2.ii.b–c), his work was hardly known. (I've cited it myself, and so has Colby—K. M. Colby and Stoller 1988: 44; but I've never come across another reference to it.) More to the point, it was too early: he'd followed this theoretical path as far as it was then possible to go.

Perhaps it's still too early, even now. Current work on user-friendly "soft-bots" or computer/VR agents, for example, includes attempts to give their speech an 'emotional' dimension (see 13.vi.b–d). These are painfully crude in comparison with Clippinger's model, constructed almost thirty years ago. However, the growing attention to theories of computational architecture may make it possible for his ideas to be developed.

d. Meaning matters

Although—because of Chomsky—psycholinguistics started with a focus on syntax, it soon moved on to meaning. For that, after all, is what syntax is *for*. Broadly speaking, questions could be asked about the meaning of vocabulary, or sentences, or whole texts.

Vocabulary meaning was the main focus of an intriguing book by Miller, co-authored by Johnson-Laird (1936–). Described as a study of "psycholexicology", it sought to explain the development and use of words by means of procedural semantics.

A procedural semantics represents the meaning of language in terms of how the mind (or AI system) actually processes it. Words and sentences are thought of as mini-programs, as sets of instructions to the hearer to search for or set up certain representations in their mind and to perform certain computational/inferential operations on them. Winograd (1972) was one of the first cognitive scientists to recommend this type of approach, but as explained in Chapter 9.xi.b he didn't claim that his program SHRDLU was psychologically realistic. It was Miller and Johnson-Laird who introduced procedural semantics into empirical psychology.

Their 750-page book *Language and Perception* (1976) aimed to show how we develop word meanings by means of our sensori-motor interactions with the world, and how we use them thereafter. It ranged over perceptual psychology and psycholinguistics; the psychology and neurophysiology of motor action; non-extensional and deontic logic; and philosophical research on (for example) definite descriptions, semantic primitives, and concepts in general. And it offered detailed procedural definitions of many words, including prepositions such as *on*, *in*, *under*, *above*, *left*, and *here*.

In justifying their fine-grained *and* interdisciplinary approach, they said:

> So detailed a discussion of "That is a book" runs the risk of making an almost reflex act [i.e. understanding that sentence] seem peculiarly complicated. The very simplicity of the example makes analysis verbose, but without it there is a danger of suggesting something more than, or something different from, what we have in mind. The meaning of "book" is not [1] the particular book that was designated, or [2] a perception of that book, or [3] the class of objects that "book" can refer to, or [4] a disposition to assent or dissent that some particular object is a book, or [5] the speaker's intention (whatever it may have been) that caused him to use this utterance, or [6] a mental image (if any) of some book or other, or [7] the set of other words associated with books, or [8] a dictionary definition of "book", or [9] the program of operations (whatever they are) that people have learned to perform in order to verify that some object is conventionally labelled a book. We will argue that the meaning of "book" depends on a general concept of books; *to know the meaning is to be able to construct routines that involve the concept in an appropriate way*, that is, routines that take advantage of the place "book" occupies in an organized system of concepts. (1976: 127–8; italics added)

As you'll have guessed, if you didn't know it already, every one of those nine rejected disjuncts was a semantic theory held by some psychologists and/or philosophers.

Miller and Johnson-Laird posited a set of semantic primitives (see Chapter 9.viii.c), but of a novel kind. They allowed that some words are undefinable by other *words*. But they insisted that they're analysable psychologically. The meaning, they said, is carried by underlying concepts ("ultra-primitives") that don't correspond exactly to any natural-language words. These are contained—and often combined—in interpretative procedures that may be recursive and/or hierarchical. In short, to learn even an apparently 'simple' word may be to acquire a fairly complex procedure for using it.

On this theory, the semantics of those words whose meaning depends on our material embodiment is biologically plugged in—much as the semantics of low-level descriptions in vision is plugged in (Section v.b–d, below). And that includes a lot of words: hierarchical class inclusion, for instance, depends—according to Miller and Johnson-Laird—on the more primitive concept of spatial inclusion. But it didn't follow that the meanings of lexically indefinable concepts are innate. On the contrary, the two authors used up many pages in describing how infants learn the meanings of words.

They argued, for example, that some meanings (some interpretative procedures) can't be acquired without movement in and action on the material world. They cited evidence showing that infants learn the word "in" before they learn "on", and both of these before "under" and "at". And they pointed out that understanding these words requires the availability of bodily action schemata. For if it's physically possible to put *x* in *y*, then a 20-month-old child asked to put *x* on *y* will instead put it inside *y*; and if it's possible to put *x* on *y*, then a child asked to put *x under y* will place *x* on top of *y* instead. Their explanation of why children learn the word "at" later still was that its meaning is more complex: it involves the abstract notion of *region* and (when fully developed) concepts of *size*, *salience*, and *mobility*.

The computer programs (not actually implemented) that Miller and Johnson-Laird provided for *on*, *in*, and *at*, and many other words besides, were written as ATNs (9.xi.b). They were designed to interpret various linguistic features, such as the tense and aspect of verbs, and the use of definite descriptions (*The* such-and-such . . .). The chosen features were very general—within one language, or even across many; they were clearly defined in syntactic terms; and their meanings (and truth conditions) were relatively clear.

Syntactic cues helped determine the sort of processing that was specified in their ATNs. So an interrogative sentence would immediately prompt a search for the information requested, whereas a declarative sentence would (indirectly) lead to a search only if this was necessary in order for the program to construct its semantic representation. Likewise, the active verb in *Did Mary meet John at two o'clock?* instructs the hearer to search their memory for events in which Mary meets someone, to locate any in which she meets John, and to see if one of those happened at two o'clock. By contrast, the passive verb in *Was John met by Mary at two o'clock?* instructs the hearer to locate John's meetings first, then the subset in which he meets Mary, and finally the one (if any) which occurred at two o'clock. This analysis, said Miller and Johnson-Laird, explains our intuitions that the two sentences *both do and don't* have the same meaning.

Despite having been Chomsky's earliest collaborator, Miller was now swimming against the Chomskyan tide. For he and Johnson-Laird opposed Chomsky on the "autonomy" of syntax (9.ix.c). So did the philosopher Richard Montague, who in 1971 had "died in his home in Los Angeles, at the hands of persons still unknown" (Furth *et al.* 1974). But his radical ideas wouldn't be picked up by them—more strictly, by Johnson-Laird—until later (see Section iv.d). Meanwhile, they argued that the psychological evidence didn't support Chomsky's claim that a syntactic representation is built before the semantic one. Rather, syntactic cues are used during interpretation (as in the Mary/John examples), to help construct a representation of the meaning.

Their book attracted some interest, although not as much as it deserved. George Mandler (personal communication) feels that "the title was all wrong—in 1976 people interested in language weren't interested in perception, and also *vice versa*". A quarter-century later, the citations totalled only about 700. Moreover, very few of the citations in the first fifteen years were in mainstream psychological journals, as opposed to journals of linguistics, AI, and developmental psychology (at that time, not a high-status field).

In retrospect, Johnson-Laird (personal communication) believes their research would have made more of an impact if they'd published the 200-page paper they'd written by 1972. But they decided to turn it into a book—which grew, and grew, and grew:

> There was a great exchange between George Miller and Phil Johnson-Laird when they were finishing that book on perception and language. They had gone through revision after revision after revision after revision and I can't remember whether it was Phil or George who said, "Well, we've got to decide whether it's going to be perfect or Thursday." So they decided it was going to be Thursday, to the great relief of Harvard University Press. (J. S. Bruner, on whether one can ever publish "the final account of anything", quoted in Shore 2004: 8)

Maybe there'd been too many Thursdays already. For few people read the book carefully from cover to cover. One critic, for instance, accused them of reducing language to perception, or perceptual primitives—which they'd specifically said isn't possible. Many people didn't bother to read it at all, restricting their view to the journals. And the two authors didn't publish any follow-up research. (Johnson-Laird turned to other topics, namely inference and problem solving by means of mental models: see Section iv.d–e, below.)

Together with Winograd's SHRDLU, *Language and Perception* did encourage its readers to aim for some kind of *procedural* semantics. But the details were largely ignored. Instead, most of the opposition raised broad issues in the philosophical psychology of language. For instance, Jerry Fodor (1978*a*, 1979) accused Miller and Johnson-Laird of being crypto-verificationists. (For a reply, see Johnson-Laird 1978.) That was fair enough: after all, they'd raised the philosophical issues in the first place. But they'd also offered detailed hypotheses about the meaning and acquisition of particular words, and those hypotheses weren't carefully addressed. Alternative procedural models of individual words, specified in comparable detail, were rare—not to say conspicuously lacking.

Given the unsuspected complexities of *at*, and even the hidden complexities of *on*, that's hardly surprising. But it was due also to the fact that, by the late 1970s, many psycholinguists were considering not single words, but sentences, conversations, or even stories.

They weren't (usually) writing computer programs. But they were doing research prompted by computational ideas—specifically, by recent AI research on NLP (9.xi.d).

The important influences were Schank and Abelson's theories of conceptual dependency (CD) and scripts, including Wendy Lehnert's work on question answering (soon to be mocked in the ''Chinese Room'' argument), and David Rumelhart's models of story grammars. These NLP ideas were followed up by experimentalists, many of their papers gracing the pages of the recently founded journals *Cognitive Psychology* and *Cognitive Science*.

For example, John Bransford and Marcia Johnson (1972, 1973) studied the spontaneous inferences—about cause, intention, or spatial location—that people make in interpreting single sentences or very brief texts. Gerard Kempen (1977) applied CD theory to explain common hesitations and speech errors, less florid than those studied by Clippinger. As for story grammars, which represented the plots of stories, these were studied in psychological terms—and related to computer models of text interpretation (van Dijk 1972; Rumelhart 1975; J. B. Black and Wilensky 1979; J. M. Mandler 1984). Many psychologists, including Bower and Jean Mandler, tried to find out whether people's understanding of and memory for stories are grounded in high-level concepts like the schemas, plans, goals, scripts, MOPs, TOPs, and TAUs emanating from Yale in the 1970s (J. M. Mandler and Johnson 1976; Thorndyke 1977; G. H. Bower 1978; Black and Bower 1980; Lichtenstein and Brewer 1980; R. P. Abelson 1981*a*; J. M. Mandler 1984). These concepts were grounded in Abelson's earlier work on plans and scripts (see Section i.c, above). So a MOP (memory organization packet) denoted the central features of a large number of episodes or scripts unified by a common theme, such as *requesting service from people whose profession is to provide that service* (Schank 1982). TOPs (thematic organization points), too, were high-level schemata that organized memories and generated predictions about events unified by a common goal-related theme (such as *unrequited love* and *revenge against teachers*); but, unlike MOPs, they stored detailed representations of the episodes concerned, rather than their thematic structure alone. And TAUs (thematic abstraction units) were defined as abstract patterns of planning and plan adjustment, such as *incompetent agent, hypocrisy*, and *a stitch in time saves nine*, that aid not only language understanding but analogy and reminding as well (Dyer 1983; and see Chapter 9.xi.d).

Just what these psychologists found out is a tricky question. Because the ideas about schemas and scripts were higher-level and less precise than ideas about syntax, or than Miller and Johnson-Laird's psycholexicology, their research was less detailed accordingly.

Bransford's experiments, for instance, couldn't show that CD theory captured the specifics of human psychology. At best, they showed that it reflected *something like* what goes on in human minds. Similarly, the discovery that people do indeed remember stories in terms of their thematic structure showed only that *something broadly like* the Yale group's TOPs and TAUs was at work. It would have been surprising if that weren't so, for these concepts were intuitively identified in the first place, and TAUs were even named by clichés (see Chapter 9.xi.d). Critics therefore objected that CD theory—whether programmed or not—was no more than common sense dressed up in novel jargon (Dresher and Hornstein 1976).

However, theories constructed with *no* thoughts about computational processes, such as Frederic Bartlett's pioneering version of schema psychology (which the NLP scientists had picked up), were even vaguer. Accordingly, they were even less likely to prompt detailed questions about what actually goes on inside our heads. Given what was going on in AI at the time, it's no accident that a host of experimental studies on language understanding by means of semantic schemas were done in the mid- to late 1970s.

7.iii. Explanation as the Holy Grail

Chomsky's work didn't turn psychologists' eyes towards only language: it turned them towards psychological explanation as such. Over the next quarter-century, *what counts* as an explanation was a controversial topic within cognitive science.

Today, it's not easy to appreciate how novel this was. Quite apart from *what* computational psychologists were saying about explanation, *that they were saying anything at all* outside the opening pages of an introductory textbook was surprising. Despite Craik's having devoted a whole book to the "philosophy" of psychological explanation in the early 1940s (Chapter 4.vi), this topic wasn't much debated. Worse, it was widely shunned. As late as the 1960s, student texts would still celebrate psychology's "liberation" from philosophy—only to follow with a tendentious statement of operationalist positivism, presented as though it were plain fact.

To be sure, the Third Force personality theorists had questioned the positivist party line. Perhaps their most philosophically sophisticated work was Gordon Allport's defence of "idiographic" (and proactive) explanations, as against "nomothetic" (and reactive) ones (G. W. Allport 1942, 1946, 1955). Very broadly speaking, these mapped onto Cronbach's "applied" and "academic" approaches. Idiographic explanations, Allport said, were based in human empathy, and they applied to *particularities*: what his colleague Bruner has recently called "human beings in specific situations" (Chapter 1.iii.f).

In response, the psychologist Paul Meehl (1920–2003) at the University of Minnesota, where philosophy of science was a special strength, had recently provided a thorough discussion of this issue (Meehl 1954). He championed the nomothetic approach. For Meehl, reliable personal prediction could be based only in some "statistical cookbook", whether represented in the tables and tick-boxes of the MMPI (a personality test developed at Minnesota) or tacitly in clinicians' heads. (Prediction as pattern recognition.)

In choosing to write at length on these issues, however, Meehl was an exception. Most experimentalists agreed with him, but didn't bother to say so. In short: for 'Newtonian' psychologists, philosophy was a dirty word. Hence the scandal when Chomsky not only started talking about explanation, but defiantly placed himself within a highly unfashionable—namely, rationalist—philosophical tradition (9.ii–iv).

By the early 1980s, things had changed. A hugely influential book on vision opened with a chapter on 'The Philosophy and the Approach' (D. C. Marr 1982: 8). One year later another influential book, on reasoning, started by defending a Craikian analysis of explanation—and a functionalist philosophy of mind (Section iv.d, below). *BBS* featured a lengthy debate on the philosophical relation between computation and cognition, soon followed by an even lengthier book from the main protagonist (Pylyshyn 1980, 1984). And when Colby (1981) described PARRY in the pages of *BBS*, he—and the peer commentators—devoted many *BBS* column inches to discussing *what it means* to say that a model is "equivalent" to the theory and/or phenomenon being modelled (Colby 1981: 532–4).

A host of other examples could be given. In a word, the philosophy of psychology had become respectable again. (It was always respectable for *philosophers*, of course: see Chapter 16.)

a. Competence and performance

I said, in Chapter 6.i.e, that Chomsky thought of transformations as timeless mathematical functions whereas psychologists thought of them as actual mental processes. That's true, but misleading. He was happy enough to interpret them *also* as psychological processes, when the evidence went that way—as it did, for a while, with respect to his and Miller's "derivational" theory. But when the evidence became problematic, he declared it irrelevant and retreated to mathematical linguistics (cf. Itkonen 1996: 493).

The reason why he "pulled the rug from under" the psychologists in the 1960s *wasn't* that experiments were casting doubt on the psychological reality of deep structure and transformations. It was that he believed he'd found a more formally elegant way of representing the abstract structure of language.

A 'pure' mathematical theory isn't under any threat from facts. But the claim that it describes something in the real world is. By the mid-1960s Chomsky had moved beyond pure mathematics, for he was now making claims about language (abstractly conceived) and *mind* (9.ii and vii, and Section vi.a below). He even confessed that "linguistic theory is mentalistic, since it is concerned with discovering a mental reality underlying actual behavior" (1965: 4). This meant that he couldn't justify his ignoring psycholinguistic experiments simply by saying, "I'm a mathematician: get off my back!" Rather, he justified it by drawing a psychological distinction between *competence* and *performance*.

Performance concerns what people actually do when they speak on particular occasions (English as she is spoke). It's complicated by many different factors, including "memory limitations, distractions, shifts of attention and interest, and errors (random or characteristic) in applying [one's] knowledge of the language in actual performance" (Chomsky 1965: 3). And it's what experimenters have to work with when they observe their speaking subjects.

Having defined performance, however, Chomsky said almost nothing about it. He'd have had no interest in Clippinger's work, for instance. So far as he was concerned, performance got in the way of the *really* interesting issues. It was the other concept, competence, which fascinated him.

For the record, Chomsky's neglect of performance eventually led to "a gulf between linguistics and the rest of cognitive science that has persisted until the present" (Jackendoff 2003: 652). This explains the "irony" mentioned in the preamble to Chapter 9, namely, that although theoretical linguistics was hugely important in the origins of cognitive science, it's now almost invisible. Various people, including two ex-pupils of Chomsky, have recently tried to effect a rapprochement between theoretical and empirical studies of language (e.g. Pinker 1994; Jackendoff 2002, 2003). In *Aspects of the Theory of Syntax*, however, Chomsky had erected a solid firewall between the two.

The notion of competence applied not to real-world speakers, with their *ums* and *ahs* and egregious grammatical errors, but to "the ideal speaker–listener" (1965: 3). Indeed, his second major book (*Aspects*) opened with the words "Generative Grammars as Theories of Linguistic Competence".

Competence, he said, is the underlying knowledge of grammar that's possessed by every native speaker—considered not as a list of facts but as a system of generative processes. A "fully adequate" grammar must assign a structural description to any sentence of the language, indicating "how this sentence is understood by the ideal speaker–hearer" (1965: 4–5).

Introspection is useless for discovering this: "A generative grammar attempts to specify what the speaker actually knows, not what he may report about his knowledge" (1965: 8). Since "any interesting generative grammar" will be dealing with "mental processes that are far beyond the level of actual or even potential consciousness", it follows that "a speaker's reports and viewpoints about his behavior and his competence may be in error" (p. 8).

The native speaker is needed, to judge word strings as acceptable or not. But beyond that, the study of competence is an abstract exercise—to be done by linguists, not experimentalists. (How Chomsky went about it is described in Chapter 9.)

His notion of competence underlay his view of *explanation*. To explain language understanding, he said, was to show that it exemplified the structural principles defined by his generative grammar. A theory may be merely "descriptively adequate", in that it reflects/predicts a relatively small set of data. But one which is "explanatorily adequate" identifies the underlying mechanisms, so can be extended to many other examples outside the initial data set. Chomsky's view was that in any fully "adequate" explanation, the abstract ideal must always come first.

This prioritizing of competence quickly became one of the most well-known aspects of his work. (Newell and Simon, in emphasizing "task analysis", were making a similar point at much the same time: see Section iv.b.) Clearly, Chomsky's sort of explanation was very different from operationalist laws linking dependent/independent variables, and negating some theoretically unmotivated null hypothesis.

Others soon followed him in asking what a psychological explanation *is*. And many focused on something he'd ignored: how *a functioning computer model* can be seen—and judged—as a psychological theory.

The first serious discussion of computational explanation in psychology was written around the time of *Aspects*, by Chomsky's disciple and colleague Fodor (1968; see 16.iv.c). Granted, Hilary Putnam (1960) had already argued that psychology should be computational (16.iii). But he'd been thinking of folk psychology rather than the professional variety—and he hadn't even mentioned computer modelling. (To be fair, there wasn't much to mention—but the Logic Theorist had already made its mark.) Fodor went further. Dismissing the reductionist assumption (held by the behaviourists and by Hebb) that any explanations of S–R correlations must be neurological, he saw the "special science" of psychology as offering computational explanations.

But even Fodor didn't discuss particular computer models in detail. As computer simulation became increasingly common, many people started asking *just how* a program could count as an explanation. Some were professional philosophers, such as

Gunderson (1968, 1971), then at UCLA (later, at Princeton). Others were psychologists, able to relate their philosophical arguments to specific data and theories in the field.

Foremost among these were Newell and Simon, whose "Physical Symbol System" (PSS) hypothesis was a general philosophy of mind *and of explanation*. The next subsection could have been devoted to them—but their PSS hypothesis will be discussed in Chapter 16.ix.b, instead. However, the vision researcher David Marr (1945–80) was important too.

b. Three levels, two types

Some ten years after *Aspects*, Marr developed a "three-level" account of psychological explanation which he explicitly likened to Chomsky's, and which received almost as much attention as Chomsky's had done.

Marr's first, theoretically most basic, level was the "computational" level. This identifies *the task* of the psychological domain concerned. Chomsky had shown, he said, that the core task of language is parsing. And the task of vision, he argued, is mapping from 2D information on the retina to 3D information about the scene. At the computational level, it's defined in terms of timeless mathematical functions, saying nothing about *processes*. (Compare: "competence" and the "ideal" speaker.)

Unlike Chomsky, however, Marr was a psychologist: he wanted to know *how things happen* in the mind/brain. So he posited two more levels: one for the mind, one for the brain.

The "algorithmic" level specifies a set of information processes capable of computing the abstract functions identified at the computational level. In principle, many different processes will be capable of doing this. The psychologist's job, then, is to propose candidate algorithms, to test their coherence/results in computer models, and to find out which ones are used in the real world. Finally, the third level deals with "implementation". It describes the material embodiment of the chosen algorithm, showing how *those* processes can be carried out by *this* stuff—namely, the neurones (or, in a computer model, the hardware).

It had been Minsky, not Chomsky, who'd first alerted Marr to the idea of the computational level. Minsky had remarked, at a talk on the cerebellum given by Marr in 1972, that to know *what questions we should be asking* about the cerebellum we must concentrate on "the problem of motor control", considered as a set of abstract constraints (see 14.v.f). For Marr, this was an epiphany. He generalized Minsky's remark to vision, and later (recognizing Chomsky as a soulmate) to psychology as a whole.

At a vision conference in late 1973, he spoke of two 'Levels of Understanding'. He developed this idea with his colleague Tomaso Poggio, who was thinking along similar lines (W. Reichardt and Poggio 1976; Marr and Poggio 1977). By 1976, the two levels had stretched to three. And in his book, the three-level theory took pride of place.

Previous research on vision—theories of stereopsis, for example—was dismissed by Marr as largely irrelevant. He criticized his predecessors less for getting their facts wrong than for having asked the wrong questions in the first place. And that, he said, was because they'd ignored the computational level. He even said the same thing about Newell and Simon's work on mental arithmetic (Section iv.b)—although he admitted that he couldn't say what basic "task" it was which they were ignoring (1982: 348).

In short, his three-level theory was supposed to cover *all* psychological explanation. Psychologists who didn't adopt this approach might provide intriguing *descriptions* of behaviour—but they were doing natural history, not science.

There were three important caveats, often raised over the following years. First, there's no such thing as "the" algorithmic level. There are usually many different layers of processing, many different virtual machines, involved in a 'single' psychological phenomenon. Marr himself posited several stages of visual processing (see Section v.b). And the architectural theories discussed in Section i.e–f are more complex yet—though Marr, of course, wouldn't have regarded them as *explanations*. (One philosopher proposed an intermediate explanatory level, "Level 1.5", which identifies the function being computed *and* the information used to do so, but without specifying an algorithm: Peacocke 1986; cf. Peacocke 1996.)

Second, there's no such thing as "the" implementation level. There are retinas, neural networks, neurones, dendrites, synapses, neurotransmitters, ions . . . all of which must figure in an adequate neuropsychology (and in persuasive computer models thereof: see 14.ii). And third, what's theoretically prior needn't be chronologically prior. One can usefully study the retina, for example, without first having an abstract definition of the task of vision (see 14.iv.a).

Marr had given the computational level priority: questions at the other levels must be posed in light of analyses at the first. And the theoretical priority, for him, went from second to third level too: knowing what the algorithm is (or may be) enables us to ask the right questions, to look for the right things, when we study the hardware. Chomsky had said the same, with respect to linguistics and the neurophysiology of language:

> [The] mentalistic studies [of language] will ultimately be of greatest value *for the investigation of neurophysiological mechanisms*, since they alone are concerned with determining abstractly *the properties that such mechanisms must exhibit and the functions they must perform*. (Chomsky 1965: 193; italics added)

In short, both Marr and Chomsky (like Broadbent before them) pointed out a main theme of Chapter 14: that a computational psychology can tell the neuroscientist what to look for.

In practice, however, there's a back-and-forth dialectic between the levels. Marr allowed that discoveries about neurophysiology (lateral inhibition or feature detectors, for instance: 14.iii.b and iv) can sometimes suggest hypotheses about the information processing involved. He even allowed that, in principle, processing constraints might affect the theory at the computational level:

> Finding algorithms by which Chomsky's theory may be implemented is a completely different endeavor from formulating the theory itself. In our terms, it is a study at a different level, and both tasks have to be done . . . [Nevertheless, it] even appears that the emerging "trace" theory of grammar (Chomsky and Lasnik 1977) may provide a way of synthesizing the two approaches—showing that, for example, some of the rather ad hoc restrictions that form part of the computational theory *may be consequences of weaknesses in the computational power that is available for implementing syntactical decoding*. (D. C. Marr 1982: 28–9)

So Marr's "first/second/third" labelling was what Karl Popper (1935) had called a "rational reconstruction" of scientific psychology, not a description of what actually goes on when psychologists do their work.

As well as defining three "levels" of explanation, Marr defined two "types" of theory. Type 1, he said, is based on an abstract identification of the task, as described above. Type 2 is very different—and, in practice, difficult or even impossible to achieve.

A Type 2 theory applies if and when the information processing is carried out by "the simultaneous action of a considerable number of processes, *whose interaction is its own simplest description*" (D. C. Marr 1977: 38). So a Type 2 explanation would be nothing less than a complete description of the processes responsible for the phenomenon concerned. What happens, happens: in explaining how it happens, there's no more—and, crucially, no less—to be said.

Marr wasn't the first to suggest that some psychological phenomena might be of this kind. In his Hixon Lecture in 1948 (Chapter 12.i.d), von Neumann had declared:

> [It] is futile to look for a precise logical concept, that is, for a precise verbal description, of "visual analogy". It is possible that *the connection pattern of the visual brain itself* is the simplest logical expression of this principle. (von Neumann 1951: 24; italics added)

But if that's so, it's not clear that we should speak of explanation (or theory) at all. Explanation is a psychological concept: it covers any account, scientific or otherwise, which makes something *more intelligible to someone* than it was before (Boden 1962). Sometimes, the neural "connection pattern" may be simple enough for us to understand how it produces the behaviour. This may apply to the female cricket's recognition of her mate's 'song' (15.vii.c). But mammalian nervous systems are far more complex. Certainly, one can (today) simulate a vast amount of psychological and neurological data in a single computer model. But one runs the risk of having an all-singing, all-dancing model that performs well (i.e. that matches the experimental data) but which no one can actually *understand* (see 14.vi.d).

c. The sweet smell of success

The pyrrhic victory promised by a complex Type 2 simulation prompts the question of what a "successful" computer model would be. What counts as a "good match" between the model and the *theory* it's supposed to be modelling? And what counts as a "good match" between model and *data*?

In the very early days of cognitive science, these questions weren't at the top of the list. Probably the people who took them most seriously were the AI pioneers Newell and Simon. It was difficult enough to get a computer program to do anything interesting, never mind worrying about degrees of match. All too often, the lack of match was painfully obvious—as in the quasi-personal models described in Section i.a–c.

In the late 1970s and 1980s, however, a great deal of ink was spilled on these philosophical/methodological questions. That was inevitable, for by that time many programs could at least *pass* as psychologically relevant. Short of raising one's hands in wonder (or horror), how could they be evaluated?

The most widely discussed penman was the Canadian psychologist Zenon Pylyshyn (1937–), already well known because of his counter-intuitive theory of imagery (Section v.a, below). The very first issue of *BBS* included a philosophical paper by him, and a similar piece joined it two years later (Pylyshyn 1978, 1980). A book chapter appeared in 1979, and was included in a popular anthology in 1981. And his book carried the argument further (Pylyshyn 1984). Thanks in large part to the interdisciplinary peer commentary in *BBS*, his ideas gained a wide audience within the field.

Pylyshyn championed a strong version of mind-as-machine. He had no time for "the computer *metaphor*". Computational psychologists should bite the philosophical bullet, and allow that a computer model might do *the very same things* as the mind does, *in the very same way*. In other words, mental processes *really are* computations (by which he meant formal transformations defined over representations: 16.iv.c).

A *successful* computer model, according to Pylyshyn, is one in which some (specifiable) aspects are "strongly equivalent" to the mental processes concerned. That is, both can be represented by the same program in some theoretically specified virtual machine. As for how psychologists can test for this, he said, they needn't depend only on direct evidence, such as 'matching' protocols in problem solving. That one system—mind or machine—takes computational steps which the other one doesn't, may show up (indirectly) in memory use, error patterns, and time factors.

Not all computationalists were entirely happy with Pylyshyn's account. Sloman (1978, ch. 5), for example, argued that whether a program did things "in the same way" as human minds was an ill-defined question, not least because which similarities/differences were relevant could depend on context. Nevertheless, the discussion spurred by Pylyshyn's work sharpened the issues significantly.

Besides helping cognitive scientists to decide which programs were genuinely interesting, Pylyshyn's work on explanation addressed a truism that was increasingly being used as an objection. Computational psychologists were often told: "Just because a computer does something in a certain way, it doesn't follow that the mind/brain does it in that way too."

This was (and still is) trotted out triumphantly as though it applied *only* to computer modelling. In fact, it was a special case of something much more general: what philosophers of science call "the underdetermination of theory by evidence" (e.g. Laudan 1998). In principle, *every* empirical data set can confirm (can be explained by) indefinitely many theories. (Compare: infinitely many curves can pass through a given set of points.) In short, chemists and computationalists are in the same philosophical boat—but most people notice the boat only in the latter case.

Usually, scientists would be lucky to have even two or three well-confirmed theories to choose from. As Chomsky had (in effect) said in his own defence, *My grammar fits the facts. If someone wants to oppose it, let them produce a rival that fits as well, or better*. (Eventually, they did: see 9.ix.d–f.) Pylyshyn's work suggested rational ways of choosing between superficially equivalent computer models.

Even more to the point, computational concepts and/or modelling had enabled psychologists to formulate rigorous explanatory hypotheses *for the first time*. As Chomsky, Newell and Simon, Marr, and Pylyshyn all pointed out, the vagueness quotient of previous theories was immeasurably higher than that of those couched in computational/informational terms.

d. Chasing a will-o'-the-wisp?

Could psychological explanation cover individual thoughts? Could it show precisely why *that* idea, rather than any other, came to mind? (Why *that* association, *that* slip of the tongue, *that* dream content?) And could it explain why *this* particular belief is accepted, rather than some other one?

Many people have answered "No". For Descartes, reasoning involved human "freedom": even in a deductive argument, he said, we can freely choose not to assent to the conclusion. Similar views are popular today, especially in the contexts of moral choice and creativity.

Eventually, cognitive science would address both of these (see Section i.g, above, and Chapter 13.iv, respectively). But its early focus was on more mundane types of thought. These included the New Look's studies of how concepts can affect perception; various forms of 'hot' cognition; and problem solving of diverse kinds (Sections iv.b–e, below). The hope—indeed, the assumption—was that conceptual thinking could be corralled by science.

This hope flourished for some twenty years. But in the early 1980s, Fodor caused a sensation (again!) by attacking it as a delusion. Having championed computational psychology since its inception, he now argued that it couldn't cope with the higher mental processes (Fodor 1983: see 16.iv.d). More specifically, it couldn't explain the origin of individual beliefs.

Explaining individual beliefs in scientific terms, he said, is impossible, because of the many degrees of freedom—i.e. the many generative possibilities—available in conceptual thought. Non-demonstrative inferences are less constrained than deductive ones are. Indeed, virtually any concept or belief can turn out, with sufficient subtlety and imagination, to be relevant to any other. For instance, skilled teachers or poets can draw previously unsuspected analogies to mind, so that their audience accept new beliefs and/or come to see things in a new way.

According to Fodor, this doesn't apply only to poetry, metaphor, or imaginative teaching. It applies also to what he called "the fixation of perceptual beliefs" (1983: 102–3). To believe (because we can see it) that there's a cup of coffee on the table involves not only vision, but maybe smell, and certainly memory—not least, for conceptual/linguistic knowledge about cups, coffee, tables, social rituals, and perhaps even table manners.

The integration of all these psychological sources can't be scientifically captured, said Fodor, because common sense (central processing) isn't *modular* (see Section vi.d–e below, and Chapter 16.iv.d). Modular computations are *encapsulated* and *domain-specific*: that is, they can't be influenced by each other, nor by high-level concepts or beliefs. Common-sense reasoning isn't encapsulated, since it consults many different kinds of information in computing answers to its questions. Nor is it domain-specific, since it can answer questions about many different things (1983: 104 ff.). (To do so, Fodor claimed, it doesn't use a proprietary form of representation but a common, domain-general, "Language of Thought": 16.iv.c.) In short, it's scientifically unmanageable.

We can have hunches, of course: everyday gossip abounds with them, and so does Freudian psychology. Sometimes, we can even find experimental support. (For instance, Maier's 1931 study of functional fixedness showed that the idea of tying a weight to a string, to turn it into a pendulum, can—note: *can*, not *must*—be prompted by seeing the experimenter brush against it 'accidentally', causing it to swing: 5.ii.b.) But it's impossible for a scientific theory to predict someone's detailed thoughts, or even to explain them *post hoc*. Even if we knew of every single idea in someone's mind, we couldn't be sure just which ones had led them to think *this* thought: the system is too complex.

This made sense of Allport's defence of idiographic explanation (see above). When we want to explain particular thoughts of particular people, we must rely largely on common sense, personal empathy, literary sensitivity, or a psychotherapist's 'intuition'. But whereas Allport was content to regard these as scientific "explanations", Fodor wasn't. He saw them merely as more or less well-informed, more or less plausible, hunches—or, in other words, hermeneutic interpretations (16.vi–viii).

For Fodor, then, individual thoughts are inexplicable. It followed (or so he claimed) that computational psychology—"the only scientific psychology we've got"—must be restricted to "modular" phenomena such as syntax and low-level vision. He regarded these as scientifically tractable, being innate competences unaffected by conceptual thought (see Section vi.d–e below, and 16.iv.d).

The implication was that computational psychologists should abandon the more 'sexy' topics: neurotic rationalization, political attitudes, creative analogy, and even rational problem solving. Fodor claimed that "it is becoming rather generally conceded" that AI work on problem solving, "despite the ingenuity and seriousness with which it has often been pursued", had produced "surprisingly little insight". In short, it was "a dead end" (1983: 126). To try to explain thinking is to chase a will-o'-the-wisp.

Many readers interpreted Fodor's 1983 book as the death knell of computational psychology, with respect to the higher mental processes. Those who'd been unsympathetic to cognitive science in the first place were especially easily persuaded. As they saw it, they'd been vindicated. Hermeneutics had won! Indeed, Fodor himself had almost said so:

> [In explaining central cognitive processes], cognitive science hasn't even *started*: we are literally no farther advanced than we were in the darkest days of behaviorism . . . If someone—a Dreyfus, for example—were to ask us why we should even suppose that the digital computer is a plausible mechanism for the simulation of global cognitive processes, the answering silence would be deafening. (1983: 129)

If Fodor, an arch-priest of cognitive science, was declaring much of it to have been a monumental waste of time, why would they want to argue with him?

However, this reaction was justified only if (1) Fodor was right in denying modularity to central processes, and only if (2) one *expects* a psychological explanation to cover the detailed origins of particular thoughts.

The first point drew two rather different replies. These were closely interrelated however, since the anthropologist Daniel Sperber (1942–) was a prime proponent of each of them. On the one hand, evolutionary psychologists would soon posit modules for many types of thought normally regarded as "central" (recognizing cheats, for instance: Cosmides and Tooby 1992, 1994)—and Sperber agreed (2002). (Fodor himself was unpersuaded, as we'll see: Section vi.e below.) On the other hand, Sperber challenged Fodor's claim that any belief can be inferentially linked to any other.

He did this, together with the linguist Deirdre Wilson (1941–) , by formulating a theory of communication and thought in which the philosopher H. Paul Grice's (1967/1975) advice on conversation—*Be cooperative—and in particular,* *be relevant!*—was taken seriously (Sperber and Wilson 1986, 1996). Indeed, "advice"—like Grice's terminology of "conventions", "norms", and "maxims"—was the wrong word. Their approach didn't merely go beyond Grice, but replaced his convention-based analysis with a new "principle of relevance"—*Every act of ostensive communication communicates the presumption of its own optimal relevance*—which was involuntary, exceptionless, and evolved (1986: 155–71).

Sperber and Wilson rejected Fodor's suggestion that laboured scientific inference is a good model for everyday, instantaneous, understanding (1986: 66–7). Similarly, they denied the GOFAI assumption that deliberate reasoning (needed by literary scholars and historians, puzzling over obscure texts) is required for spontaneous interpretation (p. 75). This, they said, isn't done by logical inference, whether deductive or inductive, but by non-demonstrative guessing. However, the guesses are constrained by what we take to be relevant—where *(pace* Fodor) some things are more relevant than others. Without such constraints, effective communication simply couldn't happen. (Similarly, effective problem solving couldn't happen if *just anything* might be useful; in other words, this was a verbal/conceptual version of the notorious frame problem: 10.iii.e.)

Sperber and Wilson defined "relevance" in terms of a cost–benefit analysis, weighing effort against effect. The more information-processing effort it would take to bear x in mind in the context of y, the more costly this would be: and high cost gives low relevance. The more implications (regarding things of interest to the individual concerned) that would follow from considering x, the more effective it would be: and high effectiveness gives high relevance.

They didn't suggest (paradoxically) that we pre-compute just what effort/effect would be involved in considering this concept/belief, or that one. But they did say that there must be psychological mechanisms having much the same result: quasi-modules, evolved for recognizing relevance in speaker's utterances—and in other problem situations too.

For example, our attention is naturally (*sic*) caught by movement, because moving things are often of interest. (Think tigers!) Similarly, even the newborn baby's attention is preferentially caught by human speech sounds (see Section vi.h, below). In general, current sensory input indicates relevance. So (the potentially ambiguous) "Put the blue pyramid on the block in the box" is assumed—*without* conscious inference—to apply to *the one and only blue pyramid already sitting on the block* if perception shows that such a thing exists; otherwise, the pyramid is put *onto* the block (see 9.xi.b).

Besides being built into our sensory systems, relevance recognition is built into our memories. It's no accident, Sperber and Wilson said, that similar and/or frequently co-occurring memories are easily accessible, being 'stored' together in scripts, schemas, and conceptual hierarchies.

Different individuals may adopt different cognitive strategies, which vary in the measure of cost or benefit they attach to a given conceptual 'distance' (see Chapter 4.v.e). Similarly, different rhetorical styles involve different levels of cost and/or different types of information processing in both speaker and hearer.

In 'literal' speech, potential ambiguities usually aren't noticed. Rhetorical styles such as hyperbole, irony, sarcasm, and metaphor are different. Here, utterances are like garden-path sentences (Section ii.b, above) in that the first interpretation is implausible: they must be recognized as non-literal before they can be properly understood.

Again, Grice had said something like this already. But Sperber and Wilson went into much greater depth. They pointed out, for example, that his definition of irony as *saying one thing but meaning the opposite* doesn't capture the "echoic" nature of irony, nor explain "why a speaker who could, by hypothesis, have expressed her intended message directly should decide instead to say the opposite of what she meant" (p. 240). Ironic utterances require second-order interpretation, and assumptions about the speaker's knowledge and intentions, for their actual implications to be worked out (pp. 237–43). In short, decisions are required about what is—what possibly could be—relevant.

With respect to Fodor's pessimism about understanding the higher mental processes, they didn't deny that one could, at a pinch, find a tortured 'relevance' in virtually anything. To that extent, he was right. But they did deny that this would be possible in general: life's too short. Even poets have to provide enough context to make their meaning communicable. And everyday speech, in general, has to be understood *immediately*.

This raises the second point distinguished above: Fodor's assumption that a scientific explanation of thought (if such were possible) would explain every individual belief in detail. That assumption was unreasonable.

Some years before the appearance of Fodor's book, Sloman had argued that scientific explanation *in general* identifies abstract structures generating distinct classes of possibilities (Sloman 1978, chs. 2–3). Correlational "laws" and event predictions are sometimes available, but they're a special case. Physics is thus an example of science, not the paradigm case. Its laws (including the mathematical patterns rediscovered by AI programs: Chapter 13.iv.c) define *structured sets of possibilities*, much as computational psychology does. In physics, the structures are often simple enough, and the initial conditions accessible enough, for us to deduce 'full' explanations (and even predictions) of specific events which interest us. (Hence our ability to predict the time and place of a space vehicle's landing on the moon.) In the psychology of human thought and personality, that's not so.

This is why computational psychologists tended to pay less attention to statistics than their orthodox peers—an attitude guaranteed to bemuse, even shock, many experimental psychologists. If one's question is "What do people, mostly, do?", then statistics (such as Meehl's "cookbook") are essential. But if one's question is "*How is it possible* for someone to do *this* (which may have been done only once)?", then statistics are irrelevant. What's needed instead is a generative theory showing how the episode concerned could arise. That counts as explanation, too.

Statistics are needed, of course, to show that information-processing mechanisms of various kinds are indeed present. In Chapter 8.i.b, for instance, we'll see that the discovery of conceptual "prototypes"—which replaced Bruner's feature-list picture of concepts—involved many careful statistical measures of behaviour. Similarly, psychologists' work on schemas, from Bartlett onwards, had used statistical methods to discover them. By contrast, AI theorists relied mainly on introspection and common sense in positing specific schemas and scripts. That didn't matter if the main problem was *how schemas in general might be structured and processed.* But it mattered more if the question was *which specific schemas people in culture x have in mind* (hence much of the suspicion of AI, on the part of empirically trained psychologists).

Sloman's account of scientific explanation was compatible with Fodor's claim that the human mind is too complex, and our knowledge of the contents of any specific mind too limited, for us to explain every individual belief. What a computational psychology of thinking could do, however, is to show—in general terms—*how a particular thought is possible* (and maybe even unsurprising) in the first place. To identify the underlying processes and structures—from specific schemas to personal architecture—that make individual thoughts possible is, in an important sense, to *explain* them.

(That is, I agree with Sloman here. Some psychologists wouldn't. Bruner, for instance, now explicitly contrasts an "explanatory", generalizing, psychology with an "interpretive" or "narrative" one, which deals with *particularities*—see Bruner 2002, and Shore 2004: 40–1, 157.)

A prime example of such 'possibilistic' explanation was already in the pipeline while Fodor was writing his 1983 book. Karmiloff-Smith's (1986, 1990*a*) theory of "representational redescription" explained how increasingly imaginative, or creative, thinking can develop from limited and uncomprehending skills. It didn't predict *just which* imaginative idea would arise. But it did say what *structural types* of idea could arise, and (up to a point) how. Eventually, Karmiloff-Smith (1992) would use that theory as part of a wide-ranging critique of Fodor's views on modularity (Section vi.h, below).

In sum: despite being right about the explanatory elusiveness of individual thoughts, Fodor's despairing obituary of non-modular psychology went too far. Fortunately, it didn't bring computational studies of thinking to a halt.

7.iv. Reasoning and Rationality

How people reason was a prime topic of cognitive science from the start, thanks to the AI thunderclap caused at Dartmouth by Newell and Simon (6.iv.b and 10.i.b). The electric charge was carried on Simon's early 1940s ideas of bounded rationality and heuristics. Over the next half-century, those ideas would increase in power. In the early years of the new millennium, the philosophers would hold meetings on "dual-process" theories of reasoning (at Fitzwilliam College, Cambridge, for example), and—shockingly—would invite empirical psychologists to speak there. Frege must have been turning in his grave (see Chapter 2.ix.b).

The psychologists' message, in a nutshell, was that rationality-in-practice can't be bound by logic. To be sure, Frege's distinction between logical norms and psychological processes was genuine. But it didn't follow that real thinking should always match the standards of logical rationality. For 'ought' implies 'can'—and, considered as real computational systems, we *can't* think in a purely logical way. There's not world enough, or time.

Newell and Simon themselves, and John R. Anderson (1947–) too, used a new method of programming from about 1970 to model bounded rationality. And in the 1970s–1980s Johnson-Laird argued that non-logical models *in the mind* are used to ground even 'logical' thought.

As for those cognitive psychologists who weren't computer modellers but who stressed people's reliance on heuristics, their research spoke with forked tongue. On the one hand, it confirmed that people typically don't employ so-called 'rational' methods, and often make *mistaken* judgements accordingly: the *bounds* were more prominent than the rationality. On the other, it showed that the unthinking use of very simple heuristics can be astonishingly *effective*: the bounds made rationality possible. The first point was due primarily to Daniel Kahneman and Amos Tversky (and to Peter Wason and Johnson-Laird) in the 1970s, the second to Gerd Gigerenzer in the 1990s. Gigerenzer's specific suggestions were new, and shocking. But as we'll see, his central insight had been expressed by Simon some twenty years earlier.

This work escaped from the laboratory, and excited the general public. The view that our everyday judgements are "irrational" was as threatening to our self-esteem as Freud's stress on unconscious urges. N. Stuart Sutherland's trade book *Irrationality: The Enemy Within* (1992) sold briskly, and the topic was widely featured in the media. Ten years later, the public (their appetite already whetted by Sutherland's book) were fascinated by Gigerenzer's suggestion that we often think *best* when we ignore maths and logic—as our evolutionary ancestors did.

Moreover, a committee in Stockholm pricked up their ears. The only Nobel Prizes yet awarded for Psychology—well, technically for Economics (there's no prize set aside for Psychology)—went to Simon in 1978 and to Kahneman in 2002. (Tversky had succumbed to melanoma in 1996, and Nobels aren't awarded posthumously.) Both were honoured for discovering that the realities of decision making are very different from the idealized 'rational decision theory' of neo-classical economics. Kahneman, for example, had found that people will take a special trip to buy a $15 calculator for $10, but won't do so to save 'the same' $5 on a $125 jacket. In purely economic terms, that made no sense. In other words, cognitive science had shown how very *inhuman* the dominant picture of 'rational man' had been.

A non-psychological coda: in fact, Simon won his Nobel not for humanizing economics but for attempting to do so. To his lasting regret, the Stockholm committee—and people concerned with practical business management (who'd appointed him Professor of Industrial Administration at Carnegie Mellon)—were more appreciative than the 'pure' economists:

> Economists did not flock to the banner of satisficing with its bounded rationality. These ideas still remain well outside the mainstream of economics. (1991: 364)

This wasn't self-regarding paranoia. For it's confirmed by a recent remark from a professional economist:

> It is striking that economists, *after neglecting Simon's ideas for decades*, are now close to [still only "close to"!] accepting them, *but still cite Simon only rarely.* For example, Stigler is widely cited as the economist who brought optimal search theory into economics in the early 1960s, whereas Simon had already brought both optimal and satisficing search into economics in the mid 1950s. (Conlisk 2004: 193; italics added)

The only economists who *have* been influenced by him for many years are the computational economists, or market modellers—including, now, evolutionary economics (see Chapter 15.ix.d and Mirowski 2002: 529–30).

Simon's explanation for this blindness on the part of economists was the need for "backbreaking empirical work", requiring "close, almost microscopic, study of how people actually behave". The economists, he said, had never been interested in that.

In his autobiography, he remembered "armoring myself against the aesthetic lures of neoclassical economics, so responsive to mathematical elegance and so indifferent to data" (1991: 53). He'd tried to persuade mathematical economists to take account of data, aka cognitive psychology (Simon 1978). But they were almost as dismissive of it as 'Fregean' philosophers would be.

a. Simon's ant

If Newell and Simon had moved far away from orthodox behaviourism by the mid-1950s, they hadn't dismissed it as worthless. On the contrary, they claimed to be "natural descendants" of both behaviourists and Gestaltists (Newell *et al.* 1958*a*), and said:

> Today [i.e. 1961] psychology lives in a state of relatively stable tension between the poles of Behaviorism and Gestalt psychology. *All of us have internalized the major lessons of both.* (Newell and Simon 1961: 110; italics added)

The main strengths of behaviourism were its detailed observation and its recognition of environmental influences. They valued both. And the second was epitomized by the most famous insect in cognitive science: Simon's ant.

In his (still) widely read Karl Taylor Compton Lectures on *The Sciences of the Artificial*, given at MIT in 1968, Simon described an ant walking on the ground (1969, ch. 3; cf. Simon 1962). Its behaviour appears highly complex, zigzagging around countless tiny pebbles and lumps of earth. But if the *behaviour* is complex, the mechanisms generating it aren't: "An ant, viewed as a behaving system, is quite simple." The creature can't plan its locomotion ahead of time: it doesn't have the brain. Instead, it responds to environmental cues, encountered from moment to moment. If it meets an obstacle, it just turns away and continues walking.

That is, it's a biological version of Grey Walter's ELSIE (Chapter 4.viii.a). (But much more interesting: recent work on insects has discovered many different navigational mechanisms—see 15.vii.) Moreover, it's potentially open to what A-Lifers would later study as "stigmergy": a 1950s biological term meaning *social integration brought about by individuals responding to environmental signs deposited by other individuals*, as in ant-trails caused by the laying-down of pheromones (see Bonabeau 1999, and 15.x.a). In short, said Simon, the complexity isn't in the ant, but in "the environment in which it finds itself".

So what?—Well (Simon continued), as with ants, so with people: "Human beings, viewed as behaving systems, are quite simple." For instance, someone doing arithmetic is often driven by environmental cues, such as the relative positions of the numbers written on the paper. In brief, the physical (and, for humans, cultural) world constitutes an "external memory" storing crucial information outside the organism. He even suggested that long-term memory itself should be viewed "less as part of the organism than as part of the environment to which it adapts".

Like most of Simon's psychological ideas, this one too was rooted in his economics. It had arisen in the 1940s, when he read the town planner Lewis Mumford's book on medieval cities. Their beauty wasn't planned, but grew "out of [today, many would say 'emerged' from] the interaction of many natural and social forces" (Simon 1991: 98). He'd realized then that market economics needed to allow for the influence of "externalities (for example, noxious odors wafted from the stockyards to surrounding neighborhoods)". In short, internal mechanisms don't suffice to explain the richness of what actually happens.

The phenomenologist Maurice Merleau-Ponty (1945/1962) had made a similar point, at greater length (16.vi–vii). Simon didn't cite him, and perhaps—despite his exceptionally wide reading—wasn't familiar with him. If that's so, he wasn't alone. As we saw in Chapter 2.vi, this branch of philosophy had diverged from the scientific/empiricist tradition long before. It was unusual enough for someone with Simon's intellectual background to have read the Gestaltists carefully; to have devoured Merleau-Ponty too would have been highly eccentric.

For some twenty years, Simon's ant was largely ignored by cognitive scientists, especially in AI. Some 1960s social psychologists had reached a similar position by a different path (Section i.c above), but they weren't much read by cognitivists. And James Gibson (1977, 1979) soon made an essentially similar point. But he was *persona non grata* because of his rejection of "representation" and "computation" (Section v.e–f, below).

In the late 1980s, however, the importance of automatic response to the environment would be rediscovered by cognitive psychologists (especially Gigerenzer, as we'll see), and by the situated roboticists—who mistook it for a revolutionary insight. In A-Life generally, much would be made of the fact that organisms are *situated* in their physical and/or cultural environment (15.vii–viii.a). And in A-Life-oriented philosophy, the long-neglected phenomenologists would become names to conjure with (16.vii).

Why the delay? After all, Simon was a towering presence in cognitive science. If he'd realized the importance of environmental cues in the 1940s, hinted at it in scattered 'asides' in the GPS papers, explicitly highlighted it in 1962 and 1969, and (as we'll see in the next subsection) implemented it in the late 1960s–1970s, why was it so often overlooked?

In a sense, he himself—with Newell—was to blame. First, the excitement caused by their top-down planning programs was so great that bottom-up 'ant insights' were ignored in the very early days of GOFAI. Second, even when they abandoned GPS planning for productions (to which the ant insight was crucial), their main emphasis, still, was on information processing *within* the mind. In short, despite their obeisance to external memory, they didn't prioritize *material embodiment in the physical world*, as the situated robotocists later would—and as the phenomenologists already had.

Nevertheless, Simon's 1940s notion of bounded rationality had always had a situated ant hidden inside it. For when asked what the difference was between *bounded* rationality and *irrationality*, he used the analogy of a pair of scissors: the mind is one blade, the structure of the environment is the other. To understand behaviour, one has to consider both—and, in particular, *how they fit*. Looking at one only (as cognitive psychologists sometimes tend to do) is about as fruitful as cutting paper with a single scissor blade.

b. Productions and SOAR

Simon himself made obeisance to the ant by means of a new form of programming he developed with Newell in the late 1960s: production systems. This approach revolutionized AI, and gave a very different flavour to psychological modelling.

Even while GPS was still being hailed as a breakthrough, they'd realized that it needed 'humanizing' much as neo-classical economics had done. GPS possessed full knowledge of strictly limited, and static, domains. But life's not like that. A theory of problem solving should explain how we manage to be rational in a largely uncertain, and continually changing, world. That, said Newell and Simon, was what production systems could do.

These programs were sets of IF–THEN rules, or "productions". Each rule specified (one or more) actions to be taken in response to (one or more) particular cues, or conditions. Some of the cues and actions arose from, or modified, the external environment. But most specified information processing within the system. In other words, these were 'ant programs'—with most of the action being triggered *inside* the ant.

The internal actions included the setting-up of a new goal, or sub-goal (implemented as a new condition); and one type of cue was a reminder of the current goal. If goal A was current, in *these circumstances* (also specified in the IF-side of the rule), then the system would perform *those actions*; if goal B was current, it would do something else. So hierarchical goal seeking was included in production systems, not by top-down planning as in GPS but by bottom-up procedures designed to work in an untidy world. Since a given condition might crop up at any time, behaviour could be interrupted by, and/or instantaneously adapted to, unpredicted events (including thoughts like *Oh, that reminds me . . .*).

They were intended to be biologically plausible in another way, too. Simon's paper on 'The Architecture of Complexity' (1962) had argued that, in evolution, individual mutations must give rise to *distinct* sub-procedures. Accordingly, each production was logically independent—so a new one could be added at any time. (Damaging interactions with pre-existing rules had to be avoided by foresight and/or debugging, since evolutionary programs weren't yet available: see 15.vi.)

Production systems (as described so far) were a universal programming language: see Chapter 10.v.e. It's no wonder, then, that AI technologists used them for many purposes. The first volume of the journal *Artificial Intelligence* described a program that *learnt* to play poker, written by one of Newell and Simon's students (Waterman 1970). The first 'practical' applications were DENDRAL and MYCIN (see 10.iv.c), and the HEARSAY speech system—which was part-planned by Newell himself (Newell *et al.* 1973; Reddy *et al.* 1973; Reddy and Newell 1974). Others soon followed, including the "expert systems" that galvanized AI funding around the world in the mid-1980s (11.v).

But Newell and Simon weren't primarily interested in technology. Their 900-page *Human Problem Solving* (1972), which introduced production systems to psychologists, was an exercise in *psychology*. Because of this, they deliberately constrained the power of their production systems, to match the bounded rationality of human beings.

For example, they took Miller's "magical number seven" seriously (Chapter 6.i.b). So the eight-point summary of their theory included these two claims:

(1) [The human information-processing system] is a *serial* system [i.e. only one rule fired at a time] consisting of an active processor, input (sensory) and output (motor) systems, an internal LTM and STM [long-term and short-term memory] and an EM [the "external memory", or perceptible field].

(3) *Its STM holds about five to seven symbols, but only about two can be retained for one task while another unrelated task is performed.* All the symbols in STM are available to the processes

(i.e., there is no accessing or search of STM) [in effect, it's a blackboard: see Chapter 10.v.e]. (1972: 808; italics added)

These were no mere metaphors, but literal hypotheses:

(7) *Its program is structured as a production system*, the conditions for evocation of a production being the presence of appropriate symbols in the STM augmented by the foveal EM. (p. 809; italics added)

In short, Newell and Simon were trying to describe the mind's *general architecture*, not merely how it functions in specific situations.

Several of the eight theory points referred to the *time* needed for the mind/brain to process information. "Elementary processes", it was said, take about fifty milliseconds, while "writing [into LTM] a new symbol structure that contains K familiar symbols takes about 5K to 10K seconds", and "accessing and reading a symbol out of LTM takes a few hundred milliseconds". Out of context, that might suggest that theirs was a *neurological* theory. But it wasn't. The symbols were presumably implemented in the brain by Hebbian cell assemblies, but Newell and Simon weren't concerned with that. They defined *symbols* in an abstract, computational, way (see Chapter 16.ix.b). As they put it, their information-processing theories represented "a specific layer of explanation lying between the behavior, on the one side, and neurology on the other" (p. 876).

Counting milliseconds wasn't the only evidence of attention to detail. The book reported levels of program/protocol matching, in domains such as chess and crypt-arithmetic, that far surpassed any previous computer models (for a fuller discussion, see Boden 1988: 154–70). As a result, their new methodology was adopted by many computational psychologists—and not just for problem solving. It was soon used to model child development (Richard M. Young 1974, 1976), and later applied to detailed motor skills such as typing (Card *et al.* 1983).

Their own production systems were both precise and economical. For example, consider this cryptarithmetic problem:

$$\begin{array}{rl} \text{DONALD} & (D = 5) \\ +\text{GERALD} & \\ \hline \text{ROBERT} & \end{array}$$

As you'll discover if you try to solve it yourself, this isn't a straightforward exercise. Nevertheless, Newell and Simon explained their subject's behaviour by *only fourteen* IF–THEN rules (p. 192).

This was logician-as-ant: "Human beings, viewed as behaving systems, are quite simple." But the emergent complexities of human behaviour can be considerable. So some of the fourteen rules dealt with goal–sub-goal organization. Some directed the attention (e.g to a specific letter, or column, in the sum shown above). Some recalled previous steps in the process, so took account of intermediate results. Some handled the generation—and recognition—of false starts. And some enabled the backtracking needed to recover from them. In short, purposive behaviour was being modelled—but not by top-down planning.

Just as their earlier programs had been designed only after careful "task analysis"—their term for the identification of what needs to be done if the problem is to be solved—so were these (1972, ch. 3). The logical constraints of cryptarithmetic and chess were carefully thought out beforehand. Moreover, their eight-point architectural theory provided the general context for every individual program. So they didn't fall foul of Drew McDermott's (1976) complaint that early AI workers were often playing around, rather than theorizing in a systematic way (see Chapter 11.iii.a).

They did, however, eventually fall foul of Marr, for whom "computational" explanation referred to unconscious, automatic, modular processing (Section iii.b, above). Cryptarithmetic is far from automatic, as anyone who's torn their hair out over "SEND + MORE = MONEY" can testify. Marr had no quarrel with Newell and Simon's emphasis on task analysis, nor even with their analyses of their chosen tasks. But he rejected their choice of tasks to be analysed. As he put it: "I have no doubt that when we do mental arithmetic we are doing something well, but it is not arithmetic" (1982: 348). And he added that "we are very far from understanding even one component of what that something is". In other words, he saw their apparently human-like programs as superficial gimmickry, not explanation.

A more common critique was that Newell and Simon were behaviourists in disguise, since individual productions are essentially similar to S–R connections. As self-confessed "descendants" of both behaviourist and Gestalt psychologists, they accepted the parallel. But they pointed out that, unlike the behaviourists (even including Hull: 5.iii.b), most of their stimulus–response—i.e. condition–action—pairs concerned *internal* processes, described in *information-processing* terms.

Newell and Simon's remarks about the brain met with resistance, too. If they'd followed the doctrine of multiple realizability strictly, they'd have been immune to criticism on this count. As it was, their brain theorizing led some psychologists to ignore their approach. So Walter Weimer, for instance, said this:

> Everybody in psychology knows [Herb Simon] is great, but nobody's ever read him. I never have taken him seriously, and I can tell you why. When you have a man who sits there and looks you straight in the eye and says the brain is basically a very simple organ and we know all about it already, I no longer take him seriously. And Simon tells you that constantly in his books and lectures. . . . [He's] only kidding himself when he tells us that we already know how the head works. That is not cognitive science, but abject cognitive scientism. . . . [He] does not do cognitive psychology. He does something distantly related to it, but he doesn't do it. (interview in Baars 1986: 307–8)

In 1980 Newell—with John Laird (1954–) and Paul Rosenbloom (1954–)—started work on a new type of production system, to model the architecture of cognition *as a whole.* SOAR—the acronym stood for Success Oriented Achievement Realized—was implemented in 1983 (Laird *et al.* 1987). Reasoning in SOAR was a multidimensional matter, for the system integrated perception, attention, memory, association/inference, analogy, and learning.

SOAR differed in many ways from the 1972 production systems, which had modelled specific tasks (such as cryptarithmetic) rather than *general* intelligence. Its increased flexibility was due to several factors.

For example, different types of problem (some 'closed', others more open-ended) could all be handled within the same framework for defining problem spaces. Conflict resolution, needed when *several* rules have matching conditions, was handled differently—so that all the unfired (but potentially excited) rules remained visible to the system as a whole, instead of being repressed. Ant-like responses, or 'situated' behaviour, were combined with internal deliberation (in that sense, SOAR was a 'hybrid' system). Indeed, deliberation was often turned into reflex responses, so that a problem initially solved by the former was later dealt with by the latter. This involved "chunking", whereby sub-goal settings that had frequently been executed in sequence were rolled into one rule, which improved efficiency (and respected Miller's magical number seven).

Nevertheless, SOAR built on the insights pioneered in GPS and the 1972 programs. It treated problem solving as goal-directed movement through a problem space, and defined actions and operators suitable for various (specified) situations. In addition, it handled both procedural and declarative knowledge (as ACT had done earlier: see subsection c, below).

SOAR attracted huge attention across cognitive science. A variety of comments, including many by 'big names', plus a lengthy reply were published in *BBS* (Newell 1992), and another batch of comments/reply in *Artificial Intelligence* (Stefik and Smoliar 1993). The interest concerned not only its technicalities but also its general philosophy, elaborated in Newell's William James Lectures, given at Harvard in 1987 (Newell 1990, chs. 1–3, 8). (Because of the notice it received, he soon wrote several summaries: Newell 1992; Newell *et al.* 1993.) It consisted of two main claims.

One was already familiar, since the early days of GOFAI: a highly abstract account of mind-as-machine known as the Physical Symbol System (PSS) hypothesis (16.ix.b). This held that psychology concerns the generation and transformation of symbolic expressions, conceptualized at "the knowledge level". Like the IF–THEN rules for "DONALD + GERALD = ROBERT", those expressions could be (and sometimes are) verbalized by the subject. A paper on the SOAR updating of PSS was included in a 1990s collection on the foundations of AI (Rosenbloom *et al.* 1992). But by that time, the PSS debate—though still 'live'—was old news.

The second claim was more novel, and more interesting to model-builders. It was a methodological directive, underlain by the assumption that there are psychological mechanisms underlying *intelligence in general.* Instead of "microtheories" dealing only with specific tasks, Newell now called for "unified theories". These, he said, try to capture the general principles underlying all cognition: not just reasoning, but memory, language, perception, and attention too. In other words, the *architectural* aspect, which had been in the background of his 1970s research with Simon, was now brought to the forefront.

Simon himself didn't approve, as we'll see. Arbib, too, had reservations: "no single, central, logical representation of the world need link perception and action—the representation of the world is *the pattern of relationships between all its partial representations*" (1994: 29).

But many people were persuaded, and SOAR has been in continuous development ever since. Among the philosophers who thought well of it were Dennett (1993*a*) and, in particular, Richard Samuels (forthcoming). Today, it's widely used for both technology and psychology. A recent account runs to no fewer than 1,438 pages (Rosenbloom *et al.*

1993), and Soar Technology Inc. is one of the commercial offshoots. The practical tasks being handled run from medical diagnosis to factory scheduling.

Charles Babbage, given his own pioneering work on factory management, would have been impressed (3.i.a). But Simon wasn't. SOAR was Newell's baby, not his. They were still colleagues at Pittsburgh, but their thirty-year collaboration had ended. The 1960s and 1970s had seen a host of joint publications, but the 1980s saw only two: both historical commentaries on their earlier work (Simon and Newell 1986; Newell and Simon 1987). Although both men were still doing psychological AI, their interests had diverged.

Simon was dubious about the whole SOAR exercise. In his autobiography, he wrote:

> *In cognitive science there is currently [i.e. in 1990] a preoccupation with questions of general architecture, which I do not share.* There are great debates about whether the human mind is to be modelled by SOAR (Allen Newell), Act* (John Anderson) [see subsection c, below], connectionist nets (Jay McClelland) [see Chapter 12], or something else. I have been more interested in . . . "theories of the middle range"—programs such as GPS, EPAM, . . . and BACON, which simulate human behavior over a significant range of tasks but do not pretend to model the whole mind and its control structure.
>
> *It is not that I regard the broader architectural issues as unimportant; but, even when solved, they do not explain how very general schemes are adapted to perform particular classes of cognitive tasks. The architectures have almost more the flavor of programming languages than of programs.* (1991: 328; italics added)

While SOAR was occupying his one-time collaborator, Simon was investigating learning, creativity (13.iv.c), and mathematical education. He saw the last as especially important—indeed, it had been "a major hidden objective" in his research on learning. Worried about the prospects for democracy in high-tech societies full of "alienated" (non-scientific) intellectuals, he wanted to know how to overcome the common resistance to mathematics: "There is no question I would more like to answer than this one before my research career ends" (1991: 330).

The implication that he might outlive his research career wasn't fulfilled. He died in 2001, still at the height of his intellectual powers. (Newell had predeceased him, in 1992. For a discussion of Newell's AI obituary, see Chapter 10.v.b.)

c. The ACTs of Anderson

If Newell and Simon's 1972 theory was the first attempt to model the computational architecture of cognition as a whole, John R. Anderson's ACT (1976) was a close second. Indeed, early-ACT was even more clearly guided by this aim, which for Newell became paramount only with the birth of SOAR.

John R. Anderson (1947–) was a Canadian psychologist educated at Stanford, and employed at Michigan, Yale, and (from 1978) Carnegie Mellon. (He shouldn't be confused with the Brown/UCLA neurophysiologist James A. Anderson, who also worked on associative memory: Chapter 12.v.b and e.) His early work on human associative memory (acronym: HAM) included computer models co-published with Gordon Bower, whose prime research interest was language understanding (Anderson and Bower 1972, 1973; cf. G. H. Bower 1970, 1978).

The HAM theory (and mini-models) was based on a propositional network representing the structure of memory and linguistic meanings (Anderson 1976: 39–47). That is, it was more concerned with the *contents* of memory than with its *function*. Accordingly, it was soon superseded by ACT, a model (and an acronym) based on the adaptive control of thought. (Some of the differences between HAM and ACT were described in Anderson 1976: 270–90.) After an unproductive flirtation with ATNs, Anderson picked up Newell and Simon's new technique of production systems to focus on the *interactions* between memory and other cognitive processes.

Since a system as complex as ACT couldn't be built overnight, there were in fact several versions, named (when people could be bothered) with successive letters of the alphabet. The "ACT" described at great length in Anderson's 1976 book was in fact ACTE (Anderson 1983: 17). Soon afterwards, ACTE spawned ACTF, which modelled the *acquisition* of productions (Anderson *et al.* 1977/1980). So meaning, memory, problem solving, and learning were all grist to ACT's mill even in the early 1970s. The theory—and its implementation—was continuously developed over the years. The first comprehensive account appeared in 1976, and the learning version (ACT*) was described some six years later (Anderson 1982, 1983).

In 1980 Anderson published a textbook on cognitive psychology (now in its fifth edition) in which ACT as such was downplayed—but AI in general, and production systems in particular, were prominent. Moreover, the guiding aims underlying ACT were evident throughout the text. In the opening chapter, for example, he declared:

> Certain subdomains within the field—for example, perception, memory, problem solving, and language—are becoming well understood. Still, the form of a theory that would specify *how all the subfields in cognitive psychology interconnect* is still very unclear. Later chapters will present some of the theories that have been proposed to explain how these subareas are connected . . . (1980: 17–18; italics added)

And in Part II on 'The Representation of Knowledge', which began with an account of the striking early 1970s experiments on mental imagery, he supported "dual-code" theories, according to which visual images and verbal memories are stored in different ways (see Section v.a, below). However, he argued that (as in ACT*) *long-term* memories in either case are stored in some abstract "propositional" code that's neither visual nor verbal.

In comparison with other forms of computational psychology in the 1970s, ACT was unusual in three ways. First, it employed both declarative and procedural representations, of domain knowledge and practical skill respectively (see 10.iv.a). These resembled Gilbert Ryle's "knowledge that" and "knowledge how" (16.i.c). But Anderson's focus was on the fact that a person may know (for instance) that a certain Euclidean theorem is true, without being able to use it in a geometrical proof. In acquiring the skill of doing so, the learner gradually constructs a set—maybe, thousands—of production rules by trial-and-error application of the declarative proposition in many different circumstances.

Second, ACT combined GOFAI insights with (localist) connectionism, in the form of a network allowing spread-of-activation through its links. When the system was instigated, most people thought of connectionism as being very different from, or even opposed to, GOFAI (see 4.ix and 12.ii–iii). But there had been an earlier 'half-way-house': Quillian's (1968) semantic networks (9.xi.e and 10.iii.a). ACT could be seen as "a special case of the Quillian model—a special case sufficiently well specified to make predictions and be proven false" (Anderson 1976: 291).

Third, it was an *architectural* theory. That is, it was "a theory of the basic principles of operation built into the cognitive system" (1983, p. ix). Newell and Simon's 1970s work was informed by architectural assumptions too, as we've seen, but it didn't stress mental integration as heavily as Anderson did. It's no accident that his first book (1976) was called *Language, Memory, and Thought*.

So Anderson swam against the tide when, in the early 1980s, the mind was commonly viewed as a number of non-interacting modules (Section vi.d–e, below). Resisting this intellectual fashion, he directly contradicted a 'modular' quotation from Chomsky (1980*a*) on his opening page. He still stressed mental *integration*: "Memory, language, problem solving, imagery, deduction, and induction are different manifestations of the same underlying system" (1983: 1). Modularity, he said, applied only to "peripheral" processing. Whereas Fodor (1983) had recently ruled conceptual thinking out of court for a scientific psychology, Anderson's work was unabashedly aimed at the *higher* mental processes:

> *A major presupposition ... is that higher-level cognition constitutes a unitary human system. A central issue in higher-level cognition* is control—what gives thought its direction, and what controls the transition from thought to thought. Production systems are directed at this central issue . . .
>
> It needs to be emphasized that production systems address the issue of control of cognition in a precise way that is relatively unusual in cognitive psychology. Other types of theoretical analyses may produce precise models of specific tasks, but *how the system sets itself to do a particular task in a particular way* is left to intuition. In a production system the choice of what to do next is made in the choice of what production to execute next. Central to this choice are the conflict resolution strategies . . . Thus production systems *have finally succeeded in banishing the homunculus from psychology*. (1983, pp. ix–x; italics added)

(Here, both Fodor and Anderson could have been right. One may be able to discover the general principles underlying conceptual thought without being able to explain particular instances of it: see Section iii.d, above.)

Someone may have to construct "thousands" of production rules in acquiring a skill because the relevant declarative knowledge (e.g. a Euclidean theorem) can be used in many different ways. According to Anderson, the person has to learn what task goals (and sub-goals, and sub-sub-goals . . .) are relevant in which task circumstances, and what results a particular action will give in the various circumstances. This requires both teleological sensitivity and immediate feedback during problem solving. It also requires time:

> The acquisition of productions is unlike the acquisition of facts or cognitive units in the declarative component. It is not possible to simply add a production in the way it is possible to simply encode a cognitive unit. Rather, *procedural learning occurs only in executing a skill; one learns by doing*. This is one of the reasons why *procedural learning is a much more gradual process than declarative learning*. (1983: 215; italics added)

One shouldn't assume that Anderson's production rules were simple. Some were. But others, built up only after long practice, were not. An example of a production used in reading the word "EACH" contained no fewer than eighteen IF-conditions (some of which were themselves conjunctions), and five THEN-results (1983: 144).

According to Anderson, this reflects an important difference between how beginners and experts achieve a task (recognize a pattern, understand a sentence, solve a problem, manipulate a tool . . .). Experimental data, including some from Simon's research on scientific reasoning, suggest that where beginners use several steps, the expert uses only one. Simon, and Newell too, explained this in terms of declarative chunking, as we've seen. Anderson preferred a form of 'procedural chunking'.

As he put it, borrowing AI jargon, the expert has "compiled" knowledge of the task whereas the novice has "interpreted" knowledge (1983: 216, 255). So in ACT*, the 'composition learning operator' would convert a set of individual production rules that had often been carried out sequentially into a *single* production, that could be executed *without* having to access the declarative knowledge retrieved and interpreted by the beginner (1983: 235–41).

This throws light on an old puzzle in philosophy. The fourth "rule of method" identified by Descartes (1637), i.e. *recapitulation*, seems at first sight far too boring to constitute 25 per cent of a revolutionary 'Method of Rightly Conducting the Reason and Seeking for Truth in the Sciences'. But Descartes wasn't concerned here only with someone's remembering what they'd already read. He was interested also in their *coming to understand it better*. He said that someone who read his argument, or a complex geometrical proof, over and over again would eventually come to see the relation between the initial premisses and the final conclusion directly, or intuitively. Before that point, they'd have to rely on remembering that they'd arrived at the conclusion by going through many previous steps—and memory, he pointed out, is fallible. Introspectively, this seems right; and it fits the phenomenology of other skills besides reasoning, such as improvising on the piano (Sudnow 1978/2001). Anderson's theory suggests a way in which this mental progression could happen.

Anderson was eager to roam beyond the ivory tower. By the late 1980s, he was working on educational applications of his ideas about how people acquire new skills. So he designed 'intelligent tutors' that gave instruction in LISP, geometry, and algebra. These were used in the Pittsburgh Public School System, and elsewhere.

On the basis of this experience, he developed the ACT-R version and changed his approach to automated tutoring (Anderson 1993). Instead of trying to write programs that *emulate* the student, he now aimed at providing helpful learning environments, offering domain knowledge and feedback. The "general principles" he followed in designing computer tutors included communicating the goal–sub-goal structure that underlies learning, minimizing the load on working memory, providing immediate feedback, and adjusting the grain size of instructions as learning progressed (Anderson *et al.* 1987, 1990, 1992). The last of these reflected his theory of how novices differ from more advanced learners (see above).

ACT is still being improved, giving us ACT-R, ACT-RP, ACT-RPM . . . (Anderson 1993, 1995). Like Stephen Grossberg's ART family (14.vi.c–d), however, it risks becoming too complex to be readily intelligible. Moreover, not all computational psychologists are convinced that the underlying philosophy is sound. Newell's collaborators, for instance, regard SOAR as a superior hybrid architecture (they compared the two systems in Newell *et al.* 1989). And Simon, as we've seen, remained sceptical about *all* attempts to model the general principles of the mind.

d. Models in the mind

Where Newell and Simon, and Anderson too, had prioritized models *of* reasoning, Johnson-Laird—in the 1980s—focused rather on models *in* reasoning. Granted, he supported (and developed) his theories by doing computer simulation—and chided those colleagues who didn't: "Sadly, many experimental psychologists make no use of computer modelling" (p. xii). But his core hypothesis was self-confessedly Craikian (Chapter 4.vi): that "human beings understand the world by constructing working models of it in their minds" (Johnson-Laird 1983: 10 ff.).

Starting with seven talks given at Stanford early in 1980, followed by a 500-page book in 1983, he elaborated this idea as a novel computational theory of thought. It was a development of the procedural semantics that he and Miller had initiated at the end of the 1960s (Section ii.d, above). But it was even more closely integrated with work in the *philosophy* of meaning, as we'll see.

He'd already done some widely cited experiments on reasoning in the early 1970s, with his adviser Wason (1924–2003) of University College London (Johnson-Laird and Wason 1970; Wason and Johnson-Laird 1972; Johnson-Laird 1975). Using the "card selection" task originated by Wason some years earlier (1966), they'd shown that problems of identical logical form are much easier to solve when expressed as 'real' examples (e.g. involving the postage required for letters of different kinds) rather than as abstract *p*, *q*, and *r*. Indeed, this was true even of professors of formal logic.

The usual response had been to bemoan the "irrationality" of mere mortals, and pass on. For Johnson-Laird, however, the failures of the professors of logic sounded a warning bell. Apparently, something powerful was involved here, which enabled every Tom, Dick, or Harry to succeed in realistic cases but could desert even professional logicians when abstract examples were in play. He spent the next decade trying to discover what this "something" might be. And his answer was: mental models. (Later, two evolutionary psychologists would suggest a different "something", namely, a mechanism evolved to detect cheaters: see Section vi.d, below.)

His version of bounded rationality acknowledged the successes as well as the failures of human thought. But it didn't explain them in terms that Fregeans would respect. For Johnson-Laird, even logical reasoning doesn't depend on formal logic chopping, but on quasi-perceptual representations within the mind. As he put it:

> Mental models owe their origin to the evolution of perceptual ability in organisms with nervous systems. [David Marr has] outlined a computational theory of vision that largely accounts for the derivation of perceptually based models of the world [see Section v.b–d, below] . . .
>
> Mental models can take other forms and serve other purposes, and, in particular, they can be used in interpreting language and in making inferences. These roles are a natural extension of their perceptual function . . . Discourse, however, may be about fictitious or imaginary worlds, and hence our propensity to interpret it by building models of the states of affairs it describes frees us from the fetters of perceptual reality. (1983: 406–7)

Consider syllogisms, for instance. Johnson-Laird dismissed all previous psychological theories as—in Chomsky's terms (Section iii.a, above)—inadequate. They failed to explain why some syllogisms are intuitively easy and others hard, and couldn't be generalized to the development or teaching of syllogistic reasoning—nor to other types of inference (1983: 65–6).

Here's an example of an easy syllogism:

Some of the artists are beekeepers.

All of the beekeepers are chemists.

Nearly everyone (correctly) infers that some of the artists are chemists—and some people also (correctly) infer that some of the chemists are artists. "Nearly" everyone—but not quite. It's significant, said Johnson-Laird, that "the only person whom I have ever known to get the answer wrong is a distinguished philosopher who tried to exploit his logical expertise!"

I've seen the same thing happen, when playing around with Lewis Carroll's Sorites. Everyone in the room, including my 16-year-old daughter, got it right—except the friend with several logic publications under his belt. (A Sorites puzzle provides several premisses, and you must discover the one and only conclusion which follows from all of them taken together. In Carroll's hands there might be as few as three or as many as fifty premisses (Carroll 1977: 386–9), ranging from *No shrimp is remarkable for sagacity* (p. 407), through *No discontented judges are chickens* (p. 420), to *Any good-tempered man, who has lent me money and does not care for appearances, is willing to shake hands with me when I am in rags* (p. 387). Not all his premisses were as credible as these, so commonsense guessing couldn't help in dealing with them: e.g. *Brothers of the same height always differ in Politics* (p. 421) or *All spiders are healthy, except the green ones* (p. 406).)

By contrast, hardly anyone can deal with this:

All of the bankers are athletes.

None of the councillors are bankers.

(In case you're wondering, the only valid conclusion is *Some of the athletes are not councillors*.) "Few people", reported Johnson-Laird, "are able to cope correctly with [these] premises" (p. 67).

But why? And why is the "easy" syllogism more likely to yield *Some of the artists are chemists* than the equally valid *Some of the chemists are artists*? Indeed, why do these differences (and many others discovered by Johnson-Laird) persist when the argument is expressed informally in everyday conversation, instead of 'artificially' as a syllogism?

Johnson-Laird's view, in brief, was that the verbal formulation of the problem—*any* problem—prompts us to build an internal analogue of the state of affairs portrayed in it. This model is constructed in *understanding* the incoming sentence/s. Once built, it's then available for use in making inferences.

An initial model may be more or less easy to change, on receipt of further information of various kinds. And it may yield the answer to a specific 'question' (the search for a specific inference) more or less readily. Sometimes, mere (mental) inspection will suffice; at other times, further modification of the model will be required. This explains the ease/difficulty of problems of different forms, or of the same problem expressed in different ways.

For example, suppose someone is told the following:

A is on the right of B.

C is in front of B.

D is on the left of C.

Johnson-Laird claimed that they would construct an internal spatial representation, in which—in true Craikian style—the items standing for A, B, and C would bear relations to each other analogous to those between the real things (as described by the three sentences). With this representation in place, the person could then use it, for instance, to infer that A *is not* on the left of D.

His claim was perhaps false. But it wasn't hand-waving. For in this case, and many others, he provided a computer program detailing computations capable of constructing the model and of drawing inferences from it. His effective procedure for syllogistic inference (pp. 97–110), for example, had three main parts:

1. Construct a mental model of the first premise.
   [For instance,] the representation of a universal affirmative assertion has the following structure:

All of the X are Y:	x = y
	x = y
	(y)
	(y)

   where the number of tokens corresponding to x's and y's is arbitrary, and the items in parentheses represent the possible existence of y's that are not x's. [Comparable models were given for *Some of the X are Y*, *None of the X are Y*, and *Some of the X are not Y*.] (1983: 97–8)
2. Add the information in the second premise to the mental model of the first premise, taking into account the different ways in which this can be done. (p. 98)
3. Frame a conclusion to express the relation, if any, between the 'end' terms that holds in all the models of the premises. (p. 101)

The logical modalities were defined/explained accordingly. A conclusion was *possible* if it was true in at least one model, *probable* if true in most models, *necessary* if true in all models, and *impossible* if true in none.

Devotees of 'pure' rationality would baulk at this. For instead of a formal deductive proof, step 3 involves an *inspection* of all the models that happen to have been built. If the program (or person) had missed some out during construction (steps 1 and 2), an invalid conclusion could result. To be sure, Johnson-Laird's programs included procedures designed to avoid this (for instance, the inclusion of y's *in parentheses*, above). In the general case, however, complete modelling wasn't—and, of course, isn't—guaranteed.

In addition, the (various) "figural" effects on ease/difficulty of solution were modelled by computational rules based on the assumption that "working memory operates on a 'first in, first out' basis" (p. 105). It follows, for instance, that the "natural" order in which to state a conclusion is the order in which the terms were used to construct a mental model of the premisses. This explains the different probabilities of the two correct conclusions in the artists/beekeepers/chemists example.

(For the record, Johnson-Laird's views haven't changed radically since then. He says now that "the biggest development in my theory of reasoning since the '83 book is probably the discovery that people only represent what is true": personal communication, 2003. After detailing various common errors, including some which virtually *no one* can resist, he concludes: "Such illusions occur in many, many domains of reasoning. Their moral is that we normally think only about what is true, and not what is false.")

e. The marriage of Craik and Montague

Johnson-Laird's computer programs for building and manipulating mental models helped support his theory. But as he pointed out, full explanatory adequacy would require a principled account of *all possible* mental models. That he couldn't provide. However, he did offer an incomplete "typology" of models: six "physical" (including images) and four "conceptual". Moreover, he listed ten general constraints which all such models must satisfy (1983, ch. 15).

These constraints were inspired partly by considerations of computational feasibility (i.e. bounded rationality), and partly by Montague's model-theoretic semantics. This was one of the few semantic theories which Johnson-Laird *hadn't* discussed in his 1976 book—even though Montague had written (and died) some years earlier (Chapter 9.ix.c). Now, he made up for lost time.

Montague's philosophy, he said, laid out the structure of semantic interpretations "with a pure but almost unreal clarity" (p. 167). It specified "what is computed in understanding a sentence", whereas psychological semantics should specify "how it is computed". As he remarked, this was a version of the logic/psychology distinction formulated by Frege (2.ix.b).

The ten general principles of Johnson-Laird's theory were these:

1. The principle of computability: Mental models, and the machinery for constructing and interpreting them, must be computable (p. 398).
2. The principle of finitism: A mental model must be finite in size and cannot directly represent an infinite domain (p. 398).
3. The principle of constructivism: A mental model is constructed from tokens arranged in a particular structure to represent a state of affairs (p. 398). [It is "a *Craikian* automaton" (p. 403).]
4. The principle of economy in models: A description of a single state of affairs is represented by a single mental model even if the description is incomplete or indeterminate (p. 408).
5. Mental models can directly represent indeterminacies if and only if their use is not computationally intractable, i.e. there is not an exponential growth in complexity (p. 409).
6. The predicability principle: One predicate can apply to all the terms to which another applies, but they cannot have intersecting ranges of application (p. 411).
7. The innateness principle: All conceptual primitives are innate (p. 411). [He explicitly rejected Fodor's claim that all *concepts* are innate: see 16.iv.c, below.]
8. There is a finite set of conceptual primitives that give rise to a corresponding set of semantic fields, and there is a further finite set of concepts, or 'semantic operators', that occur in every semantic field serving to build up more complex concepts out of the underlying primitives (p. 413).
9. The principle of structural identity: The structures of mental models are identical to the structures of the states of affairs, whether perceived or conceived, that the models represent (p. 419).
10. The principle of set formation: If a set is to be formed from *sets*, then the members of those sets must first be specified (p. 429).

In illustrating what he meant by these ten constraints, he showed how mental models (in his sense) differed from other forms of internal representation that had been suggested by computational psychologists—such as schemata, prototypes (Chapter 8.i.b), images, and propositions.

Ambitiously wide-ranging as it was, Johnson-Laird's new book attracted considerable interest—and no little disagreement. One critic, a long-time researcher on syllogistic reasoning, complained of Johnson-Laird's 'Mental Muddles' (Rips 1986). But his erstwhile collaborator Miller, assessing the theory in the *London Review of Books*, described it as 'A Model Science' (G. A. Miller 1983*b*). While much remained to be done, he said, Johnson-Laird was on the right track.

(He stayed on that track. Recently, he's used mental-model theory to describe real-life choices, like those which had interested Simon as an economist. In particular, he's stressed the dynamic interplay between reasoning, judgement, and decision making: Johnson-Laird and Shafir 1994.)

Philosophers were interested too, not least because Johnson-Laird had trespassed on their territory. He used Montagovian semantics, for instance, in offering a *psychological* explanation of our grasp of "reference" and "truth". And in so doing, he made various assumptions about ontology, a classic problem of metaphysics (see constraint no. 9)—and of GOFAI (10.iii.e and 13.i).

The prime bone of contention was whether how we *actually* reason should be of any concern to philosophers. This bone had been nibbled already, in response to Johnson-Laird's (and Wason's) earlier work. For example, at a meeting held in 1978 the philosopher of science Henry Kyburg (1980) had declared:

> I lost track of the number of times the maxim "Ought implies can" was solemnly enunciated [in two papers on inductive reasoning written by Winograd and Boden]. Whatever its virtues in ethics, the maxim is false for logic. One ought to be consistent; one ought not to offer invalid deductive arguments. No one lives up to these norms; but they are valuable precisely because they can be approached; formal logic gives us standards by which we can measure our approach to them. (Kyburg 1980: 376)

The task of "inductive logic", he continued, is to develop comparable standards for inductive arguments: formal measures that tell us when someone is leaping to conclusions, or resisting overwhelming evidence. He granted that AI modelling might help. To that extent, he welcomed communication across the disciplinary boundary. But it was AI, not psychology, that was to be the partner discipline: "an inductive program should embody standards and norms reflecting the way people *ought* to argue inductively" (italics added).

That complaint was made two years before Johnson-Laird's Stanford lectures. Unknown to Kyburg, he was already planning to use the ten constraints to ground not only deductive reasoning (artists and beekeepers) but also the inductive standards (*sic*) that Kyburg had been asking for.

Whereas philosophers found the abstract constraints intriguing (whether satisfactory or not), most psychologists ignored them. They focused rather on the study of the *particular* mental models described by Johnson-Laird. One reason for this was the fearsome difficulty of Montague's work, which had to be 'translated' even for *linguists*, who were well accustomed to abstract formalisms and argument about semantics (see 9.ix.c). (That's probably why Johnson-Laird hadn't discussed it earlier: the first 'translation' appeared when *Language and Perception* was already in press: Partee 1975.) Psychologists were thankful to escape into their laboratory.

Many even forgot Johnson-Laird's insistence that mental models are Craikian, never mind Montagovian. So 'unprincipled' talk of *schemas*, for example, continued to flourish. In short, the search for Chomskyan explanatory adequacy was taken more seriously by Johnson-Laird than by his experimental followers.

f. Irrationality rules—or does it?

Where Johnson-Laird looked to Montague and even to ontology (cf. constraint no. 9, above), others used a more traditional approach: experimentation. The Israeli psychologists Kahneman (1934–) and Tversky (1937–96) showed in the 1970s that intuitive judgements aren't ruled by the standards of logic or probability theory.

Both one-time (mid-1960s) postdocs at Bruner's Center for Cognitive Studies, Kahneman and Tversky—K&T for short—started off from *A Study of Thinking* (Shore in 2004: 133). They'd accepted Bruner's view that perception and thought are guided by expectations, and by various heuristic strategies for working with them (see 6.ii.b–c). They wanted to discover the constant ('base rate') error that people make because of their expectations about what things are most likely. But if their approach was traditional, their findings weren't. Indeed, the editor of the *Psychological Review*, namely Mandler, was forced to reject their "path-breaking" work, having sent it to "an overrated elder statesman of psychology" for peer review (G. Mandler 2002*c*: 205).

They didn't appeal to computer models, nor even to specific AI ideas. But they did stress heuristics (they'd been influenced by Simon as well as by Bruner). They'd both been trained in philosophy as well as psychology, but instead of model-theoretic semantics their interest was in the philosophy of science and probability theory.

K&T focused on probabilistic thinking, which is endemic in everyday life (Kahneman and Tversky 1972, 1973; Tversky and Kahneman 1973, 1974, 1981, 1982). Like Babbage (Chapter 3.i.b), they were well aware that inductive thinking can let us down—but they wanted to know just how it works. And like Simon, they were more interested in practical decision making than in 'purely intellectual' judgements (Kahneman and Tversky 1979).

Simon himself was sympathetic to their views. Indeed, when Gigerenzer told him that he thought K&T's work to be inconsistent with Simon's notion of bounded rationality, he was "surprised". Apparently, however, Gigerenzer persuaded him. For later, when he asked Simon what he thought of K&T's followers describing their work as a study of bounded rationality, he replied:

> That's rhetoric. But Kahneman and Tversky have decisively disproved economists' rationality model. (quoted in Gigerenzer 2004*a*: 396)

The reason for Simon's word "rhetoric" was that K&T had forgotten the ant. The ant's behaviour is partly explained by *the detailed structure of the environment*, but K&T ignored this. Besides focusing on mental processes (cognitive heuristics and illusions), they considered 'the world' only in the guise of the laws of probability.

The heuristics they claimed to have identified—though *without* using computer models or computational concepts—were said to involve "natural assessments", of ease of recall, or similarity, or relation to some prior judgement. So K&T discovered that people tend to overestimate the probability of a state of affairs if actual instances of it are easily recalled ("availability"), or if it fits their pre-existing stereotypes ("representativeness"). In addition, they're reluctant to adjust their initial judgements in light of potentially relevant information that's provided later ("anchoring and adjustment")—an effect previously noted with respect to affectively laden decisions by the theory of cognitive dissonance (Section i.c, above).

K&T argued that these heuristics guide judgements as to whether (for instance) a brief personality sketch describes a lawyer or an engineer, or whether I'm likely to get cancer if I smoke thirty cigarettes a day. In general, they're reliable. But sometimes they let us down, even when the 'correcting' data are available.

Thanks ("thanks"?) to the representativeness heuristic, for instance, the information that the population from which the mystery personality was drawn consists of thirty engineers and seventy lawyers (or vice versa) has little or no effect on people's decisions to assign the person to one class or the other. It's their preconceived ideas about engineers and lawyers which make the difference, not the base rate in the population. In the laboratory, who cares? But it's another matter when we consider a jury having to decide, supposedly *on the basis of the evidence presented in court*, whether a librarian is guilty of murder. (This example may ring warning bells: see the discussion of "cognitive illusions" below.)

Some heuristic-grounded mistakes are highly predictable. One is the Monte Carlo fallacy, in which a roulette player wrongly believes that a preceding run of blacks increases the probability of the next throw's being red, or a coin-tosser imagines that twenty heads must be followed by a tail. Even if a clear warning-explanation has been given, only the most resolute among us can withstand this judgemental bias entirely.

Another is what K&T called the conjunction fallacy. When their experimental subjects were told that Linda is single, intelligent, a graduate in philosophy, and has demonstrated against discrimination and nuclear weapons, 85 per cent decided that she was "more likely" to be *a bank teller and active in the feminist movement* than to be *a bank teller* (Kahneman *et al.* 1982: 91 ff.). But a conjunction can't be more probable than one of its conjuncts. Clearly, K&T said, people were being influenced by the representativeness heuristic.

As the persistence of the Monte Carlo fallacy illustrates, it can be difficult to counter or prevent biased reasoning. Indeed, it's often deliberately encouraged. Effective rhetoric presents the evidence in such a way as to maximize the likelihood that the hearer will make *this* judgement rather than *that* one—which may or may not be the correct judgement. Often, such persuasive presentation is done unthinkingly. But K&T's work led to a flurry of research on how to do it in a relatively systematic way.

For instance, they discussed the rhetorical implications of the fact that people generally give more weight to potential losses (risks) than to potential gains (Kahneman and Tversky 1979). This fact had been 'discovered' nearly thirty years earlier by the economist Harry Markowitz, who eventually received the Nobel Prize for his work on risk minimizing in the stock market (Markowitz 1952; Rubinstein 2002). But, as K&T acknowledged, canny communicators had sensed it intuitively long before that. Indeed, Tversky's *New York Times* obituary reported him as having said that he merely examined in a scientific way things about behaviour that were already known to "advertisers and used-car salesmen" (and, one may add, politicians).

K&T's work, and similar research directed by the social psychologist Richard Nisbett (Nisbett and Ross 1980), promoted considerable discussion in the 1980s about the extent to which people are rational, and also about *what it means* to say that a judgement is rational. K&T themselves believed they'd shown that we're largely *irrational*, declaring that humans are guided by "cognitive illusions". (Likewise, Nisbett highlighted our "shortcomings" in the title of his influential book.)

However, the Oxford philosopher L. J. Cohen (1981) used the pages of *BBS* to deny that they'd done any such thing—and even to question whether *any* experimental methodology could do so. He pointed out, for instance, that to start from a false premiss doesn't prevent one's arguing validly on the basis of it. Gamblers who commit the Monte Carlo fallacy won't win a fortune. But they're thinking rationally rather than irrationally, given their false belief about probabilities. Similarly, given their mistaken (non-Bayesian) assumptions about probability, their inference to the false premiss is itself rational. For sure, they aren't acting *non-rationally*, for their behaviour is determined by the semantic content of their beliefs and desires. That is, they're rightly viewed from the "intentional stance"—which presupposes rationality, so defined (Chapter 16.iv.b).

Even Cohen's defence of the rationality of everyday reasoning, however, was less startling than what happened next.

g. Evolved for success

In the late 1980s, Gigerenzer (1947–) came onto the stage—and rewrote the script. For if K&T had largely codified common sense, Gigerenzer came up with some highly counter-intuitive findings. (For recent compilations, see Gigerenzer and Todd 1999; Gigerenzer and Selten 2001.)

His 'negative' position was stated in the subtitle of one of his papers: 'How Intelligent Inferences [Only] Look Like Reasoning Errors' (Hertwig and Gigerenzer 1999). His 'positive' position harked back to Simon's ant. (Indeed, he took Simon's phrase "bounded rationality" as the title of one of his books: Gigerenzer and Selten 2001.) For his central claim was that intelligent animals—including *Homo sapiens*—have evolved decision-making mechanisms that let *the environment* do the work.

Certainly, he said, some highly educated humans can use formal logic or Bayesian probability theory. But extraordinarily simple—"fast and frugal"—heuristics can match, or sometimes even surpass, these 'rational' methods. With respect to reasoning, one might say, less is often more (compare the developmental advantages of "starting small": 12.viii.c.) "The rationality of heuristics", Gigerenzer said, "is not logical, but ecological."

Describing people as "intuitive statisticians" (Gigerenzer and Murray 1987), Gigerenzer used frequency-based statistical theory as a model of what goes on in the mind. He saw this suggestion as just one example of a common creative strategy in science, the "tools-to-theories" heuristic—*mind-as-computer* being another (Gigerenzer 1991*b*; Gigerenzer and Goldstein 1996*b*). That fitted his overall position, which was that the environment inspires most of our thinking. But whereas his theory had come from (environmentally triggered) creative thought, the intuitive statistics—he said—had come from biological evolution.

That didn't mean, of course, that they couldn't be encouraged and supported by education. At present, cultural numeracy is thought of in terms of precise (maximizing) arithmetic. Gigerenzer (2002*b*) semi-seriously foresees a time (fifty years hence) when the President of France and the President of the World Health Organization may share a flower-decked platform with historians of psychology and economics to celebrate the WHO's latest achievement: abolishing innumeracy in the developed world, much as illiteracy has been abolished already. But innumeracy will be defined by them—as it was by H. G. Wells, according to Gigerenzer (but see Tankard 1979)—as statistics, not arithmetic.

(That's *not* to say that Gigerenzer recommends the usual statistical practices of experimental psychology. On the contrary, he regards these as often "mindless" and irrelevant: "Statistical rituals largely eliminate statistical thinking"—Gigerenzer 2004*c*: 587.)

In a variety of experiments, Gigerenzer found that if problems are posed in terms of population frequencies instead of individual cases, people are much less likely to make a misjudgement. For instance, he provided the 'profile' which K&T had used to describe "Linda", but then told his subjects that "There are 100 people who fit the description above," and asked "How many of them are *bank tellers*, and how many are *bank tellers and active in the feminist movement?*" (Gigerenzer 1991*a*). The error rate, or conjunction fallacy, dropped sharply: from K&T's 85 per cent to between 10 and 20 per cent. (For a recent paper casting even more doubt on the prevalence of the conjunction fallacy, see Hertwig and Gigerenzer 1999).

Thus far, psychologists (and philosophers) could—and many did—insist that bounded rationality is inferior to 'real' rationality, as identified by logic or mathematics. Gigerenzer might have shown that our intuitive reasoning isn't quite so bounded as K&T had claimed—but bounded it still was. After all, the ideal error rate is zero. Now, Gigerenzer came up with another surprise, not to say a bombshell.

Using computer modelling, he and Daniel Goldstein ran a competition between optimal mathematical algorithms and his own suggested heuristics (Gigerenzer and Goldstein 1996*a*, 1999; Gigerenzer 2000, ch. 8). Amazingly, they found that simple intuitive heuristics can often do *just as well as*, or even *better than*, the supposedly optimal rational methods. (Remember: this was being done on computers, which have no problems with the maths.)

In one experiment, for example, the best mathematical methods for doing integration, such as multiple regression, were compared with a heuristic called "Take the Best (Ignore the Rest)"—TtB for short. What TtB tells the system to do is to base its inference *only* on the cue which in the past has discriminated best between alternatives. All other cues are ignored. In other words, a large amount of supposedly relevant information is discarded.

One can well believe that the one-item ("frugal") TtB is *useful*, in an unfriendly and fast-changing world. But surely, mathematically optimal methods would be better? Not so, apparently. In the computer models run by Gigerenzer and Goldstein, Ttb surpassed all the mathematical methods in speed, and matched or surpassed them all in the proportion of correct inferences. (Further computer simulations run by Nick Chater suggest that other simple heuristics are at least as plausible as TtB: Chater *et al*. 1997.)

Another set of counter-intuitive results involved Gigerenzer's "Recognition" heuristic. (This wasn't the same as K&T's "availability", which referred to ease of recall, not recognition.) Like TtB, it asked only one question: "If one of two objects is recognized and the other is not, then infer that the recognized object has the higher value with respect to the criterion." (Notice: *any* criterion.) This simple rule accounted for the success of 'pure' (i.e. uninformative) brand-name advertising. And it explained more bizarre phenomena, too.

For instance, Gigerenzer's German students were much better than US students at deciding whether San Diego or San Antonio has the larger population—even though most of them had never even heard of San Antonio. But that was the point. If one's heard of a foreign town, it's likely to be populous. There are exceptions: one can hear a visitor reminiscing about their home town, or see a foreign movie featuring a small village . . . But whenever our ignorance is systematic rather than random, so that recognition is strongly correlated with the criterion of interest, then the question "Have I encountered it?" is likely to be the best clue.

The Recognition heuristic evolved because, for species other than *Homo sapiens*, ignorance almost always is systematic. Ecologically important features tend to be noticed; and features that are noticed tend to be ecologically significant. It's only humans who can generate interest in largely irrelevant matters, like those featured in pub quizzes and Trivial Pursuit. Facing pointless questions such as these, the Recognition heuristic can let us down. But it can let us down in important decision making too (including decisions made in the law courts), where our culture has provided us with knowledge divorced from systematic significance. So although Gigerenzer (2004*b*: 80) doesn't say that ignorance is bliss, he does say that—often—"Less is more."

In short, Gigerenzer offered a radically new vision of "bounded rationality". It's not a regrettably necessary second-best—as Simon and everyone else had assumed. Rather, it can (often) give accuracy, not mere approximation. Admittedly, what's being approximated are the *norms* of rationality: they hadn't been forgotten. But they were being used by Gigerenzer as evaluative ideals, not as blueprints of 'the best' reasoning processes.

(Recently, he's raised the stakes still further. Now, he considers not just rationality but happiness, too:

> Satisficers are reported to be more optimistic and have higher self-esteem and life satisfaction, whereas maximizers excel in depression, perfectionism, regret, and self-blame. (Gigerenzer 2004*b*: 80; cf. B. Schwartz *et al*. 2002)

No wonder, then, that he follows this sentence with "Less can be more.")

To put it another way, Gigerenzer's claim was that *the environment itself* gives us the information we need to get the right answers: we don't need recourse to maths or logic. This ant insight about the importance—indeed, the sufficiency—of environmental cues was largely responsible for his quarrel with K&T about cognitive illusions (Gigerenzer 1991*a*, 1996; Kahneman and Tversky 1996).

He made two charges against K&T. First, that their one-word "heuristics" were so vague that they explained everything and nothing (as had been said of cognitive dissonance theory in the late 1950s). For instance, both the Monte Carlo fallacy and its opposite, expecting a long run of blacks to *continue*, were attributed to *representativeness* by K&T (Gilovich *et al*. 1985: 295; Tversky and Kahneman 1974: 1125). Second, that our minds are guided not by cognitive illusions (i.e. subjective probabilities) but by veridical perceptions of real-world patterns (i.e. frequencies). K&T countered that judgements of frequency are equally 'subjective'. Gigerenzer replied, in turn, that his computer models enabled him to say just when frequency judgements would be valid and when they would not.

K&T also declared that "representativeness (like similarity) can be assessed experimentally; hence it need not be defined a priori" (1996: 585). Gigerenzer (2004*b*) sees this as an egregious example of *Hypotheses non fingo* (Chapter 5.i.a). K&T, he says, had regressed to behaviourism. Indeed, decision-making research in general abounds with "surrogates for theories . . . from one-word explanations to mere redescription to ying–yang dichotomies". Yet testable computer models of heuristics have been feasible for many years. Evidently, then, even in the twenty-first century not all cognitive psychologists are cognitive scientists.

The general public, of course, couldn't have cared less about the arcane reaches of probability theory ("frequencies", and the like). Nor did they get excited about "theories" versus "surrogates for theories". But much as they'd previously been interested in popular accounts of K&T's work, and Nisbett's too, so they were now intrigued by Gigerenzer's findings. His own popular writings were widely read, not least because they didn't merely describe irrationality in important life decisions but also gave advice on how to avoid it.

The public's interest in A-Life and evolutionary psychology, which burgeoned in the early 1990s (Chapter 15.x.a and Section vi below), added power to his elbow. By this time, Gigerenzer was Director of the Centre for Adaptive Behaviour (*sic*) and Cognition at Berlin's Max Planck Institute. The ant insight, indeed, was flagged in the title of his millennial book, *Adaptive Thinking: Rationality in the Real World* (2000). Accordingly, the statistical heuristics he described were presented as *evolved adaptations*, essentially comparable with the behavioural reflexes of crickets (15.vii.c). Most evolutionary psychologists had concentrated on motivation (goals, sexual preferences, selfishness, altruism . . .) and perception (a species' sense organs will 'fit' its habitat and motor repertoire). Now, Gigerenzer had moved beyond these, into topic-neutral reasoning.

It's little wonder, then, that Steven Pinker (1954–)—author of *The Language Instinct* (1994) and scourge of "the blank slate" (2002)—approved. He even provided a puff to die for. The cover of Gigerenzer's trade book *Reckoning with Risk* (2002*a*) was emblazoned with Pinker's accolade: "Gigerenzer is brilliant and his topic is fabulous."

h. Give thanks for boundedness

Gigerenzer's "fabulous" topic received an intriguing added twist a couple of years later, when two of his colleagues reversed the causal direction assumed in his work.

He'd said, in effect, that simple heuristics evolved because of inescapable processing limits in the brains of cognitive creatures, *Homo sapiens* included. But Ralph Hertwig and Peter Todd now argued that those very processing limits have evolved because they're required by simple heuristics—which have been selected because of their (highly adaptive) speed and robustness (Hertwig and Todd 2003: 223–8).

To be sure, no brain can have absolutely unlimited processing capacity. But its specific limits, on this view, aren't inevitable—and, way back in our phylogenetic history, they didn't exist. They've been evolved, just as successful heuristics have been—with the latter driving the former, not the other way around. So Gigerenzer's "simple heuristics" aren't *compensations* for the boundedness of human rationality: in evolutionary terms, they're partly *responsible* for it.

Counter-intuitive though this may seem, it's actually a special case of a more general phenomenon. For there's persuasive evidence that processing limits can *help* one to learn what must be learnt (Chapter 12.viii.c–d). Research on "starting small" in the development of language (Elman 1993), and on "representational trajectories" in general (Clark and Thornton 1997), implies that bounds on rationality are a Good Thing, especially—but not only—in infancy.

So Simon's insistence on bounded rationality has been stood on its head. Limits on our processing power are to be welcomed, not merely acknowledged. Without them, we'd be overwhelmed by the richness of the environment. Our minds would be swamped by what James called the "big blooming buzzing confusion" which is our "immediate sensible life"—that is, perception shorn of all conceptual interpretation (James 1911: 50).

The particular ways in which our rationality happens to be bounded are neither inevitable nor, in some cases at least, accidental. Whether comparative psychology and/or computational studies will ever enable us to say just which limits have been specifically selected, and why, remains to be seen. What about the "magic number", for instance: why not ten, or four, rather than seven? (see 6.i.b). Such questions should ideally be considered by people thinking about the computational architecture of a wide range of *possible* minds (Sections i.e–f and iii.d, above).

In sum, Gigerenzer and his colleagues argued for an especially strong version of the thesis that, despite our many failings, we're actually pretty good at doing what we need to do.

A recent commentary on twentieth-century psychology, especially social psychology, has pointed out that the dominant strategy has been to identify, and attempt to explain and/or "fix", *deviations* from accepted norms—whether these be norms of rationality or of civilized interpersonal behaviour (J. I. Krueger and Funder 2004). Festinger's study of the cult of the Guardians on the planet Clarion (i.c, above) was one of many examples; Stanley Milgram's (1963, 1974) astonishing work on obedience was another; and—initially—the study of visual illusions yet another. One reason for the popularity of this strategy is that counter-intuitive findings, besides being interesting in themselves, have a better chance of escaping the scorn of Senator Proxmire and his ilk (6.iv.f; cf. Krueger and Funder 2004: 316). A more "balanced" psychology would concentrate also on our *strengths.*

Within cognitive psychology, that's started to happen. Work on visual illusions, for example, has increasingly seen these not as failings but as grounded in normally *successful* adaptive mechanisms, which don't happen to fit the unusual circumstances concerned (6.ii.e). Gigerenzer, too, saw not failings, but strengths.

It's not that he diverted his eyes from the failings, looking only at the strengths. Rather, he saw strength in a given aspect of behaviour where others saw weakness. Even Simon, the major champion of bounded rationality, generally implied that it's an unfortunate—though necessary—limitation. Gigerenzer, by contrast, would expect to find "simple heuristics that make us smart" even in an ideal world. (This switch from negative to positive hasn't yet spread across psychology as a whole: "so far [these cognitive psychologists'] influence on social psychology has been limited"—Krueger and Funder 2004: 311.)

7.v. Visions of Vision

Gregory and his New Look colleagues weren't the only ones to look anew at vision (Chapter 6.ii). In the early 1970s there was a sudden resurgence of experimental work on the forbidden topic of imagery. The core questions were computational, concerning the nature of 'imagistic' representations and the processes that could affect them.

The 1980s saw a sea change in theoretical styles. The key questions were still computational—if anything, more so than ever before. But the interest switched from top-down hypothesis-based interpretation to interpretation by means of bottom-up automatic processing.

In some circles, the New Look had never found favour. Wittgensteinian philosophers had no time for it (see 16.v.f). Nor did all psychologists. Even as it was being designed in the late 1940s, a distinctly *non*-identical twin was approaching the catwalk (J. J. Gibson 1950). In his theory of "direct" perception, Gibson resolutely avoided talk of models and interpretation. And when psychologists later spoke of computational processes in perception, Gibson was lying in wait to ambush them.

a. Icons of the eyes

The major steps in the renaissance of imagery as a fit topic for psychology were two publications in 1971. At first sight, they were very different: one ran to 600 pages (including over forty pages of bibliography), the other only to three.

The first was Allan Paivio's book on *Imagery and Verbal Processes*. This added chapter and verse to the core argument that he'd published two years earlier in the *Psychological Review* (Paivio 1969). The second was the paper on 'Mental Rotation of Three-Dimensional Objects' by Roger Shepard (1929–) and his student Jacqueline Metzler. These offerings, from the universities of Western Ontario and Harvard respectively, revivified a long-neglected dimension of cognitive psychology.

Paivio (1925–) reminded readers of a wide range of largely forgotten research. This included various counter-intuitive findings, such as a classic demonstration of confusion between visual imagination and perception (Perky 1910). If someone is looking at a screen, and is asked to imagine seeing a ship depicted on the screen, they can do so. However, if an image of a ship is secretly projected onto the screen, the person may not realize this. In Hume's terminology, they mistake an *impression* for an *idea.* (An anti-hallucination, one might say.) Paivio's work was startling primarily because it had until recently been forbidden, being incompatible with the behaviourist hegemony.

Shepard and Metzler were being daringly unfashionable too. But their main claim to fame was that they reported startling new results. They'd studied people's ability to imagine rotations of 3D objects (see Figure 7.3). In a nutshell, they'd found that

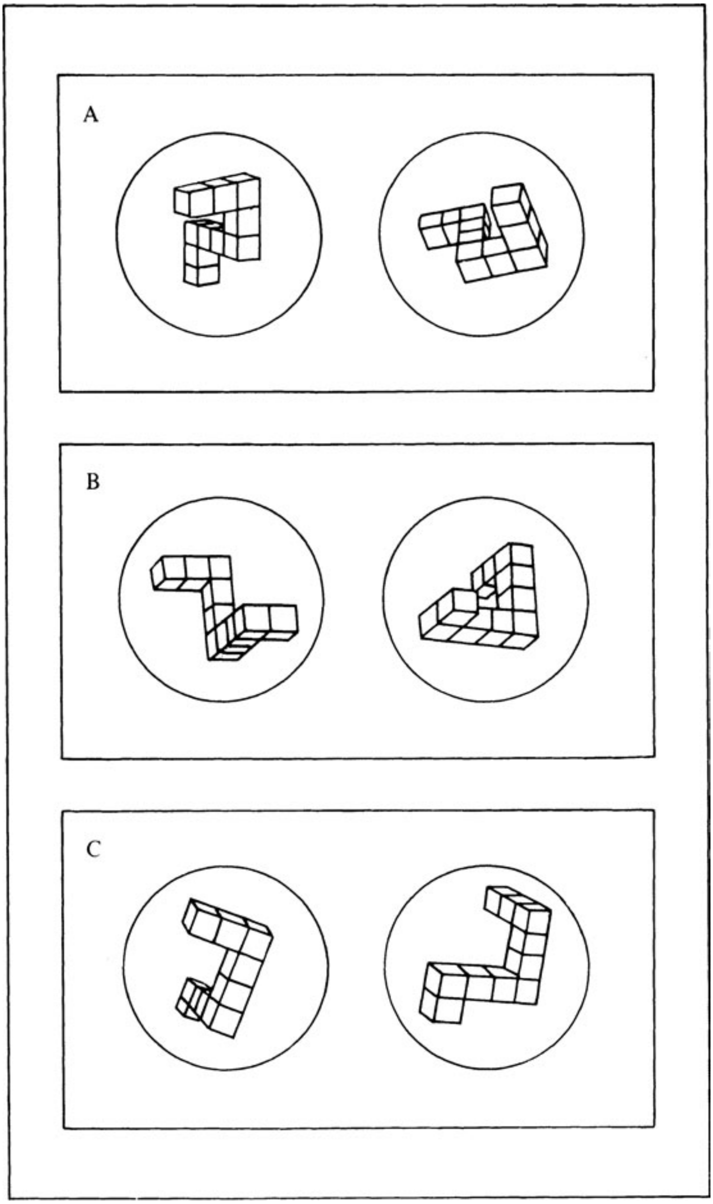


FIG. 7.3. Each of these six drawings represents a three-dimensional structure made up of ten cubes stuck together. People were asked to look at pairs of drawings and to decide whether the drawings showed one and the same object (rotated) or two different objects. Three of the 1,600 pairs used are shown here. The A-pair and the B-pair show rotations (in the plane of the page and perpendicular to the page, respectively), but the C-pair does not. Adapted with permission from Shepard and Metzler (1971: 702). Copyright 1971

the time taken for someone to decide whether two objects are identical, but shown in different orientations, is directly proportional to the degree of rotation involved.

In both cases, the researchers' main focus wasn't on the bizarre experimental data, but on their theoretical implications. Both Paivio and Shepard and Metzler argued that internal visual representations ("images") exist. Further, they both insisted that they're fundamentally distinct in nature from verbal representations—and, by implication, from the symbolic representations favoured by GOFAI.

Work on computer vision ("scene analysis") at that time represented visual/perspectival features by symbolic expressions like those used to represent verbal meanings (10.iv.b). But Paivio argued that "iconic" representations are very different. Similarly, Shepard and Metzler took their rotation experiments to show that some analogue (*sic*) of real 3D rotation was actually going on in the brain.

Immediately, experimental work on imagery blossomed. Besides a host of follow-up studies by Shepard and others (e.g. Shepard and Cooper 1982), the Harvard psychologist Stephen Kosslyn (1948–) initiated a rather different series of studies—which caused even more of a sensation than the rotating multi-cubes had done (Kosslyn 1973, 1975; Kosslyn and Pomerantz 1977).

For instance, he asked people to memorize a map of a fictitious island, showing the church, the lighthouse, the river and its bridge, and so on. Next, he asked them to visualize the map in their minds and to focus on a certain place: the lighthouse, say. Then, they were told to imagine a spot moving from the lighthouse to the bridge (or the church), and to press a button as soon as the spot reached the destination. Amazingly (or so it seemed to Kosslyn), the time taken to press the button varied according to the distance on the map between the positions of lighthouse, bridge, and church.

Again, the conclusion drawn by the experimenter was that there must be some property of the mental image which is analogous to real distance, and some process of traversing an image which is analogous to crossing real space. Kosslyn claimed, for example, that in image traversal as in actual traversal, all the intermediate points on some path between two points have to be visited in getting from one to the other. That's implied, he said, by there being some quasi-spatial form of representation, whose inherent properties allow transformations (such as imagined movements) isomorphic with spatial transformations. He used the tendentious term "image-scanning" to describe how this representation is supposedly used.

The Shepard–Kosslyn data caused a furore. Nonetheless, some people felt they were too good to be true.

Shepard and Metzler's subjects had remarked that they were "rotating images in their minds". But so what? Psychologists had known for sixty years that such remarks can't be taken at face value. For instance, people may be mistaken when they sincerely claim to be 'reading off' a visual image (of letters arrayed in rows and columns) much as they can read off a visual perception; for information (about diagonals, for example) that's available in the perceptual case isn't accessible here (Fernald 1912). In other words, the conscious phenomenology needn't match the underlying representation and/or the method of accessing it.

Even more to the point, some psychologists argued (*a*) that these new results weren't exciting at all, since they were only to be expected, and (*b*) that alternative explanations were possible. Indeed, (*a*) and (*b*) were closely linked.

The key critic here was Pylyshyn. In his 1973 paper 'What the Mind's Eye Tells the Mind's Brain' (a cheeky reminder of the classic 'What the Frog's Eye Tells the Frog's Brain': 14.iv.a), he argued that our power of imagery is deeply imbued with knowledge. As he put it, it's *cognitively penetrable*. It's not determined by the mind/brain's fixed functional architecture (FFA), but by the person's knowledge, or beliefs (see 16.iv.d). That knowledge, he said, is reflected when we're asked to imagine something.

We know, for instance, that it takes more time to walk a greater distance. So only an idiot would press Kosslyn's destination button immediately, having been asked to imagine a long traversal. By the same token, if Kosslyn had asked his subjects—as Pylyshyn now did—to imagine a magical transposition from the lighthouse to the church ("Beam me up, Scottie!"), the button would have been pressed instantly. (There were some puzzling exceptions: people *ignorant of psychophysics* experience imaginary after-images in complementary colours—Finke and Schmidt 1977; Pylyshyn 1984: 247 ff.)

The crux, for Pylyshyn, was that knowledge is stored propositionally, as descriptions. These were assumed to be computational *in the sense defined by Fodor* (1975: see Chapter 16.iv.c–d). And that, he said, gave us two ways of approaching the debate. On the one hand, *if imagery involves descriptions, then it will be cognitively penetrable*. On the other, *if images are quasi-spatial analogues, then the experimental results will not be cognitively penetrable*. The Shepard–Kosslyn data, he said, could be explained by cognitive penetrability. They therefore hadn't shown the need for any special, quasi-spatial, form of representation, provided by the FFA.

Various philosophers and psychologists, even including the Harvard behaviourist Richard Herrnstein, discussed imagery at length (e.g. Fodor 1975: 174–94; Sloman 1978, ch. 7; N. Block 1981, 1983; R. Brown and Herrnstein 1981).

Several of the philosophers warned against reification. To say "He has (I have) an image of a bridge" needn't imply that there's an object—namely, an image of a bridge—which the person has. Simply, the person *is imagining* a bridge—meaning that they're able to talk about an absent bridge in sensible ways. *Having an image* is a predicate (applying to the person) rather than a relation (between the person and a mental object). So the debate that was raging at that time was largely ill-posed. Instead of asking, "Are images descriptions?", people should have been asking, "In what ways are the representations underlying the power of imagery like, and unlike, descriptions?" They should have thought less about *images* and more about *the ability of imagination*: what we do when we're asked to imagine something—and how we manage to do it.

"How we manage to do it" was highlighted by Sloman (1971; 1975; 1978, ch. 7). Where Paivio had spoken of "iconic" and "verbal" representations, Sloman distinguished "analogical" and "Fregean" types.

In an analogical representation, he said, there's some interpretative mapping, or significant isomorphism, between the structure of the representation itself and the structure of the thing represented. To understand an analogical representation is to know how to interpret it by matching these two structures, and their associated inference procedures, in a systematic way. (In other words, similarity isn't enough: Chapter 4.vi.b.) To understand a Fregean representation, such as a sentence or a LISP expression, is to use some procedure essentially comparable to what Frege called the application of logical *functions* to *arguments*. (These were ideal types: 'one' representation may have both analogue and Fregean features.)

Sloman's distinction was clearer than most accounts of analogue representations—a term used in many different ways in any case (Haugeland 1981*b*). That was largely due to his experience of AI. The AI researcher who provides a representation of something must also provide a clearly specified way for the system to use ('understand') it. In the 1970s, however, the need to specify not only what a particular representation is, but also *how it can be used*, still wasn't appreciated by most psychologists (or philosophers, either).

A lot of ink was spilled on the relative merits of iconic versus descriptive representations (for a review, see Boden 1988: 27–44; for Paivio's most recent version, see Paivio 2005). The debate continued well into the 1980s, with critiques of Pylyshyn's methodology and further Shepard–Kosslyn experiments to the forefront. John R. Anderson even put the cat among the pigeons by arguing that it's impossible to discriminate empirically between various forms of representation. The busy programme of experimentation on imagery was a waste of time. Or rather, it might—it had—come up with intriguing new data. But it would never settle the question *iconic or descriptive?*

Anderson agreed with Pylyshyn that propositional representations *could* underlie what we call imagery, and that the concept of iconic representation was unacceptably vague (Anderson and Bower 1973, sect. 14.5; Anderson 1978, 1979). But no experimental evidence could decide between them, because a propositional code could (in principle) represent anything. What was needed, then, was a judicious use of Occam's razor to identify horses for courses:

> At best . . . this is what the imagery-versus-propositional controversy will reduce to—that is, a question of which gives the more parsimonious account of which phenomena. . . . [There] is *no way to prove one is correct and the other wrong*. Moreover, even if parsimony could yield a decision between propositional and imagery theories, there would still remain the possibility that there are *other, fundamentally different, representations* which are as parsimonious as the preferred member of the propositional–imagery pair. (1976: 13; italics added).

(A few years later he relented, up to a point. It was the notations used to express representations which were indistinguishable, he now said, not the computational processes defined in relation to them—1983: 46.)

Anderson's suggestion that there may be other forms of representation would eventually be widely accepted (Chapters 10.iii.a, 12.v–vi and x, 13.iii.a, and 14.viii). However, that took time. In 1980 the first edition of his textbook declared:

> Interestingly, researchers seem to be strongly divided into two camps: those claiming that there is only a propositional-code representation and those claiming that there is only a dual-code [iconic plus verbal] representation. Out of what is probably a false sense of parsimony, no one seems willing to admit that there might be three codes—abstract, verbal, and visual [though] a proposal that comes close to this eclectic point of view [is Kosslyn's] computer simulation for visual imagery. (Anderson 1980: 126)

Evidently, his message hadn't yet been heard.

As his final comment implies, however, it wasn't only experiments on human subjects which had flourished. Computer simulation had forged ahead too. Kosslyn's model (1980, 1981) was soon announced to the world in a trade book (1983). He'd accepted Pylyshyn's point that descriptive knowledge can influence imagery. But he still posited a base of quasi-spatial "depictive" representations in the FFA, transformed in 'spatial' ways (e.g. 'rotation'). These were stored in a *matrix* which "functions like a space"—with a limited extent, a specified shape, and a "grain of resolution" (highest at the centre). Like Anderson himself (and Grossberg too: 14.vi.c–d), Kosslyn continually adapted his model to include new experimental findings.

He optimistically described the most recent version (Kosslyn 1996) as "the resolution of the imagery debate". One doesn't have to share that optimism to allow that psychologists' ideas about the possible types of visual representation have come a long way since the 1970s.

For the record, Shepard (1984) eventually used his epochal 1971 data as the seed of a theory of perception *in general*. That theory became so influential that *BBS* recently devoted a special issue to it (Shepard 1994/2001). (His own historical perspective on this progression is given in Shepard 2004.)

The mental-rotation experiments had suggested that the mind/brain models external reality to a remarkably precise degree. Shepard's explanation of this appeared as the very first paper in the new journal *Cognitive Psychology* (Shepard and Chipman 1970). He argued that the phenomenon was due to "second-order isomorphism" between similarities among shapes and similarities among their internal representations:

> [The] isomorphism should be sought—not [as Craik, and most of his followers, had assumed: 4.vi.a] in the first-order relation between (a) an individual object, and (b) its corresponding internal representation—but in the second-order relation between (a) the relations among alternative external objects, and (b) the relations among their corresponding internal representations. Thus, although the internal representation for a square need not itself be square, it should (whatever it is) at least have a closer functional relation to the internal representation for a rectangle than to that, say, for a green flash or the taste of a persimmon. (Shepard and Chipman 1970: 2)

In other words, this was "a call for the representation *of* similarity instead of representation *by* similarity" (Edelman 1998: 450). (This idea was later developed into a computational theory of vision in general by Shimon Edelman. As he put it, the task of the visual system is to "*represent similarity between shapes, not the geometry of each shape in itself*" (1998: 451)—which is why people are much better at recognizing similarities between shapes than at perceiving shape as such.)

As for how the brain is able to recognize similarities, Shepard later claimed that a number of cognitive universals act as "the evolutionary imprint of the physical world". Just as our biological circadian rhythms have internalized the diurnal movement of the earth so, he said, have our perceptual mechanisms internalized other aspects of the world and our bodily relations to it: the geometry of observed objects-in-motion, for example.

These mechanisms were comparable to Kantian intuitions and schemas—but placed in us by evolution, not God. That is, Shepard's 1984 theory was an instance of evolutionary psychology—a nativist enterprise that had hardly begun when he did his initial experiments on imagery (see Section vi below, and Chapter 8.ii.d–e).

b. Vision from the bottom up

A huge change in the way that psychologists thought about vision was brewing in the late 1970s. Some bubbles were formed as a result of Marr's first papers on vision (1975*a*,*b*,*c*), and of his general statement in MIT's book on home-grown AI (D. C. Marr 1979). But it boiled over in 1982, with the publication of Marr's book on the subject. The book was widely read even by people with no special interest in vision, because of its challenging claims about the nature of psychological explanation *in general* (Section iii.b, above). But visual psychologists, of course, had even more reason to be interested.

Marr had cut his computational teeth in the late 1960s, by pioneering formal theories of the brain (Chapter 14.v.b–e). By the mid-1970s, however, that work couldn't be carried any further. Thanks largely to Minsky, whom he met in May 1972, he now turned his attention to vision—and to computer modelling, as opposed to abstract formal modelling. (The story of his eventful encounter with Minsky is told in Chapter 14.v.f.)

Despite his intellectual debt to Minsky, he didn't approach computer vision in the way that was dominant in Minsky's AI Lab at the time. Instead of favouring GOFAI work, he drew inspiration from MIT's Berthold Horn, who was doing detailed research on optics and psychophysiology. Marr's computer models were connectionist (but not PDP), consisting of many simple processing units operating in parallel and communicating locally (see Chapter 12). As such, they were very different from GOFAI models. Indeed, much of Marr's energy through the 1970s, until his premature death in 1980, would be devoted to criticizing GOFAI scene analysis (10.iv.b), and the New Look assumptions that informed it.

In particular, he avoided talk of top-down processing guided by learnt concepts, or high-level expectations. He frequently pointed out that we can see (locate, distinguish, manipulate) things we *don't* expect to see, because we've never seen them before.

When Captain Cook's sailors first met a kangaroo, they were instantly able to pick it out from the bush, see the soft texture of its fur, and locate it precisely—whether to stroke its flanks or to shoot it in the head. And the ship's artist was able to draw it. On his return to England, many people thought his picture a flight of fancy, so different was this animal from any they'd seen before. But the artist had 'used his eyes'. Of course, he'd used his brain too (remember Gregory?)—but the visual more than the associative cortex.

Marr's view was that for this sort of thing to be possible, we must be able to rely on *general* image-processing mechanisms. He wasn't denying that top-down perceptual influence occurs: how could he, given the intriguing playing-cards data described in Chapter 6.ii.a? But his prime interest was in the automatic, bottom-up, processing that enables us to see even unfamiliar things.

It's because this processing is automatic that, as Gregory had shown, many visual illusions are culture-neutral and persist even when we *know* they're illusory. The processes involved aren't modifiable by conceptual knowledge (in Pylyshyn's terms, they're not cognitively penetrable) because they're built-in, honed by millions of years of evolution. In other words, Marr was positing what Chomsky and Fodor would later call visual *modules* (see Section vi.d, below).

His research at MIT was "computational" in three senses:

* First, it was grounded in his three-level theory of psychological explanation (iii.b, above).
* Second, it offered specific algorithms intended as descriptions of visual processing—for detecting (for instance) edges, texture, or depth.

* And third, it involved extensive computer modelling, on MIT's state-of-the-art machines. (At first, even these weren't powerful enough for what he wanted to do: see 14.v.f.)

Crucially, his theory—and his computer models—assumed that visual information is passed bottom-up rather than top-down. (Remember the kangaroo.)

It was already known that light-intensity gradients are computed (or, as some people still preferred to put it, detected) in the retina. In other words, the 2D image on the retina, caused by the light entering the eye, is coded by a series of nerve impulses representing the 2D-changes between light and dark. That's done by cells in the retina itself. But Marr argued that the mapping between raw light intensities, or even intensity gradients, and a 3D description of the object concerned is far too complex to be computed in one step. There must be a *series* of visual representations, of increasing abstractness, between the retinal image and the final perception (see subsections b–c).

It follows, he said, that a theory of vision, in identifying these, must specify distinct representational primitives at each stage, showing how they might be constructed from the primitives of the stage before. And because information that will be required for later computations mustn't be lost at earlier representational stages, the theorist should prove (*sic*) that specific sorts of information can be implicitly preserved at a given level, even if they're not explicitly coded by the primitives of that level.

By the time Marr turned to the study of vision, the neuroscientists had spent twelve years discovering the rich variety of feature detectors in visual cortex, and even longer on studying the retina (Chapter 14.iii.a and iv). In addition, the psychophysicists (such as Horn) had related what was known about the retina to theoretical optics. Unlike most connectionist modellers (of whom he had a low opinion: 14.v.f), Marr took these facts seriously, keeping as close to them as he could. And in doing so, as in his earlier work on the brain as a whole, he *made sense* of the neuroscience. Christopher Longuet-Higgins later described neurophysiology pre-Marr as "a theoretical vacuum", despite the profusion of intriguing data (Chapter 14.v.f).

Besides making sense of the neurophysiology, Marr set a new standard of rigour for the psychology of vision. He offered an integrated set of precisely specified, and computer-testable, hypotheses about visual representations and processes. Even if they turned out to be wrong (as his first theory of stereopsis, for instance, soon did: 14.v.f), the onus was on others to replace them with theories of equal clarity. (Stephen Grossberg was attempting something similar at much the same time, but for lack of rhetorical crispness his work wasn't widely read: see 14.vi.a.)

Sutherland, ever contemptuous of vagueness in psychology (Chapter 5.ii.a) and a committed computationalist to boot, was full of admiration. He'd been won over even before Marr started publishing on vision in the late 1970s, due to Marr's papers on the brain—and their personal friendship. He described Marr's volume *Vision* (1982) as "perhaps the most important book on the subject to appear since Helmholtz's *Physiological Optics*" (1982: 692).

Unfortunately, Marr himself didn't live to see this encomium. With typical English understatement, his Preface declared that "in December 1977, certain events occurred that forced me to write this book a few years earlier than I had planned". He was referring to a diagnosis of leukaemia. By the time the book went to press, overseen by his MIT colleagues, Marr was dead.

c. Maths and multimodels

Marr had set his sights high. He'd aimed to revolutionize the psychology of vision, not by replacing one highly respected theory by another but by providing *the first theory worthy of any respect.* (Helmholtz, to be sure, was an intellectual hero: but the mid-nineteenth century was far too early for *theories.)* This was why one reviewer said:

I urge psychologists to read [his book], *if only to ponder in a spirit of self-criticism whether Marr is justified in his largely dismissive remarks about the contributions of psychology so far* to the study of visual perception. His view is widely shared by many scientists from different disciplines, so it deserves close attention. (Morgan 1984: 165; italics added)

In his critique of previous accounts, Marr was uncompromising. We need not plausibility, not persuasion, but mathematical proof:

extreme care is required in the formulation of theories because nature seems to have been *very careful and exact* in evolving our visual systems . . . Having to formulate the computational theory of a process introduces *a great and useful discipline* into the subject. No longer are we allowed to invoke a mechanism [based on correlations, or Fourier transforms, for instance] that seems to have some features in common with the problem [e.g. stereopsis] and to assert that the mechanism works *like* the process. Instead, we have to *analyze exactly* what will work and be prepared to *prove* it. (1982: 75; all italics added except "*like*")

Just what sort of beast was this 'first ever' visual theory? In general, Marr argued from first principles—the optics of the image-forming process (where the 'image' is the pattern of excitation on the retina)—in suggesting how the visual system interprets light intensities. As he put it:

From an information-processing point of view, our primary purpose now is to define a representation of the image of reflectance changes on a surface that is suitable for detecting changes in the image's geometrical organization that are due to changes in the reflectance of the surface itself or to changes in the surface's orientation or distance from the viewer. (1982: 44)

He identified six very general (physical) constraints on physical surfaces, including the fact that they're organized on different levels of detail—so requiring "a number of different [representational] processes, each operating at a different scale" (p. 46). The basic elements in the retinal image are intensity changes, but these are structured (organized) in a way that "yields important clues about the structure of the visible surface" (p. 51). Therefore, the image structure "needs to be captured by the early representations of [it]". If it weren't, then the information about organization would be lost, and couldn't be reconstructed later.

Marr posited several levels of visual representation, each building on the one before. First, there's the Primal Sketch. Next, the $2\frac{1}{2}$D Sketch. And finally, the 3D model (or 'object model'). And he did what he'd said should be done (see above): at each stage, he defined the representational primitives precisely, and related them to the primitives of the stage below.

Distinct algorithms were defined for computing particular aspects of visual perception—depth, orientation, motion, colour, and so on. These were automatic, and procedurally separate from each other. In other words, they were "informationally encapsulated": Section vi.d below. (Later, it was discovered that the different aspects are dealt with by distinct anatomical regions in visual cortex: Zeki 1993.)

These algorithms, in turn, could consist of several sub-levels of processing. The Primal Sketch, for example, is built up in several hierarchical stages.

First, retinal cells identify the changes in light intensity ("zero-crossings") in the image. At base, that's all that the visual system has to go on. The "raw" Primal Sketch codes these in terms of (2D) shape, size, orientation, and discontinuities. That is, it distinguishes blobs and bars of various widths and lengths, and locates bar-ends and curves.

Next, *groups* of bars or blobs are distinguished, on grounds of common shape, size, or orientation. Finally, the full Primal Sketch represents the overall disposition of the groups: so, for example, a boundary is constructed between two sets of groups sharing a common orientation, as in Figure 7.4. (Notice that Figure 7.4 already includes much of the information relevant to the Gestalt principles of "good form".) In short, the Primal Sketch explicitly codes information about (2D) *textures*.

To go from 2D textures to 3D surfaces requires further computation. So the system now constructs the $2\frac{1}{2}$D Sketch. This represents the 3D surfaces in the scene, and their orientation and depth—all described *relative to the viewer*. In effect, the $2\frac{1}{2}$D Sketch tells you (for instance) that a certain surface is 3 feet away from you, now, and oriented at the same angle as you are, now.

But what you usually need to know is what objects (not surfaces) are out there, and where they are in 3D space, irrespective of your viewpoint. A fully 3D coordinate frame, which explicitly relates visible surfaces to (partly invisible) objects, is constructed at the third level: the 3D model.

This codes volumetric as well as surface properties, using the information about edge and surface contours contained in the $2\frac{1}{2}$D Sketch. Now, it's possible for the visual system to represent the fact that two different retinal images depict one and the same physical object, whose shape, size, volume, and location are all *independent* of viewpoint. (This progression from visual subjectivity to objectivity would later be used to explain how concepts develop from non-conceptual content: Chapter 12.x.f.)

Marr posited a multitude of internal 3D models, to explain the objective interpretation of unfamiliar 2D silhouettes. But (unlike the models highlighted by the New Look) these are automatically constructed bottom-up, not learnt and applied top-down. In describing them, he drew on work done by GOFAI's Thomas Binford and the mathematician Harry Blum. Binford had spoken of "generalized cylinders" in discussing the computer recognition of shape *in general* (1971; Agin and Binford 1973; Nevatia and Binford 1977). Blum (1973) had focused on biological shapes, and had defined a bottom-up method for identifying the *axis* of an unfamiliar body or body part.

Marr argued that certain optical constraints on the image-forming process can be exploited *only* by a representation based on "generalized cones". This is the class of volumes generated by moving a closed curve along an axis, where the curve may change its size—and, in a fully generalized cone, its shape. (Think of the in-and-out mouldings

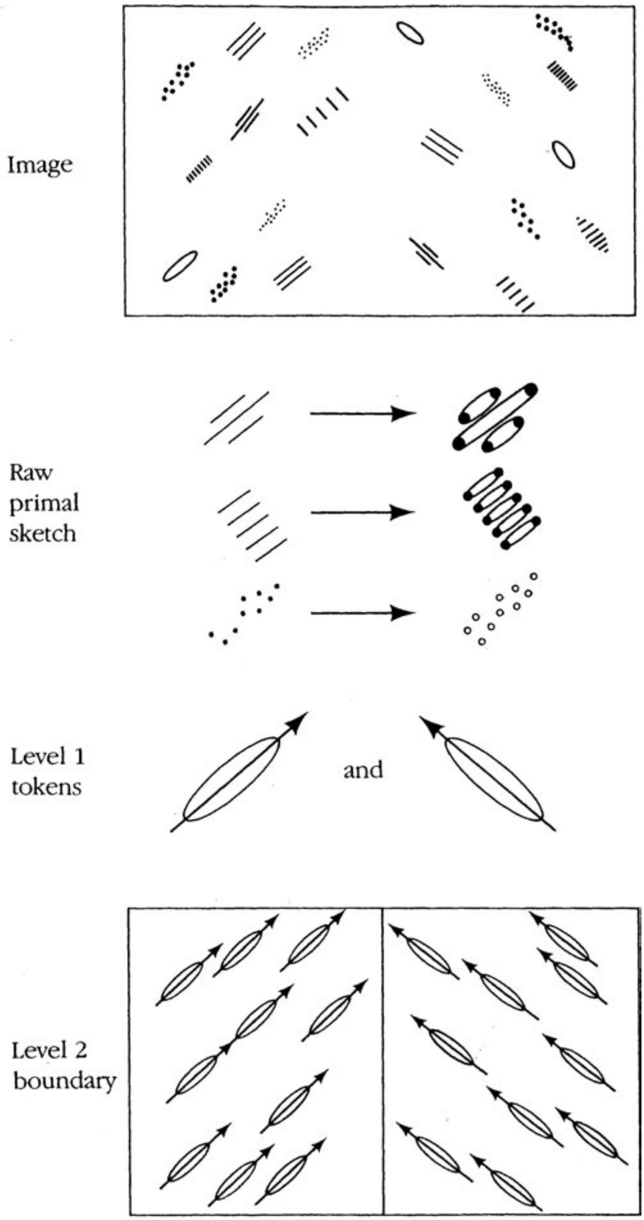


FIG. 7.4. A diagrammatic representation of the descriptions of an image at different scales which together constitute the Primal Sketch. At the lowest level, the raw Primal Sketch faithfully follows the intensity changes and also represents terminations, denoted here by filled circles. At the next level, oriented tokens are formed for the groups in the image. At the next level, the difference in orientations of the groups in the two halves of the image causes a boundary to be constructed between them. The complexity of the Primal Sketch depends upon the degree to which the image is organized at the different scales. Reprinted with permission from D. C. Marr (1982: 53)

on the vase in Figure 5.1.) And he suggested, though without offering a proof, that animal bodies can be perceived from their silhouettes by identifying the hierarchical organization of their axes (limb, thigh, foot, toes, arm, head, trunk . . .). Since each body part can be coded as a generalized cone, Binford and Blum could be united.

Blum had remarked that a mammalian body can be described in terms of one main axis (usually, horizontal), or as a major axis with four minor axes representing the limbs. A crawling human differs from a horse, and a rearing horse from a walking man, in the spatial relations between the constituent axes—such as the angle between head and body, and/or the length of the neck. Of course, one must learn to *recognize* these species differences. (Kangaroos seemed strange in eighteenth-century England not only because of the babies peeping out from their pouches, but also because of their unusual pattern of body axes.) But Marr insisted that animal bodies can be *seen* by the untutored eye, thanks to bottom-up axis-finding mechanisms evolved by the visual system.

Marr's theory of how vision progresses from 2D subjectivity to 3D objectivity was seen as hugely exciting by many psychologists. But his views on the multitudinous 3D models made less of a splash. They were too 'mathematical' to be biologically plausible.

In principle, perhaps, every 3D shape consists of one or more generalized cones. In practice, many can't be economically described in this way. Think of a piece of crumpled paper, or a velvet scarf draped carelessly over a pile of books. A finite visual system couldn't function in real time if it had to construct 'cone descriptors' for every visible object. (Shape description in the general case is still an unsolved problem, even in technological AI where one can 'cheat' as much as one likes; for two ingenious, and very different, efforts to deal with it see: Pentland 1986; Kass *et al*. 1987.)

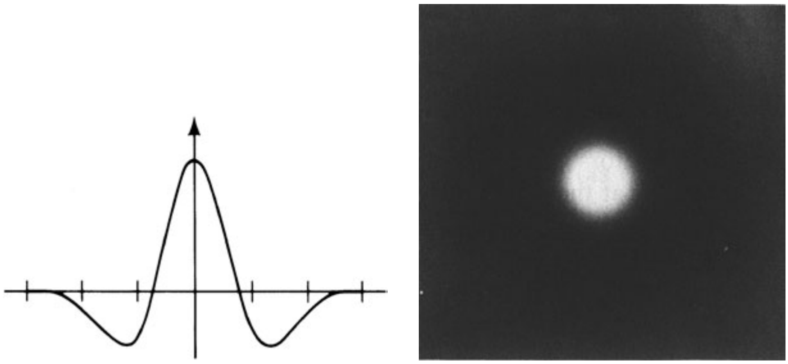
Very likely, animals don't bother with overall shape descriptions but use special-purpose cues, including IRMs (Chapter 5.ii.c), to recognize certain objects quickly. (As Gigerenzer might put it, they employ "fast and frugal heuristics" for visual recognition.) In that sense, theories of 'situated' cognition are more realistic than Marr's mathematical purism (see Section iv.a above, and 15.viii.a).

d. The fashion for Mexican hats

Marr's mathematization of vision was evident, too, in his fondness for Mexican hats—not on the head, but inside it.

"Mexican hat" is the name given to a particular mathematical curve, which looks like a sombrero: see Figure 7.5. In Marr's theory, Mexican hats (or DOG functions: see below) were used to code the intensity array when constructing the "raw" Primal Sketch. What's interesting here is that Marr, working with Ellen Hildreth, used what would previously have been thought a very *non*-psychological way of coming up with them (Marr and Hildreth 1980; Marr 1982: 53–73). That is, he relied on wholly a priori argument to give Mexican hats pride of place.

He started by assuming—perhaps wrongly: see below—that evolution, over millions of years, must have come up with the mathematically *optimal* way of coding changes in light intensity. And he argued (at length) that the best such operator must be computationally efficient, mathematically simple, insensitive to differences in edge orientation, and sensitive to differences in size. A number of mathematical operators, some of which had been used by other psychologists of vision, were defined and



(*a*) (*b*)

FIG. 7.5. (*a*) shows the Mexican hat function (defined as the second-order differential of a Gaussian) in one dimension. (*b*) shows it in two dimensions; the light intensity indicates the value of the function at each point. Adapted with permission from D. C. Marr and Hildreth (1980: 191); first reprinted in D. C. Marr (1982: 55)

compared. And the winner, on these purely a priori grounds, was what's known as the Laplacian operator.

(I'll omit the equations, even though for Marr they were of the essence. Mathematical readers won't need them, and others might not understand them. Indeed, they might not even get to see them: when I used Marr's crucial equation elsewhere, it appeared with an egregious misprint—and no erratum slip—*despite* my having forewarned the copy-editor/compositor against making that very mistake, and *despite* my having pointed it out at two stages of proofs, and on publication—Boden 1988: 64; for the correct version, see D. C. Marr 1982: 54.)

However, a pure Laplacian wouldn't do, because (lacking any size variable in its definition) it can't produce size sensitivity. Marr and Hildreth needed to define a class of filters that would *blur* the image at different spatial resolutions, by getting rid of all lower-scale detail. They chose what's known as the Gaussian distribution, which does have a size variable in it.

This had two advantages. First, although in principle it extends to infinity, in effect it's localized to a specific (circular) part of the image. So different Gaussians could be functioning in different places at one and the same time, which could account for our ability to see fine detail in only *part* of the image. Second, a Gaussian function is smooth at the edges: its value doesn't fall suddenly at the boundaries of its frequency (scale) or localized field. That meant that it was unlikely to introduce spurious intensity changes that weren't present in the original image.

The final step was to define a brand-new mathematical operator, by *combining* the Laplacian and Gaussian functions. It's this operator whose curve is the Mexican hat—and which was misprinted despite my repeated warnings. It's localized to a specific part of the image. And by giving a specific value to the size variable (inherited from the Gaussian), it would pick up light-intensity changes *at a given scale.* Since a large group of Mexican hats could be simultaneously applied to different points in the image, operating at different scales (i.e. tuned to different spatial frequencies), they could together detect a very rich variety of light-intensity changes.

All this, you'll notice, without a single reference to biology. In other words, this was the "computational" level of explanation. But Marr had long been interested in real brains, and was no less interested in real vision. His next step, however, *wasn't* to go straight to the neurophysiologists and ask them what they'd found. Instead, he used a priori argument again, asking how Mexican hats *could* be embodied in a physical mechanism (whether evolved or engineered).

At this point, the DOGs entered the picture. Marr and Hildreth proved mathematically that the Mexican hat operator is near-equivalent to a DOG function (Difference of Gaussians). This compares two Gaussians—one positive and one negative—at different scales. The best match between DOG and hat, they showed, would occur when the two Gaussians in the DOG function have space constants in the ratio of 1:1.6. In that case, the two curves are almost identical (see Figure 7.6).

It followed, they said, that Mexican hats could be near-perfectly approximated by physical mechanisms—whether natural or artificial—capable of computing DOG functions at differing scales, where the ratio between the scales is 1:1.6. Indeed, Marr suggested that specific cells of the retina and the lateral geniculate body (the second 'way station' in the visual system) were actually DOG detectors (1982: 64).

The DOGs hadn't been conjured out of nowhere. They'd long been barking in neurophysiology, as well as mathematics. As far back as the 1950s, the ON-centre and OFF-centre retinal ganglion cells, discovered twenty years before that, were found to be sensitive to changes in light intensity (see Chapter 14.iii.c). By Marr's time, various theories, and even computer models, of vision had already incorporated them. But

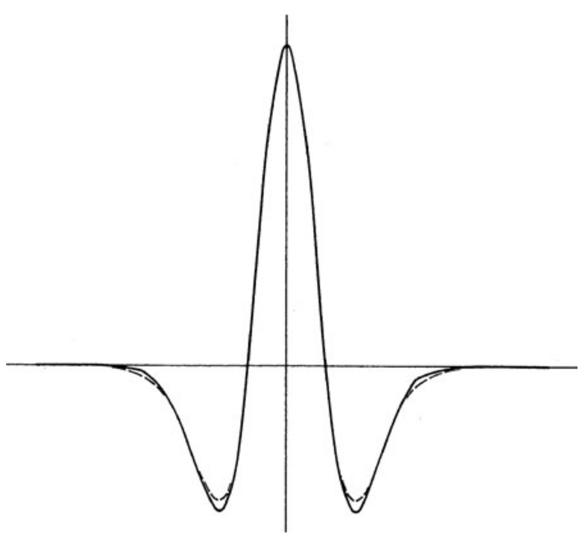


FIG. 7.6. The best possible match between the Mexican hat operator and the DOG function, shown by the continuous and dotted lines respectively. Adapted with permission from D. C. Marr and Hildreth (1980: 217); previously reprinted in D. C. Marr (1982: 63)

those psychologists had been working *from the physiology to the mathematics.* Marr, by contrast, was (notionally) working in the opposite direction. (Notionally, because—as remarked in Section iii.b—the physiology can, and in this case probably did, provide a hint that *such-and-such* mathematical operator might be the one worth thinking about.)

As with the Marr–Binford cones, Marr's purist mathematical approach was questioned here too. As Sloman (1983) pointed out, the assumption that evolution *must* have found the computationally optimal solution for a visual task was a dangerous one. Genetic algorithms in AI sometimes find the optimal solution—but not always (15.vi). In biological evolution too, satisficing may often win out over optimizing—as we saw in Section iv. Especially where speedy reaction is needed, quick-and-dirty methods (reflexes, heuristics . . .) may be more adaptive than purist ones. This applies to vision no less than problem solving.

In other words, Marr was closer to GOFAI than he was willing to admit. Quite apart from borrowing specific GOFAI ideas, such as Binford's work on generalized cylinders and Alan Mackworth's (1973) on "gradient space", he shared the early-GOFAI emphasis on *general* methods. And for Marr the mathematician, general meant not only universal but optimal. It's not clear that biological organisms always have that luxury.

There were countless other objections to his views. These concerned every level of his theory, even including the very first stage of zero-crossings. One critic, for instance, argued that the retina can't be using Mexican hats, because the spatial ratio of the centre and surround functions of the relevant cells is larger than 1:1.6 (Robson 1983). Others gave an even deeper mathematical analysis of the task of stereopsis than Marr had done (Longuet-Higgins and Prazdny 1980). And John Mayhew (1983), a researcher at Sheffield University, went so far as to say that Marr's theory of stereopsis was "almost completely wrong".

Yet within the very same breath, Mayhew acknowledged "an immense intellectual debt" to him. Even then, it was already clear that Marr had initiated what Imre Lakatos (1970) had called "a scientific research programme": a series of studies extending over many years, generated by a theory that's amended as discovery proceeds but whose central insights remain. Various aspects of his original theory have dropped by the wayside, as we've seen. But Marr's influence—his search for precisely defined (and neurophysiologically located) algorithms, computing many-levelled visual representations—is still strong today.

e. Direct opposition

The people who niggled about Mexican hats and DOG ratios, or who charged that Marr was "almost completely wrong", were nevertheless on his side. Indeed, they often presented themselves as his intellectual disciples (e.g. Mayhew and Frisby 1984). They thought he was doing psychology in the right way, even if he didn't find all the right answers. Some others didn't: namely, the Gibsonians.

The Gibsonians had moved into Psychology's House in the 1940s, long before Marr crossed the threshold (Chapter 5.ii.c). They'd been hugely influential. Gregory, for instance, had been "saturated" by their views as a student (see Chapter 6.ii.e). And although their carpets were similar to Marr's (like him, they were grounded in the detailed physics), their self-assembled theoretical furniture was very different. For one thing, they had an alternative view of what vision is *for.* For another, they didn't speak in terms of computation, or representation. When other people did so, they bridled.

As regards what vision is for, we've seen that Marr defined the task of vision as converting 2D information to 3D information. (He didn't say much about the fourth dimension, except on how 'optical flow' could aid 2D-to-3D mapping.) But isn't that an overly abstract, disembodied, view? After all, the *point* of vision, its biological function, isn't to emulate a geometer's knowledge of 3D space, or a sculptor's either. It's to navigate 3D space, to manipulate 3D objects, and in general to recognize opportunities for (3D-located) actions of various kinds: approaching, fleeing, fighting, eating, courting, grooming, mating . . . and so on. Also, and especially in social species such as *Homo sapiens* and chimpanzees, it includes the recognition of the emotional state and action-readiness of others.

These opportunities, termed "affordances", were highlighted in Gibson's later work, his "ecological" theory (1977, 1979). This excited many biologically minded psychologists because of its explicit linking of perception and (appropriate) *action.* Its general focus was on how the organism's sensory systems are adapted to the environment it lives in, and how they serve—or even prompt—the behaviour needed to survive in that environment.

Gibson wasn't a behaviourist—but he wasn't a mentalist, either. He had no time for talk of "images" on the retina, and even rejected apparently more innocent vocabulary:

> The function of the brain is not even to *organize* the sensory input or to *process* the data, in modern terminology. The perceptual systems [of all the senses, not just vision], including the nerve centers at various levels up to the brain, are ways of seeking and extracting information about the environment from the flowing array of ambient energy. (1966: 5)

He saw affordances as *properties of the environment*, even though they were defined by reference to the organism's needs. For example, a clear passage really is a pathway, a cherry really is edible, and a solid floor really is safe to walk on. In other words, Gibson wasn't focusing on the subjective meaning of object properties, but on their objective ability to be biologically relevant (useful/dangerous).

This interpretation of affordances was all of a piece with his philosophical realism (see below.) Nevertheless, it presented perception as inherently *meaningful.* Some philosophers, notably Edward Reed (1954–97), who soon became one of Gibson's leading disciples, pricked up their ears accordingly (E. Reed 1996*a*,*b*; Reed and Jones 1982). How it's possible for organisms/humans to support meaning is a fundamental philosophical problem (see Chapter 16).

When Marr turned from the brain to vision in 1972, the ecological approach was already making waves. Gibsonians were preparing for a three-week conference, to be held at the University of Minnesota's Center for Research in Human Learning in 1973 (R. Shaw and Bransford 1977). Gibson's key book appeared a few years later, in 1979 (the year he died).

By that time, however, Marr was terminally ill. In so far as he learnt from Gibson, it was the earlier work which he'd considered. For ecological psychology hadn't come out of the blue. It was grounded in Gibson's pioneering mid-century research on perception—first vision (1950), but then generalized to the other senses (1966).

In those early writings, done at Cornell, Gibson had sought to turn psychologists' attention *away from* the organism. Instead of focusing on behaviour, or even neurophysiology (for instance, the retina), he'd looked to pervasive features *of the environment.* Specifically, he'd considered the ambient light that impinges on the organism as it moves through the world.

In short, Gibson—like the maverick biologist D'Arcy Thompson before him—took the implications of *material embodiment* seriously. Much as D'Arcy Thompson had seen basic physical forces as moulding bodily forms and behavioural repertoires (Chapter 15.iii), so Gibson saw the physics of the environment, such as the details of light and sound, as moulding perception.

Even then, Gibson was interested in how vision can be *useful.* Like Broadbent, he'd worked extensively with pilots in the Second World War. He'd been particularly struck by the fact that pilots seemed to control their planes, in landing and take-off, by constant reference to the ground and the horizon. But 'ground' and 'horizon' weren't terms recognized by orthodox psychophysics. In short, visual psychologists had been missing the point:

> As I came to realise, *nothing of any practical value* was known by psychologists about the perception of motion, or of locomotion in space, or of space itself. The classical cues for depth referred to paintings or parlour stereoscopes, whereas the practical problems of military aviation had to do with takeoff and landing. (1982: 15)

In discussing the nature of the ambient light, Gibson pointed out that it contains a rich array of information about the world surrounding the organism, and the various objects within it. And that information is *structured,* being constrained by environmental "invariants" in how light is reflected from physical things.

For example, textured surfaces "expand" and "contract" in a lawful manner as we move towards and away from them (or even as we move our heads), or as *they* move, in relation to us. Similarly, the apparent size of an object changes constantly as it moves, if the movement has a constant speed; if the speed varies, the non-constancy of the change in apparent size provides information about the change in velocity. Again, the ratio of an object's height to the distance between its base and the horizon is constant—and this, said Gibson, explains the puzzling phenomenon of size-constancy. It's puzzling because, as Descartes had remarked, the size of the relevant part of the retinal image varies as the object moves near or far—yet we don't *see* the object as changing in size.

Descartes, and Helmholtz too, had explained this in terms of what we'd now call top-down (conceptual) inferences. Gibson, by contrast, was saying that information about size constancy is *already there*, in the light itself.

This approach was revolutionary: previous theories of psychophysics had concentrated on *local* features. They'd considered independent 'stimuli' or 'signals', such as the difference in light intensity between two adjacent points. Gibson was 'anti-Newtonian' in rejecting those atomistic categories.

He wasn't ready for nativism, however. At mid-century, that was still a step too far. Granted, he held that we don't need to *learn* to deal with visual information: visual systems have evolved to pick it up directly—and to act accordingly. In that sense, he was bordering on nativism. But what we've inherited is the ability to respond to information that's actually out there, not the ability to use unconscious inference to "go beyond" the information given. Where the New Look psychologists, and soon Chomsky too, constantly stressed the poverty of the stimulus, Gibson insisted on its richness.

One famous demonstration, designed by his wife Eleanor (a developmental psychologist), after a visit to the Grand Canyon, involved the "visual cliff" (E. J. Gibson and Walk 1960). This was a sudden change in depth, which babies (and kittens too) can perceive even before they can walk (see Figure 7.7). The evidence—remember: the baby can't speak yet, either—is that he/she refuses to crawl across the drop. (Of course, it's covered by a strong glass floor!) Yet no such chasm has ever been seen by the child before. Moreover, the baby doesn't just *see* the drop: he/she *does* something (stops crawling) as a result. This, said the Gibsons, is an example of an evolved connection between sensitivity to what's out there—i.e. rich visual information—and environmentally appropriate action.

When the visual cliff was first reported, it seemed that learning was *in no way* involved, and that the baby *simply would not* cross the drop. Later, it became clear that things are more complicated—and the visual cliff spawned a still-continuing experimental 'industry' accordingly.

For instance, 9-month-old babies, who can't yet crawl, may show fear (increased heart rate) if lowered over the 'deep' end of the cliff, but in 2-month-olds the heart rate decreases, suggesting raised interest (Campos *et al.* 1970, 1978). Recognition of the drop, then, appears earlier than fear of it. There's evidence that the degree of wariness depends on the baby's past experience of locomotion and of heights (Bertenthal *et al.* 1984; Campos *et al.* 1992), but counter-evidence that it depends on maturation (Richards and Rader 1981). Moreover, the mother may be able to coax the baby across (her voice being even more persuasive than her smile), but if she puts on a frightened expression then the baby won't move—presumably, because it picks up her fear (Sorce

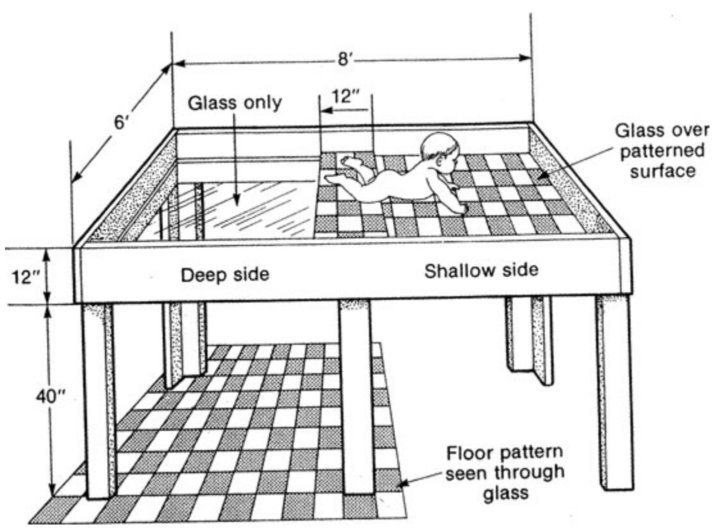


FIG. 7.7. Gibson's visual cliff. If the baby turns round and crawls towards the 'cliff', it stops at the edge of it—although the invisible glass floor continues. Reprinted with permission from Dworetzky (1981: 187)

et al. 1985; Campos *et al.* 2003; Vaish and Striano 2004). And mother-enticed babies look at the mother continually while crossing the cliff: only gradually do they become able to cross 'autonomously'.

In short: vision, locomotion, emotion, and social behaviour develop in subtle interaction with each other (for a recent review, see Campos *et al.* 2003). In Jean Piaget's terminology, they develop "epigenetically", thanks to an interplay between evolution (nature) and experience (nurture).

All that was for the future, however. Most psychologists still took it for granted that 'nature versus nurture' is a sensible opposition—see Section vi.g, below. As for Gibson, he was less concerned with how vision develops than with how it works. (Notice the then typical assumption that the latter can be understood without considering the former.) And he stressed automatic, environment-driven, mechanisms.

Marr did that too, of course. So what was the disagreement between them?

f. Let battle commence!

Marr had some kind words to say about Gibson—moderated by an important criticism:

> [Gibson asked] How does one obtain constant perceptions in everyday life on the basis of continually changing sensations? This is exactly the right question, showing that Gibson correctly regarded the problem of perception as that of recovering from sensory information "valid" properties of the external world. (Marr 1982: 24)

> In perception, perhaps the nearest anyone came to the level of computational theory was Gibson (1966). However, although some aspects of his thinking were on the right lines, he did not understand properly what information processing was, which led him to seriously underestimate the complexity of the information-processing problems involved in vision and the consequent subtlety that is necessary in approaching them. (p. 29)

Reading those words cold, one might infer that Gibson was a computational psychologist in the usual sense of the term, but that his theory of algorithms and internal representations left something to be desired. One couldn't be more wrong. The word "computational", here, was intended in the (atypical) sense defined in Section iii.b. Broadly, Marr meant only that Gibson had taken the constraints of physics seriously in trying to say what vision does. As for Gibson's theory of algorithms and representations—there wasn't any.

Gibson was adamant, right from the start, that perception is *direct.* Indeed, he devoted many pages to philosophical argument (well, he was a maverick!) in favour of direct realism. He had no time for Descartes's representational theory of perception, nor for Kant's hidden world of noumena either (Chapter 2.vi.a). Indeed, his commitment to realism led him to criticize Gregory for concentrating on illusions—by definition, *unrealistic.* (Hence his dismissive remark about "parlour stereoscopes".) According to Gibson, the information in the ambient light is there to be discovered, and is effortlessly (though actively) "picked up" by the visual system. It's neither altered (processed) nor interpreted by the eyes and brain. As he often put it (e.g. 1979: 54), we don't see *light*, we see *objects.*

For Marr, by contrast, we don't see objects until after 'seeing' (processing) light—and many other things besides, namely the representations making up the Primal and $2\frac{1}{2}$D

Sketches. For Gregory too, seen objects are the result of unconscious inference, or interpretation. So when the New Look psychologists and (later) Marr started talking about internal representations, and the computational processes involved in constructing and interpreting them, Gibsonians rejected the whole mentalistic story. *There are no representations*, they said—and no computations, either.

The result was a running battle between Gibsonians and computational psychologists in general. Sometimes, Gibson won new converts. Neisser, for instance, switched camps in the early 1970s. He abandoned his "analysis by synthesis" approach to perception, replacing it by a more ecological, realist, theory—developed in a book he dedicated to Gibson (see Chapter 6.v.b). But that was pre-Marr. After Marr, the stakes were upped. For his theory, which appealed to some similar (physics-based) insights, had enthused people at least as much as Gibson's had done. Moreover, it had helped them to realize how very difficult it is (that is: how much information processing actually has to be done) to see an edge, never mind an object.

One computationalist attack on Gibson was launched by Fodor and Pylyshyn (1981). They were less interested in the specifically visual issues than in his concept of affordances—which they castigated as "a dead end" (1981: 192). The place to which his pathway was supposedly leading was an understanding of meaningful behaviour in terms of environmental properties (see above). But they despaired of his philosophical compass.

In particular, they complained that he offered no principled way of deciding *what counts* as an affordance, nor of *how* affordances can be grasped. Mental states and processes (beliefs, goals, inferences . . .), they said, are necessary to understand how affordances are exploited—and even how they're recognized in the first place. Some animals may have a few automatic links between perception and action (e.g. IRMs). But intentional behaviour can be explained only in terms of computations over representations (Chapter 16.iv.c). Admittedly, the philosophy of intentionality was a mare's nest. But ecological psychology couldn't help.

Almost immediately, four of Gibson's closest colleagues rose to the bait (Turvey *et al.* 1981). Even if high-level human cognition involves mental states and inferences, they said, perception doesn't. It can be explained by "natural laws", which focus on observable organism–environment regularities rather than hypothetical mechanisms inside animals' heads. And indeed, many animals had already yielded to the Gibsonian treatment. Ecological psychologists had studied a range of species, showing how they found different affordances (in effect, different environments) in the real world. But it was the objective properties of that world (for example, optics), together with the bodily potential of the animals concerned (their "effectivities"), which accounted for their behaviour. Meaning, in the sense of adaptiveness, can be given a naturalistic explanation. Human-level semantic meaning might be another matter—but that's not what Gibson had been talking about.

Another explosive skirmish was staged in the interdisciplinary pages of *BBS*. Soon after founding the journal (while Marr was still fighting for life), Harnad circulated a target article by Marr's MIT colleague Shimon Ullman. The title announced the challenge: 'Against Direct Perception'.

Ullman (1980) expressed the mystification common to all computational psychologists: that the Gibsonians seemed to believe in some unexplained, quasi-magical process of "information pickup" (Gibson 1966, ch. 13). Gibson had used the metaphor of a radio, which doesn't interpret the radio waves but picks them up directly and "resonates" with them. A radio isn't magical, to be sure. But it's far less complex than even 'low-level' vision. If one looks hard at the neurophysiology, one finds ample evidence that specific parts of the eye/brain do (compute) specific things. Moreover, the engineering task of building a visual system had turned out to be different in kind from—and vastly more difficult than—building a radio. In short, said Ullman, whether or not one accepts the details of Marr's theory, one must allow that vision is far from "direct".

Among the many peer commentaries—*Marr* v. *Gibson* was a fight that everyone wanted to join—was a judicious piece by Geoffrey Hinton (1980). Hinton agreed with Ullman that there's no such thing as a-procedural information pick-up. Low-level vision is indeed automatic—but that means that it invariably happens, and that it can't be influenced by conscious effort or other top-down factors. It doesn't mean that it 'just happens', as if by magic. A lot of information processing has to go on for us to see objects, or even edges and surfaces. Anyone who doubts this, he said, should try getting a computer to distinguish those things.

Gibson's mistake, Hinton continued, was to think that "computation" must mean either conscious deliberation (as the Wittgensteinians were saying) or formal–symbolic processing of the type favoured in GOFAI. Now, in 1980, we had a better sense of various types of bottom-up visual processing—of which Marr's was one example and Hinton's another. (He might have added, but didn't, that "computation" is a word whose meaning isn't fixed but gets richer as AI progresses: 16.ix.) In short, one could be a computationalist without falling into the traps rightly feared by Gibsonians.

Hinton's vision research had been strongly influenced by Sloman, when they'd collaborated at the University of Sussex (12.v.h). And Sloman (1989) soon sought to bring Gibson's *later* ideas inside the computational camp. His own work on vision, like his later discussions of personal psychology, had always been done in an 'architectural' spirit (see 10.iv.b). In other words, he was concerned with the role that sight, our most well-developed sense, plays in the mind as a whole. In outlining the potential for "a Gibsonian computational theory of vision", he argued that affordances are hugely important—and entirely consistent with a computational approach. In particular, he pointed out that there might be learning mechanisms which could link specific visual cues with all manner of associations, including widely diverse opportunities for action.

Gibson and Marr died within a year of each other, but their spirits still live. Researchers in what's sometimes called nouvelle AI (that is, situated robotics and enactive perception) favour Gibson, as do those neuroscientists and philosophers who stress "embodiment" as a crucial ground of our psychological abilities. Michael Turvey and Peter Kugler, at the University of Connecticut, have been especially influential in discussing the nature—and emergence—of intentional movement and perceptual "measurement" (Kugler and Turvey 1987, 1988; Kugler *et al.* 1990; Turvey and Shaw 1995). Like their late mentor Gibson, they stress the physical properties of interacting dynamical systems—so wouldn't want to be called "computational" psychologists.

In short, the battle between ecological realism and representationalism continues today. That's so not only in the psychology of vision but also in A-Life (Chapter 15.vii–viii), neuroscience (14.viii.a–c), and philosophy (14.viii.d and 16).

But perhaps, as Sloman (among others) had said, one shouldn't be engaging in a *battle*. A recent target article in *BBS* (again!) argues that both these flowers can blossom in the same flowerbed. Joel Norman (2002) tries to "reconcile" the Marrian and Gibsonian perspectives, largely by relating them to neuroscientific discoveries that happened after both men's deaths. (I'm using "Marrian" here as code for "constructivist": Marr himself isn't cited by Norman.)

The peer commentaries—including one from Neisser, who switched from constructivist to ecological psychology in the early 1970s but is now more catholic in his tastes (Neisser 1994)—show that there's been significant advance. Yes, there are still people who regard one approach as more significant, more interesting, than the other. (Neisser is one: see Chapter 6.v.b and Neisser 1997.) But there's a common realization that vision can't be fully captured by either.

7.vi. Nativism and its Vicissitudes

The fourth behaviourist assumption identified in Chapter 5.i.a was that one must look *outside* the organism to understand why it does what it does. In particular, nativist psychologies are wrong-headed: there is no innate knowledge, and there are no inborn psychological powers (apart from a few very simple reflexes). That's largely why the ethologists (5. ii.c) had been relegated to the back corridors of Psychology's House.

The earliest cognitive science made no difference. Neither informational psychology nor the New Look gave any hint of nativism—and nor did 1950s Chomsky. *Syntactic Structures* put many cognitivist cats among the behaviourist pigeons: remember Jenkins's rueful "Everybody in the meeting jumped on me." But nativism wasn't one of them. Chomsky was still keeping quiet about that. Even when he excoriated Skinner in 1959, he didn't use nativism as part of his armoury (Chapter 9.vi.e and vii.b).

In the mid-1960s, however, Chomsky came out of the closet—and with a bang, not a whimper. In a flurry of books within only four years (1964, 1965, 1966, 1968), he made a scandalous psychological claim, which put nativism at the very heart of his linguistics. An adequate theory of language, he now declared, "incorporates an account of linguistic universals, and *it attributes tacit knowledge of these universals to the child*" (1965: 27; italics added).

Many psychologists—and philosophers and linguists, too—baulked at this. Some attacked his a priori arguments for nativism (innate ideas), while others criticized the empirical evidence, or lack of it, for linguistic universals (Chapter 9.vii.c–d). The fact that many major thinkers in centuries past would have been less shocked (9.ii–iv) didn't save his theory, in these critics' eyes.

However, early work in developmental linguistics, and unsuccessful attempts to teach language to apes, persuaded some others that he was right. This increased people's interest in the inherited capacities of different species, whether linguistic or not. Over the last twenty-five years of the century, then, the ethologists moved into more capacious quarters within Psychology's House. Moreover, much of the extra space was allotted to their new close cousins, the evolutionary psychologists (see 8.iv–v).

The Piagetians, by that time, needed expanded house room too—and this made for complications. For Jean Piaget, "Nativism: *yes* or *no*?" had never been an acceptable question (5.ii.c). Eventually, that Piagetian view prevailed. By the turn of the millennium, nativism *as commonly understood* was almost as discredited as it had been in behaviourist days.

Nevertheless, our biological inheritance was being acknowledged more strongly than ever before. The fourth tenet had been rejected—but with an interesting twist.

These changes weren't due to cognitive scientists alone, for some of the crucial evidence described below (especially in subsections b–c) was garnered by people with very different concerns. An ethologist who chose to live with a band of gorillas, or a psychologist who tried to teach language to chimpanzees, might have no interest whatever in computational ideas. But others, who did, used their findings as evidence in theorizing about the types of information processing going on in infant and adult human minds.

In short, nativism could have been resuscitated without any conceptual impetus from AI, or from Chomsky either. As a matter of historical fact, however, it owed a great deal to both.

a. The words of Adam and Eve

Those psychologists who were willing to take Chomsky's nativism seriously looked first to the development of language in children. Here, the social psychologist Roger Brown (1925–97) had a head start. For as a denizen of Bruner's Center, he'd known of Chomsky's heresy even before it was published.

In the early to mid-1950s he'd studied the development of language (R. Brown 1958), in connection with the Whorfian hypothesis (Chapter 9.iv.c). But that work had focused on words/concepts (not grammar), and had been done in the usual way: many different children, of various ages, were studied simultaneously, and their behaviour was compared. A few years later, he pioneered a very different methodology: developmental psycholinguistics as it's understood today.

In 1962 Brown began a long-running and scrupulously detailed study of the development of language—initially in just two children, "Adam" and "Eve", but "Sarah" soon joined them. This work was to be hugely influential. Linguistic development became a thriving speciality within psychology, the techniques becoming more discriminating as the years passed. Indeed, by the time that Sarah was included in the pioneering study, the investigators had already learnt that they should record certain subtle syntactic/morphemic distinctions which (with Adam and Eve) they'd formerly ignored.

Brown's team, which included the young Ursula Bellugi (who later did influential studies of American Sign Language and Williams Syndrome), discovered many intriguing facts about how the children's utterances became longer and more grammatically complex. Sometimes, they got "a shock" (R. Brown 1973: 284), when their naturalistic observations *didn't* match expectations based on late 1950s experiments exploring the grammatical capacities of 4- to 7-year-old children. (They managed to 'explain away' almost all of these surprises: pp. 284–93.)

For instance, they noted when two-word utterances first appear (at between 20 and 28 months) in the "Stage I" child, and how some words come to be used as hooks on which to hang many others (*milk all-gone*, *Daddy all-gone* . . .). And they studied the development of particular grammatical constructions, such as plurals and the past tense. With respect to the past tense, Brown's team found that the irregular forms (ate, went, came, did) preceded the regular ones (*walked*, *lifted*, *laughed*)—pp. 311–12. (For some other verb changes, such as third-person inflections, the regular preceded the irregular.) Some irregular pasts (*came*, *fell*, *broke*, *went*, *sat*) occurred very early indeed, and irregular pasts were always more frequent than regulars in the infants' speech, as they are in the speech of adults (p. 260).

Brown's general conclusion was that there are many structural regularities in the development of language, and that these don't depend primarily on the input (the mother's/carers' speech). The order in which specific grammatical structures (plurals, definite descriptions, possessives . . .) arise doesn't match the mother's speech: even her 'baby-talk' includes all these features from the start. It must, therefore, depend on sources within the child's own mind. In other words, there was seemingly some evidence for what Chomsky called a "Language Acquisition Device" providing innate knowledge of, or preparedness for, grammatical rules.

The Chomskyans soon claimed additional evidence for their case. Psycholinguists found that when the child comes to possess a large number of regular forms, the (correct) irregulars disappear—or anyway, occur less reliably—for a while. So an infant who'd previously said *went* appropriately would now say *goed*. Combined with the fact that young children will also say *mouses* and *sheeps*, this seemed to be strong evidence for the nativist position. (The past tense, in particular, was put under a developmental microscope. The details, excruciatingly boring in themselves, would suddenly become hugely interesting in the early 1980s: Chapter 12.vi.e.) Chomskyans saw such over-regularizations as proof that their guru was right. For, they said, these quasi-plurals and quasi-pasts weren't *learnt*: the child never heard its carers saying "goed" or "sheeps". They could have been generated only by applying GOFAI-type rules, such as *add '-ed' to the verb stem*. Indeed, any regularities that didn't reflect the speech of the carers, including the two-word to three-word progression, seemed to imply some inborn, language-specific, 'program'.

The Piagetians weren't convinced. They argued that Chomsky's nativism was over-simple, that language develops epigenetically from both active experience and biological maturation (Piatelli-Palmarini 1980). But there was much mutual misunderstanding, not least because Piaget's own writings were notoriously vague. Conrad Waddington, the biologist from whom Piaget had borrowed the concept of epigenesis, was unknown to psychologists (and regarded as a maverick by other biologists: see 15.iii.b). Most non-Piagetians of the 1960s–1970s saw epigenesis as even more mysterious than Chomsky's nativism (see subsection g, below).

Many 1970s psychologists, then, believed that Chomsky had been vindicated—or, at least, that there was strong evidence in his favour. Part of that evidence was the success of various GOFAI programs in parsing natural language (9.xi.b). Clearly, formal rules *could* be used to interpret grammatical structure. And phenomena like the infant's over-regular *goed* seemed to show that this is what goes on in human heads.

Twenty years after Chomsky's nativism shocked the psychological world, another scandal would arise—thanks to connectionist AI. David Rumelhart and James McClelland (1986) argued that *mouses* and *goed* might result from learnt statistics, not innate rules (see Chapter 12.vi.e). In the early 1990s, others showed that a connectionist system might be able to "start small" (compare: two-word utterances before three-word ones), enabling it eventually to learn a complex structure *without* being primed on what to look for (12.viii.c–d). And, also in the 1990s, developmental neuroscientists would give some firm empirical content to the notion of epigenesis (subsection i below, and 14.ix.c).

But all that was in the future. Meanwhile, some psychologists decided to focus on the second half of Chomsky's nativism. This was his claim that *Homo sapiens* is the *only* species to possess language.

b. Some surprises from ethology

Psychologists had two ways of confronting Chomsky's humanism. They could seek evidence that non-human animals possess language naturally, or they could try to teach it to them in the laboratory.

Informal studies of both kinds had been done for centuries, and people had also investigated feral children (9.ii.c). What was different now (from the mid-1960s on) was that Chomsky's formal theory was being used—by some people, anyway—as a criterion of language. In a word: no grammar, no language. So the central question was no longer "Do animals communicate?", nor even "Do they develop/learn vocabulary?" Rather, it was "Can they develop/learn syntax?" I say "develop/learn" because Chomsky had insisted not only that non-human animals *don't* develop language naturally, but also that they *can't* acquire it artificially (9.vii.c–d). As he put it:

> [All] normal humans acquire language, whereas acquisition of *even its barest rudiments* is quite beyond the capacities of an otherwise intelligent ape. (Chomsky 1968: 59; italics added)

Whether animals in the wild develop language naturally was a matter for the ethologists. Previously unfashionable, in the last third of the century they became highly visible. As late as 1971, they were still being sidelined. Pribram remarked:

> [At] the International Congress of Psychology in Tokyo [in 1971], comparative psychologists held a symposium on the state of their field. *The tone was rather gloomy*. They had not heard of the chimpanzee's challenge to the uniqueness of man. Washoe, Sarah, and Lana—and a whole new generation of scientists and apes—are attempting to spell the end of Cartesian dualism [i.e. the split between animals and man]. (Rumbaugh 1977, p. xvi; italics added)

Soon afterwards, the gloom was dispelled. The ethologists attracted attention from professional psychologists and philosophers—and from the media-influenced public (including 'animal rights' movements) around the world.

Some 1960s ethologists were more interested in *animals* than *theory*. For example, in 1960 Jane Goodall—then 26 years old (having paid her boat passage to Africa in 1957 with saved waitressing tips) and working as secretary to the anthropologist Louis Leakey—went to live with a group of chimpanzees by Lake Tanganyika. She revolutionized people's view of these creatures, for she discovered that their social skills were hugely more diverse and complex than had been thought. And, by implication, so were the cognitive skills required to generate them.

Her Ph.D. was published in 1968, by which time she'd already returned to Africa: a fuller account appeared some twenty years later (Goodall 1986). When her work, and comparable 'living research' with gorillas by Dian Fossey (1983), entered the public arena the opposition to vivisection on primates soared accordingly. Some countries tightened their rules as a result (see 2.ii.f).

But Goodall and Fossey were special. Most ethologists studied their chosen species either by relatively brief trips to the wild, or by observing them in quasi-natural captivity. In the latter case, they typically did experiments to discover the bounds of their behavioural capacities, much as Konrad Lorenz had done years before with his greylag goslings. And with the rise of cognitive psychology, some started asking questions about animals' *cognitive* capacities too. Specific computational processes and models were rarely mentioned (but see Boden 1983). Nevertheless, animals were seen as information-processing organisms, whose psychology—in some cases, anyway—was broadly comparable to ours.

By the mid-1970s, Donald Griffin (1915–2003) had amassed enough evidence to publish a book called *Animal Awareness* (1976)—a phrase which, even then, still had the capacity to shock. Strictly, and despite the reference to 'Mental Experience' in the subtitle, he was talking about cognition, not consciousness. (He remedied that later: Griffin 1992.) In other words, he was asking whether intentional concepts in general can be ascribed to some animals, and if so what the limits of their intentional capacities are.

The self-styled "cognitive ethologists" (Griffin 1978) went far beyond IRMs. Seeking to get "inside the mind of another species" (the subtitle of Cheney and Seyfarth 1990), they looked for evidence of language—and representation, intention, planning, imitation . . . all distinctly non-behaviourist concepts—in various species.

A cautionary 'aside': Here, let's ignore Thomas Nagel's (1974) famous argument that we simply can't get inside the mind of a non-human animal, if this means *knowing what it's like to be* that animal. And let's ignore the question of whether any creature capable of intentionality ("aboutness") must also have consciousness: after all, it's possible to discuss the former while ignoring the latter (see, for instance, Searle 1980). Both intentionality and consciousness are philosophical minefields. Ascribing either to animals is doubly problematic, but that's what the cognitive ethologists were prepared to do. Their work often contained some philosophical argument, and as the years passed the philosophers discussed cognitive ethology in their turn. (Some early interdisciplinary discussions are in the *BBS* peer commentary on Griffin 1978 and Premack and Woodruff 1978; later examples include Heyes and Dickinson 1990, 1995; Dennett 1991, chs. 7, 14; 1996; and Allen and Bekoff 1997.)—Of course, we shan't be able to ignore this philosophical can of worms for ever. We'll open it later (in Chapters 12.x.f; 14.viii.d, ix.b, and x–xi; and 16 *passim*).

The cognitive ethologists found many surprises, such as the alarm calls of vervet monkeys. These were observed in 1977–8 by the husband-and-wife team Robert Seyfarth and Dorothy Cheney, then at UCLA (now, at the University of Pennsylvania). They insisted that these warning signals have *semantic*, not just emotional, content—referring (*sic*) specifically to snakes, or to leopards, or to eagles (Seyfarth *et al.* 1980). They said this not only because of patterns of co-occurrence (snake calls happen only in the presence of snakes), but also on the basis of the monkeys' behaviour. When the calls were recorded and played back in the absence of any predators, the vervets did the 'appropriate' thing. For instance, they took to the trees to escape from a snake on the ground—who, of course, wasn't there at all.

Moreover, Cheney and Seyfarth (1990) discovered that vervet babies have to learn the alarm calls, gradually restricting the scope of reference. At first, they'd produce the eagle signal when any bird was present; eventually, it would occur only for eagles (alias predators). Towards the end of the century, similarly reference-restricted warning cries were found in other species. Even more surprisingly, the ethologists sometimes observed 'modifier calls' which, when produced in combination with some other call, apparently altered its meaning. In addition, previously unsuspected forms of communication were discovered in dolphins and whales. (Many of these findings fascinated the general public: whale "songs" even entered the pop charts.)

Predictably, these phenomena were often described—even by ethologists—as "the rudiments of language". However, the ethologists hadn't found *grammar*.

Most animal communications are discrete signals, like the cries of the vervets. To be sure, birdsong often consists of long strings of sounds. But there's no evidence of compositional structure or 'creative' generativity. Mature nightingales can sing many different songs, some of impressive length and complexity. But this isn't *generative* complexity. The nightingales are more like a pianola with a dozen different (sometimes cut-and-pasted) rolls in it than a person, or even a Stage 2 child, coming up with a new sentence/word-string every few minutes. Moreover, although greater complexity in an individual bird's song correlates with greater success in attracting mates, there's no evidence of semantic content, nor any hint that the *order* of the cries in the 'combination signals' makes any difference to their functionality.

Thirty years of this type of ethological research have shown that the cognitive capacities of many animal species are far greater than psychologists had previously thought. That's true even if one believes that *representation* isn't the right way to explain them (see 14.vii and 15.vii–viii). Indeed, even crickets and frogs possess information-processing mechanisms of surprising power, inflexible though they may be (14.iv and vii, and 15.vii). But the natural linguistic (i.e. syntactic) endowment of non-human animals is, apparently, nil.

c. From Noam to Nim

Even before Goodall first ventured into Africa, an entire experimental industry had arisen alongside human psycholinguistics: the increasingly ingenious effort to teach language to non-human animals in captivity. (Mostly primates, but eventually also parrots: Pepperberg 2000.) For, given Chomsky's claim that even the "barest rudiments" of language are beyond the capacities of apes, the challenge was clear: was there anyone out there who could coax chimpanzees, our closest cousins, to speak?

At first, it seemed—on biological grounds—that the answer must be "No!" For Eric Lenneberg (1921–75), a biologist admirer of Chomsky, had shown in intriguing detail not only that human bodies are nicely prefigured for the spoken word, but that the chimpanzee's oral cavity and musculature make speech (i.e. pronouncing phonemes) impossible. The very *anatomy* of language, it seemed, is species-specific (Lenneberg 1964, 1967).

However, Elizabeth McCall had recently argued in her MA dissertation that American Sign Language (ASL), which uses finger movements, may be a 'real' language—that is, one with a grammar (McCall 1965). Spontaneous signing by the deaf had been the subject of speculation for centuries: Descartes was just one of many who'd discussed it (Chapters 2.iii.c and 9.ii–iv). But again, Chomsky's work had sharpened the relevant questions. McCall's suggestion was later taken up by Bellugi (with Edward Klima), who found that ASL is indeed a syntactically structured language (Klima and Bellugi 1979; Bellugi and Studdert-Kennedy 1980).

So maybe all the past attempts to coax language from apes had failed simply because they can't *speak*? Even Viki, a chimp whose lips and tongue had been constantly 'shaped' by the fingers of her human foster parents in the 1940s, had learnt to produce only four word-ish—very '-ish'—sounds (K. J. Hayes and Hayes 1951). However, chimps have fingers nearly as nimble as ours. Perhaps one should try to teach them sign language, instead?

The first psychologists to attempt this were the husband-and-wife team of R. Allen and Beatrice Gardner, at the University of Nevada (1969, 1971, 1974). Like Winthrop and Louise Kellogg a quarter-century earlier (Kellogg 1933), they took an infant chimp into their house to be raised much as a human child. Unlike the Kelloggs, however, they used not speech but ASL (the version in which each handmade sign stands for its meaning 'directly', rather than spelling out the English word). They used all the teaching methods they could think of, including showing their own ASL signs to the animal; gradually shaping its behaviour by rewards (operant conditioning); moulding its hands into the required position; and simply 'chatting' to it while playing, feeding, and so on.

Their protégée Washoe (named for Washoe county, Nevada) eventually learnt to recognize and produce about 240 ASL signs. (More accurately, her trainers *reported* that she did so: see below.) Most of these stood for *classes* of object, being correctly applied (for example) to a new bird, never seen before. On at least 300 occasions, Washoe produced novel combinations that 'made sense'. (The same caveat applies here: see below.) Some of the sign combinations that she was specifically taught were five signs long.

The Gardners were persuaded that Washoe's performance matched that of a 2-year-old child, and they set out to persuade the world. By the early 1970s their chimp-child was famous, featured in newspapers and TV programmes in many different countries. (*Hello!* magazine hadn't yet been founded: perhaps its camera bulbs would have been flashing too.)

But had Washoe, in learning to sign, learnt a sign *language*? The Gardners said "Yes!" They pointed out that Washoe sometimes came up with new combinations of signs, thus—so they claimed—paralleling the "creativity" of language which Chomsky had emphasized so strongly (9.iv.f and vii.b). One famous example was her signing *water bird* on first seeing a swan. The awkward question, however, was whether she was signing *water-bird*, or whether she was merely signing *water*, quickly followed by *bird*. After all, both the water and the bird were clearly visible. And what if she'd happened to sign *bird* before *water*? For native speakers of English, "bird-water" isn't nearly so persuasive a combination (though it might be interpreted by enthusiasts as the sentence *There's a bird on the water*).

It turned out on closer examination that Washoe's 'two-word utterances' didn't respect word order, whereas Stage 1 children do. Although the two-word child can't yet say either "Dog bites cat" or "Cat bites dog", she can distinguish these situations by saying *dog bite* or *bite dog*, respectively (R. Brown 1970). It doesn't follow that Washoe couldn't 'tell the difference' between the two situations. But she couldn't *tell* that difference. In English, word order is an important grammatical cue—one of the "barest rudiments" of the language. There was no evidence that Washoe could deal with it.

Some years later, in 1982, the media reported worldwide that Washoe, by then with Roger Fouts at the University of Oklahoma, had taught her adopted son Loulis to sign. Or, less tendentiously, that—being raised in her company—he'd learnt to sign spontaneously (Fouts *et al.* 1982). However, Loulis didn't respect word order either.

Washoe's contemporary Sarah, though less suitable as chat-show material, also became famous. Unlike Washoe, she didn't 'make remarks' spontaneously, nor use a 'human' method of communication. Indeed, it wasn't clear that she was *communicating* at all. Rather, she responded to the tasks/questions set to her by her trainer David Premack (1925–), aided by his wife, Ann, by moving plastic tokens, or 'symbols', on a magnetized board. (She'd been taught by means of operant conditioning.)

Sarah's 'linguistic' accomplishments, it appeared, were far greater than Washoe's. Indeed, they were staggering. According to Premack (1971), they included *yes–no* interrogatives, *wh*-interrogatives, negatives, word compounds, plurals, quantifiers . . . and more, many more.

For instance, she was given tokens for *same* and *different*, and trained to pick the right one when presented with two objects from the same or different classes. Next, a token for the question mark, *?*, was placed between two objects, and Sarah was trained to replace it with one—the right one—of the *same/different* tokens. Premack interpreted this as a simple *wh*-question, namely, "*What* is the relation between the two objects?" Many other tasks, more complicated than this one, were also mastered by Sarah. Seemingly, she'd learnt a great deal more than the barest rudiments of language.

But had she, really? Psychologists sympathetic to Chomsky suggested, in effect, that Sarah's superficially impressive behaviour was Newtonian at heart (e.g. R. Brown 1973: 32–51). With respect to the more complicated tasks, her 'successes' might have been due to chains of conditioned reflexes, as seen in pigeons playing ping-pong (Chapter 5.iii.a, above). In this context, it was significant that her success rate, about 80 percent, *didn't* vary with what people see as the 'difficulty' of the task. Her performance, one might say, was too good to be true.

Moreover, Sarah's behaviour was elicited in very narrowly defined situations. Brown (1973: 48) compared her grasp of "Tokenese" with his own of Japanese. He'd received some lessons ingeniously "programmed . . . in an almost Skinnerian way", and had learnt to reply appropriately to sentences like those in the training set. He'd thought he was doing well. Faced with 'real' Japanese, however, he was virtually helpless. He could neither interpret nor generate sentences he hadn't come across before (and the problem wasn't merely his lack of vocabulary).

Finally, many of Sarah's 'successes' could have been due to unconscious signalling on the part of the human experimenters. Robert Rosenthal had recently shown that rats and horses could learn to respond to such signals (R. Rosenthal and Fode 1963; Rosenthal 1964; Pfungst 1911/1965, introd.). In some cases, the signals were so slight (e.g. a postural change caused by increased muscular tension) that they were invisible *even to other humans highly motivated to observe them*. If rats and horses could do this, then presumably people could do it too. It followed, for instance, that any experiment on telepathy/ESP where *someone perceptible by the subject* already knows the answer is worthless. And the implications didn't stop there. As Rosenthal wryly put it:

> That many experimenters over the years may have fulfilled their experimental prophecies by unintentionally communicating information to their subjects may be a disquieting proposition. (Rosenthal, in Pfungst 1911/1965, p. xxii)

For chimp-language research, Rosenthal's findings were potential dynamite. Over the years, the trainers' experimental methods were altered so as to minimize experimenter bias. But Herbert Terrace (1979*a*,*b*, 1981), for instance, had the honesty to admit that films of his ape subject—a cousin of Washoe's, mischievously named Nim Chimpsky as the experiment got under way—showed that the animal was often unknowingly cued by the psychologists working with him. Unlike Sarah (but like Washoe), Nim sometimes signed spontaneously. Nevertheless, Terrace concluded that his stalwart efforts to train Nim to 'talk' had been *unsuccessful* (Terrace 1979*a*,*b*, 1981; Terrace *et al*. 1979). The sarcastic baptism had backfired: Chimpsky had vindicated Chomsky.

Besides the type of experimenter bias described by Rosenthal, there's the type of bias highlighted by the New Look psychologists. One sees what one expects to see—and what one *wants* to see. This explains the highly sceptical remarks of the only person on the Washoe team who, being profoundly deaf, was raised with ASL as their native language:

> Every time the chimp made a sign, we were supposed to write it down in the log . . . They were always complaining because my log didn't show enough signs. All the hearing people turned in logs with long lists of signs. They always saw more signs than I did . . . [They] were logging every movement the chimp made as a sign . . . Sometimes [they'd] say, "Oh, amazing, look at that, it's exactly like the ASL sign for *give*!" It wasn't. (quoted in Pinker 1994: 338)

So where I said (above) that Washoe learnt 240 signs and produced 300 sensible combinations, it would have been more accurate to say that she was *perceived* as doing so by her trainers. Just how overgenerous their interpretations were, it's difficult to know. But that they *were* overgenerous is sure.

Other efforts to refute Chomsky's view on apes and language included two conducted at Georgia State University. These were Duane Rumbaugh's (1977) work with Lana, and its successor: Sue Savage-Rumbaugh's still-continuing training of Kanzi (1991; Savage-Rumbaugh and Lewin 1994; Savage-Rumbaugh *et al*. 1998).

By the late 1990s, the Georgia team were confidently claiming that their long-time pupil had acquired linguistic and cognitive skills equal to those of a $2\frac{1}{2}$-year-old child. Their work bristled with intriguing experiments, in which their animals—unlike Sarah—used 'language' to help further their own purposes.

For example, one chimp learnt to use abstract 'symbols' to ask another chimp for a specific tool. (The second couldn't see what the first was doing, so couldn't work out for itself which tool was needed at the time.) And in January 2003 the *New Scientist* reported that Kanzi had made up four 'words' for himself. Apparently, he was emitting sounds meaning *banana*, *grapes*, *juice*, and *yes*—and a team member insisted, "We haven't taught him this. He's doing it on his own." The next steps, the researcher continued, would be to find out whether Kanzi was trying to imitate human speech, and whether other chimps would learn to treat Kanzi's sounds as *communications*.

Like Sarah, however, Savage-Rumbaugh's chimps were accused by sceptics of being well-conditioned rather than well-spoken. Kanzi's newly minted 'words' could have been due to the Rosenthal effect. Even his seeming (limited) comprehension of syntax, or word order, could have been closer to Rosenthal than to Chomsky. And the tricky question remained: *why*, if chimps can emulate a 2-year-old, can't they emulate a 3-year-old or 4-year-old . . . ?

The progress—or lack of it—made in a decade of dedicated work can be gauged by comparing two highly detailed reviews (Ristau and Robbins 1982; Wallman 1992). Strictly, the jury's still out. Who knows what may be achieved tomorrow? But don't hold your breath. As yet, there's no clear evidence that any non-human animal can learn a syntactically structured, indefinitely creative, language. Descartes, it seems, was right.

But maybe Chomsky wasn't. For besides agreeing with Descartes that no ape could ever learn language, he'd also posited a dedicated "Language Acquisition Device" providing the human baby with an innate knowledge of universal grammar (9.ii.c and vii.c–d). His disciple Steven Pinker argued for that position in a best-selling book on *The Language Instinct* (1994). However, not everyone was convinced.

Non-Chomskyans suggested that whatever it is which enables babies to acquire language may be some combination of mechanisms evolved for more general purposes—including visual perception and problem solving. The psycholinguist Elizabeth Bates (1947–2003), for instance, famously said: "Nature is a miser. She clothes her children in hand-me-downs, builds new machines in makeshift fashion from sundry old parts" (E. Bates *et al.* 1979: 1). If these "sundry old parts" *make language possible*, that's not to say that they're dedicated to it alone (cf. 12.x.g). More recently, the neuropsychologist Arbib (2003, 2005) has named seven general capacities for which neuroscientific evidence exists, and which may underlie the evolution of language. (For an overview of current ideas on its development, see Tomasello and Bates 2001.)

d. Modish modules

Nativism is nativism: why stop at language? Chomsky himself had speculated that the mental "organ" for language is only one of several such organs (1980*a*). Now, in *The Modularity of Mind* (1983), Fodor picked up Chomsky's mental-organs idea, and Marr's work too, and ran with them very far and fast indeed.

He didn't merely say that inherited mental organs ("modules") exist. He argued that *only* behaviour which is grounded in them can be grist for psychology's mill: "the limits of modularity are also likely to be the limits of what we are going to be able to understand" (1983: 126). To be sure, he added the words ". . . given anything like the theoretical apparatus currently available". But he didn't expect to hear of a sufficiently bright idea some time next week. Instead, the 'higher mental processes' were being ruled out of bounds (see Section iii.d, above).

Provocative as ever, he increased the shock by subtitling his new book 'An Essay in Faculty Psychology'. The nineteenth-century faculty psychologists had been ousted from Psychology's House long ago (hence no mention of them in Chapter 5.ii). Was he inviting them back in? Not quite: he wasn't heralding 'organs' for Will and Imagination. But he was resuscitating their notion that we're born with various mental powers, which are—and remain—distinct.

By a module (alias a faculty), Fodor meant a biologically evolved input system triggered by domain-specific ("proprietary") information, whose computations are automatic and "encapsulated". That is, they can't be influenced by each other nor, top-down, by concepts or beliefs (Chapter 16.iv.d). He was using the distinction that he and Pylyshyn had made earlier, between processes defined by the FFA and cognitively penetrable phenomena (Section v.a, above). But the FFA was now seen as comprising a significant number of domain-specific components.

If Fodor's book carried a negative message (*Hands off the higher mental processes!*), it also carried a positive one: *Inborn faculties exist, and can be found—and understood*. Finding them, he said, involves "carving nature at its joints" (p. 128). He discussed only language and vision, because computational theories—in Marr's sense—had been outlined only for those. His own account of language included David Lewis's (1969) convention of truth telling as one of the constraints:

> We say of *x* that it is *F* only if *x* is *F*. Because that convention holds, it is possible to infer from what one hears said to the way that the world is [much as we can infer from visual stimulations what the 3D layout of the scene is]. (Fodor 1983: 45)

(Lewis's term *convention* wasn't well chosen: "truth telling" isn't like wearing a hat, or even driving on the left/right of the road, for we can't decide not to follow it.) This, by the way, enabled Fodor to escape from the trap of methodological solipsism (Fodor 1980*a* and 16.iv.d). The logic of Fodor's argument left space for other, as-yet-undiscovered, domain-specific faculties. He speculated in passing about possible motor modules. But some people went much further: namely, the evolutionary psychologists (cf. Chapter 8.ii.d–e).

Evolutionary psychology was ethology made good. It was both more cognitive and—by 1980—more 'respectable' than the work mentioned in Chapter 5.ii.c. The new data about the mental powers of chimps and gorillas had helped, as had the growing appreciation of Simon's ant. Many evolutionary psychologists specifically highlighted computational ideas (Tooby and Cosmides 1992: 64–9, 94–108, 112–14). Moreover, Marr and Fodor were often cited in support of evolutionary accounts of sociocultural behaviour (see Chapter 8.ii.e). And Pinker (1997) wrote a popular trade book in which Chomsky and evolutionary psychology were celebrated together.

A wide variety of modules were posited by evolutionary psychologists, each supported by its own adaptationist story. For instance, Leda Cosmides (1957–) and John Tooby (1952–) attributed the recognition and punishment of "cheaters" to an inborn faculty evolved to detect and sanction people "violating a social contract" (Cosmides and Tooby 1992: 180; see also Cosmides 1989).

Such a faculty, they argued, would be very useful to a social species, for without it various types of antisocial parasitism would emerge. Whereas formal logic is a general-purpose inference mechanism, cheat detection—they said—involves a dedicated, and largely automatic, system. They claimed that this explains the startling results of Wason and Johnson-Laird's experiments (Section iv.d). Indeed, these were seen as strong evidence in their favour, since they showed that people are far better at detecting violations of if–then rules if (besides being familiar) they involve *cheating on a social contract* (pp. 180–206).

Given that *Homo sapiens* is a social species, cheat detection wasn't the only candidate for 'interpersonal' modules: a number of others were posited too (A. W. Byrne and Whiten 1988). The most important, a faculty for attributing intentional states to other people, is discussed at length below (subsection f). Three more suggested modules—a body-ratio assessor, a symmetry spotter, and a viable-habitat recognizer—are mentioned in Chapter 8.iv.a; and a fifth, a face recognizer (for which there is strong neuroscientific evidence), is described in Chapter 14.ix.c.

But the candidates for modularity numbered many more than these. Indeed, the mind was repeatedly compared to a Swiss army knife, a tool consisting of many little tools—each of which does a different job, and each of which functions separately.

One of the most extreme expressions of this viewpoint came from the neuropsychologist Charles Gallistel. He even argued, at a time when Hebb's name was echoing loudly in psychology's halls, that there's *no* content-neutral learning mechanism (see 14.ix.g). As for modules, he asked:

> Did the human mind evolve to resemble a single general-purpose computer with few or no intrinsic content-dependent programs? Or does its evolved architecture more closely resemble an intricate network of functionally dedicated computers, each activated by different classes of content or problem, with some more general-purpose computers embedded in the architecture as well? . . . [In other words, does] the human mind come equipped with any procedures, representational formats, or content-primitives that evolved especially to deal with *faces, mothers, language, sex, food, infants, tools, siblings, friendship, and the rest of human metaculture and the world*? (Gallistel 1990: 94; see also Gallistel 1995)

His answer—no prizes offered!—was a resounding *Yes!*

Just how separately the different modules function was a bone of contention. For instance, some people stressed our ability to mix concepts from different domains. The developmental psychologist Karmiloff-Smith showed that such mixing is impossible for infants, but becomes possible later (subsection h, below). And the archaeologist Steven Mithen (1996*a*,*b*) argued that the evolution of modern man from earlier hominids involved a move from a 'pure' Swiss-army-knife mind to one of "cognitive fluidity", where concepts evolved for one domain can be applied in another. Members of *Homo sapiens* can think of people also as animals, or even as objects. These types of racism were impossible for our phylogenetic cousins *Homo neanderthalis*, said Mithen, because for them people could only be *people*.

As the example from archaeology may suggest, modules became fashionable way beyond the pages of psychology books. By the turn of the century, even theologians of a distinctly traditional cast of mind were using the term en passant, without comment or explanation. So the Chancellor of York Minster, while bemoaning the unremitting rise of secularity, remarked:

> [The] decline of organized religion . . . does not necessarily mean that people are losing an inclination to seek personal identity through *modules of understanding* which they would probably call 'spiritual'. (E. Norman 2002, p. viii; italics added)

This was in a little book written for a general audience, and utterly lacking in jargon—theological or otherwise. Clearly, the author assumed that his readers would know what he meant. (Whether there really are modules for spiritual understanding is discussed in Chapter 8.vi.b–f.)

That's why, when Pinker—in the very same year—published yet another trade book putting the case for modules, some of his own colleagues accused him of flogging a dead horse. In *The Blank Slate* (2002), he ridiculed fourth-tenet externalism and championed an inborn "human nature". A number of psychologists (in reviews, and in Internet discussion groups) complained that he was wildly exaggerating the extent to which people still believed in John Locke's "blank slate" (6.i.c).

Pinker's critics were exaggerating, too. On the one hand, postmodernism in the humanities—and anthropology (8.ii.a–c)—still had a strong influence. Besides their anti-realist take on science (1.iii.b), the postmodernists still insisted that human minds are culturally constructed *rather than* (not *as well as*) biologically grounded. On the other hand, there was some resistance to nativism within cognitive science itself. The connectionist McClelland was now generalizing his notorious mid-1980s attack on innate grammar (12.vi.e) to cover the concepts—animals, plants . . . and so on—believed by evolutionary psychologists to be innate semantic modules (McLelland and Rogers 2003; T. T. Rogers and McClelland 2004). Even "Theory of Mind" (see below) was said by some to arise from general connectionist roots.

The point of historical interest, however, is that many psychologists felt that Pinker was attacking a straw man. Nativism, it seemed, had graduated from professional heresy to near-orthodoxy in a mere thirty years. (It was still anathema for many of the general public, however: so Pinker's book didn't entirely miss its mark.)

e. But how many, exactly?

Speculations about Neanderthal man were bound to raise eyebrows, not to say blood pressure. But they weren't the only ideas to be scorned by critics as Just So stories.

Rudyard Kipling's (1902) tales of how the elephant got its trunk, and why we indulge the cat who walks by himself, were hardly less fanciful—so these critics thought—than many claims made in evolutionary psychology. The modules posited were so numerous, and so socioculturally 'sexy' (going way beyond boring Mexican hats), that they threatened to undermine Darwinian psychology's hard-won respectability.

One could be bothered by this proliferation of modules even as a modularist oneself. Fodor, for instance, refused to accept all the examples posited by the evolutionary psychologists. He countered Pinker's Swiss-army-knife account of *How the Mind Works* (Pinker 1997) with his own *The Mind Doesn't Work That Way* (Fodor 2000*b*), a longer version of his stinging squib in the *London Review of Books* (1998*c*).

Fodor's problem wasn't the nativism: "I'm a committed—not to say fanatical—nativist myself" (2000*b*: 2). (He even took this opportunity of reiterating his belief in an innate Language of Thought: 16.iv.c.) Nor was it the commitment to computationalism, which he still thought "far the best theory of cognition that we've got". Rather, it was Pinker's "ebullient optimism" about what a computational psychology could actually achieve:

> I would have thought that the last forty or fifty years have demonstrated pretty clearly that there are aspects of higher mental processes into which the current armamentarium of computational models, theories, and experimental techniques offers vanishingly little insight. And I would have thought that all of this is common knowledge in the trade. How, in light of it, could anybody manage to be so relentlessly cheerful? (2000*b*: 2–3)

His own sympathy was with A. A. Milne's Eeyore: "'It's snowing still' said Eeyore. '. . . And freezing . . . However,' he said, brightening up a little, 'we haven't had an earthquake lately.'" That, Fodor remarked, captured his mood exactly.

In part, Fodor was repeating his pessimism about explaining the higher mental processes (Section iii.d, above). He did this by attacking what the anthropologist Sperber (1994) had called the "massive modularity thesis", or MM thesis (Fodor 1987*a*; 2000*b*, ch. 4). The MM thesis (in Fodor's words) is the view that "the mind is mostly made of modules". In Sperber's account, modules came in many formats and sizes, including micro-modules the size of a single concept. In short, there are lots of them.

Fodor was happy to allow that there may be more modules than he'd originally expected. But how are their outputs integrated? Where, as Descartes would have put it (2.iii.c), is the *common* sense? How is it that, faced with perceptually intractable illusions such as the Müller-Lyer diagram, we aren't actually fooled by them—because we *know* they're illusions? What domain-specific module could possibly explain that? None, said Fodor, explicitly aligning himself with "the New Rationalists" (see Chapter 9.ii.a). In short, *many* modules needn't imply *massive* modularity. (Sperber eventually admitted that his MM thesis had been "extremist". Nevertheless, he insisted that it was true enough to justify further "speculation" about modules: Sperber 1994.)

As for just how many modules there may be, Fodor was relatively miserly. Marr's work on vision offered the clearest examples, but recent research on human infants had plausibly suggested others—not only in folk physics, but in folk psychology too (see below). However, Sperber and others had gone overboard: "the modularity thesis gone mad" (Fodor 1987*a*: 27).

The adaptationist arguments used by the evolutionary psychologists, said Fodor, were largely bogus. For one thing, tiny changes in brain structure may have huge (i.e. relatively sudden) effects on the behavioural phenotype. Indeed that seems to be so, for chimps' brains are much more like ours than their behaviour is. It follows, given our current ignorance about the brain, that we don't *have to* posit a gradual evolutionary shaping of each aspect of human behaviour to explain 'how we got here from there'.

For another, adaptationists often commit "fallacies of rationalization" (Freud's word). For example, Cosmides and Tooby had argued backwards from their recognition that it would be useful for a social species to possess a cheat-detection faculty. But, said Fodor, from the fact that *if* someone's motive were to increase his chances of survival/reproduction *then* the best thing to do would be X, it doesn't follow that he *actually* did X because he (or his selfish genes) consciously or—*pace* Freud—unconsciously 'wanted' that evolutionarily happy result. The person's conscious avowal that he did X *because he wanted something else* is an alternative, and often more plausible, explanation. In other words, cognitive science can be nativist without being Darwinist—if that means massively, reductively, adaptationist.

Fodor wasn't the only philosopher contributing to the modularity debate. Many others queried *just what was meant* by "module", as well as asking whether this or that version of the modularity hypothesis was coherent and/or plausible (useful collections are: J. L. Garfield 1987; Segal 1996; Carruthers and Chamberlain 2000).

For example, Samuels (1968–) noted a threefold ambiguity (Samuels 1998, 2000; Samuels *et al*. 1999). Modules, he said, were assumed to be:

(1) innate stores of information about a specific domain, ranging from organized sets of items to single concepts, or
(2) information-processing mechanisms, applied only in a given domain, or
(3) both.

Samuels defined a compromise, the "Library Model of Cognition", according to which modules are:

(4) stores of domain-specific information processed by general-purpose mechanisms. (An example of the latter might be the general preference for moderate novelty, discussed in Chapter 8.iv.c.)

The Library Model, he argued, is just as evolutionarily plausible as MM.

Tony Atkinson and Michael Wheeler (2004) offered a further alternative. Besides showing that there's rarely a clear distinction between "domain-specific" and "domain-general" processes (because domains can be defined at different hierarchical levels), they pointed out that there may be

(5) innate general information-processing mechanisms *as well as* specific ones.

Each of these alternatives (Samuels and Atkinson–Wheeler) is a form of MMM thesis, the three Ms here positing *moderately* massive modularity (Carruthers 2003).

For our purposes, what's interesting is the *computational* flavour of almost all of these philosophers' discussions. Anyone could grumble about the spurious plausibility of Just So stories, of course—and many did. (It was a favourite gambit of the postmodernist opponents of evolutionary psychology, for instance.) But the arguments about informational integration, and about general versus domain-specific information-processing mechanisms, could have been suggested only in an intellectual climate which took for granted that the causes of behaviour can/should be conceptualized in functionalist (computational) terms. The philosophy of mind had changed unrecognizably since the 1950s (see Chapter 16.i).

(By the end of the century, the notion of "module" had changed almost unrecognizably too. Many psychologists and neuroscientists now argued that modules are "inborn" only in a highly qualified sense. Instead of being preformed, they *develop* epigenetically: see subsections g–i, below.)

f. Theory of Mind

Developmental psychologists' research on babies' understanding of the physical world—movement, for instance, or solidity, or size-constancy—made some astounding discoveries in the 1980s. For Piaget's claim that real-world knowledge arises only from active manipulation of it was being cast in doubt. Manipulation, once it has appeared, may well enrich the infant's object concept. But it was now becoming clear that Piaget had hugely underestimated babies' abilities.

For example:

* newborns have some sense of size-constancy (Slater *et al*. 1990),

* and various aspects of the physical world become salient one after another (Spelke 1991).
* Babies only $2\frac{1}{2}$ months old show surprise when one (hidden) solid object apparently passes through, or jumps over, another instead of moving along a connected, unobstructed pathway.
* Four-month-olds assume that a hidden object will maintain a constant size and shape.
* And by six months they also expect unsupported objects to move downwards, and moving objects to continue to move in the absence of obstacles.

In short, even very young babies have grasped a significant portion of what the AI scientist Hayes was calling "naive physics" (13.i.ii). Comparable discoveries were being made about many other concepts, too (Carey 1985; Carey and Gelman 1991). But some of the most high-profile nativist research concerned "Theory of Mind" (ToM).

The idea of ToM originated in 1970s studies of chimpanzee behaviour, and was buttressed by 1970s discoveries in neuropsychology. However, two of the seminal publications appeared in 1983 and 1985, when Fodor's book on modularity was making intellectual waves, and it became strongly associated with that concept. Indeed, Cosmides and Tooby—two leading proponents of the Swiss army knife—later wrote the introduction for a widely read book on ToM (Baron-Cohen 1995).

The label "Theory of Mind" was coined in the late 1970s by the primatologists Premack and Guy Woodruff (P&W). After training the long-suffering Sarah to use quasi-linguistic tokens, Premack had turned to ask what intentional understanding she had. For example, could she attribute goals and plans to other agents? P&W's answer was *Yes!*

In one experiment, for instance, they familiarized Sarah with heaters—and how they were lit. Then, they showed her a video of a shivering experimenter next to an unlit heater. They gave her two pictures to choose from: one showing a lit match, the other some unrelated scene. Almost always, she chose the match. Now they raised the stakes. Sarah was given three pictures: a lit match, an unlit match, and a burnt-out match. Again, she almost always chose the lit match. A similar experiment, with comparable results, involved a person trapped in a cage, and a key (ratcheted up to a key, a twisted key, and a broken key).

These results showed, said P&W, that Sarah was applying a theory of mind. She realized that the human wanted to be warm, that a sub-goal was to light the heater, and that a lighted match was a means to that sub-goal. Moreover, P&W interpreted their label literally: "A system of inferences of this kind is properly viewed as a theory" (1978: 515).

Published in *BBS*, P&W's paper received very wide attention. One item of the extensive peer commentary was especially fruitful. The philosopher Dennett (1978*d*) pointed out that their results, like Premack's previous work on Sarah's language, could be explained as due to conditioning. (P&W had actually admitted this, but said an intentional explanation was simpler.) But he added some constructive comments.

Based on his account of the intentional stance (16.iv.a–b), Dennett listed five necessary conditions for attributing ToM—"beliefs about beliefs"—to an agent. We can't be sure that chimps meet these conditions, he said, because they lack language.

But children do have language. Moreover, children evidently appreciate the subjectivity of belief, for they can attribute *false* beliefs to others, and make reliable predictions accordingly. For instance, their enjoyment of Punch and Judy involves their knowledge that Punch believes—mistakenly—that Judy is trapped in the box he's about to throw off the cliff.

By the summer of 1979, Dennett's analysis was being applied in work on children, not chimps (J. Perner, personal communication). It inspired an ingenious experimental design involving pretend play with puppets, developed by Heinz Wimmer and Josef Perner (1948–)—then at the universities of Salzburg and Sussex respectively. They already knew that infants have "meta-representation" (beliefs about beliefs) and even understand that different individuals—for instance, people sitting on opposite sides of a screen—may have different beliefs. Now, they discovered that this meta-representation is severely limited: the full subjectivity of belief isn't yet understood (Wimmer and Perner 1983).

In a nutshell, they found that 3-year-olds can't grasp the fact that people may have beliefs that are false. A different version of their original test was soon defined by Simon Baron-Cohen (1958–), at University College London (UCL). He showed, for instance, that a 3-year-old will say that Sally, who left the room just before the marble she'd put into a basket was moved into a box by Ann, will look for it *in the box* on her return (Baron-Cohen *et al.* 1985). That's why infants don't lie: they *can't* lie, for they can't comprehend that a statement they know to be true might be thought by someone else to be false. (It turned out later that pygmy 3-year-olds can't conceive of false belief either, and become able to do so at around 5 years just like their Western counterparts: Avis and Harris 1991—and see Chapter 8.vi.d.)

As for when subjectivity (and deception) are at last understood, Wimmer and Perner pointed to 5/6 years of age. The Sally–Ann ToM test put it at 4 years old. But the central 'nativist' point was the same: some developmental (not purely experiential) factor enables children to realize, eventually, that people have *minds*.

ToM became a major experimental industry in the late 1980s. (For a few of the products, see Astington *et al.* 1988; Wellman 1990; Perner 1991; Whiten 1991; Leslie 2000. And for an account of how ToM-endowed children develop *trust* appropriately, see P. L. Harris and Koenig forthcoming.) Here, two points are especially relevant. On the one hand, there was a dispute about *how ToM functions*. On the other, there was the suggestion that ToM is a *module*.

Regarding the first, some psychologists—such as Perner himself (1991)—followed P&W in taking the "T" in ToM literally. They believed that the 4-year-old develops a *theory* that people have subjective beliefs and goals. Thenceforth, the child/adult interprets and predicts behaviour by inferring hypotheses from that theory. In effect, the New Look's notion of perception-as-science (6.ii) was being generalized to the perception of mind. Others disagreed. ToM, they said, is the ability to *simulate* the other person's behaviour. One puts oneself in the place of the other, empathizes with them, adopts their point of view. Then, one 'runs through', or imagines, the thoughts/behaviour that would result if starting from *that* place.

This approach was originated by the Cambridge philosopher Jane Heal (1986) and the Missouri philosopher Robert Gordon (1986). Heal spoke of "replication", Gordon of "simulation"—and it was Gordon's term which was generally adopted. Later, after some years of heated dispute, Heal clarified what's meant by simulation and showed that it could be understood in two different ways (Heal 1994, 1996, 1998).

She pointed out that the general assumption that 'theory versus simulation' is an *empirical* dispute was misleading. Theory theory (in a "strong" sense) is a priori false, and simulation theory a priori true. Or rather, it's true if one takes simulation theory to mean "co-cognition": the ability, at the personal level, to think about the subject matter of the thoughts of others. The reason is that thinking about Anita's thoughts about opera, or coffee, *necessarily* involves thinking about (exploiting our own knowledge of) opera, or coffee. (According to theory theory, all we have to consider is folk psychology, or the nature of *thoughts*, never mind opera and coffee—Heal 1998: 485–8.) The empirical question is what "sub-personal cognitive machinery" is involved in implementing such co-cognition. One possible answer (there are others) is that:

> when we think about others' thoughts we sometimes "unhook" some of our cognitive mechanisms so that they can run "off-line" and then feed them with "pretend" versions of the sorts of thought we attribute to the other. (Heal 1998: 484)

The "theory theory" versus "simulation theory" debate filled many pages through the 1990s. Because it concerned the nature of folk psychology, it intrigued philosophers as well as psychologists. Lively interdisciplinary discussions resulted, in *BBS* and elsewhere (e.g. P. L. Harris 1991; Goldman 1993; M. Davies and Stone 1995*a*,*b*; Carruthers and Smith 1996; Peacocke 1994: 99–154). The epistemologist Alvin Goldman, at Rutgers University, even developed a "hybrid" account, in which both simulation and 'theorizing' played a part (Goldman forthcoming).

Recently, a third possibility has been proposed by Alan Leslie (1951–). An ex-student of Bruner's at Oxford, Leslie was originally employed at UCL but is now at Rutger's Center for Cognitive Science. As required by Dennett's five conditions for ascribing second-order beliefs, he considers both true/false belief and positive/negative desires. And he claims to have provided "the first information-processing model of belief–desire reasoning" (2000: 1235).

Leslie sees ToM as a dedicated frame, or schema, associated with a processing mechanism that fills the frame slots in a particular way (see Chapter 10.iii.a). The child "is endowed [*sic*] with a representational system that captures cognitive properties underlying behavior" (p. 1235). This ToM representation marks four things: an agent; an agent's attitude (e.g. pretends-true, believes-true, believes-false); an aspect of the world that "anchors" the attitude (e.g. a telephone, a banana); and the content of the attitude (e.g. "it's a telephone"). The processing mechanism operates whenever attention is directed onto an agent's behaviour. It *selects* a content for the agent's belief, which by default is assumed to cohere with reality. (This makes evolutionary sense, for most beliefs have to be true if we are to survive.)

In false-belief tasks, the default assignment has to be inhibited. That's done, Leslie says, by *general* information-processing mechanisms, which take at least four years to develop. But if the non-factual content is somehow made more salient (for instance, by asking "Where will Sally look for the marble *first*?"), then 3-year-olds can pass the ToM test. Indeed, even a 2-year-old can understand his/her mother pretending, with exaggeration, that a banana is a telephone (Leslie 1987).

As for the second point remarked above, we've seen that ToM research didn't need the concept of modularity to get started. But the two became closely associated in the mid-1980s. This wasn't due only to the 'modishness' of modules. To the contrary, there was strong evidence that ToM is an inherited faculty. Modules (by definition) function—and so can malfunction—independently. If ToM is a module, there should be cases where it fails to develop, even though other psychological powers are normal. And this, it now appeared, was so.

The evidence came from a trio at the MRC's Cognitive Development Unit at UCL: Baron-Cohen, Leslie, and Uta Frith (1941–). They'd been working on ToM, pretend play, and autism—and in the mid-1980s they brought these three ideas together. They showed that ToM can fail to develop even though other aspects of intelligence are normal. And the result, they said, is autism—a combination of poor social skills, lack of eye contact, delayed language, and no pretend play—or its milder form, Asperger's syndrome. (The early papers are Baron-Cohen *et al.* 1985; Leslie and Frith 1987; see also U. Frith 1989, 1991; Baron-Cohen 1995, 2000*a*.)

"These *three* ideas" (ToM, pretend play, autism): modularity wasn't one of them. The UCL work was partly 'prepared' by recent work in clinical neuropsychology—which strongly suggested nativism. Two London-based colleagues, Elizabeth Warrington and Timothy Shallice, had discovered surprisingly specific behavioural deficits, often associated with damage to specific brain regions:

* In cases of language deterioration, for instance, low-level concepts were typically lost before high-level ones (Warrington 1975; cf. Quillian 1969);
* long-term and short-term memory could be damaged independently, and were seemingly stored in different parts of the brain (Shallice and Warrington 1970);
* and motor action could be impaired by different flaws in planning, which mapped onto different brain lesions (Shallice 1982).

Frith (personal communication) now remembers these discoveries as having encouraged her group to pursue ToM, whereas the Chomsky/Fodor writings on modularity had little or no influence on them.

The UCL view was revolutionary—and highly controversial. Autism had previously been blamed on emotional deficits, or on inadequate mothering. The revolution might have happened a few years earlier, for it was already known that autistic children wouldn't engage in pretend play. Indeed, that was part of Leo Kanner's (1943) original definition of the syndrome. What Leslie had offered was a retrospective explanation, based on the fact that pretend play involves temporarily accepting/attributing false beliefs.

However, his ideas were so "radical" at that time that he had "incredible difficulties" publishing them (U. Frith, personal communication). Moreover, a lack of pretend play was the least of sufferers' problems: who cared? As it turned out, Leslie's paper on play didn't appear in print until two years *after* the trio had published their suggestion that autistics don't develop ToM (Leslie 1987).

Their first experiments, done by Baron-Cohen as a Ph.D. student, showed that the 25 per cent of autistic children who have normal causal/mathematical intelligence cope very badly with false-belief tests. Twelve-year-olds fare far worse than *much* younger controls, and also than 10-year-old intellectually retarded Down Syndrome children (success rates were 20 per cent, 86 per cent, and 85 per cent respectively). Even Frith, already an expert on autism, was taken aback by these findings: "I was amazed by Simon's first results—I had to go along and see for myself" (personal communication).

Over the following years, further evidence for ToM modularity emerged. (And evidence *against* it, too: see subsection h.) Some came from brain scanning done by Frith's husband's team at Hammersmith Hospital (C. D. Frith and Frith 2000; Baron-Cohen *et al.* 1994). They reported brain areas that 'light up' when normal people are using intentional concepts, but don't seem to be activated in Asperger subjects.

Other evidence was behavioural. Baron-Cohen—now co-directing Cambridge's Autism Research Centre (the MRC's UCL Unit was disbanded in 1998, when its Director John Morton retired)—recently showed that autistics' weakness in the domain of folk psychology can coexist with very high intelligence in the physical domain (2000*b*). Indeed, he has suggested that Sir Isaac Newton's personal failings, remarked at the opening of Chapter 5, may have been due to Asperger's (Baron-Cohen and James 2003).

In addition, there was new evidence of sex linkage. It was already known that autism is more than twice as common in boys than in girls. Now, it was found that the testosterone level before birth (in the amniotic sac) varies inversely with the amount of eye contact—and the vocabulary size—during the first two years of life. (This applies to normal children: data aren't yet available for autistics.) Baron-Cohen (2002, 2003) now contrasts "empathizing" people/brains with "systemizing" people/brains, and even describes autism as 'The Extreme Male Brain'.

Despite the autism evidence, whether ToM could strictly be counted as a *module* wasn't clear. The more it was assimilated to theoretical inference in general, the more problematic this was. Leslie allowed general processing mechanisms to play a role, but he did define a four-slot *dedicated* ToM representation. Even so, this version of ToM was very different from the fully automatic processing and highly specific input information of modules for Mexican hats.

The murkiness was increased by the unclarity of just what "module" means (see subsection e, above). Moreover, a close colleague of the UCL trio, the developmentalist Karmiloff-Smith, had formulated a theory that not only undermined the MM thesis but also complicated the concept of nativism itself (see subsection i). And she'd specifically argued that the autistic's ToM deficit might be due to *general computational* rather than *specific representational* deficits (1992: 168).

While the developmental psychologists were energetically studying ToM, its originators—the primatologists—were doing so too. Many of them attributed ToM to non-human primates. They did so on the basis of studies (and 'natural' observations) of imitation, self-recognition, social relationships, deception, role taking, and perspective taking.

Sometimes, the conclusion was that these animals had some ToM, but not much. Specifically, they apparently don't understand beliefs, but do understand some psychological processes, such as seeing (Tomasello 1996; Tomasello *et al.* 2003*a*,*b*). They—and some non-primate species, too—can also recognize "basic" emotions such as fear and anger (see i.d, above) from their conspecifics' facial expressions (Sripada and Goldman 2005). So to ask whether chimps have a theory of mind, *Yes or No?*, is "exceedingly misleading": there's no such thing as "a monolithic 'theory of mind' that species either do or do not have" (Tomasello *et al.* 2003*b*: 239, 240). To put the point in another way, ToM has evolved gradually, rather than springing into existence fully formed (Povinelli and Preuss 1995).

But such caution was rare—especially in the early days, and especially in the newspapers. Predictably, media accounts of ToM in the jungle proliferated in the 1990s. The public were entranced—and the animal-rights movement further enraged (see 2.v.a). Indeed, their guru—the philosopher Peter Singer—founded the Great Ape Project, whose aim was to persuade governments and individuals that some non-human primates have rights comparable to ours and should be treated accordingly (Cavalieri and Singer 1993). (Today, the World Transhumanist Association recommends ascribing moral rights also to man-machine cyborgs, and to some computer agents, too: see <http://www.transhumanism.org>.) It seemed, then, that Goodall and Fossey had been vindicated—with knobs on.

However, all was not as it seemed. Cecilia Heyes (1960–), a philosophically sophisticated psychologist at UCL, argued in *BBS* that in every reported case the results could have occurred (1) by chance, or (2) by non-mentalist association, or (3) by inference based on non-mental—e.g. causal—categories (Heyes 1990, 1998; cf. Povinelli and Vonk 2003). Modulo the latter concession to cognitivism, this was Sarah-and-the-tokens all over again.

Heyes noted that primatologists (including P&W, as we've seen) usually supported their ToM explanations by appeal to theoretical parsimony—including Dennett's pragmatic defence of the intentional stance (Heyes 1998: 109–10). Occam's razor was taking precedence over Lloyd Morgan's canon (see 2.x.b). But Heyes favoured the canon. Moreover, she said, these experimenters had misinterpreted the notion of parsimony. Their ToM explanations might appear "simpler" than long-winded associationist accounts, but they weren't. For one thing, the so-called convergent evidence rarely rested on independent (i.e. non-ToM) assumptions.

She wasn't saying that ToM in animals is impossible. (She even proposed an experiment, based on conditional discrimination training and transfer tests, to find out whether chimpanzees really do have the concept *see*: 112 ff.) And she admitted that many would think her a "killjoy", making "carpingly narrow objections" to "elegantly bold ideas". Her own instinct, indeed, was *not* to shout for the nit-picking methodologists. Nevertheless:

> [It] is precisely because Premack and Woodruff's question is important and intriguing that it warrants a reliable answer; and without some sober reflection, acknowledging the limitations of current research, we may never know whether nonhuman primates have a theory of mind. (p. 114)

g. The third way

Given the many meanings of modularity sketched above, it was abundantly evident by the end of the century that nativism wasn't a clear concept. In truth, it never had been.

For example, Chomsky's mid-1960s attempt to resuscitate rationalist doctrines of innate ideas had been criticized at the time by leading empiricist philosophers (9.ii.c and vii.c). They'd shown that nativism could be interpreted in many different ways, some of which weren't supported by Chomsky's claims.

Even more to the point, an entire school of psychology, which had been moving beneath the mantle since the 1920s (5.ii.c), was committed to a compromise position. Piaget had repeatedly described himself as taking a "middle way" between nativism and environmentalism, rationalism and empiricism . . . and other tempting philosophical dichotomies (Boden 1979: 5 ff.). He saw the question *Is behaviour x innate or isn't it?* as ill-posed. Instead of pure nativism or pure environmentalism, he favoured *epigenesis*: a self-organizing dialectic between biological maturation and experience.

But "self-organization" and "dialectic" weren't buzzwords in the ears of the fashionable. So although by the 1960s Piaget's ideas had been hugely influential in the theory and practice of education, they were much less so in experimental psychology as such. Indeed, he'd been ignored, not to say scorned, by most tough-minded psychologists—including early cognitive scientists such as Sutherland (5.ii.a and c).

Piaget was well into his sixties by that time—not a good age for adopting a brand new technology/methodology. To be sure, he'd previously approved of cybernetics and now approved of the GOFAI approach to psychology too (Boden 1979, ch. 7). So he was quick to say: "I wish to urge that we make an attempt to use it" (Piaget 1960). On reaching his seventies, he remarked that the techniques of computer simulation promised to be "the most decisive for the study of structures" (Piaget 1967: 318). And in 1969, in a Radio Canada TV interview with Thérèse Gouin-Decarie, he "wished strongly" that the AI approach would be explored (Jean Gascon, personal communication). But it was much too late for him to do so himself.

Younger bloods, however, leapt in. Owing to the widespread indifference to his work remarked above, they were rather few. Nevertheless, in the early 1970s several computationalists tried to implement aspects of his theory by way of production systems. These attempts were scattered across the USA (Klahr and Wallace 1970, 1976), Canada (Baylor *et al.* 1973; Baylor and Gascon 1974; Baylor and Lemoyne 1975), and UK (Richard M. Young 1974, 1976).

For instance, Richard Young (1944–), at the MRC Unit in Cambridge, studied the micro- and macro-developmental changes involved in children's grasp of seriation—specifically, arranging blocks to make a staircase (see 5.ii.c). True to Newell and Simon's inspiration, Young relied on an abstract task analysis, and also gathered behavioural protocols in the laboratory. Both were reflected in his computer model. Besides the 'correct' and 'incorrect' picking-up and placing of blocks (which Piaget had described), it included details such as the child's stretching out a hand to pick up a block but withdrawing it before the block was actually touched; touching a block without picking it up; and double-tapping one block with a finger moving along the staircase.

Young unified his various models—which represented both different Piagetian "stages" and different children—by way of a theoretical matrix defining a space of seriation skills. The three dimensions of the space were *selection* of the next block, *evaluation* of the structure built so far, and *placement and correction* of individual blocks. Any type of seriation behaviour, including characteristic errors as well as successes, could be defined by locating it within this space. Adding and/or removing certain rules (with appropriate prioritizing) would take the system from one child to another, from one experimental session to another, or from one stage to another. For example, the transformation from the concrete to the formal operational stage was effected by adding *IF you want to add a block to the staircase THEN pick up the biggest block*.

That was all very well—but it wasn't clear that Young was simulating what Piaget had been talking about. Whatever "self-organization" might mean, it didn't include an extra rule's being added by the external hand of the programmer. Moreover, stage transformations were supposed by Piaget to be holistic, so that visual and tactile seriation (for instance) develop simultaneously. But because Young's systems were so nicely matched to his specific experimental protocols, they weren't readily generalizable to other domains (although he did generalize from blocks to discs).

In any event, Young—like his fellow modellers in the USA and Canada—wasn't aiming to advance Piaget's theory in any fundamental way. The seriation matrix was new, to be sure, but it added system not substance. Trying to clarify the theory was challenge enough.

Throughout the 1980s, the challenge was taken up by others—including some at Carnegie Mellon, the birthplace of production systems (Klahr *et al.* 1987; Siegler 1983, 1989; Newell 1990). Although most of these models focused on behaviour at a given developmental stage, a few tried to model development as such (e.g. Wallace *et al.* 1987).

Just for the record: In the 1990s, production systems were still going strong (e.g. Klahr 1992). But they'd been joined by a constructivist approach developed by Minsky's student Gary Drescher (1989, 1991). Drescher modelled Piaget's notion of 'schema' in a GOFAI program which, by simulating bodily action in the physical world, gradually built up concepts of persisting, perceiver-independent objects. Since it didn't start out with any specific world knowledge, Drescher claimed to have solved the chicken-and-egg problem of empiricism: how to learn the "relevant" environmental regularities, given innumerable candidates, without already knowing which ones they are. (In one sense, he needn't have bothered: as we'll see, Piaget's view that newborns aren't predisposed towards specific environmental patterns was mistaken.) Later still, GOFAI was joined by connectionism, when Thomas Shultz modelled seriation in a network using "cascade correlation" (12.ix.c).

While the production-system Piagetians were still at work in the 1970s–1980s, others *were* trying to move the 'third way' forward. Eventually, they succeeded—although some of Piaget's key claims were modified/abandoned in the process. Throughout the last quarter-century, the concept of epigenesis was enriched by new theoretical insights and empirical findings.

In the 1990s epigenesis became prominent in A-Life, where Waddington was 'officially' promoted from maverick to guru (see 15.iii.b and Boden 1994*a*, introd.). It had revived in biology too (Piaget's first love), being applied—for example—in understanding bone anatomy (Lovejoy *et al.* 1999).

But the main impetus had come from psychology and neuroscience—with connectionist modelling playing a role as well. Indeed, an influential six-authored book called *Rethinking Innateness* (Elman *et al.* 1996) spelled out epigenesis from each of these disciplinary bases. (And from a base of dynamical theory too: Esther Thelen's account of the A-not-B error in terms of bodily skills, not the object concept, is outlined in Chapter 14.ix.b.)

No one reading that book could rest easy with the simplistic interpretations of nativism that had been held for so long, by defenders and opponents alike. There were some critical reactions (e.g. Fodor 1997; Samuels 2002). Nevertheless, the huge complexity of development, and its importance for understanding *adult* minds, could no longer be gainsaid.

This interdisciplinary revival was carried out by many different people. I'll discuss three in particular (one here, two in later chapters), but the caveat spelled out in Chapter 1.iii.e–f applies here, as elsewhere. None of them was a lone spirit, theorizing in isolation from their developmentalist colleagues.

One of the earliest prime movers in the reawakening of epigenesis was the neuroscientist Jean-Pierre Changeux, whose approach will be outlined in Chapter 14.ix.c–d. Others included Jeffrey Elman, one of the six co-authors of *Rethinking Innateness*. We'll see in Chapter 12.viii.b–c that he contributed two important ideas to connectionism, namely recurrent networks and "starting small"—where the latter was an insight into epigenesis *in general*. In this chapter however (and also in 12.viii.d), I'll focus on yet another of the six co-authors, the psychologist Karmiloff-Smith (1938–).

Karmiloff-Smith (1990*b*) aimed to cement a "marriage" between Chomsky and Piaget, and between Fodor's anti-constructivist nativism and Piaget's anti-nativist constructivism (Karmiloff-Smith 1992: 10). (Fodor was unwilling, and tried to make the cement crumble: 1997, 1998*d*.) She saluted Fodor for having had "a significant impact on developmental theorizing" (1992: 1). But she challenged his two major claims: that 'higher' thought can't be explained, and that adult modules are already preformed in the infant. (The first challenge is discussed in subsection h, below, the second in subsection i.)

Like her fellow enthusiasts for the third way, she didn't see developmental psychology as a specialism, safely ignored by 'straight' psychologists. On the contrary, she said, cognitive science *as such* needs a developmental perspective. So she was always irritated by well-meaning people who gushed "You must love babies!" (This would happen even more often after her popular TV series and best-selling books, some written with her daughter Kyra: Karmiloff-Smith 1994; Karmiloff and Karmiloff-Smith 1998, 2001.) Besides the fact that they wouldn't have said that to her male colleagues, or anyway not in the same tone of voice, this betrayed a fundamental misunderstanding. She studied development not because she loved babies but because she wanted to understand human minds.

Karmiloff-Smith became a student of Piaget's in Geneva in 1967, and was a member of his research group in the 1970s. From 1982 to 1988 she worked alongside the ToM trio at UCL, and then founded the Neurocognitive Development Unit at the Institute of Child Health (attached to UCL and to the Great Ormond Street Hospital for Children). In the 1960s, she'd been a professional simultaneous translator for the United Nations, including two years working in Beirut's Palestinian refugee camps. She's one of those rare people who came to science late but made a significant contribution anyway.

How significant? Well, we'll see in the rest of this section that she provided a rich trove of empirical data showing that Piaget's theory of "stages" was mistaken, and his focus on "accommodation to error" greatly overdone. She defined a computational theory of the development of the higher mental processes. She offered an epigenetic account that turned the MM thesis upside-down. And she helped to institute a novel rapprochement between psychology (developmental, cognitive, and clinical) and neuroscience (ditto, ditto, and ditto).

h. What makes higher thinking possible?

Karmiloff-Smith was no orthodox Piagetian. For one thing, she didn't posit holistic stage changes. Rather, she spoke of a series of "phases" within each domain (e.g. language or drawing) and micro-domain (e.g. using pronouns, or the definite/indefinite article). And her data showed that the different domains don't march in step. For another thing, she allowed, even "insisted", that

> there are *some* innately specified, domain-specific predispositions that guide epigenesis. Young infants have more of a head start on development than Piaget granted them. (1992: 172)

This had become clear in the recent explosion of research on very young babies, sometimes only a few hours—or minutes—old. In the mid-1970s, studies of early turn taking, eye-gaze following, and pointing had shown that these activities "scaffold" the growing intersubjectivity between baby and mother, including language (Scaife and Bruner 1975; Bruner 1978; C. B. Trevarthen 1979; see 6.ii.c). By the early 1990s there was a mountain of evidence that babies can do much more than Piaget had thought.

For instance, newborns pay more attention to human speech than to other sounds; within a few days, they prefer their mother's language; and soon afterwards they can recognize some of its syntactic properties. Further examples included seeing things as continuously existing objects (5.ii.c); looking at faces, especially the eyes (14.ix.c); and anticipating 'realistic' physical movements. (See Carey 1985; Sophian and Adams 1987; Mehler *et al.* 1988; Gallistel 1990; Spelke 1991; Butterworth 1991; Carey and Gelman 1991; Leslie 1991.)

However, since babies can't use language, or pencils, or pianos . . . as adults can, the nativist has a problem:

> The more complex the picture we ultimately build of the innate capacities of the infant mind, the more important it becomes for us to explain the flexibility of subsequent cognitive development. (Karmiloff-Smith 1992: 9)

The most influential nativist of the day, namely Fodor, had announced this to be impossible: the higher mental processes were out of bounds (Section iii.d, above).

Karmiloff-Smith disagreed. In the mid-1970s, when she was still in Geneva, she'd begun a long-continuing programme of experiments—on language, weight balancing, drawing, making maps, and writing—designed to test her ideas about "representational redescription", or RR (e.g. Karmiloff-Smith and Inhelder 1975; Karmiloff-Smith 1979*a*,*b*,*c*, 1984, 1986). She believed that RR is found only in *Homo sapiens*, and identifies what's specifically human about human cognition.

According to RR theory, adult intelligence is achieved by successive representational changes, whereby information that was previously *implicit in* the system becomes *explicit to* the system. (The earlier representation is supplemented, not replaced: it's still available for use.) These changes aren't driven by error—Piaget's "accommodation" to the external world (5.ii.c)—but by autonomous internal development. That is, they happen only *after* mastery has been achieved at the lower level. The child constructs increasingly explicit "theories" of its own skills, which it uses to vary those skills in increasingly flexible ways. Eventually, skills learnt in one domain or micro-domain can be used in others too.

A similar progression happens when adults learn to read late in life, or to play/improvise on the piano (Karmiloff-Smith 1986: 97–8; 1992: 16–17; cf. Sudnow 1978/2001; Hermelin and O'Connor 1989). David Sudnow's subtle account of the subjective experience of learning to improvise on the piano is especially interesting here. He made no claim to identify the psychological mechanisms involved. But he did indicate the development of previously undescribed motor, perceptual, and musical schemata—which psychology (e.g. RR theory) should be able to explain.

RR isn't the work of an instant: constructing a new theory takes time. Indeed, the child who previously performed successfully starts making mistakes, which disappear when the new representational phase has been consolidated (so graphs plotting task success are U-shaped). But although much of the observable behaviour is the same as before—the beam is balanced, or the appropriate word pronounced—the cognitive processes that generate it are very different.

Karmiloff-Smith posited four levels of (meta-)representation, culminating in verbalizable consciousness. Each new level describes the skill more explicitly, adding opportunities for self-monitoring and (eventually) voluntary self-control—that is, freedom (Section i.g, above). The order of skilled movements, and—later—their organization into hierarchical (sub)routines, is marked. Once an aspect has been marked, it can be varied. Steps (subroutines) can now be omitted, iterated, inserted at different points, or even smoothly integrated into a routine developed to deal with a very different category. In other words, the child (or skill-learning adult) becomes successively more imaginative, or creative (Boden 1990*a*: 63–73).

The many detailed experiments which put empirical flesh onto these RR-theoretical bones included a series on drawing (Karmiloff-Smith 1986; 1992: 155–61). Children between 4 and 11—all of whom could fluently draw an *ordinary* man (or animal, or house)—were asked to draw "a funny man" (or animal, or house), or "a man that doesn't exist". Karmiloff-Smith found, as she'd predicted, that their ability to do this develops phase by phase.

The youngest children couldn't do it at all. They'd learnt how to draw a man (animal, house) successfully, and unvaryingly (e.g. head or roof first), but that was it. The first changes affected the shape and/or size of elements (e.g. a bubble-shaped arm, and/or a tiny head) or of the whole thing (e.g. a house shaped like an ice-cream cone). Then, elements could be deleted, although at first only the last part normally drawn. (Compare: novice pianists can halt a piece at mid-point before they're able to start it from there.) The 5-year-olds could draw a cross-category figure (e.g. a house with wings) if asked, but only by adding the cross-category elements *after* the main picture had been drawn—and very few of them did this 'of their own free will'.

Only from about 8 or 9 years of age were extra elements smoothly, and spontaneously, inserted into the drawing while it was being done (e.g. a second head, with no 'unnecessary' lines between it and the single neck). And only then was an element's position/orientation changed (e.g. the head put on the side of the shoulder), or elements from different categories inserted (e.g. man and animal, to give a 'centaur').

Comparable results were observed in the other domains, in each of which the children showed a growing ability to comment on what they were doing. With respect to speech, for instance, Karmiloff-Smith (1986; 1992, ch. 2) found that when children first use the words *the* and *a* (or their French equivalents) correctly, they have no insight into their speaking skill. If asked to pick up "a watch", they will (correctly) pick any of the several watches, whereas if asked for "the red watch" or "my watch", they pick a particular one; and they use the right word in describing what they picked up. But they can't reflect on their speech, to say what the relevant difference is. They can't do this even when they start correcting themselves for wrong usage ("the watch" when there are two watches, for example). At first, they can rectify the mistake only by providing the right form of words, not by giving the general principle. Later, however, they can explain that if there'd been two watches on the table it would have been wrong to say "I picked up the watch" (or "Pick up the watch"), because the listener wouldn't know which one was meant. At last, they've achieved a conscious grasp of the structure of their own linguistic skill.

Or rather, they've consciously grasped *some* aspects of it. Even highly articulate adults don't always know just what they're doing when they speak. For instance, they can't explain why they use a pronoun rather than a full noun phrase in particular communicative contexts (Karmiloff-Smith *et al.* 1993). We can all use *she* to track the role of the main character in a story—but without realizing that we're doing so, never mind how we do it. (That's why pronouns are so difficult to handle in NLP *text* generation: see Chapter 9.c and f.)

Karmiloff-Smith suggested that we lack metalinguistic awareness here because decisions about pronominalization are taken rapidly on-line, depending on what's just been said to the listener. In other words, redescription doesn't happen because there's nothing in LTM to be redescribed. (In her later work she found that "notational" skills are sometimes varied earlier than language or music making; for instance, children can sometimes delete parts of a "funny man" earlier than she'd expected. Her explanation was that the physical mark provides the child with cues for self-monitoring which otherwise would be made available only by RR.)

As her remark about text pronominalization implies, she thought about thinking in computational terms. This had been true almost from the start. She'd learnt LISP programming by the late 1970s, and used GOFAI as a source of concepts, metaphor, and inspiration. Her account of what the meta-representations enable the child to do recalled GOFAI planning, including Gerald Sussman's self-correcting HACKER (Chapter 10.iii.c). And she compared the lowest level of skill to a compiled program and post-RR skills to interpreted programs, employing "procedural" and "declarative" knowledge respectively (1990*a*; but see 1992: 161–2).

Later, she turned to connectionist AI (1992, ch. 8). She was drawn to PDP because it showed that global, stagelike, transformations can occur as a result of continuous local changes. For example, the development of the past tense (sketched in subsection a), which Chomsky and Fodor had ascribed to general computational *rules*, appeared to happen as a result of cumulative changes in system statistics (see 12.vi.e). What's more, PDP networks initially capable of learning many different things became more specialized, less plastic, with training. This was analogous, she felt, to the process of modularization (see below).

Karmiloff-Smith's connectionist work is described in Chapter 12 (Sections viii.d and e, and ix.a)—as is Elman's and Kim Plunkett's, both also co-editors of *Rethinking Innateness*. As we'll see, GOFAI is (still) better at modelling what meta-representations enable the system to do once they've arisen, connectionism—perhaps—at modelling how they arise in the first place. Having seen that RR was "precisely what is missing thus far [i.e. in about 1990] from connectionist simulations" (1992: 179), Karmiloff-Smith sketched some ways in which RR might come about (A. J. Clark and Karmiloff-Smith 1993). However, her suggestions were highly programmatic. Others used cascade correlation to model cognitive development (12.ix.c). But their success, too, was limited—hence that cautionary "perhaps". From the computational point of view, the development of RR is still opaque.

What of RR theory's challenge to Fodor? It outlined how certain structural types of high-level thought become possible—which is largely what a psychological science is about (see Section iii.d). But it couldn't explain, still less predict, particular cases in detail. (No Mexican hats.) It couldn't say *just why* an 11-year-old decided to draw a house with wings, as opposed to something else equally bizarre.

Compare what Fodor had said about analogy. He'd remarked that it's often involved in scientific discovery—in the Rutherford–Bohr solar-system model of the atom, for instance. But he'd despaired of its being understood:

> [Analogical] reasoning would seem to be . . . a process which depends precisely upon the transfer of information among cognitive domains previously assumed to be mutually irrelevant. By definition, encapsulated systems [i.e. modules] do not reason analogically . . . It is striking that, while everybody thinks that analogical reasoning is an important ingredient in all sorts of cognitive achievements that we prize, *nobody knows anything* about how it works; not even in the dim, in-a-glass-darkly sort of way in which there are some ideas about how [scientific] confirmation works. I don't think that this is an accident either. In fact, I should like to propose a generalization; one which I fondly hope will some day come to be known as "Fodor's First Law of the Nonexistence of Cognitive Science". It goes like this: the more global . . . a cognitive process is, the less anybody understands it. (Fodor 1983: 107; italics added)

His charge that "nobody knows anything" about analogy had been rebutted by RR theory. For scientific discovery is houses-with-wings with knobs on. RR had even indicated some very general types of self-monitoring that can twiddle the knobs to good effect. How the Rutherford–Bohr atom *could possibly arise* was thus somewhat less obscure than before.

But how it *actually* arose wasn't. For the reasons outlined in Section iii.d, we'll probably never know—not even with the relevant diaries, notebooks, and recollections available from the scientists concerned. Moreover, AI models of analogy still fell short of the human reality in many ways (see 13.iv.c and 12.x.a). Despite the necessary cavils about "nobody knows anything" then, Fodor's First Law remained standing.

i. The modularization of modules

In "insisting on" the post-1970 evidence of babies' abilities, Karmiloff-Smith had accepted a form of nativism—but not Fodor's. Indeed, she turned the MM thesis on its head. Instead of "prespecified modules" she posited "a process of modularization" (1992: 4). To the extent that the adult mind/brain is modular, this happens—she said—not because it was born that way but as a result of development.

For many years, this was an interesting speculation rather than a proven fact. To be sure, it made sense from the epigenetic point of view. Indeed, Piagetian neuroscientists were already discovering aspects of brain development that made it broadly plausible

(14.ix.c–d). But specific proof, for specific cases, required advances in cognitive neuroscience which didn't happen until the late 1980s.

Karmiloff-Smith was waiting for them (1992: 5). Meanwhile, and alongside her RR studies of normal development, she'd begun to study abnormal development too. In Geneva she hadn't had links with a hospital. But at UCL's Cognitive Development Unit (funded by the *Medical* Research Council) she did. Moreover, her UCL colleague Frith was already working on autism when she arrived, and Frith's student Baron-Cohen was about to discover some surprising comparisons between autism and Down Syndrome. Those comparisons bore on the issue of modularity, as we've seen (subsection f, above). Her interest aroused, Karmiloff-Smith too started working with congenitally brain-damaged people.

She became especially interested in Williams Syndrome (1990; 1992: 168–71; Karmiloff-Smith *et al.* 2003). Besides some physical abnormalities, such as 'elfin' faces and SVAS (a narrowing of the aorta where it leaves the heart), Williams patients have an unusual cognitive profile. They (seem to) develop language normally, though up to a year late, and face recognition too. However, they're gravely retarded in general intelligence, including number, problem solving, spatial skills, and planning (their IQs range between 40 and 85: Tassabehji 2003).

For instance, Karmiloff-Smith reported a conversation with an 18-year-old Williams patient who said she was very interested in vampires. This young woman, unable to tie her shoelaces or perform simple spatial-alignment tasks, spoke about vampires thus:

EXPERIMENTER: What do vampires do?
WILLIAMS PATIENT: They break into women's bedrooms in the middle of the night and sink their teeth into their necks.
EXPERIMENTER: Why do they do that?
WILLIAMS PATIENT: (Clearly never having asked herself the question) Maybe they are inordinately fond of necks.

(Karmiloff-Smith 2001)

The grammar and vocabulary were impressive. But the conceptual understanding was not.

So far, so Fodorian. Williams Syndrome seemed to provide strong evidence for modularity. And MM theorists said so: Pinker, for instance, spoke of language being "unimpaired" in several congenital conditions while other faculties were damaged (1994: 51).

However, by the early 1990s it was clear—from neuroscientific as well as behavioural evidence—that face recognition, for example, isn't a preformed mechanism (14.ix.c). To the contrary, it gradually becomes more discriminating and domain-specific. In general, it now seemed that innate predispositions do no more than lead the baby/child to attend to certain broadly specified types of input: three-blob 'face' stimuli, for instance, or human speech sounds, or the mother's eyes. But the child eventually processes more richly specified inputs, in more encapsulated and domain-specific ways. In short, modules aren't there from the start. They're *produced* by epigenesis.

If that's so, one would expect that still-developing 'modules' are less separate than Fodor had believed. Damage to one would probably be reflected in some way by damage to another. From the epigenetic point of view, the notion that one or two developing modules could remain intact while others are grossly deficient was highly implausible.

Moreover, there was growing evidence that if a module can't develop normally—because of lack of input, for example—the brain's plasticity compensates to some extent (14.ix.d). So in ferrets whose auditory nerve is cut, the auditory cortex may develop systematically structured *visual* feature detectors (Sur *et al.* 1988, 1990). Similarly, brain scanning had shown that the auditory cortex in congenitally deaf children comes to be used for computing *visuo-spatial* input in linguistically relevant ways (Karmiloff-Smith 1992: 172—and see Chapter 14.ix.d). In short, much the same function (though not exactly the same, as we'll see) comes to be served by different parts of the brain.

Accordingly, a number of psycholinguists got involved in interdisciplinary studies of congenital brain damage. The ASL specialist Bellugi, for instance, had done so with respect to Williams Syndrome in the 1980s (Bellugi *et al.* 1988, 1991). Karmiloff-Smith's work, too, became increasingly interdisciplinary.

At the Neurocognitive Development Unit, she was collaborating with a wide variety of clinicians and neuroscientists, including her partner Mark Johnson (1960–): see 14.ix.d. Soon, she was working with clinical geneticists too, including a team at Manchester's St Mary's Hospital. In addition, she helped to build new connectionist models (and reviewed older ones, including the past-tense learner: 12.vi.e) to show that the 'same' behavioural deficiency in child and adult need not necessarily be due to the same underlying causal mechanisms (M. S. C. Thomas and Karmiloff-Smith 2002).

(As yet, those "causal mechanisms" have been modelled largely by ignoring issues of timing and synchrony in the nervous system. But Karmiloff-Smith's team expect this to change:

> There are likely to be temporal parameters that have a material effect on the ability of a network to represent relational information; variations in such parameters would be new candidates for mechanisms to drive cognitive development....
>
> Only computational implementation will clarify what notions such as processing capacity, speed of processing, and inhibition involve, and how they may be related both to each other and to components of implemented systems. For example, in considering the notion of speed of processing, computational specification forces certain issues to be addressed. Of what is there a higher rate per second? By what precise mechanism does this rate alter the quality of computations? Are changes in speed the outcome of changes in other parameters, in the way that increasing the discriminability of processing units might allow recurrent circuits to settle more quickly into stable states? Or is it a primitive, on a par with the velocity of neural conductance? (M. S. C. Thomas and Karmiloff-Smith 2003: 148)

The omission of timing in most connectionist modelling has often drawn criticism from neuroscientists, and others. Recently, however, people have started taking these issues on board: see Chapter 14.ii.d and ix.g.)

The combination of these disciplinary approaches strongly suggested that the Residual Normality (RN) assumption is mistaken. According to RN, atypical development can produce selective deficits while the rest of the system develops normally. That RN is untrue is, of course, just what the third way would lead one to expect.

Karmiloff-Smith's mature position was presented to a general audience—and to the media, who featured it widely—in her BPS Centenary Lecture in November 2001. She showed that all the disciplines just mentioned must be integrated if we're to understand intelligence, whether normal or abnormal. We must consider the "complex pathways from gene-to-brain-to-cognitive-processes-to-behaviour" (2001: 206).

That may sound like a platitude. But what she meant by integration wasn't simply inter-level consilience. Rather, it was consilience-with-epigenesis.

She pointed out that recent neuroscience had described many cases of brain plasticity, in which deficits in one region affect others (see Chapter 14.ix). At the psychological level, this meant that even apparently 'pure' syndromes aren't as pure as all that. So for example, when I said (above) that Williams children develop language "normally", that wasn't quite accurate. On closer examination, their language—vocabulary, semantics, grammar, pragmatics, and reading—had turned out to be abnormal after all (Paterson *et al.* 1999; Karmiloff-Smith 2001; Karmiloff-Smith *et al.* 2003). For instance, English-speaking Williams infants have trouble learning words with the first syllable accented (e.g. teddy) but not the last (e.g. guitar); and they acquire a fairly extensive vocabulary *before* they're able to use it for name-based categorization (Nazzi 2003).

Much the same applied to their apparently 'normal' face recognition. Yes, they could recognize faces successfully. But *the processes by which* they recognize faces are unusual. Instead of considering the face (or the car, or the monkey . . .) as a whole, with the parts interrelated within it, they 'count out' the individual facial features. Moreover, brain scans indicated that they mostly use the left cerebral hemisphere, whereas 'global' face recognition uses the right.

Further experiments showed that Williams patients have impairments in low-level activities such as eye-movement planning, and in patterns of neural firing in the brain. For instance, the brain-firing of Williams adults resembles that of normal 3-month-olds. They may also have abnormal *neurones*, since laboratory mice with 'Williams genes' deleted—see below—sometimes develop unusual dendritic spines (Tassabehji 2003). Such neurological details may underlie the behavioural impairments which led to Williams Syndrome's being noticed in the first place. Yet other studies showed that the differences in proficiency (spatial, language, social . . .) between a Williams adult and a Down adult *are not* the same as those between a Williams infant/child and a Down infant/child.

In a sense, these new findings were more of the same. The latter discovery, for example, was a special case of the epigeneticist's general claim that the adult isn't simply a larger version of the child. (So dissociations in adult life, after strokes perhaps, are *not* reliable evidence for 'inborn' modularity: see 14.x.b, and M. S. C. Thomas and Karmiloff-Smith 2002.) And as for the separation of mental powers, Karmiloff-Smith had already pointed out that ToM isn't so untouched in Down Syndrome as the modularists had believed: although Down children can pass the Sally–Ann test, they can't cope with the more difficult ToM tests which normal 7- to 9-year-olds can manage (1992: 170). By the turn of the century however, the degree of psychological and neuroscientific detail had been hugely increased.

What's more, genetics had now entered the picture. Where modularists—and journalists—were quick to speak of "the" gene for behaviour X, Karmiloff-Smith was one of the many who criticized this. (Another, who gave a host of examples from non-human animals, was the biologist Matt Ridley: 2003.) Her Centenary Lecture reported new illustrations of the fact that single genes don't have single effects. So the gene trumpeted by the Associated Press—admittedly, with some scepticism and much mockery—as "the grammar gene" is no such thing (Pinker 1994: 297–8; Gopnik and Crago 1991). Not only does it affect language in a *number* of ways (whereas some had linked it to a specific syntactic phenomenon), but it also affects other intellectual abilities.

As for Williams Syndrome, this had recently been shown to involve the deletion of sixteen genes on one copy of chromosome 7. (Two years later, the count had risen to twenty-five: Tassabehji 2003.) However, Karmiloff-Smith's Manchester collaborators discovered that people can lack a subset of those genes without showing an 'equivalent' subset of symptoms. Even with 70 per cent missing, a person could show (similar physical symptoms, such as SVAS, but) *no* impairment of intelligence. Yet with only two genes deleted (both included in the 70 per cent just mentioned), Williams Syndrome appears.

Why did Karmiloff-Smith choose that strange lecture title, 'Elementary, my Dear Watson . . . '? Well, although system complexity makes life difficult for the psychologist, it can also ease it a little. Any clinical syndrome has major effects (i.e. those we happen to be most interested in), but it will have side effects too. If some of these are observable more easily and/or earlier, diagnosis may benefit. This 'scattering' is useful also for theoretical psychology. If we know that a variety of effects tend to be found together, we can ask *What underlying computational process* could give rise (in cooperation with others, of course) to *just these* effects? So tiny clues, sometimes found in unexpected places, can help cognitive scientists to understand the full range of human intelligence—but they'll need the detection skills of a Sherlock Holmes.

By the new millennium, then, the MM thesis had been given a very different twist. Yes, "there are *some* innately specified, domain-specific predispositions that guide epigenesis" (1992: 172). But also, "the brain progressively sculpts itself, *slowly becoming more specialized* over developmental time" (2001: 207; italics added). In short, the third way was being driven further through the interdisciplinary forest.

7.vii. Satellite Images

The six psychological stories told above described very different topics, each covering many sub-topics. Most of the researchers involved chose to focus on only one of them—often, indeed, on sub-sub-(. . .)-topics that I haven't even hinted at. That was unavoidable: they were down on the ground, doing the detailed work.

Something that's unavoidable may nevertheless be regrettable. So Cronbach, for instance, had complained about the "fragmentation" of 1950s psychology. Years later, Newell would make similar complaints:

> Allen Newell, typically a cheery and optimistic man, often expressed frustration over the state of progress in cognitive science. He would point to such things as the "schools" of thought, the changes in fashion, the dominance of controversies, and the cyclical nature of theories. One of the problems he saw was that the field had become too focussed on specific issues and had lost sight of the big picture needed to understand the human mind. (John R. Anderson and Lebiere 2003: 587)

His own view of what "the big picture [of the mind]" would have to be like is sketched in subsection c, below. But one can also ask about the big picture *of computational psychology itself*.

If, like a latter-day Cronbach, we were to look down from a great height on this large area, what would we see? And how would the picture have altered over the last half-century?

a. A telescopic vision

The change that would be most evident, given a camera orbiting far above, is *a growing realization of the subtle complexity of the human mind*.

Intuitively, this complexity was already recognized. Cronbach's "philosophers or artists" had a good grasp of it (5.ii.a). So did his "scientific psychologists", when gossiping with/about their friends over coffee. But in their explicit theorizing, they didn't. Most ignored mental processes and semantic content entirely and/or tried to reduce them to highly abstract general principles, such as 'Newtonian' S–R laws (5.i and iii).

The early cognitive psychologists avoided S–R laws, but their theories were often hardly less simplistic. For they were frequently posed as dichotomies: serial versus parallel processing, analogue versus digital representation, single-trial versus continuous learning . . . and the like. This might have delighted Claude Lévi-Strauss (8.vi.c), but it didn't delight Newell. He argued that 'You Can't Play Twenty Questions with Nature and Win' (1973*c*), because mental processes can't be corralled within such crude theoretical categories. Gradually, more and more psychologists came to agree with him on that general point, even if they didn't accept his specific theories.

The computationalists of the 1960s, having accepted the mentalist baton from their non-Newtonian predecessors (Chapters 5 and 6), tried to delve deeper into the specifics of human minds. They even tackled personality and emotion (Section i.a–b, above). But their efforts, especially in the more personal areas, fell woefully short of the human reality. They didn't yet have powerful computational tools, whether machines or programming languages (10.v). More importantly, they didn't have appropriately detailed computational *ideas*.

But that changed, as the orbiting camera would show. Over the next forty years, this late twentieth-century version of *verum factum* (1.i.b) produced a huge crop of fruit. New computational concepts enabled psychologists to conceive theories about *precisely what may be going on* when we think. And not just when we "think": the satellite evidence would support the claim made at the outset of Chapter 1.ii, that cognitive science isn't concerned only with cognition.

The successes, failures, and limitations of programs actually run on a computer enabled psychologists to hone their ideas in a way that no verbal speculations could (iii.c, above). Similarly, they enabled them to express theories far more detailed than had previously been possible.

* Compare, for instance, what Abelson said about "attitude change" with what Dienes and Perner said about why it's more difficult for a hypnotist to persuade someone to hear voices than to make their arm rigid (Section i.a and h).
* Compare work in scene analysis (6.ii.e and 10.iv.b) with post-1980 theories of low-level vision (Section v.b–f above, and 14.vi).

* Compare Colby's part-semantic, part-numerical, account of different types of anxiety with Sloman's architectural analysis and nursemaid program (Sections i.a and f).
* Compare LT and GPS with Newell and Simon's production systems, or with Johnson-Laird's mental models (Section iv).
* Compare Markovian models of language with grammatically structured ones, and 'perfect' speech with Clippinger's anxiety-ridden pronouncements (Section ii and 9.vi–xi).
* Compare Skinner's simplistic accounts of language learning with the mini-programs for language acquisition offered by Miller and Johnson-Laird (Section iv.d–e), or with models of past-tense learning (12.vi.e).
* Compare early theories, and computer models, of rationalization and the attribution of agency (Section i.c) with Sperber and Wilson's theory of communicative relevance (iii.d), or with recent neuroscientific work on schizophrenic delusions about bodily 'takeover' (i.i).
* Or compare the all-too-familiar fluff about creativity (tact forbids . . .) with cognitive–computational theories of it, and with computer models of musical composition or of the generation and comprehension of familiar types of joke (Chapter 13.iv.c).
* Finally, compare Morton's confession in 1981 (p. 232) that "Experimental psychology has a disastrous history with respect to its relevance" with recent computational work on emotion (i.d–f), reasoning (iv), and various clinical syndromes (vi.i and 12.ix.b).

The point is not that the theories named in the second half of each of these comparisons are *correct*. Maybe they are, maybe they aren't. More likely, they are *and* they aren't. In other words, each of them suggests some promising directions—and indicates that theories *of at least that level of complexity* will be needed to explain the mental phenomena concerned. That realization in itself is a genuine advance.

Some readers may be shifting uneasily in their seats at this point. "What about the ant?", they may be saying (Section iv.a). "The satellite pictures show us Simon's ant, crawling on the ground—but it *reduces* the mental complexity needed to explain behaviour." Well, yes. But much of the "missing" complexity is transferred to the environment—especially to the cognitive technologies made available by human cultures. And ant-theory as a whole is more discriminating than the previously popular Twenty Questions dichotomies.

The camera-on-high would record that no one took much notice of the ant, at first. Or if they did, they pictured it crawling mostly *inside* the brain. In Newell and Simon's production systems, for instance, most of the *conditions* were supposed to be mental, not environmental, events (iv.b). By the 1990s, however, psychologists—especially those sympathetic to A-Life (Chapter 15)—were more ready to give embodiment its place, and the environment its due. One example was Gigerenzer's work on "simple heuristics that make us smart" (Section iv.g, above), but the renaissance of Gibsonian psychology counts here too (v.e–f). So does low-level Marr, despite his battles with the Gibsonians.

Crawling alongside the ant, and visible even from the sky by the end of the millennium, was the concept of *epigenesis* (Section vi.g). This was imported into psychology by Piaget

(5.ii.c). But his writings weren't taken seriously by the early computationalists (5.ii.a), partly because they seemed too philosophical to be respectable (Boden 1979, chs. 1 and 5). It was difficult to think about epigenesis in detail: one needed to consider both broad developmental trends and micro-developmental changes contingent on the environment (vi.h). And one needed to consider neurology, too. It wasn't until very late in the century that the epigenesis of (a few) abilities could be described in both psychological and neurological terms (vi.i above, and 14.ix.c–d).

Also visible from a great height would be the continuing and (despite challenges from the ant-lovers: 13.iii.b) deepening concern with *representation*. Craik had kicked this ball into play in the 1940s (4.vi). So had McCulloch and Pitts—not once, but twice. Their two representational balls were differently shaped, much as soccer and rugger balls are: one logical, the other probabilistic (4.iii and 12.i.c–d). Since then, psychologists' studies of representation (aka schemas) have burgeoned. Abelson's earliest notion of "scripts", for instance, concerned the mental representation of familiar interpersonal concepts (i.c). And representation *as such* was discussed with respect to vision (v.a–f), rationality (iii.d and iv.d–e), development (vi.f–h), consciousness and disturbances thereof (i.h), and psychological explanation in general (iii).

From the satellite, empirical research on representation that's described in other chapters would be visible too. This was focused, for example, on conceptual prototypes (8.i.b); on whole-animal integration (14.vii and 15.vii); on connectionist and distributed representations (12 *passim*); and on their implementation in the brain (14 *passim*, but especially 14.viii). In addition, people disputed the psychological relevance of GOFAI representations in general (10.iii.a and iv.a, and 13.iii.b).

Last but not least, the sky-high camera would record pertinent work being done by philosophers. Some were trying to buttress various versions of the representational theory of mind (12.x and 16.ii–iv), others to question it (16.v), and others to declare it fundamentally absurd (16.vi–viii). But one didn't have to be a professional philosopher to raise, and explore, such issues. Indeed, the camera's earliest takes would record that Craik himself had discussed representation in philosophical terms, noting for instance that *similarity* between X and Y isn't enough to make X a representation of Y (4.vi.b).

As for where the representations (and the responses) come from, even the grainiest photographs would show an increasing interest in evolution over the last sixty years. Craik had spoken of inbuilt/evolved models for making adaptive sense of perception (4.vi.b–c). The "evolutionary psychologists" of the last quarter-century had a great deal more to say about such matters (Sections i.i, iv.g, v.b–f, and vi above, plus nearly all of Chapter 8). Psychologists now have a much better sense of the similarities between *Homo sapiens* and other species, and of the huge—mostly language-based—differences too (Sections i.g, vi.a–c, and vi.h, and Chapters 8 and 9 *passim*).

Much of the relevant evidence was garnered as a result of asking specifically computational questions. But data gathered from non-computational sources, such as primatology (smudgy images of Washoe, Sarah, Lana, and Kanzi here), were woven into a computational fabric.

Closely associated with the evolutionary movement was the notion of "modules". This gained added respectability from Chomsky's linguistics and Fodor's philosophy of mind, both of which came out of the same Massachusetts stable (vi.d above, 9.vii.c–d, and 16.iv.c–d). However, it was countered to some extent by the growing emphasis on epigenesis (vi.i).

The satellite images would indicate a continuing background of interdisciplinarity, despite the foreground fractionation of computational psychology into multiple specialisms. In some areas, indeed, the interdisciplinarity deepened with the passage of years. One illustration of that is the developmental work described in Section vi.i. Another, as yet less detailed, is the rapprochement between cognitive and neuroscientific accounts of delusional states (i.h–i, above).

In the most recent years, the celestial camera would also pick up myriad activities of brain scanning. Occasionally, these would illuminate some specific psychological claim. However, and despite the enthusiastic pronouncements of their discoverers, most brain-scan data don't have much psychological relevance—except in the very broadest sense. So it's not clear that they should be counted as "discoveries" at all (see 1.iii.f). The theoretically alert satellite-image interpreter would see them less as *message* than as *noise* (14.iii.a and x.b). (In future decades, they might indeed form part of the message—but only if the relevant psychological theory has been developed.)

b. Forking footpaths

Different footpaths afford different flowers. That's as true in psychology as it is in field botany. The general methodologies visible from on high have changed over the years. Information theory and GOFAI were the first. For it was the early 1950s mathematical and New Look psychologists, soon joined by Newell and Simon, who got computational psychology off the ground and provided its first, intoxicating, successes (6.i–iv).

The satellite images transmitted in that mid-century period would also show connectionism. The early models of Hebbian learning (which illustrated very general features of neuropsychology) aroused late 1950s psychologists' interest (5.iv.b–f and 12.i–ii). In particular, they were intrigued by Selfridge's Pandemonium, and even more by Rosenblatt's perceptrons.

But that flurry of interest didn't last. For one thing, connectionism would be near-crushed under a huge obstacle thrown across its path in the late 1960s (snarling close-ups of Minsky and Papert, here: 12.iii). For another, connectionism couldn't yet be used to address specific psychological topics—such as the past tense, for example (12.vi.e). So it dropped out of the picture, remaining invisible for some twenty years. (Set at a higher resolution, the satellite camera would have detected important work, on associative memory, going on behind the scenes: 12.iv–v.)

Over the years, the camera would show GOFAI persisting as a major influence. It was used—and is still being used—to address many long-standing psychological topics, from personality to problem solving. Every section of this chapter showed 1960s work with the GOFAI imprimatur. These researchers theorized in GOFAI terms, and sometimes drew inspiration from GOFAI as such—for example, from work on knowledge representation (10.iii.a) and from computer-vision research on "scene analysis" (6.ii.e and 10.iv.b).

From the mid-1980s, the satellite images of GOFAI would jostle alongside images of connectionism (see Chapter 13). So viewers would see part-GOFAI theories of psychopathology (e.g. clinical disturbances of speech and everyday action), and of hypnosis and absent-mindedness too (Section i.g, and 12.ix.b). In addition, they'd see part-GOFAI work on the society of mind and/or mental architecture in general (Section i.e–g).

The camera would confirm that psychologists concerned with *mind as machine* have followed two different pathways (1.ii.a). (Or perhaps two different games: GOFAI soccer and connectionist rugger.) At first, the two weren't clearly distinguished—and anyway, most people who were interested in the one were also interested in the other (Chapter 4 *passim*). When a schism eventually occurred (4.ix), most psychologists took the GOFAI route. But the 'cybernetic/connectionist' road remained intact, and would later be heavily trodden (12 *passim*). One track within it would reflect interest in "dynamical" systems, now being studied in psychology, neuroscience, and A-Life (14.ix.b and 15.viii.c, ix, and xi).

Finally, what of the famous Zeitgeist? Would a sky-high viewer of computational psychology's history be as aware of this as Edwin Boring was in 1957? Probably not, since his canvas was stretched over many more than fifty years (although in Chapter 2 we skated lightly over many centuries). Within just one half-century, there's less time for the rise of new Zeitgeists.

However, our period did see one important example. This new viewpoint was chronicled and celebrated by Theodore Roszak (1969) as "the counter culture" (i.iii.c; see also Toulmin 1999, ch. 5). Postmodernism in the humanities was one expression of it, and the popular New Age movement was another. In general, Enlightenment optimism about the all-encompassing relevance of science, and about the likely success of technologies based on it, was roundly rebutted. For many people now, science was the enemy, and technology deeply suspect.

The most obvious effect that this cultural development had on professional psychology was in social psychology and certain parts of clinical psychology. (Psychology-in-anthropology was affected even more disastrously: see 8.ii.b–c.) 'Scientific' approaches were firmly rejected—and explicitly insulted. Presumably, Cronbach was appalled. Certainly, Broadbent was. So much so that he wrote a book, and prompted a professional conference, specifically to defend "empirical" psychology, including its informational and computational versions, from this counter-cultural attack (6.i.d).

Many members of the general public, doubtless including actual or potential psychology students, turned away from computational ideas as a result of their sympathy with the counter-culture. While some were driven primarily by philosophical worries, others were motivated, at least in part, by the militaristic context of computational psychology and AI in general (Chapter 11.i; see also Edwards 1996, ch. 6; Haraway 1986/1991). But with the advent of computational theories of self-organization and distributed cognition, some former opponents were part-mollified (e.g. Kember 2003). Hard-headed businessmen were affected, too: in theories of business management in the 1990s, there was a drift from the formal to the concrete, and from hierarchical to distributed control (Toulmin 1999, ch. 5).

Even *within* computational psychology, there were repercussions. From the satellite in outer space, one would be able to see that 'ant-friendly' theories were in general viewed more favourably by people who sympathized with Roszak. Decentralization, distributed control, bottom-up processing, emergence, epigenesis, embodiment, self-organization, 'simple' heuristics . . . all these concepts were attractive to people with counter-culturalist leanings (1.iii.d). And all crept into psychology in the last quarter-century, as well as informing A-Life and much of neuroscience.

They weren't usually put there by card-carrying New-Agers (though sometimes they were: Varela *et al*. 1991; Agre 1988, 1997). Indeed, almost all of them had been hovering in the background since the very earliest days. And some, such as Newell and Simon's decentralized blackboard architecture, were provided by very un-New-Age-ish people (iv.b). But with the scent of the counter-culture wafting in the air, their reception was eased. (And for professionals seeking to seduce the media, the way was clear: Chapters 12.vi.a, vii.a, and x.a, and 15.x.a.)

In sum, the satellite film would show widening, and deepening, activity over the last fifty years. Both major footpaths would be prominent, and various minor tracks would be seen to branch off them. Countless detailed examples of what John Ziman (2000*a*) called "reliable knowledge" have resulted from this research. If the sub-sub-sub- . . . topics could have been mentioned here, that would have been unmistakable. As it is, the six psychological stories told above, along with the relevant portions of other chapters, will have to suffice.

c. The Newell Test

In leafing through the satellite pictures to get an overall view of what's happened in the field, one could do worse than bear in mind Newell's criteria for psychologizing about the mind as a whole. He claimed that a theory of the human mind must consider, and a plausible computer model of it should implement, twelve functional properties—and his own SOAR system attempted to do just that (Section iv.b, above).

John ('ACT') Anderson and Christian Lebiere have recently suggested using these twelve properties as the "Newell Test" for cognition (Anderson and Lebiere 2003). This isn't intended as a post-millennial version of the (computer versus human) Turing Test, but as a way of comparing two computer models with each other. In a historical context, it offers a way of tracking research interests, and of estimating progress, over the last half-century.

The Newell Test assumes that we're considering an AI model with psychological, not merely technological, pretensions. And it advises us to ask whether it can do the following things:

1. Behave as an (almost) arbitrary function of the environment
   —Is it computationally universal, as opposed to (wholly) dedicated?
2. Operate in real time
   —Given its timing assumptions, can it respond as fast as humans?
3. Exhibit rational (that is, effective) adaptive behaviour
   —Does the system yield functional adaptation in the real world?
4. Use vast amounts of knowledge about the environment
   —How does the size of the knowledge base affect performance?
5. Behave robustly in the face of error, the unexpected, and the unknown
   —Can it produce cognitive agents that successfully inhabit dynamic environments?

6. Integrate diverse knowledge
   —Is it capable of common examples of sensory and intellectual combination?
7. Use natural language
   —Is it ready to take a test of language proficiency?
8. Exhibit self-awareness and a sense of self
   —Can it produce functional accounts of phenomena that reflect consciousness?
9. Learn from its environment
   —Can it produce the variety of human learning?
10. Acquire capabilities through development
    —Can it account for developmental phenomena?
11. Arise through evolution
    —Does the theory relate to evolutionary and comparative considerations?
12. Be realizable within the brain
    —Do the components of the theory exhaustively map onto brain processes?

(Adapted from John R. Anderson and Lebiere 2003: 588)

Applying this test to classical connectionism (Chapter 12) and to ACT-R (Section iv.c above), Anderson and Lebiere judge that—by and large—ACT-R is better, but that connectionism wins out on several points.

They point out, however, that to satisfy what psychologists in general are usually aiming for, Newell's twelve *functional* criteria would need to be supplemented by at least two more:

13. Match human mental processes.
    —Given that a computer model 'works', and fulfils the twelve demands listed above, does it also match the actual details of human psychology?
14. Be useful for practical applications.
    —Can the theory be used to improve everyday practice in psychotherapy, education, entertainment, and other areas of applied psychology?

These fourteen criteria have all been touched on in this "psychological" chapter. But—as a satellite image of *cognitive science as a whole* would show—they involve matters touched on in other chapters too. Here's a summary concordance:

1. Freedom from the environment:
   Sections i.h, iv.b–c, and vi.h.
   Chapters 8.v–vi, 9.iv.f, 9.vi.c–d, and 12.ix.b.
2. Real-time operation:
   Sections ii.c and iv.g.
   Chapter 14.vi.c–d and ix.g.
3. Rational/effective adaptive behaviour:
   Sections iv and vi.i.
   Chapters 4.viii, 5.ii.c. 5.iii, 6.iii, 8.iii–vi, 10.iii.c, 13.iii.b–c, 14.vi–vii, and 15.vii–ix and xi.
4. Vast environmental knowledge:
   Section iii.d.
   Chapters 9.iv.a–b, 10.iii.e, 13.i, and 14.vi.c–d.

5. Robustness in face of error/uncertainty:
   Section iv.g–h.
   Chapters 12 *passim*, 13.iii.b, and 14.vi.
6. Integration of diverse knowledge:
   Sections i.e–f, iii.d, and iv.c.
   Chapter 14.vi.c–d and vii.
7. Language:
   Sections ii, iv.d–e, vi.a–c, and vi.i.
   Chapters 4.iii.c, 6.i.e, 8.i.a–b, 9 *passim*, and 12.vi.e.
8. Self and self-awareness:
   Sections i and vi.f.
   Chapters 5.ii.a, 12.iii.d, 13.vi.e, and 14.x–xi.
9. Learning:
   Sections iv.c and vi.g.
   Chapters 5.iv.b–f, 6.ii.b–c, 12 *passim*, and 15.v.
10. Development:
    Section vi.
    Chapters 2.vi.b, 5.iii.c, 9.vii.c–d, 12.viii.d, 14.ix.c, and 15.iv.
11. Arise by evolution:
    Sections iv.g, v.b–f and vi.
    Chapters 8.ii.d–e, 8.iii–vi, 9.iv.e, 14.vii, and 15.vi–vii.
12. Realizable in the brain:
    Sections iv.b, v.b–f, and vi.i.
    Chapters 4.viii.c–d, 5.iv, 12.i.c–d, 14 *passim*, and 15.vii and xi.a.
13. Match human psychology:
    Sections i–vi *passim*.
    Chapters 8, 9, 12, 14, and 16 *passim*.
14. Be practically useful:
    Sections i.a and d–f, iv.a, and vi.i.
    Chapters 9.x–xi, 10.iv.c and v.g, 12.vi–vii, 13.v–vi and vii.b, and 14.x.b.

What would our judgement be, were we to look at today's computational psychology with these questions in mind? We'd have to admit that we're still a very long way from a plausible understanding of the mind's computational architecture, never mind computer models of it (Section i.e–g, above). And as remarked elsewhere (especially Chapters 8, 9, 13, and 16), we're still a long way from human-level modelling of language and the recognition of relevance—even supposing this to be in principle possible. In other words, psychological modelling is still bedevilled by the open texture of language, and by the frame problem in general.

Nevertheless, if we bear the Newell Test (or even common sense) in mind, many lines of progress can be seen. For instance, a half-century of work on nativism has hugely increased our understanding both of the facts and of the concept itself (Section vi—and see also item 10 in the concordance above). We're much more sophisticated about what "nativism" means, so less ready to assume that a particular ability must be *either* innate *or* learnt (dichotomies, again). We realize that learning can take place before birth, as well as after it. We've got a better understanding of the distinction between learning and development, and of how specific developmental changes are needed to support higher-order thought. And we've got a better sense of how (animal and human) behavioural studies can be integrated with neuroscience on the one hand and computer modelling on the other.

These advances happened in the context of cognitive science in general, and computational psychology in particular. Without the new forms of non-Newtonianism that arose in the 1950s (Chapters 5 and 6), they wouldn't have occurred.

d. Low focus

Even the low-resolution images described above picked out a fair amount of detail. What if the satellite camera were set at a lower resolution still?

At the low-focus extreme, the overall story would relate a gradually growing recognition by psychologists, especially cognitive psychologists, that a new form of explanation was available. (Or, if you prefer, two new forms—reflecting the two pathways to mind-as-machine.) And not merely available but, in many people's view, superior to anything that was available before.

The Oxford cognitive psychologist D. Alan Allport, for instance, didn't mince his words. The advent of AI models, he said some twenty-five years ago, was the "single most important development in the history of psychology" (1980: 31). As if that weren't praise enough, he added that "artificial intelligence will ultimately come to play the role vis-à-vis the psychological and social sciences that mathematics, from the seventeenth century on, has done for the physical sciences". In effect, Longuet-Higgins was saying the same thing, if less dramatically:

> It is perhaps time that the title "artificial intelligence" were replaced by something more modest and less provisional . . . *Might one suggest, with due deference to the psychological community, that "theoretical psychology" is really the right heading under which to classify artificial intelligence studies of perception and cognition*. (1981: 200; italics added)

Those comments are notable not only for their rhetorical power but also for the high professional standing, and scrupulousness, of both Allport and Longuet-Higgins. These men weren't playing to the gallery. Nor were they so closely identified with the field, as Newell was, that *of course* they'd champion it (Newell had said long ago that AI might as well be called "theoretical psychology": 1973*b*: 25–6). But nor were they alone. By 1981, at which time computational psychology had existed for a quarter-century, there were plenty more such remarks scattered in the literature.

That's not to say, however, that AI models were universally accepted by psychologists. At much the same time as those two encomia were being declared, another British cognitive psychologist said to me that the field was "too specialized" to be the theme of the first sub-meeting of the History and Philosophy of Psychology group within the BPS.

If interpreted as an intellectual judgement, that was a mistake. The computational approach (symbolic and connectionist combined) is held by its practitioners to be potentially applicable to all mental processes, so—if they're right—it's not "specialized" but universal. In other words, Neisser's fear that computational psychology would become "a narrow and uninteresting specialist field" was misguided (see Chapter 6.v.b).

However, if interpreted as a statistical observation about people's concerns at the time, her comment was correct. (The general professional reluctance to take computational theorizing on board was partly due to the lack of hands-on experience discussed in Chapter 6.v.a–b.)

It wouldn't be correct today. The interest aroused by the work described in this chapter has been—and is—increasingly strong.

Consider, for example, the BPS conference held in London in 2004. An entire day—one plenary session, plus half a dozen sessions running in parallel with others, plus two lengthy 'slots' for symposiasts' discussion—was devoted to the psychology of creativity *considered from the computational point of view*. If even creativity can be looked at in this way (cf. 13.iv), it's hardly surprising that many of the other 2004 conference sessions reflected this approach too. Indeed, they didn't even bother to say so: whereas session titles like 'Creativity and Computers' still have power to shock, computational ideas about perception, memory, reasoning, and language are ten tens a penny. Professionally speaking, computational psychology has arrived.

That's not to say that *computer models* are scattered like autumn leaves on the ground. Even psychologists who are sympathetic in principle may not be able to use that methodology (as opposed to the computational concepts driving it) to address their chosen research topics. In current studies of psychopathology, for instance, some people are using explicitly computational analyses and/or models. (Remember the research on autism and Williams Syndrome in Section vi.f and vi.i; and consider the analyses of pathological action errors mentioned in Chapters 12.ix.b and 14.x.b.) But many cognitive theories of psychopathology are computational only in a very broad sense, with no attention to detailed mental processing—still less, to computer modelling (e.g. D. M. Clark 1996).

Even so, the computational movement—or if you prefer, the cognitive revolution—has influenced the general form of these clinicians' theorizing. To that extent, even MGP's most flamboyant promises have been partially met (6.iv.c; and cf. 17.iv).

e. The bustling circus

In the late 1950s, Alan Baddeley feared for the future of psychology:

> [With respect to the controversy over Edward Tolman and Clark Hull: see 5.iii] I was very struck by the capacity of psychological theory to change direction rapidly, apparently as a result of fashion rather than evidence. This led to the question, commonly raised at the time, as to whether psychological theory could ever be cumulative. (Baddeley 2001: 345)

His own response was "to favour theorisation based on relatively simple generalisations tied to robust phenomena", and to opt for Stephen Toulmin's (1953) philosophy of science, which allowed for a pragmatic clutch of separate theories, rather than the then popular axiomatic approach (Chapter 9.v.a).

Professional psychology today is closer to Toulmin's vision than to Hull's. It's not "cumulative" in the sense that physics is. (Although even physics, notoriously, can't yet integrate relativity with quantum theory.) But the various topic areas are closer in spirit to each other, and to the other cognitive sciences, than they were when Baddeley was a young man.

Nevertheless, there are persisting areas of what Cronbach called "desert". Many psychologists, still, ignore the computational viewpoint. Some of them are more concerned with *What?* than with *How?* They don't deny that there are how-questions to be asked. They may even allow that computational ideas would be needed to provide the answers. But they aren't interested in asking them. These people include some primatologists (but see Section vi.e–f above), some evolutionary psychologists (but see 8.ii.d–e, iv, and vi), and most social psychologists and psychometricians—in other words, what Cronbach termed the descriptive/comparative psychologists.

Others spurn *any* 'scientific' approach, computationalism included. They believe that meaning, intentionality, personality, purpose, and consciousness simply *cannot* be explained naturalistically (Chapter 16.vi–viii).

These desert dwellers include postmodernists such as Kenneth Gergen (1994, 2001), who thirty years ago was among the first to attack empiricist social psychology (6.i.d). They also include the personality theorists and therapists whose predecessors were dismissed by Cronbach as "philosophers or artists". Some of them are specifically offended by what they see as the dehumanizing, technologistic, influence of computer models—even more alien than rats (1.iii.c–d, 6.i.d, and 8.ii.b–c). In general, they agree with humanists such as Giambattista Vico (1.i.b) and Wilhelm von Humboldt (9.iv) that knowledge of human minds is essentially different from, and even superior to (better grounded than), knowledge gained by science.

This particular area of intellectual "desert" is less scorpion-ridden in professional psychology than in anthropology, but it exists nonetheless (6.i.d and 8.ii.b–c). Hardly anyone attempts to cross it, from either side. That's not surprising: it's a special case of a fundamental split in the philosophy of mind and epistemology (14.xi and 16.vi.b).

A few brave souls have recently ventured forth to reconcile the seemingly irreconcilable (15.viii.b, 16.vii.b–d, ix.e–f, and x.c; and see M. W. Wheeler 2005). The most ambitious is the AI-based cognitive scientist Brian Cantwell Smith. However, his account of computation and mind is idiosyncratic to a startling degree (16.ix.e–f). For sure, it's not going to convince the masses. In any event, it's more concerned with metaphysics than with psychology. The specifically psychological desert-crossers include Francesco Varela, James Thompson, and Eleanor Rosch, whose recent writings on mind and cognition stress the importance of *embodiment* (Varela *et al.* 1991; see 15.viii.b). But despite the occasional date tree (13.iii.e), their pathway hasn't led to many significant oases of experimental/computational research.

In short, Cronbach's remarks on the fragmentation of psychology are still apt. The 2004 BPS conference, like its US equivalent half a century before, was "a circus, but a circus far grander and more bustling than any Barnum ever envisioned" (Cronbach 1957: 671). Given the differences of topic interest and of philosophy just remarked, the circus will continue to bustle for many years yet. Computational psychology, however, will be a star performer in the ring.

8

THE MYSTERY OF THE MISSING DISCIPLINE

Anthropology studies the cultures constructed by human minds, and by which they're largely constructed in turn.

That's shorthand, of course. Anthropologists have defined "culture" in many different ways (Kuper 1999). Moreover, some "cutting-edge cultural anthropologists" have "virtually eliminated" the term (Strauss and Quinn 1997: 1). Besides problems of definition, postmodern anthropologists today see "culture" as an unacceptably essentialist notion (they prefer terms like "discourse", "interest", and "strategy"). But I take it that we know, roughly, what we mean by culture—and that, as the cognitive anthropologist Bradd Shore (1996: 9) has said, we need to refine the notion rather than discard it.

I take it, too, that culture is paradigmatically human. It's not purely linguistic, since it covers material artefacts and rituals, and even bodily posture and gestures too. Indeed, the archaeologist Colin Renfrew (2003) argues that the external symbol storage of monumental architecture (e.g. Stonehenge) was the next important step in cultural evolution after language—an honour sometimes granted to cave-painting (Mithen 1996*b*) or writing (M. Donald 1991). Nevertheless, culture is very close to, and often expressed in, language—which is essentially human (see 7.vi.c).

So anthropologists *don't* normally include "fledgling birds mimicking their species-typical song from parents", or "rat pups eating only the food eaten by their mothers" as examples of culture, even though some psychologists do (Tomasello 1999: 4). Nor do they see "young chimpanzees learning the tool-use practices of the adults around them" as culture—although promoters of the Great Ape Project, which stresses the *similarities* between the higher primates, typically do (Cavalieri and Singer 1993). If the young birds, rats, or chimps didn't engage in that early learning, most other remarks about their minds—or, if you prefer, their lived worlds—would remain much the same. But that's not true of people. In Jakob von Uexküll's terminology (see 5.ii.c), the *Umwelten* of *Homo sapiens* are overwhelmingly cultural, or semiotic: human beings don't just live *with* culture, but in it, through it, and by means of it. Or in John McDowell's terminology (16.viii.b), culture—and in particular, language—is our "second nature": it's part of our ethology, without which we wouldn't be recognizably *human* at all.

One might expect, then, that anthropology would form part of cognitive science. For language has been a central concern of the field ever since the late 1950s (Chapter 6.i–ii), and the importance of "cognitive technologies" in general—which include not only language but also cave painting, architecture, and writing—was made much of by Jerome Bruner from the early 1960s (6.ii.c). Yet it's usually missing from lists of the disciplines involved. For instance, it wasn't included among "the disciplines contributing to cognitive science" in a leading recent textbook on *Understanding Intelligence* (Pfeifer and Scheier 1999: 39). Nor did it appear in the titles of the six chapters on individual disciplines that opened the huge *MIT Encyclopedia of the Cognitive Sciences*, or *MITECS* (R. A. Wilson and Keil 1999). If it's mentioned at all, it's apt to be downplayed as "a trace substance" (Simon 1994*b*: 1).

When cognitive science began, things were very different. Many people then thought it obvious that anthropology should play a significant role. By the 1980s, however, it had become near-invisible.

Anthropological questions were continuing to feature nevertheless. Indeed, an expert on one African culture declared in the 1990s that "The study of cognitive processes, however incipient in cognitive psychology, *allows us to reformulate many classical problems of anthropology*" (Boyer 1994, p. xi; italics added). Strictly speaking, then, the discipline isn't "missing" so much as unacknowledged.

This chapter explains why that is. It starts by outlining the origins and development of cognitive anthropology. Section ii shows why the field dropped out of sight, even though highly relevant research was still going on. Sections iii and iv discuss teamwork in navigation and cross-cultural aspects of aesthetics, respectively. The history of ideas about cultural evolution is sketched in Section v. Finally, Section vi considers religion: its universality, and its underlying psychological mechanisms. It also asks why some 'chains' of communication are stable enough to avoid turning into a game of Chinese Whispers: were that not so, culture would be impossible.

8.i. Anthropology and Cognitive Science

Work labelled "cognitive anthropology" started in the early 1960s, and the closely related "ethnoscience" even before that. It was seen as a key aspect of the incipient cognitive science and an inescapable part of its future.

Admittedly, there were sceptics right from the start: we've already noted Ulric Neisser's charge that a computational approach must be "indifferent to culture" (Chapter 6.v.b). Moreover, anthropology wasn't one of the disciplines listed on the dust jacket of the first book devoted to "cognitive science" (Bobrow and Collins 1975). Nevertheless, the editor of the series—entitled Language, Thought, *and Culture*—was himself an anthropologist (Eugene Hammel). And Marc de Mey, writing in the early 1970s, included it in *The Cognitive Paradigm* (published ten years later), pointing out that rituals and institutions are the expression of representational systems (belief systems) that define a world and a way of life (1982, p. xv).

Moreover, in 1978 anthropology was officially identified as one of the six major dimensions of the field. Its place in the cognitive pantheon seemed assured.

a. The beginnings of cognitive anthropology

The scent of cultural questions was wafting in the air from the very earliest days of cognitive science. The cybernetics movement at mid-century had attracted the anthropologist Gregory Bateson, an ex-student of the schema theorist Frederic Bartlett (5.ii.b). He described cultures as self-balancing systems capable of containing 'contradictory' multilevel relationships (e.g. Bateson 1936). And the maverick Gordon Pask did some work on criminal subcultures that one might describe as nascent urban anthropology (4.v.e).

What's more, anthropologists in the 1950s were starting to move away from purely descriptive ethnography towards more theoretically based approaches—some cognitive, some not (D'Andrade 2000). So the ethnoscientists of the late 1950s tried to unite anthropology with formal linguistics.

They sought semantic equivalents of the finite set of phonemes which—as had recently been discovered—constitute all human languages (see Chapter 9.v.d). So, for instance, cultural anthropologists such as Ward Goodenough (1956, 1965), Floyd Lounsbury (1956), Benjamin Colby (1966), and David Schneider (1965) asked whether a componential analysis could capture kinship terms in every natural language—including the terminologies used by their fellow Americans, from Yankees to Pawnees.

In the early 1960s, psychology entered the equation. This wasn't unexpected. Bruner's Harvard mission station was founded in 1960 with the intention that anthropology should feature strongly in it. He'd been interested in the subject ever since his days at Duke University, where he'd heard William McDougall's tales of the famous 1898 Anthropological Expedition to the Torres Strait (Bruner 1983: 133–6). And on his arrival at Harvard, he'd encountered several leading anthropologists—notably Clyde Kluckhohn (1905–60), who'd done pioneering work on the Navaho.

Kluckhohn also helped found the Department of Social Relations, Bruner's interdisciplinary 'home' (Parsons and Vogt 1962). The Department would train many now-famous anthropologists. These ranged from Clifford Geertz (1926–), who eventually became strongly *anti-cognitive* (Geertz 1973*a*), to his close contemporary A. Kimball Romney (1925–), one of the first to encourage cognitive anthropology (Romney and D'Andrade 1964). Indeed, Romney trained and/or influenced "nearly a third of the people who have done significant work" in the field (D'Andrade 1995*a*: 245).

So Bruner's Center for Cognitive Studies included anthropologists as valued members from the start. During my stay there (1962–4) Franz Boas, Edward Sapir, and Benjamin Whorf were frequently mentioned, and Humboldtian questions about culture, language, and thought were rife (Chapter 9.iv).

Another name in the air was A. Irving Hallowell (1892–1974), an expert on Native American tribes who'd long considered not only how cultures appear from the outside but how they're experienced by—and how they affect—the people living in them. That is, he'd generalized 'Humboldt–Whorfism' from language to personality. A few years earlier, in a book of essays celebrating his sixtieth birthday, he'd called for a new field of *ethnopsychology*, which would study "concepts of self, of human nature, of motivation, [and] of personality" in different cultures (Hallowell 1955: 79). The Department of Social Relations contained a number of people—not least, Gordon Allport, Henry

Murray, and David McClelland—deeply interested in personality theory, and some of them had regular contacts with the Center.

So the many 'cultural' questions heard in the corridors hadn't *originated* from the mind-as-machine hypothesis. But they'd been *sharpened* by the formalist ethnoscientists. And they were soon to be *broadened* with the help of the computational ideas of the Center's co-founder George Miller.

Miller's brainchild, the MGP manifesto (see 6.iv.c), inspired one of the first examples of cognitive anthropology, and one of the most widely cited (A. F. C. Wallace 1965). Indeed, Miller *et al.*'s *Plans and the Structure of Behavior* was the only work mentioned in this paper's References, except for three by the author himself.

Instead of MGP's "hammering a nail", Anthony Wallace (1923–) used "driving to work" as his main example. He described this activity as guided by plans, or rules, of different types—some drawing on very general knowledge, some on the cultural context, and some on personal experience. The Image, one might say, was being attended to at last (see Chapter 6.iv.c).

The driver's "cognitive map", according to Wallace's account, is many-levelled. It represents (for instance) routes and landmarks. It codes stop signs, traffic lights, and places—such as schools and shopping malls—where children may dart out into the road. It covers visibility, weather, traffic level, and road conditions. And it includes the tools of the trade (clutch, indicators, gear shift . . .), the bodily actions required to operate them, and the driver's comfort when doing so.

"The simplest model of how this total process operates", said Wallace, "is to consider the driver as a cybernetic machine" (1965: 287). He was noting the need for monitoring (of self, vehicle, and environment) and feedback—both of which had been included within the TOTE unit. His nine rules for "Standard Operating Procedure", seven outside-of-car dimensions (allowing 216 combinations), and five car controls (allowing forty-eight combinations of actions for "unitary response") in effect defined a TOTE hierarchy, with "choice points" at specified junctures.

Wallace suggested that analysis in terms of Action Plan, Action Rules, Control Operations, Monitored Information, and Organization could explain other tool-using activities, even including hunting and warfare (p. 291). Whether it could also capture "behavior in social organizations", he said, "remains to be seen" (p. 292).

Wallace wasn't the only one to embark on a cognitive anthropology. A multi-authored collection of papers under that label appeared in 1967, and was soon reissued (Tyler 1967), and others appeared in due course (e.g. Dougherty 1985). "Mathematical" cognitive anthropology was heralded too (Ballonoff 1974), and information/feedback theory used to distinguish the idea systems and subsystems—religious, economic, kinship, marriage . . .—accepted in Punjab (Leaf 1972). (The use of computers in anthropology had been the topic of a conference as early as 1962; however, this discussion included some decidedly *non-cognitivist* approaches, such as HOMUNCULUS: see Hymes 1965 and Chapter 7.i.c.)

The early 1970s saw a few anthropologists sketching "culture grammars" (B. N. Colby 1975). These were intended to capture underlying structural possibilities, much as computational psychologists were trying to do for stories (Rumelhart 1975). And in 1977 the Society for Psychological Anthropology (SPA) was founded, under the umbrella of the American Anthropological Association (AAA).

By this time, however, Wallace's concerns for material culture (traffic lights and gear shifts) and social institutions (shopping malls and traffic laws) had faded into the background. They'd be resuscitated later, as we'll see in Section iii. Meanwhile, the focus had turned to language, and especially to classification.

Wilhelm von Humboldt's view (9.iv) that culture is primarily represented in, and learnt from, *language* had seemingly triumphed. So much so, indeed, that when one Wallace sympathizer in the early 1980s started writing a book on his field, he became "disillusioned" and lost interest in the project (E. L. Hutchins 1995, p. xii; see Section iii, below). However, that was because he favoured a different focus of research, not because there was no cognitive anthropology being done.

In fact, a lot of work had been done—and cognitive psychology was a close partner in the enterprise.

b. Peoples and prototypes

In the late 1960s and 1970s, cross-cultural studies of the structure and processes of thought blossomed (e.g. Warren 1977). Most of the interest was in whether some ways of conceptualizing the world are psychologically 'natural', and therefore universal, or whether categorization is basically arbitrary and culture-dependent.

The novelist Jorge Luis Borges had presented an "ancient Chinese" taxonomy of the animal kingdom:

> [Animals] are divided into (a) those that belong to the Emperor, (b) embalmed ones, (c) those that are trained, (d) suckling pigs, (e) mermaids, (f) fabulous ones, (g) stray dogs, (h) those that are included in this classification, (i) those that tremble as if they were mad, (j) innumerable ones, (k) those drawn with a very fine camel's hair brush, (l) others, (m) those that have just broken a flower vase, (n) those that resemble flies from a distance. (Borges 1966: 108)

This was obviously a joke: no such taxonomy could exist. But why? What, exactly, was 'unnatural' about it?

An answer was offered by the Berkeley psychologist Eleanor Rosch (1938–) (Rosch 1978: 27), and by two of her Berkeley colleagues: the anthropologists O. Brent Berlin and Paul Kay. (Kay later transferred from the Department of Anthropology to Linguistics; both he and Rosch had been trained in the interdisciplinary halls of Social Relations at Harvard.) Their work was hugely influential, and brought psychology and anthropology closer together.

Berlin (1936–) and Kay (1934–) had been working on colour names throughout the 1960s. They hoped to use colour to resolve the debate about linguistic relativity, aka the Whorfian hypothesis.

Comparing vocabularies in nearly a hundred different languages, they'd identified startling differences in colour classification—and in the experience of colour (O. B. Berlin and Kay 1969). For example, native English speakers see blue and green as 'obviously' similar—but some cultures don't. So far, it might have seemed, so Whorfian. But they'd also found interesting similarities, presumably due to inborn biological mechanisms—a very non-Whorfian idea.

For instance, when asked to identify the just-noticeable differences between patches of colour, people show no cultural differences. Even more strikingly, some colours seem to be ‘basic’ to all languages—and some are more basic than others. As Berlin and Kay put it, their “major findings” were:

> (1) the referents for the basic color terms of all languages appear to be drawn from a set of eleven universal perceptual categories, and (2) these categories become encoded in the history of a given language in a partially fixed order. (1969: 4–5)

Accordingly, the subtitle of their 1969 book on colour terms referred to *Their Universality and Evolution*. Both key hypotheses were later refined, thanks to the—continuing—explosion of interdisciplinary research inspired by their work. But universality and evolution remain crucial to anthropologists’ understanding of colour (P. Kay and Maffi 1999).

Rosch (then publishing as Eleanor Heider) was one of the first flames in that explosion. At the start of the 1970s she continued Berlin and Kay’s line of thought by doing cross-cultural experiments (Rosch/Heider 1971, 1972; Rosch/Heider and Olivier 1972). Working with the Dani tribe of New Guinea (now West Papua)—who have only two colour terms, ‘translated’ as *cool/dark* and *warm/light*—she found that colour grouping and memory for colours are indeed influenced by language. Nevertheless, her results implied—as Berlin and Kay had suggested—a universal base underlying the cultural variety. Certain (“focal”) colours are perceptually salient: they attract the infant’s attention more readily, their names are more easily learnt and remembered, and they’re more likely to have special names in the language.

She soon extended her research to categorization *in general* (Rosch 1973, 1975, 1978; Rosch and Mervis 1975). Her theory of “prototypes” identified various ways in which Borges’s imaginary taxonomy was psychologically absurd. And it provided a more realistic view of concepts than BGA had done (Chapter 6.ii.b).

For example, it confirmed the common-sense intuition that a robin is a ‘better’ example of a bird than is an owl or an ostrich. She even measured the difference: her experiments showed that a robin has a typicality rating of 1.02, an owl 2.96, and an ostrich 4.12 (1978: 36). On the BGA property-list view, this made no sense: a bird is a bird, is a bird . . . Robins and ostriches are *equally* birds, even if robins are much the more familiar.

Prototypes were relevant to AI too, for they explained various knotty aspects of knowledge representation. Consider the oft-cited AI example “Birds fly, but emus don’t”. Rosch’s description of prototypes could defuse the apparent contradiction, and justify the default assumption that—in the absence of information to the contrary—if something is a bird, then it flies. Similarly, the theory helped to explain why the frame problem can arise in respect of virtually any natural-language word, or common-sense reasoning (see 10.iii.e and 7.iii.d).

But as well as describing prototypes, Rosch offered an explanation of why they are as they are. The categories used within any culture, she said, have been developed according to two universal psychological principles. One is “cognitive economy”, whereby the strain on memory and information processing is minimized. (Bounded rationality, again: see 6.iii and 7.iv.)

For example, hierarchical concepts prevent discriminations being coded unnecessarily. Why think/speak of *cats and dogs and birds and fish and flies and* . . . when one can think/speak simply of *animals*? If the properties one’s interested in can be predicted by *animals*, then the high-level term is an economical way of storing that information. (A warning: Rosch often said she was talking about the "structure" of information, not its "processes". She wasn't a *computational* psychologist, and later co-authored a radical critique of mainstream cognitive science: Varela *et al.* 1991.)

The other general principle is "perceived world structure". Whereas BGA's concepts were defined by arbitrary sets of geometrical features, everyday concepts are based on our perception of real structures in the real world. If we see feathers we expect wings, because feathers and wings really are highly correlated. So the concept of *bird* is 'natural' (and culturally widespread), whereas a concept defined as *suckling pigs and stray dogs* isn't. Granted, culture can sometimes provide the correlations. If it were the custom, perhaps justified by a particular myth, to provide a portion of suckling pig to every stray dog encountered on the street, a concept covering the two animals might well develop.

"Prototypes" were Rosch's explanation for a host of psychological phenomena that had been noticed previously (by Ludwig Wittgenstein for instance: 9.x.d) but which she detailed more fully. In summary, concepts don't have clear-cut boundaries—again, compare BGA. A concept is actually a 'cloud' of slightly different concepts (each one defined within a large set of discriminatory features) centred on a paradigm case, or prototype. More accurately, there may not be a single paradigm case: *several* items might have typicality ratings very close to 1. Poker and tennis, for example, are both more typical examples of *game* than patience is, so are more likely to act as paradigms (cf. Wittgenstein 1953: 31 ff.). A prototype is a statistically central measure, which may or may not correspond to an actual representation.

Prototypes aid cognitive economy, because prioritizing *one* (relatively central) case within a continuously varying set helps us to think quickly and decisively. For example, Rosch showed that people can judge whether something is or isn't a bird more quickly if it's a robin than if it's an owl or an ostrich.

To put things the other way around, investigating the judgement speeds of people in a given culture provides one with a key to the semantic structure of their conceptual networks—and their practical priorities. The hierarchical level at which the prototype is found is the one which provides the most useful information—namely, the most inclusive level that captures the structure of attributes perceived in the world. ("Perceived" is important: city-dwelling children were found to have a less differentiated concept of *tree* than rural children do.)

In short, Rosch claimed that the basic discriminatory features for all concepts are shared. It's their grouping into classes which is culture-dependent. So by the late 1970s she felt that Whorf's linguistic relativity (9.iv.c) must be taken with a very large pinch of salt:

> When many of us first came in contact with the Whorfian hypothesis, it seemed not only true but profoundly true . . . (Rosch 1977: 95)

> At present [however], the Whorfian hypothesis not only does not appear to be empirically true in any major respect, but it no longer even seems profoundly and ineffably true. (Rosch 1977: 119)

Many anthropologists were intrigued. Her theory raised a host of questions about which features are basic, what prototypes are favoured by different cultures, and how their concepts are structured.

Berlin, for instance, was by this time gathering data on "folk biology": how various cultures classify plants and animals (O. B. Berlin 1972, 1974; Berlin *et al.* 1973). He saw Rosch's work as highly relevant in asking to what extent biological categories are culture-dependent (O. B. Berlin 1978: 9, 24). That opinion is still widely shared by cognitive anthropologists today.

c. Hopes and a hexagon

In the late 1970s, when Berlin and Rosch were prime names to conjure with, the early hopes for anthropology as a key player within cognitive science were still strong.

That's clear from Howard Gardner's (1943–) widely read book *The Mind's New Science: A History of the Cognitive Revolution* (1985), which contained an entire chapter on anthropology. First, Gardner outlined the contributions of Edward Tylor (1832–1917), Boas, Sapir, and Lucien Lévy-Bruhl (1857–1939). Then, he discussed more recent work—especially the structuralism of Claude Lévi-Strauss (1908–) and the componential analysis of ethnoscience (pp. 236–57). He also touched briefly (pp. 242 ff.) on the views of Geertz and Daniel Sperber (1942–)—of whom, more below.

This highlighting of anthropology wasn't an idiosyncratic decision on Gardner's part. His book was sponsored by the Sloan Foundation, being one of the many projects they funded under the banner of "cognitive science" (6.iv.f). As such, it would have been unthinkable not to give space to anthropology. For the Report submitted to the Foundation, when they were considering (from 1976) whether they should give hugely generous support to the field, had picked out anthropology as one of the six key disciplines (State of the Art Committee 1978).

This Report was written by experts, not bureaucrats. Those responsible included Michael Arbib, George Miller, Donald Norman, Zenon Pylyshyn (all mentioned in Chapters 6 and/or 7), the psycholinguists Joan Bresnan and Ronald Kaplan (see Chapter 9.ix.b)—and the San Diego anthropologist Roy D'Andrade (1931–), yet another graduate of Harvard's Department of Social Relations.

The common research objective of cognitive science, they declared, was *to discover the representational and computational capacities of the mind and their structural and functional representation in the brain* (p. 76). Their 'Sloan hexagon' (Figure 8.1) represented anthropology as no less crucial to this quest than Psychology, Philosophy, Linguistics, Neuroscience, and Computer Science.

Moreover, "Anthropological linguistics" and "Cognitive anthropology" were identified as essential subdomains. And both featured in the key example. This example, which comprised some 50 per cent of the Report, described the recent "transdisciplinary" findings on "the names we give to colors"—including, of course, those due to Berlin (pp. 76 ff.).

To be sure, not everyone accepted the Sloan Report's definition of cognitive science. In fact, it aroused such virulent disagreement that it wasn't officially published until some years later (6.iv.f). This isn't surprising: as remarked at the outset of Chapter 1, there's *still* no agreed definition of the field. In the late 1970s as now, different basic assumptions and methodologies were favoured by different individuals.

Moreover, this was hardly a disinterested debate. With $15 million at stake, people were seeking to cultivate their own garden patch. The committee members were all

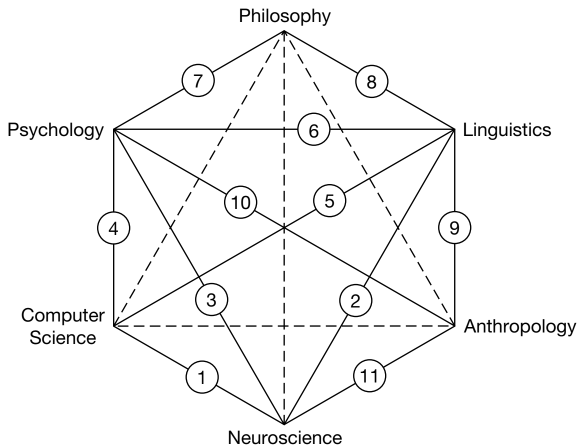


Fig. 8.1. The Sloan Hexagon (designed in 1978). Original caption: Subdomains of Cognitive Science: 1. Cybernetics. 2. Neurolinguistics. 3. Neuropsychology. 4. Simulation of cognitive processes. 5. Computational linguistics. 6. Psycholinguistics. 7. Philosophy of psychology. 8. Philosophy of language. 9. Anthropological linguistics. 10. Cognitive anthropology. 11. Evolution of brain. Redrawn with permission from Machlup and Mansfield (1983: 76)

highly respected cognitive scientists, and they'd taken formal advice from over twenty others—not to mention their many informal contacts and Sloan-relevant meetings. But they didn't include any specialist in straight computer science, or even AI. Disgruntled computer scientists felt that their discipline had been underplayed, even though AI was included in the hexagon.

When the money started flowing, disagreements—and jealousies—multiplied. There was a widespread (though not universal) welcome for Sloan's opening strategy of distributing a large number of small grants to many different places. But when the few large-scale grants were made, tempers rose. (The key AI departments, of course, had become used to receiving ARPA/DARPA largesse, originally provided by Joseph Licklider: Chapter 10.ii.a.)

But if the Sloan Report was controversial, it wasn't perverse. Anthropology was seen in this important document as a key part of the overall endeavour—and very few, if any, of the intended audience were unhappy with that. They might want more money for their own pet discipline, but they wouldn't have dreamt of arguing that anthropology didn't deserve any.

d. More taxonomies (and more Darkness than light)

Encouraged by the Sloan accolade, work in cognitive anthropology continued throughout the last quarter-century. Research on classification was the first to advance.

Kinship—a traditional focus of anthropologists' concerns—was joined as an object of interest by many other classificatory systems. Studies of ethnobotany, ethnozoology, ethnopsychology, folk medicine, and folk theology (for example) burgeoned. Each of these involved fieldwork in various cultures, to gather the necessary data.

Today, Rosch's theory still provides a theoretical basis for analysis and cross-cultural comparison (cf. Ellen 2003: 52). However, some anthropologists now believe that her results weren't due simply to statistical patterns in the environment: more robins than ostriches, and *always* feathers. For they see classifications and folk taxonomies as resulting also from inborn predispositions to think in terms of "essences" for natural kinds. In this, they're picking up on the current fashion for positing "modules" in evolutionary psychology (see Chapter 7.vi.d–e, and iv.a below).

(That fashion is regarded as mistaken by some psychologists. For instance, the influential connectionist James McClelland argues—backed up by computer modelling—that apparently "innate" domain theories could arise from familiar types of connectionist learning: McClelland and Rogers 2003; T. T. Rogers and McClelland 2004. His argument is closely comparable to his connectionist assault on Chomskyan innate grammar, which caused a sensation in the mid-1980s: see Chapter 12.vi.e.)

The anthropologist Scott Atran (1952–), for instance, says:

> Humans, let us suppose, are *endowed* with highly articulated cognitive faculties for "fast-mapping" *the world they evolved in, and for which their minds were selected*. The "automatic" taxonomic ordering of phenomenal species, like the spontaneous relational ordering of colors, would then be a likely product of one such faculty. (Atran 1990: 65; italics added)

> Humans appear to be inherently disposed to classify living things according to presumptions about their underlying physical natures. Cross-cultural evidence indicates that people everywhere spontaneously organize living kinds into rigidly ranked taxonomic types despite wide morphological variation among those exemplars presumed to have the nature of their type. (pp. 70–1)

The remark about "rigidly ranked taxonomic types" is over-optimistic. For it's now clear that Berlin underplayed the variation among folk biologies. He tried to assimilate all taxonomies of living things to a hierarchical, quasi-Linnaean, model. But it's turned out that not all classificatory systems are like that (Ellen 2003, esp. 53–5). Atran's other claim, however, may be better grounded. Certainly, his reference to "the world [we] evolved in" is an example of a form of thinking that's recently become common within cognitive anthropology (see Sections ii.d–e and iv–vi, below).

The explosion of research in ethnobiology would probably have happened anyway, given Berlin's provocative start. But it benefited from funding led by medical/pharmaceutical interests, and sometimes by the pharmaceutical companies themselves. This high-tech industry was now anxious to consider the previously scorned indigenous knowledge of local flora and fauna—not least, because the relevant species were rapidly disappearing from the rainforests.

Whether such research involved exploitation, deceit, political knavery, and worse became a topic of controversy worldwide at the millennium. For at that time Patrick Tierney published his outraged—and outrageous—book *Darkness in El Dorado* (2000).

He charged two anthropologists who'd worked with the Yanomami in the Amazon forests—James Neel (1915–2000) and Napoleon Chagnon (1938–)—with all the sins mentioned above. He even added deliberate genocide, claiming that Neel and Chagnon had knowingly caused "hundreds, perhaps thousands" of deaths through improper use of a measles vaccine. Most scandalous of all, this was said to be not a tragic mistake but a deliberate experiment to test Neel's "fascistic eugenics".

Tierney wasn't chasing minnows. Chagnon was described by the University of California's (Santa Barbara) Center for Evolutionary Psychology as "perhaps the world's most famous living social anthropologist" (Tooby 2000*a*). Neel had recently died, as had Tim Asch, the ethnographic film-maker attached to the team. But he'd been a physician and "a founder of modern medical genetics". (And, for the record, a fierce opponent of eugenics for sixty years: Tooby 2000*b*.)

This caused "the most sensational scandal to emerge from academia in decades" (Tooby 2000*b*). Tierney's accusations were vociferously spread by many anthropologists, including some officials of the AAA—which had known about them from the early 1980s. But they were just as hotly disputed by others. John Tooby, in his role as President of the Human Behavior and Evolution Society, checked them thoroughly and found them to be utterly "fraudulent" (Tooby 2000*b*). Admittedly, he was *parti pris*, since Chagnon was a leading member of the Society. In the end, however, they were officially declared groundless in a report published by the University of Michigan, Neel's former employer (Cantor 2000).

Moreover, the AAA admitted that they'd been "unable and unwilling . . . to address them in a fair and open manner" (Albert *et al.* 2001). (They didn't actually rescind their criticisms, however: in February 2005, yet another attempt was made by a few AAA members to make them do so.) Meanwhile, they'd split the anthropological 'community' right down the middle. Or rather, since the two sides were by that time highly unequal in terms of numbers, it accentuated the pre-existing gulf between the 'scientific' (marginal) and the 'interpretative' (mainstream) anthropologists: see Section ii.a–c below.

The specialist ethnographers who did the wide-ranging post-Berlin research on cultural categories weren't the only anthropologists to develop an interest in patterns of concepts. For it became increasingly evident that the various classifications found within a given culture overlap significantly.

Computational explanations were given for this. It had been suggested in the early 1980s that, for reasons of informational economy, similar concepts are likely to be used for classifying both human social relations *and* non-human domains (B. N. Colby *et al.* 1980; Ohnuki-Tierney 1981). Today, there's a rich store of ethnographic data, concerning both lexical codes and 'wordless' cultural practice, to support that view. As Roy Ellen (1947–), a cognitive anthropologist at the University of Kent, has recently said:

> [All] human populations apprehend the social in terms of the natural world and the natural in terms of metaphors drawn from the social world. The two are intrinsically complementary, although in certain neurological pathologies they may conflate in unusual ways [see Chapter 7.vi.d–e] . . . The classificatory language we use for plants and animals is derived from the way we talk about genealogical relations, and we understand the functional dynamics of both organisms and ecological systems in terms of our experience of participating in social systems . . . (Ellen 2003: 50)

The topic of this book, *mind as machine*, is of course no exception. Indeed, Ellen immediately went on to point out that "technology provides many productive analogies", one of which is to see "the brain [or the mind] as a computer".

e. And modelling, too

Seeing the mind/brain as a computer became even more attractive for cognitive anthropologists in the mid-1980s, when PDP (parallel distributed processing) connectionism entered the picture (see Chapter 12.vi). For this methodology offered a way of talking about concepts without having to talk about language (grammar), and without having to adopt static, cut-and-dried definitions. Rosch's "fuzzy" concepts and "central" prototypes could be modelled at last—and with some (limited) neurological plausibility.

Maurice Bloch (1939–), at the London School of Economics, was one of the first anthropologists to welcome connectionism. He argued that it was better suited than GOFAI to model culture-specific categorizations, which typically happen very quickly:

> Since much of culture consists of the performance of these familiar procedures and understandings connectionism may explain what a great deal of culture in the mind–brain is like. It also explains why this type of culture cannot be either linguistic or "language-like". Making the culture efficient requires the construction of connected domain-relevant networks, which by their very nature cannot be stored or accessed through sentential logical forms such as govern natural language. (M. E. F. Bloch 1991: 192)

Besides rebutting the symbolist school in anthropology, who'd assimilated cultural knowledge to linguistic codes, Bloch was also defending the traditional method of participant observation. In this approach, "knowledge" (of foreign categories, for instance) is picked up by the visiting anthropologist more or less effortlessly, and typically without the informants' being able to justify their own judgements explicitly. Similarly, he said, connectionist learning doesn't depend on verbal (programmed) instruction, but on the system's tacit recognition of statistical patterns in the input examples.

But it wasn't all about connectionism, for Bloch also approved other ideas from cognitive science. In particular, he picked up on "mental models" and schema theory: the spirit of Bartlett—accompanied by those of Kenneth Craik, Robert Abelson, Marvin Minsky, Michael Arbib, and Philip Johnson-Laird—was hovering in the background. Although he criticized psychologists for downplaying the sociocultural and non-linguistic aspects of "meaning", Bloch unambiguously recommended cognitive science to his colleagues (M. E. F. Bloch 1991, 1998).

That shouldn't have been too shocking. After all, Bartlett had been dealing with 'anthropological', cross-cultural, material (see 5.ii.b). And Abelson's IDEOLOGY MACHINE had been an attempt *to distinguish and also to unify* distinct (political) mini-cultures within the USA in formal–schematic terms (7.i.c).

Schema theory and connectionism were soon used to model fieldwork done in a community closer to 'home' than usual. Claudia Strauss and Naomi Quinn, in an SPA meeting held in 1989 and in their later book (1997), provided original ethnographic data on contemporary American understandings of *marriage* and *success*. These data were considered in a new way, well fitted to describe shifting-yet-stable cultural meanings. Specifically, they were analysed in terms of schema theory, connectionism, and distributed cognition ("shared task solutions").

Quinn (1987, 1991) had earlier found that all the many metaphors used by her hundreds of informants fell into one of eight classes: sharedness, lastingness, mutual benefit, compatibility, difficulty, effort, success/failure, and risk. The relations between those classes (e.g. the potential contradictions between lastingness and mutual benefit) guide Americans' reasoning about marriage. It's guided also—and complicated—by three different ways of thinking about individuals:

* as a human being, with rights, obligations, and capacities;
* as a bearer of a social role, such as spouse, parent, or teacher;
* and as someone with personal characteristics, such as generosity or idleness.

Further complexities are added by the varying emotional commitments to the eight features on the part of different individuals (and of the same individual at different times). In short, there are multiple, and sometimes mutually conflicting, constraints to be borne in mind.

It's no accident, then, that when PDP connectionist models became widely known, in the late 1980s, Strauss and Quinn chose them as a way of exploring their hypotheses. For PDP is specifically concerned with multiple constraint satisfaction (Chapter 12.v–vi).

This approach could be used to model the thoughts—and emotional weightings—of individual informants (Quinn had recorded many verbatim quotations). It could also be used to explore possible constellations of American 'marriage thinking' in general. In short, connectionism offered a way of testing the authors' hypotheses that *these* schemas, inferential patterns, and beliefs were in play. (The informants themselves weren't usually able to identify them by introspection.)

Another study of shared task solutions, not in marriage but in seamanship, also used connectionist modelling (see Section iii.a, below). Here, PDP was valued for its ability to model not only fuzzy judgements, but also different patterns (and persuasiveness) of communications between one person and another.

By the 1990s, cognitive science—sometimes, with psychodynamic theory—was providing much of the theoretical inspiration for psychological approaches to culture (D'Andrade and Strauss 1992). For instance, Strauss (1992) described how differentially salient marriage-relevant schemas such as *success* and *breadwinner* can direct behaviour in American blue-collar workers. She showed that what directs one worker isn't exactly the same as what directs another, because even people in the 'same' subculture think about their life-world somewhat differently. Accordingly, she argued that anthropologists must recognize semantic organization at the personal (as well as the cultural) level.

At much the same time, at Emory University, Georgia, cognitive science was being called on to provide yet another definition of "culture". Shore (1945–), a long-time ethnographer of Samoa (Shore 1982), redefined cultures as distinct sets of mental models (Shore 1996). In explaining what he meant by this, he drew inspiration from Bartlett's work on schemas (5.ii.b), Erving Goffman's (1959, 1967) descriptions of the tacit conventions of everyday life, Roger Schank and Abelson's concept of scripts (7.i.c, 9.xi.d), and Johnson-Laird's mental models (7.iv.d–e).

It's mental models, said Shore, which distinguish the construction of meaning from mere information processing (1996: 157). They cover every aspect of cultural life, whether linguistic or non-linguistic. So he specified examples at several different scales of experience, running from bodily postures and table manners all the way to myth, marriage, and religion. They weren't seen as 'purely' cognitive. To the contrary, they were said to underlie, and help construct, the specific motivations and emotions typical of the culture concerned. (In that sense, they resembled William McDougall's notion of *sentiment*: see 5.ii.a.)

Samoan *alofa*, for example, is an attachment emotion whose socio-psychological functions differ significantly from those of Westerners' *personal love* (Chapter 7.i.f). For instance, it's *alofa*—according to what the Samoans themselves say—which motivates them to adopt relatives' children, and even to give up their own babies willingly for adoption by kinfolk. (My own Polynesian friends, in the Cook Islands, often speak of someone's "birth mother" and "feeding mother".) This widespread Polynesian custom may have evolved, as functionalist anthropologists would put it, to distribute scarce resources efficiently. But the point here is that the concept, or mental model, of *alofa* plays an explanatory and motivational role that's intriguingly different from the Western concept of personal love.

Similarly, Samoan grief at the death—specifically, the murder—of a loved one is different from ours, though recognizable as grief (Shore 1982, ch. 1). Samoan 'apology' or 'reparation' for a murderous act on the part of a family member is not what one would see in the West. And Samoan anger at a loved one or authority figure is different from ours. As in other parts of Polynesia, it's not socially acceptable for such anger to be expressed directly. Moreover, Eleanor Gerber (1985) has shown that it's not conceptualized or experienced in the same way that 'Western' anger is. (Gerber's work is a fascinating example of the 'ethnopsychology' called for in the 1950s by Hallowell: see above.)

In Shore's terminology, the mental models of attachment and anger that are prevalent in Samoa differ from ours. Individuals in a given culture share a particular set of mental models. The cultural community may be a Samoan village (the focus of his earlier work), a Hopi or Navaho gathering, or a US high school or baseball team. The last four examples were among the communities studied by his Center for Myth and Ritual in American Life (MARIAL), set up at the millennium and funded—like so much else in cognitive science—by the Alfred P. Sloan Foundation. Like Strauss and Quinn, then, he was now looking close to home for much of his ethnographic data.

As well as studying specific cultures or subcultures, Shore *compared* them, in terms of the extent to which their members share the same mental models. He even claimed that *a common biological context* underlies emotions (such as love and *alofa*) conceptualized—and practised—by different cultures in different ways. In short, he was highly unorthodox.

He'd already criticized the dominant (Geertzian) anthropological approach for being too "particularistic": its methods of *local* interpretation, he said, can't show the extent to which meanings are, or aren't, cross-culturally shared (Shore 1988). If there is any universal base to human cultures (a suggestion firmly avoided by most of his fellow professionals, as we'll see), these methods wouldn't find it. At the millennium, in an essay on globalization (Shore 2000), he not only made many cross-cultural comparisons but committed the cardinal sins of rejecting cultural relativism, and of criticizing certain aspects of some non-Western cultures. He even said, in opposition to the sort of 'identity' politics that perfused the simultaneously published *Darkness in El Dorado*, that some of these aspects aren't worthy of preservation, and would be better lost.

Just how unorthodox this was will become clear in Section ii.b–c. Here, it's worth noting that Shore's plea for a more scientific anthropology—note his 1988 title: "Interpretation Under Fire"—didn't spring from a lack of knowledge or sympathy. His BA had been in English at Berkeley, and his early ethnographic work (based on a two-year stay in Samoa undertaken even before he turned to academic anthropology) was done in an interpretative spirit (Shore 1982). Moreover, he was deeply influenced by Bruner, who moved towards the interpretative side of anthropology in the last quarter-century (6.iv.d).

But Shore's interpretations, and his teaching, were based on huge amounts of data:

> [The students were expecting] a lot of anecdotes about my personal experiences, and then a vague account about empathetic insights, what Weber called *Verstehen*, that comes from just living with people. Interpretation as a kind of intuitive speculation. Well there is that of course, but what I brought in were piles of genealogies, demographic data, detailed data on political titles and their holders in a Samoan village, hundreds of questionnaires and the data analyses that resulted from them and of course thousands of pages of interview transcripts. I just went on and on until the students looked at me completely baffled and said, "This is symbolic anthropology?" And I said "Well", I said, "You know, symbolic anthropology does have data, and because of the problems of interpretation and the ease with which we can read into things—I mean, there's no question that human beings with a great imagination can read any interpretation they want into virtually any text—we hold ourselves all the more accountable to data. (Shore 2004: 72)

In addition, he insisted that we sometimes need what Bruner called "the recognition of the right answer versus the wrong answer". To be sure, Samoan *alofa* and Western *personal love* form part of different discourses, which can't be neatly mapped onto each other. But anthropology needs *theories* as well as "conversations":

> Rather than trying to sort of resolve disagreements, the tendency [among anthropologists] is to reframe the issues as part of different discourses, and wall them off from one another. [... This] strategy leads to a kind of rejection of theorizing itself ... It's one thing to show a kid that her wrong answer is the right answer to another question, and quite another to suggest that all issues of disagreement are really simply to be handled by a kind of post-modern bunker mentality in which each question is simply dismissed as part of a different discourse. At an earlier time we would have said that we were simply playing different [Wittgensteinian] language games. More recently we use Richard Rorty's image of simply different conversations. They all share the notorious post-modern resistance to meta-narratives [theories], or in a way to going meta [generalizing] at all. (p. 154; cf. 1.iii.b)

Shore, then, had a respect for "scientific" methods (and even for "scientific" explanations) as well as hermeneutic ones. In fact, he was a rare case of someone sympathetic to both sides of the intellectual divide within the profession (see ii.a–c, below.)

(Another rare case is the philosopher of science Nicholas Maxwell, whose account of science as "wisdom" provides a strong counterpoint to value-blind scientism: 1984, 2004. Significantly, he considers science as a whole—his own scientific expertise is in quantum physics. If the self-styled scientific anthropologists had been familiar with his work, they might have been able to deflect some of the odium of their interpretative colleagues without risking a descent into postmodernism: see Section ii.c, below.)

Science-based discussions were going on *outside* professional anthropology, to explain how cultures are possible in the first place. For instance, the primatologist Michael

Tomasello (1999) cited Theory of Mind and RR theory (representational redescription) as essential prerequisites of "culture" properly so-called (Chapter 7.vi.e and h–i). These mechanisms, he argued, enable humans to acquire, develop, and transmit culture—which non-human animals can't do. Many other evolutionary psychologists (*sic*) were trying to explain cultural phenomena (see Sections ii.d–e and iv–vi, below). Despite many disputes about detail, they had the support of their professional peers. Shore didn't: he was swimming against the tide.

However, he wasn't the only one. By the 1990s, there'd been enough work in cognitive anthropology to justify a history of it—written by a one-time member of the Sloan Committee (D'Andrade 1995*a*). (There'd also been a survey of computer-based research, but this dealt more with ethnographic data processing and mathematical modelling than cognitive simulation: Fischer 1994.)

To cap it all, the relevance of computational theorizing—and of computer models—was confirmed in a mid-1990s encyclopedia article. Colby, a pioneer of culture grammars in the early 1970s (B. N. Colby 1975), advised intending cognitive anthropologists to study various AI skills. These included knowledge representation, NLP, connectionism, and symbolic logic (B. N. Colby 1996: 214–15).

Today, encyclopedia articles on this topic may mention a recent addition to the AI-modelling stable: agent-based distributed cognition (see Chapter 13.iii.d–e, and cf. Section iii.b below). One such, written by David Kronenfeld (1941–), the first anthropologist to try swimming in these waters (Kronenfeld and Kaus 1993), concludes by saying that "agent-based computational models seem to offer one very promising method" for understanding culture, and especially the relation between individual and shared schemas (Kronenfeld 2004*a*). Another, by the same author (2004*b*), also ends by recommending computer models—and mentions the online publication *Journal of Artificial Societies and Social Simulation*.

Known as *JASSS*, this was begun in 1998. Another online journal in this area, founded just a year later, is *MACT*: *Mathematical Anthropology and Culture Theory*. *MACT* has organized sessions on 'Cultural Systems' and 'Cognitive Clarity' at Vienna's European Meeting on Cybernetics and Systems Research since 2002, and many *MACT*-initiated papers appear in the EMCSR journal *Cybernetics and Systems*.

Apart from the online journals (for no one could have predicted the Web), that was just what the early cognitive scientists would have expected. For links with both AI and cybernetics had been on the cards since the beginning, as we've seen.

Maybe that counts as a happy ending. But the intellectual journey that led there was far from happy. Meeting in a smoky Viennese café during their first EMCSR conference, the *MACT* editors helped launch both the Salon des Refusés and SASci (Paul Ballonoff, personal communication)—of which, more below. Neither of these would have been needed if things had gone to plan. In other words, if the expectations implicit in the Sloan hexagon had been fulfilled there'd have been no Salon, and no SASci.

8.ii. Why Invisibility?

One might imagine, given the history sketched above, that anthropology and cognitive science hopped into bed together in the late 1950s and remained happy bedfellows ever after. They didn't. Hopes were high at the beginning, but as time went by the two were mentioned in the same breath ever more rarely.

Why? How did anthropology come to be described by a leader of cognitive science as a mere "trace substance"? What happened, to make it vanish?

There are two answers. On the one hand, the anthropologists themselves—well, most of them—decided to give the cold shoulder to psychology in general, and cognitivism in particular. On the other, much of the relevant research was renamed—and the new label didn't include the word "anthropology".

These changes were due to two very different intellectual movements of the late twentieth century, each of which spread way beyond anthropology. The first started in the 1960s and blossomed in the early 1970s. The second came soon afterwards. By the end of the century, cognitive anthropology was travelling in the shadows, largely incognito.

a. Psychology sidelined

Anthropology's current invisibility can be superficially explained by the fact that cognitive anthropologists remained rare beasts. The history of the field mentioned above reported that "most of the work has been carried out by a shifting core which has never been larger than about 30 persons" (D'Andrade 1995*a*, p. xiv). If more British names had been included, the number would have risen—but not much. Evidently, departments of anthropology weren't good breeding grounds for this species of animal.

Few university researchers means few students to pick up the baton. So in the absence of a critical mass, cognitive anthropology couldn't flourish. But why was there no critical mass? The excitement in other areas of cognitive science was enormous, so why not in anthropology too?

The reason is indicated by two key chapter titles in Strauss and Quinn's end-of-century book. This contained a defensive early chapter on 'Anthropological Resistance' and an optimistic final chapter called 'Beyond Old Oppositions'. Clearly, the authors felt that they were walking through a minefield.—And it was their anthropology colleagues who'd laid the mines.

That is, the profession's accepted career path had taken a very different route from the one endorsed by D'Andrade and his fellow authors of the Sloan Report. In the last quarter of the century, most anthropologists had little sympathy with their cognitive colleagues. They didn't want anthropology to be part of Donna Haraway's "cyborg science" (Chapter 1.i.b). Indeed, they didn't want it to be seen as intellectually dependent on (individualistic) *psychology* of any sort. That being so, studies of folk taxonomies (for example) went out of favour. As for cognitive science, anthropologists in general had no interest in being part of an interdisciplinary endeavour in which psychology was a crucial dimension.

The flight from psychology began in the early 1970s, and was led by Geertz (since 1970, at Princeton's Institute for Advanced Study). He scorned what he called "the cognitive fallacy", the idea that "culture consists of mental phenomena" (Geertz 1973*b*: 12). Goodenough's definition of culture as knowledge ("whatever it is one has to know or believe in order to operate in a manner acceptable to its members") was specifically rejected (Geertz 1973*b*: 11).

As Geertz put it to his biographer Fred Inglis (a scholar not of anthropology, but of English literature):

> I want to do more, attempt to use some of the techniques that literary critics use, that historians use, that philosophers use, to explicate cultural matters. (Inglis 2000)

The "philosophers" he had in mind weren't the analytically minded philosophers of cognitive science, but the neo-Kantians—including the postmodernists (Chapters 1.iii.b–d, 2.vi, and 16.vi–viii). Since Geertz fell within the long-standing hermeneutic tradition of social science (Sherratt 2005), his preferred method was "interpretation":

> The concept of culture I espouse . . . is essentially a semiotic one. Believing, with Max Weber, that man is an animal suspended in webs of significance he himself has spun, I take culture to be those webs, and the analysis of it to be therefore not an experimental science in search of law but an interpretive one in search of meaning. (Geertz 1973*b*: 5)

To go "in search of meaning" was to try to give a "thick" description of someone's behaviour (a term he borrowed from Gilbert Ryle: 1949). A thick description reports (for example) not a movement of a hand or an eyelid, but a greeting, or an expression of complicity.

Moreover, a wink or wave can mean very different things in different cultures: knowledge of *local* meanings is what's required. So Geertz insisted that reliable interpretation could result only from cultural immersion. The anthropologist, he said, must be a *participant* in the culture, not an *observer* of it. Most cognitivists were (and remain) appalled: "we deplore the rejection of explanation in favor of interpretation in anthropology at present" (Strauss and Quinn 1997: 9, 259).

Instead of calling for the fruitful collaboration between psychology and anthropology which the early cognitive scientists had hoped for, Geertz declared cognitive psychology to be *persona non grata*. Or rather, psychology *considered as the study of the processes going on inside the individual's head* was dismissed. Geertz's position had nothing to do with hostility to computers. (Because of their position in the science wars, outlined below, interpretative anthropologists would naturally be suspicious of computational psychology; but in principle that's a separate matter.) It was rooted, instead, in an unorthodox view of the mind.

Specifically, Geertz persuaded his professional colleagues to accept an externalist philosophy of mind (a position which became newly popular in the philosophy of mind itself a quarter-century later: 16.vii.d). He insisted that "mind" is naturally located *outside* the head, in the cultural institutions, conventions, and artefacts with which so-called individual minds are imbued. (As a one-time member of Bruner's Center at Harvard, he was in effect generalizing Bruner's views on "cognitive technologies": Chapter 6.ii.c.)

As he put it: "Thinking consists not of 'happenings in the head' (though, happenings there and elsewhere are necessary for it to occur) but of a traffic in . . . significant symbols" (Geertz 1966: 45). So not only should anthropology not be psychological, but psychology should be anthropological:

[The evolution of humans was very extended, since] the phylogenetic history of man took place in the same grand geological era—the so-called Ice Age—as the initial phases of his cultural history. Men have birthdays, but man does not.

What this means is that culture, rather than being added on, so to speak, to a finished or virtually finished animal, *was ingredient*, *and centrally ingredient*, in the production of that animal itself. . . .

Most bluntly, it suggests that *there is no such thing as a human nature independent of culture* . . .

[. . . Chartres cathedral can't be understood merely as stone and glass, but only as a creation of a specific religious culture.] It is no different with men. They too, every last one of them, are *cultural artifacts*. (Geertz 1966, sect. iii; italics added)

In other words, cultural variations are constitutive of human minds, not superficial additions to them.

Many cognitive scientists would have rejected Geertz's "most blunt" expression (that there's "no such thing" as a human nature independent of culture). But one didn't have to be a postmodernist to agree with him to a large extent. The cognitive anthropologist Shore refers to "Geertz's profound but elementary insight", and puts it like this:

The study of human nature minus culture does not produce a more basic understanding of the human but an understanding of a *protohuman*, a creature that is all bioessence but *lacking recognizable qualities of human existence*. (1996: 33; italics added)

He deliberately speaks of "culture in mind", to suggest "both an ethnographic theory of mind and a cognitive theory of culture" (1996: 13). In other words, Shore sees Geertz as having gone too far, posing a false dichotomy between "private" mind/psychology and "public" culture/anthropology (1996: 51). Rather, anthropology is "the study of human nature *through* the study of human difference" (1996: 379).

One example of the way in which Shore applies his notion of "culture in mind" is his recent definition of "techno-totemism" (1996, ch. 6). He argues—like Haraway, but in a less antagonistic spirit—that we have ways of thinking, and even concepts of humanity and of self, that are deeply informed by specific aspects of our culture's technology. Cognitive science itself, of course, is one expression of techno-totemism (cf. also Gigerenzer 1991*b*). Indeed, Shore sees "modularity" as a foundational schema of modernity (1996: 117–18), without which techno-totemism couldn't arise. And by modularity he means functional separation in general, as stressed by Herbert Simon's *The Sciences of the Artificial* (of which evolutionary "modules" are a special case: Chapter 7.vi.d–e).

By the mid-1990s even Bruner himself no longer saw anthropology and computational psychology as part of the same enterprise—and he favoured the former. Although in 1983 he'd "regretted" not having got more involved with computational theorizing (see 6.ii.d), and although he still allows that computational scientists have a role to play in "the study of man" (in Shore 1996, p. xiii), he also complains that AI has "trivialized" psychology (Bruner 1997). The remedy, he says, is to make psychology more like anthropology—and both of them less "impersonal" (2002: 111–12). Specifically, he asks for a "narrative turn", as exemplified by Geertz's interpretative anthropology—and by the Annales school of historians too.

Geertz was recently described by Bruner as "surely . . . the most cited and most often attacked anthropologist of our era" (2002: 118). But he sees him as being on the winning side: "one increasingly widespread view is that culture is as much a prod to the development of human cognition as human cognition is to the development of culture" (p. 52). He'd said that in the 1960s, of course (Chapter 6.ii.c). But his work on cognitive technologies was done in an experimental context. Now, he prefers interpretation to experimentation.

So, too, do most anthropologists. For Geertz's arguments revolutionized the field. That's partly why anthropological linguistics, so prominent in Kluckhohn's vision for the Department of Social Relations, has gone out of fashion—especially in the USA. Chomsky's influence, including his excessive emphasis on English (Chapter 9.vii.d), led to detailed studies of "exotic" languages becoming much rarer in departments of linguistics. And because of Geertz's 'literary' approach in anthropology, they're rarer there too. The focus is less on nouns than on narrative. (In Europe, this effect is less marked. Nijmegen's Max Planck Institute for Psycholinguistics, for instance, set up a "Cognitive Anthropology Research Group" to study neo-Whorfian questions about language and cognition: e.g. D. Hill 1993; Pederson 1993.)

Within anthropology (and other disciplines too), interpretation and experimentation—or anyway, objective observation—were destined to clash. As Jerry Fodor has recently said, in discussing evolutionary psychology:

> Cultural relativism is widely held to be politically correct. So, *sooner or later*, political correctness and [nativist] cognitive science are going to collide. *Many tears will be shed*, *and many hands will be wrung in public*. (Fodor 1998*c*; italics added)

In anthropology, as we'll see next, it was sooner rather than later. The collision happened years ago, and the tears have already flooded the floors.

b. Skirmishes in the science wars

Gardner, some twenty years ago, had seen the writing on the wall. While the Sloan monies were being generously distributed and thankfully spent, he remarked:

> [Much] of anthropology has become disaffected with methods drawn from cognitive science, and there is *a widespread (and possibly growing)* belief that the issues most central to anthropology are better handled from *a historical or a cultural or even a literary perspective*. (1985: 44; italics added)

He was right. An ill-tempered schism was developing within the profession, between those who were suspicious of scientific approaches, including computationalism, and those who weren't.

Anthropologists had long been used to having colleagues with different interests: the bones or the beads, one might say. But this wasn't a matter of choosing between physical and cultural anthropology. It was a fundamental philosophical disagreement about how to study culture. Specifically, it was a special case of the "science wars" mentioned in Chapter 1.iii.b–c.

Initially, the conflict was played out privately, at anthropological conferences and within university departments of anthropology. Some groups split into two as a result. Even when they didn't, the psychologically minded anthropologists sometimes fled to more welcoming homes (at Harvard, for example, largely to Education).

The battle was especially fierce in America, where many felt that an intellectual commitment to the discipline should feed a practical commitment to 'identity' politics too. (Hence the passion stirred up by *Darkness in El Dorado*.) D'Andrade sees the political radicalization of US campuses in the Vietnam war as a crucial factor in the transformation of anthropology (2000: 221 ff.). The sociologist Adam Kuper (1999) agrees, regarding the US debate as fuelled by hangovers from the civil rights movement and Vietnam. Europeans, he said, with their bitter memories of the two world wars, were less open to a politics celebrating cultural difference, less ready to regard universalist approaches as banal.

(Most non-cognitive anthropologists avoid universalism. But proponents of "reflexive" anthropology do claim that there's a universality in human emotions. For instance, one Westerner mourning his wife said he could empathize with the Ilongot headhunter in the Philippines who told him that "rage, born of grief, impels him to kill his fellow human beings": Rosaldo 1989; cf. Behar 1996. Indeed, he added that *his task as an anthropologist* was to learn to empathize with the 'alien'. Many of his colleagues argued that this approach is self-indulgent and self-deceptive, and obscures important cultural differences. The discussion of grief in Chapter 7.i.f suggests that they're right. Although there may well be some universal emotional core, grief will be experienced—not just expressed—in significantly different ways across cultures with different concepts of love, marriage, loyalty, obligation, and so forth. Indeed, the grief felt by Strauss's American informants will likely differ, to the extent that their concepts of marriage differ: see Section i.e, above.)

The fight didn't stay private for long. When the Stanford department bifurcated in 1998, and also during the rancorous run-up to the fracture, reports from the war zone reached the general public. They appeared in widely read media such as *Science* and *The Nation*, for instance.

This was a sign of the times. The growing disaffection with science, chronicled (and encouraged) by Theodore Roszak's cult book *The Making of a Counter-Culture* (1969), was evident not just in the public arena but in the academy as well. A similar professional split was threatening psychology too, although somewhat less public (and, as it turned out, less damaging: Chapter 6.i.d). Moreover, new currents in the theory of *literature and history* prompted many highly publicized battles on both sides of the Atlantic.

One such battle was triggered by a spoof article written by the physicist Alan Sokal, subtitled 'Towards a Transformative Hermeneutics of Quantum Gravity'. This was innocently published by a postmodernist journal, and reprinted with a hilarious commentary by the author (Sokal and Bricmont 1998, esp. 199–258).

The *casus belli*, here, was the shameless—and ignorant—misuse of scientific terminology in some postmodernist literary/philosophical writings. (If you doubt this, just look at the quotations given in the book.) Specifically, the problem was that these writers were treating physics as what Sperber calls a "symbolic" communication, when in fact it isn't (see Section vi.c, below).

The *New York Review of Books* ran commentaries by the physicist Steven Weinberg and the literary scholar George Levine, among others. And the *London Review of Books* published a desperate attempt by John Sturrock (1998), an author on structuralism and translator of Proust (and the *LRB*'s consulting editor), to defend the indefensible. Sokal, who'd admittedly made some injudicious remarks in his post-publication commentary, was mocked as "le pauvre Sokal". This, said Sturrock, was what Jacques Derrida had called him, with "a seen-it-all-before sigh". Many other protagonists entered the ring.

It was a fierce fight, and a dirty one. The pens were filled not with ink, but venom. (It didn't help that the disagreements fuelling the science wars were political as much as philosophical: see I.iii.b–c.)

Other clashes were rooted in the hostilities between Leavisites and literary theorists at the University of Cambridge. This conflict hit the international presses twice: in the 1980s when the structuralist Colin McCabe wasn't promoted to a permanent post in English, and in the 1990s when an honorary doctorate was offered to Derrida. A letter to *The Times* (9 May 1992) signed by many Cambridge philosophers protested that Derrida's work consisted of "little more than semi-intelligible attacks upon the values of reason, truth, and scholarship".

If that was the version for public consumption in the staid columns of *The Times*, in private the invective was vicious—and it was returned in kind. Anyone even touching on the dispute risked being insulted by one side or the other. (I received an abusive letter from a Cambridge-based fellow Fellow of the British Academy, a scholar of English based in Cambridge, merely for inviting him to take part in an Academy discussion of post-structuralism.)

In that atmosphere, whatever value there was in the positions of either side was ignored, or not even recognized, by the other. If this was true in the context of literary studies, it was also true in the context of science. The fact (noted in Chapter 1.iii.b) that the social constructivists had made some important points about the history and sociology of science was forgotten by those outraged by their relativism. And the more someone subscribed to "the Legend", the more hostile they would be. John Ziman's even-handedness was much needed, but missing.

For instance, Derrida's point that because a text is written in a public language one can (contra John Austin: 1962*a*) interpret it without reference to the intentions of the author, was expressed more provocatively (and much less clearly) than it might have been (Derrida 1977*a*,*b*), and countered more forcefully too: "*not just a series of muddles and gimmicks [but] a large mistake*" (Searle 1977, 1983*a*: 77; italics added).

As for cultural examples, the sociologist Pierre Bourdieu (1977, 1990) likened human life to a "game" (with influential "umpires") whose systematic rules—covering both bodily practices and associated "beliefs"—we don't choose, but are born into. (Hence his scare quotes around *beliefs*.) Also, he highlighted the near-inescapable constraints implicit in social conventions (such as gift giving), and in culturally approved artefacts (such as cutlery). For instance, if someone receives a gift there are—in any given situation, in any given culture—well-understood conventions about what type and value of gift they should give in return, and when that should be done. These constraints aren't strictly *rules*, he said, but psychological mechanisms which have much the same effect:

> The active presence of past experiences ... deposited in each organism in the form of schemes of perception, thought, and action, tend to guarantee the "correctness" of practices and their constancy over time, *more* reliably than all formal rules and explicit norms. (Bourdieu 1990: 54; italics added)

Such work was potentially relevant to anthropology, but was ignored by the 'scientific' camp. They weren't merely being perverse: the postmodernists' jargon, and their often *deliberate* unclarity, made their insights difficult to find. But the high emotions aroused by the very public hostilities precluded most opponents from searching for them in the first place.

In Cambridge, after a hugely divisive and ill-tempered debate, Derrida's degree was awarded. The neo-Kantians had won that battle. Within anthropology, however, they'd won the war.

c. Top dogs and underdogs

In a mini-history tellingly titled 'The Sad Story of Anthropology 1950–1999', D'Andrade reported the rout of traditional anthropology (including cognitivism) by the postmodernists:

> By the mid-1980s, critical anthropology had become mainstream. [Its goal] was to critique hidden and open oppressions of Western bourgeois culture: its racism, sexism, nationalism, homophobia, *and scientism*. The Enlightenment . . . came to be seen as a well of poison . . . The World Bank and the International Monetary Fund were enemies, *science was an enemy*, and *rationality was a destructive force*. (D'Andrade 2000: 223; italics added)

In the USA, the cognitivists "struggled" to get their work included in degree programmes in anthropological theory and history—and that's still true today (Strauss and Quinn 1997: 255). Across the pond, it's taught in several UK and Scandinavian universities (the latter, thanks to the influence of Fredrik Barth at Oslo's Ethnographic Museum: T. Hall, personal communication). But the degree in Anthropology with Artificial Intelligence at Sussex exists no more (see Chapter 6.iv.e). Initiated in 1975, it was withdrawn in the early 1980s because the interested students were increasingly being dissuaded by their Anthropology tutors from taking it. And there's still only one M.Sc. in the UK dedicated to cognitive anthropology, founded by Bloch at LSE. So although Bloch's 1991 paper (see Section i.d) was accepted by—or anyway, published in—a leading professional journal, he's still outside the mainstream.

The victory was officially recognized in the early 1980s, when the anti-science faction gained the ascendancy in the all-powerful AAA. The 'scientists' bitterly accused them of mounting a takeover, and peppered their communications with searing criticisms of the party line. But to no avail.

At the AAA's business meeting in Atlanta in 1994, a lengthy motion attacking the postmodernist hegemony was presented by Berlin and Romney, among others. It began:

> For the last ten or fifteen years, anthropology has been engaged in a struggle for its disciplinary life. Established notions of scientific evidence and scholarship have been jettisoned as unwarranted baggage in anthropology departments across the country. Post-modernist ethnography, post-processual archaeology, and cultural studies rhetoric have become the dominant canon in much anthropological writing [including, now, our flagship journal the *American Anthropologist*].

It ended by calling for the journal's editorial policy to be re-examined, so as to include "the diversity of approaches that have always been the hallmark of our discipline". The motion failed, by 112 votes to 86.

The following year saw a debate on 'Objectivity and Militancy' between D'Andrade (1995*b*) and Nancy Scheper-Hughes (1995), a prominent feminist anthropologist at Berkeley. D'Andrade presented "a defense of objectivity and science", saying that ethical/political commitments (like those which would underlie the *Darkness* controversy a few years later) aren't a matter for anthropologists as professionals, but as private citizens. The widespread anthropological emphasis on "oppression, demystification and denunciation" was fundamentally—and often explicitly (e.g. Rosaldo 1989; Scheper-Hughes 1992)—opposed to an objective approach. It was less an example of anthropology than a transformation of it:

> Originally, I thought these attacks [on traditional anthropology and ethnography] came from people who had the same agenda I did, just different assumptions about how to accomplish that agenda. I now realize that an entirely different agenda is being proposed—*that anthropology be transformed from a discipline based upon an objective model of the world* to a discipline based upon a moral model of the world. (1955*b*: 399; italics added)

This, he argued, was not only inappropriate but self-defeating, since any respectable moral position—and "any moral authority that anthropologists may hold"—must rest on an objective understanding of the world. Moreover, the current moral model was ethnocentric and even colonial in its vision of "evil" as a lack of equality and freedom: "In my opinion these are not bad values, but they are very American. These are not the predominant values of [many modern-day societies]."

D'Andrade's defence of objective anthropology appeared, alongside an unrepentant riposte from Scheper-Hughes, in a leading professional journal. So did Stephen Reyna's (1994) and Charles Lindholm's (1997) similarly aimed salvos. Ernest Gellner (1988) addressed a more general audience, but backed up his journal article with a characteristically punchy little book waving the banner for "reason" (1992). None of these defences took Ziman's measured stand on postmodernism versus the Legend: the odds were far too high for that.

The journal editors were more accommodating to the profession's underdogs than were the officials of the AAA. In 1997–8 an attempt was made to initiate a "Computational Anthropology" Section within the AAA. This was thwarted, and the web pages were deleted by the 'parent' Anthropology Department at UCLA. (This was largely because another section imagined that their territory was being threatened: "Their mission was to study people who used computers. Ours was to use computers to study people"—Nick Gessler, personal communication.)

Taking things outside the family onto the public stage, neo-Kantian anthropology was criticized at length in a stand-alone book (Kuznar 1997). It was also attacked in papers included in widely read collections on the science wars (Gross and Levitt 1994/1998; Gross *et al.* 1996). Predictably, these hostile writings, like D'Andrade's contribution to the debate in the peer review journal *Current Anthropology*, cut no ice with the opposition.

The new millennium saw the 'scientists' fighting back once more. Much as the Impressionists in Paris had mounted the Salon des Refusés in 1863, because they couldn't get their canvases hung in the main exhibition, so in November 2002 the disaffected anthropologists held a Salon des Refusés (they borrowed the name) in New Orleans. This new Salon, too, had a European provenance, having been mooted in Vienna by the *MACT* editors and colleagues six months earlier. It was held because the AAA had rejected several of their suggestions for symposia, including two on distributed cognition (Section iii below).

This wasn't a matter of spurning substandard papers by individuals, but of ruling entire topics and/or methodologies out of court. The reason was the holism and anti-psychologism of neo-Kantian approaches. Most of the profession had no time for the notion of distributed knowledge, which—thanks to PDP connectionism—had entered cognitive anthropology in the 1980s. As D'Andrade had pointed out a few years before the Salon:

> questions concerning . . . distribution [of knowledge] did not even make sense within much of the mainstream framework of cultural anthropology. One has to have a notion of separate units before the study of their distribution has any meaning. (D'Andrade 1995*a*: 247)

The Salon was advertised as being "concurrent with the AAA conference". A press release announced the foundation of a Society for Anthropological Sciences (*sic*), or SASci, and complained of "the anti-science bias of much contemporary anthropology". The founders hoped that their Society would be accepted as a special-interest group within the AAA, but were prepared to make it a breakaway group if necessary.

Meanwhile, one of their number—Nick Gessler of UCLA—had initiated the so-called Lake Arrowhead conferences: annual meetings on 'Human Complex Systems', the first being held in 2000. The Call for Papers for the 2003 conference invited submissions on (for example) agent modelling; artificial societies and artificial cultures; simulating social intelligence; and emergent social structure. It could have been mistaken for the CFP for an A-Life get-together, or even one on distributed processing in 'straight' AI. (It probably will be—if so, it may be another example of renaming leading to invisibility: see below.) In fact, the primary motivation was to advance anthropology.

Lake Arrowhead was an oasis of sanity, in the opinion of SASci members. The draft manifesto for SASci was part-written by Gessler, and distributed on the cognitive anthropologists' email list. It called for anthropologists to seek general principles, whether psychological or not: detailed fieldwork on individual societies was necessary, of course—but as initial data for scientific explanation, not as a final 'story'. And besides stressing SASci's commitment to scientific method, it declared: "We expect that effective computer simulation of cultural models will eventually lead to more rigorous mathematical representations."—*Anathema!*

If cognitive anthropologists are today near-invisible within the AAA, it's not too surprising that they're barely visible to their fellow cognitive scientists. Indeed, their discipline as a whole will very likely be scorned: one well-known experimental psychologist recently complained of "a clearly postmodernist atomization and often trivialization of 'the human science' " (G. Mandler 1996: 26).

As for people outside cognitive science, they can't be expected to place anthropology in it. After all, most of the anthropologists they know, or know about, wouldn't want to be there.

d. What's in a name?

Despite the opposition from the AAA, cognitive anthropology grew through the 1980s–1990s. A few examples, such as the research of Strauss and Quinn, were mentioned in Section i.d; another will be discussed at length in Section iii. But many more developed in a way that left anthropology largely unseen. The reason, in two words, was *evolutionary psychology*.

The last two decades of the century saw an explosion of research in this new field. In general, it focused on the information-processing mechanisms underlying animal and human behaviour (see Chapter 7.vi.d–f). Some of these helped to explain our cognitive abilities, some our social life, and some were relevant to culture as such (Buss forthcoming). One and the same mechanism could even affect all three, as we'll see. Indeed, culture's being "given its due" was one of the five 'Good Thing' bullet points listed by the linguist Steven Pinker (1954–) in *The Language Instinct* (Pinker 1994: 411).

The "explosion" wasn't just in research, but occurred in the publicly accessible media too. Over the next decade, Pinker made continuing efforts to bring this approach to a wide audience. His books on *How the Mind Works* (1997) and *The Blank Slate* (2002) were highly readable paeans to evolutionary psychology. He wasn't the only one. Henry Plotkin, Professor of Psychobiology at UCL, published a culture-centred introduction to the field that reached far beyond dusty library shelves (1997). Lionel Tiger—Charles Darwin Professor of Anthropology at Rutgers—was even more visible. From 1998 on, he wrote on a wide range of cultural topics in media such as *The New Yorker*, *The Wall Street Journal*, and New York's *Daily News* (Tiger 2003). And a non-technical book by two leaders in the field—one based in an anthropology department—is being readied for the press as I write (Boyd and Richerson forthcoming). Its elegantly written chapters should put evolutionary anthropology (*sic*) firmly on the public's map.

One shouldn't imagine that the newborn evolutionary psychology was primarily concerned with 'cultural' evolution. It wasn't. A few people were interested from the start in the evolution of culture as such: development in science, technology, or religion, for instance (see Sections v–vi). But most weren't. At first, there was more interest in how cultures are made possible than in how they change.

Even so, more evolutionary psychologists focused on mechanisms relevant to *sociality* than to *culture*. The universal interest in gossip, for instance, or the disapproval of cheating, were said to be mechanisms evolved to aid the efficiency of social groups (Barkow 1992: 627–31; Cosmides 1989; Cosmides and Tooby 1992). The more culturally relevant examples included mechanisms said to underlie aesthetic preferences of various kinds (Section iv), and others said to give rise to religion (Section vi).

In any event, the name of the game (literally) was *evolutionary psychology*. This new label didn't mention anthropology. And what is, in effect, the *MITECS* anthropology chapter is called 'Culture, Cognition, and Evolution'. "Culture" had survived, but "Anthropology" hadn't—*even though* the chapter was written by two anthropologists (Sperber and Hirschfeld 1999).

In short, the older discipline was largely hidden within the younger one. The new terminology had caused it to vanish.

e. Barkow's baby

Things could well have happened otherwise. The anthropologist Jerome Barkow (1944–), an expert on West African societies based at Canada's Dalhousie University, had called for a 'Darwinian Psychological Anthropology' in a leading anthropological journal as far back as the early 1970s (Barkow 1973).

That had been a daring thing to do: Darwinism wasn't generally favoured in the social sciences. That was mostly for philosophical reasons (Winch 1958). Also, many associated it with the horrors of Nazi Germany (Campbell 1965: 25). Indeed, an equally daring *psychologist*, Donald Campbell, had recently complained about the "overwhelming" rejection of evolutionary theories of culture (Campbell 1965: 19). Anthropologists' suspicion was strong, as detailed in a paper published on the centennial of the *Origin of Species* (White 1959*a*). The few brave souls willing to mention Darwin and anthropology in the same breath weren't easily heard (e.g. White 1959*b*; Sahlins and Service 1960).

However, Barkow's approach seemed less outrageous to his colleagues then than it would some years later. For the virulent anti-science movement in anthropology (described above) hadn't yet got off the ground. And Geertz was only just setting out on his anti-psychology crusade. So Barkow's approach persuaded some of his readers. Indeed, the 'evolutionists' were able to mount no fewer than two symposia at the AAA meeting of 1976 (Chagnon and Irons 1979). No need for a Salon des Refusés, yet.

But Barkow's suggested label was a mouthful. If he'd abbreviated the fourteen syllables to a punchy "DPA", it might have caught on. In that case, his home discipline would be less invisible now.

(To say that the label might have caught on, among those who were interested, isn't to say that the activity might have caught on, among anthropologists in general. Thanks to the science wars, there was no chance of that. As much as twenty years later, a 'scientific' anthropologist remarked, with admirable understatement, that "in anthropological theory, the very mention of evolutionary constraints is bound to trigger some hostility, not always of a strictly rational nature"—Boyer 1994: 15.)

Besides foreseeing a Darwinian anthropology in the early 1970s, Barkow did a great deal to initiate it in the early 1990s—albeit under another name. He was the senior editor of what became the seminal publication in the field of evolutionary psychology: a volume of specially commissioned papers on *The Adapted Mind* (Barkow *et al.* 1992).

Like MGP's manifesto for computational psychology (Chapter 6.iv.c), this manifesto for evolutionary/computational psychology was partly prepared at the Stanford think-tank. Subtitled 'Evolutionary Psychology *and the Generation of Culture*', it introduced many non-specialists to evolutionary psychology—and to some of its cultural implications. Indeed, Barkow wasn't the only anthropologist in the driving seat. One of his two co-editors, Tooby, was an anthropologist also, as we've seen. Only Leda Cosmides (Tooby's wife) was officially a psychologist. However, the content of the book showed how misleading these disciplinary distinctions had become.

The three co-editors made a point of allying evolutionary psychology with cognitive science:

* Their 'statement of intent' repeatedly assimilated the two (Tooby and Cosmides 1992: 64–9, 94–108, 112–14).
* In their opening pages, they praised cognitive scientists for adding rigour to psychology, and for asking *how* things happen in human minds (Cosmides *et al.* 1992: 7–11).
* They adopted Jerry Fodor's talk of "modules" (7.vi.d).

* They claimed to be following Marr's advice about abstract "computational" explanation (7.iii.b and v.b–d), since (they said) evolutionary psychology bases its hypotheses about cognitive mechanisms on the *environmental constraints* faced by our hunter–gatherer ancestors and our cultural forebears.
* And, tacitly calling on Sperber's work on relevance (7.iii.d, and v.d below), they stated that:

> evidence about the structure of memory and attention [in general] can help cultural anthropologists understand why some myths and ideas spread quickly and easily while others do not. (Barkow *et al.* 1992: 12)

In other words, they deliberately blurred the boundaries between psychology and anthropology—which Barkow himself had done in his DPA paper. This was even more of a professional heresy in 1992 than when Barkow first suggested it. Most late-century anthropologists agreed with Geertz that

> The main source of theoretical muddlement in contemporary anthropology is [the view that] culture [is located] in the minds and hearts of men.
>
> Variously called ethnoscience, componential analysis, and cognitive anthropology . . . this school of thought holds that culture is composed of psychological structures by means of which individuals or groups of individuals guide their behavior. (Geertz 1973*b*: 11)

In some quarters, however, the heresy spread. SASci members complained that although Geertz's followers were happy to speak of the self, meaning, and identity, they weren't prepared to talk about "the psychological processes and structures that help explain these" (Strauss and Quinn 1997: 9). And in his provocative Malinowski Memorial Lecture given in London in 1984, Sperber (based at Paris's CREA, or Centre de recherche en épistémologie appliquée) declared:

> There exists . . . no threshold, no boundary with cultural representations on one side, and individual ones on the other. Representations are more or less widely and lastingly distributed, and hence more or less cultural. (Sperber 1985: 74)

He even offered *psychological* reasons why certain representations rather than others are "widely and lastingly distributed" (see Section v.d).

Barkow's colleagues accepted the traditional concept of "culture", which many of their contemporaries had spurned. But they decomposed it into three types, defined—provocatively—in terms of different sorts of *information processing* (Tooby and Cosmides 1992: 121). As they saw it:

> Culture is not causeless and disembodied. It is generated in rich and intricate ways by information-processing mechanisms situated in human minds . . . To understand the relationship between biology and culture one must first understand the architecture of our evolved psychology. (Cosmides *et al.* 1992: 3)

> [Our] central premise . . . is that there is a universal human nature, but that this universality exists primarily at the level of evolved psychological mechanisms, not of expressed cultural behaviors. On this view, cultural variability is not a challenge to claims of universality, but rather data that can give one insight into the structure of the psychological mechanisms that helped generate it. (p. 5)

It followed from this that "the socially constructed wall that separates psychology and anthropology (as well as other fields) will disappear" (Tooby and Cosmides 1992: 121).

In arguing that anthropology and psychology were—or anyway, should be—closely related, Barkow wasn't saying anything he hadn't said twenty years before. He'd written his DPA paper partly because he'd been trained as an interdisciplinary animal (in Chicago's "Committee on Human Development" programme), so was wary of Chinese walls.

Moreover, he'd always used ideas from cognitive science. In the early 1970s he'd posited internal representations mediating social status (Barkow 1975, 1976). Later, he did so in respect of gossip too (1992: 627–31). And he'd explained culture and consciousness in terms reminiscent of MGP, speaking of goals and sub-goals, plans and sub-plans, and codes and sub-codes—at base inborn, but hugely varied and complicated in acculturated human adults (1989).

As a corollary, Barkow had always insisted on the "unity" of science. He didn't mean this in the logical positivists' sense: reducibility to physics (see Chapter 9.v.a). Rather, he meant that explanations at various levels should be *compatible* even though they're not *reducible* (Barkow 1983, 1989). His theory of consciousness, for instance, accommodated anthropological, psychological, neurological, and evolutionary perspectives.

Within cognitive science, the need for such inter-level consilience is a platitude. But it's not so regarded throughout anthropology, as we've seen. With respect to the mainstream of the profession Barkow was, and remains, a maverick.

The same is true of cognitive anthropologists in general. In the eyes of the AAA, they were studying forbidden topics in forbidden ways. The rest of this chapter describes some examples of these professional heresies.

8.iii. Minds and Group Minds

One of the forbidden topics featured at the New Orleans Salon des Refusés was distributed cognition, a concept prominent in cognitive science since the 1980s (Chapters 13.iii.d–e and 12.v–vi). Here, knowledge is represented across many different places, where this involves not duplication (copying) but distribution: i.e. sharing, in the sense of division of labour. The "many places" may lie within an individual brain (or network), or within a group of intercommunicating organisms (or networks, or robots).

Most anthropologists are interested in distributed cognition, though they may call it role assignment, kinship relations, shared/individual cultural schemas, division of labour, or task sharing. But the scientifically minded can now call on computational concepts to help clarify the issues involved. Indeed, we saw above that the online journal *JASSS* has been founded specifically to describe "social" computational theories and simulations.

People working on this topic sometimes speak informally of "group minds", although that term is usually avoided in print. More often, they speak of "the cognitive properties of groups". They see "planning" not as an in-the-head phenomenon, but as a dynamical engagement with the environment, including other people. The anthropologist Lucy Suchman (1951–), for example, described the "interactive" planning she'd observed in the operations room at San Jose airport—an account that would influence research in AI (Suchman 1987; see 13.iii.b).

Similarly, the philosopher John Searle distinguished "collective intentions" from "individual intentions", arguing that the former can't be analysed in terms of the latter. A pass in a game of football, for instance, isn't made up of the kicks/headers of the individual players. He was well aware of the strangeness of such a claim:

> How, one wants to ask, could there be any group behavior that wasn't just the behavior of the members of the group? After all, there isn't anyone left to behave once all the members of the group have been accounted for. And how could there be any group mental phenomenon except what is in the brains of the members of the group? How could there be a "we intend" that wasn't entirely constituted by a series of "I intend"s? (Searle 1990*c*: 402)

His own answer was in terms of a definition of (and notation for) "collective intentionality", which assumed "a preintentional Background of mental capacities that are not themselves representational" (p. 401).

Other cognitive scientists appeal to a background of a different kind, namely the ready-made technologies provided by one's culture (including language as just one example). Bruner had done this as early as the 1960s (6.ii.c). But some of today's writers make this approach the ground of a highly unusual concept of the "individual". Specifically, they *deny* that the enculturated individual's mind, or self, stops at the skull or the skin.

One such denier is the philosopher Andy Clark (Chapter 16.vii.d). Another, whose research on navigation is often cited by Clark, is Edwin Hutchins (1948–). Hutchins is an anthropologist at San Diego. Besides being a colleague of D'Andrade, he's close to San Diego's pioneering connectionist group (Chapters 6.iv.e and 12.vi–vii).

Hutchins has also worked with Norman on interactive interfaces, an interest partly fuelled by his concern for how naval crewmen use the instrumentation aboard ship (E. L. Hutchins *et al.* 1986). Indeed, his account of distributed cognition on ships has recently been generalized into an analytical tool for thinking about the design of workplaces in general, of which interfaces—and human collaboration via computer interfaces—are important examples (Hollan *et al.* 2000; Y. A. Rogers forthcoming). In addition, he has studied the interactions within teams of programmers cooperating on complex software maintenance (Flor and Hutchins 1991). So besides learning from non-anthropological cognitive science, he has contributed to it too (see 13.iii.d–e).

a. Models of seamanship

Hutchins started doing cognitive anthropology thirty years ago, but his chief concerns have developed in an interesting way.

In the mid-1970s, he spent two years studying reasoning strategies in Papua New Guinea. His book on *Culture and Inference* (1980) used schema theory to describe conversations he heard on PNG's Trobriand Islands—primarily, disputes about land tenure. Despite the 'exotic' subject matter and cultural presuppositions, his conclusion was that there's no essential difference between the reasoning of Trobriand Islanders and Westerners. By implication, then, reasoning is a human universal. There was a caveat, however: since psychologists still don't fully understand 'home-grown' reasoning (see

Chapter 7.iv), anthropologists aren't standing on firm ground when they compare it to reasoning in other cultures.

His later work moved beyond language and reasoning, to include material artefacts and non-linguistic behaviour. (Wallace revivified, one might say.) Indeed, it was he who was disillusioned by the linguistic—not to mention the 'literary'—turn when trying to write a survey of cognitive anthropology (see Section i.a, above).

By far the most of Hutchins's post-PNG fieldwork has been done in the community of commercial airline pilots (personal communication). Like Craik and Donald Broadbent, but at the cultural not individual level, he has studied the operation and design of airline cockpits and the nature of pilot training (he holds a commercial pilot's certificate). In cockpits, he says, one can literally "step inside the cognitive system". Unlike the processes inside human heads (about which Hutchins is agnostic), much of the internal organization and operation of this cognitive system is *directly* observable (1995: 128–9).

What's of special interest here is Hutchins's broadly similar research on navigation at sea (1995). Most of the fieldwork for his book on this topic was done on US Navy ships, but some involved canoe navigation in Micronesia; besides, he's an accomplished amateur sailor. His work is relevant not only because he uses computational ideas, but also because he challenges the assumptions underlying much current cognitive science (and virtually all of the computational psychology discussed in Chapter 7). His extension of "the mind" *beyond* the person's skull/skin is one indication of this.

Like the evolutionary psychologists mentioned above, Hutchins pays homage to Marr's views on explanation (7.iii.b). But because of the nature of the task, he's able to take Marr more literally. He specifies four abstract computational constraints that must be satisfied by any successful (adaptive) method of navigation. He describes a variety of algorithms (navigational procedures), and a variety of implementations (in brains and/or artefacts), in 'Marrian' terms. And he provides computer models—connectionist networks-of-networks—confirming some of his claims.

The four abstract constraints (too complex to summarize here, but see Hutchins 1995: 52–8) are:

(1) The Line of Position constraint.
(2) The Circle of Position constraint.
(3) The Position–Displacement constraint.
(4) The Distance–Rate–Time constraint.

Different combinations of these four constraints determine the answers to the navigator's key questions: "Where am I?", "How far have I come?", "Where is my intended port-of-call?", and "How do I get there from here?"

As for *how one can compute* those answers while crossing vast expanses of ocean, there are two fundamentally different ways of generating navigational algorithms that satisfy the constraints. One is to think of the vessel and stars as moving, while the islands remain stationary. The other is to think of the vessel as fixed, while the islands and stars move towards it and then pass it by. (It's not just thinking, but *seeing* too: Hutchins and Hinton 1984.) The first representation is used by Western sailors, the second by Pacific islanders.

From the *computational* point of view (in Marr's sense), and considering only navigation (not how these pictures fit with other theories about the world), there's nothing to choose between them. But the *algorithms* derived from the one or the other will differ. Indeed, some 'obvious' questions posed from within one point of view simply won't make any sense to someone familiar only with the other. (Compare the fact remarked in Chapters 1.iii.a and 2.vii.f, that some scientific questions posed in the past make little or no sense today.)

(Also, although Hutchins doesn't discuss this, navigational practices which might seem to be 'obviously' implied by others may nevertheless be absent. For instance, *north* in Icelandic is judged by careful reference to the stars when at sea, but not on land. There, the judgements are radial; so a land traveller headed for the northern quadrant is said to be travelling north even if they're 'really' going north-east: Haugen 1957.)

Moreover, any instruments devised to *implement* navigational information processing will embody the option favoured by the culture using them. Once implemented in instruments, the conceptual scheme appears further objectified. Alternative schemes therefore look even more like groundless myths and mind games, mysteriously capable of compensating for gross ignorance.

Specific aspects of alien practices may be misunderstood accordingly. David Lewis's fascinating *We the Navigators* (1972), for instance, is trustworthy when he describes *what the Pacific islanders do* during their voyages. But it's not (or not always) reliable when he tries to explain *why they do it* (E. L. Hutchins 1995: 78–83).

The actions of the US Navy sailors could in principle be implemented (in their heads) as a global top-down plan, *à la* MGP—or Wallace. Alternatively, they could be implemented as 'antlike' production systems (Chapter 7.iv.b), wherein "each crew member only needs to know what to do when certain conditions are produced in the environment" (Hutchins: 199; cf. Fararo and Skvoretz 1984; Boden 1984*a*,*b*).

Hutchins shows that, in essence, the second of these is how it's actually done. The organization of specific duties ensures that the appropriate sequence of actions can be performed efficiently and flexibly, without burdening even the lowliest rating with a cognitive map of the entire navigational process. ("Ensures" is over-optimistic: accidents do happen, and some are tellingly analysed by Hutchins—e.g. pp. 241–2.)

In human societies, many tasks are shared, in the sense that they're distributed across several different individuals. This doesn't include a tug-of-war, where every man does the same thing and all have access to the same information about progress/success. Rather, people holding distinct roles do different things, and have different types of information. Often, specific communicative procedures have to be instituted (*sic*) in order to keep their diverse activities "in step" and to monitor progress in the overall task. (Compare Wallace's traffic lights and stop signs, for instance.) The communication may pass (heterarchically: see 10.iv.a) both upwards and downwards in the status hierarchy, and sideways too. And there are many ways of communicating besides speaking: pointing, for example, or raising a flag.

Hutchins illustrates these points in detail, describing elaborate shipboard procedures for finding and communicating relevant information. In a real sense, the sailors interact not only with each other but also with the ship and its instrumentation—including the naval charts. For Hutchins, knowledge amassed over many centuries by mariners, scientists, shipbuilders, and instrument makers is distributed *also* in these naval artefacts.

Even the placement of the chart table is significant, and coheres with the organization of activity aboard the ship.

b. Networks of navigation

The need for communication, given differential access to information, is where Hutchins's computer models come in.

Solving navigational problems is often difficult. The evidence may be incomplete (e.g. weather conditions may preclude use of some instruments), and inconsistent (e.g. instrument readings and/or crew members' beliefs don't match). The strong point of PDP-connectionist AI, as compared with GOFAI, is that it 'naturally' provides multiple constraint satisfaction (Chapter 12.v). Connectionist systems can reach *the most coherent* interpretation, even if some evidence is missing or contradictory. Just which they settle on depends on the evidence available/communicated at the time.

Hutchins uses *networks made up of networks* (12.ix.a) to explore "the relationships among properties of individuals and properties of groups" (p. 247). Put more provocatively, he's studying the relations between minds and group minds.

His networks model both individual sailors (e.g. the captain, helmsman, or radar operator) and the teamwork involved when the crew members act as a community. The sailor networks show what psychologists call confirmation bias: once a hypothesis has been formed, people pay more attention to evidence which confirms it than to contrary data (cf. 7.i.c). And the crew networks can model situations in which sailors disagree (about what they see, or what they think should be done), fail to make their opinion known to some other relevant person, or fail to convince that person of its truth.

A system composed of two or more networks has at least seven parameters that aren't found in a single network (p. 248). Three concern the distribution of structure and activation-state across the members of a community of networks; four concern communication among them. So Hutchins's computer models represent a sailor's schemata for interpreting what he sees/hears; his access to environmental evidence; his predispositions and current beliefs; who talks to whom, about what, and when; and how persuasive they are.

For instance, *current beliefs* are represented by the initial pattern of activation across the units of a sailor net. The *persuasiveness* of sailor *x*'s communications to sailor *y* is coded by the strength of the connections between the two nets concerned. The *topic* is coded by the pattern of interconnectivity among the units of the communicating sailor nets. And *who talks to whom* is captured by the pattern of interconnections among the nets.

Hutchins studied how systematic variations in these seven parameters affected the performance of the multi-network as a whole. For instance, a real-life situation in which two ships collided was modelled partly by degrading the 'visual' information available to the 'captain' (pp. 241–2, 249). And he showed that it's not always true that the best way to improve the performance of a group is to improve the communication among its members. In some conceivable circumstances, improved communication between individual minds reduces the effectiveness of the group mind. The confirmation bias typical of individuals is echoed—indeed, strengthened—at the group level, making the group interpretation even more hasty and intransigent (pp. 253 ff.).

The "attitude change" effected by one person communicating their belief to another was studied in the very early days of computational psychology (7.i.c). It was recognized, then, that the second person's level of confidence in the belief concerned *is not* always raised, and can even be lowered—and the computer simulations were designed accordingly. Hutchins's models are hugely less crude than those 1960s examples. But they assume—what he admits is "the most problematic simplification in the system" (p. 250)—that communication does always have this confidence-raising effect.

How much the second sailor network's belief activation is raised, however, depends (among other things) on the persuasiveness of the first. Persuasiveness was varied in different runs (pp. 251–9), and could have been modelled in even more detail (p. 251).

A main determinant of persuasiveness is the individual's social status. In hierarchical organizations (such as the crew of a navy ship), the leader's opinion counts very heavily, and can overrule the opinions of inferiors even if they have more direct access to the evidence. In consensus societies, such as a Quaker meeting, that can't happen: everyone—eventually—has to agree.

Hutchins's models illustrate two drawbacks of such (consensus) organizations. First, impasse can result if conflicting opinions have settled before communication begins. Second, if attempts are made to unblock the system by changing the pattern of access to evidence, this may prevent *any* reasonably coherent opinion from being reached. Finally, in systems where the solution is reached by voting, the result may differ from what it would have been had further communication been allowed.

"The cognitive properties of groups", Hutchins concludes, "are produced by interaction between structures internal to individuals and structures external to individuals" (p. 262). The social organization is crucial:

> Doing without a social organization of distributed cognition is not an option. The social organization that is actually used may be appropriate to the task or not. It may produce desirable properties or pathologies. It may be well defined and stable, or it may shift moment by moment; but there will be one whenever cognitive labor is distributed, and whatever one there is will play a role in determining *the cognitive properties of the system* that performs the task. (p. 262; italics added)

Four things follow from Hutchins's account:

* Anthropology and psychology are intimately connected.
* Psychology isn't enough. Even *social* psychology, if this means how individuals are affected by their social context, isn't enough. The matters highlighted by Abelson, for example, are important (Chapter 7.i.c)—but they leave a crucial dimension of analysis untouched.
* Classical AI, and all computational psychology inspired by it, is incapable *in principle* of capturing the cognitive properties of groups—aka cultural phenomena. (Compare the critique of orthodox cognitive science outlined in 13.iii.e.)
* And distributed AI, by contrast, can counter the excessive individualism of which many critics of GOFAI complain. (Hutchins favours PDP, as we've seen; however, distributed AI isn't necessarily connectionist: see Chapter 13.iii.d–e.)

In short, computational anthropologists can consider not only minds but also group minds—and they can do this without mystery mongering.

8.iv. Mechanisms of Aesthetics

Anthropologists know better than anyone that different cultures dress, dance, and decorate in different ways. Music, for example, has diverse styles and social significance for distinct societies (Nettl 2000; Mache 2000). Even within a given society, distinct subgroups may value very different genres. Indeed, a new way of doing art will typically involve explicit defence of a new aesthetic—as happened with computer-based "interactive" art, for instance (see 13.vi.c and Boden forthcoming). In short, arts and crafts vary greatly across the world. (The arts more than the crafts, because of the different psychological mechanisms involved: crafts depend on universal Gibsonian affordances, whereas fine art depends on culture-specific themes and stylistic references—7.v.e–f and Boden 2000*a*.)

Neo-Kantians, following Humboldt (9.iv.b), are likely to stress the *uniqueness* of each culture's aesthetic. However, some—such as Boas (1927)—have argued that aesthetic values in all cultures are rooted in an appreciation of technical skill and patience. And a fortiori those, like Barkow, who hope for a *Darwinian* psychological anthropology will seek some universal basis here. Beauty is in the eye of the beholder—but perhaps, like the three Graeae sisters of Greek mythology, all human beings share a single eye?

In the last twenty years of the century, there was a surge of interest in this question. It had been revived partly because of evolutionary theory in general, and partly because of cognitive science's stress on specific information-processing mechanisms.

a. From Savanna to Sotheby's

The sociobiologist Edward Wilson (1929–) was one of the first proponents of the single eye (E. O. Wilson 1984). His influential "biophilia" hypothesis was applied to all members of *Homo sapiens*, not just three. Indeed, it was applied even more widely: all the way from the art experts at Sotheby's to the busy members of his favourite species, ants.

The biophilia hypothesis claimed that animal species, including humans, have evolved to find certain aspects of their habitat attractive. They tend to pay attention to them, and to prefer them—in the behaviourists' language, to be reinforced by them. The relevant aspects of the environment, Wilson said, range from specific chemicals in water or food to 'friendly' conspecifics, and include many visible features.

Wilson himself saw this not merely as a disinterested scientific hypothesis but also as the empirical basis for a "conservation ethic" (E. O. Wilson 1993). To the extent that anthropologists favoured conservation—of mountains, flora, and fauna, and of human *experience/knowledge* of these things too—they could find support in Wilson's writings.

More to the point for present purposes, the biophilia hypothesis implied that species-universal aesthetic preferences underlie our culturally diverse practices. The question naturally arose as to just what those preferences might be. In the decade following Wilson's book (and Fodor's *Modularity of Mind*, published just a year earlier), evolutionary psychologists posited various "modules" supposed to underlie aesthetic judgements.

It was found, for instance, that—despite varying cultural preferences for 'fattipuffs' and 'thinnifers'—there's a universal attraction to certain ratios of body measurements in males and females (Singh 1993; G. F. Miller 2000: 246 ff.). In a nutshell, people are aesthetically valued for their probable efficiency as reproductive partners (Hersey 1996). From an evolutionary point of view, that made perfect sense. But other aesthetic values weren't so obvious. Let's consider just two examples: landscape and symmetry.

The appreciation of landscape—including people's responses to city squares and to offices or hospital wards with/without windows—was studied by environmental psychologists (e.g. Ulrich 1983, 1984, 1993; Kaplan and Kaplan 1982; S. Kaplan 1992). Evolutionary assumptions often lay in the background.

The zoologist/ecologist Gordon Orians (1932–), in particular, saw these matters in an evolutionary context (1980, 1986; Orians and Heerwagen 1992; Heerwagen and Orians 1986, 1993). His core hypothesis was that people would prefer certain types of scenery because of information-processing mechanisms evolved for selecting advantageous habitats.

Consider animals forced to flee because of predators, or lack of food. When should they stop, and stay put? Only when it's safe to do so. If they make camp far from water, or with no hiding places and/or no escape route, they're less likely to survive. So a disposition to stop running when one comes to a place having these valuable features is likely to be selected. It will become 'natural' to prefer such places: to tarry when one reaches them, to explore them (to gather information about them), and perhaps to settle comfortably in them. The first stage is crucial, for the other two can't happen without it.

So far, so speculative. But Orians reported that (in various species) the decision to tarry is taken automatically and very quickly, whereas exploration takes longer and is more 'cognitive' in nature. Moreover, clues predicting advantageous future states should be more useful than indicators of current, and perhaps transient, states—and some species behave accordingly. For instance, many birds use general patterns of tree density and branching as "primary settling clues", rather than trying to assess food supplies directly (Orians and Heerwagen 1992: 555–6).

Findings like this had promoted Orians's idea from a Just So story to a hypothesis for which there was some specific evidence. Now, he looked for comparable evidence in humans too.

Just how the relevant features are recognized wasn't at issue (but remember the discussion of affordances in Chapter 7.v.e–f). However, he called his approach "computational" in the Marrian sense that its purpose was "to guide research into the psychological mechanisms that promote adaptive functioning in different environmental contexts" (p. 561). Specifically, he predicted that the clues concerned would typify the resources of the African savannas where humans originated:

> If we assume that [we've evolved] psychological mechanisms that aid adaptive response to the environment, then savanna-like habitats should generate positive responses in people . . . This is because the savanna is an environment that provides what we need: nutritious food that is relatively easy to obtain; trees that offer protection from the sun and can be climbed to avoid predators; long, unimpeded views; and frequent changes in elevation that allow us to orient in space. Water is the one resource that is relatively scarce and unpredictably distributed on the African savannas [so it's likely to function as a 'direct' settlement clue]. (p. 558)

Borrowing a method already in use (Balling and Falk 1982), Orians showed photographs of landscapes to people from widely differing geographical habitats and cultures. Subjects were asked which images they found most attractive, or which places they'd most like to visit or live in. These questions aren't equivalent, of course: modern tourists visit many places they wouldn't want to live in, much as fashionable late eighteenth-century gentlemen hung 'sublime' Romantic landscapes on their walls depicting places they'd never willingly inhabit. Nevertheless, Orians's results, whatever the question, showed a clear preference for all the clues listed in the quotation above (and for the presence of animals, too).

Aesthetic preferences for landscape art, from garden design to painting, were studied in these terms. For example, Orians saw what Stephen and Rachel Kaplan (1982) had called "mystery" as inviting the second stage: exploration. And indeed, roads winding behind hills, bridges affording (*sic*) safe access to distant terrain, and even partially blocked views were all found attractive by his human subjects.

Moreover, nineteenth-century garden design apparently confirmed one of his more startling discoveries. Orians found that people around the world are attracted by images of trees shaped like those on the African savanna (Orians and Heerwagen 1992: 559). Even more amazingly, they prefer those on *high-quality* savanna (his photographs showed only one species of tree, whose shape varies markedly with the availability of water). The favoured trees had a spreading canopy, moderately dense foliage, and a trunk that branches close to the ground. Humphrey Repton (1752–1818), the renowned English landscape designer (and author of three books on garden theory), was quoted as recommending trees of that very type (p. 561).

Orians's results were later replicated by others—at least in part (Ruso *et al.* 2003: 280–1). These follow-up researchers included two environmental psychologists at UC Davis: Robert Sommer (Sommer and Summit 1995; Sommer 1997) and Richard Coss. Coss, in particular, had long studied the role of perceptual and emotional mechanisms in art (e.g. 1965, 1977, 1981).

On turning to Orians's savanna hypothesis, he found that 3- to 5-year-old children (across cultures) find wide-crown trees less "pretty" than adults do. However, they see them as the best trees "to climb to hide", "to feel safe from a lion", and "to find shade" (Coss and Moore 1994; Coss and Goldthwaite 1995; Coss 2003: 82–5). These results, from children far too young to be experienced climbers, suggested "a precocious understanding of tree affordances useful historically and currently to avoid predators and to prevent dehydration" (Coss 2003: 84).

As for preventing dehydration, Coss also argued that the widespread (universal?) aesthetic preference for shiny things, including lustrous fabrics, is based in visual mechanisms evolved to identify water (Coss and Moore 1990; Coss 2003: 86–90). A sensitivity to reflectance could have helped early *Homo sapiens* to find expanses of still water at a distance and/or partly hidden by trees or shrubs, or water dispersed as dewdrops on—lick-affording—plants. Today, 7-month-old babies are twice as likely to lick or suck glossy things as dull ones.

The water example is highly plausible. Think of silver lurex, or of Lord Leighton's sensuous portraits of satin-clad women; or consider the chromium and glistening paint on new cars. And meditate on the fact that bone artefacts more than 70,000 years old show signs of 'useless' polishing, as do 14,000-year-old ivory figurines too (Coss 2003:

88–9). (And recall that the German *schön*, beautiful, has the same root as *scheinen*, to shine.)

The tree example, however, may seem mind-boggling. But the discovery of IRMs had already shown that visible shapes can be coded in the genome (e.g. the moving hawklike cross that prompts the gosling to crouch: Chapter 5.ii.c). And it was discovered at much the same time as Orians published his first 'habitat paper' that birds who naturally prey on snakes, but have never seen one, show strong aversion to wooden dowels painted with wide yellow and narrow red rings—like the skin of a venomous coral snake (S. M. Smith 1977). So it's not impossible that a particular tree shape is somehow favoured by our genes.

Humboldtians will bridle, pointing out that few people regard the savanna as their ideal environment. Woody Allen presumably prefers New York; and many favour the flamelike cypresses of Provence over the spreading canopies of Africa. But Orians knew that. He allowed that experience of other habitats can result in emotional attachments that make *them* seem ideal. His point, rather, was that such preferences have to be learnt. By contrast, everyone responds positively to savanna clues, whether they've experienced them or not.

b. The seductiveness of symmetry

Another feature valued by cultures all around the world is symmetry. Boas (1927) listed it as one of the universal aesthetic values in "primitive" art. But unlike savanna trees and shininess, it has no obvious adaptive advantage. Indeed, it's highly abstract: delightful to mathematicians, perhaps, but why should the rest of us care? Can it *really* be as important as Boas implied?

Its power is seen, for example, in the *maneaba*—a sacred meeting house, one per island—of the Kiribati, the people of the Gilbert Islands (Maude 1980: esp. 15–16, 20, 24). The floor plan of every *maneaba* is almost perfectly rectangular (exactly fitting one of nine length/breadth combinations), and the two main posts supporting the ridge-pole are almost perfectly vertical. "How clever of them", you may condescendingly think, "to get so close to Euclid—they might have got even closer if they'd had tape measures." You'd be way off the mark. The eastern side is deliberately made slightly longer than the western side. Moreover, at one point during the highly ritualized construction, which measures not by metre rules but by limb-lengths and pandanus fronds, the posts *are* vertical. But the Kiribati then push them towards the west by a hand's width, so as to overhang the territory of the evil spirits. This cultural practice couldn't have arisen if symmetry weren't a very special value indeed.

It was appreciated even in prehistoric times. When *Homo ergaster*, some 1,400,000 years ago, first made stone hand-axes to recognizable standards—as opposed to merely knocking flakes off rocks, to use as cutting edges—symmetry was a key feature (Kohn and Mithen 1999; Mithen 2003). And when *Homo sapiens*, perhaps 50,000 years ago, started to vary hand-axe designs, symmetry remained a key characteristic in the tools and art of all prehistoric societies.—But why?

In answering that question, evolutionists noted that symmetry is *present* in nearly all known species, and is *perceptible*—and even *preferred*—by many of them. For instance, psychologists have discovered that people can discriminate nicely between symmetric and asymmetric faces and body shapes, and that they find the former more attractive (for reviews, see Etcoff 1999 and Thornhill 1998). (Much of this work was done in the 1990s, by David Perrett's group in Edinburgh: see Chapter 14.iv.d.) Indeed, symmetry is attractive to monkeys too—and even to carrion crows. As long ago as the late 1950s, these animals had been seen to prefer items carrying regular patterns over irregularly patterned ones (Sutterlin 2003: 134), and the evidence has mounted since then (e.g. James R. Anderson *et al*. 2005).

Three evolutionary explanations were offered. The first suggests that animals will tend to be symmetric. The others suggest that they're likely to be able to recognize symmetry in other animals.

First, bodily symmetry is efficient, requiring less genetic coding than asymmetry. (A centipede with a hundred differently shaped legs would need a much-enlarged genome to develop them.)

Second, visible symmetry/asymmetry is a useful clue to fitness (P. J. Watson and Thornhill 1994). All manner of things can go wrong in the development of egg into adult. Symmetries, then, can act as indicators of 'good genes'—which is to say, a good choice as a mate.

And third, symmetrical patterns (especially those with several axes of symmetry) are relatively easy to spot, since they look much the same despite variations of orientation and/or viewpoint (Enquist and Arak 1994). Their usefulness for identifying conspecifics would favour sexual selection—although their usefulness to predators, in identifying a potential lunch, would be a selective disadvantage. (The third hypothesis was seemingly supported by a computer model that evolved increasingly complex symmetries. It turned out, however, that this result was an artefact of the simulation design: Bullock and Cliff 1997.)

The third explanation was offered as an alternative to the second. In principle, however, all three could be jointly true. That is, symmetry might be favoured by natural selection for coding efficiency and/or for ease of recognition, and by sexual selection also (see below). But whatever the evolutionary explanation/s may be, the fact remains that human beings have a remarkably good eye for symmetry—and also tend to prefer it.

An information-processing mechanism capable of recognizing symmetries *in general* is a different kind of beast from one which recognizes only hawklike crosses. But we know that visual matching of the images in the two eyes is 'built in' for stereopsis (14.v.f). Some broadly comparable matching mechanism could have evolved to cope with symmetries.

c. Universality in variety

The claim that there's a universal mechanism for appreciating savanna, or symmetry, raises a problem for any 'massively modular' aesthetics (see Chapter 7.vi.e). For artists sometimes deliberately break symmetries, or paint landscapes that are far from inviting.

Contrast the classicism of Andrea Palladio with the asymmetrical architecture of Daniel Libeskind, for instance, or the symmetry of a diamond tiara with the challenging jewellery of the American goldsmith William Harper. (Even *classicism* has its variants: Palladio was a more faithful respecter of symmetry than his fellow classicist Nicholas

Hawksmoor.) And where landscape is in question, contrast the rural scenery of John Constable with the unnatural panoramas of Salvador Dali.

One doesn't have to go to Western art to find examples. No cultural pattern is safe from variation, not even in communities which value change much less than industrial societies do.

What's more, such variation is often admired as "interesting", or "creative"—where what this means, put very crudely, is that it surprises us. But why value surprises? Wouldn't life be more comfortable without them? Why evolve a capacity (and a preference) for surprise, if current behaviour suffices for survival? In short, how can evolutionary psychology explain this pervasive aspect of human cultures?

An answer was recently suggested by Geoffrey Miller (1965–), in a 500-page tour de force drawing on a treasure chest of evidence from psychology and anthropology, and half-a-dozen other disciplines besides (G. F. Miller 2000). (He was well aware that evidence is needed to turn a Just So story into a plausible scientific hypothesis.) He'd been working on this topic for some ten years, at Stanford, Sussex, LSE, and UCL—with a brief sojourn at the Max Planck Institute in Munich. Now, he's at the University of New Mexico.

Miller is a cognitive scientist who has worked on A-Life computer models, and on perceptual representation in humans (Cliff and Miller 1995; G. F. Miller and Freyd 1993; see Chapters 15.vii.c and 14.vii.c, respectively). He's wary of the old-fashioned computer metaphor being taken too literally:

> [To] psychologists who pride themselves on their seriousness . . . the mind is obviously a computer that evolved to process information. Well, that seems obvious now, but in 1970 the mind as a computer was just another metaphor . . . The mind-as-computer helped to focus attention on questions of how the mind accomplishes various perceptual and cognitive tasks. The field of cognitive science grew up around such questions.
>
> However, the mind-as-computer metaphor drew attention away from questions of evolution . . . creativity, social interaction, sexuality, family life, culture, status, money, power . . . As long as you ignore most of human life, the computer metaphor is terrific. Computers are human artifacts designed to fulfill human needs, such as increasing the value of Microsoft stock. They are not autonomous entities that evolved to survive and reproduce. This makes the computer metaphor very poor at helping psychologists to identify mental adaptations that evolved through natural and sexual selection. (G. F. Miller 2000: 153)

But although he scorned GOFAI, Miller was committed to mind-as-machine, understood in the catholic sense defined in Chapter 1.ii.a. And he saw evolutionary theory as the key to cultural variation.

Miller's book pointed out that Charles Darwin posited two principles of evolution: natural selection (for survival) in *On the Origin of Species* (1859) and sexual selection (for mate choice) in *The Descent of Man* (1871). Darwin used the second to explain courtship behaviour in animals, and anatomical features such as the hugely expensive peacock's tail (Cronin 1991). Miller, over 100 years later, applied it also to the runaway increase in brain size of *Homo sapiens*.

This didn't happen, he argued, because it enabled us to solve survival problems more efficiently: killing sabre-toothed tigers, for instance. Rather, brain size increased because it allowed us to represent—to generate and/or recognize—increasingly complex motor/perceptual patterns. In a word, it offered more opportunities for surprise. (Surprise, by definition, involves deviation from some recognized pattern.)

A preference for a moderate degree of novelty has evolved in various species. Even very young babies (and monkeys) can be rewarded simply by giving them new images to look at. This novelty preference isn't an input-triggered *module*, like Orians's settlement mechanism, but an aspect of information processing in many different domains. In general, then, one can expect that moderately surprising behaviour on the part of a potential mate will be attractive.

If 'surprisingness' is combined with fitness indicators such as intelligence, determination, and muscular control, so much the better. Someone who can carve a symmetrical hand-axe, paint a geometrical design onto a pot or a T-shirt, or dance and drum to a complex beat, must have all of these. (Hence the fact noted long ago by Boas, that all cultures find skilled craftsmanship beautiful.) So someone capable of making a deliberate *departure* from the familiar style (e.g. by breaking the symmetry) isn't only advertising their intelligence and self-control, but is also surprising—and thereby attracting—the perceiver.

The departure mustn't be too great. If there's no intelligible connection with the previous style, the surprise won't be welcomed—as the Parisian Salon des Refusés showed (Boden 1990*a*/2004, ch. 4). But moderate pattern breaking is favoured. (What counts as moderate varies: 'traditional' cultures tolerate a lesser degree of change than 'modern' cultures do, and even these include more and less change-tolerant subcultures.)

Miller didn't claim that sexual selection is the *only* principle underlying cultural variation and individual creativity. It certainly can't explain why a particular society favours a particular style. The geometrical nature of Islamic art, for instance, has nothing directly to do with biology but is explained by theological objections to representing human figures. In other words, Miller was no more a cultural reductionist than Barkow was.

He even allowed that it's not easy to assess just how strong the evidence for his position is. For example, if mate selection were the *only* factor, then males should write more books, compose more music, win more Nobel Prizes, and lead more political and religious campaigns than females do; and their interest in such cultural patterns should rise markedly at puberty. And indeed, that's so: "Demographic data show not only a large sex difference in display rates for [creative] behaviors, but male display rates for most activities peaking between the ages of 20 and 30" (82–3). But there are many *cultural* factors operating too. If women (still) produce fewer aesthetically valued behaviours than men, that's partly because their cultures (still) offer them fewer opportunities, fewer role models, less self-confidence, and sparser encouragement when they do break cultural moulds.

In sum: this was a form of the MMM thesis (7.vi.e). Cultures have developed largely in the service of mate selection, thanks to domain-general psychological mechanisms for generating, detecting, and valuing changes in behaviour. But domain-specific mechanisms, such as a preference for certain landscapes or body ratios, have evolved too. That's why *universal* savanna preferences coexist with a huge *variety* of cultural patterns. The description of this extraordinary variety is what anthropology is about.

These explanations of aesthetic preferences have drawn on examples of *biological* evolution. But people sometimes speak of *cultural* evolution, too. What have cognitive scientists said about that?

8.v. Cultural Evolution

Darwin himself, in *The Descent of Man*, suggested that cultures evolve. And clearly they do, in some sense. After all, ''evolution'' is loosely used today to mean any putatively progressive series of changes. It can even be used to describe putatively *regressive* changes, such as the current secularization of Anglicanism bemoaned by the Chancellor of York (J. Norman 2002; see Chapter 7.vi.d). The word was used with various meanings in Darwin's time too—for instance by Herbert Spencer (1820–1903), who wrote at length about social evolution (Robert M. Young 1967). That's why Darwin avoided the term, speaking of ''descent with modification'' instead (2.vii.e).

Unlike some of his predecessors, he wasn't merely describing evolution. He was explaining it—and in a new way. The question for Darwinian anthropologists, then, is whether *descent with modification* can explain the process of cultural change.

The first person to explore this idea seriously was William James, who opened a lecture to the Harvard Natural History Society by saying:

> A remarkable parallel, which I think has never been noticed, obtains between the facts of social evolution on the one hand and of zoological evolution as expounded by Mr. Darwin on the other. (James 1880: 441)

He went on to praise Darwin for separating ''the causes of production'' and ''the causes of maintenance'' of variations. Social evolutionists, he said, can ignore the first—much as Darwin ignored them in the physiological domain. Their question, rather, is how sociocultural *selection* (maintenance) happens.

In modern terms, James's distinction is between *how cultural ideas arise* and *how they spread*. Cognitive science has had something to say about both.

Today, there are five 'schools' of cultural evolution, each offering slightly different answers to James's questions (Laland and Brown 2002).

* The first three have been mentioned already: sociobiology (e.g. Wilson on biophilia); then human behavioural ecology (including the work of Orians and the Kaplans); and evolutionary psychology (e.g. Barkow—but see also subsection b, below).
* The last two to emerge were gene-culture coevolution and memetics (subsections b and c, respectively).

It has taken some fifty years for those five schools to develop. The beginnings lay in the mid-twentieth century. By the 1960s, several people—from several disciplines—were using Darwinian ideas in trying to answer James's two questions. Initially, however, the second question was more prominent than the first.

a. Evolution in the third world

One mid-century Darwinian was the cultural anthropologist George Murdock (1956), then at Yale (where he'd recently taught Goodenough). He'd already had an impact on anthropology in the 1940s, having provided a systematic empirical "coding" of social institutions which he'd applied to some 300 cultures. This classification encompassed a society's customs (such as rituals, punishments, and rewards), beliefs, and collective ideas. Now, he suggested using these cross-cultural comparisons to study cultural change, and the extent to which it resembled evolution.

However, Murdock was a methodologist rather than a 'grand theorist'. He was more interested in showing how specific cultural claims could be stated and tested than in making general claims himself. A more widely influential mid-century voice was the philosopher Karl Popper (1902–94).

Popper wasn't interested in anthropology as such, caring little about what different peoples happen to believe about this or that. But he was interested in how a society can develop a viable political system (1944/1957, 1945), and in how science can progress in a rational manner (1935, 1963). Ever since the 1930s–1940s he'd explained both these things in terms of carefully constrained trial and error, or "conjectures and refutations"—but without mentioning Darwin.

The initial ideas or hypotheses, he said, could come from anywhere. Politicians can draw on common sense, social science, historical example, and the like. Scientific ideas can originate not only in the laboratory but also in dreams, fables, religion, or even metaphysics. (This was an unorthodox view in the 1930s, for the logical positivists of the Vienna Circle had declared metaphysics to be meaningless: Ayer 1936.) In short, the context of discovery (what James had called "the causes of production") was very different from the context of justification ("the causes of maintenance"). In the first, anything goes. But the second is ruled by Popper's criteria of falsifiability and resistance to falsification.

In the late 1960s, he put his position in more explicitly Darwinian terms (1965, 1968). The unconstrained context of discovery was now a source of 'variation'. Falsifiability and lack-of-falsification were scientific 'fitness' and 'survival'. And epistemology in general was to be thought of in an evolutionary way. So, for instance:

> The growth of knowledge . . . is not a repetitive or a cumulative process but one of error-elimination. It is Darwinian selection . . . Classical epistemology . . . can only be described as pre-Darwinian. (1968, sect. 8)

(A digression, which relates to Section iv.b above: At that stage, Popper thought Darwin's theory of natural selection—the survival of the fittest—to be "almost tautological": a piece of unfalsifiable metaphysics, albeit prompting questions of great scientific interest—1965, sect. xviii. Later, he recanted, saying that the theory was not only testable but in some cases falsified—1978: 344 ff. For, as Darwin himself had pointed out, the peacock's tail isn't "useful". It's the result of sexual selection, not utility-driven natural selection—cf. Cronin 1991.)

Popper argued that human evolution in general is largely driven by our use of "exosomatic organs", comparable to wolves' lairs or beavers' dams. These included not only material artefacts—weapons, paper, computers (used to do sums: "to support argumentation")—but *intangible* objects too, namely publicly accessible ideas. He even posited a "third world", alongside the physical and mental worlds, to hold those ideas. Scientific discovery, he said, happens not (subjectively) within the minds of individuals but (objectively) within the scientific community. A new idea can't become part of science unless it's communicated, made available for criticism. (This was a new interpretation of the Cartesian "cooperation" necessary for science: see 2.ii.b–c.)

Popper's evolutionary ideas had an effect. Other philosophers, such as Stephen Toulmin (1961, 1972), applied them to a wide range of historical examples within the 'culture' of science. The biologist Richard Dawkins (1941–) would later acknowledge Popper's position on the third world as an anticipation of "memes" (1976: 204). And as we'll now see, they entered psychology too—reinforcing a Darwinian account that had been embarked on independently.

b. A new mantra: BVSR

Science—and cognition in general—was seen in evolutionary terms also by the psychologist Campbell (1916–96). A student of Edward Tolman and Egon Brunswik in the 1940s (5.iii.b), Campbell spent most of his professional life at Northwestern University. He started out as a social psychologist, with a special interest in cross-cultural psychology. He soon became a leading expert in experimental/statistical methodology, which helped to earn him the presidency of the American Psychological Association in 1974.

But Campbell was much more than a methodologist. His professional interests ran all the way from cybernetics, through perception in New Guinea, to practical pedagogy. His William James Lectures in 1977 were on 'Descriptive Epistemology: Psychological, Sociological, Evolutionary'. His *New York Times* obituary was headed 'Master of Many Disciplines'. And he died as Emeritus Professor (at Lehigh University, Pennsylvania) of Sociology, Anthropology, Psychology, and Education.

The root of this unusual interdisciplinarity *wasn't* a commitment to mind-as-machine: Campbell was a cognitive psychologist, not a cognitive scientist. However, in his Presidential Address to the APA, he dated his "fascination" with evolutionary theory from his reading of W. Ross Ashby's *Design for a Brain* in 1952. In that book, he said, "the formal analogy between natural selection and trial and error learning is made clear" (Campbell 1975: 1105). That formal analogy underlay his interdisciplinarity, for he became convinced that, where Darwinism is concerned, one size fits all. In other words, he used evolutionary theory as what Daniel Dennett (1995*a*) would later call a "universal acid", capable of solving/dissolving every problem within the social and biological sciences.

By the early 1960s, he'd already sketched 'Darwinian' accounts of perception and creative thinking (Campbell 1956, 1960). Like the New Look psychologists (Chapter 6.ii), he'd compared perception to scientific reasoning. But unlike them, he described both in evolutionary language. That vision and science can provide us with world knowledge, he felt, is just as amazing as the intricate anatomy of the eye itself. (As he put it later, "Does the power of visual perception to reveal the physical world seem so great as nearly to defy explanation?", and "Do you marvel at the achievements of modern science, at the fit between scientific theories and the aspects of the world they purport to describe?"—1974*a*: 139.)

He'd originally been inspired by Brunswik. But when Popper's philosophy of science was translated in 1959, it found a ready reader in Campbell. (He cited it, for instance, in his paper of 1965: 27). Soon, he started sprinkling his Darwinian acid over other aspects of culture too (Campbell 1965).

Like all acids worthy of the name, Campbell's had a formula: BVSR. This stood for "Blind Variation and Selective Retention". That was his way of expressing what Darwin had called "descent with modification". But instead of applying it to physical morphology and behaviour, he—like Popper, at around the same time—applied it to the anatomy and influence of cultural ideas.

There was nothing metaphorical about this, for Campbell had defined BVSR in abstract terms: blind variation and selective retention of items within category *x*. Category *x* might be biological (species, feathers, kidneys . . .), or cultural (artefacts, dance, kinship systems, science, religion . . .), or even computational (cellular automata, robots . . .). It's because evolution can be understood abstractly that A-Life workers can sensibly say that their systems evolve (15.vi).

John von Neumann had realized this in the mid-1950s (15.v). But when Campbell defined BVSR twenty years afterwards, von Neumann's later work was still largely unknown even in the AI community. For psychologists and anthropologists, BVSR was a new idea. (Many would have read Popper, of course; but Campbell made more of an effort to match specific concepts of Darwinian biology.)

A respectable theory of BVSR, he said, requires one to specify "mechanisms for introducing variation", a "consistent selection process", and "mechanisms for preserving and/or propagating the selected variants" (1960; cf. 1965: 27). More specifically, it requires one to ask to what extent biological concepts can find analogues in cultural contexts. And in a move to placate the "cultural relativists" in anthropology, who "emphasize the excellent adaptedness and internal coherence of simple cultures as of complex ones" and who reject notions of 'progressive' evolution accordingly, he said:

> [One] may be a cultural relativist within the framework of a correct [i.e. non-'progressive'] social evolutionary theory. Indeed, the relativist's frequent emphasis upon the adaptiveness or functional validity of customs that seem bizarre to outsiders *implies* a selective retention and elimination process. (1965: 39; italics added)

Despite his disclaimer regarding evolution-as-progress, Campbell's approach was most avidly taken up by historians of science and, especially, technology (e.g. Basalla 1988). For in the latter case, detailed information about the structure and development of artefacts was relatively accessible. Moreover, it seemed—to some extent (see below)—to fit. So Ziman has said:

> In my own limited reading [anyone who knew Ziman will take that phrase with a pinch of salt: M.A.B.] I have come across suggested technological analogues of what an evolutionary theorist would term *diversification*, *speciation*, *convergence*, *stasis*, *evolutionary drift*, *satisficing fitness*, *developmental lock*, *vestiges*, *niche competition*, *punctuated equilibrium*, *emergence*, *extinctions*, *coevolutionary stable strategies*, *arms races*, *ecological interdependence*, *increasing complexity*, *self-organization*, *unpredictability*, *path dependence*, *irreversibility*, and *progress*. (Ziman 2000*b*: 4–5)

Like Popper before him, Campbell saw epistemology in general through Darwin-tinted spectacles. He spoke of "a continual breakout from boundaries", followed by testing and further development/breakouts towards increasingly "reliable" knowledge. In short, he used BVSR as the core of an "evolutionary epistemology", or EE (Campbell 1974*a*,*b*).

This was a descriptive enterprise, not a normative one. It was therefore "undertaking a different task from that of traditional analytic epistemology" (1974*a*: 140). In Fregean terms, it wasn't philosophy, but psychology (see 2.ix.b). However, some leading philosophers, notably Toulmin (1961, 1972) and Willard Quine (1969), were engaged in a similar activity—what Quine called "naturalized epistemology".

Campbell allowed that *within professional philosophy* this was "a minor heresy" (1974*a*: 140). Within psychology, it began as a heresy too. But it grew apace. For when "evolutionary psychology" surged in the early 1980s, Campbell was there waiting. His EE approach now got favourable attention, for example, from the biologist Plotkin (1982), from philosophers of biology such as David Hull (1982, 1988*a*), and from the multi-talented duo of Robert Boyd and Peter Richerson (Boyd and Richerson 1985).

Boyd (1948–) was a member of UCLA's Department of Anthropology, though his initial training had been in physics and ecology (including energy management). Richerson (1943–) was an environmental scientist and zoologist, at UC Davis. As ecologists, they favoured theories of biological evolution focused at the level of *genetic populations*—which led on naturally to concerns about *cultures*. They developed a "dual inheritance" approach, showing how genes can influence cultural evolution and how culture can affect biological evolution. (They were later criticized by some anthropologists for neglecting cultural evolution *as such*, i.e. change wholly within the realm of cultural constructs—much of which is neutral with respect to biological fitness—Durham 1991: 437 ff.)

The Stanford geneticists Luigi Cavalli-Sforza and Marcus Feldman had been modelling the co-evolution of biology and culture even earlier (1973, 1981). Initially, they'd focused on matching DNA to human populations, and later moved on to the evolution of languages. The work on language has been roundly criticized by the Sussex linguist Larry Trask (Trask 1996: 376–404). I mention it here not because it was valuable but because it was an influential example of the evolutionary Zeitgeist. Boyd and Richerson were informed by that Zeitgeist too. However, they were looking at examples of specific cultural practices described by anthropologists.

They'd published their first joint paper comparing cultural and biological evolution in the mid-1970s (Richerson and Boyd 1976), and many more had appeared since then. Their 1985 book, which pulled their previous work together, was even more interdisciplinary than most exercises in cognitive anthropology. Perhaps for that very reason, it wasn't generally thought of under the label "anthropology" (see Section ii.d). But it threw new light on the nature of culture.

The key questions concerned what sort of mind/brain is capable of imbibing (and contributing to) culture, and how/why it evolved. The main influences on cultural evolution, they said, include "biased transmission": the fact that the process of cultural transmission itself can favour certain variants over others. This can take place in three ways.

"Direct" bias happens when people adopt an idea, or practice, because they judge it to be valuable. (Jogging is 'good for you'.) "Indirect" bias occurs when a variant is selected because it's associated with a valued variant. (Role models admired for their success are often copied in ways that have nothing to do with the feature which made them valued in the first place.) Third, "frequency dependent" bias is in play when the spread of a cultural variant among a population affects its probability of adoption by later generations. (A tourist resort can become increasingly fashionable merely because it's already fashionable; but it may later lose custom because people choose to swim against the tide.) These factors, and especially the second, explain why so many cultural traits have no clear advantage—and why some can even be maladaptive.

Boyd and Richerson identified various types of cultural subsystem, correlated with particular psychological mechanisms for 'deciding on' and effecting their transmission. The correlations weren't merely superficial, still less accidental. On the contrary, they were explained as adaptations to distinct kinds of environment—not rocks or trees or expanses of water, but different statistical patterns of variation.

The two authors drew on rich ethnographic data in expressing, and exploring, their theories. But they had another way of doing this too, namely, mathematical modelling. They defined a number of models detailing how ideas ("information in brains") might be conserved and altered, and how they might be affected by different environmental conditions—including the variability of other animals' behaviour.

For example, they showed that whether imitation ("social learning") is more adaptive than working out how to do something for oneself depends on how "cost-effective" that learning would be. Different statistical properties in the environment favour different types of social learning. One is blind copying of some—any—conspecific. Others are guided by heuristics, such as "copy dominants" or "go with the majority" (which require the capacity to recognize dominants and majorities). High variability in the environment is correlated with the ability to make complex cognitive analyses of the problem situation. And human behaviour, of course, is hugely variable.

In other words, some results of their formal models underscored the more intuitive arguments of Geoffrey Miller (Section iv.b, above). Human culture is not only made possible by human intelligence, but drives it to evolve further. (In their most extensive discussion of intelligence, they attributed increase in brain size to the evolution of cognitive strategies adapted to environmental deterioration: Richerson and Boyd 2000.)

Boyd and Richerson weren't merely expressing vague analogies between biology and culture. They were doing a systematic study of well-defined cultural possibilities, grounded in what was known about computational psychology—and anthropology, ecology, and biology. Much as Hutchins would later show how different patterns of communication emerge from different network models, so they showed how distinct cultural phenomena would result from distinct computational assumptions.

Today, some twenty years later, EE is a thriving industry (e.g. Radnitzky and Bartley 1987; Campbell and Cziko 1990; Heyes and Hull 2001). It became an "industry" partly because there was much disagreement about just how relevant BVSR really is to cognition and culture. For example, was Ziman right to claim that those twenty-one neo-Darwinian concepts could all be applied to cultural (technological) change? Answering such questions turned out to be a very lengthy, and still ongoing, exercise.

Here, let's just note two main dimensions of discussion:

* How blind is Blind?, and
* What are the units of cultural variation?

Darwin hadn't insisted on absolutely blind variation, for his occasional willingness to consider "panspermia" allowed for the inheritance of acquired characteristics. (Panspermia was the idea that the sex cells are formed by the agglomeration of tiny 'copies' of cells sent from all parts of the body.) However, he believed that descent with modification could be blind, and often said that it is. That is, variations don't happen because of their likely biological usefulness. In particular, he rejected Jean-Baptiste Lamarck's view that *conscious desire and effort* can result in (acquired) heritable variations.

His views prevailed. Thanks to the work of August Weismann in the 1890s (Chapter 2.vii.e), plus modern genetics, twentieth-century neo-Darwinism taught that "Blind" really did mean *blind*. Hence the title of Dawkins's book (the one in which he introduced his A-Life "biomorphs"), *The Blind Watchmaker* (1986). Admittedly, recent research suggests that there may be tiny glimmers of 'vision' in the origin of some biological variations (Jablonka 2000, esp. 33 ff.). In general, however, blindness rules.

This posed a problem for cultural BVSR, because cultural/conceptual variations are rarely generated blindly. To the contrary, they're highly Lamarckian. People—from mechanical engineers to Popper's "experimental" politicians—typically have a specific goal and/or fitness criterion in mind when they try to come up with new ideas. The anthropologist William Durham, for instance, intimated as much in the 1970s and spelt it out at length some years later (Durham 1976, 1979, 1991).

BVSR devotees, however, argued that this didn't make Campbell's formula irrelevant. The way he'd put it was to say that "In going beyond what is already known, one cannot but go blindly" (1974*b*). That was true *in principle*, given a strict interpretation of "beyond what is already known". But how true was it *in practice*?

Detailed studies by historians of technology provided an answer, for they showed that even the most carefully designed artefacts have some unpredictable features. Their fitness for their intended purpose therefore has to be tested—and developed—in the real world (Ziman 2000*b*,*c*; Wheeler *et al*. 2002). That's even more true of politics, as Popper had taken pains to point out. And it's true also of creative ideas in art, which typically develop by continual interaction between the artist and his/her still-uncompleted artefact or text (A. Harrison 1978). In short, although people evolving new ideas aren't completely blind, their vision is only limited.

The second dimension of disagreement about EE concerned the units of variation: what are they? Here, Campbell's writings were largely eclipsed in the late 1970s by Dawkins's. This wasn't because Dawkins was saying anything essentially different, but because his lucid, even racy, prose demanded less from the reader than Campbell's did. In particular, he provided a seductive new label: *memes*.

c. The meme of memes

Dawkins's phenomenally successful *The Selfish Gene* (1976) kicked off the popular surge of interest in Darwinism in the last quarter-century. It sold to all manner of people, and was translated into many languages. One person enthused by it was the Microsoft billionaire Charles Simonyi, who began recommending its ideas to his co-workers in the early 1980s (A. Lynch, personal communication). In the 1990s, Simonyi founded the Chair of the Public Understanding of Science at Oxford, specially designed for Dawkins.

Most of the book concerned the evolution of physical phenotypes: flowers, feathers, eyes, and so on. The final thirteen-page chapter, however, dealt with 'Memes'. And the penultimate sentence declared: "We are built as gene machines *and cultured as meme machines* . . . " (1976: 215; italics added).

Like genes, said Dawkins, memes are "replicators", whose success requires three properties: longevity, fecundity, and copying fidelity (p. 208). They are the units of variation in evolving cultures: a meme is "a unit of cultural transmission, or a unit of *imitation*" (p. 206). Examples included "tunes, ideas, catch phrases, clothes fashions [e.g. stiletto heels], ways of making pots or of building arches". In other words, any concept, belief, or cultural practice counted as a meme. Interlinked groups of memes were "meme-complexes", which included whole cultures, specific belief systems, and socially established styles of doing things.

The meme-complexes attributed to "Socrates, Leonardo, Copernicus, and Marconi" have survived hundreds of years longer than the four men themselves (p. 214). So far, so banal. But now Dawkins made a startling claim:

> [When] we look at the evolution of cultural traits and at their survival value, we must be clear *whose* survival we are talking about. Biologists . . . are accustomed to looking for advantages at the gene level (or the individual, the group, or the species level according to taste). What we have not previously considered is that a cultural trait may have evolved in the way that it has, simply because it is *advantageous to itself*. (p. 214)

"Advantage", for memes, meant survival in the minds of mankind. And the more the merrier: a meme's fitness was its capability of being imitated, so as to enter human minds as quickly, widely, and stably as possible.

It followed, he said, that we don't *need* to explain prominent cultural traits—music and dancing, for example—in terms of biological survival value. To be sure, many of them (even including his *bête noire*, religion) may actually have such value. They may aid group cohesion, for instance. Or they may increase fitness by way of Darwin's second evolutionary principle, sexual selection (Section iv.b, above). But the basic evolutionary concern is the meme's "selfish" ability to survive and spread in the minds/brains of enculturated individuals.

Dawkins described memes as "viruses of the mind". Mostly, he compared them to biological viruses:

> When you plant a fertile meme in my mind, you *literally* parasitize my brain, turning it into a vehicle for the meme's propagation *in just the same way* that a virus may parasitize the genetic mechanism of a host cell. (1976: 207; italics added)

But sometimes he spoke of "informational" computer viruses, too:

> For data on a floppy disc, a computer is a humming paradise just as cell nuclei hum with eagerness to duplicate DNA . . . Computers are so good at copying bytes . . . that they are sitting ducks to self-replicating programs: wide open to subversion by software parasites. (Dawkins 1993)

The analogy had originally been drawn by the psychologist Nicholas Humphrey. On reading a draft of his friend's book, Humphrey had remarked that memes are like viruses, since they propagate themselves by "parasitizing" human brains (Dawkins 1976: 206–7).

That applied to *all* memes, so Dawkins often described memes-in-general as viruses. Sometimes, however, he seemed to be thinking of viruses as disease bearers—in which case beliefs "backed by good reasons" such as science weren't included, but religious beliefs were (see below). (A later writer described memes more neutrally, as "contagious", pointing out that laughter, happiness, and joy are contagious too: Lynch 1996, 2002.)

If Dawkins's view was "startling" for most of his readers, it was less so for some anthropologists. For (as he pointed out in passing), one of their number—F. Ted Cloak (1931–) at the University of North Carolina—had already said much the same. Dawkins cited a mid-1970s paper by Cloak (1975*a*), in which he'd used the software/hardware distinction in speaking of the "programming" of the nervous system. But there'd been much earlier ones, too (Cloak 1966, 1968, 1973). In subsection d, we'll look more closely at just what Cloak had said, and ask why it still hadn't caught on—as Dawkins's term immediately did.

Six years later, in the more academically oriented *The Extended Phenotype*, Dawkins gave a clearer definition. And he cited Cloak in doing so: "a meme should be regarded as an item of information residing in a brain (Cloak's 'i-culture')" (1982: 109). But the damage had been done. His loose definition of memes on the word's introduction had already led, and would lead again, to various conflicting accounts, some more catholic than others. To add to the confusion, some of those would occur in books almost as popular as Dawkins's—so the notion spread ever more widely.

Dennett, for example, broadcast it far and wide in his best-selling *Consciousness Explained* (Dennett 1990; 1991: 199–226). He used "meme" to include not only neural representations of ideas but also the ideas themselves; the virtual machines comprising mind, self, and consciousness; cultural artefacts; and socially accepted ways of behaving. And, by the way, he mentioned neither Campbell nor Cloak—who'd recently published a fuller version of his "instructional" theory (Cloak 1986). Dawkins got all the credit.

Dennett was one of many. The historian Peter Munz, an ex-pupil of Popper who'd already published books on myth and religion as well as science, offered a version of EE expressed in the language of memes (1993). Memes spawned "memetics", and a new (online) journal was founded in 1997 by Manchester University's "Centre for Policy Modelling", the *Journal of Memetics: Evolutionary Models of Information Transmission*. Two years later, Susan Blackmore (1999) used Dawkins's phrase "meme machine" as the title of a best-selling popular book announcing "a new science": memetics.

For many, that was a step too far. If "meme" had become a buzzword for some cognitive scientists (and for many members of the public), it was regarded with suspicion by others (Aunger 2000). There were two problems: vagueness, and lack of match to psycho-cultural reality.

Those who'd followed Campbell in championing evolutionary accounts of culture didn't necessarily favour talk of memes. The cognitive anthropologist Bloch (2000), for instance, accused the memeticists not only of reinventing anthropological wheels but also of repeating some of the mistakes previously made by his professional colleagues. (The 'invisibility' of anthropology discussed in Section ii presumably hadn't helped.) And the philosopher Hull defended cultural evolution—that is, a focus on heritable variation and competitive selection of cultural features—but had few kind words for memes (D. L. Hull 1982, 1988*b*).

The same was true of Boyd and Richerson. They did sometimes use the word "meme", for convenience. More often, they spoke (technically) of "cultural variants" or (informally) of "ideas", "beliefs", "values", and "skills". They defined culture as "(mostly) information in brains" (forthcoming, ch. 3). They granted that some cultural information is stored in artefacts: pots, for instance. Perhaps, they said, some youngsters learn to decorate pots by looking at old pots rather than talking to old potters. In general, however, cultural knowledge is linguistic.

But memes, they argued, weren't helpful in thinking about cultural change. Dawkins had defined memes as discrete entities, faithfully transmitted from one brain to another. On the contrary, they said, the actual units of cultural variation and inheritance are neither. They denied that Darwinian evolution *necessarily* involves "copying fidelity", arguing (and demonstrating by their models) that blending inheritance is compatible with BVSR evolution. For example, learning (and historical change) in speech pronunciation could conceivably be caused by alternative sets of computations in the infant's mind, one of which computes average (i.e. blended) phonological values in adults' speech. Provided that there are also sources of heritable variation (slight differences in the anatomy of the larynx, for instance, or mishearings on the part of the child), evolution can occur. In sum, cultural variants are *not* close analogues to genes, but "different entities entirely, perhaps a rather diverse class of entities, about which we know distressingly little".

Boyd and Richerson weren't alone. The general view was that the analogy between genes and memes isn't nearly as close as Dawkins had suggested. Sceptics pointed out that memes—unlike genes—can't be crisply identified. They appear to include concepts, schemata, scripts, theories, cultures, and cultural practices, none of which are easily defined. (Consider Rosch's work on concepts, for example: Section i.b, above.)

They aren't always copied faithfully, but are often contaminated by new cultural associations, or by metaphor. Indeed, such contamination can be a matter of 'Lamarckian' choice. They don't have separate lineages, for while one can imitate one's parents' or neighbours' ideas one can also (thanks to books and other media) imitate ideas far away in time and space. They can be discarded and replaced by others, whether voluntarily or not: Paul on the road to Damascus is one example, acceptance of a new scientific theory another. And whereas genes are clearly distinct from the phenotypic characters they 'cause', memes are seen sometimes as the brain mechanisms that underlie a concept or cultural practice and sometimes as those phenomena themselves.

None of this controversy denied that anthropologists had been on the right lines when they'd used certain types of pottery, or specific elements of myth, to indicate past cultural movements and influences. Such evidence was just what an evolutionary approach would favour. Similarly, anthropologists' accounts of one tribal group's gaining dominance over another could now be understood as describing "group selection in action" (Sober and Wilson 1998: 191). But, quite apart from the non-blindness of Blind, BVSR should be applied to cultural matters only with care—and it didn't gain from pseudo-scientific talk of memes.

d. Cloak uncloaked

Dawkins's theory of memes had been anticipated, and in some detail—but neither he nor his non-anthropological readers realized it. Indeed, not all of the anthropological readers did. For the relevant work wasn't well-known. Cloak, one might say, was cloaked—so much so, as to be near-invisible.

His first two papers on cultural evolution had appeared in a highly obscure source, not held today by either the British Library or the Library of Congress. (I was given copies by his follower Aaron Lynch.) And an important lecture given at an anthropological gathering in 1973 had—by accident—remained officially unpublished, although it was available in the conference preprints and from Cloak himself. Indeed, he was eventually denied tenure—and at least one intending graduate student left the profession in disgust as a result (Aaron Lynch, personal communication).

This setback may have been part-grounded in the scantiness of Cloak's publications list. But perhaps his colleagues also felt that his "radical" suggestions for restructuring the whole of anthropology were too ambitious, not to mention too uncomfortable. In addition, his grandiose claim that his theory could account for "all known biological, social, and cultural phenomena" was likely to invite scepticism, not to say mockery.

What was radical about Cloak's approach wasn't any commitment to computational theorizing. His first relevant paper appeared only a few months after Wallace's, but was very different. It didn't rely on ideas about computational processes, whether from MGP or anyone else. However, it did focus on the transmission of *information* ("instructions"), both cultural and genetic; and by the early 1970s he was talking about the "software" and "programming" of the nervous system. In that sense, he was aligned to the early cognitive scientists, Wallace included—and working at a higher level of abstraction than most anthropologists.

In an address written for the AAA's annual meeting in 1966, Cloak had argued that previous comparisons between biological and cultural evolution had picked the wrong biological units (i.e. species) on which to base the analogy. The important likeness, he said, was to gene flow (the diffusion of instructions) between geographically separate but reproductively linked populations. And he continued:

> I must beg leave to enter a provisional concept here, *analogous to the gene*, defining it as "that which is transmitted when cultural diffusion takes place". I call it the *unit of cultural instruction*, or *UCI for short, putting an asterisk before it as a reminder that it is provisional. (Cloak 1966: 8; first italics added, second in original)

As in the biological case, he said, an idea (*UCI) survives if it has some adaptive advantage—"technological, social, or moral"—for its bearer. (He would retract this later: see below). But unlike the biological case, cultural diffusion requires that the new ideas be "selected" and "fixed" not by chance but according to specific structural features of the receiving culture.

Cloak ended his address by referring gnomically to unspecified "empirical evidence" showing that the sequence of cultural transmission is "predictable". He wanted his anthropological audience to focus on theory, not ethnographic detail:

> [I intend to continue my empirical studies of cultural change, but] I think that more attention should be directed to *the theoretical problem of the nature of the unit of cultural instruction* (the *UCI) and the principles by which *UCI's and cultural structures interact in the ongoing process of cultural microevolution. (1966: 10; italics added)

Two years later, his new theoretical unit had become less "provisional". Cloak (1968) used the work of Niko Tinbergen and Konrad Lorenz (Chapter 5.ii.c) as the inspiration for a "cultural ethology", which distinguished "behavioral propensities" from "modes of acquisition". Cultural learning, he said, includes imitation and receipt of verbal instructions—which can affect the manner in which even inborn propensities are exercised. So women flirt differently in different cultures (1968: 39). The "inborn" components (the smile, the enlarging of the eyes, and the wrinkling of the skin at the bridge of the nose) occur in different orders, and some components—such as the flourishing of the nail varnish, or the fluttering of the fan—are wholly cultural.

In general, he said, studying the distribution, replication (*sic*), and if possible the natural order of adoption and loss, of behavioural variations within and between cultures will "broaden our understanding not only of the history of particular cultures but of *the mechanisms which control culture change*" (p. 41; italics added). He'd already done this, in relation to behavioural replication in immigrants to Trinidad (this was the empirical evidence he'd hinted at in his AAA address). The cultural patterns he'd studied ran from 'meaningless' phrases in ancient songs (compare: *Ring-a-ring of roses, a pocket full of posies* originally referred to the plague) to stock answers to genuine questions, answers that often were not only incomplete but two-thirds false: *Q. What do you grow in your garden? A. Corn–peas–cassava* (1968: 43).

The *corn–peas–cassava* stock phrase soon acquired a thirteen-syllable label (with no defensive asterisk). Three years before Dawkins's book appeared, Cloak (1973) addressed an international gathering of anthropologists, saying:

> Elementary self-replicating instructions [ESRIs, for short] are the constructors of life and, given life, of culture (in the sense that coral polyps are the constructors of a reef).

Crucially, those ESRIs could be interpreted materially (as genes, or neural structures) or informationally (as ideas). So he'd prefaced his talk by declaring:

> A reconstruction of Darwinism, the proposed theory . . . *brings genetic and cultural evolution into a common conceptual framework, including a common system of notation*, and it reconciles hitherto opposed viewpoints in cultural anthropology . . . For example, the evolution of functional *social* "structures", as well as of functional *material* structures, is rendered explicable.
>
> Materialistic, naturalistic, mechanistic, deterministic, the theory purports to account for all known biological, social, and cultural phenomena and to contain no terms or concepts not reducible, in principle, to physico-chemical terms. . . .
>
> *Key terms developed in the communication include*: 'determinant', 'behavioral event', 'event of natural selection' ('ENS'), 'replication', 'self-replication', 'exploitation', 'domestication', 'system (of instructions)', 'function (in a system)', 'environmental sub-region', 'frontier (between sub-regions)', 'evolutionary event', 'organism'. (Cloak 1973: 1; italics added)

In short, Cloak had outlined "a radical reconstruction of general anthropology" in Darwinian terms. And he'd done so by trying to define cultural equivalents of over a dozen key biological concepts.

In his Congress talk, and in his 1975*a* paper too, Cloak had distinguished between "i-culture" and "m-culture". The i-culture consisted of the cultural "instructions" carried in the heads of the members, while the m-culture was the material artefacts and physical practices produced by the i-culture. Dawkins mentioned that distinction. But he didn't point out that Cloak had also said this:

> [The] survival value of *a cultural instruction* is the same as its function; it is *its value for the survival/replication of itself or its replica(s), irrespective of its value for the survival/replication of the organism which carries it* or of the organism's conspecifics. (Cloak 1975*a*: 168; italics added)

That is, Cloak had come up with the very same idea which Dawkins expressed by calling memes "selfish". By this, Dawkins explained, he meant: "Once the genes have provided their survival machines [i.e. animals/humans] with brains which are capable of rapid imitation, the memes will automatically take over" (Dawkins 1976: 214).

"Meme" itself was a widely imitated meme, as we've seen. Cloak's alternatives, "unit of cultural instruction", "elementary self-replicating instruction", and "elementary cultural replicator", weren't. Like Barkow's "Darwinian psychological anthropology", they were non-memorable mouthfuls. By contrast, Dawkins's pithy (and gene-rhyming) "meme" was a jewel of rhetorical skill—and, as such, more likely to enter the history books (see Chapter 1.iii.h).

What's more, Cloak's 1973 discussion (though not his two 1975 papers) was highly formal—and very dry, with nary a concrete example. Even the leading anthropologists sitting in the Congress hall, or perusing it at their leisure in the preprints, were likely to find it tough going. For 99 per cent of the readers of *The Selfish Gene*, it would have been altogether too taxing. So one can't reasonably wish that the general public had been subjected to the anthropologist Cloak, rather than the biologist Dawkins. Cloak's work would have left them cold.

One can reasonably regret, however, that few academic readers had looked at it. For as Cloak's list of "key terms" (quoted above) suggests, his account was much more precise than Dawkins's notion of memes. Indeed, in the year when *The Selfish Gene* appeared, the luminary Campbell (1976: 381) was already describing Cloak as "one of the most meticulous and creative thinkers about social evolution".

Campbell had been one of the reviewers for Cloak's 1975*a* paper, received by the journal editor as early as February 1973 (A. Lynch, personal communication). He was so enthusiastic that he wrote to ask Cloak for copies of his earlier publications. And the remark just quoted was made in an APA Presidential Address which drew many published comments, including one from the sociobiologist Wilson.

What's more, Cloak himself was in the habit of sending his unpublished work out quite widely. (The final section of his 1975*a* paper said his ideas on culture should be spread "into such areas as ethology, anthropology, human ecology, philosophy and logic of science, computer science, statistics, experimental psychology, and neurophysiology".) And his 1975*b* paper had already been presented to the 1974 meetings of the Animal Behavior Society *as well as* the Central States Anthropological Society.

In sum, it's possible that Cloak's ideas (though not necessarily his name) had spread quite widely among sociobiologists and evolutionary psychologists by the time that Dawkins wrote *The Selfish Gene*. If so, that would help explain the rapid take-up of the rhetorically attractive meme of *meme*.

8.vi. The Believable and the Bizarre

Ever since Tylor's *Primitive Culture* (1871) and James Frazer's even more widely read *The Golden Bough* (1890), anthropologists have reported exotic beliefs about all manner of things. These range from cookery and kinship, through flora and fauna, sickness and death, all the way to shamans and religion.

The cautious have done so with some trepidation. As Humboldt pointed out long ago, identifying another culture's concepts and beliefs is problematic in principle (Chapter 9.iv.b). It's not even straightforward in *one's own* culture—which is why some philosophers argue that folk psychology can't be the basis of a science (see Chapter 16.iv.b).

For instance, do the Micronesian navigators really believe that the islands move? They *see* them that way, to be sure (E. L. Hutchins and Hinton 1984). But just what does that prove? And should one assume that all the members of a given culture share the same beliefs? Do all Polynesians, from Tuvalu to Samoa, Aoteoroa to Rapa Nui, hold the same beliefs about their ancestral homeland Havaiki? (No: for one thing, it's less celebrated in western Polynesia—Bellwood 1987: 68.) Do all modern Americans think of *marriage* in the same way? (No, despite an eightfold similarity: see Section i.d, above.) Simply *defining* "culture" in terms of (universally?) shared beliefs merely begs the question.

Gallons of ink have been spilt on these issues, by philosophers and methodologists alike (e.g. Winch 1958 and Clifford and Marcus 1986, respectively). Let's ignore them here, however, and assume that foreign ideas can be pretty well understood. Let's focus instead on three undeniable facts.

First, some alien beliefs are not just unfamiliar but—to us (i.e. the likely readers of this book)—bizarre in the extreme. And, no doubt, vice versa.

Second, others can strike us as deeply *familiar*. That's so even though they may be accompanied, in their home culture, by practices or pronouncements which we find very odd indeed.

And third, 'strange' ideas are accepted even within one's own society—and some spread like wildfire. Think of backwards-facing baseball caps. Or if that's too trivial, and too hideous, to contemplate, think of religious cults: the Moonies, perhaps, or the interpreters of messages from the planet Clarion (Chapter 7.i.c).

A number of questions arise, some of them very old. For instance:

* Why do some new ideas spread faster than others? Certainly, cults often employ ingenious brainwashing techniques to make conversions. But are they more likely to succeed with some ideas rather than others—and if so, why?
* Why do 'meaning-less' rituals (behaviour and dress) survive for centuries, even in some parliamentary democracies?
* Why do religious people, everywhere, make claims which they know to be highly counter-intuitive? Pick your own bugbear, and ask: How could anyone believe *that*?
* Finally, are there any limits? Or could *any* belief, no matter how strange, thrive in some culture, somewhere?

a. An epidemiology of belief

Dawkins had taken it for granted that some ideas (memes) will spread easily and others won't. However, he hadn't said why that is. Or rather, he'd explained it in terms of common-sense psychology. For instance, he'd attributed the spread of Christianity to its threats of hellfire, its promises of salvation and immortality, and its self-protective meme of *faith*—which disarms rational criticism (1976: 212). What he hadn't done was to ask *what cognitive mechanisms* underlie the varying survival of ideas.

The anthropologist Sperber tried to fill the gap. In his Malinowski Lecture at LSE in 1985, he said that "Representations are more or less widely and lastingly distributed, and hence more or less cultural" (see Section ii.d above), and went on to ask why *this* representation becomes "widely and lastingly distributed" while *that* one doesn't. To find the answer, he said, we need an "epidemiology" of ideas, or beliefs, which would explain why some ideas are more "contagious" than others.

In his audience, he might have seen the LSE sociologist Eileen Barker. For she'd long been engaged in a study of late twentieth-century religious cults, including the Moonies for instance (1989; Barker and Warburg 1998). Her work gave fascinating details about their conversion practices, and useful advice on how to resist them. But Sperber wasn't concerned with such brainwashing techniques.

Nor was he concerned with the question of which ideas people, whether cult leaders or anyone else, would actively *choose* to communicate. (He was criticized, accordingly: the metaphor of epidemiology downplayed the Lamarckian aspect of cultural evolution, because it ignored people's valuations of one idea as opposed to another: Durham 1991: 197–201.) Sperber's question, rather, was whether there are some ideas which would be especially easy, or difficult, to disseminate—and if so, why?

In calling for an epidemiology of ideas, he was calling for something he'd already done much to develop. His book on symbolism had appeared ten years earlier, and the one on communication was already in press when he gave this lecture (Sperber 1975; Sperber and Wilson 1986).

That second book was an interdisciplinary mix of linguistics and psychology, with anthropology hovering in the background. Sperber saw cognitive psychology as crucial here. It could illuminate the psychological processes involved in metaphor—a topic that had recently received much attention, some of it relevant to anthropology (Ortony 1979; Lakoff and Johnson 1980). Even more to the point, it could help discover which concepts are remembered well in all cultures (J. M. Mandler *et al.* 1980). His new theory of "relevance" sought to explain *what makes a concept memorable*, whether it's widely dispersed cross-culturally or not.

Sperber saw his theory of cultural evolution as an alternative to Dawkins's account, not an elaboration of it. He insisted that ideas aren't "replicators", since they don't satisfy Dawkins's criterion of copying fidelity. As outlined in Chapter 7.iii.d, he saw communication as an activity that *changes* the hearer's mental representations.

Dawkins seemed (by default) to agree with John Locke (1690), who'd said that communication involves ideas in one mind being passed on to another. But Sperber held that new ideas are actively assimilated, not passively received. Understanding is a matter of making inferences, guided by "cost–benefit" computations concerning one's own cognitive economy. In other words, the information processing involved virtually guarantees that transmission produces variants, not copies:

> What human communication achieves in general is merely some degree of resemblance between the communicator's and the audience's thoughts. Strict replication, if it exists at all, should be viewed just as a limiting case of maximal resemblance. (Sperber 1990: 30)

Moreover, the variants aren't random: the nature of the information processing puts bounds on what they will be.

Dawkins had allowed for meme variation too, of course (1976: 209). After all: no variation, no evolution. But he'd apparently assumed that faithful copying is the norm, as it is in genetics. If the double helix works properly, the new gene is identical to the old one. With respect to the copying *mechanism*, mutations are faults. Analogously, the successful communication of ideas was assumed to involve perfect copying.

As a student of culture, however, Sperber was well aware that the change in the communicated ideas can't be too great:

> [Cultural beliefs differ from personal ones in that] those representations that are repeatedly communicated *and* minimally transformed in the process will end up belonging to the culture. (Sperber 1990: 30; italics added)

What James called the "maintenance" of culture depends on the fact that cultural transmission, despite the inevitable variations, isn't a game of Chinese Whispers. This is the "cultural ratchet" which—together with Theory of Mind (7.vi.f)—enables human creativity, unlike innovations by animals, to endure and even develop through the generations (Tomasello *et al*. 1993).

But how does it work?

In the book that was in the press as he spoke, Sperber and Deirdre Wilson had concentrated on the transmission of banal sentences such as those previously discussed by H. Paul Grice with respect to "speech acts" and "conversational conventions". So *Can you pass the salt?*, *He's a snake!*, and even—an example close to my heart—*It took us a long time to write this book* (pp. 122–3) were more to the point than exotic claims about snakes as totems or 'soul twins'. Cross-cultural examples were mentioned only in passing. In the next ten years, however, he would apply the theory to many such instances, based in a wide range of ethnographic data (Sperber 1990, 1994, 1997*a*).

One of the distinctions made in Sperber's epidemiological theory was between "intuitive" and "reflective" beliefs (1990, 1997*b*). Broadly, an intuitive belief is one which someone holds on the basis of everyday experience, underlain by universally shared cognitive modules. Reflective beliefs, by contrast, are "believed in virtue of second-order beliefs about them". Examples of intuitive beliefs include *That house offers shelter*, *The stone is about to hit the water*, and *She wants me to speak to her*. Examples of reflective beliefs include *Water consists of hydrogen and oxygen*, *Baseball caps are cool*, and *God is everywhere*. Each of these is held on other people's say-so. (So are most scientific, historical, political, economic, legal . . . beliefs—Shapin 1994, ch. 1.) "X says it" is crucial in all three cases. But who counts as an acceptable "X" differs greatly.

With respect to the belief about water, "X" is held to be a *reputable* scientist, or a *trustworthy* journal or institution. That's why the word of a gentleman was so crucial to the foundation of modern science, and why peer reviewing and institutional affiliation are so important today (see Chapter 2.ii.b–c).

This second-order aspect of scientific culture is unavoidable. Even if one decided to check the constitution of water for oneself, one would have to rely on what scientists say about the theoretical reasoning (and instruments) involved. The reflective belief couldn't be turned into an intuitive belief without doing the same for all the scientific propositions involved in its justification. In practice, this isn't possible. So as Popper (and René Descartes) said, science is—and has to be—a *cooperative* enterprise.

The better grounded a reflective belief is, the more likely it is to be transmitted and maintained. Science has become a global epidemic as a result. In Sperber's words:

> Well-understood reflective beliefs . . . include an explicit account of rational grounds to hold them. Their *mutual consistency* and their *consistency with intuitive beliefs* [including experimental observation] can be ascertained, and plays an important, though quite complex, role in their acceptance or rejection. (Sperber 1990: 37; italics added)

In the case of science, *inconsistencies* with intuitive beliefs (experimental observations) are an embarrassment. They're usually disregarded as the result of 'faulty procedure', especially if they disappear on repeating the experiment. However, intractable inconsistencies do sometimes occur. These may be quietly swept under the carpet, or admitted as intriguing "anomalies" awaiting some future theory to cope with them (Kuhn 1962). These examples show that mutual consistency between reflective beliefs can *compensate* for their inconsistency with intuitive beliefs.

(One might expect this compensatory effect to be especially important for religion, where the inconsistency with daily experience is very great. And indeed, theological arguments—including Charles Babbage's analysis of miracles: Chapter 3.i.b—are largely devoted to maximizing internal consistency, and justifying the inconsistencies in the process. So theology may assist the spread of religious ideas among highly educated people. But there's a puzzle here. Many highly literate believers ignore theology, seeing it as an optional extra—or, worse, a trivial pastime of logic-chopping monks and scholars. They don't spend much effort in trying to make their ideas systematic. Yet the religion spreads, nevertheless—and its adherents even *glory* in the inconsistencies. Later, we'll consider one cognitive anthropologist's explanation of why that is.)

What of *Baseball caps are cool*? The second-order beliefs, again, are of the form "X says it"—or perhaps "X does it". But now, "X" can be the boy-next-door, the members of one's gang, a film star, or a football player. This reflective belief has spread rapidly, far and wide. But it's limited to people who respect, and want to emulate, the role models concerned (whose numbers have grown as the epidemic spread) and/or who value *being in fashion* or *appearing young* as a general practice. (Or as an occasional gimmick: the British politician William Hague, when leader of the Conservative Party, was widely mocked when he wore a baseball cap to meet the press photographers.) However, role models can come to be seen as less attractive: maybe they misbehave, or simply become boring. And fashions in clothing change, not least for commercial reasons over which the average person has no control. So this epidemic may fade away.

A more lasting, because officially sanctioned, clothing epidemic is the khaki *sulu* worn by Fijian policemen. *Sulus* are the traditional form of dress for Fijian men. Often patterned and/or brightly coloured, they're more similar to Western women's straight skirts than to the looser (unisex) Polynesian sarong. When the British colonialists imported the role of *policeman* into Fijian culture, they designed the uniform to be 'appropriately' low-key and neat (no patterns, no bright colours, no swirling folds of fabric). But instead of replicating the British bobby's trousers (as the French in Tahiti replicated the uniform of Parisian *gendarmes*), they included aspects of the local dress. This variant was far from "blind". It was a canny choice of mixed memes, intended to foster a higher degree of acceptance of the new cultural role than would otherwise have been the case.

As for *God is everywhere* (Sperber 1990: 90), this is a glaring example of inconsistency between reflective and intuitive beliefs. (How can anyone be everywhere? If He's everywhere, why can't we see Him?) And there are plenty more where that came from. What has cognitive anthropology had to say about them?

b. Religion as a cultural universal

In the late nineteenth century, Tylor (1871) coined the term *animism*, meaning "belief in spiritual beings", declaring this to be "the minimal definition" of religion. Soon afterwards, Frazer (1890) defined religion as "the propitiation or conciliation of powers superior to man which are believed to direct and control the course of nature and of human life". Since then, many alternative definitions have been offered.

Some don't focus on the content of religious beliefs, as Tylor and Frazer did, but on the attitudes of awe and mystery that attend them—as in Émile Durkheim's (1912/1915) contrast between the "sacred" and the "profane". Increasingly, suggested definitions have recognized the *social* aspects of religion, including not only rituals but also political/priestly power relations. And Geertz defined religion in terms of "symbolism":

> [A religion is] (1) A system of symbols which acts to (2) establish powerful, pervasive and long-lasting moods and motivation in men by (3) formulating conceptions of a general order of existence and (4) clothing these conceptions with such an aura of factuality that (5) the moods and motivations seem uniquely realistic. (1963/1966: 31)

One could nit-pick over definitions ad nauseam. For present purposes let's take it, following Pascal Boyer (1994), that:

* A religion is a culturally accepted set of representations (ideas, beliefs, attitudes) and ritual practices which is counter-intuitive *even to its proponents*.
* In other words, these ideas and practices posit unnatural, often supernatural, entities and causal powers.
* The relevant concepts—which differ greatly in detail, from culture to culture—are largely inscrutable, in that their meaning and implications can't be pinned down.
* The entities/powers may be associated with sacred artefacts (icons, idols, vestments, the bread and the wine . . .).
* They're believed to influence the daily lives—and sometimes the afterlives—of individuals in the culture concerned.
* And some of those individuals (shamans, priests, gurus) are thought to have special knowledge of and/or access to the supernatural beings—how to avoid them, communicate with them, or placate them.

Boyer's definition isn't wholly new. Gellner, for example, arguing that psychoanalysis is in effect a religion, said:

> A compelling, charismatic belief system . . . must engender a *tension* in the neophyte or potential convert. It must tease and worry him, and not leave him alone. It must be able to tease and worry him with both its promise *and* its threat, and be able to invoke his inner anxiety as evidence

of its own authenticity. Thou wouldst not seek Me if thou hadst not already found Me in thy heart! . . . *Demonstrable or obvious truths do not distinguish the believer from the infidel, and they do not excite the faithful. Only difficult belief can do that.* (Gellner 1985: 40; final italics added)

(The Catholic Church agrees: hence Gregory XI's discomfiture at Ramón Lull's reasoning machine—see Chapter 2.i.b.) However, the "difficulty" here could be mere implausibility, or counter-intuitiveness. What Boyer adds to most definitions of religion is the aspect of inscrutability, wherein the meaning and implications of religious concepts and claims are not just *hard to accept* but *hard to pin down*.

Thus defined, there's no doubt that religion is universal to cultures (though not to individual human beings). But why? In other words, why do such inscrutable beliefs occur over and over again?

(For current purposes, our focus is on semantic *content*: the concepts and beliefs, or what Boyer calls the religious "representations". Other important aspects of religion, including the rituals and the priestly power relations, will be ignored. Boyer himself did discuss them, but he too focused primarily on the cognitive representations involved.)

Many believers and theologians will answer that religion reflects reality. They may admit that some cultures grasp this truth more fully than others, much as some people see or hear more acutely than others. But *truth* it is. Even if that's so, however, a psychological question arises. Just as we can ask what mechanisms enable human beings to see, or hear, physical reality, so we can ask what mechanisms—provided, perhaps, by the grace of God—enable us to glimpse religious reality.

If religion is *not* a reflection of reality, then the psychological question is sharpened. If religion is illusory, why is it there? Indeed, why is it everywhere? And why is this *universal* phenomenon also hugely *diverse*?

As in medical epidemiology, there are two sub-questions here: *Where do religious ideas come from?* and *How do they spread?* Dawkins had asked both, but offered an answer only to one. He explained the spread of religious memes in terms of people's susceptibility to threats (of hellfire or excommunication), promises (of salvation or immortality), and priestly instructions (to have faith, to go to synagogue or Church, and to send the children to *shul* or Sunday School). But he didn't know where the idea had originated:

Consider the idea of God. We do not know how it arose in the meme pool. *Probably* it originated many times by independent "mutation". In any case, it is very old indeed. (Dawkins 1976: 207; italics added)

That "probably" was a con trick. He had no specific reason, *over and above* its frequency around the globe, to think that religion is likely to arise. If it has arisen many times, independently (which experimental science, for example, has not), then that fact itself requires an explanation.

The irreligious may ask these questions in a spirit of amazement that anyone could ever accept such absurdities. Freud, for instance, had no doubt that religion is an "illusion", essentially comparable to a childhood neurosis (1927). The fact that (in his view) it was a psychosocial necessity didn't prevent its being illusory. And Dawkins became notorious for his militant atheism. His discussions of religious memes—usually drawn from Christianity, but now increasingly from Judaism and Islam also—typically included vitriolic denouncements of their idiocy and evil effects (e.g. Dawkins 1993). When he said that religion is a "virus", he meant that term in the nastiest sense possible.

Bertrand Russell would have cheered him on. For besides his careful philosophical arguments, he too had a nice line in anti-religious polemic (Russell 1957, chs. 1–2, 13–14). The meme enthusiast Dennett was less ready with the polemic, but defined religion as a "maladaptive" meme, because it "discourages the exercise of the sort of critical judgment that might decide that the idea of faith [i.e. belief despite counter-evidence] was, all things considered, a dangerous idea" (1995: 349). Russell would have cheered him on, too. For he'd argued, similarly, that the contentment and resignation encouraged by Christianity prevents people from seeking scientific solutions—the only solutions possible—for the various evils that plague them.

However, one doesn't have to be a Freud, a Russell, or a Dawkins to wonder why people accept religion. Religious followers themselves admit, indeed insist, that their ideas are very strange—hence the awe and mystery surrounding the sacred. And nobody subscribes to every religious idea found across the world, not even in an attenuated, or through-a-glass-darkly, fashion. It follows that even the devout believer needs a naturalistic explanation of why *those people out there* accept *that*.

At least eight explanations have been suggested:

(1) *Perhaps it's because we use our intellect?*

This reply underlies claims that religion is, at least in part, prescientific explanation. Darwin, for example, said:

> [The belief in unseen or spiritual agencies] seems to be universal with the less civilised races. Nor is it difficult to comprehend how it arose. As soon as the important faculties of the imagination, wonder, and curiosity, together with some power of reasoning, had become partially developed, man would naturally crave to understand what was passing around him, and would have vaguely speculated on his own existence. (1871: 143)

Twenty years later, Frazer (1890) announced that "it is the imperious need of tracing the causes of events which has driven man to discover or invent a deity". Indeed, St Thomas Aquinas himself had claimed that his 'Five Ways'—including the Argument from Design and the Cosmological Argument—could prove the existence of a creator God by reason alone (but see subsection d, below). The same aim underlay the "natural theology" of the *Bridgwater Treatises*, or the Gifford Lectures (see Chapters 2.vii.b, 3.i.b, and 7.i.g).

(2) *Could there be some human ability to receive supernatural revelation—perhaps universal, perhaps confined to a priestly elite?*

Whether one answers "Yes" or "No", one must allow that there are many varieties of so-called religious experience (James 1902; G. W. Allport 1951; Bourguignon 1976: Ferrari 2002). These include visions and involuntary actions induced in states of 'possession' (Chapter 7.i.i), and the observation of unique or wonderful events supposed by the religious person to be caused and/or preordained by supernatural agency (see 3.i.b). Many cases of religious experience, including hallucinated voices and delusions of alien control of one's own body, can now be explained neuroscientifically and/or investigated by brain scanning (some references are given in 14.x.c; see also Atran 2002: 174–96).

But those who answer "Yes" will contrast "genuine" miracles, visions, or states of possession with illusory and/or self-deceiving ones. (Consider the role of the Devil's Advocate, who puts the counter-arguments when the Vatican assesses candidates for beatification. Or eavesdrop on the Fang people of Cameroon, when they argue about whether a supposed shaman really does have a hotline to the "ghosts" in the forest—Boyer 1994, ch. 6.)

(3) *Or perhaps religion satisfies an inner need for comfort and guidance?*

An especially complicated—and universalist—psychological story was told about that by Sigmund Freud (1913, 1927, 1930, 1932). It's difficult, today, to swallow it whole. Besides relying on the scientifically shaky Oedipus complex (Chapter 5.ii.a), he developed his own mythology of "the primal horde", to explain the historical origins of religion. But his insight that religion can offer various kinds of psychological security was sound. His account fitted monotheism best, but he did discuss totemism as well. Indeed, his work inspired Bronisław Malinowski's (1884–1942) pioneering studies of the Trobriand Islanders—who, said Malinowski, *didn't* develop an Oedipus complex, because in Melanesian culture the father's role wasn't as authoritarian as in Freud's Vienna (Malinowski 1915–18, 1922).

(4) *Possibly, our mental/spiritual health depend on it in some other way, linked perhaps to self-integration?*

This view was held by Carl Jung (1933), and by many Third Force psychologists (G. W. Allport 1951; Maslow 1962; see Chapter 5.ii.a). And since Freud saw religion as a protection against neurosis, he held a version of it too.

(5) *Perhaps religion aids social cohesion?*

A view favoured by many of those evolutionary psychologists who see religion as an *adaptation*, and by some sociologists and anthropologists. Thus Durkheim:

> [Religion is] a system of ideas with which the individuals represent to themselves the society of which they are members . . . The god of the clan, the totemic principle, [is] nothing else but the clan itself, personified and represented to the imagination under the visible form of the animal or vegetable which serves as totem. (1912/1915: 225–6)

(6) *Maybe there's some special faculty, or instinct, which leads us to religious belief?*

Darwin said not: he insisted that there's no "aboriginal endowment" for belief in "an Omnipotent God" or even "one or more gods" (1871: 143). (Some religious popularizers today, though not the evolutionary psychologists themselves, disagree: they even speak of "the God module".)

(7) *Or perhaps there are some more general mechanisms in human minds, evolved for other purposes but prone to generate religion?*

In other words, religion isn't an adaptation but a (possibly useful) side effect of non-religious adaptations.

Item (1), above, is a special case of this view. Recent cognitive science, however, has added further detail—as we'll soon see.

(8) *Finally, perhaps religion is learnt from the surrounding culture, just as table manners are?*

Despite its chicken-and-egg air, this is the answer that most anthropologists would give (see subsection f, below).

Each of these answers is consistent with at least one other. But three are mutually exclusive—namely (6), (7), and (8). The first two of these are nativist: they ascribe religious belief to inborn psychological mechanisms, either religion-specific or more general. The last is environmentalist—and universalist, too. That is, it respects the third and fourth 'Newtonian' tenets identified in Chapter 5.i. By implication, anything can be learnt—and whatever is learnt, is learnt in the same way.

It's these three answers which have most relevance for cognitive science. Some late twentieth-century cognitive anthropologists criticized their more traditional colleagues for their commitment to (8). They themselves were flying the flag for (7): the generation of the *extraordinary* out of the *ordinary*.

c. Symbolism

The sense in which religions are extraordinary was discussed by Sperber, in the mid-1970s. On his view, *the way in which we interpret* religious concepts is different from the way in which we interpret remarks about tables and chairs. He was the first anthropologist to explain religion in terms of the mind's underlying cognitive *processes*, and to take cognitive psychology seriously in so doing.

Abstract cognitive *structures*, to be sure, had been posited at mid-century by the then fashionable anthropologist Lévi-Strauss. He'd employed a binary semiotic "code", or symbol–meaning mapping, to analyse (for instance) different kinship systems—such as the "avunculate" pattern, in which the maternal uncle holds the family authority over a young boy. Similarly, myths and religions were supposed to be structured around basic semiotic oppositions and themes. Dichotomies such as *men/women*, *raw/cooked*, *village/forest*, *good/evil*, *elder/younger*, and others, were seen as fundamental to all cultures (Lévi-Strauss 1958/1963, 1964/1969).

Lévi-Strauss had been revolutionary in the early 1950s. Instead of focusing on cultural practices and/or artefacts, he claimed to be describing the human mind's classificatory rules, which structured those practices. That's why he was given prominence, thirty years later, in Gardner's Sloan-sponsored history (Section i.c, above). But he'd relied on abstract armchair analysis of ethnographic data, not on discoveries in cognitive psychology.

Sperber (1975), by contrast, argued that anthropologists should consider the processes by which people actually interpret symbols. In a word, this wasn't decoding, but inference. (That insight was later developed, in his co-authored book on *Relevance*: see 7.iii.d.)

As we've seen, Sperber called *God is everywhere* a "reflective" belief, accepted on the say-so of others: especially priests. But it's notoriously tricky. It's not clear just what it means, nor how we can know whether it's true *independently* of the words of the priests. (Nor is it clear how to recognize the priests in the first place, as we'll see.) Even Aquinas admitted that no particular observational belief follows from it, or conflicts with it. The same applies to the ritual sacrament of the Eucharist: the theologians tied themselves into knots over the bread and the wine.

Thomas Hobbes had little patience with them accordingly. He put the Ancient Greeks' mythical "Satyres, Fawnes, Nymphs, and the like" in the same class as his neighbours' "Fayries, Ghosts, and Goblins; and the power of Witches". The Church, he claimed, had encouraged or tolerated these beliefs "on purpose . . . to keep in credit the use of Exorcisme, of Crosses, of holy Water, and other such inventions of Ghostly men" (1651: 7). As for the dogmas pronounced by the theologians,

> [Such statements are among] those we call *Absurd*, *Insignificant*, and *Non-sense*. And therefore if a man should talk to me of a *round Quadrangle*; or *accidents of Bread in Cheese*; or *Immaterial substances*; or of *A free Subject*; *A free-Will*; or any *Free*, but free from being hindered by opposition, I should not say he were in an Errour; but that his words were without meaning; that is to say, Absurd. (1651: 20)

Three centuries later, the logical positivists agreed with him: religious statements are meaningless. As Antony Flew (1950/1955) famously put it, they suffer "death by a thousand qualifications".

If that's so, however, they take a long time a-dying. Most people might grant Hobbes the round quadrangle, but not the other examples. Religious concepts, they feel, do have some meaning. Atheists who reject *God is everywhere* normally say that it's false ("in an Errour"), not meaningless. They're happy to agree with Flew, that the meaning is highly elusive. But meaning *of some sort* there seems to be. How is that possible?

Sperber suggested an answer, by defining meaning in psychological—not logical—terms (Chapter 7.iii.d). In his sense, countless facts are potentially "relevant" to *God is everywhere*. For in the mind of a Christian believer, almost anything can be wheeled in to support, or illustrate, God's ubiquity, goodness . . . and so on (A. J. T. D. Wisdom 1944). Much the same is true, anthropologists tell us, in other cultures. The precise import of informants' remarks about their clan's totem, or the spirits in the forest, is as hard to pin down as anything Aquinas puzzled over.

For Sperber (1975), this was true of "symbolism" in general—religious concepts being a special case. The elusiveness—"inscrutability"—of symbolic meaning, he said, results from an asymmetry in the way it's interpreted. Whereas non-symbolic communication (including science) draws each claim from as many inferences as possible, symbolic language draws as many inferences as possible from each particular claim. It follows that a potentially unmanageable cloud of schema-based inferences—some symbolic, some not—attends every symbolic communication.

Think of how one reads certain types of poetry, for instance, or how one interprets certain types of visual art: ambiguity and unexpected associative richness are considered virtues, here. On Sperber's analysis, the slipperiness of religious statements, and the awe and mystery they arouse, results from this skewed interpretative process.

On Sperber's account, we don't have to be *told* to interpret the poem, picture, or prayer in this sort of way (though cultural training can lead us towards certain interpretive paths rather than others). If and when we find that we can't interpret a communication in the "rational" mode, we *automatically* switch to the "symbolic" mode. So by "rational", Sperber didn't mean consciously deliberated or argued, still less scientific. After all, science isn't/wasn't prominent in some cultures. But people everywhere make statements based on the evidence of their senses, from "The baked beans are on the top shelf" to "The totem pole is broken". Granted, culture-specific concepts inform such everyday perceptions (Chapters 6.ii and 16.iv.e). But Sperber's position was that non-symbolic interpretation kicks in first, as the (evolutionarily grounded) default procedure.

Just why we can't interpret religious statements rationally was considered at greater length by Sperber's follower Boyer. His answer relied on the contrast between the ordinary and the extraordinary.

d. The extraordinary out of the ordinary

Consider Darwin's dog. This creature, "a full-grown and very sensible animal", growled and barked fiercely when an unattended parasol left open on the lawn moved slightly, because of the breeze. That was unusual, said Darwin, since if anyone had been standing near the parasol the dog would have "wholly disregarded" it. In explanation, he attributed a primitive animism to his pet. But this animism was of type (7), not type (6):

> He must, I think, have reasoned to himself, in a rapid and unconscious manner, that movement without any apparent cause indicated the presence of some strange living agent, and no stranger had a right to be present on his territory. (Darwin 1871: 145)

In other words, the dog's ordinary reactions—the ascription of causes, the recognition of animate beings, and the assertion of the territorial imperative—were the root causes of his extraordinary ferocity.

Over a century later, the anthropologist Boyer (1959–) would offer a similar account of human religions. This didn't make him popular with his professional colleagues (Boyer 1994: 11). For most of them, as we've seen, psychology was irrelevant and human "universals" *tabu*.

Cognitive anthropologists, however, were less hostile. One of them, Scott Atran at the University of Michigan, even tried to take Boyer's theory further. He formulated "the Mickey Mouse problem", asking what distinguishes beliefs about the Disney rodent from beliefs about the gods (Atran 1998: 602). In a nutshell, his answer was: passionate personal commitment. So his evolutionary explanation drew on inherited mechanisms underlying motivation and social relations *as well as* the more strictly cognitive mechanisms stressed by Boyer (Atran 2002). Here, however, our focus is on Boyer—for whom Atran was a complement, not a rival.

By the mid-1990s, nativism had reappeared in psychology after its temporary eclipse, and had grown real teeth besides (Chapters 7.vi and 14.ix.c–d). So where Darwin could only hand-wave towards inborn mechanisms, Boyer (then at King's College, Cambridge) could call on developmental psychology, psycholinguistics, and neuroscience to locate them more closely. In addition, he could lean on Sperber's work on the cognitive mechanisms of symbolism and communication.

He'd already discussed culturally authorized "persuasion" by people regarded as truth-sayers (Boyer 1990). Now, he turned especially to religion, using many detailed examples from the African Fang people, with whom he'd lived for several years in the early 1980s (Boyer 1987, 1992, 1993, 1994).

His definition of religion (given in subsection b) was a psychological one. That's unusual: most anthropologists speak, rather, of religious *cultures*, *belief systems*, or *world-views*. Boyer regarded such terms as vague, mystical, and misleading, since they reify high-level abstractions instead of focusing on the underlying reality (1994, e.g. 51, 296). So although he often mentioned "culture", he did so as a convenient shorthand, not as a technical term. (For the record, he's now located in a Department of *Psychology*.)

He was similarly wary of theology. Religious representations, even religious experiences (naturalistically understood), are more real than religious "beliefs". Indeed, most people don't worry about the things that concern theologians. Boyer agreed with Meyer Fortes that even highly elaborate religious beliefs and practices "can be carried on perfectly well without a doctrine or lore of the nature and mode of existence of the 'beings' to whom they are ostensibly directed" (p. 94).

In short, it's a mistake to assume that people care greatly about the consistency of their religious beliefs. But that fact in itself is puzzling. Why do people seek consistency in respect of everyday trivia, but blithely ignore it in the seemingly important area of religion?

Boyer tried to give an answer. With respect to the *universal* aspects of religion, he suggested that the inferences that people link to religious statements aren't wholly unconstrained. (So when I said, above, that almost anything can be wheeled in to support religious claims, that "almost" was important.) The constraints he was talking about weren't further examples of symbolism, not even carefully argued theo-logical symbolism. Rather, they were cognitive schemas found in every human mind. These ordinary mechanisms are what give rise to the extraordinary claims of religion:

> Human evolution has resulted in the development of particular intuitive principles, geared to particular domains or aspects of the natural and social environment. Whatever cultural input can trigger some of these intuitive principles is likely to be more easily acquired and communicated, thereby giving rise to the recurrent features of cultural phenomena. Cultural recurrence (including the recurrence of religious representations) is therefore the outcome of a runaway process in the iterative application of epigenetic constraints. (Boyer 1994: 294)

The "intuitive principles" concerned cover a variety of ontological and relational schemas, in a number of different domains. They constitute our naive physics, naive biology, and naive psychology—several aspects of which had been discovered in pygmy children as well as in Westerners (e.g. Avis and Harris 1991). They've evolved for recognizing and making inferences about inanimate objects; causal connections; plants and animals; natural kinds such as water, or gold (conceptualized as having some hidden "essence"); human agency; and social status.

In making this list, Boyer was drawing not only on ethnoscientists' work on folk taxonomies (Section i.a–b, above), but also on recent developmental psychology (7.vi.i). Susan Carey (1978, 1985), for example, had noted that children overextend the words they're learning (so that "daddy" is applied to all men, and "doggie" to all four-legged animals), and suggested that animism results from this sort of overextension. Indeed, an interdisciplinary conference held at Ann Arbor in 1990 had focused on how universal cognitive mechanisms may give rise to various "cultural" phenomena (Hirschfeld and Gelman 1994).

Theory of Mind (7.vi.f), for instance, underlies anthropomorphic views about gods and spirits, from totems and witches to God our heavenly father. In using ToM (intentional) concepts to describe the supernatural, we help ourselves to the many default inferences associated with them. A father can wish, plan, love, protect, admonish, punish, and reward. And a witch, or a ghost, or a god can (by default) do all these things too.

Ghosts are especially interesting here, as is the awe/dread we often feel in the presence of a dead body—especially if we'd loved the dead person during their life. Regarding dead bodies in general, Theory of Mind naturally (*sic*) induces us to treat them as intentional systems—which, unfortunately, they no longer are. Boyer sees this mismatch as the source of the uncanniness experienced in the presence of the dead. And this in turn gives them their religious significance, and underlies the *interpersonal* dimensions of funeral rituals.

As for the religious significance of dead loved ones, that can be explained in comparable terms. Personal love is rooted in Theory of Mind, though it has weighty cultural aspects too. It's a highly complex set of cognitive, motivational, and emotional dispositions, all related to the purposes and attitudes of the loved one (Chapter 7.i.f). As a crucial computational structure within the lover's mental architecture, it can't simply be switched off at the loved one's death. Hence the inevitability of grief, and of mourning (again, see 7.i.f). (The same considerations imply that a committed believer in a personal God who loses their faith will suffer the tortures of hell in this world, if not in the next.)

And hence, too, the likelihood that some small comfort may be drawn from positing an *invisible* ghost, or soul, with whom some of the previously established personal relations can still be preserved. In other words, this isn't just superficial 'wishful thinking', but an attempt to maintain a central aspect of one's own mental structure.

Similarly, if priests or shamans or religious healers are a special *kind* of person, then they possess some hidden essence which gives them their priestly powers. (For the Fang people, this is an invisible magical organ, called an *evur*—Boyer 1994: 34–5, 45, 86–7.) However, since they look the same as everyone else, doubts can arise about their status—doubts that are *not* necessarily settled by their ancestry or gender, nor by any 'ordination' ritual they may have undergone (Boyer 1994, ch. 6, pp. 237–40, 252–3). It follows that doubts can arise also regarding the reflective beliefs about the supernatural which people are invited to accept on their say-so.

Religions, then, are natural phenomena: their key concepts are 'attractors' in our biologically rooted mental space. (In his later writing, Boyer related them to certain aspects of neuroscience: Boyer 2003.) But they're "un-natural" too (they're very *strange* attractors, a punster might say), for they *deny* some of the assumptions normally activated by these universal mechanisms. So an animal can be of only one species, but a totem may be a bear–snake. Nothing can pass through a solid wall—but Marley's ghost could, and so can the "ghosts" feared by the Fang people (Boyer 1994: 113–14). Every person is limited in power—but God isn't. Every physical event has a cause—but a creator–god doesn't. A block of wood or brass has no intentional states, nor any causal powers beyond gravity—but a graven idol does. And Everyman will die—but God won't; even animistic gods are ageless, or have an "age" that stays constant.

Communications using supernatural concepts are (up to a point) intelligible, contra Hobbes and Flew, because they automatically activate a host of familiar inferences in our minds. That's even true of 'weird' beliefs from other cultures—which is why Bartlett's subjects in Cambridge could make some sense of the alien story 'The War of the Ghosts' (Chapter 5.ii.b).

What makes such communications near-unintelligible, or slippery, is that one or more default assumptions is modified, or dropped. If witches fly on banana leaves (as claimed in the Fang religion), they'll have no difficulty in getting to wherever they want. But does it follow, or doesn't it, that they'd be 'grounded' if a forest fire destroyed the banana trees? If God is everywhere, and omniscient, then—unlike Ann's friend Sally (Chapter 7.vi.f)—He knows everyone's thoughts. But does it follow, or doesn't it, that (given that He's omnipotent too) He's part-responsible for the evil that humans do? And if He's wholly benevolent too, then what about other miseries? Darwin was one of many bothered by this:

I am bewildered . . . There seems to me too much misery in the world. I cannot persuade myself that a beneficent and omnipotent God would have designedly created the *Ichneumonidae* [a type of wasp] with the express intention of their feeding within the living bodies of caterpillars, or that a cat should play with mice. (letter to Asa Gray, 22 May 1860; in Darwin 1887)

And if God is omniscient, just how all-encompassing is that little prefix "omni"? Darwin, again:

[Do] you believe that when a swallow snaps up a gnat that God designed [or even foresaw] that that particular swallow should snap up that particular gnat at that particular instant? (letter to Asa Gray, July 1860; in Darwin 1887)

Such questions are inevitable, for they arise *naturally* from the conceptual/inferential schemas concerned. But they're also largely unanswerable, because those schemas have been fundamentally altered. (This is a psychological version of Immanuel Kant's "antinomies": metaphysical questions that we find ourselves compelled to ask, but which can never find an answer—Kant 1781.)

This, by the way, explains the phenomenon of heresy. If a powerful religious hierarchy has laid down a dogma, certain unorthodox answers may be officially denounced as "heresies". They're the ones which are likely to occur again and again, due to their close links with the intuitive schemas. Unorthodox answers that tempt only very few people aren't worth labelling as heresy.

It also helps explain the power of the priests, and the survival of religions once they've arisen. For the person who's socially sanctioned to interpret important life-relevant concepts which to the flock are unintelligible possesses a significant degree of social power. To that extent, natural religion (attained by method (1) defined above) *threatens* the status of the religious hierarchy. It's not surprising, then, that when Aquinas defined his Five Ways to the creator God, he pointed out that they said nothing about the personal God. For that, he said, we need not reason but revelation (guided, of course, by the Catholic Church). Claims to revelation, in turn, can be buttressed by religious experiences: item (2) on the list. Someone who's especially susceptible to—and/or especially skilled at inducing—florid states of possession, or strange visions, will very likely be revered as a religious authority (Chapter 7.i.i).

At this point, you may be thinking "What about quantum physics?" (thanks to Ron Chrisley for reminding me of this). Quantum physics, too, plays fast and loose with basic everyday concepts: cause, time, location, identity—not to mention Schrödinger's cat. (In that sense, quantum physics is a better candidate for Atran's Mickey Mouse problem than Mickey Mouse himself, or Karl Marx either—cf. Atran 2002: 13–15.)

The physicists themselves find this stuff weird (even "crazy": Chapter 17.i), and many others—myself included—simply can't begin to get their heads around it. If one's unable to cope with the mathematics, one can have recourse only to natural language—which is transgressed over and over again. So is quantum physics a religion?

Cynical remarks about power play and one-upmanship aside, no it isn't. Quantum physics does attract attention by (and even glory in) its inscrutability to common sense. (Sometimes, even non-physicists arrogantly trumpet the anti-commonsensical nature of science in general: L. Wolpert 1992.) But it grounds its counter-intuitive claims in as much empirical evidence as it can, justifying them by scientific experiments and by successful technologies. That is, it doesn't go in for Sperber's "symbolic" communication. Indeed, a key reason for the physicist Sokal's impatience with postmodernist interpretations of his field was that the postmodernists were doing just that (Section ii.b, above).

Moreover (recalling the other criteria in Boyer's definition), quantum physics doesn't buttress cultural rituals outside the laboratory, nor disseminate and validate sacred artefacts (with the possible exception, as I write this in mid-2004, of the *iPod*!). They don't claim the existence of strange forces that can influence specific events in the personal lives of those individuals who accept quantum physics. Similarly, they don't encourage prayer or propitiation as a way of avoiding unfortunate life events. Nor do they feel drawn to die, or kill, for physics. They're as envious as the rest of us of the status of the shamans, the power of the priests. So they may (they often do) 'play God' at cocktail parties, and even at scientific conferences involving non-physicists. But that's as far as it goes.

e. Anything goes?

Cognitive science has told us something about what sorts of bizarre ideas will arise—in James's terms, the production of religion. But are there any ideas *so* bizarre that they won't ever form the basis of a religious cult? To answer that question, we need to consider how religion spreads and how it's maintained.

(Boyer focuses on the cognitive aspects of *how* it's maintained, not the social/motivational reasons *why*. But he can allow, for example, that those cognitive aspects which buttress priestly power will be stressed by the priesthood in their own interests, and highlighted—largely thanks to them—in religious rituals. Similarly, items (3), (4), and (5) on the list of putative explanations above aren't inconsistent with Boyer's account, unless they're offered as the *only* explanation of the origin of religion. As for item (1), we've already seen that this is a special case of item (7)—which is Boyer's explanation.)

Religions spread and survive, according to Boyer, largely thanks to the flouting of the common-sense categories concerned. It's the counter-intuitive claims—about witches flying on banana leaves, or God being everywhere—which make religion so fascinating. (And, as just remarked, quantum physics too.) Like a 'face' with three eyes, they *naturally* attract even more attention than the unmodified schemas do. (They're further examples of behavioural novelty, which we've evolved to find attractive: see iv.b, above.)

That's why religious adherents themselves acknowledge, even revel in, the strangeness of their beliefs. A Fang adult takes it for granted that there are witches, never questioning this for a minute—but he does realize that witches are pretty strange. And the Church father Tertullian, converted by the courage of the Christians thrown to the lions in second-century Carthage, is famous for declaring *Credo quia absurdum est*: "I believe it because it's absurd." If his remark weren't so seductive, it wouldn't have survived for almost 2,000 years. (In fact, it hasn't survived unaltered. There's been a micro-evolutionary change, for his actual words were slightly different. Of the death of the son of God, he said *Credibile est, quia ineptum est*; and of the resurrection, *Certum est, quia impossibile*—Moffatt 1916: 170. However, saying "I believe it" is more personal—that's to say: more inference-arousing, more relevant—and so more memorable than saying "It's believable", or even "It's certain".)

It's no surprise, then, that religion is found everywhere. If it also happens to satisfy our needs for explanation, mental health, and/or social cohesion—items (1) and (3–5), above—its maintenance will be that much more likely. (Some psychological mechanisms making 'full-blown' religious belief possible are sketched in Boden 2002.) The recurrence of religion's universal core *isn't* due to cultural transmission.

But the recurrence of its superficial diversities *is*. Fang witches ride on banana leaves, Christian angels on clouds or wings. The survival of these representations from generation to generation is due to cultural transmission—or in a word, learning. For only by being a member of that culture, or an attentive visitor, can one acquire these concepts. To *that* extent, item (8) on our list, above, is vindicated.

It doesn't follow, however, that cultural learning is wholly unconstrained—a tacit assumption of item (8). Perhaps there are limits, even to religion? Are there some things which can't be believed, or can't even be thought?

Philosophers may approach this question in an 'absolutist' spirit. For instance, consider Colin McGinn's discussion of brain and consciousness (Chapter 14.x.d). He argued that we'll never find mind–body explanations. The problem (he said) isn't that the mind–body relation is *essentially* mysterious, or that minds are divorced from the material world: he assumed that consciousness arises from the brain. The problem is that our cognitive capacities aren't up to understanding it. Analogously, there's nothing mysterious about arithmetic, or the January sales—but dogs and goldfish will forever be oblivious to them. McGinn's claim, then, is that there are some truths that can never be known, or even entertained, by human minds. Some things, eminently worth thinking, simply can't be thought.

Possibly, that's so. For anyone who accepts a realist philosophy of science, this worry is inescapable. (For a non-realist, the problem can't arise.) It would be hubris to assume that we're potentially capable of understanding *every* aspect of the universe. But that's not what's at issue here.

For cognitive anthropologists, the question is different. It's this: are there some things that are thinkable in principle, but *not* capable of being widely accepted and/or reliably transmitted? Or can human cultures, differing greatly as we know they do, maintain just any idea?

Someone might cite multiply nested sentences here (Chapter 7.ii.b). For it's virtually impossible to understand them, especially without paper and pencil. We can easily get our heads around the linear version of *This is the house that Jack built*. But we couldn't cope with its hierarchically embedded equivalent. So a linguistic representation that's possible (grammatical) in principle is unintelligible in practice. However, its semantic content can be captured by a different representation, which can be understood even in the nursery. Indeed, Anglophone culture has already included Jack's house in its mythology—and the malt, the rat, the cat . . . right up to the farmer sowing his corn—by describing these things in the linear way.

A more telling example is Borges's joke taxonomy (Section i.b). In a weak sense, to be sure, he could understand it. He could describe it at his leisure, in written prose. But he couldn't have used it. He couldn't even have remembered it, except parrot-fashion—or perhaps with the help of a specially concocted mnemonic.

Some of the reasons were provided by Rosch, as we've seen. She showed that the structure of concepts, and the processes of conceptualization, simply don't fit the bizarre example imagined by Borges.

Even more to the point is Sperber's work on relevance. He and Wilson (1986) showed how inescapable constraints on information processing both bound and enable our rationality. Certain interpretations of what another person says won't arise, even though in principle they could. That's why, in Chapter 7.iii.d, I described relevance theory as addressing "the frame problem" for high-level thought. What goes for understanding goes for memory too. Something that's difficult to grasp in the first place will be difficult to retain—and to transmit. In other words, the mind's information processing influences not only James's "causes of production" of variation in ideas, but the "causes of maintenance" too.

Notoriously, beliefs can be shared in one culture which in others are literally incredible. Members of that culture can rely on this fact, in making sense (*sic*) of their fellows' communications. For instance:

> [Every] Freemason has access to a number of secret assumptions which include the assumption that all Freemasons have access to these same secret assumptions. In other words, all Freemasons share a cognitive environment which contains the assumption that all Freemasons share this environment. (Sperber and Wilson 1986: 41)

So what's irrelevant in one culture may be highly relevant in another. If a web of conceptual relations has already been learnt, a new communication may be effortlessly interpreted in a way that would never even occur to someone from a different cultural background. That's why one person can hold a belief which another can hardly understand, and certainly not credit: "How on earth can they believe *that*?"

But what of the 'Tertullian factor'? We've seen that the un-natural aspects of religious ideas help make them interesting, and therefore memorable. So perhaps the more bizarre the better?

Boyer thought not:

> [Certain] combinations of intuitive and counterintuitive claims constitute a cognitive optimum, in which a concept is both learnable and nonnatural (Boyer 1994: 121).
>
> Religious ontological assumptions generally comprise a culturally transmitted [i.e. explicit] part, which violates intuitive expectations, and a tacit, schematic part, which confirms them. Assumptions of this type *are likely to be more recurrent if they reach a cognitive optimum*, in which (1) the violation clearly marks off the putative entity or agency from ordinary objects and beings

and (2) the confirmation imposes maximal constraints on the range of inferences that can be drawn from cultural cues. (p. 287; italics added)

In short, the cognitive equilibrium between the familiar and unfamiliar must be maintained. If the balance is thrown too strongly towards the latter, the potential belief won't be accepted—or anyway, won't be widely communicated.

For instance, remember the flying saucers promised in the message from the Guardians on the planet Clarion (Chapter 7.i.c). They'd been expected in the first place only by the cult members. When they failed to arrive, the charismatic leader's second "message"—that the whole town had been saved from the flood at the last minute by the faith of the cult—was credited only by the few members still cooped up with her in her suburban house, offering each other mutual support. And when those few faithful finally tried to spread their "good news" over the media, nobody listened—except to laugh.

At first glance, rescue-by-flying-saucer seems no more bizarre than bodily ascensions or virgin birth, both of which are accepted by millions of people in "the same" culture. To be sure, the latter beliefs have two millennia of tradition behind them. But even that's not accidental. Boyer's analysis helps us understand why the idea of bodily ascension spread widely, and is still maintained by millions, whereas the vision of flying saucers didn't, and isn't. And even flying saucers and other-worldly Guardians are intelligible, having strong roots in our intuitive physics and theory of mind. Had Borges aspired to lead an 'animal' cult, he'd have found no followers: he'd have been on his own, tangled up in his artificial mnemonics. As Boyer put it, "the variability of cultural ideas is not unbounded" (1994: 5).

Most anthropologists, however, seem to assume that it is. The Borges example should give us pause. And Boyer suggested a simpler one: that *everyone* can be a medium, but only *every other day* (1994: 6). There are two things wrong with this. (In fact, they're two sides of the same coin.) On the one hand, a medium is supposed to be a special kind of person, whose essential properties (including access to the supernatural) can't be switched on and off. On the other hand, the natural (*sic*) assumption that "same causes have same effects" can't be—*can't be*, not just *isn't*—given up without strong justification. Conceivably, a medium might be able to commune with the dead only on the anniversary of the deceased's birthday, marriage, or death; or a Christian saint might be able to work miracles only at Christmas or Easter, or only on Sundays. But *every other day* . . . ? That's not merely bizarre. It's almost meaningless, since this imaginary limitation is utterly irrelevant to other 'spiritualist' representations. Even if *per impossibile* a tiny cult were to accept this every-other-day belief, it would never sweep the world.

Once an inferential constraint has been loosened, the way is left open for inferences that would otherwise be blocked. If angels are immaterial agents, they have no weight: so why shouldn't they fly around on golden wings? And once they're doing that, why shouldn't they stop to take a rest on a passing cloud? Clouds or rest, in turn, will trigger other mental schemas—some of which may be highly culture-specific. In short, this is epigenesis: the universal core can develop in many differently detailed ways (Chapters 7.vi.g and 14.ix.c). Hence the enormous cultural diversity that anthropology describes: given the creative imagination of *Homo sapiens*, weird stories about supernatural beings can blossom to our hearts' content.

However, our hearts—or rather, our minds—can't be contented by *just anything*. If too many default assumptions are flouted, the "relevant" inferences become so computationally unconstrained that they're not merely slippery but unmanageable—and unmemorable, too. In other words, they aren't really relevant at all. And since communication depends on the (mostly tacit) recognition of relevance, the problematic representation can't be transmitted either.

f. The impurity of induction

Anthropologists in general are highly suspicious of evolutionary approaches, as we've seen (Section ii.b). Chagnon, one of the two men slandered by *Darkness in El Dorado*, co-edited a large evolutionary volume soon after the sea change in anthropology had got under way (Chagnon and Irons 1979). Clearly, given the AAA's initial support for *Darkness* twenty years later, this did nothing to enhance his reputation in the eyes of the mainstream. And Boyer remarked defensively at several points in his 1994 book that universalist accounts still weren't considered *comme il faut* by most of his peers, because of "the occupational disease of relativism" (p. 111)—their chief weapon in the science wars (Chapter 1.iii.b).

Besides disliking evolutionary accounts (as scientistic), most anthropologists think they don't need them. For they generally assume that *cultural* transmission can work for all representations. This assumption depends on their faith in pure induction, item (8) on the list above. That is: any pattern, any similarity, in the input can be learnt if it's encountered often enough.

Some early cognitive scientists made the same assumption. (Not Warren McCulloch, however: see 4.iii.b and 14.ii.b.) Now, they don't. They believe, with Boyer, that

> The more people pick up available information from the environment, the more they are working on (and constrained by) implicit hypotheses about what is to be picked up. (Boyer 1994, p. x)
>
> The variety of situations that a subject experiences supports indefinitely many possible inferences, of which only a small subset are ever entertained by the subject. Unless we have a good description of the mechanisms that constrain the range of hypotheses, the empiricist account is insufficient. (p. 25)
>
> [There] is no such thing as a simple inductive device, and in fact there cannot be such a device. (p. 95)

One might say they've become more Kantian, for Kant argued that sensory experience must be informed by structuring "intuitions", or "categories" (Chapter 9.ii.c). But that doesn't mean they've all been reading Kant. (More likely, though also not necessarily true, they've been reading Noam Chomsky: 9.vii.c.) Their initial optimism was damped by difficulties, not to say failures, in *implementing* pure induction:

> This is, again, a very familiar point in cognitive modelling; unless one wants to accept indefinitely long searches, one must assume that knowledge structures [mental schemas] provide some information to constrain inferential processes. (Boyer 1994: 59)

In brief, inductive learning requires some initial guidance and/or restriction. Without that, the computational system would be overwhelmed by a host of similarities in the input, most of which are of no significance whatever. That's been shown independently by work in GOFAI, in connectionism, in linguistics, in developmental psychology, in adult cognitive psychology, in the philosophy of science, and in neuroscience.

The system may start out with relatively specific guidance—such as a module for face recognition, a "Language Acquisition Device" (not necessarily Chomskyan), or a scientific hypothesis (Chapters 7.vi.d–e, 9.vii.c–d, and 6.b–d, respectively). And/or initial processing restrictions may prevent it from being swamped by the input, being gradually relaxed thereafter (see 12.viii.c–d). Or it may benefit from a helpful input history (12.viii.e). There may even be different neural learning mechanisms for different domains: the domain-neutral associative bond may be what Randy Gallistel has called "the phlogiston of psychology" (14.ix.g).

But whatever the details turn out to be, the empiricist model of learning adopted by most anthropologists is vacuous:

> [Many] anthropological accounts of recurrence and transmission [have] a strong magical flavor . . . [Saying] that "cultural models are somehow transmitted through socialization" is not very different from saying that "performing the ritual somehow makes the rains fall". Or, to be less polemical, it is very much like saying that "turning the ignition key somehow makes the engine start", which certainly constitutes a reliable principle, but hardly a theory of thermal engines. (Boyer 1994: 265)

The theory of *mental/cultural* engines that we need is "a *rich* psychology", not an artificially simplified one (p. 288).

The banana leaves, and the angels' wings, are representations that can be easily transmitted (learnt) because they're inferentially linked to intuitive schemas about animate agents travelling through space. We don't bother to mention that if a witch/angel wants to speak to someone, they need to move to the place where that person is. Probably, we don't even explicitly think it: wings and banana leaves are much more interesting. In some languages, we might not even be able to make it explicit. But that doesn't mean we wouldn't be tacitly influenced by it: if a culture has no word translatable as *cause*, *belief*, or *intention*, it doesn't follow—contrary to some anthropologists' claims—that they don't possess those concepts tacitly (Lee 1949; Boyer 1991).

It's true that if we're challenged—or if the witch/angel is replaced by God, who we're told is *everywhere*—we're prepared to drop the commitment to location change. That's just one of Flew's death-dealing qualifications. They number "a thousand" because, where religion is concerned, no single inference is safe from dropping. Without any inferential guidance at all, however, we'd be lost. We couldn't acquire the concepts in the first place. (We couldn't even acquire religious *rituals* if they weren't rooted in intuitive intentional schemas, like those which underlay Robert Abelson's account of beliefs and actions: Chapter 7.i.c; cf. Boyer 1994: 202–5.)

Admittedly, highly abstract religions do exist: certain forms of Buddhism, for instance. However, they appeal only to highly literate specialists. The populace understands them in more concrete ways. Benedict Spinoza's (1632–77) austerely non-anthropomorphic vision of God has intoxicated many individuals, myself included. The Spinozahuis in The Hague where he died teems with his admirers, eager to sign the visitors' book as an act of homage. But his closely argued *Ethics* (1677), which rejects every familiar predicate in monotheism (even including "God is *one*"), will never become the basis of a culture-wide religion. It can be the topic of philosophy, or of psychology—but never of anthropology.

In sum, even religion has its limits. It's not the case that—in some culture, somewhere—anything goes. Cole Porter was wrong. But if Cole Porter was wrong, his contemporary Bartlett was right. In his book on memory and social psychology, Bartlett pointed out that anthropologists ignored the mechanisms that make culture possible:

> [Very little has been said about] social conduct in any genuinely observed sense, except by anthropologists *whose interests are apt to stop short at detailed description*. (Bartlett 1932: 239; italics added)

That's no longer true. Or anyway, it's no longer true of the entire profession. Thanks to the cognitive anthropologists, we now have computational accounts of a wide range of cultural phenomena. These are mostly schematic outlines, not nitty-gritty computer models. Nevertheless, the sixth point on the Sloan hexagon has been respected, polished, and sharpened.

9 TRANSFORMING LINGUISTICS

Theoretical linguistics has been enormously important in the history of cognitive science *as a whole*. But it might not have been. After all, why should psychologists, or philosophers, or anthropologists, or neurophysiologists, or computer scientists be interested in linguistics? If they happen to be concentrating on language, then perhaps it's relevant. If not, why should they bother with it?

As late as 1950, most of them didn't. And, in fact, most of them don't today. But in the late 1950s to 1970s all cognitive scientists had to pay some attention to it. This chapter explains why that was. (Why the interest then dwindled to near-zero is explained in Chapter 7.iii.a, and in Section ix.g below.)

The cyberneticists gave no thought to linguistics—and very little to language. The nearest they got was Warren McCulloch's 1920s search for a "logic" of verbs (4.iii.c), a pastime foreign to his cybernetic fellows. Even Gregory Bateson and Gordon Pask, who both championed language-based psychotherapy, weren't interested in the structure of language. When Alan Turing and others in the UK wrote the first computer programs involving language, they ignored linguistics (see x.a, below). And the (few) psychologists who focused on language paid scant attention to linguistic theory, and—crucially—didn't see their work as relevant for the study of mind *in general*. Nor was linguistics needed to spark off the cognitive revolution: that originated in psychology and AI, not linguistics (4.iii.e, 5.iii–iv, and 6.i–iii).

It turned out, however, that theoretical linguistics—specifically, the study of *syntax*—stoked the fire sparked off by other disciplines. (The contemporary work on phonetics was less relevant in this regard, and I shan't discuss it.) By the early 1960s no one involved in cognitive science, or in the philosophy of mind, could ignore it. The reason, in just two words: Noam Chomsky (1928–).

Linguistics turned computational with Chomsky. He tried to formulate a mathematical definition of language *as such*: a "computational" account, in the abstract sense later stressed by David Marr (7.iii.b). His work, including a pioneering paper on computer languages, encouraged others to persevere with, or to embark on, the computer modelling of language. But he himself didn't join them. He was highly sceptical about what's normally called computational linguistics, or natural language processing (NLP). He dipped his toes into the water only once (G. A. Miller and Chomsky 1963: 464–82)—by which time many others were already swimming in the shallows (see Section ix).

Chomsky's early publications—up to the mid-1960s—had a huge, and lasting, effect on cognitive science overall. This needn't have happened, even if his *linguistics* was, as one disciple later said, "*the* existence proof for the possibility of a cognitive science" (Fodor 1981*b*: 258). But Chomsky generalized his philosophical and methodological claims. He even defined linguistics as "part of psychology", focused on "one specific cognitive domain and one faculty of mind" (Chomsky 1980*b*: 1). Granted, he cared little about the psycholinguistic processing details—especially when they turned out to be less consilient with his theory than had at first appeared (Chapter 7.ii.a). But he saw his linguistics, in effect, as a theory of *mind*.

Specifically, he revived the then hugely unfashionable doctrine of *nativism*. This is the view that the mind/brain of the newborn baby is already equipped with knowledge of, or dispositions towards, language—and various other things, too. Modern proponents of nativism, such as his ex-pupil Steven Pinker (1954–), cite many biological data that were unknown to Chomsky (Pinker 1994, 2002). But he remains their key inspiration. Indeed, a very recent book by a leading anti-nativist has acknowledged that his "arguments [for linguistic nativism] are still widely read, *and carry as much weight today as any newly-produced publications*" (Sampson 2005: 7; italics added). In brief, he's still a major force.

Chomsky's influence on cognitive science was beneficial in many ways. In particular, he deepened the nascent questioning of behaviourism, and encouraged "mentalist" theories couched in terms of internal rules and/or representations (Chapter 6.i.e). He revivified research on nativism (7.vi), and indirectly encouraged the growth of evolutionary psychology (8.ii.d–e and iv–v). He offered a vision of theoretical rigour which inspired linguists and non-linguists alike. And, despite his own scepticism, his work encouraged others to attempt the computer modelling of mind. In short, for the field as a whole, he was a Good Thing.

Nevertheless, this chapter starts, unusually, with a health warning: *beware of the passions that swirl under any discussion of Chomsky* (Section i).

Next, Sections ii–v locate Chomsky in relation to the three centuries of language scholarship—including linguistically inspired philosophies of mind—that preceded him. The ancient writings are discussed in some detail here because he himself made a point of comparing his own work to them in a provocative way. I begin by distinguishing predecessors from precursors (in Section ii), and then consider the various writers named by Chomsky as his "rationalist" precursors (see Sections iii–iv). Section v sketches the state of theoretical linguistics during Chomsky's youth.

In Section vi, I describe how he first sought to change it. Section vii deals with his nativism, and the associated attack on behaviourism, while Section viii says a little about the subsequent changes in Chomskian theory. Various fundamental critiques of his account of language are outlined in Section ix, which also shows the need for the health warning.

Finally, the last two sections describe the early days, and the eventual maturation, of work in the computer modelling of language. These matters could have been discussed in Chapter 10, for NLP was central to GOFAI research. I decided to place them here instead, largely because the problems experienced in NLP recalled theoretical questions highlighted by the scholars featured in Sections iv–v, below.

A word on demarcation: this chapter deals with linguistics rather than psycholinguistics. The focus is on the nature of language *as such*. Psychological questions about how we understand, communicate, and remember linguistic meanings do crop up here, of course. But they're addressed more directly in many other parts of the book (e.g. Chapters 7.i.c, ii, iii.d, and iv.d–e; 8.i.a–b and vi; 10.iii.a and e; 12.x; and 16.i.c and iv.c–d). Indeed, Chomsky's influence led many linguists to ignore such questions: see Section ix.g, below.

9.i. Chomsky as Guru

Many people, including youngsters, take Chomsky as their political guru. Indeed, it's mostly because of his courageous and uncompromising political writings—which far outnumber his publications on linguistics—that he's the most-quoted living author (Barsky 1997: 3). I'll say a little about his politics in Section vii.a, below. But it's his role as a *scientific* guru that's relevant here.

I said, above, that Chomsky was a Good Thing for the development of cognitive science. So he was. But to acknowledge this isn't to accept the tenfold Chomsky myth.

a. The tenfold Chomsky myth

Those who uncritically take Chomsky as their scientific guru share ten beliefs:

* They think (1) that besides giving a *vision* of mathematical rigour, he always achieved it in his own work.
* They believe (2) that his linguistic theory was, or is now, indisputably correct in all essentials.
* They hold (3) that the (nativist) psychological implications he drew from it were convincingly argued at the time, and
* (4) that they are now empirically beyond doubt.
* On matters of history, they suggest (5) that his work of the 1950s was wholly original.
* And they suppose (6) that his writings of the 1960s were—as he himself claimed—the culmination of an august tradition of rationalism dating back 300 years.
* Some also assume (7) that without Chomsky's grammar there would have been no computer modelling of language.
* Many believe (8) that Chomsky was responsible for the demise of behaviourism.
* Most take for granted (9) that he reawakened and strengthened the discipline of linguistics.
* And many assume (10) that linguistics is as prominent in cognitive science today as it was, thanks to Chomsky, in the late 1950s to 1970s.

Each of these beliefs, strictly interpreted, is false. But all, suitably qualified, approach the truth. (The least defensible are nos. 9 and 10: in some ways, as we'll see, he *set back* the field of linguistics, and *undermined* its relevance for cognitive science in general.)

Moreover, many people in Chomsky's heyday accepted all of them (except no. 10, of course)—and a significant number still do, with no. 10 now added. That's why

Chomsky is not only a pivotal thinker in theoretical linguistics, but also a hugely important figure in the wider story of cognitive science.

However, "important" means *influential*, *provocative*, *fruitful* . . . perhaps even *largely beneficial*. It doesn't necessarily mean *right*.

b. A non-pacific ocean

The tenfold myth attracts passions of surprising depth. Surprising, that is, to anyone who believes the Legend: that science is a disinterested enterprise (see 1.iii.b).

While doing the research for this chapter, I was astonished to discover the lack of mutual respect between the two camps in linguistics, Chomskyan and non-Chomskyan. I'd known there were unpleasant tensions of course, but I hadn't realized their degree. They range from unscholarly ignorance of the very existence of important competing theories (as I discovered in a recent conversation with a young MIT linguist) to vitriolic hostility and public abuse.

As one illustration, a leading American linguist who (unusually) has an open mind on the issue of nativism has recently bemoaned "what is, I am afraid, an often disturbingly unserious, indeed irresponsible approach to the innateness position on the part of its major advocate" (Postal 2005, p. viii). Paul Postal (1936–) then quotes some "revealing", "outrageous", and "grotesquely untrue" remarks by Chomsky, the burden of which is that all the arguments against innateness are such confused nonsense that nobody ever bothers to answer them. And his verdict is that "These statements [by Chomsky] are not scientific comments but rather the analog of awful, partisan political discourse" (2005, p. ix).

As another illustration, a non-Chomskyan complained to me about Chomsky's theorizing about syntax:

> Anything that any human being actually says is at once defined by Chomsky out of the discipline, leaving him and his followers free to pursue their strange little version of navel-gazing, free from any contamination from the real world. *And I can't forgive Chomsky for perverting my discipline in this sick manner*. (Larry Trask, personal communication)

Larry Trask told me, a few months before his tragically early death in 2004, that he was happy for his remark to be quoted, since he'd said much the same in print. (And I'm confident that he'd have been happy with the italics, which are mine.)

Third, the computational linguist (and AI–vision researcher) Shimon Edelman (1957–) has recently referred to "the Bolshevik manner of [Chomsky's] takeover of linguistics", and the "Trotskyist ('permanent revolution') flavor of the subsequent development of [his] doctrine" (S. Edelman 2003). To be fair, he allows that there were cultural reasons for the "takeover" in early 1960s America (Koerner 1989). These were the widespread counter-cultural rejection of the views of the older generation (see 1.iii.c), and the post-Sputnik increase of research funding—especially at MIT (see 11.i). Thanks to that money, many new departments of linguistics were set up in the USA at that time, and it was "inevitable" that they'd be staffed by young lecturers trained at MIT (James McCawley, quoted in Koerner 1994). So Edelman isn't suggesting that Chomsky was an all-powerful tyrant, solely responsible for what happened. By the same token, the Chomskyan "revolution" wasn't caused by purely *intellectual* factors (S. Edelman 2003: 675).

And last, an anonymous linguist who reviewed this chapter accused me of pursuing "a personal vendetta" against Chomsky instead of "doing intellectual history". He, or possibly she, couldn't have been more wrong. I'm not a professional linguist, so have no axe to grind either way. (And like Postal, I have an open mind about linguistic nativism: see Chapter 7.vi.) Moreover, it's precisely because I'd done the intellectual history carefully that I'd realized just how questionable, and in certain respects just how damaging, some of Chomsky's claims were.

It's a telling sign that my account—which isn't written in a polemical spirit (though it does quote polemics, from both sides), and which gives chapter and verse for every criticism—should have been misinterpreted in that way. So be warned: these are turbulent waters. When it comes to Chomsky, few linguists, if any, are emotionally neutral.

(The same reviewer wanted me to change the chapter because "it will upset people". Well, I've no doubt that it will upset some people—though it will probably hearten others. But I haven't changed it: it's an honest account of what I found in the literature, and of what I was told in conversations with several leading linguists.)

Far from having mellowed with time, the passions have risen over the years. In Sections viii and ix, we'll see why this is so. But before then, we'll consider Chomsky's early work (up to the mid-1960s), its immediate reception, and—first of all—its intellectual background.

9.ii. Predecessors and Precursors

Let's start with some banalities:

A predecessor is a person who discussed the same topic as some later scholar.

A precursor is a predecessor who said similar things about it.

A precursor needn't have influenced the scholar concerned: intellectual anticipation isn't the same as intellectual ancestry.

Identifying precursors requires one to decide just what one means by "similar".

Similarity isn't merely a question of degree, but of nature. Two œuvres may have more or less of a certain feature, and/or they may have partly different features.

Finally, someone who interprets "similarity" too broadly not only obscures important differences, but casts all the scholars concerned in intellectual roles for which they aren't best suited. So reflected glory may be more distorting than illuminating.

a. Why Chomsky's 'history' matters

The points just listed, which relate to the caveats given in Chapter 1.iii.f, are pertinent in any discussion of the history of ideas. But they're especially relevant here. They concern not only Chomsky's intellectual pedigree, but also his rhetorical efforts to share in the glory associated with great names of the past.

Those efforts weren't mere self-seeking. Rather, they were a form of philosophical defensiveness. Chomsky had deliberately suppressed his (nativist) ideas on language-and-mind when writing his first book (1957), judging them to be "too audacious" for publication (see Section vi.e). When he did finally announce them, he defended them partly by locating them in the context of respected thinkers of the past (1965, 1966). In so doing, however, he misrepresented those thinkers in various ways.

"So what?" you may ask. "Why bother to rap Chomsky over his historical knuckles? He wasn't a historian, after all." Well, no. But historical accuracy is important here for two reasons.

He himself, in defending nativism, gave his Cartesian–rationalist labelling a high profile (and many Chomsky fans uncritically took him at his word). Today, his long-time champion Jerry Fodor (1998*c*) speaks of "the New Rationalists", of whom he counts himself one (7.vi.d). We need to know just how literally this tag should be taken.

Even more to the point, we must try to grasp the shifting subtleties of the long-standing debate about "innate ideas" if we're to understand nativism *in general*. In this context, we need to know what the philosophers he referred to actually said—which often differed, more or less, from what he implied that they said.

On the one hand, Chomsky himself repeatedly named as precursors people—dating back to the early seventeenth century—whose views differed significantly from his own, and implied that they were more similar to each other than is the case. (In so far as they are indeed his precursors, they're anticipatory rather than ancestral: he studied most of them only after developing his own ideas.)

On the other hand, the fact that linguists today situate themselves—in agreement or opposition—with respect to Chomsky might suggest that his work was a complete break with what went before. In other words, it suggests that his teachers were mere predecessors, not precursors.

In discussing these matters, we must distinguish his views on language-and-mind from his theory of language (primarily, of syntax) as such. The "one hand" (above) concerns the former. The "other hand" concerns the latter.

With regard to his grammatical theory, one might say that Chomsky had no precursors. More accurately, the sense in which others before him had "said similar things" was an attenuated one. He himself claimed that the transformational model of language was "not at all new", having been anticipated by the Port-Royal logicians of the seventeenth century (Chomsky 1964: 15–16). But that was true only in the most general sense. Even those of his immediate predecessors who developed formal theories of syntax did so in much less detailed ways. Chomsky's grammatical work, although clearly dependent on contemporary advances in formal linguistics and information theory, was largely novel (see Sections v–vi).

As for his views on the relation between language and mind, Chomsky was the first to say that he had many precursors (Chomsky 1964: 8–25). But most, he says, were long dead and largely forgotten:

> These early modern contributions were scarcely known, even to scholarship, until they were rediscovered during the second cognitive revolution, after somewhat similar ideas had been independently developed. (Chomsky 1996*a*: 7)

That remark apparently applies to Wilhelm von Humboldt (1767–1835), whose work one historian sees as having suffered "a pattern of recurrent, or successive, amnesia" in American linguistics (Hymes and Faught 1981: 98). And it applies also to the Port-Royal *Grammar* (see Section iii.c), which another historian of linguistics has described as "forgotten, [and] often misunderstood" by the end of the nineteenth century (Koerner and Asher 1995: 174). But it doesn't apply so clearly to the Port-Royal *Logic*, which was being translated into English for the fourth time just as Chomsky's first book appeared. And I know from my own experience that Herbert of Cherbury (on innate ideas) was routinely recommended to Cambridge philosophy undergraduates in the 1950s. Perhaps professional linguists weren't reading these people, but others certainly were.

These examples suggest that some of Chomsky's remarks about his precursors may need to be taken with a pinch of salt. Perhaps a sackful: a third specialist historian has gone so far as to say that Chomsky's version of the history of linguistics is "fundamentally false from beginning to end" (Aarsleff 1970: 583).

Even if that judgement is too harsh, there are systematic problems with regard to Chomsky's account. I noted, above, that talk of precursors depends on what one counts as similar. Chomsky interpreted the similarities so broadly that his descriptions of his forerunners were often misleading. In particular, his frequent uses of "rationalist" and "Cartesian" were inaccurate. And it's not at all clear that what his forebears had meant by the "inner form" of language, or by "creativity", was what he meant by these words.

The central doctrine of "Cartesian linguistics", said Chomsky, is that "the general features of grammatical structure are common to all languages and reflect certain fundamental properties of the human mind" (Chomsky 1966: 59). In other words, "Cartesian" linguistics holds that there is some inborn grammatical core of all natural languages.

Chomsky did attach a caveat to his inclusive term ("Cartesian"), pointing out that the rationalist/nativist writers he had in mind fell into more than one philosophical school. Nevertheless, he implied that they comprised a coherent tradition of humanist rationalism, one that had been forgotten but had now been revived by his own research (Chomsky 1966, 1968).

If we're to situate that research in its historical context, we need to understand how his nominated precursors differed—from him and from each other—as well as how they were alike. In this section, we'll consider their general philosophical background; specific examples are discussed in Sections iii and iv.

b. The rationalist background

In broad outline, the writers Chomsky named as his "rationalist" precursors agreed on two things. They believed that language is the origin of human thought, and that it's innate in every child.

Some—rationalists in the strict sense—interpreted "thought", here, to mean access to necessary truth. Some, but not all, remarked that the second (nativist) belief implies the existence not only of some specifically human propensity for language, but also of some common core of language as such. Some believed in, or hoped for, a past (or future) universal language, intelligible to everyone because it was (or would be) wholly or largely natural, not purely conventional. Some saw language as the essential core of humanity. And some stressed the role of language in creativity and culture.

As we'll see in Section vii.d, Chomsky wasn't a rationalist in the strong sense. He didn't posit innate access to necessary linguistic principles, still less to necessary truth. Nor was he interested in projects aimed at designing a universal language, or at locating the origins of language in prehistory. But he did believe that language is what makes us human, that language and thought are intimately connected, that language is essentially creative, that all languages share a common grammatical core, and that linguistic ability is innate in (only) the human species.

In Chomsky's own judgement, his most important precursor was René Descartes. As we saw in Chapter 2.iii.c, Descartes taught that language is uniquely human. We don't share it with any animal, nor could it be provided to a machine:

> If you teach a magpie to say good-day to its mistress, when it sees her approach, this can only be by making the utterance of the word the expression of one of its passions. For instance, it will be the expression of the hope of eating, if it has always been given a tidbit when it says the word. Similarly, all the things which dogs, horses and monkeys are taught to perform are only the expression of their fear, their hope or their joy [all of which are entirely mechanical: see Chapter 2.ii.d] . . . But the use of words, so defined, is peculiar to human beings. (Descartes 1646: 207)

No machine, according to Descartes, could "arrange words variously in response to the meaning of what is said in its presence, as even the dullest men can do". The human ability to arrange words variously was to be heavily stressed by Chomsky, who both sought to explain it and used it as a stick to beat behaviourism. However, what Chomsky meant by this ability wasn't what Descartes meant by it (see Section iv.f).

As for where language ability comes from, Descartes believed that it's provided to all human minds by God. His reason was that, because of its variability, it can't be explained in material terms. He pointed out that deaf-mutes—fellow human beings—spontaneously use gestures and make up signs that other people can learn to understand (1637, ch. 5).

It's not at all clear, however, that Descartes was a "Cartesian" according to Chomsky's definition (above). He didn't specifically say that language has a core structure, of which we have innate knowledge. Indeed, he described languages as unpredictably diverse, to be learnt from the environment much as other social customs are. And he attributed the variability of language to the innate power of reason (the ability to come up with new and appropriate thoughts), not to any property of language as such. He did believe, however, that some human abilities rest on innate knowledge.

c. The puzzle of innate ideas

In general, for Descartes, our knowledge depends heavily on innate ideas. These exist in the mind prior to any experience.

The idea of God must be innate, he said, since the concept of infinity could be neither derived nor—as Thomas Hobbes had argued (see 2.iii.b)—extrapolated from experience. The true proposition "God exists" is also innate (said Descartes), because it is self-contradictory to suppose its falsity. Similarly, he saw the necessary principles of logic and mathematics as present in the mind a priori.

For Descartes, no true *propositions* about the empirical world are innate: only experience can teach us that roses are red and violets blue. Here, he was less nativist than Chomsky, for whom knowledge of certain empirical facts about language—facts which might have been different—is inborn (see Section vii.c–d). But Descartes sometimes argued that the *sensory concepts* themselves must be innate, since no genuine causal relation between brain and mind is possible:

> [Besides the ideas of movements and figures] so much the more must the ideas of pain, colour, sound and the like be innate, that our mind may on occasion [*sic*] of certain corporeal movements, envisage these ideas, for they have no likenesses to the corporeal movements. (trans. Haldane and Ross 1911: i. 42)

Descartes wasn't the first to posit innate ideas, whether concepts or propositions (McRae 1972; Chomsky 1966: 59–72). They'd been spoken of for centuries. Plato's doctrine of *anamnesis*, introduced in *The Meno* to explain the slave boy's intuitive grasp of geometry, is a well-known example. Most rationalist writers followed Plato in attributing necessity to innate ideas. Descartes's occasional suggestion that even sensations are innate was unusual.

But familiarity didn't bring clarity. There was much controversy about just what this doctrine amounted to, in Descartes's writings as in other people's. This was so, irrespective of whether it concerned only concepts or also propositions.

Did it mean that we have *actual* ideas—of number, extension, God, or even colour—prior to any experience? Or did it mean that our *potential* to do arithmetic and geometry, to think about God, or to perceive red and blue, is a natural capacity, or disposition, possessed from birth by all human minds—as opposed to a learnt skill? And if the latter, *just what* is a "potential", "capacity", or "disposition" to have certain ideas? Are these expressions anything more than philosophically misleading ways of saying that we can, in fact, do so?

Many of Descartes's correspondents, including the arch-empiricist Hobbes, raised these questions. Comparing innate mental dispositions to a family's inborn propensity for a certain disease, Descartes replied:

> I never wrote or concluded that the mind required innate ideas which were in some sort different from its faculty of thinking; but when I observed the existence in me of certain thoughts which proceeded, not from extraneous objects nor from the determination of my will, but solely from the faculty of thinking which is within me, then . . . I termed them INNATE. (trans. Haldane and Ross 1911: i. 442)

But his reply didn't end the debate. On the contrary, it raged on long after he died.

John Locke (1690) interpreted the doctrine literally, and rejected it. Or rather: "he formulated every clear version he could think of and showed each to be either trivially true or obviously false" (Goodman 1969: 138). He likened the newborn mind to a blank wax tablet, or tabula rasa (which, trivially, has certain potentialities rather than others). Locke's view was very widely accepted at the time—not least, for its 'enlightened' political implications. Indeed, the entry on "Idea" in Ephraim Chambers's *Cyclopaedia* of 1728 stated that "our great Mr. Locke seems to have put this Matter out of dispute".

But Chambers was over-optimistic. Gottfried Leibniz (1690), specifically targeting Locke's discussion, interpreted the doctrine dispositionally, and defended it. He compared the mind to a veined block of marble, which both enables and constrains the figures that can be sculpted from it.

Empiricists accepted Locke's interpretation. They saw knowledge in general as built from passively received sensations, and language as the learnt ability to use certain words to stand for certain ideas.

In justification, they pointed out that languages are very different, and that children learn to speak as the people surrounding them do. They remarked that feral children, much discussed in the seventeenth and eighteenth centuries (Itard 1801), don't use language spontaneously, but will learn it if rescued while still young (for a modern discussion, see Curtiss 1977). They argued that dispositional accounts of innate ideas were merely pretentious ways of stating a commonplace: that people can learn language and mathematics. And they complained that positing innate ideas prevents one from asking more searching questions about the origins of knowledge. These empiricist arguments were still prominent in the twentieth century, and were marshalled by some of Chomsky's critics (see Section vii.c–d).

Those of a rationalist cast of mind favoured Leibniz's interpretation. But it was unclear just what cognitive dispositions might be involved and/or just how they contribute to knowledge. Rationalists claimed, for instance, that necessary truths are innate, but they couldn't explain why geometry and arithmetic seem to provide genuinely new knowledge.

That changed after the late eighteenth century, thanks to Immanuel Kant's *Critique of Pure Reason* (1781). The problem of innate ideas—at least as regards our knowledge of the physical world—seemed, to many, to have been solved by Kant's distinction (sketched in Chapter 2.vi.a) between empirical intuitions and categories on the one hand and sensory experience on the other. The former are (he said) innate, but are merely structuring principles. Only the latter can give actual content to our knowledge. In Kant's words: "Thoughts without content are empty, intuitions without concepts are blind" (Kant 1781: 61).

It was more difficult, however, to gloss language as innate in this sense. For there seem to be no universal structuring principles. Whereas we all acknowledge causality and Euclidean space, our languages appear chaotic. Not only do we use different words for the same thing (*children*, *enfants*, *tamariki* . . .), a fact linguists call "the arbitrariness of the sign", but we put the words together in very different ways. In short, languages differ not only in vocabulary, but also in grammar.

This seemingly incontrovertible truth, coupled with the growth of empiricism outlined in Chapter 2, led—in scientific circles—to the demise of the rationalist view of language. By the end of the 1940s, when Chomsky was a student, the predominant school of scientific linguistics favoured environmentalist accounts of language learning (see Section v.b).

That situation was partly due to the dominance of behaviourism in American psychology. But even anti-behaviourists fought shy of nativist theories of language. Thus the Gestalt psychologists, who developed neo-Kantian explanations of the perception of causation and identity, didn't do so for language. And Jean Piaget, who recognized universal features of language such as temporal order and hierarchy, saw them as based in the universals of logic. These in turn, he said, are constructed by the infant's sensori-motor activity in the physical world (Piaget 1952*b*; Piatelli-Palmarini 1980). In short, he saw language as neither unique nor innate.

In humanist circles, by contrast, the nativist viewpoint on language and mind survived. In the three centuries separating Descartes's birth from Chomsky's, it was elaborated in various ways. Literally Cartesian versions of innate ideas gave way to neo-Kantian and Romantic ones. And, in place of Descartes's mix of pure mentalism and mechanistic biology, there arose a holistic view of mind as grounded in self-organization, similar to the autonomous development of the embryo (see 2.vi). The diversity of languages wasn't denied: quite the contrary (see Section iv.b). But the basic humanist commitment to species-specific linguistic nativism remained.

9.iii. Not-Really-Cartesian Linguists

When is a door not a door? And when is a Cartesian not a Cartesian? The first question is more swiftly answered than the second.

The people named by Chomsky as Cartesians (and undeniably influenced by Descartes) differed between themselves about matters of interest to us—including some of the questions listed in Chapter 1.i.a. Many of those questions are still (largely) unanswered. So, quite apart from evaluating Chomsky's accuracy as a historian, there's good reason to consider just what they did say and just what they didn't say.

a. Descartes's disciple

Géraud de Cordemoy (*c*.1620–84), sometime Director of the Académie française, is the only person named as a precursor by Chomsky (e.g. 1996*a*: 3) who was an orthodox Cartesian. Born four years after Descartes, he was one of his leading disciples. Indeed, he was a member of the party who travelled to Stockholm in 1666 to bring Descartes's exhumed remains back to France—minus the writing forefinger, which the French ambassador was allowed to cut off, and the skull, which had been stolen. (After being sold several times, the skull ended up in the Musée de l'homme—Lindeboom 1979: 13–14.)

Descartes had died of pneumonia sixteen years earlier, having risen daily at four o'clock in the Swedish winter to teach Queen Christina (aka Greta Garbo) philosophy in her freezing castle. Cordemoy outlived his master by over thirty years. For much of that time, he was spreading, and elaborating on, Descartes's views.

In promulgating the Cartesian philosophy, Cordemoy devoted an entire essay to language, the *Discours physique sur la parole* (Cordemoy 1668: 196–256). As the title implies, he had much to say about the physical aspects of language (the lungs, speech organs, ears, and nerves), pointing out that the human bodily machine is naturally suited for producing and hearing it. He even discussed the bodily aspects of writing. Nevertheless, he argued that language is possible only for beings with souls (minds), which is to say, human beings. It can't be explained in purely physical terms, nor mimicked by artificial machines—and it isn't acquired by the learning methods used by animals.

Cordemoy marvelled at the child's acquisition of language, and remarked that it doesn't seem to depend on reinforcement:

> It's true that one usually tries to excite some emotion (such as joy) in infants, by exclaiming when one shows them something while speaking its name, so that they are more attentive and, being more affected by this method of teaching, retain the words better.
>
> But, no matter what trouble one takes to teach them certain things, one often finds that they know the names of thousands of other things, which one has not attempted to show them. And the most surprising thing about this is that, by the time they are two or three years old, and by the power of their attention alone, they are able to pick out the name of something from all of the constructions that one uses to talk about it.
>
> Next, and with the same concentration and discernment, they learn the words that signify the properties of the things whose names they know.
>
> Eventually, extending their knowledge further, they notice certain actions or movements of these things; and simultaneously observing people talking about them, they distinguish—by their ability to pay attention to the movements, and to hear people repeat certain words in combination with the names of the things or of their properties—those words which signify actions. (Cordemoy 1668: 213–14; my trans.)

Adverbs, he said, are acquired much later, because modifications of actions are less important to the infant than the actions themselves. And grammarians teaching adults a foreign language concentrate first on nouns, then adjectives, and then verbs—"in imitation of the precepts given by nature to children" (p. 215).

Words are partly physical, in so far as they are sounds produced by our vocal apparatus. In that, they resemble the cries of animals. But animal 'signs' are communications without meaning. Their communicative effect results from the harmony between beast and beast, and between animal and environment, established by God as a system of interlocking reflexes. Words, by contrast, have meaning as well as sound, for (as Descartes had taught) they are the voluntary expression of conscious thoughts guided by reason. This is evident from the unpredictability and aptness of the words produced by human beings.

Moreover, said Cordemoy, souls are universally the same. Differences in linguistic ability, and in intelligence in general, are due to imperfections in the brain, not the mind. Freed of the body, all human souls would be equal, because equally rational. (To deal with the philosophical mind–body problem lurking here, Cordemoy developed a neuropsychological version of occasionalism: Chapter 2.iii.b.)

Whether Cordemoy was a "Cartesian" as well as a Cartesian is questionable. (We've already seen that Descartes himself wasn't.) The quotations given above might be read as implying that some specifically grammatical capacity leads the infant to concentrate first on nouns, then on adjectives, and finally on verbs. And perhaps Cordemoy did believe that. However, his remarks about the modifications of actions being less important to the child than the actions themselves suggest that it is the child's general interests, not its specifically grammatical preparedness, that guide the order of language acquisition.

In sum, Cordemoy may not have believed that grammar as such is foreshadowed in the infant's mind. But he certainly believed that humans have a natural capacity for language, which has no parallel in either animals or automata.

b. Arnauld and the abbey

The belief in some universal basis of language was promoted also by Antoine Arnauld. He was a highly valued correspondent of Descartes (2.ii.g)—and of Leibniz, too (Arnauld 1662, p. xxxv).

Although he was influenced by Descartes's philosophy, Arnauld wasn't a Cartesian but a (highly controversial) Jansenist. He was known primarily for holding unorthodox views on grace and the Eucharist. More to the point, here, he rejected Descartes's claim that we have innate knowledge of God. He didn't think that God's existence can be proved by the light of reason, but taught that Christian theism is—literally—our best bet (Arnauld 1662: 357). That is, he accepted the famous "wager" argument of his friend Blaise Pascal—who spent his last years at Arnauld's home base, the Abbey of Port-Royal.

Port-Royal was a Cistercian abbey for women, allowed by papal decree to accept male seculars in retreat. Arnauld was associated with it from early childhood: one sister was appointed Abbess in 1602, and four more—as well as his widowed mother—were nuns. In maturity, he was a key member of the influential 'Port-Royal logicians', and the main author of their most famous publications: the *Grammaire générale et raisonnée* and its successor treatise on reasoning, *La Logique; ou, L'Art de penser* (Lancelot and Arnauld 1660; Arnauld 1662). These texts openly adopted some of Descartes's ideas. But, as many of their pietistic examples showed, they weren't promoting pure Cartesianism. In particular:

> Port-Royal, thanks to the excellent philosophical tools employed by Arnauld, in general grammar developed a branch of Cartesianism which Descartes himself hadn't emphasized: namely, the study and analysis of language in general, assumed to be founded in reason alone. This Cartesian branch, planted and nurtured at Port-Royal, to some extent advanced the ideas familiar in the seventeenth century, and anticipated the labours of the eighteenth century, in which it would be taken up directly by [among others] du Marsais [and] Condillac . . . (Sainte-Beuve (1867), quoted in Cornelius 1965: 121; my trans.).

The scholastic logic of the medieval period had leant strongly on grammar when it was first developed. Indeed, medieval treatises contain many statements broadly consonant with Arnauld's (and Chomsky's) views. Roger Bacon (1214–94), for example, taught that "Grammar is substantially the same in all languages, even though it may vary accidentally" (cited in Lyons 1991: 172).

But most medieval logicians—as opposed to grammarians—weren't much interested in the grammar of real sentences. Rather, they used natural language to construct a semi-formal language with its own grammar, in order to demonstrate certain formal inference patterns.

By the opening of the seventeenth century, then, the links between logic and language were primarily with rhetoric (I. Thomas 1967). The Port-Royalists changed that.

c. The Port-Royal *Grammar*

In their fresh approach to logic, Arnauld and his colleagues revived its relation to grammar—which characterizes human speech, they said, but not the mimicking of word sounds by parrots (Lancelot and Arnauld 1660: 21). They aimed to state a "rational grammar". By this they meant one that is universally shared, and based on necessary principles of reason rather than contingent (and diverse) facts of linguistic usage.

The Port-Royal group distinguished a sentence's underlying propositional structure from its final expression. In this, they were following the medieval and Renaissance logicians. They claimed that words have a "natural order", in the sense that they provide the natural expression of our thoughts. Or rather, words considered as parts of speech (Subject, Verb, Object, Copula) have a natural order, although words as constituents of phrases or idioms may not. The natural logic underlying the various parts of speech may differ from the common-sense view: all verbs, for instance, were analysed as the Copula and an attribute (so that *Peter lives* became *Peter is alive*). Language and mind (rationality) were thus seen as intimately connected.

Moreover, just as reason is universal, so are some grammatical features. These weren't formal syntactic rules, as they were for Chomsky. Rather, they were features which—because of their natural relation to thought—aid the function of language: communication. (Significantly, the two subtitles of the *Grammar* advertised 'The Fundamental Principles of the Art of Speaking' *before* 'The Reasons of the General Agreement, and the Particular Differences of Languages'.)

For instance (the group argued), all languages that distinguish singular and plural must *naturally* ensure that nouns and adjectives, and verbs and nouns/pronouns, agree in their number (Lancelot and Arnauld 1660: 148–9). For to do otherwise would be to confuse the hearer, who hopes to discover the speaker's thought.

The Port-Royalists were well aware that languages don't always behave 'naturally'. English, for instance, distinguishes singular and plural in nouns, but not in adjectives: both *man* and *men* can be *learned*. Greek is puzzling, too, since plural neuter nouns take singular verbs. Arnauld dealt with such counter-examples by saying that a "figure of discourse" or idiom (which may lend a superficial elegance to the language) will have "some word [implicitly] understood", and that the hearer copes "by considering the thoughts more than the words themselves" (Lancelot and Arnauld 1660: 149). The clear implication was that some natural languages might be more natural (closer to rational thought) than others. No prizes need be offered for guessing which one was favoured by the denizens of Port-Royal:

> [There] is scarce any language, which uses these [superficially unnatural] figures less than the French: because it particularly delights in perspicuity, and in expressing things as much as possible, in the most natural and least intricate order; tho' at the same time it yields to none in elegance and beauty. (p. 154)

Arnauld distinguished between agreement, which is universal (except in a figure of discourse), and government, which is "almost entirely arbitrary" (Lancelot and Arnauld 1660: 148). By government, he meant the fact that using one word can cause some alteration in the form of another. An example discussed at some length is the phenomenon of case: genitive, dative, accusative, and so on. The case, which is a logical relation in thought, may or may not be apparent in the form of the noun (compare the Latin *terra*/*terram* with the English *ground*/*ground*). There may or may not be a special particle for a certain case (such as the French *de*). And a verb may or may not always take the same case (consider *servir quelqu'un* and *servir à quelque chose*). Moreover, since "cases and prepositions were invented for the same use", many prepositions govern the noun they are used with—thus *ad terram*, but *ab terra* (chs. 6 and 11, and pp. 149–52).

The various languages differ greatly in whether, and if so how, they mark this or that case by changes in the form of words and/or by the insertion of extra words. Occasionally, different governments alter the sense of the governed word. So in Latin (Arnauld pointed out), *cavere alicui* and *cavere aliquem* mean *to watch over a person's safety* and *to beware of him*, respectively. Such meaning shifts are grounded not in logic, but in arbitrary linguistic convention (p. 152). (As these examples show, the Port-Royalists did consider several European languages. But their main focus was on French, and some of their universalist claims were inconsistent even with English or Greek.)

With respect to those grammatical constructions where they believed that logic is involved, the Port-Royalists gave many detailed examples of the natural order they had in mind. In discussing relative clauses, for instance, Arnauld related specific grammatical forms to their "subordinate propositions" in thought (Arnauld 1662: 118–21). He distinguished "explicative" and "restrictive" uses of words like *which* and *who*, and argued that a particular grammatical conversion (ellipsis) can produce the underlying thought in one case but not the other.

For example, the subordinate proposition expressed by the words "men, who were created to know and love God" can be attained by substituting the antecedent for the pronoun itself, thus: "men were created to know and to love God". The reason is that the relative clause here is (for Arnauld) explicative: according to his theology, the idea of "man" includes the idea of a being created to know and love God. By contrast, the restrictive clause in the sentence "men who are pious are charitable" doesn't express the subordinate proposition (arrived at by a similar pronominal substitution) that "man as man is pious". Rather, it states that men may be pious ("the idea of piety is not incompatible with the idea of man"), and that the idea "charitable" can be affirmed of the complex idea "pious man".

Chomsky described the Port-Royalists' work as an anticipation of his own contrast between "deep" and "surface" structure. Further, he endorsed some of their specific hypotheses about how these are correlated (Chomsky 1966: 33–51, 97).

He claimed, for instance, that they employed a type of phrase-structure grammar (see Section vi.c). They held that the deep structure, or thought, consists of one or more elementary (subject–predicate) propositions, whereas the surface structure may be syntactically very complex: recursively nested, for example. Moreover, expressing the thought as an actual sentence may require more than just expressing each elementary proposition in words. It may also involve rearranging, replacing, or deleting parts of the first-stage (unspoken) sentences—which is to say, transforming them (Chomsky 1966: 40).

Broad similarities certainly exist. But these concern aspects of Chomsky's thought that weren't especially original or controversial. Moreover, some of Chomsky's comparisons are questionable in detail.

For instance, he says that Arnauld's treatment of relative clauses anticipated various "modern" logical–semantic insights, including the distinction between meaning and reference "in pretty much their contemporary sense" (Chomsky 1996*a*: 7).

It's true that the explicative–restrictive contrast rests on a distinction between the meaning of the term concerned (in this case, "man") and those objects that are picked out by it (actual men—who may or may not be pious). However, some distinction between meaning and reference had already been made by medieval logicians. Moreover, the Port-Royal contrast between the "comprehension" and "extension" of a general term wasn't equivalent to any distinction made in modern logic. It muddled singular and general terms, and also gave a definition of meaning—according to which it's part of the meaning of "triangle" that the internal angles equal two right angles—which is now regarded as over-inclusive (Kneale and Kneale 1962: 318 ff.).

Indeed, modern logicians see Arnauld's definition of meaning as an example of psychologism (2.ix.b). They therefore criticize the Port-Royal group as "the source of a bad fashion of confusing logic with epistemology" (Kneale and Kneale 1962: 316). That's why, by the 1950s, the Port-Royalists were only a minority taste in philosophical logic.

Nonetheless, the Port-Royal *Logic* was a creative advance at the time, and dominated logic for the next 200 years. And their *Grammar* was regularly reprinted until 1846. Consequently, their view that grammar reflects thought was widespread throughout this period, and still respected in the late nineteenth century.

d. Deaf-mutes and Diderot

One of the thinkers who accepted this Port-Royalist view was Denis Diderot (1713–84). He was editor-in-chief of the *Encyclopédie*, the 'bible' of the French Enlightenment—a project that had started out as a scheme to translate Chambers's two-volume *Cyclopaedia* (see Chapter 2.ii.c), but which ended up as twenty-eight newly written volumes (Porter 2001: 8). And he, too, was someone named by Chomsky as a precursor.

Descartes, as we've seen, had referred briefly to deaf-mutes, in making a point about the universality of reason and communication. But Diderot wrote a 17,000 word essay about them, exploring the relations between gesture, language, and thought (Diderot 1751). Part of his aim was to discuss the origins of language, and to ask whether there is some "natural" universal language, intelligible to all human beings.

Both questions were prominent topics of debate in the eighteenth century. They featured in several novelists' tales of imaginary voyages—some of which described "universal" artificial languages and symbol systems (Cornelius 1965).

Some people argued that the spontaneous signing of deaf-mutes represented the universal gestural language from which conventional languages had originally developed, and which might now be deliberately extended to form a worldwide communication system (Knowlson 1975; Large 1985). This 'handmade' system would be naturally intelligible to all, because it would be logically transparent (much as the Russell–Whitehead logical notation was later intended to be: 4.iii.c). Others had suggested new sign languages and/or alphabets, designed for universal use (Large 1985: 19–63).

The most interesting of these projects was that of John Wilkins (1614–72), Bishop of Chester and a founder member—and first Secretary—of the Royal Society. More interested in phonetics than in grammar, he proposed a universal alphabet, systematically designed to encompass "all such kinds of simple sound, which can be framed by the mouths of men" (J. Wilkins 1668: 357–62).

His reference to "the mouths of men" was no mere rhetorical flourish. For his notation was comprised of iconic symbols depicting various positions of the speech organs: root/tip of tongue, one/two lips, top/root of teeth, back/front of palate, parts of mouth, and nose. It also marked whether or not the sound involved breathing. In addition, Wilkins designed a more easily usable alphabet. This had some highly stylized iconic features, and some coherence in using similar symbols to represent similar sounds.

The supposed intelligibility of this language wasn't grounded only in the universal ability to make speech sounds and mouth movements. For the systematic coherence spread into semantics, too. Wilkins gave every letter a significance, based on the forty classes into which, according to him, *everything* could be fitted. Each class was subdivided into differences, and these into species. To each class, he gave a two-letter syllable, and a consonant and a vowel were assigned to each difference and species, respectively. So flame was called *deba*: the *de* meant 'element', the *b* meant 'fire' (the first of the four elements), and the *a* meant a small part of the element: namely, a 'flame'. In short, semantic hierarchies were explicitly reflected in phonetics, and in spelling too.

Another aim of Diderot's essay, regarded by some as "one of the outstanding examples of literary criticism in the eighteenth century" (A. M. Wilson 1972: 123), was to distinguish various rhetorical genres. So he compared prose, poetry, drama, scientific writing, and so on.

Here, Descartes was lurking in the background. His clear prose, which reflected his views on the best "method" of thinking, had caused a revolution in scientific and philosophical writing. This is evident, for example, in the striking stylistic differences between the three versions of Joseph Glanville's *Vanity of Dogmatizing* (Glanville 1661–76). These arose because, between the first and second drafts, Glanville adopted the Royal Society's recommendation of Descartes's prose over the more florid writing of the early Renaissance. (One aspect of this was the rejection of interpretative accounts of Nature, considered as God's text, in favour of empirical ones: see Chapter 2.iii.b.)

Diderot went further, arguing that authors should choose not only the most appropriate genre for the type of thought they wished to express, but also the most fitting language. He had in mind not just choice of vocabulary (*le mot juste*), but selection among natural languages themselves. And here, Chomsky has remarked (1965: 7), he illustrated the lasting influence of Port-Royal. For he declared, as Arnauld had done before him, that French surpasses all other languages in the degree to which it matches the order of our thoughts. Everyone, in so far as they are rational, must think their thoughts in the same order. Because French matches this rational order best, he argued, it is the natural language for philosophy and science. Greek, Latin, Italian, and English are more appropriate for less reasoned enterprises, such as the theatre—and in general for persuasion, emotionalism, and deceit.

Only his French readers, no doubt, found that particular claim compelling. But Diderot's view that grammar has some principled basis in thought was widely shared.

9.iv. Humboldt's Humanism

Ten years after the final publication of the *Encyclopédie*, the Enlightenment criticisms of traditional institutions and beliefs were followed by Kant's radical critique of human knowledge itself. Accordingly, some late eighteenth-century philosophers, described as "Cartesians" by Chomsky, conceptualized language in neo-Kantian terms.

That is, they posited—however vaguely—some linguistic equivalent of the empirical intuitions and categories (2.vi.a). This was supposed to be a formative principle shared by all humankind, which moulds all conceivable languages much as the Kantian intuitions inform our ideas about physical reality.

Typically, this 'grammar' wasn't thought of as a static set of given principles—like the Port-Royal logic, or even Kant's space and time. Rather, it was a developing, self-organizing, inner form. As such, it was broadly comparable to the organic forms then being posited in *Naturphilosophie* and Johann von Goethe's biological morphology (see 2.vi.e).

One of these neo-Kantian writers was Johann Herder (1744–1803), who in a prizewinning essay on the origins of language argued that language wasn't given to us in its full form by God, but isn't wholly invented either (Herder 1772). It's part of our innermost nature, and very different from the narrowly focused and unvarying instincts of animals. Rather, it's the ground of mankind's freedom and self-development. Indeed, Herder compared the genesis of language to the mature embryo pressing to be born. He also described a nation's mother tongue as an aspect of the national "soul", or "spirit".

All these notions were to be taken up by the Romantics in general, and by Humboldt in particular. Humboldt, in turn, would have a strong influence on nineteenth-century Romanticism. (In the early 1800s, he spent four years in Paris at a time when William Wordsworth, for instance, was living there too.)

a. Language as humanity

Herder's essay was read, about ten years later, by the young Humboldt—in his teens at the time. Eventually, Humboldt became a renowned humanist scholar (and sometime Prussian ambassador to England). But he wasn't just a fine scholar: he added some enormously influential ideas of his own.

Humboldt's philosophy of language, like Kant's epistemology, has been described as a Copernican revolution (Hansen-Love 1972). For it presents the human mind as the creative origin of knowledge and culture, instead of being dependent on them for its ideas. Accordingly, his position (following the *verum factum* tradition of Giambattista Vico: see 1.i.b) promised a deep knowledge of language and culture—much deeper than scientific knowledge of material things could ever be.

Humboldt saw human beings as *set apart* from nature (including animals) by the mental faculty of language, through which the human mind or spirit is both developed and expressed. Like Herder, he thought of language in terms of organic development, describing it as a "living seed" and "an internally connected organism" (Humboldt 1836: 61, 21). He was thinking not only of the development of language in infancy but, even more, of the historical development of language from its earliest roots:

> *Language*, regarded in its real nature, is an enduring thing, and at every moment a *transitory* one . . . In itself it is no product (*Ergon*), but an activity (*Energeia*). Its true definition can therefore only be a genetic one. For it is the ever-repeated *mental labour* of making the *articulated* sound capable of expressing *thought* . . . To describe languages as a *work of the spirit* is a perfectly correct

and adequate terminology, if only because the existence of spirit as such can be thought of only in and as activity. (Humboldt 1836: 49).

Humanity as such was continually celebrated by Humboldt. He often did so in spiritual terms, as when he said that "thought at its most human is a yearning from darkness into light, from confinement into the infinite" (1836: 55). This causes a recurring difficulty when one tries to relate his work to modern linguistics and cognitive science (and when one translates his term *Geist* as "mind" rather than "spirit").

For instance, he pointed out, as Cordemoy had done before him, that human anatomy seems to be especially apt for the production of speech (a fact to be explored in fascinating detail over 100 years later: Lenneberg 1967). But he *didn't* explain these anatomical phenomena as due to the benevolent design of God. Instead, he saw them as aspects of the activity of the creative human spirit. He even linked bipedalism to—or explained it in terms of?—our language and humanity:

And suited, finally, to vocalization is the upright posture of man, denied to animals; man is thereby summoned, as it were, to his feet. For speech does not aim at hollow extinction in the ground, but demands to pour freely from the lips towards the person addressed, to be accompanied by facial expression and demeanour and by gestures of the hand, and thereby to surround itself at once with everything that proclaims man human. (1836: 56)

In short, although Humboldt's observations were often highly astute, many of his scientific questions were fundamentally different from those posed in the empiricist tradition. His work therefore sets one of the "traps" mentioned in Chapter 1.iii.a. To interpret Humboldt's questions and/or answers in terms of the explanatory categories of modern science—which Chomsky frequently did—is often to distort them.

It would be misleading, for instance, to praise Humboldt's naturalistic (i.e. non-theological) explanation of bipedalism as being about as close as a pre-Darwinian scientist could have got. For Humboldt was not only pre-Darwin: he was also pro-Goethe. In other words, his conception of "science" was very different from the empiricist's (see 2.vi).

Like Goethe (a close friend, and a neighbour), Humboldt favoured organic holism over analytic science:

The comprehension of *words* is a thing entirely different from the understanding of *unarticulated sounds* . . . [The word] is perceived as articulated, [which perception] presents the word directly through its form as part of an infinite whole, a language. (1836: 57–8)

It is thus self-evident that in the concept of linguistic form no detail may ever be accepted as an *isolated fact*, but only insofar as a method of language-making can be discovered therein. (p. 52)

Connected discourse . . . is all the more proof that language proper lies in the act of its real production. It alone must in general be thought of as the true and primary, in all investigations which are to penetrate into the living essentiality of language. The break-up into words and rules is only a dead makeshift of scientific analysis. (p. 49)

To be sure, language involves "a system of rules" (p. 62). But that system is an organic whole, which "grows, in the course of millennia, into an independent force", restricting how we can express (and think) our thoughts. Its generative principle is humanity itself ("the unity of human nature"):

> Language belongs to me, because I bring it forth as I do; and since the ground of this lies at once in the speaking and having-spoken of every generation of men, so far as speech-communication may have prevailed unbroken among them, it is language itself which restrains me when I speak. But that in it which limits and determines me has arrived there from a human nature intimately allied to my own, and its alien element is therefore alien only for my transitory individual nature, not for my original and true one. (p. 63)

The similarity between this passage and Chomsky's nativism is evident. But the differences are arguably even greater (see subsections e–g, below).

b. Languages and cultures

Drawing partly on Diderot's *Encyclopédie*, and in particular on the three volumes of its summary (the *Encyclopédie méthodique*) that were devoted to grammar and literature, Humboldt studied all the European languages, ancient and modern. But he didn't stop there.

He also described several native American languages, relying on novel data brought back by his younger brother Alexander—a noted scientist much admired by Charles Babbage (Hyman 1982: 72–4), who spent five years exploring the Americas. In addition, he was familiar with many oriental examples, including Chinese, Sanskrit, and Hebrew. And, thanks to the interest in non-Indo-European languages that was aroused by European colonialism in the eighteenth century, he also had knowledge of Melanesian and Polynesian—and the various dialects of Kawi, the ancient literary and sacred language of Java.

Kawi was especially interesting to him largely because it seemed to be a mixed language: although its vocabulary was Sanskrit, its grammar was Malayan. Moreover, its geographical position suggested (to him) that it might be linked to virtually all the known languages of the world. Given Humboldt's passion for intellectual synthesis, and his view that "language" is the fundamental unifying force within all humankind, Kawi seemed to offer the hope of integrating "old world" and "new world" languages—and cultures (Humboldt 1836, p. xii).

His three volumes on Kawi, prepared in the closing years of his life, included a 350-page introduction that gives the clearest statement of his views on *language in general* (Humboldt 1836). As well as considering syntax, morphology, and (especially) phonetics, Humboldt studied the various literatures and orally transmitted poetry and prose. When he thought of language, then, he had in mind both linguistics and literature.

These wide-ranging studies weren't mere scholarly stamp collecting: Humboldt was no Dr Casaubon. They were driven by Humboldt's novel ambition to distinguish language groups in terms of their grammatical (and phonetic) structure, rather than their historical origins. They were intended to show, too, that all languages are essentially alike:

> My aim is [not to gather the external details of individual languages, but] a study that treats the faculty of speech in its inward aspect, as a human faculty, and which uses its effects, languages, only as sources of knowledge and examples in developing the argument. I wish to show that what makes any particular language what it is, is its grammatical structure and to explain how the

grammatical structure in all its diversities still can only follow certain methods that will be listed one by one . . . (Humboldt 1836, p. xiv)

His studies also underpinned Humboldt's distinctive position on the relation between language and culture, and on the individuality of cultures.

If thoughts can be expressed only in language, difference in language implies difference in culture. Humboldt argued that each natural language is unique. *Perfect* translation is impossible, since apparently equivalent words will have different associations in the minds of speakers of the two languages (and even in individual speakers of "one" language). Nevertheless, *good* translation—on which he had many interesting things to say (Novak 1972)—can expand the language and mentality of the reader.

Although he held that everything can in principle be expressed in every language, he pointed out that in practice languages (cultures) differ in what they choose to express. Translations of literary masterpieces can thus awaken latent potential in the receiving nations. (Scientific thought, he said, varies less across languages.)

Each culture, for Humboldt, has its own *Weltansicht*, its unique manner of regarding the world. Human individuals are deeply informed by their culture, as well as by their unique life experience. And their cultural identity must be thought of holistically. For a language, like an organism, is a self-organizing whole, not a collection of independently definable parts.

c. Humboldt lives!

Humboldt's stress on the diversity, and the holism, of cultures informed some early twentieth-century work on topics relevant to cognitive science. So, too, did his views on the near-identity of language and thought.

The holistic view of language was to be taken up, for example, by the linguists Ferdinand de Saussure (1857–1913) and Roman Jakobson (1896–1982). Although his birth-date locates Saussure firmly in the nineteenth century, his general intellectual presence dates from well into the twentieth. For his influence blossomed only after his death, with the posthumous publication of his lecture notes on "general" linguistics (Saussure 1916).

After a lifetime of detailed research in historical philology, Saussure had finally considered language as such. (It was he who coined the phrase "the arbitrariness of the sign"—see ii.c, above.) He described language as a holistic system, in which the meaning of one word, or sign, can be defined only in relation to others. To include a word, such as *cat*, in a language is not to provide an inert pointer referring to some pre-existing class, but to identify some class by distinguishing it from others picked out by different words (*kitten*, *dog* . . .).

Jakobson was a neo-Humboldtian whom Chomsky has acknowledged as a major influence in the origination (not just the *post hoc* justification) of his own thinking. Based at Harvard while Chomsky was a Junior Fellow there, Jakobson concentrated on phonology, not meaning, and combined holism with nativism (compare Humboldt's "inner form", discussed below). He claimed—as it turned out, wrongly (Sampson 1974)—that the seemingly chaotic speech sounds heard around the world can be defined in terms of twelve oppositional "distinctive features" (Jakobson 1942/3; Jakobson *et al.*

1952). These, he said, comprise a hierarchical system of phonological capacities shared by all human beings. Each language selects only some of the possibilities available.

As for linguistic diversity, Humboldt's ideas were echoed by the anthropological linguist Franz Boas (1858–1942), who set in hand the systematic description of the fast-disappearing native American tongues (Boas 1911). (It was he who'd translated the story used in Frederic Bartlett's experiments on memory: see Chapter 5.ii.b.)

Whereas previous accounts of these languages had focused largely on vocabulary, Boas focused on grammar. He argued that each natural language has a distinct grammatical structure. Moreover, he said, the structural differences can't always be expressed by using shared grammatical categories, as when one says that adjectives are usually put after the noun in French but before it in English. Rather, they may need grammatical categories specifically designed to describe the particular syntax concerned.

Edward Sapir (1884–1939) and Benjamin Whorf (1897–1941) went further, claiming that virtually none of the concepts of one language can be understood (thought) by people brought up to speak another. Whorf even held that each language conceals a specific metaphysics, or ontology, unintelligible to people with different mother tongues (Carroll 1956). On this view, there is no 'true' ontology: languages differ arbitrarily from each other, and views of reality differ likewise (cf. 1.iii.b).

This claim—that a person's thought can't escape their language—was hotly disputed through the 1960s and beyond (Hoijer 1954; R. Brown 1958; Hymes 1964: 149–53; Rosch 1977; Lucy 1992). Empiricist philosophers complained that the Sapir–Whorf 'hypothesis' is slippery and vague, and if clarified becomes either obviously true or obviously false (M. Black 1959). But many social scientists, doubting the obviousness, garnered evidence to test it.

Some of these people, influenced by Chomsky's nativism (see Section vii.c–d), claimed that apparently arbitrary differences in colour vocabulary are in fact underlain by universally shared colour categories (O. B. Berlin and Kay 1969; Rosch/Heider 1972; Rosch 1973; see Chapter 8.iia–b). This early work had glaring methodological faults (Sampson 1980: 95–102). But Chomskyans still reject the Sapir–Whorf hypothesis, saying "there is no scientific evidence" for it and attributing it to "a collective suspension of disbelief" (Pinker 1994: 58).

Others followed Sapir and Whorf, suggesting that cognitive differences may be associated with, or even grounded in, syntactic variations as well as vocabulary. For instance, the Navaho language uses a different form of the verbs for handling things (such as *throw*), depending on the shape of the object handled. This grammatical distinction isn't found in Indo-European languages, and suggests that speakers of Navaho may have a relatively keen awareness of shape (Carroll and Casagrande 1966: 26–31; Lucy 1992: 198–208). Only "suggests": independent tests need to be done to confirm any specific instance of the Whorfian hypothesis.

d. A fivefold list

There's a puzzle here, however.

To concentrate on the syntactic differences between languages seems decidedly un-Chomskyan, given that Chomsky posited a universal grammar. Indeed, Humboldt's aim of describing the grammatical structure of as many languages as possible was defined as the major goal of linguistics by the very people against whom Chomsky reacted in the 1950s (see Section v.b, below). And Chomskyans, as remarked above, reject neo-Humboldtian views on the boundless diversity of languages and concepts.

Yet Humboldt was repeatedly named by Chomsky as an important precursor, who had essentially similar aims. In his first act of homage to the "Cartesian" linguists, Chomsky devoted more space to Humboldt than to anyone else (Chomsky 1964: 17–25; 1966: 19–28).—Why is this?

The answer is fivefold:

* First, Humboldt insisted on the universality of language in the human species.
* Second, he saw it as an innate mental faculty, or force, distinct from other psychological abilities and from the instincts of animals.
* Third, he saw language as inexplicable in terms of its ultimate origins (although he explained many features of specific languages by reference to earlier ones).
* Fourth, he contrasted the finite number of objects in the physical world, including the fixed number of speech sounds, with the infinite creativity of language, which can always come up with some new thought. Language, therefore, implies freedom.
* And fifth, he saw language as having an organic "inner form" that unites humanity, irrespective of culture, in a fundamental way.

Stated briefly, as I've just done, the similarities between Humboldt and Chomsky seem to be clear—indeed, overwhelming. In truth, however, they're neither.

We needn't waste time cavilling over the first two items. These (nativist) beliefs are evident in Humboldt's reply to the familiar empiricist argument that children will learn quite different languages, depending on who brings them up:

> Man is everywhere one with man, and development of the ability to use language can therefore go on with the aid of every given individual [i.e. caretaker]. It occurs no less, on that account, from within one's own self; only because it always needs an outer stimulus as well, must it prove analogous to what it actually experiences, and can do so in virtue of the congruence of all human tongues. (Humboldt 1836: 59)

He also argued (like Chomsky) that the infant's linguistic capacity must be shared with everyone else—otherwise, how could they learn so quickly (pp. 58–9)?

So far, so similar. But the last three items need fuller discussion.

e. Origins

The third item is included in the list because Chomsky claimed that there could be no evolutionary explanation of language—or at least, that we could never find one. Language, he argued, is a holistic system with an all-pervasive structure, not a collection of independently acquired habits or tricks. Humboldt had said much the same thing. But Humboldt, born almost 100 years before *On the Origin of Species*, couldn't consider the possibility of a Darwinist evolutionary account. Chomsky could, and did.

Chomsky was no less pessimistic than Humboldt about our ever understanding the origin of language as such:

> There seems to be no substance to the view that human language is simply a more complex instance of something to be found elsewhere in the animal world. This poses a problem for the

biologist, since, if true, it is an example of true "emergence"—the appearance of a qualitatively different phenomenon at a specific stage of complexity of organization. (Chomsky 1986: 62)

In fact, the processes by which the human mind achieved its present stage of complexity and its particular form of innate organization are a total mystery .... It is perfectly safe to attribute this development to "natural selection", so long as we realize that there is no substance to this assertion, that it amounts to nothing more than a belief that there is some naturalistic explanation for these phenomena. (p. 83)

Admittedly, "evolution" was listed as one of only five keywords for Chomsky's official précis of one of his books (1980*b*: 1). However, this was due not to the author but to the editor, on the grounds that talk of universal grammar predictably raises questions about origins (A. N. Chomsky, S. Harnad, personal communications).

Chomsky's pessimistic remarks of the 1980s, cited above, implied that "emergence" is inexplicable. They implied, also, that the naturalistic explanation of language would refer to some single genetic mutation, or possibly some small set of mutations, that occurred early in our species' history, whose effect on the "emergent" phenotype is opaque. If that were so, then we couldn't hope to find it/them, nor to understand its/their significance if we did.

Similar anti-evolutionary scepticism has often been associated with the complex structure of the eye. Darwin himself admitted that to explain the eye in terms of natural selection "seems, I freely confess, absurd in the highest degree" (quoted in Cronly-Dillon 1991: 15). Nevertheless, he rebutted this scepticism in general terms, and evolutionary biology can now do so in detail. It turns out that the eye is neither a miracle nor even a singularity: eyes have evolved independently a number of times. (Or perhaps not: evidence that all types of eye have evolved from the same root is given in Gehring and Ikeo 1999.) Possibly, then, we shall one day understand the evolution of language much as we now understand the evolution of the eye.

Some Chomskyans, indeed, have speculated at length about the evolution of "the language instinct" (Pinker 1994). As for Chomsky himself, he now allows that we're beginning to understand how a qualitatively different phenomenon can arise spontaneously from a simpler base (see Chapters 14.x.a–b and 15). By the new millennium, he had even suggested that language might be based in spontaneous physical self-organization:

There is little doubt that the human language faculty is a core element of specific human nature ....

Recent work suggests more far-reaching possibilities. The minimal conditions on usability of language are that languages provide the means to express the thoughts we have with the available sensorimotor apparatus. *One far-reaching possibility* is that, in non-trivial respects, the language faculty approaches an optimal solution to these minimum design specifications (with "optimality" characterised in natural computational terms). If true, that would suggest interesting directions for the study of neural realization, and perhaps for the further investigation of the critical role of physical law and mathematical properties of complex systems in constraining the "channel" within which natural selection proceeds.

What the work on optimal design of language seems to suggest is that language *might* be more like the appearance of familiar mathematical structures in nature such as shells of viruses or snowflakes than like prey/predator becoming faster to escape/catch one another [see Chapter 15, below]. When the brain reached a certain state, some small change *might* have led to a reorganisation of structure that included a (reasonably well-designed) language faculty. *Maybe.* (Chomsky 1999: 31; italics added)

However, if natural selection must somehow underlie language, as it underlies every other biological phenomenon, that's not to say that Chomsky expects a detailed evolutionary explanation. On being charged (on the evidence of the passage just quoted) with changing his views on the role of natural selection, he replied:

[I presuppose] that natural selection is operative in this case (as in others). [And I have raised some tentative] questions about the role of "the 'channel' within which natural selection proceeds." That natural selection proceeds within such a "channel" is too obvious to have been questioned by anyone. Only the most extreme dogmatist would produce a priori declarations about its role in any particular case, whether it is virus shells, slime molds, bones of the middle ear, infrared vision, or whatever.

As before, I take no particular stand on the matter, for the simple and sufficient reason that virtually nothing significant is known—even about vastly simpler questions, such as the evolutionary origins of the waggle dance of honeybees. (Chomsky, email to "evolutionary psychology" list, 19 Oct. 1999; italics added)

Chomsky's agnosticism, here, is well judged. It's not clear that one can go much further than speculation on this topic. Languages don't fossilize. Nor do soft structures such as vocal cords (although oral bones and skulls do). Moreover, animal communication systems seem to be very unlike ours, having only finite productive power—as both Humboldt and Chomsky were keen to point out.

On the other hand, mammalian movements are highly variable—and recent work suggests that gestural language is grounded in neuroscientific mechanisms (such as mirror neurones; Arbib 2005) originally evolved for controlling other bodily actions (14.vii.c). Even the logic of *subject–predicate* has been attributed to specific neural circuits in the brain, embodying mechanisms necessary for many tasks performed by non-human primates (Hurford 2003).

In sum, if scepticism about the very possibility of linguistic evolution is out of place, pessimism about our ever being able to detail it may not be (Maynard Smith and Szathmáry 1995, ch. 17; 1999, ch. 13).

f. Creativity of language

The fourth item is included on the list (in subsection d, above) because Chomsky sees Humboldt as anticipating his own notion of the creativity of language. He first argued for this in 1962, at the International Congress of Linguists in Cambridge, Massachusetts; his paper was revised and expanded before publication (Chomsky 1964: 17–22).

Certainly, Humboldt constantly described language—alias the human spirit—as creative, indeed as self-creative. And he spoke of language making infinite use of finite means:

But just as the matter of thinking, and the infinity of its combinations, can never be exhausted, so it is equally impossible to do this with the mass of what calls for designation and connection in language. In addition to its already formed elements, language also consists, before all else, of methods for carrying forward the work of the mind, to which it prescribes the path and the form.

> The elements, once firmly fashioned, constitute, indeed, a relatively dead mass, but one which bears within itself the living seed of a never-ending determinability. (Humboldt 1836: 61)

Whether Chomsky is right to see Humboldt, and the other "Cartesians", as a precursor in this respect is another matter.

Chomsky's definition of the creativity of language referred to syntax, not thought (see Section vi.b). Admittedly, when (in the early 1960s) he included semantics within his grammar, novel meaning would automatically accompany novel form. But even then, he still prioritized syntax (see Section viii.c).

Descartes, by contrast, had focused on the power of (language-informed) reason or imagination to generate new thoughts, expressible in language. And Humboldt, too, had spoken of the power of language to define new thoughts. Chomsky stressed the purely formal fact that his grammar can generate infinitely many new sentences and deep structures, saying that "this 'creative' aspect of language is its essential characteristic" (Chomsky 1964: 8), and that "recursive rules . . . provide the basis for the creative aspect of language use" (Chomsky 1967: 7). His later complaint that "A number of professional linguists have repeatedly confused what I refer to here as 'the creative aspect of language use' with the recursive property of generative grammars, a very different matter" was thus misdirected (Chomsky 1972*a*, p. viii). In short, his notion of linguistic creativity was not that of his august predecessors.

Moreover, Chomsky's concept of creativity fails to capture all the cases ordinarily covered by the term, including those which Humboldt seems to have in mind here:

> At every single point and period, therefore, language, like nature itself, appears to man—in contrast to all else that he has already known and thought of—as an inexhaustible storehouse, in which the mind can always discover something new to it, and feeling perceive what it has not yet felt in this way. (Humboldt 1836: 61)

What Chomsky calls creativity is the exploration of an unchanging conceptual space, or thinking style (namely, generative grammar). Some human creativity is like this: run-of-the-mill jazz improvisation, for instance, or mundane examples of what Thomas Kuhn (1962) called normal science. But the most interesting cases are not.

These cases involve either unfamiliar combinations of familiar ideas, or the transformation of an existing conceptual space by altering one or more of its defining dimensions (Boden 1990*a*/2004, 1994*b*; cf. 13.iv). Such transformations make it possible to generate structures that were previously impossible (Humboldt's "things not felt this way before"?). Successive transformations over days, years, or even centuries (Humboldt, again?) can evolve the thinking style in complex and unpredictable ways: the development of post-Renaissance tonal music, for example (C. Rosen 1976; Boden 1990*a*: 59–62).

Both these types of creativity go beyond what Chomsky means by the term. And both, arguably, were what the Romantics were interested in. The poet Samuel Taylor Coleridge (1772–1834), who popularized neo-Kantianism in England, highlighted the mind's propensity for combinational creativity (Livingston Lowes 1930; Boden 1990*a*, ch. 6; see also the preamble to Chapter 12, below). And transformational creativity is perhaps comparable to the more structured, developmental, creativity posited by Humboldt.

I say "arguably" and "perhaps", here, because this point depends on how we understand the fifth item on our list. Just what did Humboldt mean by the "inner form" of language?

g. The inner form

In his Kawi-introduction, Humboldt described this inner form as a creative human force that is unfurled and developed in life, a vital linguistic instinct whose expression can be shaped, favoured, and hindered by external circumstances (Humboldt 1836, esp. 48–53). The environment doesn't impose itself on a passive tabula rasa, but elicits an active response from a mind inherently suited to make linguistically relevant distinctions—between speech sounds and other sounds, for example. And, he said, even (indeed, especially) the languages of "so-called savages" show a typically human diversity of expression.

The broad similarity to Chomsky's approach is, again, evident. And Chomsky declared that Humboldt's notion of form as generative process was "his most original and fruitful contribution to linguistic theory" (Chomsky 1964: 17). But the similarity between the two linguists becomes more fuzzy on closer inspection.

Humboldt's definitions of the inner form of language were both various and vague:

> The constant and uniform element in this mental labour of elevating articulated sound to an expression of thought, when viewed in its fullest possible comprehension and systematically presented, constitutes the *form* of language. (1836: 50)

So far, so (apparently) Chomskyan. However, Humboldt immediately added a gloss that doesn't sound Chomskyan at all:

> In this definition, form appears as an *abstraction* fashioned by science. But it would be quite wrong to see it also in itself as a mere non-existent thought-entity of this kind. In actuality, rather, it is the quite individual *urge* whereby a nation gives validity to thought and feeling in language. (p. 50)

Two pages later, having remarked that "the characteristic form of languages depends on every *single* one of their smallest *elements*", and that "language, in whatever shape we may receive it, is always the spiritual exhalation of a nationally individual life", he passed from nation states to the ultimate nature of language:

> From the foregoing remarks it is already self-evident that by the form of language we are by no means alluding merely to the so-called *grammatical form* ... The concept of form in languages extends far beyond the rules of *word order* and even beyond those of *word formation*, insofar as we mean by these the application of certain general logical categories, of active and passive, substance, attribute, etc. to the roots and basic words. It is quite peculiarly applicable to the formation of the *basic words* themselves, and must in fact be applied to them as much as possible, if the nature of the language is to be truly recognizable. (p. 51)

In that quotation, Humboldt seemed to have historical philology in mind. The crucial questions would then concern (for instance) how the Indo-European languages developed, and how they differ from the Malayan group, rather than (for example) the relations between active and passive, or the hidden depths of *John is easy/eager to please*. But Chomsky interprets inner form as embracing "the rules of syntax and word formation as well as the sound system and the rules that determine the system of concepts that constitute the lexicon" (Chomsky 1966: 26–7). And Chomsky's interpretation finds support in the following remark of Humboldt:

> [We must engage in] a laborious examining of fundamentals, which often extends to minutiae; but there are also details, plainly quite paltry in themselves, on which the total effect of languages is dependent, and nothing is so inconsistent with their study as to seek out in them only what is great, inspired, and pre-eminent. Exact investigation of every grammatical subtlety, every division of words into their elements, is necessary throughout, if we are not to be exposed to errors in all our judgments about them. It is thus self-evident that in the concept of linguistic form no detail may ever be accepted as an *isolated fact*, but only insofar as a method of language-making can be discovered therein. (Humboldt 1836: 52)

Because Humboldt defined "inner form" in so many different (and imprecise) ways, it's not clear whether he really is Chomsky's precursor in this fifth sense. Indeed, one might say that he never *defined* it at all: "what it means is never revealed either by way of explanation or example, let alone definition which is a device he seems to have spurned" (Aarsleff, in Humboldt 1836, p. xvi). It's hardly surprising, then, that "for a hundred years all discussion has failed to converge on any accepted meaning" (p. xvi). ("*All* discussion" includes scholars who can read Humboldt in the original German, as I myself cannot; so I don't think the translation can be to blame, here.)

When Humboldt was more specific about just what these formal relations might be, he was often less plausible. Thus he remarked that certain stories have developed independently in different cultures, explaining this not as the result of shared human experiences but as the unfolding of innate ideas (Novak 1972: 128). He even claimed that similar sounds are used by numerous languages to express similar ideas—where he wasn't thinking of onomatopoeia, as in *buzz*, *miaow*, or perhaps *crack* (Jespersen 1922: 57).

In short, Humboldt's "inner form" of language was specified only a little more clearly than Leibniz's veins of marble. Even his friends criticized him for his irremediable vagueness (Humboldt 1836, p. xv). And one of his twentieth-century admirers, the Danish linguist Otto Jespersen (1860–1943), put it in a nutshell: "Humboldt, as it were, lifts us to a higher plane, where the air may be purer, but where it is also thinner and not seldom cloudier as well" (Jespersen 1922: 57). If such a remark could be made even by an admirer, less sympathetic critics would clearly need some persuading.

A twentieth-century linguist wishing to revive the unfashionable humanist/nativist ("Cartesian") approach would face several challenges:

* They'd have to clarify the relation between linguistic structure and meaning.
* They'd have to explain just how linguistic variability is possible, and how it differs from the changes (learning) found in other types of behaviour.
* They'd have to specify what the universal innate ideas of language might be, and reconcile them with the diversity of actual tongues.
* And they'd have to show that nativist theories of language aren't mere optional alternatives to empiricist accounts, but inescapably more appropriate.

In Sections vi–viii, we'll see how Chomsky tried to do all those things. And in Section ix we'll see that by the 1980s, despite his enormous influence over the preceding quarter-century, some of his fundamental arguments were being rejected.

First, however, we must understand why it was that "Cartesian" linguistics became unfashionable in the first place.

9.v. The *Status Quo Ante*

Chomsky's immediate predecessors had no interest in the tasks just listed. The professional linguists he encountered as a student dismissed questions about inner forms of grammar, although some did engage with Jakobson's inner forms of phonology. They assumed that languages are arbitrarily diverse systems, wholly learnt from the environment.

If Humboldt's influence was still strong (whether acknowledged or not), it was in his wish to describe the many distinct grammars exemplified by natural languages, and to avoid historical bias in doing so. These two aims suffused the tradition in which Chomsky was trained: American "descriptive", or "structuralist", linguistics.

Following Boas's lead (see Section iv.c), the structuralists produced detailed descriptions, or taxonomies, of the grammatical categories of many little-known languages. They saw linguistics as an autonomous enterprise based firmly in the present, an example of what Saussure (1916) had called "synchronic" linguistics. That is, they aimed to describe speech, morphology, and grammars by a scientific method not tainted by history—nor, as we'll see, by introspection.

Like any intellectual movement, this one included a variety of positions. With respect to structuralism as a whole, it's been said that Chomsky's linguistics "continued some fundamental traits of its predecessor, recovered others, and unwittingly rediscovered still others" (Hymes and Faught 1981: 1). What's most relevant for our purposes is that the dominant school in the late 1930s–1940s—Bloomfieldian linguistics—adopted a broadly behaviourist methodology, and that it included theorists who tried to analyse linguistic structure in formal terms. Chomsky's work would undermine the former and emphasize the latter.

a. Two anti-rationalist 'isms'

The Bloomfieldians' rejection of introspection, and their striving for formalism, were inherited from two closely related movements in early twentieth-century thought. These were behaviourism in psychology and positivism in the philosophy of science.

Introspection was ousted from (American) psychology long before being banished from linguistics. Boas's follower Leonard Bloomfield (1887–1949) started out as an admirer of Wilhelm Wundt, whose mentalistic folk psychology informed his first book (Bloomfield 1914). As a professor of German, he could read Wundt in the original: the first five (of ten) volumes of the *Volkpsychologie*, had been published by the time Bloomfield wrote his text. (A brief summary appeared in English two years later: Wundt 1916.) His book also paid homage to Humboldt, as the founder of general linguistics (1914, ch. 10).

With hindsight, it's intriguing that Bloomfield recounted Wundt's theory of "inner forms". A meaning, or inner form, may be expressed in various grammatical sequences, as in *Mary ate the apple* and *The apple was eaten by Mary*—plus, of course, equivalent sentences in other languages. Wundt had argued that linguistics should focus on *sentences*, where a 'sentence' can't be defined in terms of words or phrases, but is recognized intuitively by native speakers. He'd also held that processes of transformation (*Umwandlung*) produce grammatical sequences from inner forms.

But Bloomfield didn't follow up these ideas. In the very year in which he praised Wundt's mentalistic work in a leading psychological journal (Bloomfield 1913), another such journal published John Watson's seminal paper on 'Psychology as the Behaviorist Views It' (J. B. Watson 1913). That paper swept the board in psychology: Watson was elected President of the American Psychological Association only two years later. Eventually, Watson would defeat Wundt in linguistics too.

Bloomfield soon abandoned Wundt, and his pre-Watsonian talk of inner forms and hidden transformations. Indeed, thanks to a "shocking" book very different from his first one, he became the leader of "scientific" structuralist linguistics, which downplayed meanings and had no place for inner forms (see below). But this didn't happen overnight. Two decades would pass before the behaviourist approach conquered in linguistics.

Watson's iconoclasm had been fuelled by frustration with the experimental impasse in psychology, *not* by independent philosophical argument. But the later behaviourists justified their approach by appeal to logical positivism, which swept the philosophy of science in the 1920s and 1930s.

Several leading positivists emigrated from Europe to America shortly before the Second World War. They included Hans Reichenbach (1891–1953) and Rudolf Carnap—one of whose protégés was Walter Pitts (see 4.iii–iv).

Besides stressing observational evidence and operational definition, these philosophers tried to axiomatize physics (an aim which intrigued the young Herbert Simon: Chapter 6.iii.a). In this, they were still following Descartes's formalist dream, which had inspired modernism—in science, art, and politics—since its origin in the seventeenth century (Toulmin 1999: 152–60). Believing that other sciences could then be defined in terms of physics, they hoped to "unify" science within a single deductive system.

Some behaviourists tried to do much the same thing, expressing their theories in terms of psychological definitions and quasi-axioms. The most detailed examples were developed by Clark Hull in the late 1930s and early 1940s (C. L. Hull 1943) (see 5.iii.b). But perhaps the most ambitious appeared much earlier, when logical positivism was relatively new: Albert Weiss's 'One Set of Postulates for a Behavioristic Psychology' (1925).

Weiss listed ten "postulates", or axioms, of psychology, outlining its potential unification with physics. These ranged from the motions of electrons and protons, through behaviour (including language, as "the characteristic factor in human behavior"), to civilization. And, strange as it may seem, civilization was defined by Weiss partly by reference to "electron–proton movements" (Postulate 9). The underlying thread supposedly linking electrons to civilization was Weiss's assumption that "human behavior and social achievement are ... forms of motion", as opposed to "psychical or mental phenomena".

As for language, this was defined as a system of "sensorimotor and contractile effects" linking stimulus and response with environmental changes.

If that were so, then sentences about propositional attitudes, using (intentional) verbs such as *know*, *believe*, *want*, and *hope*, could be analysed in purely truth-conditional (extensional) terms. But it's not clear that this is possible. The truth of *Mary believed that the bracelet had been lost*, for instance, is independent of the truth of the embedded sentence about the bracelet.

Philosophers were aware of this difficulty. So much so, that many saw intentionality as an insuperable logical–metaphysical boundary between psychology and natural science (Chisholm 1967; cf. Boden 1970). (What's more, many still do: see Chapter 16.) But the behaviourists ignored it. They either avoided psychological terms entirely or assumed—with Weiss—that they could be interpreted as forms of motion (stimulus and response).

Not all behaviourists went so far as trying to axiomatize psychology, but they did accept the positivists' general viewpoint. Indeed, they shamelessly took it for granted. Psychology textbooks before the 1970s typically opened with a chapter first exulting that psychology had at last 'escaped' from philosophy, and then uncritically offering a positivist account of science.

b. The shock of structuralism

Bloomfield was influenced by both behaviourism and positivism. He was converted to the former by Weiss (Bloomfield 1931), and invited by the philosopher Otto Neurath to contribute one of the first essays in a positivist encyclopedia of "unified science" (Bloomfield 1939). (Not that one can put too much weight on that: Thomas Kuhn's (1962) devastating attack on positivism—and Popperianism—would later appear as an annexe to the same encyclopedic review.)

At one stage, Bloomfield followed Weiss's example by formulating a set of fifty postulates for scientific linguistics and, for comparison, twenty-seven postulates for historical linguistics (Bloomfield 1926). More to the point, he referred repeatedly to "stimulus" and "reaction" in his most influential book, *Language* (Bloomfield 1933, e.g. 29–30).

This was the volume which tipped the linguistic profession—at least, in America—into the behaviourist mould. To be sure, the neo-Humboldtians Boas, Sapir, and Whorf weren't converted. They continued to study language (and meaning) in much the same way as before. But the mainstream shifted.

Bloomfield's new account was seen on its publication as "a shocking book: so far in advance of current theory and practice that many readers, even among the well-disposed, were outraged by what they thought a needless flouting of tradition" (B. Bloch 1949: 92). Nevertheless, it soon became "almost orthodoxy" (p. 92).

Part of this new orthodoxy was the *rejection* of speculation on the relation between language and mind:

> [Many linguists accompany their] statements about language with a paraphrase in terms of mental processes which the speakers are supposed to have undergone. The only evidence for these mental processes is the linguistic process; they add nothing to the discussion, but only obscure it. (Bloomfield 1933: 17)

Another part was the rejection of *meaning* as a theme for linguistics. Meaning, Bloomfield argued, can't be studied scientifically. Meanings are in practice defined (even by mentalists) "in terms of the speaker's situation and, whenever this seems to add anything, of the hearer's response" (p. 144). Rigorously defining the speaker's situation is impossible, however, for we don't know what associations the speaker has learnt (a point with which Humboldt would have agreed).

Accordingly, the Bloomfieldians focused on phonology and syntax, not semantics. They identified their theoretical categories without reference to meaning, by attending to physical measurements and statistical distribution patterns. They all tried to define their terms clearly. But many wished to replace verbal definitions by formal–symbolic ones. If that could be done, they thought, they might even identify scientific (meaning-free) methods by which phonological, morphological, and grammatical categories could be reliably—perhaps automatically—discovered.

In this journey towards formalism, they were running in parallel with empiricist philosophy of language—though with less delicate tread. During most of Bloomfield's life, the dominant movements were logical atomism and logical positivism, both of which drew heavily on Bertrand Russell's logic (Urmson 1956).

Some philosophers focused on meaning. For instance, they tried to define class concepts by lists of necessary and sufficient conditions; and they discussed the semantics of definite descriptions (noun phrases starting with *the*), and of the English words *and*, *if–then*, and *some*. (There's more to the English *and* than meets the logician's eye: according to logic, *London is the capital of England and fish have gills* is unproblematically true, but these two conjuncts would never be linked by *and* in normal conversation.)

It was this general approach which led the young McCulloch to try to formulate a logic of verbs: distinguishing past, present, and future while ignoring grammatical questions, such as where to place the past participle (4.iii.c). But it didn't affect mainstream linguistics, which had outlawed studies of *meaning*. The philosophy of language that was potentially relevant for structuralism was concerned rather with *grammar*.

Some positivists discussed logical/linguistic syntax at length (Carnap 1934; Reichenbach 1947, ch. 7). Indeed, Carnap explicitly argued that the best way to understand natural grammar is to compare it with the syntax of an artificially constructed language. But this positivist work attracted only the bravest structuralist souls. It was highly abstract (Carnap's formal notation was notoriously obscure, as remarked in Chapter 4.iii.f), and said very little about the specifics of natural languages. Not until the late 1940s was there a structuralist grammar that could compare in sophistication with logical studies of syntax.

Bloomfield himself, in his "shocking" book, offered a very simple formalism to explain the appearance of new word forms. His "proportional formulae" represented the selection of new forms by analogy with previously experienced ones: see Figure 9.1. (Bloomfield 1933: 406).

Bloomfield used these formulae to explain both correct uses of regular forms, such as the plural *cows*, and overgeneralizations of irregulars, such as the past-tense *dreamed* instead of *dreamt* (cf. Chapters 7.vi.a and 12.vi.e). He applied proportional formulae also to syntactic innovations in the history of a given language: for instance, the change in sixteenth-century English when people started to introduce subordinate clauses by the word *like* (as in *to do like Judith did*) (Bloomfield 1933: 407).

sow : *sows* = *cow* : *x*

scream : *screams* : *screaming* : *screamer* : *screamed*
= *dream* : *dreams* : *dreaming* : : *x*

Fig. 9.1. Proportional formulae for creating new word forms. Reprinted with permission from Bloomfield (1933: 406)

However, to call these expressions "formulae" was to claim too much. One can't reasonably complain that they had to be interpreted intuitively, for in the early 1930s the notion of automatic computation had yet to be defined (see 4.i.c), still less implemented. Even so, they were highly informal. Apart from the proportionality sign borrowed from mathematics, there was no symbolization here, nor even an explicit separation of the words into constituent morphemes (such as *scream* and *-ed*). This wasn't a new notation, nor a grammatical calculus, merely a mnemonic for summarizing certain morphemic patterns.

Bloomfield's gloss on Figure 9.1 showed that it wasn't a description of conscious (verbally reportable) processes, either:

> Psychologists sometimes object to this formula, on the ground that the speaker is not capable of the reasoning which the proportional pattern implies. If this objection held good, linguists would be debarred from making almost any grammatical statement . . . Our proportional formula of analogy and analogic change, like all other statements in linguistics, describes the action of the speaker and does not imply that the speaker himself could give a similar description. (Bloomfield 1933: 406)

Unconscious (physiological) mechanisms were doubtless responsible for the speaker's behaviour: Bloomfield didn't believe in magic. But the linguist's job was to describe the abstract structure of the linguistic behaviour, not to speculate on the processes by which it was achieved.

c. The formalist Dane

A more ambitious linguistic formalism was published a few years later by Jespersen. Situated as he was in Europe, Jespersen wasn't a card-carrying structuralist. Indeed, Chomsky (1996*a*) names him as a "Cartesian" precursor. But his early attempts at formalism attracted praise from Bloomfield (1933: 86), and were cited by Reichenbach in his positivist analysis of "conversational" language (Reichenbach 1947, ch. 7).

Initially, Jespersen used his notation to describe only phonemes, which he analysed in considerable detail. His "analphabetic" phonetic symbolism, a twentieth-century version of Wilkins's project of the 1660s, represented many distinct states of the vocal organs. In his *Analytic Syntax* (1937), Jespersen extended his notation to cover grammatical structure too. His work was not only ambitious, but original:

> So far as I know, this is the first complete attempt at a systematic symbolization of the chief elements of sentence-structure, though we meet here and there with partial symbolizations . . . (Jespersen 1937: 97)

He compared his notation with the formulae of chemistry and logic (p. 13). And (without digressing to explain the individual symbols) we can see that it was an analytic tool far in advance of Bloomfield's proportionalities. The phrases *burning hot soup* and *wide open windows* were both rendered as "2/321", and *curious little living creatures* was "21(21(21))"—(see p. 19). The sentence *I saw the soldiers, some of them very young indeed* was represented as "S V O [1^qp1 323]"—(see p. 47), and *I've found the key that you spoke of* as "S V O (12($3^c/1^{c}$*S_2 V p*))"—(see p. 76). And the two superficially similar sentences *The story is too long to be read at one sitting* and *The story is too long to read at one sitting* were represented respectively as "S V P (32p1(I^b3))" and "S* V P(32p1(IO^o*3))"—(see p. 64). As for more complicated examples, the sentence *Brother Juniper, forgetting everything except the brother's wishes, hastened to the kitchen, where he seized a knife, and thence directed his steps straightway to the wood where he knew the pigs to be feeding* became this: "S(1 1 2(Y O p1($1^2$1)) Vp1 2(3^c S V O & 3 V O($S^2$1) 3p1 2(3^c S V O(S_2 I)"—(see p. 93). As Jespersen laid it out, it was as follows:

> Brother Juniper, forgetting everything except the brother's wishes,
> S(1 1 2(Y O p1(1^2 1))
> hastened to the kitchen, where he seized a knife, and thence directed
> V p 1 2(3^c S V O & 3 V
> his steps straightway to the wood where he knew the pigs to be
> O(S^2 1) 3 p 1 2(3^c S V O(S_2 I)
> feeding. [NB Opening/closing brackets are unequal in number.]

Jespersen stated his general policy as "to follow the sentence or word combination that is to be analyzed word for word". (Even without the detailed explanations, one can see this strategy at work in the examples given above.) However, he allowed many exceptions to this rule. Specifically, "such combinations as *the man*, *a man*, *has taken*, *will take*, i*s taking*, etc. (generally also *to take*), even *can take*, are reckoned as one unit" (Jespersen 1937: 15).

Some of Jespersen's remarks about language recall Humboldt, whom (as remarked in Section iv.g) he admired greatly—and, with hindsight, Chomsky too. For example:

> What is certain is that no race of mankind is without a language which in everything essential is identical in character with our own . . . (Jespersen 1922: 413)

> The complexity of human language and thought is clearly brought before one when one tries to get behind the more or less accidental linguistic forms in order to penetrate to their notional kernel. Much that we are apt to take for granted in everyday speech and consider as simple or unavoidable discloses itself on being translated into symbols as a rather involved logical process, a fact that is shown, for instance, by the number of parentheses necessary in some of the examples. (Jespersen 1937: 15)

The reference to the "notional kernel" betrays Jespersen's belief, previously argued in his *Philosophy of Grammar* (1924), that behind the syntactic categories which describe the partly "accidental" form of an actual language there are deeper categories that are independent of existing languages. He saw the linguist's task as to investigate the relations between notional and syntactic categories, and to discover in what way all natural languages are "essentially identical".

Despite these Chomskyan overtones, Jespersen's work was significantly different from Chomsky's. Most importantly, his grammatical formalism wasn't generative. It described the syntactic structure of a sentence as given, not how it could in principle be built. (The term "generative" is here intended in its timeless mathematical sense, indicating the class of structures defined by some set of derivational rules.) As a corollary, Jespersen didn't explicitly ask what makes word strings grammatical or ungrammatical, since his theory was applied only to sentences actually present in the corpus.

(A "corpus" is a representative sample of sentences, or phrases, gleaned from some natural source: books, conversations, interviews ... In Jesperson's time it might have held only a few score items. Today, it may hold hundreds of thousands. The CHILDES corpus, for instance, is a database containing over 150 megabytes of speech-exchanges between parents and children of various ages, some using a second language and some the mother tongue; sub-corpora can be extracted which hold only utterances of parents/carers, or only utterances of 4-year-olds ... and so on.)

Jespersen failed to consider sentence generation partly because this typically requires syntactic procedures to be performed recursively. For instance, one noun phrase can be nested inside another, and that in another ... and so on. But the mathematics of recursion wasn't yet understood.

Even fifteen years later, when functioning machines using recursion were available, the scientific potential of recursion wasn't obvious. The philosopher Carl Hempel (1905–97) observed in 1952 that "recursive definitions which play an important role in logic and mathematics ... are not used in empirical science" (Hempel 1952: 11 n. 11). As Chomsky put it in the 1960s:

> The fundamental reason for the inadequacy of traditional grammars is a ... technical one. Although it was well understood [by traditional grammarians] that linguistic processes are in some sense "creative", the technical devices for expressing a system of recursive processes were simply not available until much more recently. In fact, a real understanding of how a language can (in Humboldt's words) "make infinite use of finite means" has developed only within the last thirty years, in the course of studies on the foundations of mathematics. (Chomsky 1965: 8)

d. Tutor to Chomsky

The first linguist to take recursion seriously was Zellig Harris (1909–92) at the University of Pennsylvania—where the ENIAC was built in the mid-1940s (3.v.b). Harris taught Chomsky as an undergraduate and as a graduate student, and they kept in close contact after the younger man moved to Massachusetts.

Chomsky later acknowledged Harris as his main inspiration, and a frequent discussant of his own work (Chomsky 1975: 4). Harris was no more interested than Bloomfield in the humanist/nativist ('language-and-mind') questions listed at the close of Section iv.g. But if anyone was a precursor of Chomskyan grammatical analysis, it was he.

In 1951 Harris published a book widely received as the most important contribution to descriptive linguistics since Bloomfield's *Language*. His *Methods in Structural Linguistics* was shown to Chomsky in proof, on joining Harris's seminar in 1947. It contained a detailed discussion of how to use distribution patterns to identify phonemes, morphemes, morpheme sequences (phrases), and other linguistic classes. In addition, it offered a formalism for representing such classes. This formalism—which Chomsky adopted and modified—was used by Harris to state a finite set of grammatical rules that could describe an infinite set of sentences.

When electronic computers appeared on the horizon, Harris hoped also to define a formal method which, if expressed as a computer program, would automatically identify the grammatical classes and patterns within a set of sentences drawn from an unknown language with an alien grammar (Z. S. Harris 1951, 1968). In this, a computer-age version of a long-standing structuralist goal, he didn't succeed.

That should have been no surprise, for the task is in principle impossible. It assumes that scientific categories and hypotheses can be derived from the data by theory-free induction—a view that was already being questioned by Popperian philosophers (K. R. Popper 1935; Goodman 1951). One can't even identify phonemes and morphemes without some guiding grammatical theory (Chomsky 1962: 125). However, Harris's system was "the most ambitious and the most rigorous attempt that has yet been made to establish what Chomsky was later to describe as a set of 'discovery procedures' for grammatical description" (Lyons 1991: 34).

It turned out, many years later, that the discovery procedures needed may be less specifically linguistic than some Popperians might expect. Recent work in the machine learning of language shows that relatively "light" theoretical assumptions can go a long way, if combined with powerful statistical techniques (see Section xi.a, below). These assumptions aren't language-specific in the sense that interested Chomsky, for they aren't grammatical.

Nevertheless, the general Popperian point still stands. In the 'pure' statistical analysis of the linguistic data, words are given for free by the spaces between the input letter strings. Moreover, phonetic categories—in the form of the alphabet—are taken for granted. From the point of view of psycholinguistics, the latter is entirely defensible. Although nativism with respect to syntax is highly questionable (see Section vii.c–d), nativism with respect to phonetics isn't: there's a finite set of human speech sounds, and newborn babies pay more attention to them than they do to sounds in general. Refutation of the Popperian claim would require successful induction over purely acoustic features—and even then, the acoustically structured anatomy of the cochlea could be seen as providing an implicit theory. (Although whether, and if so in what sense, acoustic feature detectors are really "innate" is debatable: cf. Chapter 14.vi.b and x.a–b.)

Harris introduced Chomsky not only to the project of rigorous formalism, but also to the notion of transformations (Z. S. Harris 1952, 1957, 1968). This idea wasn't mentioned in *Methods*, but was already prominent in Harris's mind. Indeed, by 1947 (when the book was completed) he was discussing it in his seminars, and in conversations with other linguists (Bar-Hillel 1970: 292; Z. S. Harris 1952: 18–25). The young Chomsky was a research assistant for some of the necessary analyses (Z. S. Harris 1952, n. 1). By the time of his own first publications, Harris's work on transformations was well known.

"Discourse analysis" was, in effect, a scientific study of the phenomenon of literary genre that had so fascinated Diderot. It showed how to give a structuralist description of entire connected texts, including spoken conversation. Its first public presentation (in 1949) was to an audience of anthropologists, for Harris's motivation in defining transformations was to apply linguistics to culture and personality. One might almost say that Humboldt's assimilation of culture and linguistics was being revived—but from a very different philosophical base, and with a very different methodology.

Instead of asking a priori what type of language should be used for a certain type of task, as Diderot had done, Harris asked what sentence structures actually occur within discourses devoted to that task. He answered this question in terms of inter-sentence distribution patterns, defined in terms of grammatical "transformations" specifying relations between entire sentences or morpheme sequences. Two syntactically complex sentences might be transforms of one, simpler, kernel sentence. And he found that "often it is possible to show consistent differences of structure between the discourses of different persons or in different styles or about different subject matters" (Z. S. Harris 1952: 1).

Rhetoricians and literary critics, Diderot included, had known for centuries that some such differences exist. And they had identified various genres and authorial signatures accordingly. But discourse analysis could describe these structural differences more precisely, as well as discovering many that hadn't been noticed before.

Harris's Presidential Address to the Linguistic Society of America in 1955 summarized his approach. Two sentence structures, related (for instance) as *active/passive*, or even as *question/answer*, were classed as transforms of each other on the basis of their observed distribution patterns. Specifically, they must share the same set of individual co-occurrences of morphemes. For example, *The kids broke the window* and *The window was broken by the kids* share the same verb and two nouns, although in reverse order. (The sentence *The young people destroyed the pane of glass* wouldn't count as a transform: Harris approached meaning only indirectly, through word distributions.) Problems arise with the morpheme pair -*ed/will*, since the first co-occurs with *yesterday* whereas the second does not (*The cliff crumbled yesterday*, *The cliff will crumble tomorrow*). Accordingly, just what is to count as the relevant "sentence environment" is not a straightforward matter. Harris (1957) gave a detailed discussion of how this problem can be addressed. (I can't resist remarking that the cliff actually crumbled on 11 January 1999, a few weeks after this passage was written, when Beachy Head—a famous landmark, and suicide point, near my home—sent tens of thousands of tons of chalk crashing into the English Channel.)

e. Not quite there yet . . .

If Harris's work was a crucial inspiration for Chomsky, it was nonetheless different from his in two—maybe three—important ways.

First, Harris defined his transformations by reference to distributional (statistical), and purely surface, features of sentences, as we've seen. Chomsky, by contrast, defined transformational grammar in terms of deep and surface structures. Or rather, he did so in his publications in the 1950s and 1960s, which are the crucial ones for our purposes. (More recently, he has dropped the deep/surface distinction: see Section viii.b.)

Another often remarked difference is that Chomsky's theory was generative whereas Harris's wasn't. Chomsky certainly saw his grammar as being original in this way, and Harris apparently agreed: "Noam Chomsky has combined transformational analysis [which features also in my own system] with a generative theory of sentence structure [which does not]" (Harris 1968: 4).

This is puzzling, however. It's true that Harris's aim was to describe sentences, not to show how they could be generated. But, as noted above, his formal theory potentially covered an infinite set of sentences. In that sense, it was a generative system—even though Harris didn't describe his rules as "derivational", nor use them to discuss sentence generation (cf. Hymes and Faught 1981: 166).

The third difference is uncontestable, and the most significant of all. As Harris put it: "Chomsky [unlike myself] has produced a mathematical specification of context-free languagelike systems within a spectrum of languagelike systems" (Z. S. Harris 1968: 4). In other words, Harris didn't locate his formalism within *the class of all grammars*.

That idea was developed by Chomsky. In so doing, he brought theoretical linguistics into the ambit of information theory, computer science, AI, and computational psychology.

9.vi. Major Transformations

The major transformation in twentieth-century linguistics dates back to 1957, with the publication of Chomsky's *Syntactic Structures*. It wasn't a high-profile event. On the contrary, the volume appeared in an obscure specialist series, from a small publishing house in Holland. Chomsky himself said later that they accepted it only because it was recommended by Jakobson, and that it wouldn't have been noticed but for a long, and near-simultaneous, review in a leading linguistics journal by his student Robert Lees (1957).

Lees didn't merely recommend the book, he championed it. Chomsky's "first established convert", he "went around to linguistics conventions and got the ball rolling" (Weimer 1986: 300). As Howard Gardner put it, he was "playing Huxley to Chomsky's Darwin" (H. Gardner 1985: 189). This was to become an all-too-common role for Chomsky's students, as we'll see (Section viii.a).

Nevertheless, Lees and Jakobson weren't the only ones to be impressed. This little book, of only 118 pages (and by an author only 29 years old), revolutionized the subject.

a. Chomsky's first words

If linguists had had their eyes open, the revolution might have happened a few years earlier. For Chomsky's first book wasn't his first publication. A handful of technical papers had already appeared, most in journals not read by linguists, such as the *Journal of Symbolic Logic*.

One of these was Chomsky's paper on 'Three Models for the Description of Language' (1956). This was a hierarchical classification of grammars, defined in purely abstract (non-linguistic) terms. With hindsight, it's clear that this paper, not *Syntactic Structures*, was Chomsky's most original and most well-founded contribution—even if many of Chomsky's fans have never read it (cf. also Chomsky and Miller 1958; Chomsky 1959*a*).

'Three Models' did what Harris admitted he'd never thought of doing: it located the grammar of natural language within the class of all possible grammars. At the top of Chomsky's formal hierarchy were the "recursively enumerable" languages: namely, all languages having some finite definition. A subset of those were the "context-sensitive" grammars, which in turn contained the "context-free" grammars (see below). At the lowest level were the "regular" grammars, definable in very simple terms. (Twenty years later, other linguists would insert "indexed grammars" between the context-sensitive and context-free levels: see Section ix.d.)

Computer scientists were interested immediately. Similar research was already being done by some of them, but Chomsky's was a highly valued contribution.

His distinction between context-free and regular grammars (Chomsky 1956) helped them to codify, respectively, the lexical structure of artificial languages and the high-level structure of programs. Moreover, he proved in 1962 that different types of computational device—again, abstractly defined—would be needed to deal with the various classes of grammar. A universal Turing machine (see Chapter 4.i.c) can compute *any* definable language. Push-down stacks (see Chapter 10.v.b) can compute context-free languages (an insight that helped establish the theoretical basis of modern parsers, and of compiler design). And, since regular grammars are lower down in the hierarchy, they can compute those too. But push-down stacks aren't necessary for dealing with regular grammars. Such grammars can be computed by finite-state machines (described below).

The linguists, by contrast, didn't notice these early papers. For they weren't published in 'their' journals. (Chomsky's initial connection with MIT was the Research Laboratory of Electronics, from which the Linguistics Department later emerged: see 10.ii.a.) In other words, the scientific cooperation dreamt of by Descartes (2.ii.b–c) was—temporarily—prevented by disciplinary specialism.

Nor did they read the mimeograph/microfilm versions of the typescript of Chomsky's voluminous background research, which had been available to the cognoscenti since 1955. Even the cognoscenti had found it hard going, and very few (perhaps none?) devoured it from first page to last. This wasn't mere laziness on their part: the typescript was neither easily legible nor readily intelligible. Many long passages consisted of sequences of definitions expressed in algebraic symbolism. Indeed, in 1956 MIT Press had refused to publish it in its original form. (It was eventually published twenty years later: Section ix.d.)

In short, most professional linguists, if they'd encountered this work at all, weren't interested. They saw Chomsky's line of enquiry as "unpromising and exotic" (Chomsky 1975: 2). They weren't unaccustomed to formalism, as we saw in Section v.c–e. But whereas Harris (and Jespersen) had employed formalism in the service of meticulous empirical description, Chomsky's early research focused on formalism *as such*.

Most psychologists weren't interested either. Some mathematical psychologists realized the crucial implication of the 'Three Models' theme: namely, that human minds must have a level of computational power capable of dealing with whatever class of grammar is appropriate to natural languages. But, working within the information-theoretic paradigm, they assumed that regular grammars would suffice.

It was *Syntactic Structures*, described by Chomsky as a "sketchy and informal outline" of this background material, which set the linguistic world afire. Its impact for linguistics was immediately likened—in the review that Chomsky mentioned (above)—to the transition from alchemy to chemistry (Lees 1957: 375–6). And as we'll see, it had a revolutionary effect outside linguistics too.

b. The need for a generative grammar

The research summarized in *Syntactic Structures* was inspired by Harris's discourse analysis. It also reflected Chomsky's interdisciplinary education: at Harris's suggestion, he'd studied mathematics and philosophy as well as linguistics. Formalism had flourished in the philosophy of science for some years, as we've seen. But Chomsky went beyond mere formalism. His approach was fundamentally mathematical in a sense in which Harris's wasn't.

Chomsky asked two new questions about natural-language syntax. On the one hand, he sought to state a generative grammar—as opposed to a merely descriptive one—for a given language (such as English). On the other hand, he aimed to compare the computational power of various types of grammar.

Readers of *Syntactic Structures* weren't burdened with the bristling technicalities of his 1956 paper, but those ideas underlay the argument. Although he didn't locate his new grammar at a particular point within the grammar hierarchy, Chomsky's focus on differences in computational power largely explains the book's wide influence: across linguistics, computer science, psychology, and the philosophy of language.

The brief text of *Syntactic Structures* took a fresh, not to say shocking, approach. It denied two fundamental structuralist (Bloomfieldian) assumptions: that the goal of linguistics is to define a discovery procedure for grammars, and that language can be explained in probabilistic, behaviourist, terms. Moreover, it started out not from a rich description of actual usage but from dry definitions mentioning not a word of English, Sanskrit, or Kawi.

In the opening pages, Chomsky defined language, and the goal of linguistics, in abstract terms:

> I will consider a *language* to be a set (finite or infinite) of sentences, each finite in length and constructed out of a finite set of elements. All natural languages are languages in this sense . . . Similarly, the set of "sentences" of some formalized system of mathematics can be considered a language. The fundamental aim in the linguistic analysis of a language L is to separate the *grammatical* sequences which are sentences of L from the *ungrammatical* sequences which are not sentences of L and to study the structure of the grammatical sequences. The grammar of L will thus be a device that generates all of the grammatical sequences of L and none of the ungrammatical ones. (Chomsky 1957: 13)

A generative grammar is a set of (timeless) derivational rules. It doesn't tell us just how to build or parse a specific utterance. In other words, it isn't a computer program. However, it is a computational notion. The "device" mentioned here—and the "machine" mentioned elsewhere (e.g. p. 52)—was a purely mathematical one, as is a Turing machine (see 4.i.c–d).

But if Chomsky wasn't talking about computers he was, indirectly, talking about people:

> Any grammar of a language will *project* the finite and somewhat accidental corpus of observed utterances to a set (presumably infinite) of grammatical utterances. In this respect, a grammar mirrors the behavior of a speaker who, on the basis of a finite and accidental experience with language, can produce or understand an indefinite number of new sentences. Indeed, any explication of the notion "grammatical in L" . . . [offers] an explanation for this fundamental aspect of linguistic behavior. (p. 15)

Even so, his grammar wasn't a set of instructions for psychological processing. Rather, it represented the underlying grammatical competence of the speaker (see Chapter 7.iii.a).

In offering his new vision of what a theoretical linguistics should be like, Chomsky asked four interrelated questions:

* How can we discover a grammar for a given set of sentences?
* How can we know that a grammar generates all and only these sentences?
* How can we evaluate alternative grammars?
* And how can we express the differences between grammars, of either artificial or natural languages?

His answer to the first question wasn't what structuralists expected. On his view, no scientific ("practical and mechanical") discovery procedure can be found (1957, ch. 6). Even Harris, whose work was more careful than most, hadn't succeeded in defining one. The best we can hope for (and what Harris had actually provided), said Chomsky, is an evaluation procedure: a method for deciding, given a finite set of sentences, which of two candidate grammars is preferable. Even this is difficult to define formally, for grammars—like all scientific theories—are evaluated largely in terms of their simplicity, a notoriously elusive notion.

A candidate grammar for a natural language must generate all and only the grammatical sentences. But how do we know which these are? No search of an existing corpus can identify them. Statistics are inappropriate, since most sentences in any reasonable corpus will be one-offs: uttered on only one occasion. And if a sentence isn't there at all, so what? Any native speaker can, and frequently does, come up with new ones.

This is a matter not merely of observation, but of syntax. (At this stage, it was syntax, not semantics—still less, reason or imagination—which was given the credit for the creativity of language: see Section iv.f, above.) One can produce new sentences, for instance, by conjoining two or more old ones, or by recursively nesting one inside another. (Whether the result will always be intelligible was addressed in Chapter 7.ii.b.)

It follows, said Chomsky, that judgements of grammaticality must be left to the intuitions of native speakers. For they can reliably say whether a given word sequence is grammatical even if they can't explain their judgement.

Data-respecting critics soon complained—and still do—that by *native speakers* he meant *a single native speaker whose intuitions one trusts*—in Chomsky's case, Chomsky. Such a source hardly seems reliable: Chomsky himself changed his mind twice about his famous example "Colourless green ideas sleep furiously". And how were these armchair intuitions to be monitored? The structuralists, too, had used native informants to assess grammaticality, but had insisted that the conditions in which evidence is elicited from them be carefully controlled, to prevent experimental bias of various kinds (Z. S. Harris 1957, sect. 4). Disagreement arose, in psychology as well as linguistics, over the role of extensive sampling as against the consideration of individual cases: see Chapter 7.iii.d. Chomsky himself, however, remained content to rely on his natural, untutored, intuitions.

Assuming that we've agreed on a set of grammatical English sentences, we must ask "what sort of device can produce this set". This requires us, said Chomsky, to compare the power of different conceivable grammars. He addressed this task by starting from Claude Shannon's information theory (4.v.d).

c. Beyond information theory

Shannon had defined a number of artificial languages in the 1940s, suggesting that:

> These artificial languages are useful in constructing simple problems and examples to illustrate various possibilities. We can also approximate to a natural language by means of a series of simple artificial languages. (Shannon and Weaver 1949: 13)

Chomsky agreed that artificial languages are useful comparison points for natural language. But, with his grammar hierarchy lurking in the background, he saw the prospects for Shannon's serial "approximations" as hopeless.

The simplest example Chomsky considered, lying at the lowest ("regular") hierarchical level, was a *finite-state grammar* (*language*, *machine*), or FSG. This is a grammar—of which there are infinitely many—that describes a device with a finite number of states, including an initial state and a final state, and a finite set of rules for passing from one state (considered in isolation) to the next (cf. Shannon and Weaver 1949: 15–16). In a linguistic context, "passing from one state to the next" would involve either emitting or accepting a word, depending on whether the FSG was being used to produce word strings or to parse them, respectively.

Taking up Shannon's discussion, Chomsky pointed out that some FSGs could produce only a finite number of "sentences"—in some cases, only two (see the first state-diagram in Figure 9.2) (Chomsky 1957: 19). Clearly, such examples couldn't model the apparently infinite potential of natural languages. But a more powerful type of FSG could be defined by adding one or more closed loops, so that instead of passing directly from state n to state $n+1$, the device may—optionally—return to state n again, and again . . . before finally leaving it. As a linguistic analogy, the sentence *The man comes* can be extended to *The old man comes*, *The nasty old man comes*, etc. by inserting one or more adjectives at the relevant point. This type of FSG is recursive, so can produce infinitely many sentences (compare using five adjectives, or fifty, or . . .).

Because of the potentially infinite number of sentences of English (French, Kawi . . .), Chomsky had the extended type of FSG primarily in mind (cf. Chomsky and Miller 1958). Of the two FSGs shown in Figure 9.2, then, he was less interested in the first than in the second, which contains a closed loop. Indeed, the simpler type is now sometimes forgotten, or anyway ignored (as an uninteresting special case). So FSGs *as such* are sometimes said to be capable of producing infinitely many word strings, or sentences.

Shannon had pointed out that a specific probability could be assigned to each possible state transition allowed by an FSG, and therefore that different artificial languages could be defined by assigning distinct sets of probabilities. Such probabilistic FSGs are called "Markovian". In natural languages, too, the successor probabilities for both letters and words vary. In English, though not in Polish, the state *z* is highly improbable as a successor to *c*. Similarly, the state *man* is fairly probable, given the state *old*, whereas the state *because* is highly improbable as a successor to *the*. If realistic probability measures were added for each state transition, Shannon said, something resembling natural language would result.

To illustrate this point, Shannon suggested a number of simple rules to produce various Markovian letter strings and word strings. These strings approximated English more and more closely as the number of previous letters or words considered (zero,

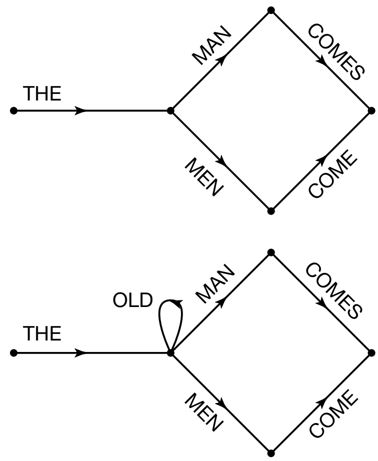


FIG. 9.2. State-diagrams for two FSGs. The first can produce only two sentences, whereas the second—because of the loop—can produce infinitely many. Redrawn with permission from Chomsky (1957: 19)

one, or two) increased. For instance, choosing words independently of each other (but guided by their individual frequencies) resulted in this:

REPRESENTING AND SPEEDILY IS AN GOOD APT OR COME CAN DIFFERENT NATURAL HERE HE THE A IN CAME THE TO IF TO EXPERT GRAY COME TO FURNISHES THE LINE MESSAGE HAD BE THESE. (Shannon and Weaver 1949: 14)

Not all of the two-word sequences included here would actually occur in English, and only one of the four-word sequences (AND SPEEDILY IS AN). By contrast, allowing the choice of each word to depend upon its immediate predecessor produced this:

THE HEAD AND IN FRONTAL ATTACK ON AN ENGLISH WRITER THAT THE CHARACTER OF THIS POINT IS THEREFORE ANOTHER METHOD FOR THE LETTERS THAT THE TIME OF WHO EVER TOLD THE PROBLEM FOR AN UNEXPECTED. (p. 14)

This passage contains many sequences of four or more words that could occur in real sentences. (Anyone but an information theorist would say ". . . that could make sense".) Indeed, the twelve-word sequence from FRONTAL to POINT could occur in a guidebook on country walks, in a sentence such as "It is because of the savage frontal attack on an English writer that the character of this point on the pathway has changed."

The method Shannon used to compose the latter word string was a simple one. Taking a book from his bookshelves, he opened it at random, and wrote down a word selected randomly from the open page. Next, he opened the book at another randomly chosen page, and read on until he encountered the previously recorded word. At that point, he wrote down the immediately following word. Then he repeated the process again and again, always using the most recently recorded word as his guide.

Clearly, he didn't pick *Alice's Adventures in Wonderland.* Indeed, he may have deliberately avoided picking a 'literary' text as his sample source. The reason is that this method would grind to a halt if any newly chosen word occurred only once in the entire book.

(i) *Sentence* $\rightarrow$ *NP* + *VP*
(ii) *NP* $\rightarrow$ *T* + *N*
(iii) *VP* $\rightarrow$ *Verb* + *NP*
(iv) *T* $\rightarrow$ *the*
(v) *N* $\rightarrow$ *man*, *ball*, etc.
(vi) *Verb* $\rightarrow$ *hit*, *took*, etc.

FIG. 9.3. Rewrite rules for a simple phrase structure grammar. Reprinted with permission from Chomsky (1957: 26)

Even this simple method was time-consuming. It would be interesting to go further, said Shannon, by considering more than one previous word. But the labour involved in hand-searching the sample text would become enormous even at the two-word stage. (George Miller, instead of searching texts, asked people to volunteer 'the next word' after having been given one, two, or . . . six words: the higher-order, more redundant, strings were remembered more easily: G. A. Miller and Selfridge 1950.)

If computers had been available at mid-century, to collect the necessary statistics, many structuralists would have been interested. Think of the textual studies that might have been done on the basis of Harris's discourse analysis, for instance. Today, such studies are possible. But in the 1950s, computing technology was still in its infancy. In practice, then, Shannon's approximations weren't followed up by those linguists whose theoretical sympathies were similar to his.

Chomsky, in any case, wasn't one of their number. He saw the statistical approach as a dead end in theory, not just in practice. He believed he could prove, as a matter of principle, that FSGs, although they can generate infinitely many new sentences, cannot produce all the examples of everyday English. Specifically, they cannot generate sentences involving nested expressions, where the selection (in Shannon's terms, the probability) of word $n + 1$ depends not on its immediate predecessor, word n (nor on the m immediate predecessors), but on some word indefinitely many places earlier. Consider this sentence, for instance: *The cat with black fur and long whiskers was sitting on the mat*. The ninth word here depends not on the eighth, but on the second: replace *cat* by *cats*, and you must change *was* to *were*. It's structure which is doing the work here, not statistics.

Such nested dependencies, Chomsky argued, can in principle occur on infinitely many levels. (We saw in Chapter 7.ii.b, however, that *in practice* they cannot.) They require a "phrase-structure grammar", in which sentences are derived by a series of ordered "rewrite rules". Rewrite rules *as such* were invented not by Chomsky, but by the logician Emil Post (1943: see 10.v.e). But Chomsky was the first to apply them to natural language. He used them to produce phrases, including terminal elements (words), on various hierarchical levels (see Figure 9.3).

Unlike Jespersen twenty years earlier, Chomsky aimed to account for every single word in the sentence. So he analysed nominal phrases like *the man* and *a man* into two parts—and when he came to discuss transformations (see below), he analysed auxiliary phrases such as *has taken*, *will take*, and *is taking*, and showed the underlying relations between them.

The complete grammatical structure of the sentence, though not the order in which the rewrite rules were applied, can thus be represented by a tree diagram in which every

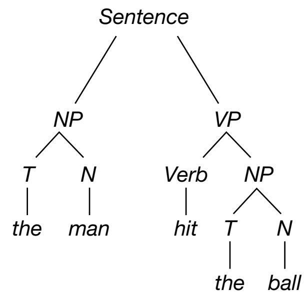


Fig. 9.4. Phrase-structure diagram showing the derivation of "The man hit the ball". Redrawn with permission from Chomsky (1957: 27)

word is separately considered (see Figure 9.4). This diagram shows how the grammatical categories are linked: from top to bottom, not—as in Markovian grammars—from left to right.

d. Transformational grammars

Phrase-structure grammars weren't new. They implicitly underlay immediate-constituent analysis, in which a sentence is recursively broken into contiguous constituents. This method had been used by many people, from the Port-Royal logicians to Harris, not only to parse unproblematic sentences but also to explain ambiguities. Linguists had long known, for instance, that the phrase structure of *The old men and women* could be either *The (old men) and women* or *The old (men and women)*, and that the meanings differed accordingly.

Similarly, the intuitive notion that different grammars might have different computational power wasn't new. Shannon, after all, had already contrasted FSGs having finite or infinite string sets.

The novelty in Chomsky's argument—as considered so far—was to define phrase-structure grammars as a general class, to prove that they're more powerful than Markovian (probabilistic) approaches, and to show that *natural* languages require grammars whose computational power is at least as great as that of a phrase-structure grammar. Significantly, he did the latter by relying, not, as Shannon had done, on some intuitive notion of computational power, but on his new classification of grammars. Each level in the hierarchy could generate all the strings generated by the level below, and more. FSGs lay at the lowest level, and phrase-structure grammars lay above them.

Chomsky was speaking here about *context-free* phrase-structure grammars. In his previous mathematical analysis, he'd distinguished between context-free (CF) and context-sensitive (CS) phrase-structure grammars.

In a CF grammar, each rewrite rule can be applied whenever the one-and-only symbol on the left-hand side is present. In a CS grammar, more than one symbol occurs on the left-hand side of the rewrite rules; a rule may rewrite one of them, and carry the others over unchanged. In the hierarchy of computational power, CS grammars had been located much higher than CF grammars. It was already clear that CS grammars included many "languages" of interest only to mathematicians, not to linguists. For linguists, the more interesting, more restrictive, and more plausible hypothesis was that natural languages have CF grammars. Indeed, the term "phrase-structure grammar" is often used to mean CF grammar.

At this point in the argument, Chomsky went further: he claimed that grammars of even greater computational power than phrase-structure grammars are needed for natural language. (My use of the word "claimed" here, instead of "proved", will be justified in Section ix.d–e.) In other words, natural languages involve a grammar located somewhere between phrase-structure grammars and universal Turing machines.

Chomsky admitted: "I do not know whether or not English is itself literally outside the range of [context-free phrase-structure] analysis." But he claimed that it could be so described (if at all) "only clumsily", by a complex, ad hoc, and unrevealing theory (p. 34). The reason he gave was that certain simple, and intuitively obvious, ways of describing sentences can't be expressed in terms of phrase structures, as then understood.

Everyday examples that require something more than a phrase-structure grammar, said Chomsky, include sentences involving auxiliary verbs (*The man has read the book*, *The man has been reading the book*); passive sentences (*The book is read by the man*); interrogatives (*Does the man read the book?*, *Will the man have been reading the book?*); *and* wh-questions (*Why/where/when/how does the man read the book? Who reads the book?*). It's intuitively obvious that there are close grammatical relations between the sentence *The man reads the book* and all these other sentences. But, Chomsky claimed, these relations can be simply expressed only by rules of the general form: *If the derivation of a grammatical sentence S-1 is such-and-such, then another grammatical sentence, S-2, can be produced by altering S-1 thus-and-so*. (*Sentence*, here, is a technical term, that covers any legal structure within a grammatical derivation. So *The man VP* is a sentence in this sense, for it is a structure derivable on the way to the terminal structure, *The man reads the book*.)

Intuitively, all these sentences seem to be related to the last one: *The man reads the book*. What's therefore required, said Chomsky, is a set of rules that can start with this sentence and derive all the others. Because rules like this define ways of transforming one sentence, or tree structure, into another (S-1 into S-2), grammars that contain them are called transformational grammars, or TGs.

A TG consists of a "base component"—a phrase-structure grammar that generates the initial trees—plus one or more transformational rules. This general definition covers many examples, including Harris's work and perhaps even Port-Royal grammar (see Sections iii.c and v.d–e). But the term TG is widely used to refer only to Chomsky's grammar, or variations of it—a fact that has aroused angry criticism, as we shall see (in Section ix.d).

TGs in general can't be located at any specific point in the grammar hierarchy, because the base component needn't be context-free (though it usually is), and because it's not clear how much—if any—computational power is added to it by the various transformations. The latter point makes it difficult to place even a particular TG in the hierarchy. The assumption implicit in *Syntactic Structures* was that a TG must lie somewhere between Turing machines and CS grammars—but just where was unclear.

In Chomsky's 1957 terminology, the simplest of the English sentences given above, and the point of reference for all of the others, is a "kernel" sentence. In other words, *The man reads the book* is derivable from its underlying grammatical form (which is derived by phrase-structure rewrite rules) by using only obligatory transformations (p. 45). These are the transformations that must be applied if the terminal sentence is to be grammatical at all. Non-obligatory transformations are applied either to the forms underlying kernel sentences, or to previous transforms thereof. In deciding which transformations are obligatory in English, and which optional, Chomsky relied on grammatical intuitions such as the one just remarked.

Chomsky's general claim, here, was that sentences of natural language must be represented on more than one theoretical level. In broad terms at least, many linguists were quick to agree. One later recalled Chomsky's argument as having had a "dramatic input" at the time:

> By God, native speakers DID associate active and passive counterparts . . . Yet the two were different and even unrelated in a phrase-structure analysis. His ideas seemed salient, dramatically salient, from the outset. (Hymes 1972)

As a result, "within a few years, quite a number of linguists were working on transformational grammar" (Chomsky 1975: 4).

Within a few years, also, the philosopher Hilary Putnam noticed that transformational grammars as defined above have the same power as a Turing machine (Putnam 1961: 101 ff.). If we allow just anything to count as S-1 and S-2, then this class of grammars has potentially infinite power. Any conceivable language, whether natural or artificial, recursive or non-recursive, could be derived by some TG (if it's computable at all). The set of prime numbers, for example, can be TG-generated.

This point was awkward for linguists claiming that TGs underlie natural languages. A scientific theory should not only explain what *does* happen, but also restrict what *could* happen (see 7.iii.d).

A grammar, for instance, should generate all *and only* the acceptable sentences of the language concerned. If any conceivable language, even including the (highly un-natural) set of prime numbers, can be 'explained' by some TG, then the class of such grammars can't exclude any possibilities. To be sure, a given TG might explain English and exclude French. (To someone seeking a TG to fit *all* natural languages, as Chomsky later did, this would be a weakness rather than a strength: see Section vii.d.) But Chomsky's supposedly surprising claim in 1957 was that each natural language requires *some TG or other*, and that *some TG or other* will suffice to explain it. Putnam's work suggested that this amounted merely to saying that natural languages are Turing-computable. That had been assumed for years by logicist philosophers (see Chapter 4.iii.c), so was hardly exciting.

The Turing-equivalence of transformational grammar was to be discussed in detail in the 1970s (Peters and Ritchie 1973). Partly because of this, by 1980 some linguists had rejected transformations altogether. They offered more restrictive scientific theories of natural language, relying on grammars low down in Chomsky's hierarchy.—But that was for the future (see Section ix.d–f, below).

Meanwhile, Putnam's theoretical point was shunted into the background for over twenty years, as linguists tried to discover *which transformations in particular*, and in which order, are needed to represent a given natural language—usually, English. The specific transformations that Chomsky hypothesized for English included, for instance, rules for converting a kernel sentence into a question either by moving the auxiliary verb to the beginning (so *The man will read* becomes *Will the man read?*), or by inserting the appropriate form of the auxiliary *do* at the beginning, and altering the form of the main verb (so *The man reads* becomes *Does the man read?*).

The details don't concern us (pp. 61–84). Nor need we discuss the rather different account of transformations that Chomsky was to give in his third book, in which kernel sentences, with their compulsory/optional transformations, were nowhere to be seen (Chomsky 1965). (Transformations as such became rarer in Chomsky's later work, as many functions previously effected by transformations were transferred to other rules of the grammar: see Section viii.b.)

More important, for our purposes, is the general form of the argument in *Syntactic Structures*. For this had implications far beyond linguistics. It alerted philosophers of language, psycholinguists, and the early computational psychologists. (One might say that it didn't alert psycholinguists so much as *create* them; the countless studies of "verbal behaviour" were focused on learning, not language—word strings, not sentences: see Chapter 6.i.e.) But it *didn't*, yet, alert psychologists in general.

e. So what?

Chomsky's analysis had radical—and to many, highly unwelcome—implications. For it followed, as he took pains to point out, that utterances (i.e. "sentences" in the everyday, non-technical sense) must be theoretically represented on several levels.

On the one hand, there is the surface structure: namely, the grammatical form of the sequence of words (morphemes, phonemes) that constitutes the sentence. The surface structure is interpreted phonetically, to produce a spoken utterance. On the other hand, there is the deep structure: the derivational history (including rewrite rules, kernel forms, and transformations), which will usually have many levels. The deep structure is interpreted semantically, to give the sentence's meaning.

(Although the technical terms "deep/surface" were introduced in Chomsky's second book, a related distinction was clearly made in the first. Similarly, the terms "competence/performance" didn't feature there, but the relevant distinction did.)

From the point of view of Bloomfieldian linguists (and behaviourist psychologists, too), this added insult to injury. Having already been told that statistical theories can't describe language, they were now being told that the observable aspects of language are underlain by a complex unobservable structure that must be represented in any adequate theory. Mentalism had returned, with a vengeance.—Mentalism, but not introspectionism: Chomsky agreed with Bloomfield that the speaker has no direct access to inner mechanisms.

One might argue that grammar is a mathematical exercise having nothing to do with mentalism. Indeed, theoretical computer scientists, who were much influenced by Chomsky's analysis of the computational power of different types of "language", gave no thought to psychology. And Chomsky himself said nothing, here, about the broader mentalistic implications that were to be so controversial in the 1960s (see Section vii.c–d).

In other words, *Syntactic Structures* contained no explicit comment on behaviourism, although Chomsky's anti-Markovian arguments were clearly relevant to it. Moreover, there was no mention of universal grammar, and therefore no hint that linguistics has nativist psychological implications, nor any relevance for the philosophy of mind. The theory of generative grammar wasn't yet being explicitly presented as "a particular sub-branch of cognitive psychology", still less as a study of "the human language faculty as such" (Chomsky 1975: 9).

The reason these "Cartesian" hypotheses weren't mentioned wasn't that Chomsky hadn't yet thought of them, but that he was deliberately suppressing them. They would have clashed with structuralist assumptions about the diversity of language and the emptiness of talk about language-and-mind—and the book was radical enough, even without them. Indeed, its ideas were so unconventional that, as he later recalled: "The one article I had submitted on this material to a linguistic journal had been rejected, virtually by return mail" (Chomsky 1975: 3). Understandably, then, he felt it to be "too audacious to raise the psychological issues which were in the background of my mind" (p. 35).

However, psychology wasn't ignored entirely. Linguistics, he said, is relevant to psycholinguistics—specifically, to the study of meaning. He was concerned not with vague claims like the Sapir–Whorf hypothesis, but with clearly specified theories about the psychological abilities (though not the processing details) involved in understanding:

> [My] formal study of the structure of language . . . may be expected to provide insight into the actual use of language, i.e. into the process of understanding sentences. (p. 103)

> [The] notion of "understanding a sentence" must be partially analyzed in grammatical terms. To understand a sentence it is necessary (though not of course sufficient) to reconstruct its representation on each level, including the transformational level where the kernel sentences underlying a given sentence can be thought of, in a sense, as the "elementary content elements" out of which this sentence is constructed. (p. 107–8)

The last remark occurred in the context of a discussion of the prospects for semantics. Here, the book was rather more acceptable to Chomsky's Bloomfieldian predecessors. For meaning was sidelined. Any theory of syntax, Chomsky argued, must be independent of semantics. Indeed, semantics is secondary to syntax, for understanding the meaning of a sentence rests on grammatical analysis.

Even so, structuralists were being challenged. Although Chomsky agreed with them that a science of meaning can't be based on vague appeals to introspection, or on unknown past associations, he did say that a (syntax-based) science of meaning is in principle possible. Indeed, semantics would play a fundamental role in his next book, in which he described a grammar as a set of rules relating the meaning of a sentence to its phonetic structure (Chomsky 1965).

These passages alerted psychologists, some of whom were aghast and others inspired. The repercussions in psycholinguistics, and the relevance of Chomsky's distinction between competence and performance, were discussed in Chapter 7.ii–iii.

In Chapters 5 and 6, we considered the growing revolt against behaviourism—to which Chomsky was an enthusiastic latecomer. Now, let's look more closely at what he had to say about the then prevailing psychological orthodoxy.

9.vii. A Battle with Behaviourism

Chomsky initiated a battle with behaviourism, but not the war itself. Others were fighting at the same time, and some had reached the battlefield earlier (see 5.iv and

6.i–iii). But it was Chomsky who flourished the sword most cuttingly and wounded his opponents most bloodily. And it was he who returned to the attack over and over again, in both his scientific and his political writing. Consequently, he's been awarded most of the victory medals, even though others merited them too.

(Not being one for half-measures, he also rejected Piaget's "epigenetic" compromise between nativism and behaviourism: see Chapters 7.vi.g and 14.x.c. Contributions to the Chomsky–Piaget debate are collected in Piatelli-Palmarini 1980.)

Chomsky's training in descriptive linguistics had been informed by behaviourist assumptions, as we've seen (in Section v.a). But in the early 1950s, he ignored this psychological movement. He looked at it in detail only after he'd finished his basic theoretical research on language, in 1956–7. Indeed, he said later that the critique of finite-state languages with which he'd opened *Syntactic Structures* was "an afterthought", not included in his larger manuscript (Chomsky 1975: 40).

From the late 1950s on, however, he pursued the behaviourists relentlessly. By the mid-1960s, he had rejected—or rather, expanded—the definition of linguistics given in *Syntactic Structures*. Now, he identified the goal of linguistics in explicitly nativist terms—with radical implications for psychology, and for the philosophy of mind.

a. Political agenda

In his pursuit of behaviourism, Chomsky was driven at least as much by political passion as by abstract argument. For he saw it in political terms, and didn't like what he saw.

He linked behaviourism with the social-scientific 'experts' whose advice to the US government, and especially to the military, he—and countless others—regarded as political anathema. AI scientists were suspect too, despite their non-behaviourist approach, for their work had made 'smart' bombs and aerial reconnaissance possible (Chapter 11.i).

Chomsky expressed this complaint on many public platforms at the height of the Vietnam war. Indeed, he spoke in the very first public demonstration against the war, held on the Boston Common in October 1965. He spoke, but he wasn't heard:

> the mobs were so hostile that none of us could say an audible word. The only reason we weren't killed, I suppose, was that there were hundreds of police—who didn't like what we were saying one bit, but didn't want to see anyone murdered on the Common. (quoted in Swain 1999: 29)

Besides braving mobs on the Boston Common, he was put in prison on more than one occasion (Swain 1999: 24). He was thus a powerful voice in the counter-culture (Chapter 1.iii.c).

He still is. For, unlike his MIT colleague Joseph Weizenbaum (11.ii.d), he continued—and intensified—his complaints long after the 1970s. He repeated them, with meticulous documentary evidence, all around the world in connection with various other events—most recently, those of 11 September 2001 in New York, and their grisly aftermath in Afghanistan and Iraq. He still berates "respected intellectuals" for justifying the actions of "violent and murderous states"—including the USA (Chomsky 2001: 73; 2005). And he still sees political engagement as a responsibility of the intellectual (1996*c*).

His speeches continue to draw a multitude of listeners: his Royal Institute of Philosophy Lecture in 2004, for instance, saw about 3,000 youngsters queuing along the London streets (Chomsky 2005). And his writings have attracted adulatory praise from political sympathizers (Barsky 1997). They doubtless drew some people to his scientific writings who wouldn't otherwise have been interested in them.

Given the combination of his political and linguistic work, he eventually became the most-quoted living writer, and the eighth most quoted in history (Barsky 1997: 3). (The other seven were Marx, Lenin, Shakespeare, Aristotle, Plato, Freud—and, in fourth place, the 'author' of the Bible.)

In making these politico-scientific links, Chomsky repeatedly criticized what he saw as the philosophical assumptions underlying the science. Specifically, he claimed that empiricist and rationalist views of humanity inherently tend to promote political oppression and freedom, respectively (e.g. Chomsky 1972*b*: 45–54 and *passim*; cf. N. V. Smith 1999).

The justice of this general claim isn't obvious; some have argued that the reverse is true (Sampson 1979*a*). For instance, Locke's political philosophy—which underlay the English, French, and American revolutions—hardly suggests that empiricism is incompatible with liberty. Indeed, Locke saw doctrines of innate ideas as politically dangerous, since they encourage "blind credulity" whereby the populace becomes "easily governed" by a "dictator of principles and teacher of unquestionable truths" (J. Locke 1690: I. iv. 25). One of his followers, Lord Chesterfield, even wrote to his son that "A drayman is probably born wyth as good organs as Milton, Locke, or Newton" (Strachey 1924–32, pp. ii–136). True to his time, however, when Locke framed the constitution of Carolina in 1669 he gave slave-owners full jurisdiction over their slaves (Porter 2001, pp. xxii–xxiii). Lockean liberty had its limits.

However that may be, this dual significance helps explain the virulence with which Chomsky attacked behaviourism in general, and Burrhus Skinner in particular. Skinner—who wrote the first book on "verbal behaviour"—not only theorized language in a manner Chomsky saw as scientifically vacuous, but also rejected traditional assumptions about the way to the good life (Skinner 1948) and humanist conceptions of freedom and dignity (Skinner 1971).

These views didn't endear him to the counter-cultural Chomsky (1.iii.c). He attacked them in typically uncompromising terms:

> Consider a well-run concentration camp with inmates spying on one another and the gas ovens [visibly] smoking in the distance, and perhaps an occasional verbal hint as a reminder of the meaning of this reinforcer. It would appear to be an almost perfect world . . . In the delightful culture we have just designed, there should be no aversive consequences . . . Unwanted behavior will be eliminated from the start by the threat of the crematoria and the all-seeing spies . . . Nevertheless, it would be improper to conclude that Skinner is advocating concentration camps and totalitarian rule (though he also offers no objection). Such a conclusion overlooks a fundamental property of Skinner's science, namely, its vacuity. (Chomsky 1973: 129–30)

The familiar vocabulary of freedom and dignity, said Chomsky, far surpasses Skinner's terminology in discussions of politics. Indeed, it's all we have: there's no scientific theory of politics, and perhaps there never can be. In refusing to use such humanistic expressions, therefore, Skinner was being not scientifically scrupulous but politically irresponsible.—Perhaps merely politically neutral? No, Chomsky insisted, for that comes to the same thing: to "offer no objection" is, in effect, to support the status quo.

Later, Chomsky widened this attack to include all quietist academics. He contrasted Albert Einstein's "very comfortable life" of research, interrupted by "a few moments for an occasional oracular statement", with Russell's passionate espousal of nuclear disarmament and vociferous critiques of the Vietnam war, which landed him in prison on more than one occasion (Barsky 1997: 32–3). (Russell was one of the first public critics of the H-bomb, and in 1958 became founding president of the Campaign for Nuclear Disarmament; his pacifist speeches and writings around the time of the First World War had also got him jailed—see Monk 1996: 525–40; 2000: 367–90, 405–13.) In general, Chomsky argued that intellectuals should actively, if perforce riskily, confront "privilege and authority"—both on public platforms and in their personal lives. Certainly, he himself has never stopped doing so.

For the rest of our discussion here, let's ignore the politics. If this brands us as irresponsible quietists, so be it. For Chomsky also gave non-political arguments against behaviourism.

b. That review!

Politics aside, Chomsky made three main claims in his ongoing critique of behaviourism:

* First, that behaviourist theories are in principle inadequate to represent grammatical structure, which exists on several theoretical levels.
* Second, that Skinner—the most influential behaviourist in this area—used covertly mentalistic terms to explain language, while having no theoretical right to do so.
* And third, that human babies, and *only* human babies, can acquire language only because they have innate knowledge of its fundamental structure.

One could coherently admit the first two claims while rejecting the (more controversial) third. Nevertheless, by the end of the 1960s, many cognitive scientists had accepted all three.

The first claim was the theoretical core of Chomsky's dispute with behaviourism, and rests on his argument in *Syntactic Structures* (presented there without explicit reference to behaviourism). The structure and "creativity" of language can't be described by Markov processes, but must be represented as a hierarchy of theoretical levels (see Section vi). The central idea here wasn't new: Karl Lashley had expressed it in the 1940s, in relation to speech and other motor skills (Chapter 5.iv.a). But Chomsky put it in mathematical terms.

He took this criticism to apply to behaviourism in general. As regards those behaviourist theories which (unlike Skinner's) posited internal Ss and Rs, he was justified: mathematically, it doesn't matter whether the Ss and Rs are observable or not. (Compare the objection that production systems are merely an internalist form of behaviourism: Chapter 7.iv.ii.)

Whether he was justified as regards all possible forms of behaviourism was less clear. Some empiricist philosophers argued that behaviourism, or (as they put it) scientific psychology, shouldn't be defined so restrictively as to include only Markovian theories based on conditioned response (e.g. Quine 1969: 96–7). (It turned out, later, that simple statistical methods can enable a connectionist network to learn the past tense, even generating the detailed errors made by children: see Chapter 12.vi.e and x.d.)

The second prong of Chomsky's attack was unsheathed in his notorious review of Skinner's book on the psychology of language (Skinner 1957; Chomsky 1959*b*). This book was published in the same year as *Syntactic Structures*, but had been circulating in draft for some time. It was based on Skinner's William James Lectures, given at Harvard in 1947, and in his own mind represented his crowning achievement. (He'd been working on it for twenty-three years—Skinner 1967: 402.)

Chomsky allowed that in his animal research Skinner had been scrupulous in defining his terms, and had made genuine scientific discoveries. On turning to language, however, he had abandoned both experiment and scruple. His central theoretical concepts of *stimulus*, *response*, and *reinforcement* were being applied so loosely as to be vacuous. And his new, specifically linguistic, concepts (such as *tact* and *mand*), though defined in putatively observational terms, were actually being understood mentalistically (*mand* as an indiscriminate amalgam of *command*, *request*, *question*, *prayer*, *advice*, and *warning*, for instance).

This was hardly surprising, Chomsky said. For if one struggled to interpret Skinner's vocabulary in a strictly behaviourist fashion, his theory instantly lost all plausibility. Moreover, Skinner had made no serious attempt to test his theory of language experimentally, relying instead on anecdote and speculation.

Chomsky did acknowledge that some operant-conditioning experiments on language had come up with surprising and non-trivial results. For instance, speakers can be—unknowingly—conditioned to produce more or fewer plural nouns, to use the personal pronoun more or less often, or to talk about one topic rather than another (Krasner 1958). The first of these is especially interesting, since it's defined in terms not of a single response, like uttering the word *I* or *we*, nor even of a phonemic pattern such as words ending in *-s*, but of a syntactic class. But Chomsky was sceptical: "Just what insight this gives into normal verbal behavior is not obvious" (Chomsky 1959*b*, n. 7).

This review, one-third as long as Chomsky's little book, attracted enormous attention. Its many readers were not only gripped by the argument, which was detailed and careful, but also amused and/or outraged by the rhetorical style. For this essay was very different from the dry and symbol-ridden *Syntactic Structures*.

Chomsky's pen had been dipped in vitriol, as well as in ink. Words such as *pointless*, *confused*, *gross*, *absurd*, *delusion*, *dogmatic*, *arbitrary*, *useless*, and *empty* leapt out from the pages. Skinner's fundamental concept (reinforcement) was scornfully said to have "totally lost whatever objective meaning it may ever have had", and Skinner himself to be "play-acting at science". There was delectable ridicule, too. According to Skinner, said Chomsky, the best way to show one's appreciation of a prized work of art owned by a friend is to shriek "Beautiful!" repeatedly in a loud and high-pitched voice, without ever pausing for breath.

This was welcome light relief from the daily academic grind. Small wonder that the piece was so widely read. Only Skinner avoided it—or so he claimed: "I have never actually read more than half a dozen pages of Chomsky's famous review of *Verbal Behavior*. (A quotation from it which I have used I got from I. A. Richards)" (Skinner 1967: 408).

Rhetoric apart, however, Chomsky's second point was unassailable. Skinner was guilty as charged: he was misusing his own terminology and betraying his own scientific method, by tacitly relying on everyday mentalistic intuitions. In that skirmish, Chomsky won hands down.

c. Nativist notions

Chomsky's third salvo caused even more sound and fury—and is still echoing today (see 7.vi, 12.vi.e, and 14.ix.c–d). More intellectually daring than the first two points, and arguably less well grounded, it was a combination of a negative claim and a positive one.

Negatively: that the child's language acquisition can't be fully explained by experience, even if this is conceptualized in non-Markovian terms. Positively: that babies must therefore have innate knowledge of the structure of language—which is to say, of the rules of some universal grammar.

The positive corollary, that there's some inborn "language acquisition device" specially apt for learning grammatical patterns, was even more obviously heretical than its negative ground. It had been "too audacious" to be included in *Syntactic Structures* (see above), and was merely hinted at in the notorious review, where Chomsky was more concerned to criticize Skinner's theory than to present his own. But it was made fully explicit—and defensively related to his august "Cartesian" precursors—in Chomsky's publications of the 1960s (Chomsky 1964, 1965, 1966, 1968). Indeed, he now built it into the very definition of linguistics as such:

> A theory of linguistic structure that aims for explanatory adequacy incorporates an account of linguistic universals, and it attributes tacit knowledge of these universals to the child. (Chomsky 1965: 27)

> Given a variety of descriptively adequate grammars for natural languages [which can be achieved if one sees the goal of linguistics as I defined it in *Syntactic Structures*], we are interested in determining to what extent they are unique and to what extent there are deep underlying similarities among them that are attributable to the form of language as such. *Real progress in linguistics* consists in the discovery that certain features of given languages can be reduced to universal properties of language, and explained in terms of these deeper aspects of linguistic form. Thus *the major endeavor of the linguist* must be to enrich the theory of linguistic form by formulating more specific constraints and conditions on the notion "generative grammar". (Chomsky 1965:35; italics added)

The negative nativist claim rested partly on Chomsky's anti-Markovian arguments, and partly on his assertion that the language the infant hears is inadequate to allow grammar to be induced so quickly. The child, he said, may hear a certain syntactic construction only very rarely. Moreover, everyday speech abounds with unfinished sentences, restarts, and grammatical errors. (People may get lost inside subordinate clauses, for instance.) So the child very often hears utterances that are grammatically flawed. Given such patchy and contaminated data, rapid learning of a perfect grammar simply couldn't happen.

This negative assertion was immediately criticized by those of an empiricist cast of mind, as being an argument from ignorance. The fact that no one has yet come up with an explanation of how children learn language, they said, doesn't mean that it's impossible to do so. Induction (as remarked above) needn't be conceptualized in the simple terms of traditional empiricism (Quine 1969: 96–7; Harman 1967, 1969). Moreover, the notion of "experience" was said to be richer than Chomsky allowed. It was no longer thought of as passive or atomistic, and with further psychological research might become richer still (M. Black 1970: 458).

This neo-empiricist shaft hit its mark, but left Chomsky standing. Facing only promissory notes from his opponents, his argument from ignorance remained in play. (It was soon followed by a formal proof that 'nested' languages can't be learned on the basis of *positive* evidence only: Gold 1967.)

As for Chomsky's comment that the infant's heard corpus is largely degenerate, this seemed obviously true to many people. But—like Cordemoy's remarks (see Section iii.a) about nouns preceding adjectives, preceding verbs—it hadn't actually been tested.

When it was, psycholinguists reported that mothers typically use a restricted dialect ('motherese') when speaking to babies and young infants, employing sentences both syntactically simpler and more grammatically correct than those of adult speech (R. Brown 1973). On the other hand, they also found that the child's syntax develops in predictable stages, two-word utterances appearing first, and the complexity increasing with age (7.vi.a). This was generally interpreted (perhaps wrongly: see Chapter 12.viii.c–d) as evidence of an innately driven developmental process, supporting the spirit, if not the letter, of Chomsky's nativism.

The positive nativist claim caused huge controversy. Much of the initial scepticism concerned what Chomsky—or indeed anyone—could mean by "innate knowledge" or "innate ideas". All of the questions, and most of the answers, that had been hurled at Descartes, Locke, and Leibniz resurfaced.

Some philosophers objected that innate knowledge is impossible, because "knowledge" implies justification (Edgley 1970; Harman 1969). Chomsky replied that unconscious knowledge (competence), unlike what Gilbert Ryle (1946) had called "knowledge that", requires neither verbalization nor justification (Chomsky 1969; 1980*a*, ch. 3).

Even when the debate was put in terms of innate ideas instead of innate knowledge, or dispositions instead of ideas, the problems sketched in Section ii.c (above) arose—and were debated with passion. High-visibility symposia pitting Chomsky against leading epistemologists and philosophers of language occurred from the mid-1960s on, and innate ideas became a hot topic of scientific and philosophical debate (e.g. *Synthese* 1967; Hook 1969, pt. ii; Stich 1975).

The discussions were often ill-tempered, and many of Chomsky's adversaries rivalled his rhetorical armoury in their attack. One leading philosopher, Gilbert Harman, said that Chomsky "misrepresents [my comments] at almost every point", to which Chomsky testily retorted: "I see no reason to try to trace the various confusions Harman develops, none of which have any relation to the views I actually hold . . . " (Harman 1969: 143; Chomsky 1969: 154). His irritation was doubtless fuelled by the fact that, some years earlier, Harman had denied the need for transformations (Harman 1963). Another influential philosopher dubbed Chomsky's theory 'The Emperor's New Ideas', and remarked:

> The theory of innate ideas is by no means crude. It is of exquisite subtlety, like the gossamer golden cloth made for that ancient emperor. But the emperor needs to be told that his wise men, like his tailors, deceive him; that just as the body covered with the miraculous cloth has nothing on it, the mind packed with innate ideas has nothing in it. (Goodman 1969: 141–2)

Willard Quine, in characteristically gentlemanly fashion, mildly declared that the behaviourist is "knowingly and cheerfully up to his neck in innate mechanisms of learning-readiness" (Quine 1969: 95–6). Reinforcement, for example, depends on "prior inequalities in the subject's qualitative spacing . . . of stimulations". And "unquestionably much additional innate structure is needed, too, to account for language learning".

But there was a sting in the gentleman's tail. Quine ended his paper thus:

> Externalized empiricism [which makes no reference to "ideas"] or behaviorism sees nothing uncongenial in the appeal to innate dispositions to overt behavior, innate readiness for language-learning. What would be interesting and valuable to find out, rather, is just what these endowments are in fact like in detail. (Quine 1969: 98)

Chomsky could answer that question only by saying: universal grammar, whatever that turns out to be.

d. Universal grammar?

What universal grammar will turn out to be—if it exists at all—is still unclear. Indeed, the very notion of *innateness* is now interpreted in a much more sophisticated way (Chapters 7.vi.g and 14.ix.c). Added to that is the fact that Chomsky himself changed his mind more than once about just what the "universal" base of language is.

Chomsky's initial postulation of universal grammar had depended only on argument, the main premiss being the supposed inadequacy of learning theory to explain language acquisition. But, as Quine had said, it needed empirical evidence also.

From 1965 onwards, Chomsky suggested a number of abstract grammatical principles that appeared to apply to several languages and might apply to all. Some concerned aspects of the base component (for example, that all languages share the same syntactic categories: noun, verb, NP, etc.). Others concerned the transformational rules.

For instance, in *Aspects of the Theory of Syntax*, the first major publication in which he made his nativism fully explicit, Chomsky said:

> Although a language might form interrogatives, for example, by interchanging the order of certain categories (as in English), it could not form interrogatives by reflection [reversing the order of words in the sentence], or interchange of odd and even words, or insertion of a [syntactic] marker in the middle of the sentence. (Chomsky 1965: 56)

Later, he posited the *A-over-A* principle. This forbids any language-specific (English, French, or . . .) transformation from picking an embedded noun phrase out of the noun phrase in which it is embedded—or more generally, from extracting any phrase of category *A* out of some larger phrase of the same category. Another example was a "principle of cyclic application" which Chomsky supposed to govern the way in which noun phrases are deleted and replaced by pronouns.

However, each of Chomsky's suggestions was challenged as applying only (if at all) to English, or to a small set of languages. Thorough testing would require detailed descriptions of a multitude of tongues.

This, of course, is what the out-of-fashion structuralists had aimed to provide. But Chomskyans didn't. One critic has described them as being as obsessed with English as the Port-Royalists were with French (Itkonen 1996: 487; cf. Section iii.c). Indeed, Chomsky himself would defiantly declare that "I have not hesitated to propose a general principle of linguistic structure on the basis of a single language"—an admission dubbed "preposterous" by this critic (Itkonen 1996: 487).

Some of Chomsky's "universals" were criticized even with respect to English. The *A-over-A* principle, for instance, was soon decomposed into several independent "island constraints" (J. R. Ross 1968), which covered the data better (and some of which hadn't been covered by the original principle). In the years that followed, Chomsky tried to integrate the island constraints into a single rule. He came up with the "Subjacency Principle", but even this didn't work for all of the island constraints. Again, Chomskyans tried to find some even more abstract principle underlying the variety of surface forms.

But non-Chomskyan linguists weren't persuaded. Worse (for Chomsky), they would eventually accuse him of constantly redefining his position over the years so as to make it, in effect, unfalsifiable. Sometimes, this would be said in waspish exasperation, caused by frustration at the still-continuing dominance of Chomskyans in the linguistic profession (see Section viii.a):

> I [have become] convinced that there is nothing, absolutely *nothing*, that could make Chomskyans admit that there is or has ever been anything amiss with their theory. In the game of linguistics, truth is a secondary consideration: not to lose face has top priority. (Itkonen 1996: 497; cf. pp. 490–4)

On this view, the passage from A-over-A to island constraints wasn't a step forward in a healthy Popperian progression of conjectures and refutations, but one of many intellectual retreats in a degenerating research programme (K. R. Popper 1935; Lakatos 1970). Today, half a century after Chomsky came on the scene, linguists are divided between these two judgements (see Sections viii & ix).

In short, Chomsky's universal grammar of the 1960s was, at best, a promissory note. Twenty years later, and largely due to Chomsky's influence, nativism would be expressed more acceptably and backed by considerable *non-linguistic* evidence (see Chapter 16.iv.c–d and Fodor 1981*b*). Twenty years later still, 'straightforward' nativism would be replaced by epigenesis, and modular theories of the mind reinterpreted, or rejected, accordingly (Chapters 7.vi and 15.x.a–c). Meanwhile, Chomsky couldn't meet Quine's challenge.

It's important to recognize, however, that (whatever one might say about his later theories) Chomsky's early suggestions on this matter had the Popperian merit of being mistaken. They weren't vague claims unamenable to testing—which isn't to say that the Chomskyans were eager to test them.

Still less were they a priori arguments from first principles, with no obvious relation to reality. On the contrary, they were empirical, falsifiable, *scientific* claims. Chomsky was saying "Languages are like *this*—but they could, conceivably, have been like *that*." As he put it:

> There is no *a priori* consideration that lends plausibility to a theory of [universal] syntactic operations [such as mine] that precludes the formation of interrogatives by left–right inversion of the corresponding declarative, or innumerable other perfectly conceivable, and quite efficient operations. It is just this fact that constitutes the scientific interest of such a theory. (Chomsky 1970: 468)

In other words, Chomsky wasn't a rationalist in the strict sense. The Cartesian philosophers of the seventeenth century took language to be close to reason, being informed by principles of necessary truth. And even Kant held that no alternative mental structure (system of intuitions) is conceivable by us (see Section ii.c). But Chomsky shared neither of these beliefs. Whether a language-universal grammar exists, and if so what it is like, are empirical questions.

On those points, Chomsky's behaviourist opponents were willing to agree. But even if he had identified a grammar suited, as a matter of fact, to all natural languages, they wouldn't have conceded defeat. For universality doesn't guarantee innateness. Max Black put the point thus:

> In science in general, reference to dispositions and powers is usually a promissory note, to be cashed by some identifiable internal structure . . . But there is no serious question at present of finding such internal configurations in human organisms: we hardly know what we should be looking for, or where to look. So far as stimulation of research goes, 'nativism' looks to me like a dead end . . . (M. Black 1970: 458)

Black's charge that no interesting research can be prompted by nativism fell wide of the mark. Cognitive scientists excited (whether for or against) by Chomsky's nativism did much interesting work as a result, the many studies of the development of grammar in children being just one example (see Chapter 7.vi). But Black was right to insist that, strictly, the nativist requires some independent evidence of internal mechanisms. Not until the 1990s would the debate on innate ideas, and inborn "dispositions", be illuminated by reference to specific neurological processes (see Chapter 14.vi.b and ix.c).

Not even Chomsky, of course, suggested that babies are born knowing French—or perhaps English, if destined for a life of persuasion, emotionalism, and deceit? (see iii.d, above). But their knowledge of universal grammar, he said, acts as a framework which guides them to attend to certain features, certain distinctions, in the language spoken around them. In syntax as in phonetics, the specific value of those distinctive features varies across natural languages: hence their apparent diversity.

On this view, the child is, in effect, like a scientist. Instead of collecting data by pure induction (which is impossible), the scientist formulates theories and hypotheses which suggest what to look for, and where to look for it. This, essentially, is what the baby has to do in learning its mother tongue.

The view of the speaker/hearer as a hypothesis tester had always been implied by Chomsky's work, quite irrespective of nativism. It was implicit in *Syntactic Structures*, in his claim that the speaker/hearer must ascribe unobservable, many-levelled, grammatical structure to utterances in order to understand them, and was made explicit in the first review (Lees 1957: 406–7). As he himself admitted, his theory that language users generate grammatical hypotheses owed much to Jerome Bruner's "New Look" in perception (6.ii), which swept cognitive psychology in the late 1940s and 1950s (Bruner 1980: 81).

What Chomsky's nativism added (in 1965) to the widely current idea of hypothesis testing was the claim that babies may not have to create their linguistic hypotheses out of thin air, nor even out of infantile dreams and visions. Rather, he said, they can produce them on the basis of a powerful theoretical framework—a "Language Acquisition Device"—already present in their minds.

9.viii. Aftermath

Chomsky's intellectual footsteps were greatly amplified in the two decades after his appearance, and are still clearly audible today.

His early papers had a huge, and lasting, influence on pure computer science. Virtually every introduction to compiler design cites Chomsky's hierarchical classification of languages (Edmund Robinson, personal communication). And a leading textbook on theoretical computer science remarks that "[Chomsky's] notion of a context-free grammar and the corresponding push-down automaton has aided immensely the specification of programming languages" (Hopcroft and Ullman 1979: 9; cf. 217–32). That's why I said (in Section vi.a) that the 'Three Models' paper was his most original and most well-founded contribution.

For cognitive science in general, his crucial writings were the formalist *Syntactic Structures* and the nativist *Aspects*—plus, for light relief, his coruscating review of Skinner. These three publications had a huge impact on psychology and philosophy, and a significant influence on AI (see 6.i.e, 7.vi.a–d, 10.i.g, and 16.iii–iv). In effect, cognitive science in the 1960s had a love affair with Chomsky: one historian of linguistics has remarked on "the sweeping and unrequited optimism of the honeymoon years of cognitive psychology and transformational grammar" (R. A. Harris 1993: 257).

With respect to linguistics as such, the sequel was clear: all linguists came to situate themselves with respect to Chomsky (Lyons 1991: 9). This was true even of those whose specialism was in some other area, such as sociolinguistics, historical linguistics, descriptive linguistics, and anthropological linguistics—all of which, and especially the last two, declined in status post-Chomsky (Sampson 1980: 78 ff., 146–7). All linguists had to decide whether to adopt some version of his account in their own work. And all had to allow that he'd set new standards of clarity for theoretical linguistics.

Whether his many followers lived up to those standards is questionable. Trouble was brewing even in the 1960s. A champion of formalization in science—echoing Leibniz's "calculemus" as the only way of achieving objectivity (see Chapter 2.ix.a)—praised Chomsky for seeding "one of the healthiest and most important controversies in psychology in this century", but complained that his leading disciples couldn't *prove* their claim that behaviourism is in principle inadequate (Suppes 1968). Twenty years later, things were even worse. One critic pointed out that, of the "thousands" of papers published on Chomsky's grammar between 1970 and 1986, *only one* (in a relatively obscure journal) attempted a mathematical statement (Gazdar 1987: 125). Indeed, Chomsky himself would eventually be fiercely criticized for lack of rigour (see Section ix.e).

In addition, he was accused of being mistaken in various ways even about *syntax*, never mind arcane philosophical topics such as innate ideas. This fact should be no great surprise. It's obviously possible to be influential without being right. What is surprising is the extraordinary emotional tone of many of those criticisms. Disinterested discussion of Chomsky's views was—and still is—hard to come by, as we'll now see.

a. Polarized passions

For linguists who specialized in syntax, semantics, or phonetics, Chomsky soon became a constant presence. Too much so, perhaps: the sociology of science hindered the science itself, as Chomskyan cliques came to dominate various conferences and journals.

Bruner, who as a non-linguist has no professional need to 'take sides' (and who'd influenced Chomsky deeply in the late 1950s: see vii.d, above), is one of many who've complained about this. He recently remarked:

> [Chomsky is a] systems builder, and ruthless systems at that. And it's so funny, because the fact of the matter is he claims to be a deep believer in democratic values. But the ways in which he goes about it, and the ways in which they, the Chomskians, have gone about it by a kind of taking over of the apparatus of linguistic scholarship in America is nothing less than just a hostile takeover of the whole damned system. Everybody else is out. (Shore 2004: 51)

Naturally, this phenomenon didn't go unchallenged within the profession. Eventually, linguists' complaints about Chomsky's hegemony in linguistics became even more heated than philosophers' debates about his theory of innate ideas (see Section vii.c). One observer recently remarked that "the level of enmity is truly stunning". As he put it, "there are people who genuinely wish that Chomsky would die, or retire, or move exclusively into political or philosophical domains, and just leave poor little linguists alone" (R. A. Harris 1993: 256).

This "blood-boiling animosity" arose from professional frustration. An English linguist surveying the international scene in 1980 saw it like this:

> The discipline of linguistics seems to be peopled by intellectual Brahmanists, who evaluate ideas in terms of ancestry rather than intrinsic worth; and, nowadays, the proper caste to belong to is American. The most half-baked idea from MIT is taken seriously, even if it has been anticipated by far more solid work done in the "wrong" places; the latter is not rejected, just ignored . . . To the young English scholar of today, the dignified print and decent bindings of the *Transactions of the Philological Society* smack of genteel, leather-elbow-patched poverty and nostalgia for vanished glories on the North-West Frontier, while blurred stencils hot from the presses of the Indiana University Linguistics Club are invested with all the authority of the Apollo Programme and the billion-dollar economy. Against such powerful magic, mere common sense . . . and meticulous scholarship (in which the London School [e.g. Firth, Halliday, Hudson] compares favourably, to say the least, with the movement that has eclipsed it) are considerations that seem to count for disappointingly little. (Sampson 1980: 235)

Comparable complaints have been made more recently, if less colourfully. A Finnish linguist in the mid-1990s remarked that Chomskyans often identify "linguistics" with "generative linguistics", and that "there are nowadays relatively few publications which subject generative linguistics to explicit critical scrutiny". The lack of publication was attributed to sociological factors:

> A lengthy discussion [on the Internet] concerning "mainstream linguistics" . . . was started by the observation that people who had to criticize the generative paradigm preferred to stay anonymous. This was interpreted as reflecting the opinion that open criticism of "mainstream linguistics" might jeopardize a person's career prospects and possibilities for publication. Although such a view was heatedly denied by representatives of the generative paradigm, at least my own experience confirms it. In several informal meetings I have found out that linguists agree with some or even all aspects of the criticism [that I am about to present]; they just do not want to say it publicly. (Itkonen 1996: 471)

In a nutshell, the "representatives of other schools seem anxious to maintain what might, depending on one's point of view, be called either 'peaceful coexistence' or a 'balance of terror' " (p. 471).

Similarly warlike language was occasionally heard on Chomsky's side of the Atlantic, too. One leading American linguist, a colleague of Chomsky's at MIT in the late 1950s and 1960s (see Section x.b), had lost patience with him by the 1980s. Victor Yngve (1920–) , arguing that linguistics should focus on people (communicators) rather than language, referred to "puffery, empty polemics, intellectual bullying, and the great-man syndrome", and bemoaned "unscientific" a priori reasoning "in support of philosophical preconceptions and maintained by early ecstatic reviews, political acceptance, largely unquestioned personal authority, and the abuse and contempt . . . poured on other positions" (Yngve 1986: 7, 43, 108). Chomsky's name wasn't mentioned. Clearly, however, the cap fitted and he was being asked to wear it.

In short, Chomsky was the high priest of a new orthodoxy, almost a new paradigm (Kuhn 1962). Most young linguists aspired to be his devoted acolytes. Heretics (if noticed at all) weren't treated lightly—a situation due largely to Chomsky's personality, which thrives on embattlement.

For example, the MIT linguist who defined island constraints (see Section vii.d) was systematically reviled and excluded by his so-called colleagues (R. A. Harris 1993: 245–6). And George Lakoff, a pioneer of generative semantics (see below), was venomously accused by Chomsky himself of (among other things) having "discussed views that do not exist on issues that have not been raised, confused beyond recognition the issues that have been raised and severely distorted the contents of virtually every source he cites" (quoted in R. A. Harris 1993: 157). Skinner, then, wasn't the last to be lacerated by Chomsky's pen.

Nevertheless, heretics there were. Not everyone agreed with him. Indeed, even Chomsky disagreed with Chomsky. In other words, he had second thoughts—and third, and fourth . . . and more. Before considering his linguistic critics (in Section ix), let's look briefly at his own revisions of his ground-breaking theory of 1957.

b. Revisions, revisions . . .

Revisions weren't slow to come, for the grammar of *Aspects* differed significantly from Chomsky's earlier approach. The main differences were its terminology of deep and surface structure, its new treatment of transformations, and its inclusion of semantics. (Besides the new treatment of *grammar*, there was also a new emphasis on nativism, as we've seen.)

The 1965 account—known as "standard" theory—lasted, with relatively minor revisions, for fifteen years, while Chomsky devoted much of his time to politics. These revisions reduced the number of transformations in his theory: in 1970, for example, he suggested that lexical rules should replace morphological transformations (Bresnan 1978: 5). But the revised standard theory wasn't his last gasp. In the early 1980s, he provided a new approach, the theory of "government-binding" (GB) (Chomsky 1980*a*,*b*, 1982).

GB theory was even more unlike *Aspects* than that had been unlike *Syntactic Structures*. Chomsky had already suggested, in the late 1970s, that what had previously been thought of as a number of different transformations (involving wh-questions and various other matters) could be seen as special cases of a more general one. By the early 1980s, only a single, highly general, transformation remained. (Known as "move alpha", its pivotal notion was subjacency.) Much of the explanatory weight was now carried by innate "principles" and "parameters" (Chomsky and Lasnik 1977).

Principles are unvarying linguistic universals. Parameters are like variables: each has a limited number of possible values, which are partly interdependent. The diversity of actual languages is explained by differences between their sets of parameter values. So infants, in acquiring language, must discover which values characterize their mother tongue, and set their internal parameters accordingly (Lyons 1991: 184–8). As Chomsky put it:

> If the system of universal grammar is sufficiently rich, then limited evidence will suffice for the development of rich and complex systems in the mind, and a small change in parameters may lead to what appears to be a radical change in the resulting system. (Chomsky 1980*a*: 66)

This theory is reminiscent of claims made by biologists who see the range of possible adult forms as highly constrained, yet different, with no "great chain of being" filling the gaps. Conrad Waddington, and his student Brian Goodwin, have argued that small changes in epigenesis can tip the embryo into one developmental pathway rather than another: see Chapters 14.ix.c and 15.ix.a–c.

Eventually, in the early 1990s, GB theory gave place to "minimalism" (Chomsky 1993, 1995). "Deep structure" was abandoned. Still more shockingly, the previously pivotal notion of grammaticality was dismissed as being "without characterization or known empirical justification" (Chomsky 1993: 44). And several new principles—including Greed, Procrastinate, and Last Resort—were introduced (Chomsky 1995: 200–12).

But minimalism was too much for some of Chomsky's critics. One reviewer remarked that it was so very different from its GB predecessor that Chomsky was here "contriving to reclaim the role of the lone revolutionary", without acknowledging that his "new" position owed much to the generative semanticists of twenty years before (see below)—Pullum 1996: 137. As for the core theoretical ideas of minimalism, these were "sketched so hesitantly as to be hard to describe", and afforded "a level of explication [that] is risible" (Pullum 1996: 140–1). He mocked the new principles of "Greed" and the like, and complained about "anthropomorphic" accounts of "phrases moving in a bid to get their needs satisfied, and abstract nodes yearning to discharge their feature burdens" (Pullum 1996: 142). Minimalism, this critic declared, involved "a complete collapse in standards of scientific talk about natural language syntax".

These heretical judgements cut no ice with Chomsky's followers. The mantle of intellectual guru had settled firmly on his shoulders in the 1960s, and sits there comfortably still. Late in 1998, for example, his theory of minimalism was reported as a quarter-page news item—not a book review, nor even a feature article—in a British national newspaper. This largely explains his reviewer's ill-tempered use of vocabulary such as "risible".

In truth, the rot had set in long before minimalism. Chomsky's own standards of rigour had always been much lower than was generally believed. This fact, which became clear (to those with eyes to see) in the mid-1970s, is important for cognitive science in general, and is discussed in Section ix.e.

The many theoretical changes in Chomsky's later writings, whether rigorous or not, don't concern us. The huge impact of Chomsky's work on cognitive science resulted from his first three books, and was already integral to the field by the 1980s.

However, two of Chomsky's post-1957 theory changes do have a wider relevance, so merit mention here. The first was his increasing stress on "modularity". His style of linguistic nativism always implied that the language "faculty"—or, as he also says, "organ"—is a self-contained part of the mind, functioning in independence of others. Over the years, he made this more explicit (now, he analyses the language faculty itself into distinct sub-modules: Chomsky 1986). Moreover, he speculatively generalized it to other mental "faculties" (Chomsky 1980*a*).

The idea of modularity didn't originate with Chomsky. Conceptualized as "hierarchy", it had already been stressed by Simon, in his widely read discussion of the design (and evolution) of complex computational systems (Simon 1962/9). Chomsky's championing of it influenced (and drew support from) research in the psychology of vision and developmental psychology (Chapter 7.v and vi); ran in parallel with the incorporation of expertise (as opposed to general reasoning methods) in AI (see Chapter 10.iv.b–c); and inspired an influential philosophy of mind developed by Fodor (16.iv.c–d).

Nevertheless, the general relevance of modularity is controversial (see Chapter 7.vi.d–i). It has been challenged, for instance, by wide-ranging research in developmental psychology (Karmiloff-Smith 1992). And Simon's arguments about the evolutionary necessity of modularity have recently been questioned by the philosopher Tadeusz Zawidzki (1998). He argues that Simon's desideratum that evolution pass through "stable intermediate stages" is met by Stuart Kauffman's models of genetic regulation (see 15.ix.b), which are *not* decomposable into separate, hierarchical, parts.

The second broadly influential change was that Chomsky included semantics in *Aspects*, having previously ignored it.

c. Semantics enters the equation

By introducing semantics into the discussion, Chomsky was trying (for example) to account clearly for the deviance of sentences like "Colourless green ideas sleep furiously", and to decide whether they should be regarded as truly grammatical.

This word string *seems* to be grammatical: two adjectives and a noun, making a nice NP, and a verb and adverb providing the necessary VP. From the syntactic point of view, then, it appears to be fine. (Some people even claimed to be able to give it a meaningful interpretation.) Nevertheless, there does seem to be something wrong with it—but what, exactly?

To deal with that question, among others, Chomsky now (in *Aspects*) said that any generative grammar must have both a syntactic and a semantic component. And he regarded the latter as "purely interpretive". That is, each deep-structure (syntactic) expression is sent to a semantic rule system, which gives it a formal description in terms of (universal) semantic primitives. These include the distinction between mass and count nouns, and between human, animate, inanimate, or abstract things.

Because the semantic component was supposed to be only interpretative, "it follows that all [this] information . . . must be presented in the syntactic component of the grammar" (Chomsky 1965: 75). But there is no a priori answer, he said, to the question whether—and if so, to what extent—it should influence the generation of syntactic structures. He did include various semantics-based "selectional rules" restricting the structures that his syntax could generate. But he pointed out that these could be transferred to the semantic component; conversely, all of the semantic interpretative rules might be applied before some of the syntactic generative rules, "so that the distinction between the two components is, in effect, obliterated" (Chomsky 1965: 158–9). In short, Chomsky regarded questions about the relation of syntax to semantics as a matter of judgement, not principle.

His close MIT colleagues Jerrold Katz and Fodor (1963) agreed that syntax and semantics are to some extent intertwined. Nonetheless, they insisted that we need a specifically semantic theory to describe and explain the role of meaning in language. Moreover, they said, such a theory would contribute to (and draw on) the psychology of language. Our ability to understand the meanings of novel sentences must depend on the meanings of the constituent words (morphemes).

This implies, they argued, that we need a semantic "dictionary" of morphemes, and also an understanding of the principles by which meanings can be sensibly combined. A dictionary and grammar alone cannot explain understanding. The prime reason for this is that many words are multiply ambiguous (*bank*, *bill*, *seal*, *bachelor*), and the dictionary must include all the possible senses. Syntax cannot always do the disambiguating for us: *He banked the bill* isn't ambiguous, but *They were approaching the bank* is. In addition, then, we need "projection rules" for using the dictionary and grammar, which take account of the meaning relations between morphemes in generating semantic interpretations of sentences.

The burgeoning interest in semantics took many forms. One of these was "generative semantics". This label, originated by Lakoff in 1963 (before the publication of *Aspects*), was attached to a number of theories in the late 1960s to mid-1970s (Dean Fodor 1977; R. A. Harris 1993, chs. 5 and 6; McCawley 1995). The single label obscured various differences, both theoretical and political.

Some generative semanticists criticized Chomsky's reliance on mathematical models, as opposed to detailed linguistic data. Some even saw Chomskyans as "scientific Calvinists" out of touch with the anti-elitist counter-culture of the 1960s (McCawley 1995: 344–5). Politics aside, the charge of mathematical affluence combined with data poverty would be levelled by many other critics too. But those generative semanticists who did focus on mathematical models argued that the basic generative system was semantically driven and interpreted by syntactic rules, rather than the other way around (see above).

It was as though they were saying "Meaning first, syntax afterwards"—which, at least on first hearing, is intuitively plausible. However, after "some years of involved and at times acrimonious argument", it was eventually agreed that there was less difference between the two positions than had been supposed (Lyons 1991: 93). Both "generative" and "interpretative" semantics concerned (timeless) linguistic competence, not performance. Giving the generativity to the syntax or to the semantics made little difference in the types of sentence that could be (non-procedurally) accounted for.

In general, a semantic theory should give us a principled account of synonymy, and explain the (non-syntactic) ambiguity of *The bill is large*, and its resolution in *The bill is large but need not be paid*. Katz and Fodor assumed that concepts are analysable into distinct semantic components, and posited semantic primitives universal to all natural languages—and all human minds. They explained synonymy in terms of shared entries in a semantic dictionary, and paraphrase in terms of shared semantic structure between sentences that might be lexically and syntactically very different (*The oculist examined me/I was inspected by the eye-doctor*). Different senses of one word (such as *bachelor*) falling in the same syntactic category (*noun*) were identified by differing "semantic markers" (primitives such as *Human*, *Animal*, *male*) and "distinguishers" (for instance, *never married*, *young knight*, *first academic degree*, *young fur seal*).

The componential analysis of meaning is a tenet of classical empiricism—found in Locke, for instance. But there are difficulties about how to identify semantic primitives (for example, how to differentiate Katz–Fodor markers from distinguishers—Dean Fodor 1977: 144–55). Worse, it's not obvious that the componential assumption is true. Even in apparently simple cases, such as the concept of *cousin*, it's not clear what the analysis should be. If one uses the 'obvious' constituents *mother*, *father*, *son*, *daughter* (and perhaps *brother* and *sister*), one can't represent someone's being a cousin without committing oneself on the sex of the various people involved (unless disjunctions are included). This problem disappears if one analyses in terms of *parent*, *child*, *sibling*, *male*, *female*; but then one can accept only blood cousins, not cousins by marriage (which would require *spouse*). Moreover, suppose the word *cousin* actually was used in this restrictive way: so what? Someone who creatively used it (in an appropriate context) to include cousins by marriage might raise some eyebrows, but would nevertheless be understood. This example illustrates what the philosopher Friedrich Waismann (1955) had called the "open texture" of language.

Scepticism about componential analysis and the identification of semantic primitives arose in linguistics, philosophy, and AI (e.g. Fodor 1970; Kempson 1977; Wittgenstein 1953: 31 ff.; Wilks 1978; Sampson 1979*b*). It was one reason why generative semantics went out of fashion. Fodor, for instance, abandoned his "primitivist" account, and later suggested—following the psychologist Eleanor Rosch (1978—see Chapter 8.i.b)—that certain psychologically salient concepts may be the semantic focus of language (Fodor 1983).

Besides these criticisms, however, a very different account of semantics—an account based in mathematical logic—began to gain ground by the mid-1970s (Dowty *et al.* 1981). Componential analysis, in the most general sense, was retained. But the components were now conceptualized as abstract ontological entities and mathematical relations, not as familiar notions such as *Human*, *Animate*, *parent*, *child*, or *male*. The assumption that the distinction between syntax and semantics is a matter of judgement and emphasis was roundly rejected. Moreover, the psychologism of Chomsky and his followers was rejected too.

In short, the Chomskyan paradigm was being challenged. This approach to semantics is outlined in the next section, along with some other important anti-Chomskyan work. Some of this would cast doubt not only on his *grammar*, but on his position on cognitive science in general.

9.ix. Challenging the Master

Some of the new theories challenging Chomsky would undermine only his linguistics, strictly so called. People not especially enamoured of NPs and VPs might not be much interested. Indeed, they might be downright bored.

But other challenges would also reject his close juxtaposition of language and mind, with its supposed implications for both philosophy and psychology. And one would drive a coach and horses through his claim that phrase-structure grammars are inadequate—so hugely improving the power of computer models of language.

In this section, I'll highlight these points of general interest, rather than concentrating on strictly linguistic nit-picking. Some nit-picking, however, there will have to be.

a. Linguistic wars

The grammarians who disagreed with Chomsky did so for a variety of reasons. Accordingly, the "linguistic wars" of the 1970s and early 1980s involved many different skirmishes (R. A. Harris 1993). One linguist in the mid-1980s identified forty-two competing grammars by name, ominously adding "among others" at the end of the list (Yngve 1986: 108).

Broadly speaking, the non-Chomskyan combatants fell into two armies. On the one hand, there were people who, perhaps even before Chomsky's first publications, took a fundamentally different approach to syntax. They might be spurred to greater clarity by his highly formal example. But they didn't accept his theory—or even his goal.

The London School, for example, didn't aim at a generative grammar from which all possible sentences could be derived (Sampson 1980: 212–35). Founded in the 1920s by J. R. Firth (1890–1960), these linguists focused on the functional properties of language as communication.

Where grammar was concerned, they asked how syntactically different types of sentence relate to different communicative contexts. They were much less interested in (Chomskyan) questions concerning word-by-word derivations of individual sentences, or decisions about whether a particular word string is grammatical. Like the Port-Royalists, they saw grammar as a communicative system that happens to be formalizable. Chomsky, by contrast, saw grammar as a formal system that happens to be used for communication. (His prioritizing of formal syntax was criticized also by philosophers working on speech acts, whose research dealt with issues—of meaning and speaker's intention—he left untouched; Searle 1972.)

Michael Halliday (1925–) was one of the London group. A specialist in Chinese, he had an early interest in machine translation, and was associated with Richard Richens and the Cambridge Language Research Unit, or CLRU (see Preface, ii). But his main contribution to linguistics was "systemic" grammar—later used in one of the first impressive automatic parsers (see Section xi.b).

Halliday classified sentences into a limited set of syntactic types. The speaker has to choose one of these, in deciding how to communicate what they want to say. Each type was defined in terms of the syntactic "features" included in it, and each feature was chosen from a feature set of mutually exclusive alternatives. The features were largely interdependent (hence the name, systemic grammar): choice of a feature not only ruled out its immediate alternatives, but also constrained the allowable feature choices from other feature sets.

Whereas the speaker must make the choices, the hearer uses them to aid interpretation. (An example of how syntax can help understanding is discussed in Section xi.c.) And both speaker and hearer are guided by context: if one syntactic type is more appropriate in context than another, then it will (or should) be chosen. Some linguists applied Halliday's approach to practical problems of language teaching, rhetoric, and literary criticism—hardly grist to the Chomskyan mill.

On the other hand, there were those who followed Chomsky onto his own ground and challenged him there. Unlike the London School, whose general approach pre-dated Chomsky, these linguists couldn't have done their work without him. They agreed that the goal of linguistics is to find some generative grammar—but not necessarily his.

Many merely quibbled about this transformation or that one. Someone could do that, yet still be playing on Chomsky's team. Others risked the charge of heresy by offering a very different transformational grammar.

But some went much further. They excommunicated themselves, by rejecting transformations altogether. And some of these, in turn, criticized Chomskyans' mathematical claims—and even Chomsky's formal skills—in uncompromising, often contemptuous, terms.

b. Who needs transformations?

Chomsky had argued that context-free phrase-structure (CFPS) grammars can't generate natural language, so must be supplemented by transformations. This strategy was theoretically inelegant, especially when—as in the early versions of Chomsky's linguistics—a long list of transformations was involved. But, for a quarter of a century, it was widely assumed to be unavoidable.

Some murmurings, however, were afoot. Even before *Aspects*, Harman had outlined a generative grammar without transformations (see Section vii.c). And the suspicious fact—first pointed out in 1961 (see Section vi.d)—that the class of transformational grammars is unconstrained, being equivalent to a Turing machine, was clarified a decade later by the Stanford logician Stanley Peters (1941–)—Peters and Ritchie (1973). Chomsky himself started to rely less on transformations (see Section viii.b). Eventually, then, some theorists stopped asking which transformations are needed, and asked instead whether they're needed at all.

One person who posed this question was Joan Bresnan (1945–), initially a student of Chomsky's at MIT. Bresnan's linguistics was clearly an example of cognitive science, and was highly influential at Stanford's (early 1980s) interdisciplinary Center for the Study of Language and Information (CSLI). Together with the computational linguist Ronald Kaplan (1946–) she developed "lexical-functional grammar", or LFG—so named because it stressed lexical rules for word morphology, and functional rules governing relationships such as *agent* and *passive* (Bresnan 1978; Bresnan and Kaplan 1982).

These two types of rule replaced some of the transformations defined in *Aspects*. Bresnan still felt some structural transformations to be necessary, and sometimes described her grammar as "transformational" accordingly (Bresnan 1978: 36–40). But she conceptualized them procedurally as online operations on a single string, not as changes in one (deep) string to turn it into another (surface) one. That is, transformations in the literal sense had disappeared.

This was no accident, for the main motivation underlying LFG was to avoid altering already built structural descriptions during the derivation or parsing of a sentence. Bresnan and Kaplan saw this strategy as computationally clumsy and psychologically implausible. Instead of (sequentially ordered) transformations, LFG provided several levels of description that apply simultaneously. In generating a sentence structure, each partial description could only add to, not cause changes in, the partial descriptions already accepted on other levels.

Bresnan took Chomsky's psychologism even further than he had done. In accordance with his structuralist training, Chomsky had developed his grammar first and only then suggested that it had psychological implications. Moreover, when psycholinguistic evidence went against his theory, he retreated into grammar as such: "[my] generative grammar does not, in itself, prescribe the character or functioning of a perceptual model or a model of speech-production" (Chomsky 1965: 9). But LFG was conceptualized from the beginning as the ground of detailed psycholinguistic hypotheses. As Bresnan put it:

> [A realistic grammar requires that we] define for it explicit realization mappings to psychological models of language use. These realizations should map distinct grammatical rules and units into distinct processing operations and informational units in such a way that different rule types of the grammar are associated with different processing functions. (Bresnan 1978: 3)

A realistic grammar, she also said, should be tested by computational methods, as well as by psychological experiments. A prime reason for her collaboration with Kaplan, who also saw grammars as "mental representations of language", was that he'd written computer programs—using ATNs (see Section xi.b)—intended to simulate what goes on in people's minds when they understand a sentence (R. M. Kaplan 1972, 1975). Similar models, and a related programme of psycholinguistic studies, were developed by two of Kaplan's co-researchers—who showed, for instance, how relative clauses could be parsed without backtracking (Wanner and Maratsos 1978; Boden 1988: 100 ff.).

Some years later, the computational linguist Martin Kay (a one-time member of CLRU: see Preface, ii) located Bresnan's work—and that of others discussed below—within the overall space of possible human parsing strategies (M. Kay 1980). He argued that the psycholinguist needn't be strictly constrained by considerations of computational elegance, still less by a devotion to step-by-step (sequential) algorithms. There are many reasons for thinking that people process language in parallel, in the sense that a number of different parsing strategies are at work at the same time, and are somehow integrated in generating the completed parse tree. And there are reasons for thinking that both top-down and bottom-up methods may be involved.

Each point in Kay's abstract space defined an *algorithm schema*, identifying a function to be computed but not saying just how this should be done. Actual algorithms could be generated in a principled way by combining a schema with an *agenda* listing various psychological strategies. These could direct both depth-first and breadth-first search (see Chapter 10.i.d). In addition, Kay discussed in general terms how parsers (using *charts*) could be constructed to avoid unnecessary duplication of effort, such as repeating prior computations during backtracking. When computer parsers proliferated in the 1980s and 1990s, this seminal attempt to see the wood as well as the trees was to be highly influential.

c. Montagovian meanings

Whereas Bresnan had insisted that grammar and psychology should be very closely related, some others who challenged Chomsky's account of language made a point of avoiding that claim. Richard Montague (1930–71) at UCLA was an important example (Dowty *et al.* 1981).

Montague was a logician and philosopher of language, working in the strongly anti-psychologist tradition of Frege (see Chapter 2.ix.b), and inspired by Carnap rather than Chomsky. His work wasn't an instance of cognitive science. He had no interest in the mind or mental processes, nor in computational linguistics. And his theoretical concepts were drawn not from computer science, but from mathematical logic. However, his approach appealed to some linguists whose views were highly relevant to cognitive science, as we'll see. In addition, it was taken up by a highly influential psychologist working on language and reasoning (Chapter 7.iv.e).

His influential paper on "universal grammar" had nothing to say about the relation between language and mind, still less innate ideas (Montague 1970). By invoking a universal grammar he meant, rather, that all languages, both natural and artificial, should be described in the same way. In this belief, he was echoing Carnap (see Section v.a). What was novel about his approach, and what commended it to some linguists, was the unusually close theoretical relation between syntax and semantics.

His system was called "intensional logic", because it dealt with meanings as well as logical formulae. It contradicted Chomskyan assumptions about the autonomy of syntax. It also rejected all semantic theories that merely offered translation into a more basic language—whether this was some form of logic, or a language of semantic primitives. For Montague, to translate one language into another isn't to address the central problem. The crucial questions of semantics concern the relation between linguistic expressions (of whatever kind) and entities in the world.

Logicians had long defined semantics in terms of truth conditions: what would have to be the case in the world for the expression to be true. And they had long assumed compositionality, so that the truth conditions of a complex expression depend recursively on those of its component parts. So far, so good—but just how were truth conditions to be conceptualized?

The logical positivists had answered this question in terms of (pure) observation protocols, but by mid-century it had become clear that this approach had intractable problems. In the 1960s (though not published until after his death in 1970), Montague developed a more abstract answer, and applied it to various types of sentence in English (Montague 1970, 1973).

Criticizing Chomskyan grammar as mathematically imprecise and unsystematic, he went back to Russell's set theory. With this as his ground, he defined "the set of all possible worlds" in terms of an abstract ontology of individuals, properties, *n*-ary relations etc. The semantics of a linguistic expression was then defined as its denotation (its model) in some possible world. That model was found by applying recursive mapping rules, such that each syntactic form (NP, VP, Det, etc.) had a corresponding semantic form. A proper name, for instance, always maps onto some individual.

This 'rule-for-rule' approach coupled syntax and semantics much more closely, precisely, and systematically than Chomsky and his followers had done. The theory of semantic markers, for example, had posited "primitives" such as *Animate* and *Human* (see Section viii.c). But to capitalize a familiar term of English isn't to display its meaning. Moreover, it wasn't clear just how to interpret the bracketed lists of semantic primitives offered by such theories: their syntax didn't establish their meaning. They were understood intuitively, much as the higher-level words (such as *bachelor*) were. In Montague's hands, by contrast, some specific—albeit highly abstract—meaning was definitively implied by the syntax.

Montague's theory was mathematically elegant, but couldn't solve most of the problems about meaning that had long plagued linguistics. He held, for example, that if a rule-for-rule mapping of a given English sentence succeeds in all possible worlds, it is necessarily true—and semantically equivalent to every other such sentence. Yet the component words of these distinct sentences may arouse very different associations in the hearer's mind. If, with Humboldt (or even Bloomfield), one regards these as part of the "meaning" of language, then Montague's theory is inadequate.

Again, semantic relationships such as synonymy, implication, sub/superordinacy, and consistency/inconsistency were rigorously defined by Montague. Synonymy, for instance, requires that both expressions be true in the same set of possible worlds. However, this abstract definition may not help us to decide whether two actual words are synonymous. (Montague agreed with Humboldt that accurate translation from one natural language to another is impossible—not because of different mental associations, but because it's unlikely that any two sentences in the different languages will share the same mapping in model theory.) Nor did his approach help in defining *cousin*: all the problems noted in Section viii.c, including the open texture of natural language, remained.

The new semantics offered clear definitions for everyday words such as *the*, *every*, *some*, *all*, *any*, *never*, *ever*, all of which are notoriously difficult to define in dictionary style. They were interpreted by Montague as set-theoretic expressions defined over possible worlds, whose logical–mathematical relations could be precisely stated.

However, most of the long-standing problems about assimilating natural language to logic had been brushed aside, rather than solved (see Chapter 4.iii.c). As for sentences about propositional attitudes, these were as intractable as ever (Dowty *et al.* 1981: 170–5). No semantic mapping for *Mary believed that the bracelet had been lost* could be completed, because the bracelet's fate still had to be left open (see Section v.a).

Model-theoretic semantics was a further step along the road of formalism pioneered by Frege, Russell, and the early Wittgenstein. This road had been followed also by Carnap and Reichenbach. But whereas in the 1930s–1940s few linguists had paid much attention to Carnap's work on language, by the 1970s linguistics itself was largely formalized, and had been strongly linked (by Chomsky) with the analysis of artificial languages. In other words, the ground had been prepared for Montague.

Even so, persuading linguists to read Montague took some time and effort. His work was so fiercely abstract, and so tersely written, as to border on unintelligibility. Most linguists tackled it by way of some introductory account, preferably one relating set theory (more familiar to philosophers of language than to linguists) to transformational grammar. Barbara Partee (1975) was especially influential in introducing Montague's ideas to other linguists. But even four years later, professional journals were still prepared to publish explanatory notes on Montague rather than original research, defending their decision by reference to his forbidding writing style (Halvorsen and Ladusaw 1979: 185).

After the mid-1970s, when (thanks to Partee) linguists in general became aware of him, Montague's work soon came to dominate theoretical semantics, and various theories of syntax too. (Ironically, Chomsky himself now puts much less stress on formalization than he used to, and even queries the relevance of "most of the results of mathematical linguistics"—Lyons 1991: 199.)

Psycholinguists, however, virtually ignored Montague. (Although some would take up his theories in the early 1980s: see 7.iv.e.) Being ignored by the psycholinguists wouldn't have bothered him at all. As remarked above, his theory was a purely logical–ontological one, making no claims about psychology.

d. Transformations trounced

Among the linguists inspired by Montague in the mid-1970s was Gerald Gazdar (1950–), of the University of Sussex. Together with Ewan Klein, Geoffrey Pullum, and Ivan Sag, Gazdar developed a new syntactic theory: generalized phrase-structure grammar, or GPSG (Gazdar 1979*a*, 1981*a*, 1982; Gazdar *et al.* 1985). GPSG adopted Montague's model-theoretic approach, and applied it to a much wider range of English syntax than he had done.

Like Montague, Gazdar treated language as a purely formal system, not as a psychological phenomenon. Nevertheless, he was inspired also by the psychologically oriented Bresnan. What drew him to Bresnan's work wasn't her use of grammar to relate language and mind, but her rejection of syntactic transformations.

Another formative influence was Peters—who in 1973 had expanded Putnam's insight about the Turing equivalence of transformations in general (see above). Gazdar took this mathematical discussion still further. Besides co-developing GPSG, he directly challenged some of Chomsky's formal claims about the power of various types of grammar. In so doing, he implicitly cast doubt on arguments that had influenced cognitive science in general (see Section ix.e–f).

In formulating GPSG, the prime aim of Gazdar and his associates was to develop a single-level (non-transformational) tree-based grammar that would describe English elegantly. That task could be, and often is, approached without any concern for the abstract mathematical properties of language. But two of the authors (Gazdar and Pullum) had already claimed that the widely accepted arguments against CFPS grammars were either formally invalid or based on false empirical premisses. Indeed, their work had revived interest in this long-neglected question, and led to some improved arguments for the opposing view (G. Gazdar, personal communication).

Partly because of this interest (which not all syntacticians shared), and partly as a matter of methodological discipline, the GPSG group took pains to prove that their new account of syntax was mathematically equivalent to a CFPS grammar.

The GPSG authors were careful not to claim that GPSG can describe every natural language, although they pointed out that it seemed to fit about twenty diverse tongues (Gazdar *et al.* 1985: 15). But they did assume that it could describe the whole of English.

Soon afterwards, it became clear that this was almost true, but not quite (see below). Nevertheless, it was evident by the early 1980s that GPSG could successfully describe a host of English sentences which Chomsky had said require appeal to transformations. Moreover, no extra rules had to be added to link each syntactic form with its semantic interpretation. These relations were transparent, by virtue of a model-theoretic account of the mapping between syntax and semantics.

GPSG captured the grammatical intuition noted in Section vi.d, that *The man reads the book*, *The book is read by the man*, and so on, are closely related. Nevertheless, it considered only the surface string, so didn't need transformations (which were introduced to derive surface-level from deep-level structures). It specified only a set of phrase-structure rules. GPSG was more flexible than the phrase-structure grammars considered by Chomsky, because in addition to the familiar syntactic categories (S, NP, VP, Det, and so on) it contained three other formal devices: slash-categories, metarules, and linking rules.

A slash-category marks a constituent with a syntactic gap: so S/NP, for instance, represents a sentence with a noun phrase missing. As this example shows, the nature of the gap was defined in terms of the basic (and familiar) phrase-structure rules. Given slash-categories, the problem arose of defining how the grammar should deal with them. Consider S/NP: this must denote either *an NP-missing-an-NP followed by a VP*, or *an NP followed by a VP-missing-an-NP*. These are the only possible interpretations, given that the basic grammar prescribes that S $\rightarrow$ NP + VP. Other slash-categories may allow more than two possibilities.

One might have expected, therefore, that GPSG would include an inelegant profusion of extra rules, several for each slash-category. Not so: instead, Gazdar's grammar contained a small number of metarules. Each of these was a general rule schema, from which (together with the basic rules) the new rules required for a given slash-category could be derived. Finally, the (more numerous) schematic linking rules dealt with the housekeeping: they defined how to introduce and delete slash-categories, and enabled the gap to be carried down to lower levels of the parsing tree as necessary.

This new grammar not only dealt with all the sentences generated by transformational grammar, but also explained some things simply which Chomsky could explain only in tortuous ways. For instance, consider these two sentences: (1) I think *Fido destroyed the kennel*, and (2) The kennel, I think *Fido destroyed*. Most linguists (including Chomsky) had assumed that the two italicized expressions fall into the same syntactic category—namely, a (subordinated) sentence. But because the second expression isn't a complete sentence (it cries out for a noun phrase telling us what Fido destroyed), they had to give a complicated account of (2) in order to show this. GPSG, by explicitly labelling the gap, placed the italicized strings in different syntactic categories. But it enabled one to give equally simple descriptions of how to generate the two whole sentences (Gazdar 1981*a*).

GPSG had a significant influence on linguistics, being taken up by a number of syntacticians in the early 1980s. Clearly, it could achieve a great deal—but how much? It seemed to encompass English, and at least twenty other languages too. But there were growing doubts. It was already known that some European languages contain phrase-structure trees lying outside GPSG, and in 1984 two further languages—one from West Africa, another a Swiss dialect of German—were found to contain recalcitrant sentences (Gazdar *et al*. 1985:16; Gazdar 1998). Indeed, English itself, as Klein had discovered, contains some sentences—involving nested comparatives—that couldn't be described by GPSG.

Accordingly, Sag went on to develop an improved, but no longer context-free, version: "head-driven" phrase-structure grammar, or HPSG (Pollard and Sag 1994). And Gazdar showed that both GPSG and HPSG were special cases of "indexed" grammar. This is a general type located just above context-free grammar in the abstract Chomskyan hierarchy, but still appreciably below context-sensitive grammar (Hopcroft and Ullman 1979: 389–92).

Even within linguistics, however, GPSG didn't attract attention as widely as it deserved. This was largely due to the sociological factors cited in Section viii.a—including the *not invented here* syndrome that still pervades MIT. Stanford, to be sure, was open to Gazdar's theory—but CSLI didn't rule the world.

Today, much of the best work on non-Chomskyan syntax (including LFG, GPSG, and HPSG) is done in departments of computational linguistics, not of linguistics as such. This is only partly because the theorists concerned are interested in computer modelling. More to the point, they are "locked out" of the many departments of linguistics still dominated by the Chomskyan paradigm. One indication of this dominance is the fact that the term "generative grammar" is sometimes used as if it referred only to Chomsky's work—the current version of which is arguably not generative at all (Gazdar *et al*. 1985: 6).

e. Why GPSG matters

In cognitive science *as a whole*, GPSG was even less well known than in linguistics. In a sense, it was its own worst enemy. For it was yet more fiercely abstract than Chomsky's early publications had been. Non-specialists would need plenty of black coffee.

Moreover, the GPSG linguists, unlike Chomsky, had no desire to discuss broad issues of "language and mind". And, unlike Bresnan and Kaplan, they had no interest in psycholinguistics. They drew no psychological morals from their grammar, and no psychological discovery could either support or falsify their (purely formal) work. Some psycholinguists, including Janet Fodor and Rosemary Stevenson, did experimental research inspired by GPSG (G. Gazdar, personal communication). But because of the GPSG group's explicitly anti-mentalist stance (and their limited visibility in professional linguistics), most psychologists were unaware of their work.

Nonetheless, it was—and is—potentially important for cognitive science in five different ways. (Four are discussed in this subsection, and the fifth in the next.)

First, GPSG questioned Chomsky's fundamental critique of CFPS grammars. He had admitted that he couldn't strictly prove that English lies beyond any conceivable phrase-structure grammar. Now, his claim (his hunch) that it could be so analysed, if at all, "only clumsily" had been challenged. GPSG, which covers most if not quite all English sentences, is a formally elegant theory, not a clumsy one. It didn't deserve Chomsky's contemptuous accusation that it was "simply a needlessly complex variant" of a transformational theory, supplemented by "a class of superfluous devices" (Gazdar 1981*a*: 280–1).

Moreover, if natural languages can indeed be described by some context-free phrase-structure grammar (a CFPSG, for short), then a huge mathematical literature becomes potentially relevant. This literature was developed (partly as a result of Chomsky's papers of the early 1950s) in theoretical computer science, to deal with the parsability and learnability of programming languages.

Compiler programs, for instance, have to parse context-free programming languages in order to translate them into machine code. This is "perhaps the most researched and best understood area of formal language theory" (Gazdar 1981*a*: 275). In other words, and as Montague had already shown, the mathematical analysis of language—and, significantly, the comparison of natural and artificial languages—can be taken much further than Chomsky took it.

Third (a corollary of the previous point), the standards of rigour set by Harris and raised by Chomsky had been raised further still. Indeed, Chomsky's own standards had already been criticized by people outside the GPSG group. The Stanford-based logician Montague didn't even deign to criticize them, but contemptuously ignored "the attempts emanating from the Massachusetts Institute of Technology" (Montague 1973: 247). He was presumably offended by (among other things) Chomsky's basing his core claim about the necessity of transformations not on a proof, but on a hunch.

Others agreed, and accused Chomskyans in general of not having done their mathematical homework. As remarked in Section vi.a, the technical evidence behind *Syntactic Structures* wasn't published until 1975. Meanwhile, the famous mimeograph ("three thick volumes of faint purple typewriting with handwritten annotations") had probably been read only by a handful of those who had bothered to get hold of it (Sampson 1979*c*: 356).

One reviewer of the published version confessed that he himself had for years taken on trust Chomsky's hints that the theoretical lacunae in *Syntactic Structures* had already been filled in by the work presented in those purple pages. He confessed also that when he did finally get hold of a copy (while waiting to receive the published book for review), he "rapidly laid it aside" because of its near-illegibility. And he acidly commented:

> The publication of this book [in 1975] symbolizes a remarkable situation in the recent history of linguistics, a situation which can hardly have many parallels in other disciplines . . . [For the best part of two decades], during the heyday of Transformational Grammar, its hundreds of thousands of advocates in the universities of the world not only didn't but *couldn't* really know what they were talking about. (Sampson 1979*c*: 355–6)

Part of the explanation for this unhealthy state of affairs was that most linguists couldn't have understood Chomsky's manuscript even if they had read it. Realizing this, they'd been content to leave its arcane mathematics to the "high priests" at MIT, assuming that it answered all their theoretical questions. But they were mistaken. Those with mathematical eyes to see would find, when they did actually read it, that Chomsky's argument was often "maladroit" and "perverse", and sometimes "just plain wrong" (Sampson 1979*c*: 366).

(I must confess that I can't justify this claim at first hand, for I *don't* have mathematical eyes to see. Several specific examples have been pointed out to me, but I have to take them on trust. However, I'm a colleague of two of Chomsky's major 'mathematical' critics, Gazdar and Sampson, and I've pressed both of them hard on whether they really mean what they say in the passages I've quoted. They do.)

Many of Chomsky's mistakes, according to Sampson (1979*c*), were "elementary", and some were crucial. For example, he'd repeatedly claimed that the syntactic phenomenon of "Affix Hopping" can be stated as a single transformation (though he never showed just how to do so). That claim had persuaded many linguists into his camp. Yet it now appeared, Sampson complained, that one of his own formal definitions rendered it impossible. In other words, Affix Hopping had arguably been "a standing refutation of [his] theory throughout its history". Yet Chomsky had never discussed this problem (Sampson 1979*c*: 365).

In light of such points, the professional adulation of Chomsky was—or anyway, should have been—distinctly embarrassing. To be sure, most linguists weren't mathematical animals. But their misplaced admiration for Chomsky's formal work was due to more than mere ignorance. It was encouraged by sociological factors: his by-then-established grip on departments of linguistics, and his towering status as a worldwide guru.

Similarly negative judgements of Chomsky's work and influence have been expressed for twenty years by Gazdar and his colleagues. Indeed, it was Pullum who recently dismissed Chomsky's theory of minimalism as "risible" (see Section viii.b). Admittedly, Gazdar had earlier declared—and still does (personal communication)—that *if* any work in syntactics is to be called "foundational", as Frege's work was foundational for mathematical logic, then it is Chomsky's—especially his hierarchy of grammars (Gazdar 1979*b*: 197). But that's not to say that he saw all of Chomsky's "proofs" (in Chomsky 1975) as equalling Frege's in respect of precision and accuracy. To the contrary, he didn't.

Commenting on Chomskyan linguists in general, Gazdar's group pointed out that "mathematically respectable" proofs of general claims about grammars are very difficult to obtain. For instance, new versions of transformational grammar had been proposed (by Chomsky and others) which were held to be formally equivalent to, though more elegant than, some previous version; and linguists often claimed that such-and-such a structure cannot be generated by this or that grammar. But proving it was a different matter. Since most Chomskyans didn't even try to do so, the GPSG theorists were less than impressed:

> Such claims [about the formal equivalence and computational power of different grammars] are made in many contemporary linguistic works without there being any known way of demonstrating their truth. In some works we even find purported "theorems" being stated without any proof being suggested, or theorems that are given "proofs" that involve no definition of the underlying class of grammars and are thus empty. (Gazdar *et al.* 1985: 14)

Rigour was necessary, they said, not just for clarity and a good mathematical conscience, but for providing explanation. Thus any proposed "universal" should be a necessary consequence of some formal definition of the class of natural grammars. Otherwise, it is (at best) a description of some feature found, as a matter of fact, to be universal—without any understanding of why this is so (pp. 2 ff.). This point was similar to Marr's claim that we can explain visual processing only if we can relate it to (ideally, derive it from) the abstract constraints on vision as such: Chapter 7.iii.b.

A fourth point of interest for cognitive science as a whole is that GPSG implied that Chomsky's nativist 'argument from ignorance' against behaviourism must be reconsidered. Even if the simple learning theories popular at mid-century are inadequate to explain language acquisition, because they cannot parse phrase structure, some more sophisticated—but still purely surface-level—version might succeed. In other words, GPSG might be learnable (without the need for any dedicated acquisition device), even if transformational grammar isn't.

To be sure, the framework for this more powerful type of 'induction' might be innate: even Quine had allowed that "unquestionably much additional innate structure is needed . . . to account for language learning" (see Section vii.c). Even so, and although linguistic nativism hadn't strictly been disproved by Gazdar's work, the empirical evidence demanded years before by Black had become even more necessary.

One reason why GPSG didn't spread widely throughout cognitive science was that evidence for some sort of language-specific nativism was by then more abundant. Besides the developmental studies mentioned in Section vii.c, it included analysis of the many detailed features of human anatomy and physiology seemingly evolved specifically (*sic*) for language (Lenneberg 1967), and the apparent impossibility of teaching even simple grammatical structure to non-human primates (Chapter 7.vi.c).

In addition, neuroscientific evidence for *visual* nativism had—seemingly—been accumulating since the 'Frog's Eye' discoveries in the late 1950s (Chapter 14.iv & vi). And the new popularity of modular theories of mind (see above) discouraged reliance on general learning mechanisms to explain language acquisition.

That's not to say that nativism had been conclusively proved, for it hadn't. Despite the best efforts of Chomskyans to marshal their evidence (Pinker 1994), the facts didn't—and still don't—show that language, even linguistic universals such as coordination and hierarchical structure, can't be learned and/or evolved (Sampson 1997, esp. chs. 3 and 4). Furthermore, even if the nativists were right, Black's point still stood: the nature of the innate dispositions supposedly concerned, and of their neurophysiological implementation, were—and still are—unclear.

Indeed, the notion of nativism is itself unclear. Recent research in both psychology and neuroscience has shown how over-simple it is to label behavioural or anatomical features as 'innate' (see Chapters 7.vi.g and 12.viii.c).

The fifth important aspect of GPSG is perhaps the most significant of all. It has deep implications for both psychology and software technology, and especially for NLP.

f. Computational tractability

Because of its equivalence to a CFPS grammar, GPSG is less computationally demanding than transformational grammar. And that means, in a nutshell, that more can be achieved by relatively simple systems—whether minds or machines—than Chomsky believed.

With respect to psychology, GPSG suggested (for instance) an explanation of why people can parse natural language so rapidly (Gazdar 1981*a*: 276). And it lessened the difficulty of seeing language as the product of evolution, since context-free parsing algorithms are less evolutionarily improbable than transformational ones. More generally, if natural languages are indeed CFPS structures, then psycholinguists don't need—so shouldn't propose—psychological theories with the power of a context-sensitive grammar.

This is a special case of Lloyd Morgan's canon, framed in the late nineteenth century to promote intellectual hygiene in comparative psychology (see Chapter 2.x.b). Conwy Lloyd Morgan had forbidden explanation in terms of "a higher psychical faculty" if the behaviour in question could also be explained in terms of "one which stands lower in the psychological scale" (Lloyd Morgan 1894: 53).

For sixty years, this hugely influential advice could be followed only by relying on some intuitive sense of what counts as a "higher" psychical faculty. But Chomsky's hierarchical classification of grammars had, in effect, defined "higher" in terms of distinct levels of computational power, requiring processing mechanisms of specific types—with or without push-down, for example. (This enabled 'extra' levels to be inserted in the hierarchy: indexed grammar, for instance, lying above CFPSGs but still significantly below context-sensitive phrase-structure grammars, or CSPSGs.)

In short, the linguistic hypothesis that natural languages can be described by CFPSGs implied a psychological hypothesis about the minimum computational power required by the human mind/brain. Humboldt's "creative spirit of humanity" would have to rise as far as CFPSGs to generate language, but—if the GPSG theory was correct—need rise no further.

As for the software implications of GPSG, these were quickly recognized and highly influential. By the early 1980s, this grammar had already been adopted in various NLP projects in Europe, Japan, and the USA.

One of these was the "Alvey Tools Grammar", developed for the UK's Alvey Project (11.v.c) and still in use in various places today. And Sag's HPSG, which shares many features with GPSG although it isn't context-free (it permits unbounded recursion in certain circumstances), is now probably the most widely employed grammar formalism in NLP, especially in Europe (G. Gazdar, personal communication). (Very recently, Gazdar (1998) has defined a class of grammars, which includes GPSG, that is mathematically equivalent to a type—known as "linear tree adjoining grammars"—now gaining popularity in NLP circles.)

These NLP projects of the 1980s fell clearly into "technological" rather than "psychological" AI (see Chapter 1.ii.b). GPSG's psychological plausibility was irrelevant. So was the 'pure' linguist's question whether it could describe *every* syntactic construction in *every* human language. From the point of view of NLP research, that question is comparable to theological disputes about angels dancing on pinheads. What was important about GPSG, besides its success in describing almost the entire range of English (and some other languages), was its computational tractability.

Even devotees of GPSG, however, don't deny Chomsky's seminal importance. His work, besides affecting computer science as such, profoundly influenced NLP. Or rather, his classification of different types of grammar did so. His ever-changing syntactic theories have had less influence here. Partly because of his own lack of interest in, even hostility to, computer modelling, very few Chomskyan programs have been written (but see Chapter 7.ii.b). And, for reasons remarked above, many computational linguists are explicitly anti-Chomskyan. However, to be anti-Chomskyan isn't to be uninfluenced by Chomsky: LFG, GPSG, and HPSG were all developed in reaction to his pioneering efforts.

Moreover, Chomsky's formal–generative approach to linguistics in the late 1950s (and his linking of different grammars to different automata) buoyed the spirits of people already doing computer modelling of language, and encouraged others to start trying. (Section x will recount the early development of this field.)

g. Linguistics eclipsed

The last item within the tenfold Chomsky myth (i.a, above) was that linguistics is as prominent in cognitive science today as it was in the 1950s–1960s. That's not true. Ironically, its eclipse was largely due to Chomsky himself.

Part of the problem was his continual recasting of his own theory of syntax, described above. But that alone wouldn't have sufficed: if the price of doing good cognitive science had been keeping up with Chomsky, then that price would have had to be paid. More important than his inconvenient changes of mind was his mid-1960s distinction between *competence* and *performance* (Chapter 7.iii.a). This put a theoretical firewall between the psychology of language and linguistics as such. The firewall prevented commerce in both directions. Linguists, in effect, had been instructed to take no heed of psychology. But by the same token, psychologists were tempted to regard abstract linguistics as largely irrelevant to their concerns.

Researchers in non-linguistic disciplines didn't stop working on language, of course. But they stopped looking to Chomskyan theory for their key inspiration. As for the people who weren't working on language, they no longer gave linguistics a second thought. The general lessons (about formalization and generativity) that Chomsky had taught had been well learnt, and were no longer thought of, at least by the youngsters entering the field, as having any special connection with linguistics. By the 1990s, the discipline was "arguably far on the periphery of the action in cognitive science" (Jackendoff 2003: 651).

Recently, things have started to change. For instance, Pinker has put linguistics in the context of cognitive psychology, and in particular of evolutionary psychology (Pinker 1994, 1997, 2002). Moreover, the late 1990s saw a new field being named: cognitive linguistics. Its adherents are *linguists*, but linguists who take special note of the cognitive processes involved in using language (see 7.ii, preamble, and 12.x.g).

Similarly, the Brandeis (now Tufts) linguist Ray Jackendoff (1945–) has recently tried to integrate linguistics with psychology, neuroscience, and evolutionary theory (Jackendoff 2002, 2003). An ex-pupil of Chomsky, Jackendoff is still wedded to the idea of a universal grammar. But he claims to have shown (what Chomsky questioned) that it's possible for this to have evolved *incrementally*. He also argues that semantics doesn't need to be "paralysed" by a failure to solve the philosophical problem of intentionality (2002: 280). And he couches his argument in computational terms, describing a parallelist architecture (blackboard-based: see 10.v.e, and xi.g below) in which the rules of grammar are directly involved in language processing.

Jackendoff's book is described as a "masterpiece" by one *BBS* commentator, namely the philosopher Daniel Dennett (2003*b*: 673). Others disagree. For instance, another philosopher (F. Adams 2003) complains of its cavalier attitude to semantics and intentionality (although Dennett defends Jackendoff on this point—2003*b*: 673–4). And a neuroscientist objects to its assumption that the brain has evolved specialized components of language, as opposed to a small number of general capacities (he names seven) that make it possible to discover such "components" over the course of many millennia and/or to learn them within a few years (Arbib 2003: 668). Right or wrong, however, Jackendoff's theory is a far cry from Chomsky's chastely firewalled linguistics.

Or perhaps not? Employing the revolutionary analogy cited in Section i.b, Edelman has described Jackendoff's book as

> one of several recent manifestations in linguistics of the Prague Spring of 1968, when calls for putting a human face on Soviet-style "socialism" began to be heard (cf. the longing for "linguistics with a human face" expressed by Werth 1999: 18). (S. Edelman 2003: 675)

Edelman's prognosis for Chomskyan theory isn't hopeful:

> In a totalitarian political system, this may only work if the prime mover behind the change is at the very top of the power pyramid: Czechoslovakia's Dubcek in 1968 merely brought the Russian tanks to the streets of Prague, whereas Russia's Gorbachev in 1987 succeeded in dismantling the tyranny that had sent in the tanks. In generative linguistics, it may be too late for any further attempts to change the system from within, seeing that previous rounds of management-initiated reforms did little more than lead the field in circles. . . . *If so, transformational generative grammar, whose foundations Jackendoff ventures to repair, may have to follow the fate of the Communist bloc to clear the way for real progress in understanding language and the brain*. (S. Edelman 2003: 676; italics added)

To be sure, Edelman isn't a disinterested critic. For he argues, contra the Chomskyans, that the inductive learning of hierarchical structure is in principle possible. He and his colleagues have devised a pioneering "ADIOS" algorithm (Automatic DIstillation Of Structure), which can induce both context-dependent and context-free grammars (Solan *et al*. forthcoming). It uses a combination of statistics and structured generalization to extract hierarchical regularities from input sequences without knowing what patterns to expect, and without being given clues as labels on sequence parts (e.g. part-of-speech tags on the input words). Its only presupposition is that the sequence contains "partially overlapping strings at multiple levels of organization" (p. 3).

ADIOS has been applied to large sets of 'grammatical' sentences (e.g. the Bible and the *Wall Street Journal*), and also to the CHILDES corpus of spontaneous—and largely 'ungrammatical'—speech. (Those scare quotes are needed because Edelman rejects Chomsky's view that only strings fitting some set of abstract syntactic rules can be termed grammatical: language, for Edelman, is what people actually speak.) The system works even better with CHILDES than with the *WSJ* corpus, which Edelman (personal communication) explains in terms of Jeffrey Elman's stress on the importance of "starting small" (see Chapter 12.viii.c): whereas CHILDES contains many short word strings, the *WSJ* contains many longer ones.

In addition, ADIOS has coped well with six natural languages, including English, French, and Chinese. It has also been tested on artificial context-free languages, some having thousands of rules. And it has even been applied to DNA and protein sequences from several species. In each case, it has learnt to recognize the hierarchical patterns involved, and to reject anomalous sequences.

As its success with CHILDES illustrates, it has done this *despite* having ungrammatical strings in the input (which Chomsky saw as a huge barrier to inductive learning). Moreover, simulating the "creativity" of language, it has generated new (and legal) structures too—the first learning algorithm to do so. (The patterns it classifies in the learning phase are transformed into rewrite rules, which are then used for the generation phase.)

It doesn't follow that this is what infants do. Indeed, the authors point out that ADIOS has no access to conceptual knowledge, nor any "grounding" of the verbal input in current action and/or environment. (They also remark, however, that babies appear to use both statistics and rule-learning in acquiring language: Saffran *et al*. 1996; Seidenberg 1997; G. F. Marcus *et al*. 1999.) But ADIOS's success does dispatch arguments for the *necessity* of linguistic nativism.

In sum, linguistics may regain its place in cognitive science—if not as a *root* of theorizing in the other disciplines, at least as an equal, and collaborative, partner. If that happens, however, linguistics will be a very different enterprise from that engaged in by Chomsky.

9.x. The Genesis of Natural Language Processing

At mid-century, Harris wasn't the only person hoping to apply computer technology to linguistic problems. On both sides of the Atlantic, there were people "enchanted by the idea that [machine-based experiments to test linguistic hypotheses] could now be performed in the flesh rather than only in the spirit" (Bar-Hillel 1970: 293). And some were already doing so.

The late 1940s and early 1950s saw the first computer models for NLP. These attempted machine translation (MT), automatic classification, information retrieval (IR), text generation, and (for a while) the induction of grammars.

As it happened, these topics weren't the focus of the early research in any of the three pioneering AI labs at MIT, Stanford, and Carnegie Mellon (see 10.ii.a). Consequently, when people talk about the origins of AI they often ignore work in MT which pre-dated their foundation. By the same token, however, when MT suddenly fell into disfavour in the mid-1960s (a story told in Section x.e–f, below) AI as a whole—although it suffered—wasn't so badly affected as one might have expected.

a. Ploughman crooked ground plough plough

Hopes for automatic translation long pre-dated the computer age. It had been envisaged in some of the "universal language" projects of the seventeenth century (see Section iii.d, above). And the first (purely mechanical) MT patents had been taken out, in Russia and France, in 1933.

Whereas the French patent merely described a simple paper-tape dictionary that stored equivalent words in several languages, Petr Smirnov-Troyanskii's (1894–1950) machine could transform a grammatical analysis of a sentence in one language into its equivalent in another. Moreover, he claimed that it should also be possible to automate the (two-way) conversion between these analyses and actual sentences. But he wasn't able to demonstrate this, for lack of funds.

As late as 1944, the Institute of Automation and Telematics refused his bid for support; and his 1948 design for an electromechanical machine similar to the Harvard Mark I (see 3.v.b) was similarly ignored. (He died two years later, so had no chance to develop it.) This lack of appreciation was perhaps inevitable, for his ideas about MT were far ahead of his time:

> In retrospect, there seems to be no doubt that Troyanskii would have been the father of machine translation if the electronic digital calculator had been available and the necessary computer facilities had been ready. History, however, has reserved for Troyanskii the fate of being an unrecognised precursor; his proposal was neglected in Russia and his ideas had no direct influence on later developments; it's only in hindsight that his vision has been recognised. (W. J. Hutchins 1986: 24)

With the development of computers in the UK and USA, their use for various language-related purposes soon followed. In 1946, for instance, Andrew Booth (1918–) of Birkbeck College, London, suggested using computers for translation—and had designed and built a computer by 1947 (Lavington 1975: 5). In the late 1940s he started work with Richard Richens on what would become a lifelong project (Richens and Booth 1955; Richens 1958; Booth 1967, p. vi), and in the early 1950s he co-edited the first volume on MT (W. N. Locke and Booth 1955).

Richens, who was later attached to Margaret Masterman's Cambridge Language Research Unit (CLRU), has been credited with "the first serious contribution" to MT (Bar-Hillel 1959: 303; 1970: 303). His work (initially carried out by hand simulation using punched cards) was interesting not least because it considered grammar as well as vocabulary: the input verbs were split into stem and affix, so that *start*, *started*, and *starting* would have only one vocabulary entry, while *-ed* and *-ing* could be attached to indefinitely many different verbs. Booth and Richens's first experiments on MT were mentioned at a conference (on 'Science Abstracting') in Paris in June 1949, and featured briefly in the *Scientific American* six months later. But they became more widely known thanks to Warren Weaver's memo on MT, written after Booth's visit to the USA in 1947.

Booth's discussions in 1947 with the information theorist Warren Weaver, then Director of the Natural Sciences Division of the Rockefeller Foundation, indirectly influenced the initiation of MT research in America. MT programmes were set up at several US universities in the early 1950s, as a result of an influential memorandum written by Weaver two years after Booth's visit (Weaver 1949; W. J. Hutchins 1986: 24–30).

Financially, the burgeoning of NLP was largely driven by Cold War funding. This was especially true of machine translation. For military purposes, the prime languages were English and Russian, but in the USA they were soon joined by Vietnamese (Newquist 1994: 115).

The first public demonstration of an MT system, in January of 1954, translated forty-nine Russian sentences into English (using a 250-word vocabulary and six rules of grammar). Despite the six rules of grammar, this was basically a word-for-word translator. As such, it was hugely fallible. Nevertheless, it was used right up to the mid-1960s by the USA's Atomic Energy Commission (which was monitoring nuclear sites in Italy, as well as America), and by other governmental agencies too. In other words, it was *still* state-of-the-art: nothing better (for practical use) than word-for-word translation had been achieved.

That program, developed with CIA funding at Georgetown University, was one of many MT projects initiated in the USA in the 1950s. Even in the UK, much of the money for MT and other types of NLP came from various parts of the American defence programme. For instance, CLRU (founded in 1954) was partly supported by the US Office of Naval Research.

Predictably, NLP was developed in the Soviet Union too. Following the visit of a Russian computer scientist to the USA in 1954, for whom the Georgetown demonstration was proudly repeated, several research programmes were set up in the USSR (Hutchins 1986: 37, 133–45). By 1959, there were many hundreds of Soviet researchers in MT alone.

Intellectually, the field was driven by linguistics, by various logical and philosophical theories of language, and by the recent discussions of communication in cybernetics and information theory (see Chapter 4.v). Much early NLP research made a serious attempt to think about language systematically and/or to work towards practically useful ends.

But to say it was serious isn't to say it was successful. The well-known story—or rather, the group of mutually inconsistent stories—about "The spirit is willing but the flesh is weak" being rendered (after translation and retranslation) as "The whisky's fine, but the meat is rotten" is almost certainly apocryphal (W. J. Hutchins 1986: 16–17). However, there were certainly many such early-MT howlers. In the mid-1950s, for instance, Virgil's precisely inflected Latin sentence *agricola incurvo terram dimovit aratro* was once translated at CLRU as *ploughman crooked ground plough plough* (Masterman *et al*. 1957/1986). As remarked in the Preface, ii, this happened because CLRU used a thesaurus, not a dictionary.

With no computable theory of syntax—as opposed to a few simple programmed grammatical rules—available at the time, that's hardly amazing. (A fairly modest dose of syntax would have sufficed to deliver *A/The ploughman ploughs the ground with the crooked plough*.) And with no adequate theory of semantics either, it's not surprising that the CLRU's computational work on meaning didn't prevent this infelicity. Not until the early 1970s would there be powerful computational models of syntax, or of syntax allied with semantics (see xi.b, below).

b. Shannon's shadow

Shannon's theory of information was an important early influence on NLP. He himself used it in the late 1940s to produce "approximations" to English, as we saw in Section vi.c.

With hindsight, it's easy to see that these were word strings rather than sentences, because neither grammatical structure nor meaning were represented in information theory. At the time, this wasn't so obvious. Karl Lashley's (1951*a*) arguments against Markovian theories (see Chapter 5.iv.a) were unknown to most linguists and logicians, and Chomsky's more formal critique hadn't yet been published. Accordingly, many people were enthused by Shannon's ideas about the automatic generation of language.

Besides attempts to generate English text probabilistically, a number of people in the 1950s tried to induce grammars by statistical programs. In effect, they were aiming at the automatic discovery procedure sought by structuralism (see Section v). They focused on artificial rather than natural languages, not least because natural syntax was both complex and unclear.

Chomsky, for instance, cooperated with the psychologist George Miller (then at Stanford University, but soon at Harvard) in describing the formal properties of finite state grammars (Chomsky and Miller 1958). They also did experiments to find out what strategies people use, consciously or otherwise, to discover them (G. A. Miller 1967). Letter strings were labelled by the experimenters as grammatical or ungrammatical, and the subjects tried to learn to distinguish the two classes. If they could actually state the regularities involved, so much the better.

Their Cambridge colleague Bruner had investigated the information strategies used in concept formation (see Chapter 6.ii.b). Moreover, when Miller and Chomsky first described their experimental work (at the University of Michigan in 1957), they alerted the information scientist Ray Solomonoff, a participant at the Dartmouth meeting in the previous year, to define ways of inducing (artificial) grammars automatically (Solomonoff 1958, 1964). (These methods underlie the 1990s work on statistical grammar induction mentioned in Section v.d.)

That there should be linguists, psycholinguists, and information theorists at one and the same venue in 1957 was no accident. Communication between these groups had been growing throughout the 1950s. The Dartmouth meeting had caused an extra upsurge of interest, especially in AI models of learning and problem solving (see Chapter 6.iv.b).

But even earlier, in 1951, the information-theoretic approach to language was already predominant at Harvard and MIT—where Chomsky and his wife, Carol, respectively, were then working:

> Few linguists in Greater Boston at that time dared not use freely "message" and "code", "information" and "bits" in their shop talk, and nobody was "in" if he did not master, or at least professed to master, a good amount of probability theory and statistics. Everybody who was somebody in the field would sooner or later show up at MIT . . . All of us were enormously impressed by Shannon's well known [probability-based] experiments in what he called "approximations to ordinary English" and were convinced that speech, in English or any other language, was a Markov process. From this to the conviction that the English language, i.e. the set of all English sentences, can be generated by a Markov source, was only a small step, and I am not sure that we noticed at the time that this was a step at all. (Bar-Hillel 1970: 294–5)

The person reminiscing here, and quoted above as initially "enchanted" by the possibilities of the new technology, was the logician Yehoshua Bar-Hillel (1915–75). He'd encountered the logical positivists' approach to language in 1935, while studying at the Hebrew University in Jerusalem. He experienced their work as "nothing short of a revelation", later describing Carnap's volume on the logical analysis of language as "the most influential book I read in my life" (Bar-Hillel 1964: 1). And Bloomfield's linguistics "kept the fire of my interest burning".

So Bar-Hillel shared the structuralists' dream of an inductive discovery procedure for grammars. His hope was intensified when Harris, on a visit to Palestine in 1947, remarked that he was considering using the new electronic computers for this purpose. And, at close of the 1940s, he became "the first prophet in Israel" of cybernetics, including its potential relevance to the mind–body problem (Bar-Hillel 1964: 5).

With these views firmly in place, in 1950 Bar-Hillel accepted a fellowship at the University of Chicago, where he got to know Carnap personally. In the following year he visited MIT, well primed to be drawn into the enthusiastic information-theoretic community described above. Indeed, he was very soon appointed the first director of MIT's MT research. His remit was to survey the nascent field, to assess its prospects, and to plan MIT's research accordingly. His initial survey—of which, more below—was submitted in 1951, and in 1952 he organized the first MT conference (Hutchins 1986: 34–6).

Bar-Hillel didn't stay at MIT very long. On his return to Jerusalem in 1953, he was succeeded by Yngve, also from the University of Chicago. Yngve, now in his eighties, has been described by one NLP expert as "still the most original of all computational linguists" (Y. Wilks, personal communication).

In the 1950s, Yngve used both cybernetic and symbolic approaches. Besides doing early work on MT (Yngve 1955), he studied statistical methods for generating English (Yngve 1961) and developed the COMIT programming language, the first to be designed specifically for NLP (Yngve 1958). In the 1960s, he wrote a *Scientific American* article describing MT to the general public, and a technical survey defending MT against a damaging attack from an official committee (see below) (Yngve 1962*c*, 1967).

Yngve also made an early, and influential, attempt to relate syntactic theory to specific hypotheses about how people process language (Yngve 1960). That was an indication of the difference between his interests and those of the more austere grammarians, whether Chomsky or Gazdar. Over the years, his interests moved more towards "people" and away from "language" (Yngve 1986). And as remarked in Section viii.a, he attacked Chomsky on sociological as well as intellectual grounds.

During Bar-Hillel's brief stay in Massachusetts in the early 1950s, he met Chomsky. The two men interacted regularly, and Chomsky expressed his gratitude to Bar-Hillel several times in his early manuscript on the logical properties of language (e.g. Chomsky 1975: 31).

By 1955, Chomsky had accepted an appointment at MIT's Research Laboratory of Electronics (jointly with Modern Languages). In 1957, as remarked above, he and Miller were doing psychological experiments that immediately prompted information-theoretic work aimed in part at the development of NLP. And his hierarchy of grammars, as we've seen, helped to suggest what type of computational device would be needed to compute natural language.

Nevertheless, Chomsky never shared Bar-Hillel's enchantment with NLP. On the contrary, he was deeply sceptical from the outset:

> My personal reaction to this particular complex of beliefs, interests, and expectations [that information-theoretic computer modelling, combined with empiricist psychology, would make NLP possible, and further the positivist unification of science] was almost wholly negative. The behaviorist framework seemed to me a dead end, if not an intellectual scandal . . . I had no personal interest in the experimental studies and technological advances. [I felt the latter to be] in some respects harmful, [leading people to focus on] problems suggested by the available technology, though of little interest and importance in themselves. As for machine translation and related enterprises, this seemed to me pointless, as well as probably quite hopeless. As a graduate student interested in linguistics, logic, and philosophy I could not fail to be aware of the ferment and excitement [in the early 1950s]. But I felt myself no part of it and gave these matters little serious thought. (Chomsky 1975: 40)

When he did become interested in them, soon after completing his *magnum opus*, he wrote his fierce review of Skinner (see Section vii.b). His only foray into NLP work was a program schema that used transformational grammar (rewrite rules), not statistics, in its processing (G. A. Miller and Chomsky 1963: 464–82).

Nor was he a fan of AI in general. Quite apart from any intellectual reasons for scepticism, there appears to have been some unpleasant rivalry between him and Marvin Minsky—both young, brilliant, famous, and ambitious. In the mid-1960s, when the *Alchemy* scandal was at its height (see 11.ii.b), he went over to the MIT AI Lab, to sit in on one of the heated debates between Weizenbaum and Hubert Dreyfus on one side and Minsky and Seymour Papert on the other. David Waltz, an AI student at the time, now recalls:

> I remember once Noam Chomsky came over [to these debates] also. There was personal animosity between him and Minsky, and people were very rude to Chomsky when he came to the AI lab. They booed when he talked, and he was very miffed. (interview in Crevier 1993: 123–4)

As for Bar-Hillel, and partly because of his discussions with Chomsky, his love affair with—though not his interest in—NLP was to be short-lived, as we shall see.

c. Love letters and haikus

Despite the "ferment and excitement" aroused at mid-century by information theory and statistical cybernetics, some NLP pioneers chose instead to use the newly available digital computers as symbol manipulators.

Even very early on, these machines could be programmed with rules (written in binary notation) for selecting words from distinct grammatical classes. But with the introduction of list-processing languages, such as IPL and COMIT, in the mid-1950s (see 10.v.b), hierarchical syntactic structures could be represented more easily.

Probably the earliest computerized text generator was Turing's playful love-letter program. This was written in the late 1940s for MADM, the world's first electronic stored-program digital computer (Chapter 3.v.b). Using random numbers to choose the words, MADM produced this (Lavington 1975: 20):

Darling Sweetheart,

You are my avid fellow feeling. My affection curiously clings to your passionate wish. My liking yearns to your heart. You are my wistful sympathy: my tender liking.

Yours beautifully

M.U.C. [Manchester University Computer]

The program hasn't survived. But presumably the last two lines, apart perhaps from the adverb *beautifully*, were provided in 'canned' form, while some overall template instructed the machine to select an adverb here, a noun there, and perhaps to pick two endearments (*Darling*, *Dearest*, *Sweetheart*, *Love* . . .) for the first line. Template fillings of this type were soon to be used at CLRU to generate texts fitting the strict constraints of a Japanese haiku (Masterman and McKinnon-Wood 1968; Masterman 1971). Two examples are:

All green in the leaves
I smell dark pools in the trees
Crash the moon has fled

All white in the buds
I flash snow peaks in the spring
Bang the sun has fogged

As the CLRU team realized, the apparent meaningfulness of these mechanically generated haikus was due primarily to the human reader, whose "effort after meaning" (Bartlett 1932: see 5.ii.b) projected significance onto the initially puzzling lines. But if the CLRU realized this, many others didn't. The program was exhibited at the first international exhibition of computer art, and impressed the visiting public more than it should have done (J. Reichardt 1968).

d. Wittgenstein and CLRU

Some of the intellectual sources of early NLP were surprising, being very far in spirit from structuralist linguistics and information theory, and from symbolic computing too. This applies, for instance, to the pioneering research done at CLRU under the direction of Masterman (1910–86).

Their computational work was informed by the anti-positivist philosophy of the later Ludwig Wittgenstein. In scientific circles, this approach to language was nothing if not unorthodox. Wittgenstein himself had likened it to Goethe's views on morphology—see Chapter 2.vi.e–f (Monk 1990: 303–4, 501).

Although these new ideas weren't published until after his death, Wittgenstein had lectured on them—and his lecture notes had circulated—in Cambridge for many years. Moreover, he'd dictated some of them to Masterman herself (see Preface, ii). The lectures provided a radical critique of his own earlier (logicist) position (Wittgenstein 1953), and a very different view of language. Whereas he had once declared that "Everything that can be said at all can be said clearly" (Wittgenstein 1922: 27, 79), he now saw an indefinable penumbra of meaning surrounding every statement.

One of his main points was that words don't have cut-and-dried (essentialist) definitions, in terms of necessary and sufficient conditions. Rather, the items falling under a given word (*game*, for example) are linked by a network of "family resemblances" (Wittgenstein 1953: 31 ff.). Analogously, relatives may share the family nose, the family eyebrows, the family fingernails . . . without any one person having all these characteristics. Admittedly, some items are better examples of the concept concerned, so can function as "paradigm cases". (These have Rosch-typicality ratings close to 1: see Chapter 8.i.b.) But there's no clear cut-off point.

Masterman was convinced by the doctrine of family resemblances, and disturbed by the all too evident weakness of word-for-word dictionary translations (first attempted by Richens and Booth 1955 and Yngve 1955). Her critique of the early word-for-word efforts (Masterman 1967) was backed up by her own thesaurus-based approach to NLP (Masterman 1957, 1962). One of the early AI workers influenced by her research was M. Ross Quillian, who visited King's College, Cambridge, in 1961 to give a talk about his early ideas on semantic networks (Chapter 10.iii.a).

The thesaurus was envisaged as a semantic interlingua, through which the translator could pass from one natural language to another. (For an early application of CLRU's interlingua approach, see the discussion of "catataxis" in Parker-Rhodes 1956.) Many examples of semantic ambiguity, which would defeat a word-for-word MT procedure, could be resolved by this means. The semantically ambiguous word would fall under at least two different heads in the thesaurus, and the one actually selected by the program would be the one under which some other words in the sentence fell also. So *I put my money in the bank* and *I know a bank whereon the wild thyme blows* would be recognized as dealing with financial and rural matters, respectively. (Syntactic analysis would be needed to deal with *I drew some money from the bank to buy some wild thyme*.)

Initially, Masterman used a thesaurus derived by human intuition, namely, a cut-down version of Peter Roget's pioneering work published 100 years earlier. But her group also developed procedures for constructing thesauri automatically. Formal clustering methods were used to identify word classes, in terms of the co-occurrence of various words. Those word classes determined lexical substitutability for the purposes of machine translation. In addition, they aided in the automatic classification, indexing, and retrieval of documents (Sparck Jones 1988). (The pioneering librarian Gabriel Naudé would have been intrigued: see Preface.)

CLRU's thesaurus approach offered flexibility of various kinds. It could be applied to texts of any length, not just to isolated sentences. It was a source of semantic primitives that could be used to define meaningful messages (or "gists"), based on *actor–action–object* patterns, that were helpful for single-language interpretation and précis as well as for translation. And, not least, it enabled Masterman's group to take some account of the unpredictable extensibility of language. This had been stressed by Wittgenstein and Waismann—and, as Masterman often remarked, by the philosopher Ernst Cassirer (1874–1945), who followed Humboldt in highlighting the unceasing development of language (Cassirer 1944).

The extension of language was modelled, for instance, by Yorick Wilks (1939–). Like Masterman, he was especially interested in the interpretation of metaphysical texts—dismissed as "meaningless" by the logical positivists fashionable at mid-century. He wrote a program in the 1960s to assign semantically coherent representations to textual snippets drawn from the writings of a wide range of philosophers.

Wilks's program could do this successfully for some test paragraphs from Descartes, Leibniz, and David Hume, *without* any need to extend word meanings. But other test paragraphs did require that. Accordingly, his program would locate the semantically problematic word in a chunk of text (not a trivial matter), and then use thesaurus-driven methods to identify its meaning with some other term or phrase within the passage (Wilks 1972: 166–72). If the problematic word was an extension of sense relative to some interpretation already stored in the (thesaurus-based) dictionary, Wilks's EXPAND algorithm could develop the rules for 'literal' interpretation so as to make sense of the anomalous word in context.

This procedure wasn't foolproof. What if there were no other suitable word in the text, and/or some major sense of the problematic word had been omitted from the dictionary? And what is to count as a "major" sense, anyway? (Bar-Hillel, clearly, was hovering in the background.) Nevertheless, it gave acceptable readings of some notoriously tricky texts, including passages drawn from Wittgenstein's *Tractatus Logico-Philosophicus* and Spinoza's *Ethics*. Short of torturing it with extracts from *Finnegans Wake*, one can hardly imagine a more taxing test. In short, this early NLP model tried to escape the logicist straitjacket by acknowledging the creative extensibility of language. (Some of Wilks's later work did so too: his "preference semantics" was used in automatic parsing, interpretation, and inference—Wilks 1975.)

The methods developed by CLRU weren't intended as theories of how human beings translate, interpret, or classify. As remarked in Chapter 1.ii.b, this NLP group was doing technological, not psychological, AI. (Quillian, by contrast, was at least as interested in simulating human psychology as in producing useful technology: 10.iii.a.) But the practical demands were taken seriously from the start. One of the strengths of CLRU was its emphasis on careful testing and experiment to compare IR (information-retrieval) systems (Sparck Jones 1988: 14–17, 25–6). Mere plausibility wasn't good enough—and was sometimes found to be misleading. In short, Drew McDermott's well-deserved criticisms of much early AI didn't apply to Masterman's group (see 11.iii).

Whether Wittgenstein himself would have approved of any computerized approach to translation or classification, even one (such as Masterman's) part-inspired by his own ideas, is another matter. He didn't explicitly discuss any of the budding AI work. Nor do we know of any informal meeting between him and Turing, still less any discussion between them about the possibility of AI. (For an entertaining account of an imaginary meeting, see Casti 1998.) But when Turing attended some of Wittgenstein's lectures on the philosophy of mathematics, the two men repeatedly disagreed on fundamental points (Monk 1990: 417–22). The whole tenor of Wittgenstein's mature philosophy suggests that he wouldn't have welcomed computational work on language.

In part, this follows from his view of language as a network of social conventions rooted in the human "form of life", a notion that can hardly be applied to a computer. But in part, it follows from his stress on the flexibility and open texture of language (and concepts), which seems to lie beyond any scientific explanation.

This flexibility may be approximated by some computational methods, such as CLRU's thesaurus. But even if anti-logicist NLP is in practice less inadequate than logicist approaches, the philosophical problems remain. For Wittgenstein, presumably (S. Shanker 1998)—and for his follower Dreyfus, certainly (H. L. Dreyfus 1972; Dreyfus and Dreyfus 1988)—neither type of NLP can escape the fundamental limitations of AI in general. The Wittgensteinian position agreed with what Descartes had argued long ago (2.iii.c): machine mimicry of language use can only be highly imperfect.

e. Is perfect translation possible?

However, in this area of AI as in others, the question whether some task could in principle be performed (or mimicked) *perfectly* by a machine must be distinguished from the question whether it could in practice be performed (or mimicked) *well enough to be useful*.

The first question is especially difficult where translation is concerned, because it's not even clear what—if anything—would count as perfection in the human case.

Humboldt had claimed that no human translator can achieve perfect equivalence (see Section iv.b, above). In scientific texts, he allowed, translators could hope to do their work with no important loss of meaning. But in literary contexts, they couldn't (Novak 1972). In discussing the (many) possible criteria for a good literary translation, he suggested that the translator should choose words and sentence structures that somehow indicate the original language. The more one does this, however, the less acceptable (in the derivative language) the new sentence will become. Mark Twain's spoof of the German habit of delaying the verb illustrates the point:

> I am indeed the truest friend of the German language—not not [*sic*] only now, but from long since—yes, before twenty years already. And never have I the desire had the noble language to hurt; to the contrary, only wished she to improve—I would her only reform. It is the dream of my life been . . . I would only some changes effect. I would only the language method—the luxurious, elaborate construction compress, the eternal parenthesis suppress, do away with, annihilate; the introduction of more than thirteen subjects in one sentence forbid; the verb so far to the front pull that one it without a telescope discover can. With one word, my gentlemen, I would your beloved language simplify, so that, my gentlemen, when you her for prayer need, One her yonder-up understands . . .
>
> After all these reforms established be will, will the German language the noblest and the prettiest on the world be. (Twain 1897)

Moreover, it's not even clear that this particular criterion of fidelity to the source language should be considered at all. Humboldt himself was well aware that there are conflicting views on what counts *in principle* as a high-quality translation. In short, "perfect" translation is a chimaera. And even high-quality translation is often very difficult.

If this is true for expert human translators, the difficulties facing machines (or rather, their programmers) are so much the greater. Both meaning and grammar stand in their way.

Meaning is notoriously elusive: if it can be pinned down in principle, which even empiricists often deny (Quine 1960), it doesn't follow that it can be captured in practice. Semantic ambiguity and vagueness, not to mention metaphor, are obvious problems. Moreover, a translation machine (eschewing word-for-word translation and statistical methods) needs a universal grammar or an artificial interlingua to mediate between languages, and/or full descriptions of the relevant pair of natural-language grammars. But defining a grammar to cover a single language, never mind a universal grammar, is no mean feat, as we saw in Section ix. And to define a grammar isn't to program it: successful parsers weren't available until the early 1970s (and true computational effectiveness had to await GPSG, more than a decade later).

Most of these theoretical points were made in the 1950s by an increasingly disillusioned Bar-Hillel (1951, 1953, 1960). Despite having been enchanted by the linguistic promise of the new technology in the 1940s, he later expressed deep pessimism—not only about machine translation, but about computational linguistics in general (Bar-Hillel 1970: 298 ff.). He even apologized for his own early influence on the field:

> I myself was led into confusion by the strong surrounding currents [of cybernetics] and was responsible to a degree for increasing this confusion and leading others into blind alleys from which some never returned. (Bar-Hillel 1970: 289)

In recanting his earlier beliefs, Bar-Hillel didn't merely lament the unexpectedly slow progress. He claimed to prove that certain things, their feasibility largely taken for granted by the young NLP community, simply cannot be done by automatic means.

For example, Harris's suggestion that texts made up of syntactically complex sentences can be represented as transformations of several much simpler kernel sentences had led many to hope that NLP could proceed by normalizing texts to some simpler form. Bar-Hillel proved, by contrast, that "Not everything that can be said at all can be said, in a given language, by using syntactically simple sentences exclusively" (Bar-Hillel 1963: 31).

As for semantic ambiguity, this often calls for world knowledge as well as knowledge of language as such. The simple sentence *The box is in the pen*, for instance, can be understood only if one knows that playpens are large enough to contain boxes whereas writing pens are not (Bar-Hillel 1960, app. iii). An MT program concerned specifically with such topics could of course be given this information beforehand. But, Bar-Hillel insisted, such priming can't be done in the general case.

In short, what he called FAHQT (fully automatic high-quality translation) is impossible.

f. Is adequate translation achievable?

Bar-Hillel was complaining about the "science-fictional claims" made by some proponents of NLP, namely those who were promising FAHQT. He wasn't arguing that computer models of language are wholly useless.

In his first report to MIT, he'd said that whereas high-accuracy, fully automatic, MT isn't feasible, "there appear to be less ambitious aims the achievement of which is still theoretically and practically valuable" (Bar-Hillel 1951: 154). Almost a decade later, when he surveyed the progress of MT in the USA and UK (and USSR), Bar-Hillel again stressed the impossibility of human-quality MT (Bar-Hillel 1960). Instead of aiming for this unattainable goal, he said, AI should aim for imperfect-but-intelligible automatic translations, possibly supplemented by human post-editing.

Alongside this potentially hopeful advice, however, he argued that even this less ambitious project should be undertaken in a different way. In his second report, after MT had been under way for some years, he rejected approaches based on statistics and information theory, which he'd previously thought so promising. Nor did he have much time for the then-fashionable methods of GOFAI (Good Old-Fashioned AI: see Chapter 10). He criticized virtually all the major MT groups by name, and attacked the most common methodologies.

The immediate effect of Bar-Hillel's second report was almost to destroy faith in MT on the part of the general public, including scientists not working in the field. A best-selling popular book on *Computers and Common Sense* by Mortimer Taube (1910–65), for instance, took up Bar-Hillel's negative remarks while ignoring his positive ones (Taube 1961).

Taube's book announced in highly emotional terms that MT was impossible and MT research—indeed, AI in general—a waste of taxpayers' money. The nation's monetary resources, Taube said acerbically, would be better devoted to "research looking toward the Second Coming" than to research aiming to build machines with "intelligence, knowledge, and knowledgeability". His dismissive conclusion was that "One may wonder why reputable scientific journals publish material of this sort and why it should have an audience beyond the readers of the Sunday supplements."

Taube, an internationally famous librarian, knew some of his computer onions: he'd pioneered powerful new forms of indexing and information retrieval. Even so, his attack cut little ice with the AI community. Being called "ignorant" and "jejune", and being accused (repeatedly) of "scientific aberration", led to so much offence that they hardly bothered to marshal a defence. Moreover, the book was—as one early cognitive scientist put it—"neither reliable nor responsible", being largely comprised of "allegations presented as facts, of misunderstanding, of debaters' tricks identical to those he decries in others, and of statements about the work of others which are simply untrue" (Reitman 1962: 718).

However, Taube's attack encouraged an attitude of scepticism, not to say ridicule, in the general reader. And the "general readers", in this case, included not only counter-culturalists such as Theodore Roszak (1969: 295) but also people who might at some point have to make decisions about funding policies for MT.

As if that weren't damaging enough, Bar-Hillel's critique also dampened the optimism of MT researchers who were aiming to approach human-quality results. Those who were willing to tolerate many linguistic infelicities, provided that these didn't compromise understanding, were less set back. Even so, and even though significant (military-led) funding continued for over a decade, many MT workers lost heart. At Harvard, for example, almost all research in this field had ceased by 1964.

In that year, the scepticism deepened further. A survey had just been conducted by the US government's Automatic Language Processing Advisory Committee (ALPAC), chaired by the Bell Telephones executive John Pierce (Hutchins 1986: 164–7). Their report was thirty-four pages of dynamite—plus ninety pages of smouldering appendices. It concluded that previous MT research had been a failure, that fully automatic MT was impossible, and—even worse—that there was "no immediate or predictable prospect of useful machine translation".

This negative judgement wasn't a purely scientific one. It was partly based on ALPAC's view that not enough customers would want to use MT to make it commercially viable. Machine aids for human translators, however, might well be feasible.

The effect on informed (or rather, "semi-informed") public opinion was predictable. Yngve described it like this:

> The [general] public is now where the pioneers were many years ago, vaguely aware of the difficulties but allowing that the thing is possible, while the workers, with more than a little knowledge now, see quite clearly that there is very much yet to be done. And the semi-informed public gives indications of swinging back to [their] original position, that the task is virtually impossible, therefore no proper area for research. In this they may be echoing a few despairing noises coming from some of the workers who had expected a quick victory. (Yngve 1967: 453)

Despite criticisms that the ALPAC report was "narrow, biased, and shortsighted", and imbued with a "hostile and vindictive attitude", it had a devastating effect on MT research in the USA. The activity was virtually halted for over a decade. And the devastation was near-instantaneous: "Funding for [MT] projects dried up within weeks of the ALPAC report, and several research labs shut down soon thereafter" (Newquist 1994: 116).

It had a negative influence in the UK and Soviet Union, too. Work on translation at CLRU, for instance, ceased in the late 1960s—even though the Unit itself continued, and did valuable work on various aspects of information retrieval (Sparck Jones 1988; Wilks forthcoming).

If the negative aura of ALPAC could cross oceans, it could cross some intellectual boundaries too. It threw a shadow over AI in general, and NLP in particular. Although the three main AI departments weren't much affected (they weren't doing NLP anyway), several US universities that had been planning to start departments of AI decided not to do so (Newquist 1994: 117).

Nevertheless, ALPAC didn't end NLP as a whole. On the contrary, it favoured increased support of *basic* research in computational linguistics. If the embarrassingly prolific MT cousins of *ploughman crooked ground plough plough* were ever to be avoided, this would require a better understanding—and effective computational implementation—of theoretical linguistics.

9.xi. NLP Comes of Age

NLP would come of age (in the 1970s) with the acceptance of more realistic goals for MT, with the appearance of effective computer parsers, and with the recognition that, besides processing single sentences, one must also model the rhetorical coherence of entire texts. The last two lines of development—which required advances in general AI—are still under way.

Moreover, it's now clear that improved statistical methods, let loose on huge samples of real-life linguistic data, can go much further than Shannon's early critics imagined. Even as late as 1987, that was less clear. At a statistics/linguistics conference in Oxford, sponsored by the Engineering and Physics Research Council (EPSRC), the atmosphere was civilized but there was no real communication (G. Gazdar, personal communication). At a similar meeting held at the Royal Society in 1999, there was (I witnessed that for myself). What's more, while the EPSRC may have expected all the benefit to go from the statisticians to the linguists, in fact it went in both directions.

For instance, Markovian three-word chunks can now be used to make word predictions almost as well as people can, although they can't generate grammatical sentences as reliably (Pereira 2000). These probabilistic models typically induce "hidden" rules from huge databases, and some rely on symbolic rules or tree structures derived from theoretical linguistics (Gazdar *et al*. 2000). Many are trained on collections made up of several hundreds of millions of words. Even so, not all sentences (or newly coined words) can be included. Chomsky's complaint that Markovian theories can't allow for the creative use of language is countered by "smoothing algorithms" that avoid assigning zero probabilities to unencountered events.

Some advances, whether symbolic or statistical, are of primarily technological interest. They will affect our daily lives, but not necessarily our understanding of the mind. Others are of interest only to highly specialized linguists. Below, I outline a few post-1960 landmarks, selected for their relevance to cognitive science *in general*.

Strictly, a certificate of maturity for NLP would also require advances in speech processing—both understanding and synthesis. The former involves the analysis of continuous speech into individual words, and the classification of acoustically different sounds as one and the same phoneme. It had to await the development of vocal-acoustic technology, and also required an integration of several theoretical levels: acoustic, phonetic, morphemic, syntactic, and semantic.

Since I've virtually ignored phonetics throughout this chapter, I'll ignore speech systems too. An early machine for learning to recognize phonemes was mentioned in Chapter 6.ii.c, but this was merely an exploratory toy. The pioneering HEARSAY program of the early 1970s will be mentioned in Chapter 10.v.e, because of its innovative "blackboard" architecture, which integrated processes based in syntax and semantics with processes grounded in phonetics. And the 1980s connectionist system NETtalk is discussed in Chapter 12.vi.f, because of its methodological interest. But speech processing as such isn't crucial for our purposes. Apart from a few remarks at the close of the chapter (subsection g), it won't feature here.

a. MT resurrected

As for machine translation, the ALPAC authors were shown in the long term to have been over-pessimistic. But their suggestion that basic research in computational linguistics would be helpful was justified. Bar-Hillel, by contrast, turned out to be correct on both main counts: although human-quality MT isn't achievable in the general case, practically useful MT is. Masterman's key problem of word-sense disambiguation, for example, can be approached today by methods fundamentally similar to hers but much more powerful (Ide and Veronis 1998).

There are now many MT systems in daily use (W. J. Hutchins 1994, 1995). Most, as Bar-Hillel had predicted, deal only with a highly restricted area, but some can cope with a fairly wide range of topics. They are used, for example, to scan foreign-language news media or scientific publications, or to express political debates and legislation.

Their translations are typically good enough to be understood by informed human readers, and/or to show whether it would be worthwhile to ask a human translator to improve them. Sometimes, only minimal post-editing is required. But sometimes (for instance, when Japanese is involved), significant pre-editing and post-editing are needed.

The SYSTRAN program is one well-known example (P. J. Wheeler 1987). A descendant of an early Russian–English system funded by the US Air Force, it was written in the late 1960s by a team led by Peter Toma, a participant in the famous Georgetown project. The European Community adopted it for English and French in 1976, since when it has been broadened to include all the official languages of the European Union. It is used also by NATO and the International Atomic Energy Authority, and by commercial companies such as Xerox and General Motors.

Some of these applications appear at first glance to evade Bar-Hillel's criticism. For many documents in English can be translated by SYSTRAN into other European languages with near-perfection. The trick is to use only a small subset of English for the original document. A number of other commercially available programs can also provide near-perfect translation of technical manuals and the like, by using highly restricted vocabulary and syntax.

What might well surprise Bar-Hillel is that some very recent MT systems rely heavily on statistical pattern matching (W. J. Hutchins 1995: 440–1; 1994). Rule-based approaches have given way to corpus-based ones, and the emphasis has shifted from syntax to the lexicon (Hutchins 1994, sect. 9). Instead of careful syntactic analysis, these systems rely on probabilistic methods based in some huge corpus of paired source/translation texts. Shannon, one might say, has returned with a vengeance.

IBM's CANDIDE system, for instance, draws on the voluminous (and multi-topic) French–English 'Hansard' of the Canadian Parliament. In the mid-1990s this program contained no grammatical rules whatsoever, relying purely on statistical matching of words and phrases. Its performance amazed most observers, given that the analytical approach had been largely taken for granted in NLP for thirty years. Even ALPAC had seen detailed computational linguistics as essential for automatic translation. Almost 50 per cent of phrases were acceptably translated, being matched either with their database originals or with equivalent expressions.

However, one must not forget the other 50 per cent. To deal with those, future versions of CANDIDE will include some morphological and syntactic information, as well as more sophisticated statistical methods. But grammar and morphology will be used as sparingly as possible. In other words, the team expect the statistics to trump the syntax. It remains to be seen whether they are right, and to what extent MT can afford to ignore theoretical linguistics.

Certainly, grammatical correctness is MT's Achilles' heel. The general assumption, *pace* CANDIDE, still is that this demands painstaking attention to syntax (gender, number, tense, and so on). Measures of intelligibility and correctness have shown a dramatic improvement in the former, but much less progress on the latter.

For instance, in the two-year period 1976 to 1978, the intelligibility of translations generated by SYSTRAN rose from 45 to 78 per cent for the raw text, and from 95 to 98 per cent for the post-edited text (W. J. Hutchins 1986: 261). Human translation, it's worth noting, gave not 100 per cent but only 98–9 per cent.

SYSTRAN's correctness, where every tiny error counts, scored much lower. In 1976 the measure for grammatical agreement was only 61–80 per cent; and in 1978, only 64 per cent of words were left untouched by the human post-editors. Even so, human post-editing of a page of SYSTRAN output took only twenty minutes in the mid-1980s, whereas normal (fully human) translation would have taken an hour. However, over half of the amendments involved substituting a different word, not simply altering a word ending. In both cases, the main source of errors seemed to be the dictionary, which in 1978 contained only 45,000 entries. Accordingly, the computerized dictionaries have been gradually enlarged.

The more ambitious EUROTRA system was multilingual as opposed to severally bilingual, dealing with forty-two different language pairs (King and Perschke 1987). It held 140,000 words in its prototype stage, in 1988: 20,000 words for each of seven languages, with the expectation of at least two more to come. But no operational version was ever built. EUROTRA was influential, nevertheless. It was recently recognized in an official report as providing much of the basis for later work in MT across Europe (*Euromap Report* 1998: 59).

More ambitious still would be an MT system that could switch between languages in very different language groups, such as English, Japanese, and Hindi. In Japanese, for example, the words aren't segmented as they are in English (no equivalent of our *post-ed*, for the past tense of *post*), and the phrase orderings in the parse trees are reversed (Powell 2002: 112). Although there are MT programs combining Japanese and English, and also for switching between several of the many languages of India, they aren't as efficient as the best European-language systems.

(I've mentioned corpus-based work only in MT, but similarly statistical approaches are increasingly being used in other areas of computational linguistics too: see Sampson and McCarthy 2004.)

b. Automatic parsing

If computerized dictionaries are relatively easy to improve, automatic parsing is a different matter.

NLP in the 1950s and early 1960s, as we've seen, was hampered by the lack of a powerful theory of syntax. Computational work on syntax started to show results in the early 1970s. This development was largely due to the post-Chomsky interest in formal grammars. (This is *not* the same thing as an interest in Chomsky's formal grammar: as remarked in Section ix.f, very few people used Chomsky's theory as the basis of their NLP programs.)

The first impressive computer parser was written by William Woods at Bolt, Beranek & Newman (BBN), using "augmented transition networks", or ATNs (W. A. Woods 1970, 1973). The computational advantage of ATNs, which soon became widely used in NLP, was that they considered the input sentence in one pass, working left to right on one word or syntactic constituent at a time. Nevertheless, they could deal—recursively—with nested sentences or noun phrases (such as *the judgement of the court*), and with nouns embellished by several adjectives.

In effect, they worked top-down, continually making syntax-driven predictions about grammatical form and testing the input word to see whether it fitted the current prediction (see Boden 1988: 91–102). On encountering the word *the*, for example, the ATN would predict either an adjective or a noun, in that order; but if an adjective were found, it would immediately predict another one before looking for a noun. This simple procedure would enable it to recognize, for instance, *The girl*, *The clever girl*, and *The clever French girl*.

Woods used his parser in his LUNAR program, which answered questions about lunar geology, drawing on NASA's data about the mineral samples collected from the moon on the Apollo 10 mission. Its answers were in 'written' English (though Woods soon worked also on the HEARSAY speech system). This early version couldn't handle relative clauses in a theoretically elegant manner. Nevertheless, LUNAR was a huge advance on the question-answerers of the 1960s (surveyed in Simmons 1965, 1970).

These included the BASEBALL program, co-authored by Carol Chomsky (B. F. Green *et al.* 1961). This could search and reason about its database so as to answer questions such as "How many games did the Yankees play in July?" (see Chapter 10.iii.a). To deal with the English-language input, BASEBALL and its contemporaries had relied on a menu of ELIZA-like pattern matches (10.iii.a), not on parsing. Simple rules enabled ELIZA to switch pronouns so that (for example) the input *I ***** you* would elicit *WHY DO YOU ***** ME*. Woods's program, by contrast, modelled syntactic structure in some detail.

ATNs had originally been defined by a research group in Scotland (Thorne *et al.* 1968). Their NLP program was successfully tested on Lewis Carroll's 'Jabberwocky', among other things. It was based on transformational grammar, and built syntactic representations of the input sentence on both deep and surface levels. Woods adopted the general approach of ATNs, but eschewed Chomsky's grammar.

Nor was he the only one to do so. Because of the computational load involved in mapping between two entire syntactic structures, and thanks to the development of non-Chomskyan grammars (see Section ix), very few NLP projects have implemented transformations. The most interesting exception is Mitch Marcus's PARSIFAL, a program inspired not only by Chomsky's syntax but also by his views on language-and-mind (see Chapter 7.ii.b and M. P. Marcus 1980).

The Scottish team, like Marcus, had been doing psychological AI. They saw Chomsky's theory, and their left-to-right ATNs, as modelling some of the mental processes involved in language. Woods wasn't much interested in whether his parser was psychologically plausible. But some people used his approach to formulate specific psychological hypotheses about how people use language—and memory, too (see Chapter 7.ii.d and iv.d–e). The first to do so was Kaplan (1972), who later went on to co-author a psychologically motivated grammar, LFG, that has been borrowed extensively for technological purposes (see Section ix.b). Another was David Rumelhart, who used ATN models of grammar to study the psychology of reading errors (Stevens and Rumelhart 1975).

This swinging pendulum of intellectual influence illustrates a general point made in Chapter 1.ii.b. The boundary between psychological and technological AI, and therefore between what is cognitive science and what is not, is often fuzzy. ATNs were first devised (in Scotland) with psycholinguistic intent, soon adapted (by Woods) for technical purposes, and then picked up (by Kaplan and others) for use in psycholinguistics, memory research, and theoretical syntax. The new formal grammar that eventually resulted was later adopted by a variety of technological NLP researchers—some of whom would hardly recognize a mental process if it punched them on the nose.

A further illustration of this point was provided by the most famous achievement of early NLP: Terry Winograd's program SHRDLU (Winograd 1972; Boden 1977, ch. 6). The name, by the way, wasn't an acronym but a slyly subversive joke. In the publishing technology of the 1960s, one row of the keyboard of a standard linotype typesetting machine consisted of these letters. Typesetters would often signal a mistake by inserting them in a faulty line, so that the proof-readers would easily spot that a mistake had been made. Bad proof-reading might result in this deliberate gibberish being printed in the final text. That fact was made much of in the counter-cultural *MAD* magazine, which was deliberately peppered with multiple occurrences of this nonsense word. As a devotee of *MAD*, Winograd (1946–) decided to use it as the name of his program (T. Winograd, personal communication).

Winograd's program was written, at MIT's AI Laboratory, in the heyday of NewFAI. In 1961 George Ernst (with Minsky and Shannon on his thesis committee) had started to build a robot that would pick up blocks from a table, build them into towers, and put them into and out of a box. And by the late 1960s, the MIT robotics team were trying to integrate computer vision with their robot's problem solving, as well as working on a better robot hand (L. J. Fogel and McCulloch 1970: 256–65). It was in this intellectual environment that Winograd's doctoral research began.

The overall aim was to enable the team to give instructions to the robot in English, and for the robot to respond in real time. But for that to be possible, AI needed to advance in English as well as robotics.

Winograd's thesis title, 'Procedures as a Representation for Data in a Computer Program for Understanding Natural Language', showed that he wasn't interested only in English, or even NLP. To the contrary, he was largely concerned with questions about virtual architectures for AI. Indeed, his concepts of "heterarchy" and "procedural programming" had a huge impact on GOFAI (see Chapter 10.iv.a).

What's more, he wasn't doing psychological AI—except, perhaps, in the most general sense. Although his opening sentence was "When a person sees or hears a sentence, he makes full use of his knowledge and intelligence to understand it," he was careful to stress that SHRDLU wasn't intended as a detailed model of what goes on in people's minds.

Nevertheless, his work was considered so significant by the cognitive science community that an entire issue of the journal *Cognitive Psychology* was devoted to it—and it was simultaneously published in book form by a leading university press. In explaining their unprecedented decision, the psychologist editors said:

> Winograd's system is not a "simulation", but it incorporates important ideas about human syntactic, semantic, and problem-solving abilities, and, in particular, about their interactions in understanding natural language. Human intelligence goes far beyond this system, and human

language comprehension may turn out to differ in major respects from the means employed here. At the very least, however, Winograd's system should prove an invaluable tool for thinking about what we do when we understand and respond to natural language.

(Winograd 1972, p. vii)

Their reference to "interactions" between syntactic, semantic, and problem-solving abilities, and Winograd's opening sentence (quoted above), indicated the sense in which SHRDLU went far beyond previous NLP work.

Like Woods, Winograd provided a powerful computer parser (based on Halliday's systemic grammar: see Section ix.a). It could even parse long sentences with a syntactic sting in the tail, such as: *How many eggs would you have been going to use in the cake if you hadn't learned your mother's recipe was wrong?* But the most important NLP novelty was that the parser was closely integrated with semantic analysis, problem solving, and world knowledge (see 10.iv.a).

SHRDLU's world knowledge—of a table, a box, and various coloured objects that could be moved by a robot arm—had to be laboriously programmed in. So Bar-Hillel's argument citing *The box is in the pen* still held. As Winograd himself pointed out, many obvious implications of English sentences about boxes, blocks, and tables were invisible to the program.

This was a prime reason why, in the 1980s, he rejected the approach underlying his early work (Winograd 1980*a*; Winograd and Flores 1986). He'd been persuaded, by Fernando Flores and Dreyfus, that various neo-Kantian thinkers were better guides to language than the logicist philosophers whose views imbued GOFAI—and most computational linguistics, too (see Chapter 16.vi–viii). So his 1986 book, co-authored with Flores, highlighted the hermeneutic philosophers Martin Heidegger (sixteen index entries) and Hans-Georg Gadamer (nine), the autopoietic theorist Humberto Maturana (fifteen), and the later Wittgenstein (two). None of these had figured in his earlier work—and many of his erstwhile admirers were aghast. (For more on Winograd's volte-face, see Chapter 11.ii.g.)

The cognitive integration, in SHRDLU, went beyond the fact that syntax, semantics, reasoning, and world knowledge were all included in the one *program*. The crucial point was that they could interact in the processing of a single *sentence*. When interpreting the syntactically ambiguous instruction *Put the blue pyramid on the block in the box*, for instance, the program checked to see whether there was one and only one blue pyramid already sitting on the block, or (if not) whether there was one and only one block already inside the box. (Compare this with Quillian's semantic network, which used *purely lexical* information to disambiguate sentences like *I threw the man in the ring*: 10.iii.a.) And in obeying the instruction, and deciding how to reply to it, SHRDLU might have to do some problem solving, to enable the robot arm (which could pick up only one thing at a time) to get at the blue pyramid concerned.

With the help of predicate logic ("one and only one thing such that . . ."), also included in the program, Russell's theory of definite descriptions (see 4.iii.c) had been implemented in computational form. However, Winograd defined the program's lexicon, and its syntactic and semantic terms, not as formulae of the predicate calculus but as mini-programs. So his definition of *the* didn't simply assert (as Russell had done) that this word was apt only if there was exactly one thing that fitted the description given. Rather, it instructed the system to go and look for such a thing on encountering the word. As a result, when SHRDLU was asked to "Grasp the pyramid", there being *three* pyramids in the scene, it sensibly replied, "I don't understand which pyramid you mean." However, if a specific pyramid had already been mentioned in the conversation, it assumed that "the pyramid" referred to *that* one. Similarly for *pyramid*, whose definition was a procedure telling SHRDLU to consider—and, if appropriate, to look for—a thing of a certain sort. The syntactic definitions, of *noun*, *adjective*, and the like, were also expressed as procedures rather than assertions.

An ATN too, of course, would look for a 'suitable' word on encountering *the*, or *pyramid*, and would employ some specific procedure to find a noun, or a verb. But these syntactic categories weren't *defined* procedurally. This gave ATNs a generality that SHRDLU lacked, for *some* ATN could be devised for *any* grammar.

SHRDLU thus constituted a pioneering example of "procedural semantics", an approach to meaning and understanding that became widely influential in cognitive science. As we saw in Chapter 7.ii.d and iv.d–e, procedural semantics was taken up in studies of language and problem solving. It also affected work in practical AI (10.iv.a). With respect to technological NLP, the specifics of Winograd's parser were less influential than Woods's ATNs. But Winograd's work had shown the possibility of integrating syntax and semantics (and world knowledge, when available) in understanding language.

This general lesson could be applied by those who came after him, even if they used a different grammar and method of processing. For example, the author of the speech program whose many errors betrayed underlying anxieties (7.ii.c) singled out Winograd, for having enabled him "for the first time . . . to get a handle on how to think about and represent cognitive and linguistic processes" (Clippinger 1977, p. xv). That stuttering program was very different from the grammatically perfect SHRDLU. Nevertheless, SHRDLU was crucial in its ancestry.

Awesome though SHRDLU was, it wasn't quite so impressive as my description, above, has suggested. For one thing, the "conversation" wasn't really a conversation:

> I believe that in the process of debugging I got it to go through the whole dialog in a single run, but I wouldn't want to state that for sure. Most of the work was done in a more fragmentary manner. (T. Winograd, personal communication)

(Each fragment was relatively speedy: the system, when running compiled, was "fast enough to carry on a real-time discourse", with between 5 and 20 seconds needed for the combined interpretation and response; and the display was designed to move at the speed of a real arm: personal communication.)

For another, the system couldn't be used 'off the shelf':

> The program was at times run effectively by others (including Stu Card who was there for a summer and helped get out many of the bugs), but only after they took some trouble to become familiar with it. It wasn't at a level of robustness for walk-up users. (T. Winograd, personal communication)

In this, SHRDLU was far from unique. It was a common failing of early GOFAI programs that they couldn't be run—or developed—by others, because of bugs. Indeed, this was one of the complaints made by McDermott in his famous criticism of AI work in the mid-1970s (11.iii.a).

c. 'What I did on my holiday'

The growing interest in automatic parsing throughout the 1970s and 1980s, which fed and was fed by the theoretical work on grammars outlined in Section ix, wasn't matched in respect of automatic composition. Even a young child can write an essay on 'What I did on my holiday'. But a computer, even if it could go on holiday in the first place, would be very hard put to do so.

The output of many (non-MT) NLP programs, including SHRDLU, was based on pre-assigned templates. Although these could be syntactically varied to some extent, they were essentially similar to the 'canned' responses of very early programs such as ELIZA. Indeed, the problem of how to enable computers to generate syntactically elegant text was rarely addressed. The reasons were part-practical, part-theoretical.

For most practical purposes, the automatic composition of elegant syntax was, and still is, a stylistic luxury. Many NLP programs, such as the human–computer interfaces used by the general public, can get by with very simple syntax. Even MT programs can acceptably simplify the original syntax, provided that intelligibility is retained. Often, the original text (of technical manuals, for instance) can be written in a deliberately simple fashion without loss of meaning. And even programs that generate the text—not just the plot—of stories can 'succeed' by emulating the syntax of stories written for 5-year-olds, not of Marcel Proust.

The theoretical obstacle was a version of Bar-Hillel's problem: subtle use of syntax requires subtle world knowledge. When people use language, they typically generate syntactic structures that are both apt (with respect to the situation being described) and helpful (with respect to intelligibility). In doing so, many questions arise. For instance:

* When is clause subordination appropriate,
* and which event should be referred to by the subordinate clause?
* When are two separate sentences preferable to a single sentence in which two clauses are conjoined?
* When should one employ subjunctives and conditionals?
* How should one do so?
* And how can one choose *appropriate* referring expressions, determiners, modifiers, tense, aspect, and modal verbs?

These intriguing theoretical questions were investigated in the late 1970s by Antony Davey, who related them to various psycholinguistic phenomena. Here, the relevant point is not the psychology, but the fact that he implemented some of his answers in an NLP program (Davey 1978). He showed how syntax could be used, in a principled way, to signal the strategy driving individual games of noughts and crosses (tic-tac-toe).

Thus his program produced the following description of the game depicted in Figure 9.5:

> I started the game by taking the middle of an edge, and you took an end of the opposite one. I threatened you by taking the square opposite the one I had just taken, but you blocked my line and threatened me. However, I blocked your diagonal and threatened you. If you had blocked my edge, you would have forked me, but you took the middle of the one opposite the corner I had just taken and adjacent to mine and so I won by completing the edge.

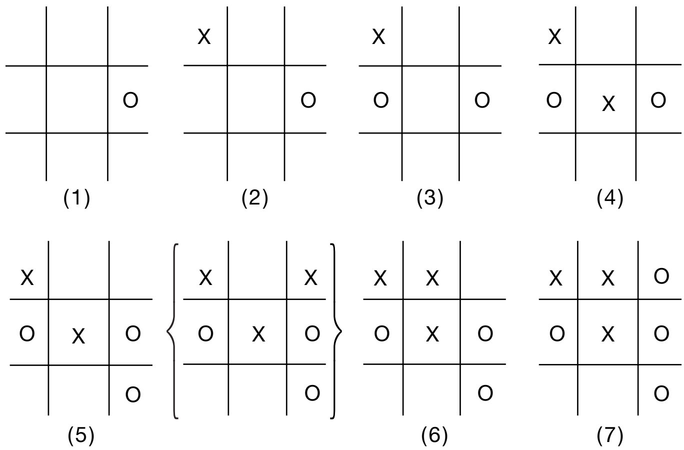


Fig. 9.5. Game of noughts and crosses (tic-tac-toe). The move in brackets wasn't actually made, but was 'imagined' by the program when it described the game depicted in the numbered frames. (Based on an example in A. C. Davey 1978: 18.)

No post-editing was done by Davey: this passage was the actual output of his program. The syntax nicely reflected the structure of attack, defence, and counter-attack informing the game, and helped distinguish strategy from tactics. For instance, it would have been less apt to reverse the last two ideas in the second sentence (as in "... but you threatened me and blocked my line"), or to start it by saying "I took the square opposite the one I had just taken and so threatened you." Many other superficially equivalent alternatives were specifically—and rightly—avoided by the program.

This was the first automatic system capable of generating complex, semantically appropriate, syntactic structures. As such, it was a significant achievement. But, for the reasons outlined above, it had little influence on the development of NLP. Its unprecedented power was unnecessary in most practical contexts, and wasn't achievable in the general case—for composing essays on holidays, for example. Davey had chosen noughts and crosses as his domain precisely because the rules are easily statable, the games are fully describable, and the strategy is readily intelligible. It would be difficult, and often impossible, to adapt his program to other domains.

Different approaches to grammar generation were sometimes considered. For instance, rules governing shifts in the focus of attention (see below) were used in choosing the active or passive voice, pronouns or definite descriptions, and the sequence of sentences within a paragraph (McKeown 1985). But these didn't match the detail of Davey's work. Consequently, today's NLP programs are asymmetrical in their grammatical abilities, much as SHRDLU was. Their expertise in parsing belies their simple-mindedness in sentence construction.

d. Semantic coherence

The maturation of NLP also required a concern for the semantic coherence and rhetorical structure of entire texts. Let's consider the first topic here (the second is discussed in subsection f).

Impressive though it was, Winograd's program had virtually no ability to deal with a text, as opposed to a series of single sentences. Admittedly, anaphora and definite descriptions were interpreted by noting what objects had been referred to in previous sentences. But there was no representation of the theme of the conversation as a whole, nor any prediction as to what the human might say next.

Still less was there any systematic representation of conversational etiquette, or of the distinction between the words uttered and the communicative intention behind them. Eventually, and partly driven by the problems involved in designing usable natural-language interfaces, these issues of semantics and pragmatics came to the fore (see 13.v).

One early attempt to identify the theme of an entire text had been made by Wilks, using CLRU's thesaurus approach (see Section x.d, above). Another, even earlier, was the "General Inquirer", initiated around 1960 by Philip Stone (1936–) at Harvard and colleagues (Stone *et al.* 1962). The method relied on an "affective" classification of the English dictionary, based on tags drawn from Fred Bales's small-group psychology. For example, *union*, *cooperation*, and *love* were coded under the Affiliation tag, whereas *fight* and *war* were classed as Hostility. Counting tag words was straightforward. And hierarchical structures, wherein one semantic tag seemed to be providing the context for another, could be found by list processing—using Yngve's new list-processing language, COMIT.

This approach provided only a very broad analysis—and could be highly misleading, to boot. The computer analysis might show Affiliation governing Hostility, for instance, or Hostility governing Affiliation. However, to decide whether these seeming contradictions reflected actual incoherence, as opposed (for example) to a threat followed by conciliatory remarks, the researcher would have to go back to the text itself.

I myself used a prototype version of the Inquirer in October 1962, as a student in Stone's team analysing the ongoing Khrushchev–Kennedy interchange about the Cuban missile crisis. (The texts were being made immediately available to us in electronic form by an international press association.) The hope was that implicit affective changes in Nikita Khrushchev's attitude might be picked up even before they'd been made explicit by him.

The attempt wasn't successful. For example, Khrushchev's speeches, but not John Kennedy's, scored very high on Affiliation. On checking the original transcripts, the explanation was clear. What Kennedy called "the USA" and "the USSR", Khrushchev (or his translator) called "the *United* States" and "the Soviet *Union*". The problematic words, of course, were then removed. But they might not have been identified in the first place, if the initial scorings hadn't been so highly counter-intuitive. In addition, it was unclear what significance—if any—should be attached to the fact that frequency counts showed one tag hierarchically dominating another.

Both Wilks's system and Stone's suffered from a broad-brush approach. They could identify general themes and overall affective tone, respectively, but couldn't follow the development of a theme from one sentence to the next. And there was no question of their being able to predict the likely content of sentences yet to come.

The first people to model these matters were the social psychologist Robert Abelson and the computational linguist Roger Schank. One reason for their collaboration was that Schank was doing psychological, not technological, AI. He did eventually produce a number of commercially marketed NLP tools, but he saw them—correctly or not—as reflecting how humans process language (see Chapter 7.i.c and ii.d).

By the mid-1960s, Abelson (at Yale) had simulated individual systems of political beliefs, predicting responses to a variety of new inputs. Soon afterwards, he outlined a computational theory of social roles, of motivational–emotional "themes", such as betrayal and cooperation, and of "scripts", such as escape (see Chapter 7.i.c).

By this time, Schank—then at Stanford, though soon to join Abelson at Yale—was doing NLP based on his conceptual dependency (CD) theory. This combined a semantic primitive analysis of verbs with a version of case grammar (Schank 1973; Boden 1977, ch. 7). He aimed not only to represent the meaning of an input sentence, but also to predict the likely topic/s of the following sentences. The verb *hit*, for example, involves an "instrumental" case. If this case slot was left open (as in the sentence *John hit Mary*), the CD representation enabled/predicted the follow-up query "What with?"—where the default inference was that the instrument was the actor's hand.

In their collaborative work at Yale, Schank and Abelson (1977) used the term "scripts" in a new way, to represent the behaviour of various actors in culturally stereotyped situations. Their restaurant script, for example, codified the roles of customer, waitress, and cashier in a hamburger bar. From the early 1970s, Schank and his students combined CD theory with this new theory of scripts to generate questions and answers about (specially composed) texts.

For instance, Wendy Lehnert (1978) wrote a question-answering program that integrated CD representations of verbs with world knowledge about hamburger restaurants. Given a mini-story including the sentence "When the hamburger arrived, it was burnt", the program would infer—what was not made explicit in the input—that the customer who had ordered it neither ate it nor paid for it. These texts provided the story snippets that would soon be used as examples by John Searle (1980) in his discussion of the Chinese room (16.v.c).

Meanwhile, Jim Meehan at Yale, and researchers in other places, had been using Schankian ideas in modelling the generation and summary of simple story plots (Meehan 1975; Rumelhart 1975; Schank and Riesbeck 1981; Lehnert and Ringle 1982). And up to the early 1980s, Schank's student Michael Dyer (at Yale and then UCLA) considered more complex types of story, involving developments of Abelson's ideas about motivational and emotional structures (Dyer 1983).

These Yale-based programs combined linguistics with GOFAI, for they relied heavily on AI ideas about planning in interpreting texts. In this, though in little else, they resembled Winograd's SHRDLU.

For example, a key concept in Dyer's NLP approach was the TAU, or thematic abstraction unit. TAUs were used to organize memory, direct the process of understanding, support analogical reasoning, and enable one story to remind the system of a different one. They were defined as abstract patterns of planning and plan adjustment, involving aspects such as enablement conditions, cost and efficacy, risk, coordination, availability, legitimacy, affect, skill, vulnerability, and (legal) liability. Those dimensions were used by the system to recognize individual episodes in the text as examples of one TAU or another, and different story plots would involve different sets and/or sequences of TAUs. The TAUs included (for instance) *incompetent agent*, *a stitch in time saves nine*, *too many cooks spoil the broth*, *red-handed*, *hidden blessing*, and *hypocrisy*.

As these labels indicate, TAUs were abstractly defined as general planning schemata, applicable to a wide range of texts. As such, they could in principle be used to describe the internal coherence of many different stories. But because of the specific world knowledge needed to consider individual plans in actual texts, Dyer's approach in practice fell foul of the Bar-Hillel problem. (It also fell foul of the McDermott problem: using meaning-rich English phrases, such as 'a stitch in time . . .', as the names for semantically impoverished technical terms: see Chapter 11.iii.a.)

Dyer's program had to be specially designed to anticipate a specific range of stories—which in turn usually had to be specially written. So this approach to NLP, or to knowledge representation in general, was limited in terms of its potential applications. If the text domain wasn't laboriously mapped onto a specific set of planning problems, a Schankian program couldn't generate questions and answers appropriate to it.

In Winograd's (unpublished) invited lecture at the 1973 international AI conference in Los Angeles, when his own name was still on the lips of everyone interested in cognitive science, he pointed out—with characteristic modesty and intellectual generosity—that Schank was addressing important problems which he himself hadn't touched. As for Abelson's work, this was being recommended at much the same time in Minsky's influential, and widely pre-circulated, paper on "frames" (see 10.iii.a). A few years later, Minsky (1977, 1985) would draw on Abelson's ideas again, and on Schank's as well, in his "society" theory of mind (see 12.iii.c).

In short, Schank and Abelson's ideas eventually influenced AI in general (and psycholinguistics too: 7.ii.d), not just NLP.

e. The seductiveness of semantic networks

To say—as Winograd did—that Schank was addressing important problems isn't to say that he was doing so successfully. Quite apart from doubts about semantic primitives in general (see Section viii.c), his own list wasn't drawn up in a principled fashion. It dropped from fourteen to twelve, to eleven. Moreover, his work suffered from an ailment that was all too common in the AI of the time: a logical and linguistic vagueness, grounded in the intuitive use of semantic links.

This charge may seem strange, since computer models can't function by intuition. Any semantic link implemented in a computer program must have some specific definition. But the case is different when one considers the human researcher's interpretation of *diagrams* of semantic networks, such as Figures 9.6 to 9.8. Many such diagrams adorned the NLP literature of the mid-1970s. They flourished in the AI work on knowledge representation, too (see Chapter 10.iii.a). Indeed, they'd been originated by Quillian (1961, 1968) to model the structure of long-term memory.

Quillian's networks represented conceptual associations of *various* kinds, including class membership and similarity (see 10.iii.a). His general approach was taken up by many NLP researchers, including Schank, to represent the meaning of sentences. (It

$$\text{John} \Leftrightarrow \text{hit} \overset{o}{\Leftarrow} \text{dog}$$

FIG. 9.6. Conceptual-dependency diagram representing "John hit the dog." Reprinted with permission from Schank and Colby (1973: 193.)

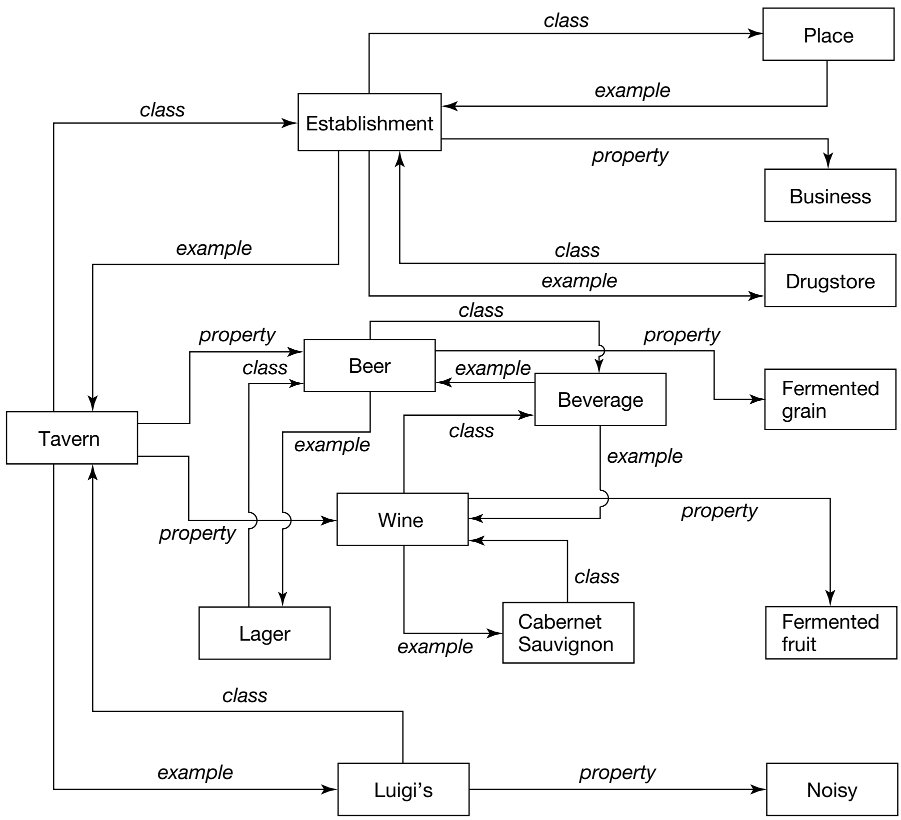


FIG. 9.7. Semantic network representing the concept of "tavern". Redrawn with permission from Lindsay and Norman (1972: 389)

was also applied to vision and to concept learning, by Patrick Winston and John R. Anderson, for instance: see 10.iii.d and 7.iv.c.) But, as semantic networks became increasingly popular, W. A. Woods (1975) rang a warning bell.

Woods pointed out that these representations were usually ambiguous, and—in their naive form—ill-suited to capture many crucial semantic distinctions. In particular, there were many problems concerning relative clauses and quantification:

> [When] one does extract a clear understanding of the semantics of the notation, most of the existing semantic network notations are found wanting in some major respects—notably the representation of propositions without commitment to asserting their truth and in representing various types of intensional descriptions of objects without commitment to their external existence, their external distinctness, or their completeness in covering all of the objects which are presumed to exist in the world. I have also pointed out the logical inadequacies of almost all current network notations for representing quantified information and some of the disadvantages of some logically adequate techniques. (W. A. Woods 1975: 79–80)

Additional problems that needed to be addressed, he said, included (among others) the representation of mass terms, probability, time, tense, and the use of adverbial modification.

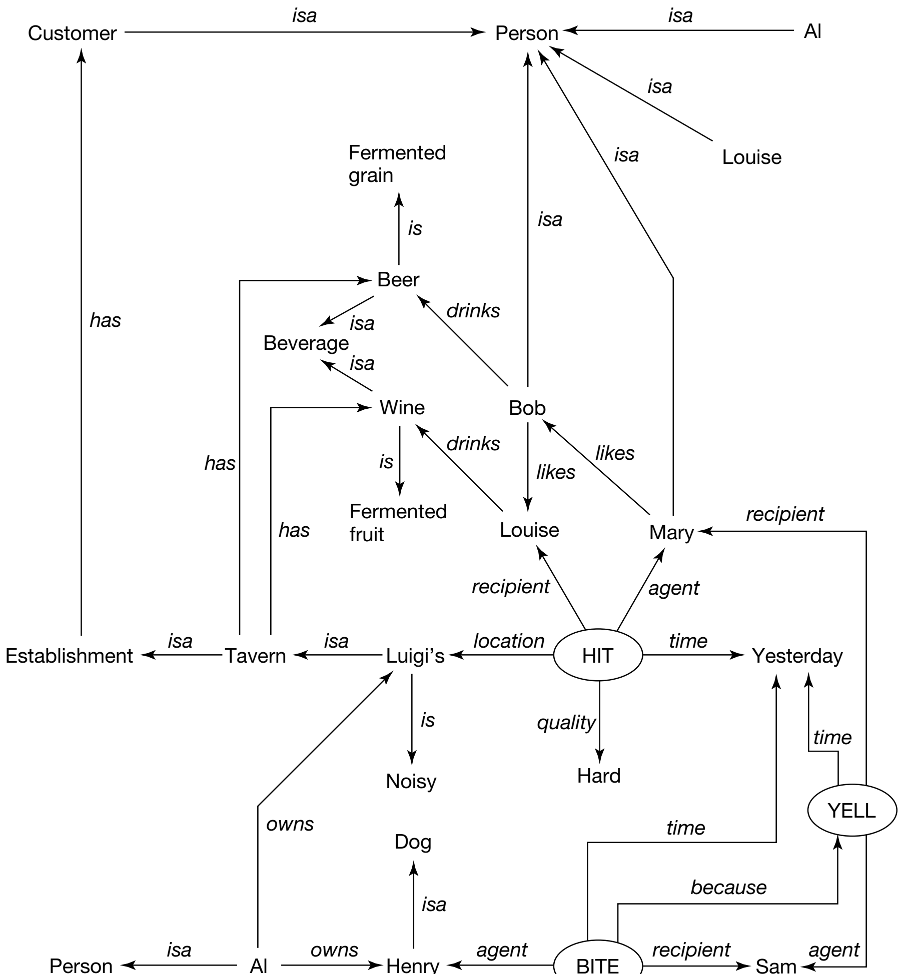


FIG. 9.8. Semantic network representing data about several people—and a dog. Redrawn with permission from Lindsay and Norman (1972: 400)

The justice of Woods's critique can be illustrated by the Schankian CD network shown in Figure 9.6. The link between "hit" and "dog" is labelled as being one of a specific kind, denoting the objective case. And Schank glosses this diagram accordingly, as being equivalent to "John hit the dog". But it could equally well be interpreted as "John hit *a* dog". If Winograd's SHRDLU had relied on this type of representation, it could not have queried the meaning of "Grasp the pyramid" when no specific pyramid had been indicated, nor interpreted that same phrase correctly later in the conversation. Moreover, if even this simple diagram is ambiguous, then one may expect more complex ones (such as Figure 9.7) to be highly problematic.

Where Schank's system was concerned, semantic ambiguity wasn't the only weakness. The system's syntactic power was grossly limited in comparison with that of SHRDLU, or of Woods's ATNs. It couldn't cope with complex auxiliary verbs and nested sentences, as in the query concerning the eggs in the cake (see above). And because it had no representation of logic, it couldn't interpret quantifiers such as *all* or *some*—nor, as we've just seen, definite versus indefinite articles.

Accordingly, those NLP workers who had a healthy respect for logical semantics and grammatical niceties weren't much influenced by Schank's work. Rather, they paid careful attention to syntax, and tried to fill the semantic gaps listed in Woods's critique.

Woods himself, for example, soon improved his LUNAR system in many ways, such as enabling it to interpret the natural-language quantifiers *all*, *every*, *some*, and *most* (W. A. Woods 1978). And others soon improved ATNs by defining a procedure (the "HOLD" function) that enabled them to parse relative clauses in an efficient, and theoretically elegant, way (Wanner and Maratsos 1978). From the early 1980s on, much NLP research implemented highly detailed syntactic theories such as GPSG and its computational cousins (see ix.d–f, above). And some developed theories of logical semantics broadly inspired by Montague, but better fitted to the constraints of actual language use (e.g. Barwise and Perry 1984).

f. Whatever will they say next?

The actual *use* of language involves not only syntax and semantics, but pragmatics too. This became increasingly clear through the 1970s, as computational linguists tried to design human–computer interfaces for interactive tasks (13.v). These included querying databases, planning (of travel itineraries, for instance), and giving/seeking advice on ongoing action (as in repairing a car).

Such applications require a rhetorically coherent conversation, as opposed to a string of isolated sentence pairs. Moreover, the human should be free to use language in everyday ways, and the computer should converse 'naturally' also.

Accordingly, NLP researchers in the mid-1970s began to model how people plan what they are going to say, how they use language to do things other than state facts, how they give or request information in a way appropriate to the interlocutor's state of mind at that moment, and how they keep track of shifts in conversational focus. In doing this, they drew on previous work in the philosophy of language.

Both Wittgenstein (1953) and John Austin (1962*a*) had shown that language can be used to do many different things, and Searle (1969, 1975) had recently formalized some of Austin's ideas. Austin (1911–60) had pointed out, for instance, that some verbs are used not to state facts but to promise, to command, to warn, or to marry. (Consider "I'm warning you . . .", "I promise", and the bride's "I do".) He believed that there's a finite number of speech acts, perhaps a few hundred, in terms of which language use could be systematically classified.

However, Austin (and Searle) didn't assume a one-to-one match between words and speech acts, because people often use language well suited to one speech act in order to perform another. For example, the verb "I suggest" can be used not to suggest, but to command. An indicative sentence can be used to ask a question ("I don't know the time"), or to make a request ("It's cold in here"). And a Yes–No question can convey a command ("Can you pass the salt?"), or elicit substantive information ("Can you tell me the way to the station?").

It follows that human speakers can—and interactive computers should—not only recognize different speech acts, but also distinguish the literal meaning of the sentence from the communicative intention of the speaker. Consequently, Searle's theory of speech acts was applied by Philip Cohen (a colleague of Woods at BBN) and Raymond Perrault to the computer modelling of conversation (P. R. Cohen and Perrault 1979).

The intellectual benefits flowed in both directions. This NLP work uncovered some flaws in Searle's theory, which was later developed—and axiomatized—as a result (Gazdar 1981*b*; Searle and Vanderveken 1985).

Cohen and Perrault drew on other philosophical work, too. H. Paul Grice (1913–88) had noted that people choose what to say—and what to leave unsaid—by considering not only truth, but also helpfulness, relevance, and brevity (Grice 1957, 1967/1975, 1969). These "conversational postulates" are involved also in understanding someone else's remarks. If they are clearly contravened, the remark will be interpreted differently: as a joke, or sarcasm, for instance. In order to apply them, both speaker and hearer must consider each other's beliefs and interests. What is helpful to one person may be unnecessary or confusing for another, or for the same person at a different time. (Grice's seminal work eventually led to a more elaborate theory of "relevance": see Chapter 7.iii.d.)

An interactive NLP system, then, should be able to adjust its output to suit different human individuals, or one individual at various points in the conversation. It should also include procedures for recognizing and correcting *misunderstandings*—on its own part, or the human's.

In implementing Grice's ideas, Cohen and Perrault used AI planning (Chapter 10.iii.c) to show how the multilevel plans of two conversationalists can be mutually adjusted so that communication takes place as intended. Each of these plans included a (continually updated) model of the other speaker's beliefs and intentions. (One might say that the programmers had sketched a "Theory of Mind" for AI systems: see 7.vi.f and 13.iii.e.) And each plan was used to guide the way in which different topics were 'naturally' introduced, or dropped, as the conversation progressed.

Similarly, Richard Power (1979) modelled the ongoing conversation between two robots cooperating in opening a door, where the relevant information wasn't directly available to both of them (see Chapter 13.iii.e). Only the robot on the bolt-side of the door could see whether the bolt was up or down. Opening the door involved problem-oriented planning concerned with how to get the door open (by moving the bolt). It also required *communicative* activities such as:

* attracting the other robot's attention;
* suggesting,
* and then negotiating, an agreed common goal
* (followed by a series of sub-goals);
* communicating or requesting information accessible to only one robot;
* confirming that this information has been duly noted;
* instructing another agent to do something which one cannot do oneself;
* . . . and so on.

From time to time, a robot would have to find out what the other robot knew and compare that with its own knowledge. This required processes that modelled each robot's changing knowledge and its (continually updated) model of the other's state of mind.

Related work was being done at much the same time by Barbara Grosz (1977; Grosz and Sidner 1979). For instance, she modelled the changing reference of terms such as "it" as the conversational focus shifted. As remarked above, focus shifting was later used in generating appropriate syntax, as well as in planning *when* to talk about *what* (McKeown 1985).

One of the general trends in 1980s NLP research was to integrate the two non-linguistic constraints of *attention* (related to focus of interest) and *intention* (related to plans and goals). An interdisciplinary workshop devoted to this aim was held in Monterey, California, in 1987 (Cohen *et al.* 1990). Many of the papers presented there showed how far NLP had moved on since Power's model of the conversation between the door-opening robots. Others were highly relevant, also, to AI work on planning—and on plan recognition, which involves the identification of the other agent's beliefs and intentions.

Several of the Monterey talks were devoted to one or another aspect of speech act theory. And Searle himself gave a paper in which he argued that "collective intentions" can be formally modelled, but can't be analysed in terms of "individual intentions" (Searle 1990*c*). Indeed, some of his remarks could be paraphrased as asserting the existence of something akin to a "group mind" (see 8.iii, preamble).

In addition, Grosz's (and other) work on the structure of conversations would eventually feed into AI teaching programs (Rickel *et al.* 2002). This enabled (for instance) virtual-reality agents to converse with human technicians about how to operate complex machinery (Chapter 13.vi.b).

None of these examples, of course, could match the human use of language. Never mind fancy syntax. The following interchange (Nicholas Negroponte's ultimate goal for NLP) could well make sense for two intimate friends, but would challenge all but the ideal example of "personalized" computers:

"Okay, where did you hide it?"

"Hide what?"

"You know."

"Where do you think?"

"Oh."

(quoted in Brand 1988: 153)

Evidently, sensible conversation depends on much more than *language*. It often needs knowledge of the situation, and of the speakers, too. Flawless computerized conversation is as unattainable as "perfect" translation and interpretation, at least in the general case.

Nevertheless, the attempt to attain these unattainable goals, and to enable computers to parse sentences of indefinite complexity, led to results of both theoretical and practical importance. Some relevant psychological research was discussed in Chapter 7.ii, and the benefits for philosophy included advances in speech act theory (see above). As for the practical aspects, applications burgeoned.

By the mid-1980s, a fairly superficial survey of "technological" NLP required no fewer than 460 large pages (T. Johnson 1985). Now, it would need many more. For significant progress has been made since then. Today's survey, in addition to updating the older topics, would need to add work on non-verbal pragmatics: not just inflection and tone of voice, but eye-gaze, hand gestures, eyebrow movements and other facial expressions, and general body posture.

All these things, which involve significant cultural variations (think of the Indian way of indicating "Yes": not by nodding, but by inclining the head from side to side), help us to interpret conversations in real life. And all are being mimicked, with more or less success, in applications of virtual reality (VR)—see below, and Chapter 13.vi.

g. A snippet on speech

Some of the technological advances in NLP over the past thirty years concern speech processing, which I've chosen not to discuss. Suffice it to say that a great deal was learnt from the blackboard-based early speech-understanding systems, such as the single-user HEARSAY and its speaker-independent successor HARPY (Reddy *et al.* 1973; Newell *et al.* 1973; Erman and Lesser 1980; Lowerre and Reddy 1980). Indeed, one of today's most successful commercial applications, a voice-recognition program marketed by Dragon Systems, was developed by Janet Baker—whose DRAGON program was a component of HARPY (J. K. Baker 1975).

Many of the difficulties that plagued these pioneering attempts (for an overview, see Barr and Feigenbaum 1981, ch. 5), and which led DARPA to halt their funding in 1976, have now been overcome. This has been done largely by combining rule-based knowledge (of phonological tree structures, for instance) with statistical pattern recognition, and by strengthening the phonetics (e.g. Rowden 1992, chs. 6–8; Stolcke 1997; P. A. Taylor 2000). Sometimes, speech recognizers have relied on self-organized "maps" of quasi-phonemes, rather than built-in phonetic representations (Kohonen 1988; see 14.ix.a).

The results have benefited both speech recognition and speech generation. They've also allowed for these to be combined in speech-to-speech translation.

Today's commercially available speech recognition systems can accept menu-based inputs from virtually any speaker on first hearing, so are widely used in telephone-answering services. HEARSAY's notorious slowness—which caused some disillusionment with AI in official circles at the end of the 1970s (Klatt 1977)—is a thing of the past. (Current speech recognizers rely on adaptive statistical models, not on GOFAI: cf. Chapter 12.) Some inexpensive desktop systems today allow one to dictate at normal speeds, without any unnatural pauses between the words. Their powers of recognition are flexible, to some degree: they can learn extra items to enlarge their vocabulary, and adapt to the individual accent of the user.

Visual speech recognition may be feasible in the near- to mid-future, since only sixteen lip-movements are needed to represent English—all of them used in saying *I thought you really meant it*. Already, "talking heads" can be generated (to 'read' input text) which pronounce words with highly effective lip-synchronization (Lucena *et al.* 2002). The sixteen lip positions may be generic, but can also be based on photographs of a specific person's face (cf. Chapter 13.vi.e).

Speech translation is progressing too. A program is being marketed which can deal with telephoned conference registrations, switching between English, German, and Japanese. Research throughout the 1990s sought to aid face-to-face commercial transactions in English between Germans and Japanese who don't speak the language fluently (W. J. Hutchins 1994, sect. 8). Indeed, the new century saw a detailed report on a speech-to-speech translation system which, unlike its few rivals, works in real time. It's not yet (as I write this page) commercially viable—although it may be, by the time you read this book. But a trustworthy reviewer has described it as highly promising, and refreshingly free of hype (Sampson 2001). This achievement is a long way from HEARSAY, whose linguistic knowledge often outweighed its phonetics—so much so, that it might emit a whole sentence in 'reply' to a cough.

Even virtual reality is taking speech on board (see Chapter 13.vi.d–e). In some computer games, one can ask a dragon, "Can you fly?"—and it does. This VR dragon is no SHRDLU: it's merely recognizing the word *fly*. But its future descendants may be able to use syntax and speech act theory to interpret the input sentence either as a question *or* as a request. Certainly, plausible interfaces for virtual reality will demand no less.

Meanwhile, a system called TESSA has been developed for the British Post Office to enable counter-clerks to communicate with deaf people whose native language is BSL, or British Sign Language (Cox *et al.* 2002). This program translates the clerk's speech into sign language, which is visibly expressed as the finger movements, bodily gestures, and facial expressions of a VR avatar. (The screen-creature even manages to look "rueful" when explaining that there are no more first-class stamps.)

By early 2003, about 400 pre-designed translations were being used by the avatar. The team had discovered early in their research that human clerks can't be trained to speak *only* the precise words defining these 400 phrases. So TESSA listens to what the clerk says and then offers a menu of up to half a dozen 'equivalent' English sentences, from which the person chooses one. It's that one which is displayed in BSL form via the avatar. In short, TESSA combines speech recognition with MT and advanced computer graphics.

Automatic speech synthesis is improving also. The first example, Ray Kurzweil's reading machine for the blind (gratefully used by Stevie Wonder, among others), was announced in January 1975. It was immediately featured on America's TV news: Walter Cronkite even allowed it to read the sign-off message for that evening's newscast. Given that it could read virtually any printed font, it was a superb achievement at the time. But it was most unnatural to listen to. The voice of its Xerox successor (Kurzweil sold the company to Xerox in 1980) is a good deal more pleasant.

In addition, there are now many more types of AI-voice application. For instance, a system recently developed for use in a hospital cardiac unit employs near-naturalistic intonation to reflect the relative importance of many different items of medical data. These are passed automatically from the operating theatre to the nurses busily preparing for the patient's arrival in intensive care (McKeown and Pan 2000). And in March 1999, my daily newspaper reported that the physicist Stephen Hawking (current holder of Isaac Newton's and Charles Babbage's Lucasian Chair) had acquired a new speech synthesizer, much more 'human'—and more British—than the one he'd been using for many years.

By the time this book is published, the choices available will doubtless be better still. I say that not just because technology inevitably moves on, but because of a specific research advance. By the close of the century, NLP researchers at Edinburgh University had produced a "dialect independent" lexicon suitable for synthesizing spoken English in many different local accents (Fitt and Isard 1999; Fitt 2003).

In this lexicon, which is based on the work of the descriptive linguist John Wells (1982), speech sounds are classified in terms of the words in which they occur—as in the "boot" vowel, the "foot" vowel, and so on. The description of a dialect for synthesis purposes specifies which of these vowel classes to merge (for instance, the vowels of "father" and "bother" for many American dialects) and which to keep separate (such as the vowels of "tide" and "tied" in most Scottish dialects). The sounds are 'glued together' to form the sentences required—"sentences", not "words", because the pronunciation of a particular word often varies according to the word that follows it.

The sounds themselves aren't synthesized from scratch, but are retrieved from the recorded speech of someone asked to read a script containing examples of each of the sounds required. In May 2004 the accents that had been synthesized in this way included received pronunciation of British English, several American dialects, and a Scottish, an Irish, and an Australian dialect (S. D. Isard, personal communication). More can, and will, be added. The collection of key phonetic symbols (in 2004, just thirty-four vowels and twenty-seven consonants) may turn out to be adequate. But it can be expanded if necessary.

Whether Hawking would be pleased to hear of this advance—which was licensed for commercial use in 2001, and is already being employed in the 'rVoice' product—is unclear. The newspapers in April 2004 reported that he was distressed about his familiar voice system wearing out, as he felt that its Dalek-like tones had become part of his identity. (As he put it when Intel Chairman Gordon Moore donated £7.5 million for a new science library in Cambridge, "I'm Intel inside myself"—Mialet 2003: 453.) In other words, he seems *not* to want his speech synthesizer to pass the Turing Test (16.ii.c).

The Edinburgh-based synthesizer doesn't quite do that, anyway. For example, dialects also vary in intonation and segment duration, but these features (which are independent of the lexicon) aren't represented in the system. So although it's clear that *this* synthesized voice is Scottish and *that* one Australian, neither sounds quite right to anyone with a good ear for accents. Nevertheless, speech processing has come a long way since HEARSAY.

From artificial voices to artificial intelligence: our next topic is the history of AI. *This* chapter, of course, has already begun that narrative. Language may be less central to intelligence than early AI took it to be (see 13.iii.b and 15.vii.a). Nevertheless, NLP is crucial to the AI project. And Chomsky's wider influence affected AI, as well as psychology and philosophy—as we'll now see.

10

WHEN GOFAI WAS NEWFAI

Difficult though it may be to believe, even one's grandparents were young once. They too knew the excitement of youth, and that first fine careless rapture. Likewise, even "good old-fashioned AI" was newfangled AI, once. And if not quite rapturous, certainly very exciting. (Sometimes careless too, as we'll see: 11.iii.a.) In short, GOFAI started life as NewFAI.

John Haugeland's GOFAI label (1985: 112–13) carries patronizing overtones absent from its alternatives: "symbolic", "classical", or "traditional" AI. It suggests a dismissive attitude all too often directed to this area of cognitive science, not to mention grandparents. My own label, NewFAI, also has a negative air. (For the record, it's my coinage, dating from the early 1980s; but others have picked it up and used it in print.) The word "newfangled" is an ancient one: I was taken aback, on a plane trip a few years ago, to encounter it in *The Canterbury Tales*. Geoffrey Chaucer used it to mean simply something *new*, but since then it has acquired a depreciatory tone. For several centuries past, according to the *OED*, it has implied *the love of novelty for its own sake or for purposes of gaudy show*. In our times, it also carries a hint of addiction to technological gizmos. All in all, then, it's hardly flattering.

My justification for using the label here is not that early AI was essentially meretricious. It wasn't. It did, however, often involve over-enthusiastic appraisals, over-optimistic predictions, premature efforts to address very complex issues, and—it must be admitted—self-deception or even dissimulation in describing what AI programs could do (see Chapter 11.iii). In brief, excitement led to overexcitement.

However, the excitement didn't spring from a shallow fascination for novelty or gadgets. It was grounded in an ambitious intellectual vision, which saw the future of theoretical psychology as the design of computational models isomorphic with (all kinds of) mental processes. Clearly expressed by Warren McCulloch and Walter Pitts in the 1940s (4.iii.e), this vision was echoed by several others in the 1950s (6.iii.c and iv.a–b). Hubert Dreyfus later traced its philosophical roots as far back as Plato (16.v.c). In its computerized incarnation, however, it was refreshingly new.

Moreover, it was seemingly endorsed by one of the first working AI programs, which found a proof for a theorem in *Principia Mathematica* more elegant than the one given by its eminent authors (6.iii.c, and i.b below). Small wonder, then, that people were excited.

A caveat, before we begin: It's not clear that all NewFAI researchers believed human thinking to be *constituted by*, or *identical with*, symbolic processes. Perhaps most did, but this was rarely made explicit. (For an early exception, see Newell and Simon 1961.) Even Alan Turing had sidestepped the issue (16.ii). "Strong" and "weak" AI (Searle 1980) hadn't yet been named, and the distinction wasn't always recognized by people at the NewFAI coalface. However, we don't need to worry about these matters until later (Chapter 16). When I speak of NewFAI/GOFAI, I'm referring to a programming methodology, not to the researcher's philosophical position. (So my usage differs from Haugeland's: in his definition, "GOFAI" includes a commitment to strong AI.)

Long though it is, this chapter would have been even longer had not much of the relevant research been discussed already. Natural language processing (NLP) was described in Chapter 9.x–xi; any NLP programs mentioned here are considered only for their relevance to AI in general. Similarly, NewFAI models of belief change and/or personality and emotion (see Chapter 7.i.a–d) are here ignored. And Allen Newell and Herbert Simon's initial work on problem solving was described in Chapter 6.iii.

The very earliest NewFAI research is described in Section i. The first AI labs and publications are noted in Section ii. Sections iii and iv, respectively, deal with the early search for generality and the later use of world knowledge in AI programs. (If asked to locate the maturing of NewFAI into GOFAI, I'd place it there.) And last, in Sections v and vi, I explain why much of the creative effort of NewFAI went into the design of new programming languages—including one which later inspired the revolutionary technology on our desks (and laps) today.

This chapter focuses on work in symbolic AI done before 1980. (In Chapter 12.vi–vii, we'll see why 1980 is a reasonable cut-off point.) As for how GOFAI fared after 1980, that story is taken up in Chapter 13. We'll see there that much of what we regard today as highly modernistic information technology was specifically inspired by visions first expressed within grandparent—or, in one case, great-grandparent—NewFAI.

10.i. Harbingers

The first paper in cognitive science, McCulloch and Pitts' 'Logical Calculus of the Ideas Immanent in Nervous Activity' (1943), was neutral as between symbolic AI, connectionism, and cybernetics. The cybernetics/symbolics schism happened later (Chapter 4.ix), largely driven by the 1950s harbingers of NewFAI.

These mid-century programs and programmatic visions implied that symbolic AI had a bright future. For some toe-in-the-water programs were capable of doing surprisingly 'intelligent' things. Moreover, the symbolic approach was better suited than the early network models to represent goal-directed action and propositional meaning. AI, it seemed, could model—maybe even match—human thought.

a. When is a program not a program?

If Gertrude Stein had said "A program is a program, is a program . . . ", she'd have been wrong. Or anyway, she'd have been wrong if she'd said this in the 1950s. For the sense in which people used the term shifted over the years.

That's not to say that there's a right sense and a wrong sense: most concepts don't have clear semantic boundaries, and this one is no exception (see 8.i.b and 9.x.d). Even today, people sometimes disagree about whether something is a program or merely an outline for a program, and about whether AI researchers should honour only the former. But the historical point, here, is that what NewFAI people were happy to count as a program changed, as implementation became more feasible.

The strong sense of *program* is a list of instructions which can—repeat, *can*—be run on some computer. (Never mind bugs: a buggy program is a program, in this sense.) But a weaker sense would cover a list of rules or instructions *written on paper and 'executed' by hand*. Such a program may not be capable of being fed to a computer. Even a *programming language* may be a purely paper-and-pencil exercise, if it hasn't yet been provided with a suitable compiler/interpreter (e.g. Newell and Simon's "Logic Language": Section v.b, below).

A yet-more-attenuated sense of the term would include a broad *design* for a program, without specification of the individual rules. In that sense, the first AI programs date back to the Second World War, when Turing—as early as 1941—wrote about how to solve problems by searching through the space of possible solutions, guided by what would later be called "heuristics". He even circulated a typescript on these matters to some of his Bletchley colleagues, including Donald Michie. Jack Copeland and Diane Proudfoot (2005: 15) describe this lost typescript as "undoubtedly the earliest paper in the field of AI", and I agree with them. However, an AI *paper* isn't the same as an AI *program*. For something to count as a program, even in the weak sense defined above, it needs to specify individual instructions, in some suitable order.

In the 1950s, weak-sense programs were often important. One example was what's *still* often referred to as "Shannon's chess program". In the late 1940s, Claude Shannon identified many of the basic problems which would have to be faced by anyone programming a computer to play chess (Shannon 1950*a*,*b*). This was highly influential work. Some ten years later, Newell and Simon acknowledged that "the framework he introduced has guided most of the subsequent analysis of the problem" (1958: 42). But their word "framework" was well chosen, for Shannon *didn't even provide a paper-and-pencil program*. What his 1950 papers provided, rather, was an abstract analysis of the task. (A paper-and-pencil chess program had already been written by Turing, but this wasn't yet well known—1947*a*: 23, 1950. It was implemented in 1951. It lost against a weak human player, was "rather aimless", and made "gross blunders"; however, it "reached the bottom rung of the human ladder"—Newell and Simon 1958: 46.)

Another example of a historically significant weak-sense program was the family role playing of Newell and Simon's Logic Theory Machine, or Logic Theorist (LT). In his autobiography, Simon recalled the historic occasion:

> *While awaiting completion of the computer implementation of LT*, Al and I wrote out the rules for components of the program (subroutines) in English on index cards, and also made up cards for the contents of the memories (the axioms of logic). At the [CMU] building on a dark winter evening in January 1956, we assembled my wife and three children together with some graduate students. To each member of the group, we gave one of the cards, so that *each person became, in effect, a component of the LT computer program*—a subroutine that performed some special function, or a component of its memory. It was the task of each participant to execute his or her subroutine, or to provide the contents of his or her memory, whenever called by the [relevant] routine . . .

So we were able to simulate the behavior of LT with *a computer constructed of human components*. Here was nature imitating art imitating nature. The actors were no more responsible for what they were doing than the slave boy in Plato's *Meno*, but they were successful in proving the theorems given them. Our children were then nine, eleven, and thirteen. The occasion remains vivid in their memories. (Simon 1991: 206–7; italics added)

Given enough children, perhaps NewFAI could have rested content with this (weak-sense) type of program?—No. For people can't be relied on to act as though they were "a component of a computer program". Even 6-year-olds will bring their intelligence to bear, following the intention behind an instruction rather than its literal meaning and/or effortlessly (perhaps unconsciously) resolving ambiguities. I was convinced of this when watching my own small son as a LOGO novice in the mid-1970s (see Section vi.a), with Stephen Salter 'playing turtle' and helping him to get the point by deliberately misinterpreting his instruction to TURN (direction unspecified, because—so my 6-year-old son thought—'obvious').

Indeed, if people didn't do this sort of thing as a matter of course then buggy programs—ignoring syntax errors and typos—would hardly exist. Even the great Turing was a culprit, here. Newell and Simon, admitting that there's "no *a priori* objection to hand simulation of a program" but pointing out that it's usually unreliable, said:

For example, there is an error in Turing's play of his [chess] program because he—the human simulator—was unwilling to consider all the alternatives. He failed to explore the ones he "knew" would be eliminated anyway, and was wrong once. (1958: 46)

Why were they bothering to argue for the superiority of strong-sense programs over weak-sense programs as late as 1958? The reason was that most researchers still didn't have easy access to a computer, so were willing—or even forced—to rely on paper and pencil instead. Herbert Gelernter's famous geometry program, for instance, was sketched (by someone else) long before he managed to implement it (see subsection d, below). And John McCarthy started designing LISP while miles away from a suitable computer (v.c). (Many *neuroscientists* still needed to be convinced of the usefulness of functioning programs as late as 1988: Chapter 14.vi.c.)

A further reason for graduating from weak-sense to strong-sense programs was that, in the 1950s, it really wasn't clear just how much was actually achievable by a computer. It was one thing to say, with Turing, that *any* computation could in principle be computed by something like the Turing machine. But what was doable in practice might be very limited. So it was an act of creative exploration, as well as a matter of intellectual hygiene (the avoidance of self-delusion), to find out.

As soon as sufficiently powerful machines were produced by IBM (and, following McCarthy's invention of time-sharing, by DEC, the Digital Equipment Corporation), mere paper-and-pencil programs became less and less persuasive. In any case, there never would be enough "children": one very early NewFAI program contained 20,000 instructions, as we'll see.

Today, then, Stein would be applauded. A program which isn't a program in the strong sense attracts little interest from AI researchers. That's why speculative work on computational architectures, as opposed to executable specifications of psychological processes, is often undervalued (Chapters 7.i.e–f and 12.iii.d). And it's why John Holland's seminal work on genetic algorithms was ignored by many AI people for nearly twenty years, given that he'd written no GA programs himself and that no computer at the time would have had the power to run them in any case (15.vi.b).

b. The first AI program—not!

The Gertrude Stein ambiguity is one reason why identifying "the first AI program" is problematic. Another concerns what people are (or were) prepared to count as *intelligent*. Again, there's no 'right' answer: the concept of intelligence is as fuzzy-bordered as any other, and perhaps more fuzzy than most.

Received opinion holds that the first AI program was the Logic Theorist (Newell and Simon 1956*b*; Newell and Shaw 1957; Newell *et al.* 1957). This is stated baldly in many places: not just in popular accounts (e.g. Crevier 1993: 44) but in scholarly ones too (e.g. H. Gardner 1985: 145; Edwards 1996: 124; Bechtel *et al.* 1998: 53 . . . and more). Arguably, it's also implied by the historian James Fleck (1982: 177), and even by Edward Feigenbaum (1936–) and Julian Feldman (1931–), both of whom were around at the time (1963: 108).

For all that, the oft-repeated claim that LT was the first working AI program is open to challenge—not to say false. Several programs, on both sides of the Atlantic, preceded it.

Newell and Simon began thinking about LT in autumn 1955 and, having been role-played by Simon's family in the New Year vacation, it was fully implemented some nine months later. Their first computer proof, of theorem 2.01 of *Principia*, was achieved in August 1956. And their first computer printout was triumphantly presented—right at the last minute—at the Dartmouth Summer Project in August 1956 (Chapter 6.iv.b). The triumph was deserved. This was the only computer printout to be shown at Dartmouth. And it was, *so far as most of the participants knew*, the first functioning program devoted to a task normally thought of as requiring significant intelligence. Moreover, it's clear with hindsight that it was hugely influential for the development of NewFAI as a whole (not *immediately*, however: see subsection g, below).

Newell had been inspired to start work on it by Oliver Selfridge's talk on pattern recognition, given at the RAND Corporation, Santa Monica, in November 1954 (Chapter 6.iii.b). Selfridge (1926–) had described not merely a programme for a (better) program, namely Pandemonium (Selfridge 1959), but also an already functioning, albeit primitive, system. This was a program written by Selfridge's colleague Gerald Dinneen to recognize letters of the alphabet (Dinneen 1955; cf. Selfridge 1955). It's relevant, here, that Newell later said:

> *Now I'd call it an artificial-intelligence program.* It was not just a simple pattern-recognition device, but it actually carried out transformations and had several levels of logic to it. (interview in McCorduck 1979: 132; italics added)

What's more, the Selfridge–Dinneen program was already known about by the cognoscenti at the time of the Dartmouth event (which happened two years after that RAND seminar). Selfridge himself was one of the Dartmouth participants. However, the early alphabet-recognizer wasn't 'sexy' enough to be thought of as modelling *intelligence* or, to use RAND's preferred terminology, to be seen as a paradigm case of *complex information processing*. Pattern recognition was assumed to be relatively straightforward, at least if—as in this case—generalization wasn't involved (cf. 12.i.c).

The Logic Theorist, by contrast, was modelling one of the peaks of human thought: Bertrand Russell's logical reasoning. Accordingly, it got the historical credit for being the first AI program *even though* it owed its existence to Selfridge's ideas and Dinneen's early system.

Selfridge's Pandemonium was successfully implemented by 1958, and is described in detail in Chapter 12.ii.d. It could well have featured as an extra harbinger, here. For Selfridge's 1954 account of it not only inspired the Logic Theorist, but gave NewFAI two important concepts: parallelism and demons. Instead of writing an ordered sequence of interconnected instructions the programmer could define a set of logically independent procedures; since they didn't depend on each other, these were *in effect* parallel—even though they had to be executed one by one in a von Neumann computer. Moreover, instead of being executed immediately these procedures (demons) could *lie in wait* to be activated by a specific cue. These ideas, too, were later taken up by Newell and Simon, in defining production systems (Section v.e, below). Indeed, the demons in Pandemonium were intellectual ancestors of Simon's famous ant (7.iv.a). Considered as a harbinger, then, Pandemonium was second to none.

The Dinneen and Selfridge pattern-recognizers aren't the only rivals for the accolade of "first AI program". Arthur Samuel's (1901–90) program for playing checkers/draughts (see subsection e) is another—with an even stronger claim to the honorific label.

It was envisaged as early as 1947. At that point, it was seen as a moneymaking exercise, to raise funds for a large computer at the University of Illinois:

> We would build a very small computer, and try to do something spectacular with it that would attract attention so that we would get more money. It happened the next spring there was to be a world checker champion meeting in the little neighbouring town of Kankakee, so somebody got the idea—I'm not sure it was mine, but I got the blame at least—that it would be nice to build a small computer that could play checkers. We thought checkers was probably a trivial game . . . Then, at the end of the tournament we'd challenge the world champion and beat him, you see, and that would get us a lot of attention. We were very naive. (A. Samuel, interviewed in McCorduck 1979: 148)

Naive, yes. But unlike Charles Babbage, whose similar 'moneymaking' scheme never resulted in anything (3.iv.c), Samuel did manage to make a machine play the game. His first program was implemented in 1949 at IBM (where he'd migrated from Illinois), on a '604' commercial calculating machine—and in 1951, on the brand-new IBM 701.

As for the *learning* version of the checkers program, this was up and running early in 1955 (Samuel 1959: 72). In Samuel's words:

> [My program] achieved this aim—to improve its playing ability through [a] learning process involving heuristics—fairly early in its existence, certainly well before 1956 . . . Except for the fact that no publicity was made of the existence of my checker program, one could argue that a program employing learning heuristics had been "fully realized" by this time . . . My checker program was one of the first programs of any size to be run on the first experimental model of . . . the IBM 701. (interview in Copeland 1993: 253)

Despite his reference to "no publicity", Samuel had demonstrated his learning program on American TV on 24 February 1956, six months before the Dartmouth event (Samuel

1959: 72). Unlike Newell and Simon, however, he attended that meeting without bringing along printout evidence. Perhaps that's why one of Marvin Minsky's first crop of AI students, whom one might have expected to know better, would later date Samuel's implementation as "1961"—a full five years after LT (Raphael 1976: 5).

Although checkers is doubtless trounced by Russell's logic as an example of intelligence, it's not 'mindless' in the way that pattern recognition apparently is. As Samuel said:

> checkers contains all of the basic characteristics of an intellectual activity in which heuristic procedures and learning processes can play a major role and in which these processes can be evaluated.
>
> Some of these characteristics . . . are:
>
> (1) The activity must not be determinate in the practical sense . . .
>
> (2) A definite goal must exist . . . and at least one criterion or intermediate goal must exist which has a bearing on the achievement of the final goal . . .
>
> (3) The rules of the activity must be definite . . .
>
> (4) There should be a background of knowledge concerning the activity against which the learning progress can be tested.
>
> (5) The activity should be one that is familiar to a substantial body of people . . . The ability to have the program play against human opponents (or antagonists) adds spice to the study . . . (Samuel 1959: 72–3)

In short, *organized* intelligence is needed for this game.

With respect to playing games (without learning to do better), others besides Samuel wrote programs several years before LT saw the light of day. Across the Atlantic, in May 1951, Christopher Strachey (1917–75) had coded a (buggy) heuristic draughts program for the pilot model of Turing's ACE machine at NPL (Chapter 3.v.c). Encouraged by Turing himself, he wrote an improved version for the Ferranti version of MADM. By the summer of 1952, he reported that this could "play a complete game of Draughts at reasonable speed" (Copeland and Aston n.d.). In addition, he showed that "non-mathematical" instructions outnumbered mathematical ones even in programs for doing numerical integration: in general, then, the *organization* of problem solving was the key to success (Strachey 1952). Samuel took some of his ideas from Strachey accordingly (Samuel 1959: 73).

As for chess, universally regarded as requiring intelligence (and often termed the *Drosophila* of AI, because it has attracted so much effort over the years), this too had already been programmed in Britain. Ferranti's Dietrich Prinz (1903–?)—who'd previously designed a rods-and-levers logic machine (see Preface, ii)—ran a brute-force chess program in November 1951. It played unimaginatively and slowly, and dealt only with the endgame (analysing all the several thousand possible moves in 'mate-in-two' problems).

Turing had been more ambitious. His TUROCHAMP chess program, begun (with the young Cambridge mathematician David Champernowne) in 1948 and still unfinished when he died, played a complete game—and used heuristics rather than brute force to do so (Turing 1953; Michie 1966: 37; Copeland and Aston n.d.). But Turing's ambition, here, didn't stretch to making his program known. He treated it as an entertaining, if challenging, project between friends, not as something to be touted at academic meetings. Being unknown beyond his immediate acquaintance, it couldn't have much influence even in England, still less in the USA.

One might mention even earlier cases, such as Turing's late 1940s program for writing love letters (Chapter 9.x.c). But that was just a toy, a joke. And it had no influence on the development of AI. Whereas it amused a few people, Selfridge's and Samuel's programs—and LT too, of course—excited many. Unlike them, it doesn't deserve (*sic*) to be regarded as a "discovery".

These examples bear out the general point made in Chapter 1.iii.f, that what's classed as a discovery is negotiable. At the dawn of AI, what counted as a program wasn't entirely clear, and what counted as intelligent (or even complex) wasn't clear either. So there was plenty of room for negotiation. There still is (Selfridge? Samuel? Strachey? Turing? Prinz? . . .). It's evident, nevertheless, that LT was/is widely *misperceived* as the first AI program.

c. How a program became a program

The history of Gelernter's geometry program nicely illustrates the shift from *program* (weak sense) to *program* (strong sense). It arose out of the Dartmouth conference. At that meeting, Minsky (having just seen the Logic Theory Machine) sketched a notional Euclidean "geometry machine", and got fellow organizer Nathaniel Rochester to persuade his young IBM colleague Gelernter to implement it (Chapter 6.iv.b).

The task took three years. Besides coding some 20,000 instructions, Gelernter's team at IBM had to invent a new programming language to describe geometrical 'diagrams' and operations (Gelernter 1959; Gelernter *et al.* 1960*a*,*b*). The operations included bisecting angles and dropping perpendiculars. This representation enabled his program to prune the search space drastically, since it tried to prove *only* those features which 'appeared' to be true in the 'diagrams'. (My scare quotes are a warning that this was *neither* computer graphics, *nor* a representation carrying the information that's available to a human being when looking at a diagram—see 13.iii.a.)

Technically, this was interesting because it showed how an internal model of a domain could be used in helping to solve problems about it. This idea, which Kenneth Craik had suggested—though in very different terms—in the early 1940s (Chapter 4.vi), would later be hugely important not only in GOFAI, but also in psychology and neuroscience (7.iv.d–e, 13.iii.a, and 14.viii).

Soon, over fifty different proofs, of up to ten steps, had resulted (Gelernter *et al.* 1960*b*: 143). But one above all attracted attention: an elegant (construction-less) demonstration of the equality of the base angles of an isosceles triangle. Gelernter himself already knew about this proof (McCorduck 1979: 188 n.). But he'd expected a different one, namely, the one used by Euclid. The unexpected proof ran as follows (see Figure 10.1):

> Consider triangles ABC and ACB.
>
> Angle BAC = angle CAB (common).
>
> AB = AC (given).
>
> Therefore the two triangles are congruent (two sides and included angle equal).
>
> Therefore angle ABC = angle ACB.
>
> QED.

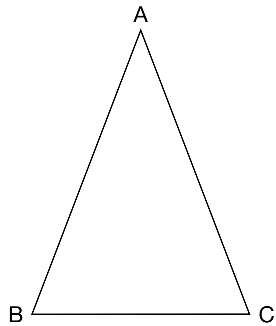


FIG. 10.1. Isosceles triangle

This proof is famous (in the history of mathematics, as in the history of NewFAI) not only because it's so elegantly simple, but also because it appears to be highly creative. A theorem concerning congruent triangles is used, which seems prima facie to be utterly inappropriate: after all, there is only the *one* triangle. Certainly, if a budding mathematician were to produce this proof spontaneously in the classroom, we'd be impressed.

(It was originally found by Pappus of Alexandria, six centuries after Euclid. But the program's computations differed significantly from those reported by Pappus himself. The reason is that, as noted above, its "diagrams" didn't carry the spatial information that real diagrams carry for human geometers. Careful comparison shows that the program was being less creative than it appeared—Boden 1990*a*: 104–10.)

What's relevant here is that Gelernter wasn't the first person to show that his program was capable of producing the proof. Indeed, he didn't even mention it in his 1959 paper. Rather, it was discovered by Minsky, using pencil and paper, long before the program was implemented.

The first public description of the incident was given by Minsky himself. This was in a 1958 forum from which, given the elegance of the proof and the identities of the participants, it was bound to spread far and wide (Minsky 1959 and Chapter 6.iv.b). And in the spreading, it was changed—by the sorts of communicative processes discussed in Chapter 8.vi. Ten years later, Seymour Papert set the record straight:

> The relevant history began at a Summer Program on Artificial Intelligence held at Dartmouth in 1956. At this meeting, Marvin Minsky proposed a simple set of heuristic rules for a program [weak sense, above] to prove theorems in Euclidean geometry. *Before any program* [strong sense] *had been written* these rules were tried (by "hand simulation") on the simple theorem (sometimes called the "pons asinorum") which asserts that the base angles of an isosceles triangle are equal. *To everyone's surprise and pleasure* these rules led to an elegant proof quite different from the one normally taught in high school courses . . .
>
> Although not entirely new, this proof is not well-known and strikes anyone with mathematical taste [in other words, not Dreyfus: Chapter 11.ii.b] as extremely elegant. So it is not surprising that *the story spread and very soon transformed itself, as stories do*, from "Minsky's rules suggested . . ." to "a program [sense ambiguous] generated a new proof" to "a computer [i.e. a strong-sense program] invented an elegant new proof". (Papert 1968: III-6; italics added)

In the meantime, Papert continued, Gelernter—"justifiably encouraged by this little success"—used Minsky's heuristics in a program which two years later was proving theorems of much greater (i.e. ten-step) complexity. In short, the hand simulation of the Pappus proof helped spur Gelernter to convert Minsky's 'program' into a genuine, executable, program.

So who should get the credit? Who *discovered* geometry programming? Hard-headed twenty-first-century AI scientists may insist that only computer-executable (strong-sense) programs count as *programs*. If so, then the prize will be awarded to Gelernter. And indeed, he and his team did most of the hard work. But someone with historical antennae will point out that he was given the general idea, and the crucial heuristics, by Minsky—and that the most famous 'Gelernter proof' was done on the back of an envelope by Minsky too. Since *the very idea* of a geometry program was revolutionary at the time, Minsky deserves much of the credit.

d. First-footings

First-footings in the New Year are, one hopes, auspicious. The lucky piece of coal at the Dartmouth Summer Project was provided by Newell and Simon, in the form of the Logic Theory machine.

We saw in Chapter 6.iii.c why LT was so exciting for psychologists. But it galvanized budding AI researchers too, who were especially interested in *how* it worked its magic. Its programmers were well aware that this magic was limited: "LT's power as a problem solver is largely restricted to problems of a certain class", namely, proving theorems in symbolic logic (Newell *et al.* 1957: 129). A few years later, then, LT gave way to a new system: the General Problem Solver, or GPS (Newell *et al.* 1959, 1962; Newell and Simon 1961).

The first two papers on GPS combined reports of what it had actually done, when implemented, with reports of hand simulations carried out (in early 1959) by the programmers. And the latter much outweighed the former:

> The program is on the RAND Johnniac. The work I am talking about here is all hand simulation. The GPS program is now being debugged, but is not yet solving any problems. Hence most of our information about it comes from extensive hand simulation. (Newell *et al.* 1962: 187)

Evidently, it was still acceptable to describe a program in the "weak" sense defined above. But the defensive nature of their comment shows that the GPS team themselves weren't happy with that: only strong-sense programs would really satisfy them.

What LT and—more powerfully—GPS did was to start with a symbolic representation of the desired object (what would normally be termed the "goal"), and identify differences between that and the current state. The differences would then be reduced one by one, until (with luck!) all had been eliminated. This was done by applying operators to the current state, which were chosen and/or ordered by heuristics. The two key ideas, here, were drawn from previous work: difference reduction from cybernetics (Chapter 4.iii.e and vii.a) and heuristics from Simon's research on decision making in management (6.iii.a).

The term "goal" was used by the GPS team in a way not exactly equivalent to normal usage:

> Each goal is a collection of information that defines what constitutes goal attainment, makes available the various kinds of information relevant to attaining the goal, and relates the information to other goals. There are three types of goals:
>
> Transform object A into object B,
> Reduce difference D between object A and object B,
> Apply operator Q to object A.
>
> (Newell and Simon 1961: 284)

Strictly, then, GPS couldn't have a goal without having some fairly definite idea of how to attain it. Whether that's also true of human beings is debatable (and relevant to the distinction between a wish and an intention).

Different goal differences required different operators. If the preconditions for applying a given operator weren't satisfied, action had to be taken to establish them. For example, a logical operator considered by LT might require that the 'current state' be a conjunction, or have the term *a* before *b* rather than the other way round. If the current state didn't match this precondition, some other operator would have to be found whose actions would alter the current state to match this precondition. The preconditions of the other operator would either have to match the current state or be capable of being made to match as a result of execution of yet another operator, and so on.

In this way, sub-problems (and sub-sub-problems . . .) in LT, and sub-goals (and sub-sub-goals . . .) in GPS, were set up as means to attaining the end. If a chosen strategy couldn't be made to work, LT/GPS would backtrack to the previous choice point and try an alternative pathway through the search space. Differing search strategies could be followed in different cases. For instance, breadth-first search involved following *every* possible branch on a given level, but each one only for a few levels down before switching to the next. Depth-first search, by contrast, picked a path and followed it to the bitter end before considering the next alternative. (The combinatorial explosion, as usual, was lurking in the background. The ability to specify sub-goals on indefinitely many levels could be too much of a good thing: relentless depth-first search to an increasing degree of detail could bog the program down.)

Success wasn't guaranteed, however: this was *bounded* rationality, after all (6.iii.a). A new understanding of "algorithm" ensued as a result.

Derived from the name of the Arab mathematician al-Khwarizmi (2.i.b), the term had come to mean a formally defined (so potentially automatic) mathematical procedure guaranteed to succeed in finding the answer. And the first computer programs, designed as they were for military use, were—or aimed to be—algorithms in this sense. But because LT relied on potentially fallible heuristics to prune the search space, its programmers *denied* that it was an algorithm (Newell *et al.* 1957: 113–14, 117–18). For all that, a heuristic program *was* a formally defined, automatable, procedure—which some people continued to call an "algorithm". So while weak-sense programs were becoming strong-sense programs, strong-sense algorithms were being joined by weak-sense algorithms. For some years, while heuristic programming was still a novelty, talk of "algorithms" was sometimes ambiguous and/or misleading for this reason. Minsky, for one, complained of the "pointless argument" that arose as a result (1961*b*: 438 n. 26). Nowadays, the ambiguity has disappeared, at least for AI professionals: anything that can be run on some computer is called an algorithm, even if it implements a heuristic program.

GPS wasn't limited to logic problems, as LT had been. Even so, the name "*General* Problem Solver" was misleading. To be fair, the programmers had said this themselves:

> GPS could really be better called GPSWP—sort of GPS with pretensions. By "general" we don't mean that GPS can solve all problems. "General" means that it is built in such a way that the specific content of the problem area is factored from the general problem-solving heuristics. Therefore it is capable of tackling a wide class of problems. I don't know how big the class is . . . (Newell *et al.* 1962: 189)

Any problem that could be represented in terms of goals, differences, actions, operators, and preconditions could in principle be solved by this program. The specific content was irrelevant. Moreover, some examples that human beings find tricky, such as the missionaries-and-cannibals puzzle, were solved by GPS (Chapter 6.iii.c).

However, it was the programmer who had to represent the problem in that form, which often required not only great effort but significant creativity. Arguably, that was the *real* problem solving—after which, the program itself merely 'turned the handle'. As the GPS team put it:

> The real gimmick, or the good trick . . . is inventing the right spaces so that the operators we found and the differences we found made these tables of connectives small enough so that there is not too much pointless search. Most of the information we put into the system . . . was introduced by constructing the right problem spaces for the program to work in. This I think was the big selection. (Newell *et al.* 1962: 187–8)

This fact would become increasingly evident over the following decade. It was clear, for example, in Saul Amarel's (1928–2002) masterly discussion of six representations for the missionaries-and-cannibals puzzle (Amarel 1968). The most powerful of these could cope with a very large number of cannibals, arbitrarily distributed on either side of the river at the initial and/or terminal stages, using a boat whose capacity could vary during the development of the solution, and allowing for several missionary 'casualties'. (Amarel had hoped to formulate general principles whereby AI programs could create improved representations for themselves, but he had to admit defeat—Boden 1977: 334–40.)

For NewFAI, the most relevant 'functional' ideas in LT/GPS were:

* heuristic programming (NB heuristics had already been used by Samuel to evaluate alternative choices, but not to generate the search tree itself: see i.b, above),
* choice of methods (in GPS, called operators),
* similarity testing (in GPS, goal differences),
* the hierarchical sub-problem tree (in GPS, means–end analysis and planning),
* backtracking (to compare/select alternative possibilities),
* separation of the 'solving technique' from the specific content,
* the representation of the problem, or
* the search space, and
* building representations of varying structure and unbounded complexity (as opposed to fixed-size vectors).

There were technical excitements too, for LT and GPS were implemented by a revolutionary programming method, made possible by new programming languages (Section v.a, below). These provided:

* recursion,
* list processing, and
* push-down stacks.

These ideas turned out to be incalculably significant for the history of GOFAI. Over the next twenty years, the field was largely focused on theorem proving, heuristic search, planning, and—increasingly—knowledge representation. Some developments, such as flexible planning (Section iii.c), were very much in the spirit of GPS. Others, such as production systems (Section v.e) and "filtering" by constraint propagation (Section iv.b), were new approaches aimed at preserving its strengths while avoiding some of its limitations.

e. The book of Samuel

Another auspicious first-footer was the IBM researcher Samuel (see subsection b, above), later a member of Stanford's AI Department. Taking advantage of IBM's test machines at night, when his saner colleagues were safely tucked up in bed, he wrote a 6,800-instruction program for playing checkers. This was less interesting to psychologists than LT was, because he didn't claim to be modelling human intelligence. Indeed, he thought that studying the way people solve a problem gives one insight into "what the real problem is", but *not* into the brain's method of solving it (McCorduck 1979: 152). To AI people, however, his program was thrilling.

Samuel described it as playing "a fairly interesting game, even without any learning" (1959: 73). But it was the learning which really excited his contemporaries. Eight years after it was implemented, it was still "the only really successful attempt at machine learning in problem-solving" (Feigenbaum and Feldman 1963: 38). When Dreyfus first fired his cannon at AI in the mid-1960s (11.ii.a), even he saluted Samuel. This was no grudging praise: Samuel, he said, had done "very impressive work—perhaps the most impressive work in the whole of artificial intelligence" (H. L. Dreyfus 1965: 5).

In *playing* checkers, the program normally used a lookahead of three moves to evaluate all the legal alternatives, before choosing the best. (Samuel discussed a number of ways in which the lookahead might be increased, depending on the dynamics of the game: pp. 77 ff.) What counted as "the best" was decided by a method in which the program chose the move *most* likely to lead to good positions for itself and *least* likely to do so for its opponent:

> It is not satisfactory to select the initial move which leads to the board position with the highest score, since to reach this position would require the cooperation of the opponent. Instead, an analysis must be made proceeding *backward* from the evaluated board positions through the "tree" of possible moves, each time with consideration of the intent of the side whose move is being examined, assuming that the opponent would always attempt to minimize the machine's score while the machine acts to maximize its score . . . Carrying this "minimax" procedure back to the starting point results in the selection of a "best move". (Samuel 1959: 76)

To *improve* its play, the program used two methods: "rote learning" and "learning by generalization". In the first, it stored all the past positions, together with its initial evaluations of them. (Samuel stressed the importance of indexing, and fast sorting and searching procedures to find the record required.) If the third move in the lookahead happened to be one for which it had already calculated an evaluation, the lookahead—in effect—was increased to *six* moves. So the system pulled itself up by its own bootstraps, with the recent evaluations benefiting from a search much deeper than the official limit of three.

The second learning method was a way of improving the evaluation decision itself, by continuously adjusting the mathematical weighting of the individual test parameters according to their success in play. The program had thirty-eight parameters marking strategic features of the game (such as "threat of fork" and "center control"). Starting with only sixteen of these, it experimented to see which were the most useful, and weighted them accordingly. The least helpful ones were replaced by others drawn from the reserve list. This method was soon called *Alpha–Beta* pruning, based on the names Samuel gave to the program and its opponent (1959: 83). It gave the program an impressive strength, namely, the ability to improve its game differently in response to different people. An opponent's idiosyncratic weak spots could be discovered, and the relevant parameters weighted accordingly—even if they weren't *usually* very helpful. (The machine's play could improve even without a human opponent, if Samuel set two copies of the program 'against' each other.)

Samuel's program was a milestone for NewFAI, in three senses. First, it provided heuristic techniques which would ground further developments for years to come. To use heuristics at all in AI was original. (Turing himself still regarded machine learning as *either* random *or* systematic, by which he meant exhaustive search: A. M. Turing 1950.) In particular, Samuel's minimaxing heuristic is still crucial in machine learning.

Samuel wasn't the first to define it (John von Neumann had done that by 1928), nor to suggest using it for computerized game playing (Shannon had done that by 1950). He wasn't even the first to implement it: Strachey had done that by 1952, using a lookahead of six moves and more. But he was the first to show how minimax could be achieved by a complex evaluation function, defined in terms of various features of game strategy—"center control", for instance. (Strachey had used a simple count of 'own' and 'enemy' pieces.) He was the first to show how that function could be continuously improved. And he was one of the first to suggest that future learning programs might be able to generate their own evaluative parameters (1959: 87, 95). (Selfridge had preceded him here; soon, a new version of Pandemonium would define its own feature detectors: see 12.ii.d.)

Second, in 1962 the program became a publicist's dream overnight, when it beat Robert Nealey, a blind chess player from Stamford, Connecticut—described in the press as a former state champion. (The match had been set up at the request of the editors of *Computers and Thought*, then still in draft.) Nealey himself was impressed:

> Our game . . . did have its points. Up to the 31st move, all of our play had been previously published except where I evaded "the book" several times in a vain effort to throw the computer's timing off. *At the 32–27 loser and onwards, all the play is original with us*, so far as I have been able to find. It is very interesting to me to note that *the computer had to make several star moves in order to win*, and that I had several opportunities to draw otherwise. That is why I kept the game

going. *The machine, therefore, played a perfect ending without one misstep. In the matter of the end game, I have not had such competition from any human being since 1954, when I lost my last game.* (Feigenbaum and Feldman 1963: 104; italics added)

The publicity generated was huge. (IBM weren't happy. They'd never really favoured Samuel's program, although it was useful for testing their new computers, because "it smacked too much of machine thinking, etc., and they wanted to dispel any worry people had with machines taking over the world and all that sort of thing": Samuel, in McCorduck 1979: 151.)

Journalists announced checkers to have fallen decisively to the computer. Most AI professionals apparently believed this too, since they abandoned it for chess (e.g. Newell *et al.* 1958*b*; A. Bernstein and Roberts 1958; Bernstein *et al.* 1958)—which soon provided NewFAI with yet another highly publicized success (see Chapter 11.ii.b).

In fact, Samuel's program wasn't so good—nor checkers so trivial—as this media-friendly achievement suggested. Not only was Nealey *not* an ex-champion of Connecticut (his rating was just below the 'master' level), but he'd made a careless mistake—perhaps because he wasn't taking his opponent seriously (Schaeffer and Lake 1996). In a return match a year later, he won all ten games roundly. Samuel himself wasn't misled. He knew the program had reached only near-master level, and worked hard on an improved version (Samuel 1967). (Nearly thirty years later, he admitted in a letter that he still "had no idea how he could make his program good enough to compete with the world champion": Schaeffer and Lake 1996. Victory at Kankakee remained as elusive as ever.)

Third, and no less compelling to the public eye, the program learnt to beat Samuel himself. This was even more significant, many people felt, than LT's feat of surpassing Russell in logical elegance. The fact that Samuel was a weak player was irrelevant. The sceptic's mantra that "a program can do only what its programmer tells it to do" now had to be seen in a new light. At that time, this was IBM's mantra too (McCorduck 1979: 159). They were so worried about the IBM 704 being seen as threatening that their salesmen were instructed to quote Ada Lovelace's deflationary remark—*[The computer] has no pretentions whatever to originate anything. It can do whatever we know how to order it to perform* (Chapter 3.iv.b)—as often as possible.

Add Samuel's claim that "one can say with some certainty that it is now possible to devise learning schemes which will greatly outperform an average person" (1959: 95), and the fuse had been lit. AI was on the way up.

f. Programmatics

Just how far up was "up"? Partly because it was still difficult to communicate *anything* to a computer (Section v, below), that question was being answered less by programs than by programmatics.

Over-optimistic predictions were rife. One in particular, that a machine would be world chess champion within ten years (i.e. by 1967), would be a prime target of Dreyfus's attack a few years later (Chapter 11.ii.a). Indeed, RAND's Paul Armer reported in his upbeat 'Attitudes Toward Intelligent Machines' (1960/1963) that even *that* prediction was regarded by some as "conservative". (Armer himself didn't agree explicitly, but his preceding sentence had been "Few would have believed in 1950 that man would hit the moon with a rocket within ten years'': p. 405.) This hype had such strong roots that by the mid-1960s it had abated only a little. Simon was then proclaiming, in a book on management, that ''Machines will be capable, within twenty years [i.e. 1985], of any work that a man can do'' (Simon 1965: 96). (We're now another forty years on . . . and still waiting.)

However, even over-optimists can sometimes be constructive. Eight of these early discussions were especially influential, and also remarkably far-sighted.

The earliest was Pitts and McCulloch's (1947) speculation about probabilistic networks and ''universals'' (Chapter 12.i.c). Soon after that came Shannon's (1950*a*,*b*) programmatic paper on chess, discussed above. Another was Selfridge's (1959) account of Pandemonium, in which (besides describing his program) he sketched future possibilities involving complex hierarchies of ''demons'' (12.ii.d). His vision inspired psychologists such as Jerome Bruner (6.ii.b–c), as well as AI researchers.

The fourth example of fruitful programmatics was Turing's (1950) paper in *Mind*. (His two papers of 1947 would have qualified here, had they been published; but they didn't appear until twenty years later: 1947*a*,*b*.) As explained in Chapter 16.ii.a, the *Mind* piece was *not* primarily about the Turing Test. That had been added as a jokey tease, which tantalized the media worldwide and diverted attention from the technical aspects. In fact, the paper was intended as an outline research programme for AI.

Turing gave only sparse hints about the results already achieved, some of which were still top-secret anyway. But what his text lacked in detail, it made up for in scope. Turing recommended AI work on intellectual activities such as chess, as well as on perception, language, and inductive learning—and pointed out that computers, like people, could make mistakes. He sketched the chess program which, when implemented a year later, would lose against a weak human opponent (Section i.a, above). And he outlined the fourfold classification of computational systems that's now normally attributed to Stephen Wolfram (15.viii.a).

Perhaps the most significant aspect of Turing's paper was that he saw AI (not yet named as such, of course) as a matter of programming computers, not building them. Quite a few people at that time were trying to produce machines capable of problem solving and/or learning, but they were doing so by means of engineering (12.ii.a–b).

However, Turing's ideas didn't immediately sweep through the scientific community: the arcane pages of *Mind* were rarely perused by them. The paper was first made more widely accessible in 1956 (see ii.b, below). Meanwhile, his equally prescient paper on 'Intelligent Machinery' (1947*b*), in which he'd declared that ''intellectual activity consists mainly of various kinds of search'' (of which ''genetical or evolutionary'' search was one possibility), remained unpublished until many years after his death. As a result, the notion that *programs,* such as the Logic Theorist, might be the royal road to AI struck many visitors to Dartmouth in the summer of 1956 as a revelation (McCarthy 1989). Even at the end of the 1950s, Frank Rosenblatt's ''Perceptron'' was a physical, not a virtual, machine (12.ii.e). (The capitalized word ''Perceptron'' names a specific piece of hardware built by Rosenblatt, whereas without capitalization it denotes a *class* of parallel-processing systems; although this class includes the Perceptron, most perceptrons are virtual rather than physical machines.)

Next, was McCarthy's 'Programs with Common Sense' (1959), which described a joint project also involving Minsky. Delivered at the seminal 1958 meeting in London (6.iv.b), this was the first published intimation of logicist AI—which would become both hugely influential and hugely controversial (Sections iii.b and e, and Chapter 13.ii).

McCarthy had been nurturing these ideas for a decade, ever since—as an undergraduate at CalTech—he'd gatecrashed the 1948 Hixon symposium and heard von Neumann's talk on automata theory (cf. 15.v.a). Today, he remembers being "turned on" to the idea of the brain as a machine by that talk, and perhaps by Wolfgang Kohler's too—although when he went back to look at their texts, he couldn't find any references to machine intelligence (personal communication). He did, however, beard von Neumann in his Princeton office, to suggest that the brain is an information-processing system *à la* Shannon. Von Neumann encouraged him to try to develop that idea. So where von Neumann's ultimate vision had been (artificial) self-reproduction, McCarthy's—from that moment on—would be (artificial) intelligence.

He soon became dissatisfied with his own equations, believing that logic must, somehow, be the way forward. One might be tempted to say that he was treading in McCulloch's footsteps. For McCulloch had used (a different type of) logic as a key idea in his 1943 paper, and had even tried to define the vocabulary of natural language in logical terms—before giving up in disgust at the difficulty of doing so (4.iii.c). But the young McCarthy didn't know about that.

Indeed, in 1955 he still knew nothing of Newell and Simon: "I had no idea that anyone was doing logic on computers" (personal communication). Meanwhile, he'd failed to persuade many people that automata theory had much to do with intelligence, and he was disappointed by the non-semantic, non-psychological, nature of the papers in the volume he'd co-edited with Shannon (6.iv.b). ("I was against behaviorism, because I used the IBM 704: lots of input/output, but also lots of internal state, which they (or anyway, Skinner) ignored": personal communication.)

So in the final section of the funding application for the Dartmouth Summer Project, he proposed "to study the relation of language to intelligence". In particular, he wanted to formulate a language that could express "conjectures" as well as proofs or instructions, and which would "contain the notions of physical object, event, etc." (McCarthy *et al.* 1955: 52–3). Now, he had his chance.

The 'Common Sense' paper described the *advice taker*, a "proposed [*sic*] program for solving problems by manipulating sentences in formal languages" (McCarthy 1959: 75). Since the inferential procedures and heuristics "will be described as much as possible [not in the particular program but] in the language itself", it followed that the *advice taker* would have a huge advantage over 1950s programs: "its behaviour will be improvable merely by making statements to it, telling it about its symbolic environment and what is wanted from it".

McCarthy's inspiration, here, was our own common sense. Most human intelligence involves natural language, where common-sense inferences are typically performed so easily that we're hardly aware that they've taken place at all. Programs, said McCarthy, should be able to do this too. Moreover, other desirable features depended on it. In particular, a program should be able to learn by taking verbal advice from a person—who needn't know anything about the structure of the program as such, and who certainly wouldn't have to *reprogram* it in order to provide an additional datum.

("Advice", here, meant telling someone a relevant fact, as opposed to instructing them on what to do.)

It followed that if a program is to be capable of learning something, it first has to be capable of being told it. How could that be achieved? The answer, McCarthy suggested, was predicate logic.

He wasn't merely saying (like the logical atomists and the young McCulloch before him: 4.iii.a–c) that natural language can be represented by some form of logic. He was saying that predicate logic could provide the common-sense inferences as well as the meaning. And the user wouldn't need to tell the computer what to do: the program would deduce not only declarative sentences, but also imperatives instructing the machine to do certain things (such as "printing sentences, moving sentences on lists, and reinitiating the basic deduction process on these lists").

As McCarthy defined his project,

> A program has common sense if it automatically deduces for itself a sufficiently wide class of immediate consequences of anything it is told and what it already knows. (ibid.)

(The example he chose to discuss was deciding how to get to the airport, given that one is sitting at home with one's car parked outside.) Much of the ensuing controversy—which continues today—concerned whether a "sufficiently wide class" of consequences could, in general, be deduced by logical means.

Already, in 1958, there were sceptics. The NPL discussants included the logician Yehoshua Bar-Hillel, who'd initially been sympathetic to formal/cybernetic approaches to language but who'd recently emerged as a major critic of machine translation, or MT (Chapter 9.x.b and e). He tartly remarked that McCarthy's ideas were "half-baked", philosophically problematic, vitiated by what would later be called the frame problem (see iii.e, below), and careless of people's capacity to change their minds—not to mention the importance of *time* in deciding how to get to the airport. He ended his many criticisms by saying:

> To make the argument [about getting to the airport] deductively sound, its complexity will have to be increased by many orders of magnitude. So long as this is not realized, any discussion of machines [such as that proposed] is totally pointless. The gap between Dr. McCarthy's general programme [i.e. program] . . . and its execution even in such a simple case . . . seems to me so enormous that much more has to be done to persuade me that *even the first step* in bridging this gap has already been taken. (in the 'Discussion' appended to McCarthy 1959; italics added)

McCarthy was unmoved. Or rather, he was moved to beef up his logic: ten years later, he admitted that the basic predicate calculus wouldn't be enough, that various modal logics would be needed too (iii.a, below). But his commitment to logicism endured—and it still does (13.i.a).

"Twin" examples of NewFAI programmatics which avoided such stringent criticism were 'The Processes of Creative Thinking' and 'Chess Playing Programs and the Problem of Complexity', both due to the LT team (Newell *et al.* 1958/1962, 1958*b*). These set out the general principles which had informed LT and would guide the writing of GPS: bounded rationality, heuristics, hierarchical structure, and so on. They caused great excitement in the small AI community and, like the other forward-looking papers mentioned here, would be cited by Minsky in his own triumph of programmatics (see below).

Soon, they'd cause excitement in other communities too. The RAND memo on creative thinking reached an interdisciplinary audience in 1962, when it appeared in a collection co-edited by the leading Gestalt psychologist Max Wertheimer (Chapter 5.ii.b). As for its twin, on chess and complexity, this was reprinted a year later in *Computers and Thought*.

Meanwhile, Simon had confidently declared in the management journal *Operations Research:* "It is not my aim to surprise or shock you . . . But the simplest way I can summarize is to say that there are now in the world machines that think, that learn *and that create*" (Simon and Newell 1958: 6; italics added). Some readers of *Operations Research* would, therefore, have been eager to see the RAND memo. But they might have been disappointed. The title mentioned "creative thinking", but the focus was on problem solving and game playing: creativity in its everyday sense was ignored. Likewise, Rochester's section on 'Originality in Machine Performance' in the proposal for the Dartmouth meeting hadn't discussed originality in the arts and sciences (McCarthy *et al*. 1955: 49). In the 1950s, that wasn't considered a fit topic for AI.

To be sure, quite a few people were working—with scant results—on "automatic programming", with the hope of avoiding some of the tedium of programming. Besides typical GOFAI programmers, these included the young Richard Laing, seeking automatic designs for cellular automata (Laing 1961*b*; cf. Chapter 15.v.b). And the 'Creative Thinking' harbinger itself predicted aesthetically valuable computer-composed music. But it said nothing specific about how this might be done. Its last-resort advice (after citing hints from George Polya: 1945), "When all else fails, try something counterintuitive", threw no light on Bach fugues, nor even on a saloon-bar pianist's mediocre efforts.

As things turned out, AI models of what's normally meant by creativity had to await the 1980s (Chapter 13.iv). Then, Simon himself would initiate work on scientific discovery (Langley *et al.* 1987). Later still, he'd consider artistic creativity too. Indeed, when invited by the *Stanford Humanities Review* to write a target article for peer commentary on the "foundational assumptions of AI", he chose instead to enter the postmodernist lions' den with a paper on literary criticism (Simon 1994*b*). But that was far in the future. In the harbinger period, it was enough if one could tackle chess or logic, or usher the missionaries and cannibals safely across the river. Creativity could wait.

g. First 'Steps'

The seventh, and most wide-ranging, example of fruitful programmatics was Minsky's 'Steps Toward Artificial Intelligence'. This paper itself developed gradually, step by step.

The earliest version (under the title 'Heuristic Aspects of the Artificial Intelligence Problem') was written in early to mid-1956, as the first draft of what would later become an MIT Technical Report. Although it was still unpublished, Minsky made the draft available informally to visitors at the Dartmouth meeting (Newell and Simon 1972: 884). It identified Minsky's major influences as Shannon, Selfridge, and Solomonoff (see 12.ii.a). Newell and Simon weren't mentioned, for the very good reason that Minsky hadn't yet heard of them.

The second draft, written after the meeting (and published by MIT's Lincoln Laboratory in December), did mention them—but only in passing (Minsky 1956*c*).

That's curious, for two reasons. First, the Logic Theorist was the only program to have been demonstrated at Dartmouth. Second, an Institute of Radio Engineers meeting held immediately afterwards (in September) had involved heated complaints backstage from Newell and Simon, who resented the fact that McCarthy—who had speculated creatively, but achieved nothing (i.e. no AI program)—had been asked to report on the Summer School. Minsky himself had been involved in the negotiations, and had agreed that this wasn't fair. The compromise reached at the IRE was that McCarthy's general report of Dartmouth was followed by Newell and Simon's detailed account of LT. But Minsky's December update of his paper still gave only scant attention to the LT team. It wasn't yet clear, at least to Minsky, that much of what they had to say might apply to intelligence in general, whether natural or artificial.

Five years later, it was. Now retitled as 'Steps . . .', and including extensive discussion of Newell and Simon's work, the paper was officially published for the first time by the IRE (Minsky 1961*b*). It's this version which was reprinted in the best-selling collection *Computers and Thought*. From that time on (i.e. 1963), it reached a wide, and ever-growing, audience.

Years later, Minsky allowed that the initial response to LT had been unfair: "like Darwin and Wallace, Darwin had done all this work and Wallace had gotten this bright idea, but they both got equal attention at the time" (in McCorduck 1979: 108). His explanation of his "perhaps surprisingly casual acceptance of the Newell–Shaw–Simon work" was that he'd sketched the Geometry Machine on the back of an envelope "in the course of an hour or so", so didn't realize just how difficult automated logic would turn out to be (McCorduck 1979: 106). Moreover, the LT team had presented their work at Dartmouth as a contribution to *psychology*, rather than to AI as such. A historical titbit, to be borne in mind when considering Minsky's later work (Chapters 7.i.e and 12.iii.d), is that he now sees the main thrust of his research and Newell-and-Simon's as reversed:

> I did not realize until much later [than the Dartmouth affair] the great joke; at almost every stage, I was in various ways more concerned with human psychology, they with artificial intelligence—but neither of us would have agreed at all with that description. (Minsky, interviewed in McCorduck 1979: 107)

Even today, 'Steps' should be highly recommended to new students of AI. They'd get a sense of some of the major, still largely unsolved, problems—as well as discovering how much 'new' research is reinvention of old wheels. At the time of its first publication, it provided not just intellectual dynamite but intellectual direction too.

Minsky provided a wealth of information, as well as analysis and speculation, for 'Steps' was backed up by a comprehensive annotated bibliography, published (also in 1961) in a different journal. Given the scattering of the relevant literature, this was very useful at the time, and helped to define the scope of the recently named new field (a revised version was included as an appendix when the discursive paper was reprinted in *Computers and Thought*).

In 'Steps', Minsky reviewed the first decade of research, and prophesied future developments. Many of his prophecies have come true—not least because he forecast problems as well as achievements. He had an eagle eye for the combinatorial explosion, for example (although he didn't use that label in discussing how to avoid it). Much as a good way of gauging the success of current cognitive science is to compare it with what was envisioned in *Plans and the Structure of Behavior* (see 6.iv.c and 17.iv), so in evaluating modern AI one could do worse than consult 'Steps'.

The outstanding characteristic of 'Steps' was that it adopted a mathematical, analytic viewpoint. (Regrettably, very few NewFAI people would follow that example: 11.iii.a.) Like McCulloch and Pitts (1943) before him, Minsky was doing what McCarthy called "meta-epistemology". That is, he was trying to identify *general* representational or computational constraints on the sorts of mechanisms that are *in principle* capable of computing particular notions.

Accordingly, Minsky defined and compared various general classes of program, mentioning specific cases only as illustrations. Samuel's checkers player, for instance, wasn't considered merely as having an ingenious learning rule, but as an example of the general class in which (*a*) evaluation depends on the estimation of "imaginary" situations, or planning, (*b*) reinforcement happens "when the actual outcome resembles that which was predicted", and (*c*) there is "some way of evaluating nonterminal positions" (1961*b*: 430–1).

Minsky considered both sides of the incipient cybernetic schism (Chapter 4.ix). He himself had experience of both: he'd started as a connectionist, combining spare parts from a B24 bomber to make a pioneering learning network, but was diverted/converted to NewFAI in 1955 by his MIT colleague Solomonoff (1926–), then working on the induction of grammars (12.ii.a). So in 'Steps', he discussed adaptive "self-optimizing" systems and early connectionist machines, and also highlighted NewFAI icons such as LT/GPS and the checkers player. Significantly, he tried to analyse the weaknesses of various AI methods, as well as their potential strengths.

His section on 'Hill-Climbing', for instance, was followed by one on 'Troubles with Hill-Climbing'. This mentioned both "local peaks" (12.v–vi) and "mesas", in which "the more fundamental problem lies in finding any significant peak at all", and which are especially likely if the evaluation procedure (or "Trainer") can detect only the *solution* of a problem (1961*b*: 411, 429; cf. Minsky and Selfridge 1961).

Another knotty problem highlighted in 'Steps' was credit assignment. In a complex program, each ultimate success is "associated with a vast number of internal decisions", so we must find some way of assigning "credit for the success among the multitude of decisions" (p. 432). In LT/GPS, this was done by breaking the problem into distinct sub-goals (later, it would be dealt with also by back-propagation in PDP connectionism, and by the bucket-brigade algorithm in evolutionary programming: 12.vi.c–d and 15.vi.a).

Property-list descriptions, too, were shown to be problematic (pp. 422–5). In particular, they were problematic if the "properties" were *unary* (defined in terms of only one object) rather than *binary* or *ternary* or . . . Consider the three diagrams shown in Figure 10.2, for instance. These structures (and substructures) can be described, and therefore compared, using familiar spatial words. A computer capable of describing/comparing them, said Minsky, would need some symbolic notation that includes (1) terms for binary relations such as *inside of, to the left of*, and *above* (and, with respect to Figure 10.2 as a whole, the ternary relation *between*), and (2) a way of specifying, and shifting, the hierarchical level at which these relations were being considered. In diagram (*c*), for instance, the triangle is inside the oval, and the circle is inside the triangle. In other words, *inside of* is a recursive relation. But being recursive doesn't necessarily mean being transitive: for some purposes, it wouldn't be

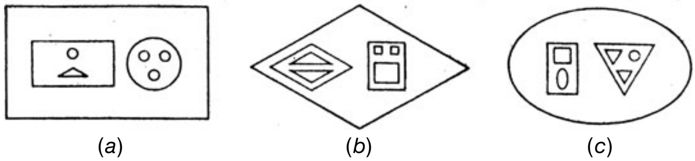


Fig. 10.2. Three diagrams for description and comparison. Reprinted with permission from Feigenbaum and Feldman (1963: 423)

appropriate to describe the circle as being inside the oval, although for other purposes it would. (These points would later be clarified in the ground-breaking ANALOGY program, written by one of Minsky's students: see 13.iv.c.)

Again, Minsky pointed out the difficulty of implementing hierarchy in neural nets (cf. 12.viii.b and ix.a), and the need for carefully ordered training sequences "to insure that early abstractions will provide a good foundation for later difficult problems" (pp. 428, 434 n. 23; cf. 12.viii.c–e and x.e). And with respect to the (general) need for recursive use of previous results, he suggested that future AI programs might use heuristic mechanisms (as in "meiosis and crossing-over") providing for "the segregation of groupings related to solutions of subproblems", and emulating "the fantastic exploratory processes found perhaps only in the history of organic evolution" (p. 434; cf. 15.vi). (John von Neumann had already said that copy errors could ground evolution in cellular automata, but he hadn't discussed how this could be achieved in practice: 15.v.a.)

Minsky discussed the various types of internal "model" (e.g. "analogous, semantic, and abstract": p. 435) which AI programs could use—and, perhaps, construct for themselves. He showed, for example, that planning depends on simplified models of the problem (pp. 442–3). And, sometimes citing his own mid-1950s work, he stressed *general* conditions regarding what would soon be called "knowledge representation" (KR). Minsky saw KR as "crucial":

> The structure of the [representation] will have a crucial influence on the *mental world* of the machine, for it determines *what kinds of things can be conveniently thought about.* (412–13; italics added)

In short, he was asking what types of *virtual* world would enable machines to perform various tasks (see Sections iii.a and v, below).

To take just one example, what kind of "mental world" must a visual program have if it is to be able to see *two* objects?

> [The] property-list scheme is limited (for any given set of properties) in the detail of the distinctions it can make. Its ability to deal with a compound scene containing several objects is critically weak, and its direct extensions are unwieldy and unnatural. If a machine can recognize a chair and a table, it surely should be able to tell us that "there is a chair and a table". (p. 422)

Besides choosing appropriate base-level property descriptors, he said, AI scientists would have to find ways of "subdividing complex objects and describing the complex relations between them". That quest would soon be the focus of MIT's programme of scene analysis, carried out under his direction (Section iv.b below, and 7.v.b–d). (It was also the key idea of Ulric Neisser's "analysis by synthesis", which eventually influenced some AI work on vision: 6.v.b.)

With hindsight, one of the most interesting aspects was Minsky's scepticism about Rosenblatt's "perceptrons", then being hyped in the press and on the airwaves (see 12.ii.e–f). This scepticism was expressed repeatedly throughout the text. For instance:

To recognize the *topological* equivalence of pairs such as those in [Figure 10.3] is likely beyond any practical kind of iterative local-improvement or hill-climbing matching procedure. (Such recognitions can be mechanized, though, by methods which follow lines, detect vertices, and build up a *description* in the form, say, of a vertex-connection table [i.e. scene analysis].) (p. 414)

How could one represent with a single prototype the class of figures which have an even number of disconnected parts? Clearly, the template system has negligible descriptive power. The property-list system frees us from some of these limitations. (p. 415)

[Rosenblatt's] nets, with their simple, randomly generated, connections can probably never achieve recognition of such patterns as "the class of figures having two separated parts", and they cannot even achieve the effect of template recognition without size and position normalization (unless sample figures have been presented previously in essentially all sizes and positions). (p. 421)

It is my impression that many workers in the area of "self-organizing" systems and "random neural nets" do not feel the urgency of this problem [of credit assignment] . . . For more complex problems, with decisions in hierarchies (rather than summed on the same level) . . . we will have to define "success" in some rich local sense. Some of the difficulty may be evaded by using carefully graded "training sequences" . . . (1961*b*: 432–3)

[The] work on "nets" is concerned with how far one can get with a small initial endowment; the work on "artificial intelligence" is concerned with using all we know to build the most powerful system that we can. It is my expectation that, in problem-solving power, the (allegedly brainlike) minimal-structure systems will *never* threaten to compete with their more deliberately designed contemporaries. (p. 446 n. 36; italics added)

And, after a careful mathematical comparison of various associative learning rules:

Incidentally, in spite of the space given here for their exposition, I am not convinced that such "incremental" or "statistical" learning schemes should play a central role in our models. They will certainly continue to appear as components of our programs [i.e. hybrid systems: 12.ix.b] but, I think, mainly by default. The more intelligent one is, the more often he should be able to learn from an experience something rather definite; e.g. to reject or accept a hypothesis, or to change a goal.

In light of the major scandal at the end of the decade, and its effects on the history of AI (Chapter 12.iii and vii.b), these early attempts on Minsky's part to counter the enthusiasm surrounding perceptrons were prophetic.

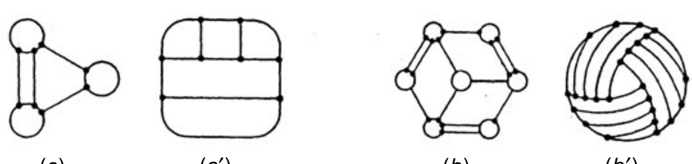


FIG. 10.3. The figures a, a' and b, b' are topologically equivalent pairs. Reprinted with permission from Minsky (1961*b*: 414)

Minsky's paper related the nascent AI to established areas of cognitive science. He cited the Gestaltist psychologists, Craik, Burrhus Skinner, Donald Hebb, Noam Chomsky, and George Miller—and Niko Tinbergen as well (4.vi and 5.ii.b–c). Twice, he praised MGP's *Plans and the Structure of Behavior* (6.iv.c). He recommended Bruner's New Look (6.ii) and Polya on heuristics (6.iii.b), and in comparing different learning strategies he mentioned mathematical psychology too (6.i.a). Neurophysiology and ethology were implicit when he defined various abstract "machines" for conditioning or noted the difficulty of "chaining" reflexes in lower animals having simpler brains (5.iii.a), and explicit when he spoke of "the bold pioneering work" of Nicholas Rashevsky (4.iii) and the "models based on brain analogies" described at the 1958 NPL gathering (6.iv.b). In addition, he remarked the recent experimental discoveries of visual feature detectors, at MIT and Harvard (14.iv.a–b).

Philosophy was included too, as when Minsky said that in thinking about "the 'mind–brain' problem" one should look to Craik and Gordon Pask, and that *freedom of will* depends on being able to distinguish our internal plan from intervention in our action (p. 447 n. 38; cf. Chapter 7.i.g). He devoted his penultimate section to 'Models of Oneself', arguing that an intelligent creature's model of the world would need to include a model of itself—which might well need to be "dual", as ours is (he'd explore this view further a few years later: Minsky 1965). The argument from Gödel's theorem (15.v. a) was based, he said, on a misinterpretation of Gödel. And the ineffability of "intelligence" and "creativity" may eventually be overcome. Programmers know, he said, that there's no mysterious "heart" in a program, only (at base) "senseless loops and sequences of trivial operations". Likewise, "[It] may be so with *man*, as with *machine*, that, when we understand finally the structure and program, the feeling of mystery (and self-approbation) will weaken" (p. 447). This was an early expression of functionalism, which Minsky's Cambridge colleague Hilary Putnam (1960) had just defined at length in an influential philosophical journal (15.iii).

In short, Minsky's attitude to these 'foreign' fields was interdisciplinarity, not condescension. He wasn't saying only that NewFAI had something to teach colleagues in other areas. It could learn from them, too.

It's relevant, here, to remark something which Minsky *didn't* say. Nowadays, he's regarded as one of the most optimistic champions of AI. He's tackling hugely difficult problems (Chapters 7.i.e and 12.iii.d), and is notorious for ebullient futuristic remarks. For instance, he once said to me that science would progress more quickly if *all* the funding money were given to AI, because AI programs will eventually surpass the intelligence of human chemists, geologists, biologists . . . and so on. That may have been tongue-in-cheek: he's nothing if not mischievous. But 'Steps' was carefully considered. Having opened by saying "we are on the threshold of an era that will be strongly influenced, and quite possibly dominated, by intelligent problem-solving machines" (p. 406), he could have closed with a vainglorious flourish. Instead, he forecast that AI programs, in the foreseeable future, would be no more than a powerful accessory to human thought:

> [With the advent of time-sharing machines] we can work toward programming what will be, in effect, "thinking aids". In the years to come, we expect that these *man–machine* systems will share, and perhaps for a time be dominant, in our advance toward the development of "artificial intelligence". (1961*b*: 450; italics added)

h. The harbinger in the Bush

That final quotation from 'Steps' had an important implication: if man–machine systems were to be thinking aids for anyone other than computer scientists, they'd need to be usable by the man/woman in the street—or anyway, in the laboratory and the library. Minsky didn't mention this fact. That's hardly surprising, for AI people in the 1950s had enough to do making computers usable by their colleagues, never mind Joe Bloggs. One computer pioneer, however, had already thought about this issue before mid-century: MIT's Vannevar Bush (1890–1974).

Bush's scientific feet were planted firmly in the ground. He'd invented the (analogue) differential analyser in 1930, and was President Roosevelt's chief scientific adviser—in which role, he supervised the Manhattan Project and other military technologies during the Second World War.

Nevertheless, his speculative paper 'As We May Think' (1945, sects. 6–8) was recently described by William Gibson as "so genuinely, so embarrassingly, so rawly prescient as to make a science fiction writer squirm" (W. Gibson 2001, p. xi). This is no small accolade: it was Gibson's hugely influential novel *Neuromancer* (1984) which introduced the public to the concept, and the potential, of cyberspace. (A copy of the magazine containing Bush's paper was recently sold for $2,200: not bad, for something bought to while away a train journey—J. M. Norman 2005.)

What Gibson termed "the weird acuity of this man's imagination" was concerned with what would now be called information technology, not AI as such. Nevertheless, the paper's insights were the first explorations of issues that were later made concrete by GOFAI and which are crucial for the practice of AI today. Accordingly, it was Bush whom I had in mind when I referred, in the preamble above, to "great-grandparent" NewFAI. His work was so hugely far-sighted that it came to look even more exciting as the years passed than it had done at the time.

Bush's declared concern was to enable people to profit from, instead of being overwhelmed by, the explosion of knowledge in the previous fifty years—not least, the research done for the Second World War. Towards the end of the war, Bush—then Director of the Office of Scientific Research—was invited by the widely read *Atlantic Monthly* to suggest what scientists might do, once they had time again for non-military pursuits. His response was a visionary essay on how they could help society cope with the information explosion (Bush 1945). In fact, his vision was already some 10 years old: the magazine contribution was based on a paper he'd written *before* the war, in 1937 (Nyce and Kahn 1989, 1991).

His essay dealt with the future recording, storing, and accessing of scientific records—indeed, of texts and images in general. He forecast many of the IT applications used in offices and libraries today. (Gabriel Naudé would have been fascinated! Indeed, research libraries would be the first institutions to use computers for handling non-mathematical information.) Bush even forecast the personal computer itself. He called this the "memex", or memory extender, and described it as "a sort of mechanized private file and library".

Bush was writing science-as-speculation, but not science fiction. He took pains to relate his suggestions to specific achievements of mid-1940s science and technology—including many examples drawn from cybernetics. He anticipated add-on gizmos (automatic cameras, scanners for storing handwritten text and images, and speech-typewriters) and interface gimmicks (a push-button that would take one straight to the first page of a document's index, or a sliding "lever" that could turn the—virtual—pages of a stored document one, or ten, or twenty . . . at a time). As you'll know from your own experience, the modern versions of these "gizmos" and "gimmicks" aren't mere toys: to the contrary, they're near-essential, today, for many kinds of intellectual research.

Even more to the point, he also anticipated much of the internal software, or functionality. For he was envisaging a machine to aid, and even to model, creative thinking as well as mere (mere??) data retrieval. The basic idea was that the memex, and its user, could create "trails" of associations: stored memory links (aka hyperlinks) that would become increasingly idiosyncratic with personal use. Bush was deliberately contrasting *flexible associative thought* with the mid-century librarian's rigid hierarchical indexing, and imagining a machine that would fit in with it.

Part of that machine would be what we now call a search-engine. One can hardly fail to be reminded of Google, for instance, on reading Bush's description of how a medieval historian might use his own personal memex (1) to find out about the Turkish and English bows used in the Crusades, (2) to compare—and (3) to explain—their efficiency, and even (4) to speculate on why the more effective (Turkish) weapon wasn't eventually adopted by *both* sides.

The need to make digital computers human-friendly would grow inexorably as their power increased. When Bush wrote his piece, however, they were only just being invented (or perhaps one should say "reinvented", with a salute to a crowded living room in pre-war Germany: Chapter 3.v.a). Very little, as yet, could be done to make the memex a practical proposition.

Indeed, it didn't become practical in the research-lab sense until the 1960s, nor in the commercial sense until the 1980s. As remarked in Chapter 1.iii.g, then, ideas (such as hypertext) crucial to the memex were born/reborn several times within a half-century.

In 1959 Douglas Engelbart's team at the Stanford Research Institute (SRI) started trying to put Bush's vision into effect. Engelbart (1925–) explicitly acknowledged that he'd been inspired by Bush (1962: 47–69). Indeed, he'd been fired up by Bush's ideas as early as 1945:

> At the end of the summer of 1945, just after the surrender of Japan, Engelbart was a twenty-year-old American naval radar technician, waiting for his ship home from the Philippines. One muggy day, he wandered into a Red Cross library that was built up on stilts, like a native hut. "It was quiet and cool and airy inside, with lots of polished bamboo and books. That was where I ran across that article by Vannevar Bush", Engelbart recalls. . . . [He] started designing computer-based problem-solving systems in 1951. (Rheingold 2000: 174–5)

Like Bush, he was thinking of a very wide range of users: "diplomats, executives, social scientists, life scientists, physical scientists, attorneys, designers". He'd also been influenced by his experiences with radar during the war, from which he concluded that a graphical display, changing in real time, might be linked to a computer—from which instructions could be fed back to alter the display.

His preliminary report on the project, prepared for the US Air Force Office of Scientific Research in October 1962, listed the team's goals partly as specific descriptions/predictions of technology (e.g. word processors), and partly as various futuristic dialogues (1962: 73–114). The latter featured an imaginary user and teacher:

> [You, as a neophyte user, will be talking to] a friendly fellow (named Joe), who is a trained and experienced user of such an augmentation system within an experimental research program which is several years beyond our present stage . . .
>
> Joe understands [that you don't know much about these matters], and explains that he will do his best to give you the valid conceptual feel that you want—trying to tread the narrow line between being too detailed and losing your over-all view and being too general and not providing you with a solid feel for what goes on. (Engelbart 1962: 73)

Those fictional dialogues with Joe are remarkable, now, for their 'modern' feel: similar interchanges go on every day in homes, computer shops, and introductory teaching labs. In addition, Engelbart foresaw graphic displays of structures which could be explored and inspected from different points of view and at different levels of detail—by an architect or a road designer, for example. (As it happened, Ivan Sutherland's *Sketchpad* was first described in the very year that Engelbart composed his report, 1962: see Chapter 13.v.c.)

At the time, however, the dialogues seemed not just fictional but science-fictional. Despite being selected as the opening paper in a forward-looking collection on information technology (Engelbart 1963), they didn't enthuse his colleagues:

> Total silence from the computer science community greeted the announcement of the conceptual framework Engelbart had thought about and worked to articulate for over a decade. (Rheingold 2000: 180)

However, ARPA's Joseph Licklider had the imagination—and the intellectual fellow feeling—to get the point. He supported (generously, as ever) Engelbart's efforts to turn some of this fiction into reliably functioning reality. It took almost a decade.

At the 1968 Fall Computer Conference in San Francisco, Engelbart's team were able to demonstrate their own memex—equipped with the ancestral computer mouse. They called it NLS—or oNLine System (Nyce and Kahn 1991). This was a personalized version of human–computer "synergy", or symbiosis (Engelbart 1962: 79 ff.; cf. Licklider 1960). Three thousand computer scientists, of whom Alan Kay (1940–) was one, attended a two-hour demonstration—followed by a standing ovation. The session is now seen as "a landmark event in the history of computing" (Packer and Jordan 2001, p. xvi). Indeed, a film of the event was screened at a thirtieth-anniversary meeting at Stanford in 1998, at which the 73-year-old Engelbart received another standing ovation (Rheingold 2000: 326).

But "reliably functioning reality" in a laboratory demo isn't necessarily a reality that the man in the street can rely on. Stewart Brand, who helped with the original demonstration, soon spread the word about personal computing in his hugely influential *Whole Earth Catalog* (Chapter 1.iii.d). But it would be many years before Kay and others would be able to turn Engelbart's ideas into *commercial* realities (13.v).

A list of the pioneering features of NLS is doubly familiar. Besides reminding one of IT facilities taken for granted today, it recalls the speculations in Bush's memex paper, now 60 years old—or 70, if one considers the 1937 draft. For NLS provided the first versions of

* text linkage (not called "trails of associations", but "chains of views");
* hierarchical hypertext (the term is Ted Nelson's, coined in 1963: cf. Nelson 1965, 1974), later used as the core of the World Wide Web (Berners-Lee 1989);
* keyword search;
* whole-text retrieval by keyword;
* links between text and graphics;
* annotation;
* message passing (i.e. email);
* word-processing (copying, reordering, and adapting written text);
* collaborative working;
* multiple (non-overlapping) windows;
* the screen cursor ("bug");
* and, last but not least, that now familiar animal the computer mouse ("I don't know why we call it a mouse. It started that way and we never changed it": Engelbart n.d.).

Some of these features were already being taken up in the ARPAnet—whose very first message was sent (from UCLA) to Engelbart's lab (Packer and Jordan 2001, p. xvi). Moreover, all were routinely used by Engelbart at SRI. And they caused a sensation at the San Francisco meeting, as we've seen. But they *didn't* immediately spread into the IT community as a whole, still less to Everyman. The mouse, for instance, wouldn't start reproducing merrily—and commercially—until the 1980s.

By that time (thanks largely to Kay), the mouse, windows, icons, and text-processing had not only been much improved but had been made accessible to anyone with a PC on their desk. At long last, historians really could use their private machines to do research on the Crusades.

A final word: the memex was a harbinger of more than desktop technology. Engelbart, though apparently not Bush, saw clearly that a memex/NLS machine would *change* human minds as well as aid them. For instance, word-processing would not only aid but also affect the creative process of composing texts (1962: 74 ff.). In general, it would enable the human computer user to have "the freedom and power of disorderly [thought] processes", which in the early 1960s was impossible (p. 89).

The psychologists Bruner and Miller had recently shown that we process differently structured concepts in different ways (6.ii.b). And in his early 1960s work on cognitive technologies (6.ii.c), Bruner was pointing out that pervasive systems of representation, or "technologies"—such as language, drawing, and writing—enter into the developing mind, shaping it as well as helping it. In 1962 Engelbart—besides remarking that the precise nature of future technologies would depend on "our [psychological] understanding of the human being", including how we process distinct "concept structures" (pp. 70, 85)—was saying much the same:

> In a very real sense, as represented by the steady evolution of our augmentation means [writing, printing, libraries . . .], the development of "artificial intelligence" has been going on for centuries. (Engelbart 1962: 79)

He predicted that in "augmenting" human intellect by using memex/NLS, information technology would provide the thinking processes listed above so as to enable people from all professions to solve "problems that before seemed insoluble", which may have lasted "for twenty minutes or twenty years". But increasing someone's capability "to approach a complex problem situation, to gain comprehension to suit his particular needs, and to derive solutions to problems" in this way wouldn't simply be adding more of the same, nor even providing a new box of tools. Rather, it would be changing the essence of how we think:

> *We do not speak of isolated clever tricks that help in particular situations. We refer to a way of life in an integrated domain* where hunches, cut-and-try, intangibles, and the human "feel for a situation" usefully co-exist with powerful concepts, streamlined terminology and notation, sophisticated methods, and high-powered electronic aids. (Engelbart 1962: 1; italics added)

Forty years later, some philosophers would take the notion of cognitive technologies to heart in a highly provocative way (Chapter 16.vii.d). Following the writings of Andy Clark rather than Bruner, they argued that the mind, the self, is part *constituted* by external technologies and systems of representation. The neo-Cartesian 'mind in the head', and even the phenomenologists' 'mind in the body, ending at the skin', was dismissed. Much as anthropologists—and Wilhelm von Humboldt—had long claimed that culture and language are integral to mind (see Chapters 8 and 9.iv.a–b), so Clark was now arguing that an Alzheimer patient's notebook, or a modern person's PC, are—literally—part of their mind, or self.

The philosopher Timothy van Gelder (a sceptic with respect to GOFAI: 16.vii.c) applied this idea to Engelbart's work. In a paper proclaiming Engelbart to be Turing's equal, when judged as "the most important pioneer in the general field of computing and intelligence", van Gelder said this:

> Engelbart's vision of computers augmenting human intelligence is, properly understood, *a vision of human self-transformation* through a bootstrapping process in which our current, technologically augmented intellectual capacities enable us *to refashion the spaces and practices within which we ontologically self-constitute*. Moreover, his crucial insight was that computer technology will be more profoundly and intimately connected with *the process of self-constitution through enhanced rational self-expression* than any previous technological forms. (van Gelder 2005, last page; italics added)

In other words, 'mind as machine' was being given an ontological, not merely an explanatory, air.

i. Spacewar

One harbinger seemed to many observers at the time to be trivial in the extreme. It was the first computer game, MIT's Spacewar (Levy 1994: 58–69).

It was also the first example of interactive computer graphics, where one could alter the picture on the VDU screen at will. Non-interactive graphics had been around for a decade or more. For instance, when Simon had visited RAND in 1952 he'd been met by "an eye opener. They had this marvelous device there for simulating [radar-blip] maps on old tabulating machines. Here you were, using this thing not to print out statistics, but to print out a picture, which the map was" (interview in McCorduck 1979: 125).

But you couldn't use RAND's "old card calculators" to change the picture before your very eyes.

Spacewar was seeded by Minsky's accidental discovery in about 1960 of how to make his PDP-1 display a circle. Initially, he used his Circle Algorithm to display particle interactions graphically on a cathode-ray tube screen. But the technique was soon adapted by playful MIT graduate students to allow them to pretend to chase and shoot down rockets in outer space.

The basic game was implemented by February 1962. But it was soon much improved, because the students spent most of their spare time playing it, and tinkering with it. By early 1963 (when I played with it on my first visit to MIT) Spacewar had rockets, torpedoes, explosions, sun, stars, constellations—and a joystick. You could use the sun's gravitational pull to increase your speed, provided that you didn't get too close. And you could escape into hyperspace by pushing a panic button, although you never knew just where you'd come out (if you were right next to the sun, it would gobble you up).

That wasn't the end of the line. The MIT hackers constantly advanced the program, the latest version being left in a drawer for the next person to upgrade. In fact, "the next person" might not even need to touch the drawer, for by 1963 the Spacewar code had been sent to the hackers' friends all over the USA. Eventually, the game spawned the "Intergalactic spacewar olympics" at Stanford, a heady meeting of enthusiasts reported in the magazine *Rolling Stone* and commemorated by the famous New York photographer Annie Liebowitz (Brand 1972). Minsky (1984*a*: 248) recalls that by 1965 the game had already become so addictive that daytime playing had to be banned, to leave the computer free for real research.

But how real was real? Spacewar may have seemed trivial to the outsider and trendy to Liebowitz, but over the years it would have a lot of highly practical spin-offs. For example, it soon contributed ideas to simulated displays for training pilots of aircraft and space vehicles. It was used by DEC as a diagnostic program for the PDP-1 (Levy 1994: 65). And it was a forerunner of the hugely more complex computer games we know today—indeed, of computer graphics and 'virtual reality', or VR, in general (see 13.vi).

j. The empty chair at the banquet

Computer graphics of a type hugely more sophisticated than Spacewar were already being seeded elsewhere in MIT—but the Spacewar hackers didn't know that. Indeed, even the specialists in computer graphics didn't know about it. They found out from an unscheduled talk, arranged at the last minute by Licklider, at their first official meeting in 1962 (Rheingold 2000: 149).

Ivan Sutherland, then a graduate student working in MIT's Lincoln Laboratory, had written a revolutionary computer-graphics program called Sketchpad (I. E. Sutherland 1963; Blackwell and Rodden 2003). Where Spacewar dealt with moving spots of light, Sketchpad enabled one to draw—or to instruct the computer to draw—pictures or geometrical diagrams of 2D or 3D structures. Amazingly, the diagrams could be moved, rotated, size-varied, shape-distorted, and even tidied up in various ways.

Sketchpad could well have been included here as yet another harbinger. However, it's described in Chapter 13.v.c instead. In short, there's an empty chair at our harbingers' banquet. (More accurately, there are two: remember Pandemonium.)

10.ii. Establishment

While cognitive science as a whole was being officially established by way of newly founded research groups and new publication practices (Chapter 6.iv–v), symbolic AI was coming into official existence too. But there was no rush to climb onto the bandwagon. Throughout the 1960s and early 1970s it would be studied in relatively few places, most of them in the USA—plus a handful in the UK and elsewhere (Edwards 1996, ch. 8; McCorduck 1979, chs. 5–7; Fleck 1982).

More accurately, it was being *bureaucratically recognized and officially funded* in only a few places. Interested individuals, of course, were scattered much more widely. In Bombay (Mumbai), for example, Rangaswamy Narasimhan (1926–), who'd directed the design of India's first operational digital computer, was doing early work on computer vision at the Tata Institute (Narasimhan 1964, 1966). (His interest in AI eventually led to the world-leading NCST: National Centre for Software Technology, now in a large building in Bombay's Juhu district; some twenty years ago, NCST won the international tender for the new train-scheduling program for the London Underground.)

Narasimhan wasn't the only one. Most computer science departments, in whatever country, contained one or two mavericks who spent some of their time on this new pursuit. A computer science department, however, wasn't the same thing as an AI department. And departments of electronics and/or engineering, which might also harbour a couple of AI 'oddballs', were different again. How did AI become established in its own right?

a. First labs

Several of the earliest AI workers were employees of IBM or RAND. These institutions weren't concerned with AI as an academic discipline, nor with encouraging youngsters into the field. Nevertheless, they both contributed greatly to it.

For instance, IBM supported Samuel (Section i.e, above) and RAND gave houseroom to Newell and Simon (6.iii.b–c). In addition, IBM played an important role in the Dartmouth Summer School, in the persons of Rochester, Samuel, Gelernter, and Alex Bernstein (6.iv.a). Besides IBM's regular employees, there were the occasional visitors—such as McCarthy. He'd spent the summer of 1955 at IBM Poughkeepsie, at the invitation of Rochester—who recommended the Dartmouth proposal because McCarthy "had been bending his ear about AI's potential for some time" (J. McCarthy, personal communication).

The consulting company Bolt, Beranek & Newman (BBN) was another very early AI player (Alperin *et al.* 2001: 7–37; Walden and Nickerson 2005). Temporarily lodged in two rooms inside MIT, BBN was founded in 1948 to develop the acoustic technologies initiated in the Second World War by (among others) the psychologists Licklider and John Swets—both of whom would join the company in its first ten years. (The start-up had been prompted by a vision of peace, not war: the United Nations building in New York. MIT's Richard Bolt won the architect's contract to advise on the acoustics, and decided that he needed Leo Beranek's help; and Robert Newman, then a graduate student in architecture, was soon enrolled too—Alperin *et al.* 2001: 13.) Psychology pervaded BBN from the beginning, both as informational theories of perception and attention (see 6.i and ii.a–b) and as a concern with man–machine integration. At first, that meant operator–tool environments like those studied by Kenneth Craik (4.vi) and Donald Broadbent (6.i.c). But in 1957, when digital computers came on the scene, Beranek hired Licklider specifically to move the firm nearer to them (Beranek 2005; Swets 2005). Licklider remained until 1962, and—given his key role in the history of AI (see below, and Chapter 11.i.b)—it's not surprising that BBN eventually employed several people mentioned in this book (including Oliver Selfridge, William Woods, and Ronald Brachman).

In 1966 the commercially oriented Stanford Research Institute (SRI) started an AI Center to do contract research. At first, most of its projects were government-funded, via ARPA. It soon became well known for its work on robotics and planning, and for vision research interestingly different from that done at MIT. Although these aspects are described below in separate subsections (iii.c and iv.b), and were studied separately in other labs, SRI's robot SHAKEY—so named because it wobbled—was one of the first attempts to integrate all three.

Another such attempt was the Edinburgh University robot FREDDY (Barrow and Salter 1969; Barrow and Crawford 1972; Michie 1973*b*). This was officially known as the Mark 1.5 project, but its more widely used name came from the acronym FREDERICK: Friendly Robot for Education, Discussion and Entertainment, the Retrieval of Information, and the Collation of Knowledge. (Edinburgh's leader, Donald Michie, was nothing if not ambitious!—see Chapter 11.iv.)

Soon afterwards, in 1970, Xerox set up their Palo Alto Research Center. (Largely because of Kay, Xerox PARC would eventually spawn a plethora of advances in human-centred AI: 13.v–vi.)

If NewFAI was to get off the ground at rapidly increasing speed, however, university-based laboratories—supporting graduate/undergraduate courses—would be needed. The first two were set up at the end of the 1950s: at CMU under Simon (6.iii.b–c), and at MIT under Minsky and McCarthy.

(In a different political climate, the honours might have gone to another Massachusetts institution instead, namely Tufts University. For a new department of "Systems Analysis" was opened there in the mid-1950s, of which Minsky was an early member. He was also one of the last members, for the department was closed by the Tufts authorities very soon after it had opened. The reason was that two of its founders, the philosopher Richard Rudner and the psychologist William Schutz, had been placed on the list of political "subversives" compiled by Senator Joseph McCarthy—Crevier 1993: 64. As a result of the witch-hunt at Tufts, Minsky left for the Lincoln lab in 1957; a year later, he transferred to MIT—initially to the maths department, but switching very soon to electrical engineering, where McCarthy was already a member.)

One might assume that MIT's first official AI project was planned at a pre-arranged meeting in someone's office, and shoehorned into official existence by several carefully crafted pages of pleas, promises, and justifications—circulated, in pristine folders, to a dozen busy committee members. Not a bit of it:

> It was all very informal. One day in 1958, Minsky and I met in the corridor of Building 26, and said "Let's have an AI project! Yes, that's a good idea!" Along came Jerry Wiesner, the head of

the Research Lab of Electronics [and President of MIT from 1972 to 1980], and he asked us—in the corridor—what we needed. We said: "A room, a secretary, a key-punch, and maybe two programmers?" He said, "And what about 6 grad. students?" MIT had just had a Joint Services (i.e. Army etc.) contract in electronics, related to defence, and he had all these extra students. A burnt-out computer—which was supposed to be ultra-safe!—gave us the room. (J. McCarthy, personal communication)

This was typical of Jerome Wiesner (1915–94). He was already renowned for having made the RLE, from 1952, not merely an electronics workshop but a wide-ranging interdisciplinary centre:

> At one stage we had twelve different fields represented in the laboratory. The Linguistics and Psychology departments grew out of groups that were started in the lab.
>
> [RLE's communication engineers were joined by] neurophysiologists and other biologists, linguists, economists, social scientists, and psychologists of the various persuasions . . . They explored each other's fields and slowly began to comprehend each other's lingo and exhibit that spirit of mental intoxication that characterizes the pursuit of an exciting idea . . . The two decades of RLE were like an instantaneous explosion of knowledge. (interviewed/quoted in Brand 1988: 134–5)

In short, MIT's AI Lab had an interdisciplinary home right from the start. It's little wonder that the second co-director (Papert) was a psychologist and (originally) a mathematician, not a computer scientist. And it's little wonder that MIT's Linguistics Department—headed by Chomsky (9.vi.a)—had a strongly computational slant. (Computational, but not computer *modelling*: not only did Chomsky eschew programming, but he was already distancing himself from his AI colleagues: 9.x.b.)

Generosity didn't come only from Wiesner, but—five years later—from ARPA too. Both Minsky and McCarthy had close ties with ARPA's first director, Licklider. An MIT psychologist with an early interest in computers, and in library science (Chapter 5.iv.f), Licklider had been on the Air Force's Scientific Advisory Board in the late 1950s, overseeing the ambitious SAGE project (11.i.b). As McCarthy remembers it:

> Licklider said "You're spending millions of dollars, but you're not looking at the (computer) science to support it." So in 1962 they said "OK, if you'll come to Washington to be involved, we'll do it." (personal communication)

Once he was involved, Licklider supported both hardware research and software research too. And his "support" was unstinting:

> [When] his office decided to support a project, that meant providing thirty or forty times the budget that the researchers were accustomed to, along with access to state-of-the-art research technology and a mandate to think big and think fast. (Rheingold 2000: 147)

For example, MIT's Project MAC received the equivalent of $25 million in mid-1990s values from ARPA between 1963 and 1970 (Edwards 1996: 269). It was directed by Edward Fredkin, a co-author—with Licklider and McCarthy—of the first paper on time-sharing (Boilen *et al.* 1963). (Later, from 1968, it would be directed by Licklider himself.) The MAC acronym was deliberately ambiguous, being variously interpreted as Multi-Access Computing (i.e. time-sharing), Man And Computers, and Machine-Aided Cognition—the last two of which recalled Licklider's "Man–Computer Symbiosis" (11.i.b).

As this catholicity implies, Project MAC covered a wide range of research, of which not all was AI—and not all of that, psychological AI. Nonetheless, NewFAI—whether psychological or technological—found shelter under the MAC umbrella. Indeed, the AI Lab set up by Minsky and McCarthy in 1959—with three whole floors of a modern building set aside for it—became a subdivision of Project MAC before graduating as a stand-alone department.

With hindsight, McCarthy sees Licklider's championship as the main reason why AI got established as early as it did, and why this happened in the USA rather than the UK—where early connectionist research, and computer science too, was at least as healthy as in America (Chapters 3.v.b–d and 12.ii.a–b). As he puts it:

> The "AI establishment" owes little to the general "scientific establishment". AI would have developed much more slowly in the U.S. if we had had to persuade the general run of physicists, mathematicians, biologists, psychologists or electrical engineers on advisory committees to allow substantial NSF money to be allocated to AI research.
>
> . . . AI was one of the computer science areas Licklider and his successors at DARPA consider relevant to Department of Defence problems. The scientific establishment was only minimally, if at all, consulted. In contrast European AI research long depended on crumbs left by the more established sciences. (McCarthy 1989)

(How those "crumbs" were first gleaned in the UK, and why they dried up in the early 1970s only to be restored ten years later, is described in Chapter 11.i.a, iv.a, and v.c.)

Initially, the MIT AI Lab was very much a joint enterprise between Minsky and McCarthy, who'd already worked together on organizing the Dartmouth meeting. Even in their 1955 funding application for that summer project, however, there'd been a clear difference between them. In a nutshell, McCarthy focused on logic, Minsky on robotics—including vision and anticipatory planning.

Minsky intended to study the machine learning of sensori-motor representations ("abstractions"), and problem solving by exploration of an internal model of the environment before experimental trials in the external world. (Because of this anticipatory exploration, he said, the behaviour would seem "rather clever", even "rather 'imaginative' "—McCarthy *et al.* 1955: 49.) McCarthy, by contrast, wanted to study "the relation between language and intelligence", in which "the trial and error processes at a higher level [than sensory data and motor activity] frequently take the form of formulating conjectures and testing them" (52–3). To do that, he'd need to construct "an artificial language" with some core strengths of English (i.e. LISP: see v.c, below). This would enable a computer to deal with conjecture and self-reference, to formulate short informal arguments, and to represent everyday notions like "physical object" and "event" (iii.e, below).

Eventually, this difference in research interests—"I never had rap sessions with Minsky" (J. McCarthy, personal communication)—led to McCarthy's decision to leave MIT in 1962 for Stanford, to establish the AI Group there a year later. (Papert took over as co-director of the MIT laboratory, and he and Minsky ran it together until 1972.) The East Coast versus West Coast rivalry that developed over the years wasn't merely about prestige or even money, but about two competing visions of what AI should aim to do. (Notoriously, papers from Stanford would be cited only rarely in work coming out of MIT.) Since AI didn't yet exist at Stanford, although computer science did, it could be moulded to suit McCarthy's own priorities.

Again, Licklider was crucial. When the Stanford AI Group was set up, "Licklider simply asked McCarthy what he wanted and then gave it to him" (Edwards 1996: 270). Later, he admitted: "It seemed obvious to me that he should have a laboratory supported by ARPA . . . So I wrote him a contract at that time." But Licklider was much more to McCarthy than an ARPA sugar-daddy. He was an intellectual partner too. As noted above, they both helped write the pioneering paper on time-sharing (Boilen *et al.* 1963). Indeed, Licklider—who put huge sums of money into the development of time-sharing as soon as he was appointed to ARPA—had been enthused by McCarthy's vision of it in the first place (see 11.i.b).

In fact, things didn't turn out quite as McCarthy had expected. For the department soon split. Largely due to Feigenbaum's presence there, a separate group—the Heuristic Programming Project—was set up in 1965, later metamorphosing into the Knowledge Systems Laboratory. McCarthy's group was now known as SAIL (the Stanford AI Laboratory). The emphases of the two research clusters were very different. McCarthy and his students were concentrating on highly abstract issues of logical reasoning and representation, based largely on "resolution" theorem proving—first formulated very soon after the founding of the department (Section iii.b). Feigenbaum's group, by contrast, were building practically useful expert systems, using facts and rules of thumb provided by domain specialists—medics, chemists, geologists, and so forth (Section iv.c).

CMU, too, benefited greatly from Licklider's largesse. But if Licklider was generous, he was also highly discriminating. When the J. Arthur Sloan Foundation provided $15 million for cognitive science in the late 1970s, they deliberately spread most of the money widely, and therefore thinly (Chapter 8.i.c). Licklider took a very different view. He felt that he knew who the top AI people were, and that between them they should get virtually all the ARPA money available. As a result, each favoured recipient got many times more than the average funding for university laboratories. That's why MIT, Stanford, and CMU quickly became *the* main university centres for AI research.

Licklider's faith in his small band of AI pioneers became doubly important in 1964, when the field suffered its first funding bombshell. A US government advisory committee blasted machine translation, declaring it well-nigh worthless—with dire consequences for MT researchers seeking to finance their work (see 9.x.e). The twenty-year shadow of ALPAC might well have been cast over other areas of AI too, whether language-based or not. And perhaps it was: the document was widely reported in the newspapers, and some university committees may have reconsidered the wisdom of embarking on AI as a result. In the few favoured labs, however, Licklider's support endured.

Meanwhile, the NewFAI gospel—in particular, its psychological version—was being spread across the seas (Chapter 6.iv.e). In 1963 Michie, enthused by his recent visit to the USA, founded a small research group with Bernard Meltzer. In 1966 this spawned Edinburgh's Department of Machine Intelligence and Perception, and (Meltzer's baby) its Department of Computational Logic. Other transatlantic visitors to MIT in this period included Ratio member Donald Mackay, and N. Stuart Sutherland—who founded Sussex University's Experimental Psychology Department in 1964 with NewFAI very much on his mind.

Visitors to the USA from even further afield included Narasimhan, who spent 1961–4 as a Visiting Scientist at the computing laboratory of the University of Illinois, Urbana (Narasimhan 2004: 250). On his return to India, he founded an AI group at the Tata Institute which soon became the National Centre for Software Technology. This was so successful that in 1986 it moved to a separate building, almost as large—if not so extraordinarily luxurious—as the Tata Institute itself.

b. The ripples spread

The AI harbingers described in Section i enthused the pioneers of cognitive science, psychologists included (Chapter 6.iv). But the NewFAI adventurers were a very small community. Moreover, they couldn't multiply the number of functioning programs overnight, since much of their effort had to go into designing new programming languages (Section v.a–e, below).

To make things worse, AI research was available only in informal laboratory research reports, or scattered across a bewildering variety of journals. Even by the end of the 1950s, then, its achievements still weren't widely known.

The possibility of NewFAI, however, was now in the air—thanks, in part, to the reprinting of Turing's daringly imaginative *Mind* paper in James Newman's *The World of Mathematics* (1956). At 2,500 pages, this boxed four-volume collection provided dozens of attractions to vie with Turing's essay—many of them already classics. But Newman's edition turned that paper into a classic too. By the time it was reprinted again in *Computers and Thought* (see below), it was already being described as "one of the best-known papers" about "the existence or nonexistence of various kinds of theoretical upper bounds on the intelligence of computing devices" (Feigenbaum and Feldman 1963: 9).

Some early programs, and their potential implications for psychology, were described in 1960, in the cognitive science 'manifesto' *Plans and the Structure of Behavior* (Chapter 6.iv.c). But the key surge of AI visibility occurred three years later. For in 1963 things changed suddenly. The very newest of NewFAI programs (the strong sense was obligatory at last: i.a, above) reached the public via Feigenbaum and Feldman's best-selling collection *Computers and Thought.*

Virtually every program later discussed by Dreyfus in his cat-among-the-pigeons RAND memo, for instance, was included there (see 11.ii.a). So were four crucial programmatic essays: 'Steps', 'Chess and Complexity', 'Attitudes Toward Intelligent Machines', and Turing's 1950 paper—the last, now placed in an appropriately *technical* context (but still mistakenly described, by the editors, as primarily concerned with the Turing Test: pp. 9–10). Admitting that AI programs were still at the low end of Armer's "continuum" of intelligence, the two young editors ended their Introduction with a rousing call to action:

> What is important is that we continue to strike out in the direction of the milestone that represents the capabilities of human intelligence. Is there any reason to suppose that we shall never get there? None whatever. Not a single piece of evidence, no logical argument, no proof or theorem has ever been advanced which demonstrates an insurmountable hurdle along the continuum. (Feigenbaum and Feldman 1963: 8)

This book was hugely influential, and is still reprinted regularly. The royalties would soon be enough to support a prize for outstanding new work, awarded at the biennial International Joint Conference on Artificial Intelligence, or IJCAI. As the editors had hoped, many readers made AI their profession as a result. It would be over twenty years before another AI collection would have such an explosive effect, on readers young and old (12.vi.a).

The same year saw a collection devoted to NewFAI models of motivation and emotion (Tomkins and Messick 1963). Although this created a flurry at the time, it was grossly premature and fairly soon forgotten (see Chapter 7.i.a–c). Five years later, Minsky (1968) edited *Semantic Information Processing,* which described the NewFAI research being done by his students at MIT. And in 1975 Patrick Winston (1943–), by then running MIT's AI Lab (which he continued to do until 1997), edited *The Psychology of Computer Vision.* This book was read by many people with scant interest in computer vision, because it contained Minsky's paper on "frames"—a hugely influential discussion of schemata in general, and how they could be implemented in AI (see iii.a, below). By the mid-1970s, a fifth widely read collection on psychologically oriented AI had appeared: *Computer Models of Thought and Language* (Schank and Colby 1973).

The floodgates had opened. NewFAI work was circulating outside the laboratory walls, and attracting responses from people in various non-AI departments, including some in the social sciences and humanities. The Association for Computing Machinery's SIGART (Special Interest Group on ARTificial Intelligence) had been publishing a newsletter since the early 1960s. In 1979–80 the American Association for Artificial Intelligence (AAAI) was set up, with Newell as its first President. (For a silver-anniversary retrospective on AAAI, see Feigenbaum 2005.) Meanwhile, AISB had already got started across the pond in the mid-1960s (Chapter 6.v.c). But largely because of a bold political move in Japan, AAAI was showered with money almost from the start (see 11.v.b). Unlike most academic or professional societies, it was able to do a great deal in encouraging its area of interest.

As for regular publications devoted to the new discipline, these were now well established. By the end of the 1960s, six volumes from Michie's Machine Intelligence workshops in Edinburgh had appeared, and a seventh was already in press. The journal *Artificial Intelligence* was about to be founded (in 1970), with Edinburgh's Meltzer as Editor-in-Chief and SRI's Bertram Raphael (1936–) as Associate Editor. The *International Journal of Man–Machine Studies* had made its debut in 1969—the year which also saw the first meeting of IJCAI, held in Washington DC. As befitted the Swinging Sixties, AI was in full swing.

The philosophers helped. Following on Putnam's highly abstract 1960 statement of functionalism (in which AI wasn't mentioned), Jerry Fodor (1968) defined psychological explanation in terms of computation and AI models (16.iv.c). The psychologists helped too: Bruner's Center for Cognitive Studies encouraged psychologists to take an interest in AI—and supported AI's claim to psychological relevance (6.ii).

So far, so Sixties. The Seventies saw the students of the true pioneers setting up specialist laboratories in particular sub-fields. For example, Newell's student Feigenbaum founded the Stanford Heuristic Programming Project and the Knowledge Systems Laboratory, also at Stanford, to focus on expert systems. And the first three monographs on AI appeared in the mid-1970s. These were Raphael's highly readable trade book *The Thinking Computer: Mind Inside Matter* (1976), which—among other things—gave an insider's view on the SRI robot SHAKEY; Winston's welcome textbook *Artificial Intelligence* (1977), which explained how students could use LISP to code the programs it described; and my own *Artificial Intelligence and Natural Man* (1977), which reported not only what AI programs could do and how, but also what they couldn't (yet?) do and why—and which related AI to psychology, philosophy, and social concerns.

Thanks in part to those three books, the general interest in AI, first aroused by *Computers and Thought,* grew phenomenally. During the last years of the 1970s, students were flocking to the already established laboratories, and others were being set up elsewhere. At the same time, the journalists were getting in on the act.

Such was the interest that Feigenbaum initiated a second, more systematic, publication on AI. Whereas *Computers and Thought* had been intended to whet people's appetites, *The Handbook of Artificial Intelligence* advised them on what to swallow—and how to chew it. "Most scientists and engineers," the editors said, "though very knowledgeable about the "standard" computing methods . . . simply had never heard about symbolic computation or Artificial Intelligence" (Barr and Feigenbaum 1981: 12). The *Handbook* should function as "A self-help encyclopedia to which the aspiring practitioner could turn for explanations of fundamental ideas, descriptions of methods, and discussions of well-known programs as case studies". The three volumes appeared close on each other's heels, a total of 1,466 pages in just over a twelve-month (Barr and Feigenbaum 1981, 1982; P. R. Cohen and Feigenbaum 1982).

In brief, the ripples had spread. But it's worth noting that connectionism wasn't mentioned in the *Handbook.*

Perceptrons (which had been prominent in Feigenbaum's *Computers and Thought)* were given only two pages, and the chapter on learning opened with a curt dismissal of the approach:

> [Perceptrons] failed to produce systems of any complexity or intelligence.
>
> Theoretical limitations were discovered that dampened the optimism of these early AI researchers (see Minsky and Papert, 1969). In the 1960s, attention moved away from learning toward knowledge-based problem solving. . . . *Those people who continued to work with adaptive systems ceased to consider themselves AI researchers*; their research branched off to become a sub-area of linear systems theory. Adaptive-systems techniques are presently applied to problems in pattern recognition and control theory. (P. R. Cohen and Feigenbaum 1982: 325–6; italics added)

The 'Vision' chapter included brief accounts of relaxation techniques (12.v.h) and Marrian low-level vision (7.v.b–d), but "parallel processing/search" had only eight index entries for all 1,466 pages.

These editorial judgements were made after consulting dozens of AI experts about the new publication (Cohen and Feigenbaum 1982, pp. xi–xii). In 1980, then, "artificial intelligence" was being perceived by most people simply as GOFAI. If connectionism was being ignored in the *Handbook of AI*, it's not too surprising that many people still use the term AI (wrongly) to cover only the symbolic variety.

c. New waves

The Journalistic ripples soon spread too. Omnibus vehicles such as *Artificial Intelligence* and the *International Journal of Man–Machine Studies* were supplemented in the 1980s by specialist journals devoted to particular sub-fields of AI.

In the 1990s, the numbers of such publications burgeoned. There are now several each in fields such as planning, problem solving, theorem proving, machine learning, computer vision, emotion modelling, legal AI, medical AI, diagrammatic reasoning, robotics, educational AI, virtual reality . . . and so on, and on. In addition, there are various journals in cognitive science, which often publish AI papers.

Most of these AI and interdisciplinary journals are complemented by their own conference series, regular workshops, and/or societies. In other words, a host of established GOFAI activities now aim to provide the cooperation and communication—including face-to-face conversations—necessary for scientific progress (2.ii.b–c).

Whether they ensure that sufficiently *widespread* communication takes place is another matter, for specialization has brought fragmentation. That's especially damaging if one's ultimate aim is to understand whole organisms, or "the whole iguana" (Dennett 1978*c*). Alan Mackworth (1945–), who entered AI at a time when virtually everyone in the field knew (not just knew *of*) everyone else, has put it like this:

> Unfortunately [given the AI dream of building integrated cognitive robots], current research in AI is highly divergent with little or no overlap between specialized subfields such as computational vision, knowledge representation, robotics, and learning. Each group has its own conferences and journals, and when they do all meet at a single conference, they diverge in parallel sessions. (Sahota and Mackworth 1994: 249)

In sum: the interdisciplinarity so characteristic of NewFAI survives, but under increasing pressure.

10.iii. The Search for Generality

The hopes pushing NewFAI forward in the 1960s were focused less on technology (*pace* Licklider, Engelbart, and Bush) than on a theory of intelligence as such. (That approach was still strong in the early 1970s, leading a well-known British AI researcher to say, "I'm not interested in programs!" M. B. Clowes, personal communication.) This is one reason why so many NewFAI ideas were taken up by psychologists. For example, the various forms of "knowledge representation" discussed below were (eventually) widely employed in theories of and experiments on human memory, and AI models were continually cited in the psychology textbooks (e.g. Klatzky 1980).

For many NewFAI workers, however, "intelligence" usually meant *intelligence in general*. In McCarthy's mind, for instance, human psychology was of no special interest, "except as a clue to possible effective ways of doing tasks" (McCarthy 1989). In drafting the Dartmouth proposal, he hadn't criticized "anybody's" (i.e. the behaviourists') way of studying human behaviour: "I didn't consider it relevant". In particular, he now disclaims being part of "the cognitive revolution":

> [Because I didn't consider psychology to be relevant to AI], whatever revolution there may have been around the time of the Dartmouth Project was *to get away from studying behavior* and to

consider the computer as a tool for solving certain classes of problem. *Thus AI was created as a branch of computer science and not as a branch of psychology*. (McCarthy 1989)

He allows, however, that Newell and Simon ("the only participants who studied human behavior") worked "both in AI as computer science and AI as psychology".

The belief—more accurately, the often unquestioned assumption—was that a relatively small number of general mechanisms (heuristics, search techniques, learning rules, representational methods, architectures . . .) would suffice to explain intelligence in all its forms. Whatever the domain of study, from logic and chess to language and vision, the aim was to uncover that generality. Special constraints might have to be added, if one was interested in modelling human minds—limits on short-term memory capacity, for example (7.iv.b). But the underlying principles would be untouched.

The commitment to generalism was strongest at Stanford, thanks to McCarthy. In the 1970s, Minsky would give it up, now arguing that many different interacting procedures were responsible for intelligence (Minsky 1979). Indeed, he later became known as the most eminent of the "scruffies", while McCarthy was acclaimed as the high priest of the "neats" (see 13.i.c). In the early 1960s, however, generalism was dominant.

Simon was someone who did take pains to examine the generalist assumption, and sought to justify it too. For example, he argued that the core mechanism of deliberation *must* be serial, so as to handle 'what-ifs' without confusion (e.g. Simon 1967). He soon became even more influential than the building of LT/GPS had already made him. His 1962 paper on 'the Architecture of Complexity', and his 1968 Compton Lectures at MIT on *The Sciences of the Artificial*, published in 1969 and never out of print thereafter, were an inspiration (7.iv.a). Together with Minsky's 'Steps', they helped set the NewFAI agenda.

I'll mention only a few examples of NewFAI generalism here. And I'll ignore chess, which isn't my thing. It's sometimes described as the fruitfly of AI, for a great deal of technical work was done on it right from the start—and still is (for some recent examples, see Berliner and Beal 1990). That's not because people wanted an excuse to spend their professional time on their favourite Sunday afternoon pastime, but because they believed that chess involved many of the major aspects of intelligence *in general*. As the GPS triumvirate put it: "If one could devise a successful chess machine, one would seem to have penetrated to the core of human intellectual endeavor" (Newell *et al*. 1958*b*: 39).

Apparently, Everyman agreed. These efforts continually whetted the public's appetite for AI. For example, the first "respectable" chess program was featured widely in the late 1950s media, from the *Scientific American*, through the *New York Times*, to *Life* magazine (A. Bernstein and Roberts 1958). The first exhibition game between the world champion and a computer took place in August 1974 (J. E. Hayes and Levy 1976: 49; see also Good 1968). The chess master David Levy's £500 bet, in 1968, that no program would beat him within ten years caused much publicity, culminating in 1977–8 in two widely reported matches (against Northwestern's Chess 4.5 and 4.7). In both cases, Levy won—much to the satisfaction of the readers of the *New York Times,* faced with the headline 'Chess Master Shows a Computer Who's Boss'.

All those programs were 'pure' software. Deep Thought—named after the computer in *The Hitchhiker's Guide to the Galaxy*—and its successor Deep Blue, which in 1997 did beat the world champion, were very different, for they relied on special-purpose chips. (These were designed by Feng-hsiung Hsu at CMU and then IBM: Hsu 2002.) The 480-chip Deep Blue could search hundreds of millions of chess positions per second. Even so, the chips were optimizing the very general AI technique of blind search, not supporting forms of representation specific to chess—as human players do (cf. Chase and Simon 1973). This enabled the public—and the defeated champion Gary Kasparov, too—to save face: the computer might now appear to be the boss, but only because it was, at base, a bully.

For NewFAI, as remarked above, chess *as such* wasn't the point. Indeed, Minsky deliberately discouraged his MIT students from working on it (interview in McCorduck 1979: 189). Chess interested some AI researchers because it could help them to develop general AI techniques. (Today, this motivation underlies the AAAI's General Game Playing Competition, which aims at programs that can play any formally described game by using general methods of representation, reasoning, and learning: see <http://games.stanford.edu>.)

Most of the 1960s NewFAI programs, in fact, weren't designed simply to achieve a specific task. Rather, they were aimed at the general problems identified by 'Steps'—and, in the late 1960s, even by Dreyfus's *Alchemy and Artificial Intelligence* (see 11.ii.a–b). In applying (so it was widely believed) to men, mice, and Martians, they were intended as contributions to cognitive science as a whole: the study of *all possible minds*.

a. SIP spawns KR

When computers were used only to do simple mathematics, the problem of knowledge representation (KR) wasn't too pressing. Enabling a digital computer to store the fact that "$2 + 2 = 4$", and even enabling it to do the sum, was relatively painless. But things got trickier when the machines began to be used to do goal-directed problem solving, theorem proving, pattern matching, visual scene analysis, diagrammatic reasoning, and—above all—interpretation of verbal texts. Enabling the human user to communicate with the machine in English, for example, was no small feat. "Semantic information processing", or SIP, required new forms of KR, appropriate for non-numerical data and inferences.

(Strictly, people should have spoken of BR, not KR. For the term "knowledge" assumes *truth*, whereas "belief" doesn't. NewFAI writers systematically ignored the K/B distinction. Later AI researchers sometimes allowed for it, especially when discussing how to revise previously drawn conclusions now seen to be false: see McDermott 1974, and 13.i.a. However, they usually retained the term "*knowledge* representation", which had become accepted AI jargon.)

With hindsight, it's surprising how long it took people, even in scientific circles, to realize that computers might be used for SIP. Babbage arguably, and Konrad Zuse certainly, had seen the potential for non-numerical computation (see 3.iv and v.a). Zuse had even suggested using computers to do CAD/CAM carpet design, allowing for deliberate weaving errors to make the carpets appear handmade (1993: 130). And (non-interactive) computer graphics had been used since the late 1940s—by the RAND team, for instance, for radar maps (see 6.iii.b). But none of these people had tried to coax their machines to deal with the meanings carried by ordinary-language sentences.

That possibility became clearer with the theoretical marriage of Turing and the *propositional* calculus in 1943 (Chapter 4.iii.e), with the 1950s efforts in machine translation (9.x.a–d), and with the development of programming languages capable of manipulating items that looked like English words (v.c, below). By the early 1960s, non-numerical computation was no longer left to isolated visionaries. It was being explored by Ph.D. students—at CLRU, MIT, and elsewhere.

Very few people outside the MT and NewFAI communities, however, had got the message. The opening page of *Computers and Thought* bore the sub-heading *What is a Computer? Is it just a "Number Factory"?*, followed by a declaration that a computer isn't just "a high-speed number calculator" but "a general symbol-processing device" (Feigenbaum and Feldman 1963: 1). Evidently, the editors felt that the point needed to be made. And despite the enormous popularity of that collection, it still needed to be made at the end of the decade. Minsky was able to shock his intended readers in the late 1960s by calling his new book "*Semantic* Information Processing", and he had to defend the very possibility of non-formal computing yet again (Minsky 1968: 11–12).

In the general public's eyes, the use of computers to do semantic information processing was most famously illustrated by Joseph Weizenbaum's ELIZA (1966; Boden 1977: 96–7, 106–11). The program's notoriety was based on the widespread beliefs that it had passed the Turing Test, and that this is the criterion for success in AI. Both those beliefs were mistaken (see 16.ii.c).

There was a third reason, however, why the program's high status in the public's perception of NewFAI was unearned—even ironic. For ELIZA *wasn't* an exercise in AI, or anyway not in psychological AI. On the contrary, it was a successor to the very simple game-playing program described in Weizenbaum's first paper (in the newly founded commercial magazine *Datamation* in the late 1950s), significantly titled 'How to Make a Computer *Appear* Intelligent'—the italics being Weizenbaum's (interview in Crevier 1993: 133).

ELIZA was a simple pattern-matcher, with 'canned' responses triggered by keywords or phrases. Weizenbaum assembled a number of "scripts", or sets of domain-specific keywords, one of which would be used by ELIZA on any given run. The most famous of these was the script intended to simulate the type of conversation characteristic of Rogerian psychotherapy—in which the patient's remarks are continually reflected back to them by the therapist.

If Rogerian ELIZA's human interlocutor mentioned their mother or father, sister or brother, it would reply "TELL ME MORE ABOUT YOUR FAMILY". If no keyword was available, ELIZA would say "WHY DO YOU THINK THAT", or pick some previous input of the form "MY *****", and ask "DOES THAT HAVE ANYTHING TO DO WITH THE FACT THAT YOUR *****". Occasionally, this produced startlingly appropriate remarks, such as the program's "DOES THAT HAVE ANYTHING TO DO WITH THE FACT THAT YOUR BOYFRIEND MADE YOU COME HERE" in response to the keyword-free input "bullies". But that was pure chance: ELIZA had no representation of the *meaning* of "father", never mind "bullies". And the nearest it got to syntax was to respond to "I ***** you" by spitting out "WHY DO YOU ***** ME", or to turn "My *****" into "YOUR *****" (as it did in generating the BOYFRIEND response).

It was already possible to do a little better than that. In 1960 an M.Sc. student at MIT, A. V. Phillips, had used ideas from Chomsky's (1957) grammar to write a pattern-matching question-answerer that 'parsed' simple sentences into subject, verb, object, time phrase, and place phrase—everything else being ignored (Bobrow 1968: 151). And M. Ross Quillian (see below) had indicated how linguistic meaning might be represented in a program. Yet Weizenbaum didn't try to soup up ELIZA's syntax, nor its semantics either. Why not? Because he wasn't aiming to make a computer 'understand' language—i.e. represent linguistic meaning. He thought that was impossible (and would say so at length in his 1976 book). He'd written ELIZA simply to demonstrate that a programming language designed for numerical computing (i.e. FORTRAN) could be used to handle words. In short, he wasn't interested in NLP.

He wasn't interested in KR either. He chose to model Rogerian psychotherapy because it's "one of the few examples of . . . natural language conversation in which one of the participating pair is free to assume the pose of knowing *almost nothing* of the real world" (Weizenbaum 1966: 42; italics added). Nor was he trying to produce a prototype of something useful, even though many expert systems would later use similar pattern-matching techniques for the human–computer interface. Indeed, he was appalled when "his" ideas—as he saw it: the priority was disputed (see 7.i.a)—were appropriated by Kenneth Colby for clinical use in psychiatry.

ELIZA was irrelevant for the advance of KR, because stored English sentences were rarely suitable for representing the information being processed by a machine. They were used by Phillips's question-answerer, to be sure, but this involved mere sentence-by-sentence matching—*not* inference. The few NewFAI programs that seemed to be solving problems in English weren't actually solving problems *in English*.

So, for instance, Bert Green's (1927–) pioneering BASEBALL program conversed in English with its users, answering their questions about the statistics of baseball teams/games—such as "How many games did the Yankees play in July?" (B. F. Green *et al.* 1961). But it didn't 'think' in English. It turned the input sentences into algebraic equations, and then solved those. Its algebraic answers were transformed into English output for the user's benefit, not for the machine's. Similarly, Daniel Bobrow's (1935–) STUDENT, one of the programs highlighted in Minsky's SIP book, solved English-language problems such as: *The distance from New York to Los Angeles is 3,000 miles. If the average speed of a jet plane is 600 miles per hour, find the time it takes to travel from New York to Los Angeles by jet*, and *Mary is twice as old as Ann was when Mary was as old as Ann is now. If Mary is 24 years old, how old is Ann?* (Bobrow 1968: 147, 213). But like its predecessor BASEBALL, STUDENT would translate the problem into a set of algebraic equations, to be solved mathematically. English had nothing, essentially, to do with it.

Evidently, then, solving problems and/or answering questions posed in English didn't necessarily mean using English to do so. In fact, English wasn't a good medium for computer KR. Raphael, in his thesis on 'Semantic Information Retrieval', put it like this:

> The most important prerequisite for the [machine's] ability to "understand" is a suitable internal representation, or model, for stored information. This model should be structured so that information relevant for question-answering is easily accessible. *Direct storage of English text is not suitable* since the structure of an English statement generally is not a good representation of the meaning of the statement. (Raphael 1968: 35; italics added)

If English text wasn't a good way of representing semantic information inside the computer, what was? Programs doing SIP had been written—by Samuel, for example—more than ten years before Raphael wrote those words. But those *very* early NewFAI programs hadn't aimed at representational generality, even if they'd aimed at inferential generality. Their data were expressed in a domain-specific form: for checkers, chess, or logic. If computers were to deal with a wider range of topics, some more neutral KR would be needed. Raphael, again: "models which are direct representations of certain kinds of relational information usually are unsuited for use with other relations. [AI needs] a model which can represent semantic content for a wide variety of subject areas" (ibid.). Likewise Minsky, announcing the key claim of his SIP book: "the route toward generality [in AI] must lie partly in . . . the representation of more and better kinds of knowledge" (1968: 13).

The most influential KR-for-SIP at that time, besides logic, was the method of semantic networks developed by Quillian (1931–). These were implemented in the mid- to late 1960s, when Quillian was working at Bolt, Beranek & Newman, or BBN (Quillian 1967, 1968, 1969). But they'd been envisaged when he was a graduate student of Simon's at Carnegie Mellon (Quillian 1961, 1962).

Quillian had come into AI after being trained in sociology. His AI aims were to enable automatic word disambiguation, comparison between word meanings, interpretation of anaphora, and associative inference. But his prime concern, doubtless encouraged by Simon, was to cast light on human memory and language understanding:

> The central question asked in this research has been: What constitutes a reasonable view of how semantic information is organized within a person's memory? In other words: What sort of representational format can permit the "meanings" of words to be stored, so that humanlike use of these meanings [by computers] is possible? (Quillian 1968: 227)

Indeed, over half of his Ph.D. thesis of 1966 would be based on experiments recording the thinking aloud of a woman interpreting language. A summary of the thesis appeared in the psychologists' journal *Behavioral Science* (Quillian 1967), and the writers he cited there included Frederic Bartlett on memory (5.ii.b), Miller on the "Magical Number Seven" (6.i.b), Bruner and Earl Hunt on concept learning (6.ii.b–c and v.a, and iii.d below), and Walter Reitman on thinking (6.v.b and 7.i.b). In addition, he drew on an up-to-date review of what psychologists knew about associative meaning (Deese 1962). And some years later, he did a systematic series of psychological experiments on memory and language (A. M. Collins and Quillian 1972). In short, he was "disposed to consider this model a psychological theory" (1967: 429).

The core insight—that word-ambiguity might be dealt with by looking for intersections of meanings—had been stated in the 1950s, by CLRU's Margaret Masterman and Robert Richens (see Preface, ii, and 9.x.d). And Quillian presented his earliest ideas at a 1961 colloquium in King's College, Cambridge, where the CLRU team reported on the latest version of their computerized thesaurus, or "semantic interlingua". But whereas their focus was solely technological (machine translation), his was primarily psychological. Moreover, his methodology was very different from the analyses/programs used at CLRU.

Specifically, Quillian pioneered localist connectionism (12.ii.i). (David Rumelhart, normally thought of as a guru of PDP connectionism, later recalled being "inspired" by Quillian to begin his own work on psychological simulation—J. A. Anderson and Rosenfeld 1998: 272.) A semantic network was composed of nodes and links between nodes. The nodes stood for concepts, objects, and events. The links represented relationships of various types, such as *superordinate*, *subordinate*, *isa*, *has-part* (and, if one wished, *aunt of*, *married to* . . . and so on). Such a network could represent both general concepts and individual instances: both cats and Tibbles, the latter linked to the former by an *isa* (IS-A) link.

On Quillian's view, both human long-term memory and the knowledge (data, meanings) possessed by a programmed network are implemented as nodes and links. Or rather, "denotative, factual information" is stored in this way. People's "plans for doing things", their "feelings about words", and their knowledge of "the conditional probabilities of word sequences" were specifically excluded (1967: 410). All of those were being explored by psychologists trying to capture the nature of meaning. Piaget, for instance, had discussed action plans in terms of sensori-motor schemas (5.ii.c); word sequence probabilities had been stressed by the informational psychologists (6.i.a and 9.x.b); and the feelings associated with words had been intriguingly highlighted by Charles Osgood's group in Illinois (Osgood *et al*. 1957). A complete theory of semantic memory, said Quillian, would have to include all those aspects too.

In a Quillian network, the inferences justified by the program's knowledge were drawn by a mechanism of "spreading activation". Whenever a node was activated, the activation would automatically spread along the links that connected it to other nodes—thus forming inferential pathways of various kinds. This simple arrangement met Raphael's demand: that the information required to generate the output from a KR be "easily accessible". (That wasn't true for logicist KR: there were no stored relevance links between propositions in the predicate calculus, only general rules of inference which could link them *if they could be found*.)

For example, consider property inheritance in semantic networks. If the class name *bird* was linked by *has* to *wings*, and if *robin* was linked as a *subordinate* to *bird*, then robins could easily (*sic*) be inferred to have wings too; and if Fred was linked by *isa* to robin, then Fred's wings could be easily inferred as well. Similarly, spreading activation enabled the network to make explicit comparisons and contrasts between the meanings of any pair of words, and to interpret ambiguous words according to the linguistic context—where the key factor in disambiguation was the *intersection* of activated pathways. Figure 10.4 shows two different pathways associating *plant* with *live*, and Figure 10.5 shows an associative structure leading to the intersection node (*sad*) for *cry* and *comfort*.

Quillian tested his networks on the English text used in the thinking-aloud experiment mentioned above. There were seven sentences, which between them contained nineteen ambiguous words. (This was pushing at the technological limits: because the program itself used up most of the computer's memory, there was room for no more than twenty definitions of English words—Crevier 1993: 82.) His first program interpreted twelve of them correctly. (It couldn't decide on four, so they remained ambiguous; and it got three wrong.) An improved version, developed in concert with Bobrow, dealt also with ambiguous prepositions. For instance, *I threw the man in the ring* was correctly interpreted, in context, as *While in the ring I threw the man*, or as *I threw the man who was in the ring*, or as *I threw the man into the ring* (Quillian 1968: 261–2).

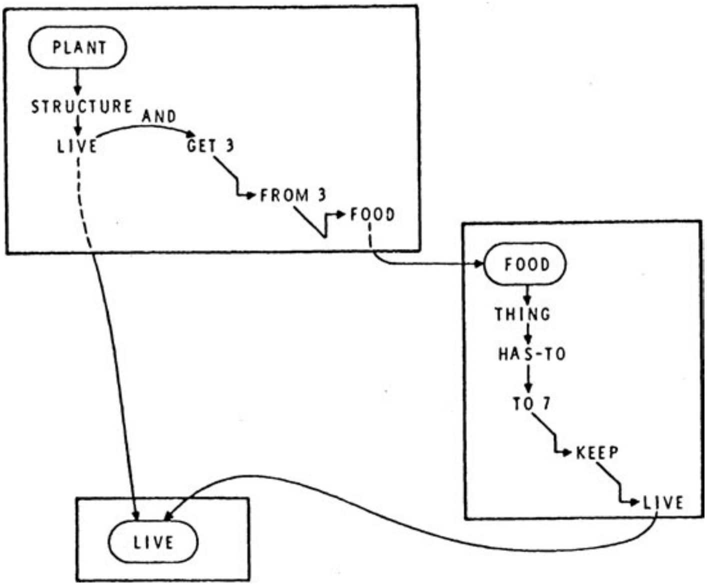


FIG. 10.4. Two paths direct from "Plant" to "Live". Reprinted with permission from Quillian (1968: 250)

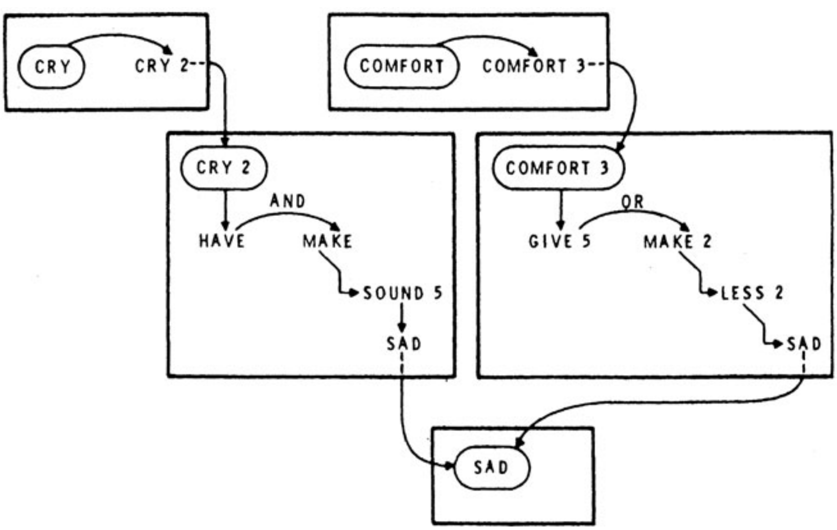


FIG. 10.5. A path from "Cry" and a path from "Comfort" which reach the same intersection node, "Sad". Reprinted with permission from Quillian (1968: 250)

In the late 1960s, semantic networks were seen as a liberation. Many people (in AI, psychology, and linguistics) adopted, or adapted, them. They weren't used only for language. For instance, they were applied to vision as well as concepts in John R. Anderson's ACT system (7.iv.c).

Granted, there were hazards lying in wait for the unwary. Various difficulties in using/interpreting semantic nets were detailed in 1975 by Quillian's BBN colleague William Woods (see Chapter 9.xi.e). Later still, some of these were elaborated by James Allen and Alan Frisch (1982), and by Ronald Brachman—another BBN colleague at the time (1977, 1979, 1983). But the difficulties didn't kill the interest. Indeed, nearly twenty years later the editors of a comprehensive volume on KR judged them to be "arguably the most popular of all techniques employed to represent knowledge in AI systems" (Brachman and Levesque 1995, p. xviii).

If Quillian's work inspired a huge amount of research over the years, so did McCarthy's harbinger project, logicism. Further claimants to the KR crown appeared as AI matured, some coming from other disciplines within cognitive science (social psychologists' *scripts* and linguists' *semantic primitives*, for instance: 7.i.c; 9.viii.c). By the mid-1980s, there was a thirty-page bibliography on the topic (Brachman and Levesque 1995: 335–65). In short, KR was a leading concern—many would say *the* leading concern—of symbolic AI.

One shouldn't conclude that people were faced with an *embarras de richesse*: a choice between myriad utterly different forms of KR. For the *richesse* consisted largely in superficial, not fundamental, differences.

This was made clear in a hugely influential paper by Minsky, circulated in draft (as was his wont) for some years before appearing as an MIT memo in 1974, and an official publication in 1975. 'A Framework for Representing Knowledge' unified the work of a number of scientists both within and outside AI. These people had said essentially similar things about how concepts work. The core idea was that concepts are structured, and that this structure—by storing co-relevant things together and by indicating just how they're related—enables sensible inference.

For example, Bartlett had posited structured "schemas" in the 1930s, and Hebb outlined hierarchical "cell-assemblies" in the 1940s (5.ii.b and iv.c). Quillian's semantic networks represented meaning structures too, although they could also depict merely contingent associations. AI vision researchers in the 1960s had defined structured object models for interpreting images (see iv.b, below). In the early 1970s, Robert Abelson had added "scripts" and "themes" defined as interpersonal concepts, and then (with Roger Schank) "scripts" understood as stereotyped ways of behaving (7.i.c). Meanwhile, Michael Arbib was describing sensori-motor schemas as well as conceptual ones (and he'd later attribute them to robots as well as to many animal species: 14.vii.b–c).

Minsky referred to some of that literature in trying to unify the superficially disparate work on KR "in Artificial Intelligence and Psychology" (1975: 211). That phrase "and Psychology" was crucial: "I draw no boundary", he said, "between a theory of human thinking and a scheme for making an intelligent machine." The paper's key claim was that common sense depends on structured "'chunks' of reasoning, language, memory, and perception". This claim was explicitly opposed both to behaviourist psychology (specifically, the first two tenets: see 5.i.a) and to AI logicism, which—Minsky said—tried "to represent knowledge as collections of separate, simple fragments".

Minsky added recommendation to unification. And what he recommended was the KR notion then favoured at MIT: the "frame". Frames were hierarchical structures ("networks of nodes and relations") representing complex objects. Each frame had fixed attributes at the higher levels, and many terminal "slots" for properties that could be filled in various ways. They occurred as "systems" of frames—for instance, the many images of a "scene from different viewpoints", or the different situations brought about by "actions, cause–effect relations, or changes in conceptual viewpoint". Since the various frames within a frame system share the same terminals, it's possible to "coordinate information gathered from different viewpoints"—in other words, to make inferences drawing on distinct sources/types of knowledge. (Later, these ideas would spawn object-oriented programming languages, in which frames were in effect treated as primitives: see v.c, below.)

Examples of frames included the concept of "arch" employed by Winston's learning program (subsection d, below), or the everyday concept of a room. A room typically has one door (occasionally, more), one or more windows, a roof, and a cuboidal shape; it normally contains furniture (distinct frames define tables, chairs, etc.); and it's usually contained in a larger building, such as a house.

However, "typically", "normally", and "usually" are weasel words. To be ultra-safe, the *number* in the *door* slot for *room* (for instance) could simply be left open, to be filled in by the programmer for each specific room considered. But that would be highly time-consuming; and it would also mean that a vision system looking for an unknown room or door, or trying to interpret an unfamiliar image which happened to depict a room, couldn't use one-door-ness as a cue. Alternatively, the number-of-doors in a *room* frame could be given a "default" value *(one)*, which would stand unless overridden by the programmer or, as a result of inferences, by the program.

In practice, said Minsky, much of the knowledge possessed by a frame-using program would be stored as default values. This was economical and efficient, but it could lead to problems. Consider exceptions, for instance. Cats have tails . . . but Manx cats don't. How could an AI representation—whether a hierarchical frame, or a set of logicist axioms, or a semantic network, or a Schank–Abelson script . . . —deal with that fact? It would be counter-productive simply to omit the tail slot from a cat frame; or to remove the axiom *If x is a cat then x has a tail* from a theorem-prover; or to delete the link between *cat* and *tail* in a semantic net; or to drop the rule, in a stereotyped script, that friendly visitors would stroke the tail of the family's cat. After all, the inference that a particular cat has a tail is far more often right than wrong. Minsky suggested various ways in which something could be assumed by an AI program unless it was specifically contradicted. Indeed, "default reasoning" soon became a key issue in AI (13.i.a).

Not for the first time, then, Minsky was surveying the land in preparation for core debates in AI. But whereas 'Steps' had (unavoidably) been programmatic, 'Framework' discussed many programming details. Its influence was due as much to the authoritative exposition of the MIT approach as to the general observations it contained.

Despite his efforts at unification, significant differences remained. Indeed, Newell (1982: 92) complained of "a veritable jungle of opinions" about KR, adding that "There is no consensus on any question of substance." Today, over twenty years later, KR is still a major concern. The early 1990s saw a 408-page special volume of the journal

Artificial Intelligence, and a further update appeared very recently (Brachman *et al*. 1991; Brachman and Levesque 2004). Evidently, KR and SIP were prizes which would be very hard-won.

b. A resolution to do better

Theorem proving had competed with checkers and chess as the first topic for real (i.e. implemented) programs. The fact that LT had found one proof more elegant than Russell's in *Principia Mathematica* had been hugely encouraging (Chapter 6.iii.c). And some, including the LT authors themselves, had been further encouraged by believing that they were modelling the thought processes of human logicians.

However, the mathematical logician Hao Wang (1921–95), then at Oxford but about to return to Harvard, soon criticized LT in pretty strong terms (H. Wang 1960). Not only could *Principia* proofs be generated more efficiently by other mechanized methods, but these methods—unlike LT—would eventually find *any* provable proposition within the relevant set. He concluded that LT wasn't a good ideal for mechanized theorem proving. More generally, he rejected human simulation as an approach to automated reasoning: it was logic that was important, not human thinking (cf. MacKenzie 1995).

Minsky disagreed, arguing in 'Steps' that whereas the *Principia* problems were simple enough to yield to exhaustive procedures, more complex problems wouldn't be. In particular, such procedures couldn't help with "the fundamental heuristic problem of *when to decide to give up on a line of attack*" (Minsky 1961*b*: 437).

In terms of Gottlob Frege's distinction between logical norms and psychological laws (2.ix.b), Minsky was in effect sitting on the fence. Unlike Wang, he wasn't restricting himself to pure logic—for he was considering the untidy practicalities of real-time computation. In terms of a distinction drawn 100 years later—and mentioned above, in contrasting McCarthy and Minsky—Wang was a neat whereas Minsky was a scruffy (see 13.i.c). On the other hand, he wasn't insisting that those practicalities be drawn solely, or even primarily, from human psychology: he was interested in the practical constraints on *any* complex computational system. He was happy to learn lessons from human psychology, when it seemed helpful. But unlike Newell and Simon (whom Frege would have accused of coming close to "psychologism"), he didn't believe that automatic theorem proving should necessarily mimic human thought.

The divide between theorem proving as logic and theorem proving as psychology was a special case of the divide between technological and psychological AI. And it endured, as this area of NewFAI advanced (MacKenzie 1995). Whichever side of this fence one favoured, the mere fact of such disagreement shows that while LT had kicked off the ball, it hadn't established the rules.

Broadly speaking, three different approaches to theorem proving developed, each of which is still with us today: logical/axiomatic, heuristic, and model-based. These were exemplified respectively by the resolution theorem-prover, the Logic Theorist, and Gelernter's geometry program.

Resolution theorem proving was driven by the same aim as Wang's: to discover a mechanized procedure that would be *guaranteed* to find a proof. It was defined in the early 1960s by the British logician J. Alan Robinson (1930–) at Rice University (J. A. Robinson 1963, 1965), and implemented by others soon afterwards (Wos *et al*. 1964, 1965). The immediate result, for Robinson himself, was a "spectacular rise to stardom in AI" (E. A. Feigenbaum, interviewed in McCorduck 1979: 219).

The reason why Robinson's work had such an impact was that it could deal with a relatively complicated form of logic. With respect to Russell's *propositional* calculus (the focus of McCulloch and Pitts: see 4.iii.c), there was no difficulty in principle. All proofs could be found by constructing truth tables. To be sure, the size of the search increased exponentially as the number of variables grew: if establishing *if (p and q) then (a or b)* was practically feasible, proving *if (p and q and r and s . . . and z) then (a or b or c . . . or n)* might not be. However, there was nothing intellectually deep about the methods, and almost any programmer could write programs for applying them. Russell's *predicate* calculus (the focus of McCarthy: see i.f, above) was a very different kettle of fish. It included existential and universal quantifiers of varying scope, and terms denoting relations of various degrees (binary, ternary . . . *n*-ary). Accordingly, it required more sophisticated methods of problem solving.

That's where resolution theorem proving came in, for it was a method for dealing with (first-order) predicate logic. Robinson wasn't the first to try to define some such method. The logician Jacques Herbrand, as long ago as 1930, had defined—though not implemented, of course—a basic algorithm for proof procedures (sometimes called "refutation procedures") which tried to establish a theorem by showing that its negation is inconsistent. In other words, the basic rationale was the assumption that not-not-X implies X.

That seems like common sense, although it was disputed by "constructivist" mathematicians (they claimed that X had to be proved directly, not inferred from a double negative: P. M. Williams, personal communication). However, non-constructivists were happy with it. Accordingly, after computers had come on the scene a number of people defined mechanizable versions of Herbrand's algorithm. One of these people was Robinson's ex-tutor Putnam (M. Davis and Putnam 1959/1960). (In his eyes, this was *logic,* not psychology; that's why he didn't mention it in his paper on functionalist philosophy of mind, written at the same time: 16.iii.b.)

Soon after Putnam's logic paper appeared, Robinson tried to implement the method defined in it—and discovered that it was highly inefficient. But he was also trying to implement another logician's suggestion at much the same time, and he found that a combination of ideas drawn from both papers was superior to either (MacKenzie 1995). That method was resolution, which—besides its relative efficiency—could deduce *new* statements from existing knowledge. That is, they could make information explicit which had previously been implicit and unrecognized. The other neo-Herbrand techniques, including Putnam's, couldn't do that: they produced only trivial consequences, by instantiating variables in logical expressions (Wos and Veroff 1987: 895).

In one respect, Robinson's theorem-prover was elegantly simple. For it used only one essential rule of inference. (The other rules, added over the years, provided the guidance/restriction strategies.) It always tried to infer a *contradiction* between the desired conclusion and the premisses. This may sound paradoxical, but it's a form of argument (*reductio ad absurdum*) that's common even in everyday chit-chat. If someone makes a statement, and their opponent can show that it contradicts their premisses and/or common knowledge, then (if the premisses are true) it must be false. So we often suppose—'for the sake of argument', as we may say—that something is the case, *precisely in order to show that it isn't*. If we can't find any contradiction between X and the shared premisses, then the disputed statement can stand.

A resolution theorem-prover aiming to prove X, then, would first try to show that X is impossible. And it would do *that* by seeking some contradiction between X and the premisses. If it found such a contradiction, then not-X (the negation of X) would have been proved. So in trying to find a plan enabling a robot (such as SHAKEY) to do something, for instance, the program would start by assuming that no such plan exists.

As usual, it wasn't all plain sailing. Robinson's initial version was very inefficient, tending to get bogged down in masses of valid-but-boring conclusions. These were drawn because they *could* be drawn, not because (in respect of the task) they *should* be drawn. In other words, it suffered from the "combinatorial explosion", a form of information overload that bedevilled many NewFAI programs (and which often bedevils human brains too). In general, the difficulty was that the computer might try to consider and/or infer too many items to make problem solving practically possible. Sir James Lighthill, in his savage attack on AI some ten years later, would write as though the combinatorial explosion was an unseen and insuperable obstacle (11.iv.a). It may have been insuperable: we'll come to that. But it certainly wasn't unseen. It was a bugbear familiar to all computer scientists, and had been mentioned many times in 'Steps' (though not under that name). And it could be ameliorated, if not wholly overcome.

At first, people assumed that better hardware (faster processing and larger memories) would enable resolution theorem-provers to scale up beyond the initial barrier of a few dozen items. But it soon became clear that better software would be needed too. Accordingly, several people tried to improve on the basic resolution algorithm. They included Robinson himself (1968), his collaborator Larry Wos (in several papers), David Luckham (1967), and Cordell Green (1969)—who applied it to question answering. These people (and their successors in later decades) defined increasingly efficient "strategies" for guiding and restricting the many inferences made by a resolution theorem-prover, so that the system knew where (not) to go and, as Minsky had demanded, when to give up.

Robinson's approach caused huge excitement in the early 1960s. One didn't have to be a logician to find it intriguing. McCarthy's harbinger paper, after all, had suggested that predicate logic could represent common-sense knowledge (and could be used to give advice while leaving the core program untouched)—so the new technique might be useful in any problem domain. Moreover, the resolution theorem-prover did what the *advice taker* had been proposed to do: it drew new conclusions. Tell it A (given that it already knew B, C, and D), and it might be able to infer E. A wide variety of resolution-based NewFAI programs were soon developed. In question-answerers, for example, the program's world knowledge was expressed as axioms, and questions were presented to it as theorems to be proved (C. C. Green 1969). Logic even entered robotics, in the STRIPS resolution system that planned actions for the SRI robot (see subsection c, below).

There was a time bomb hidden here, however. It sputtered into life about twenty years later, when a new programming language with resolution built into it was widely used in practical applications—namely, expert systems (v.f below, and 11.v.a). Non-logician users sometimes assumed that if their program *failed* to find a contradiction, then not-not-X hadn't been established so not-X had been proved. This so-called "negation by failure" was a mistake. Failing to find a contradiction meant nothing, because predicate calculus isn't what logicians term "decidable" (cf. 4.i.c).

In real life too, there's all the difference in the world between proving that something is false (real negation) and failing to prove that it's true (negation by failure). Just compare being able to prove that your partner isn't having an affair with not being able to prove that he/she is. The reason, of course, is that there are many potentially relevant pieces of evidence (premisses) of which you may be unaware, and which you have no realistic hope of discovering/deducing. (Even the private detective may fail to find all of them.) Indeed, you may even have difficulty in recognizing *just which* matters are "potentially relevant" (Chapters 8.vi.c and 7.iii.d).

Problems in very well-understood areas of science are less open to threat on this account. For one can hope (*sic*) to have identified all the relevant knowledge, and one can hope (again, *sic*) that the situation is relatively 'closed' against unpredictable complications. But even there, it's not obvious that the problems are in principle decidable. If they're not, then assuming that not-X had been proved merely because of the failure to find a contradiction isn't logically justified. In other words, proof of non-existence is very much more difficult than proof of existence.

So why wasn't resolution laughed out of court? Well, partly because expert logicians—the Putnams and Robinsons of this world—wouldn't be tempted to make that mistake. Moreover, in a logical system that's technically *complete* the drawback outlined above doesn't apply. All the required premisses and/or axioms are (by definition) given, and the rules of inference are capable of leading to every possible true conclusion. It follows (in principle) that if they *fail* to draw a particular conclusion then it's not there to be drawn, so cannot be true. And Robinson showed that resolution theorem proving is complete in this technical sense. Provided that the premisses suffice to imply the conclusion, it can *in principle* be found by the resolution method.

Even so, *in practice* it was often difficult, or impossible, to work through the deduction far enough to complete the proof. (Remember Minsky's point about knowing when to give up.) Robinson's star status waned, accordingly. Feigenbaum remembers:

> [Here] he was, [a logician] propelled to the front ranks [of AI], and suddenly felt heavy obligation to extract the AI researchers from the pit into which they were falling, the pit of the combinatorial explosion. He understood this, but was really helpless to do anything about it since he was a logician who invented a method, not an AI researcher interested in formalizing the world's knowledge. Finally he gave up, decided he was really sorry he'd got people into this trap, but he couldn't do anything about it. As AI moved away from the Resolution Method, he moved back to logic and resigned his position on the editorial board of the AI Journal, and retreated from the whole scene. (interview in McCorduck 1979: 219–20)

c. Planning progresses

AI planning sprang from Newell and Simon's mid-1950s work on LT and GPS (see Sections i.b and d above, and Chapter 6.iii.c). That work had been a revelation—indeed, a revolution.

Nevertheless, GPS had many weaknesses. One was mentioned in i.d, above: its tendency to get bogged down by forming sub-goals on too many levels. Another was its inability to outline a plan at an overall, abstract, level, leaving the details to be decided later (on execution). This made it especially ill-suited for use in robotics, where the real-world situation may change while the program is running. By the same token, it wasn't amenable to interrupts during processing. Nor could it learn from its mistakes: an error recovered by backtracking on Monday would be repeated relentlessly on Tuesday. (Some capability for learning was soon provided, however: Newell *et al.* 1960.) Certain errors, such as undoing a previously achieved (and essential) sub-goal, weren't even identifiable by the system.

In the 1960s and early 1970s, these weaknesses—and others—were overcome as means–end analysis was developed into planning programs of increasing power. Many of these were written for the Stanford robot, SHAKEY (and others for Edinburgh's FREDDY).

The STRIPS planner (an acronym for STanford Research Institute Problem Solver) used predicate calculus theorem proving to plan SHAKEY's movements in much the same way as GPS had done for abstract problems (Fikes and Nilsson 1971; Fikes *et al.* 1972*a*,*b*). It relied on a novel form of KR: a "triangle table" storing operators, their preconditions, and the newly true facts resulting from their execution: see Figure 10.6. All other facts were assumed to remain unchanged (an assumption which raised the spectre of the frame problem: see iii.e, below).

Triangle tables enabled STRIPS/SHAKEY to act, and also to learn to do so better. The key point was that they identified whole sets/subsets of actions, for achieving particular goals/sub-goals. One triangle table could be inserted (as a chunk) into another one, so representing several levels of plan complexity without having to reconsider each individual operator. Moreover, STRIPS could generalize its plans. For example, having worked out *how to get from Room 7 to Room 3 via Room 6, in order to open Window 2 (using Box 1 fetched from Room 4)*, it could express a plan schema representing *how to get from any room to any other, via some other if necessary, to open any window, using any box found in any place*. (In essence, this was done by replacing logical constants with variables.)

Since it identified the expected result of a given action, or set of actions, a triangle table could be used to monitor success—and to plan subsequent actions accordingly. It could even be (intelligently) broken up, the relevant portions being incorporated into new plans. And these, in turn, could be inserted into others.

However, STRIPS (in its first incarnation) was limited in a number of ways. As a result, successive improvements were developed in the early 1970s (see Boden 1977: 280–7, 357–70).

For instance, STRIPS (like GPS) couldn't produce an overall strategic plan of the problem before the detailed solution was started. For very simple problems, this didn't matter. But if there were many different operators, each with many preconditions, planning would be scuppered by the combinatorial explosion. What was needed was a further level (or levels) of abstraction, allowing a solution to be stated in very general terms. These would later be transformed into lower representational levels, each more detailed than the one above it—with the last having direct control of the robot's movements. (Similarly, one may plan a journey in terms of the towns to be visited, before worrying about the order in which to visit them—still less, just how to get from this town to that one.)

Initially True Preconditions (ITPs)

1	ITPs of Action 1	Action 1			
2	ITPs of Action 2 (if any)	Facts made true by Action 1	Action 2		
3	ITPs of Action 3 (if any)	Facts from cell above which are still true after Action 2	Facts made true by Action 2	Action 3	
4	ITPs of Action 4 (if any)	Facts from cell above which are still true after Action 3	Facts from cell above which are still true after Action 3	Facts made true by Action 3	Action 4
Last row		Facts from cell above which are still true after Action 4	Facts from cell above which are still true after Action 4	Facts from cell above which are still true after Action 4	Facts made true by Action 4
	Leftmost column	1	2	3	4

FIG. 10.6. Schema for a STRIPS triangle table. Adapted with permission from Fikes *et al.* (1972*a*: 259)

Accordingly, Earl Sacerdoti (1948–) developed ABSTRIPS, which solved its problems on a hierarchy of representational levels (Sacerdoti 1974). The first was the most abstract. Details were considered only when a successful plan in a higher-level problem space gave strong evidence of their importance. The crux of ABSTRIPS was that the lists of preconditions for the various operators were ordered for priority, so that the most critical preconditions were considered first. For example, if SHAKEY's goal was to push a box from one room into another, ABSTRIPS enabled it to get to the former room first (assuming that it had started out from yet another room), and then to open the relevant door (assuming that it was shut) before positioning itself by the box so as to be able to push it.

Like GPS's difference orderings, the criticality levels of the preconditions for each operator were provided in the database, not worked out by the program itself. ABSTRIPS could pass easily from one plan level to another, mapping more abstract onto less abstract representations, because the abstraction involved was merely a question of *ignoring* details in an operator's definition (details which, if necessary, could be taken up later). If the content of the definition had differed according to plan level, it would have been more difficult for the program to pass from one level to another.

Similarly, if—when considering the details—ABSTRIPS discovered that the sub-plan wasn't executable in the current circumstances, it could return smoothly to the *more* abstract level, much as GPS had backed up to previous choice points on failing to achieve a sub-goal. As Sacerdoti pointed out, this was especially important for robotics, since the real world can't in general be expected to stand still. A state of affairs that pertained at the outset might have changed by the time it became relevant.

In fact, the Stanford team deliberately teased/tested SHAKEY by occasionally moving a block to a different position. The robot wasn't capable of noticing this being done. But it might later realize that something was amiss when it came to execute a plan that assumed that the block was in its *original* position. In that case, SHAKEY would recompute the plan. (As the situated roboticists would later point out, this was very different from being in constant interaction with a dynamically changing world: e.g. R. A. Brooks 1991*b*, n. 1.)

While Sacerdoti was developing ABSTRIPS at Stanford, Gerald Sussman (1947–) at MIT was working on HACKER (Sussman 1975; Boden 1977: 286–97). This was a planner with a difference, for it monitored its own performance and corrected its mistakes. It did this by exploiting what Papert was already referring to as a "powerful idea": *bugs* (see vi.a, below). As Sussman put it:

> I believe that effective problem solving depends as much on how well one understands one's errors as on how carefully and knowledgeably one makes one's initial choices at decision points. The key to understanding one's errors is in understanding how one's intentions and purpose relate to his plans and actions . . . [So one needs a] teleological commentary about how the subparts [relate to the overall goals]. [And one needs] knowledge about how to trace out bugs and about the kinds of bugs that might be met in applying a given kind of plausible plan. (Sussman 1974: 236)

He identified five main types of bug: PCB, PM, PCBG, SCB, and DCB (that is: Prerequisite-Conflict-Brothers; Prerequisite-Missing; Prerequisite-Clobbers-Brother-Goal; Strategy-Clobbers-Brother; and Direct-Conflict-Brother). These concerned interactions between plan components, manifesting themselves (for instance) as unsatisfied prerequisites, unnecessary double moves, or failure to protect a condition that must continue to exist until a specific point in the plan. An unsatisfied-prerequisite bug differed from a prerequisite-missing one. The former was a hitch discovered only on actually trying to execute the plan; the latter would be discovered by a critical analysis of the plan's structure before any attempt was made to execute it. (Compare encountering roadworks on your way to the station with planning to reach it by a mistaken route.)

While engaged in planning, HACKER employed several CRITICS, self-monitoring procedures designed to spot/fix particular bugs. For instance, one CRITIC looked out for cases where the program, having achieved an essential sub-goal, later undid it in order to achieve another one. If such a case was found, the program would devise a "patch" for the original plan. (Besides spotting and fixing bugs, HACKER learnt to avoid them in future: see subsection d, below.)

Sussman's work helped Sacerdoti to make a further advance in planning. Part-inspired by HACKER, he abandoned ABSTRIPS for NOAH: Nets of Action Hierarchies (1975*a*,*b*). ABSTRIPS, like HACKER, had been a linear planner. Both programs assumed that sub-goals are additive, so could be considered independently—even if they were best considered in a particular order. However, sub-goals are sometimes non-additive: they interact, so that the achievement of sub-goal A may have to be deliberately undone before it's possible to achieve sub-goal B. This would happen, for example, if HACKER were asked to solve the problem shown in Figure 10.7 (Sacerdoti 1975*b*: 105–9).

ABSTRIPS could sometimes use backtracking to reach a clumsy solution to non-additive problems. But it couldn't achieve the optimal solution, because it couldn't take sub-goal interaction into account. NOAH, by contrast, could. It did this by ignoring temporal order in its initial plans. Sub-goals were represented merely as logical conjuncts to be achieved in parallel. When the plan was elaborated, on successively detailed levels, NOAH would scan for potential interactions between sub-goals and specify temporal order whenever this was needed to avoid them. The result would be a partially ordered plan, constructed so as to minimize backtracking in execution.

NOAH's secret lay in its CRITICS, inspired by HACKER's subroutines of the same name but constructive rather than destructive in nature. For these Stanford-based CRITICS added constraints to a partially specified plan, instead of rejecting incorrect assumptions that shouldn't have been made in the first place.

There were five general-purpose CRITICS to oversee the elaboration of NOAH's plans. They watched out for ways of resolving potential conflicts between sub-goals; of specifying existing objects for use rather than leaving their identity vague; of eliminating redundant preconditions; of resolving "double crosses" in which each of two conjunctive purposes denies a precondition for the other; and of optimizing disjuncts so that a choice between alternative sub-goals could be predetermined or postponed, whichever was the more sensible.

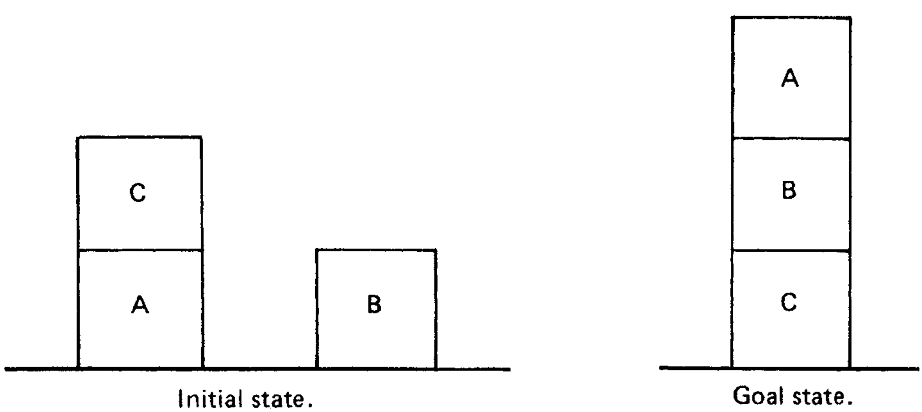


Fig. 10.7. A non-additive problem. Reprinted from Boden (1977: 360)

In addition, NOAH could be provided with task-specific CRITICS. This was done for Stanford's CBC project (Computer Based Consultant), an interactive program designed to give on-the-job advice on how to assemble a machine to novice mechanics having varied levels of expertise (Hart 1975; Sacerdoti 1975*b*: 90–109, 117–20). In order to give advice that was appropriate for the particular user, NOAH used the specific queries posed by the human mechanics as cues directing it to answer at one hierarchical plan level rather than another. So one mechanic might be told simply to "Replace the pump", whereas a less experienced person might be instructed to "Remove the 4 mounting bolts at the base of the pump using a $\frac{3}{8}$-inch open-end wrench." Similarly, the program could ask helpful questions when the user got into trouble, because it had some idea of just where in the overall task the human's difficulty might lie. In short, CBC was an early forerunner of today's "Steve", a VR system that enables mechanical advice to be given by a humanoid animation, bodily movements—and voice recognition—included (Chapter 13.vi.b).

NOAH managed to solve a problem which bemuses many humans, and which early 1970s theorem-provers had been quite unable to handle. Michie had recently identified the "keys and boxes" puzzle (too complex to state here) as a benchmark problem for AI, but NOAH worked it out in only twenty-one steps (Sacerdoti 1975*b*: 70; Michie 1974).

Similar work on non-linear plans was now being done in Michie's Edinburgh laboratory, by Austin Tate (1975). But problems remained. For instance, neither Sacerdoti's NOAH nor Tate's INTERPLAN could cope reliably with cases where each of two conjunctive goals denies a precondition for the other. (The "double cross" CRITIC could sometimes diagnose and resolve the trouble, but in other cases this wasn't possible.) As Sacerdoti put it, "the system must be creative and propose additional steps that will allow the two purposes to be achieved at the same time" (1975*b*: 42).

That sort of creativity was provided by BUILD, a program written by Scott Fahlman (1948–)—who, by the way, also invented the "smiley face" emoticon (Fahlman 1974). BUILD represented plan structures in such a way that it could solve the problems shown in Figures 10.8 and 10.9.

Both of these see-saw tasks require an additional manipulation early in the plan, which is later undone by the builder. Without going into the details of how BUILD worked (cf. Boden 1977: 366–70), let's just note that it 'knew' enough about weights and levers to foresee unwanted effects (i.e. the collapse of the structure being assembled),

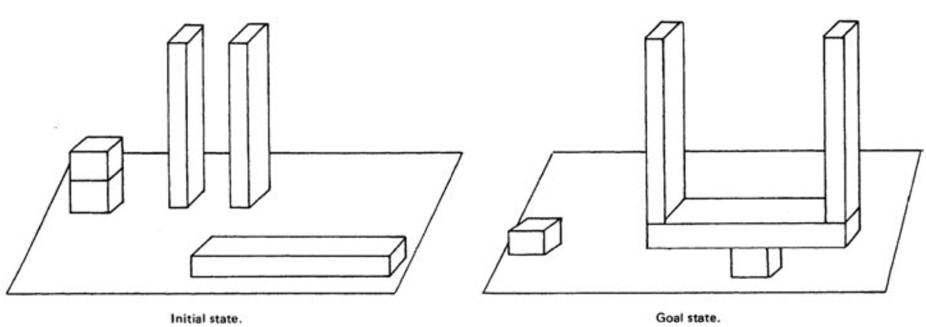


Fig. 10.8. Blocks-world problem (only one hand; no sliding). Reprinted from Boden (1977: 364)

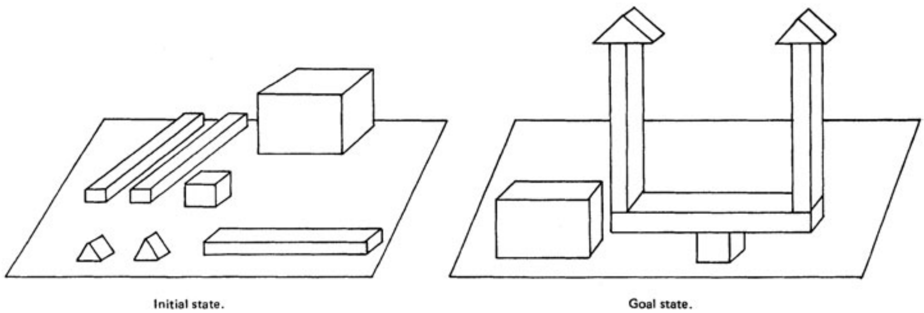


FIG. 10.9. Blocks-world problem (only one hand; no sliding). Reprinted from Boden (1977: 365)

and could forestall them—by adding steps into the plan—without introducing further contradictions. (Well, that's what Fahlman claimed, but it's not quite true: the cube used as a scaffold in Figure 10.8 can be removed at the end only by sliding, which is forbidden. When someone pointed this out on a sultry summer's day in 1978, during a talk I was giving at RAND, there was an appalled silence in the room—until a man at the back shouted "It's an ice cube!")

Ice cubes aside, it's clear that by the mid-1970s planning structures of some complexity were being handled in AI. Backtracking, generalization, abstraction, contingency planning, diagnostic critics, anticipatory corrections, on-the-spot adjustments, mapping of the current context of action, memory of past contexts . . . all these were now possible. Some programs (such as BUILD) could recognize that a current plan threatens to become overly complicated, and could interrupt it to switch to an alternative—with the option of resuming it *at that very point* if it turned out later that the first plan had been better after all. (This flexibility was due to new programming languages: Section v.d.)

By the mid-1970s, then, a number of planning techniques were available. General-purpose methods were sometimes supplemented by special-purpose data and tricks (NOAH's $\frac{3}{8}$-inch open-end wrenches, for instance). But the teleological structure of plans *in general* was the focus of all this GPS-inspired NewFAI work. And Newell and Simon's production systems provided further general features, such as tolerance of interrupts (7.iv.b and Section v.e below).

The generality was limited, however. In particular, there was no good way of modelling systems having several different, and potentially conflicting, goals. Research on multi-purpose planning had been done in the 1960s: Walter Reitman's Argus (7.i.b), and the Mars robot controller designed at MIT by William Kilmer and McCulloch. But the problems involved were by then insuperable. It was difficult enough, as we've seen, to model plans for achieving a single goal. Not until the end of the century would the Kilmer–McCulloch controller become a practical proposition (14.v.a), or multi-motive scheduling begin to be understood (7.i.e–f).

Moreover, the 1970s theoretical advances didn't prevent there being major problems in practice. The Stanford robot, for instance, spent an inordinately long time deliberating about what it should do—not because of any Hamlet-like indecision on its part, but because of plan-hierarchy complexity. (The combinatorial explosion, again.) Even when, thanks to Sacerdoti, the robot was able to recover from unexpected events, this wasn't the work of a moment. So in a constantly changing real-world environment SHAKEY, like all other NewFAI robots, was useless.

That's largely why many roboticists in the 1980s would reject planning *as such*, relying on insect-inspired reflexes instead (Chapter 13.iii.b). Nevertheless, SHAKEY had prompted a great deal of useful work—and not only in AI. The neuroscientist Arbib, for instance, was inspired by SHAKEY to start on a programme of research on visuo-motor control in frogs, which led eventually to an ambitious theory of human thought—even including religion (see 14.vii.b).

d. Early learning

During the early to mid-1950s, most AI research on learning was connectionist. Even Minsky had tinkered a connectionist learner in 1951, and devoted his Ph.D. to explaining why it worked (12.ii.a). In symbolic AI, learning machines were thin on the ground. By the mid-1950s there was only one of any real interest: Samuel's checkers player.

In the late 1950s, however, a number of people started to work in this area. One was Amarel (1962), who wrote a heuristic program which, in a sense, learnt *how to learn*: its task was to induce routines equivalent to the sixteen basic operations of the propositional calculus, but once a particular operator had been learnt it could help in learning others. (Much the same would later apply to STRIPS, as remarked above.) His draft paper was listed in Minsky's 'Steps' bibliography, but (perhaps because it wasn't officially published until 1962) it wasn't included in *Computers and Thought*. Three other early 1960s papers, however, were included—all intended as general theories, and all largely based on psychological ideas.

The first was a pattern-recognizer, an adaptation of Pandemonium (Uhr and Vossler 1961*a*/1963). It learnt its own "feature detectors", instead of having to be supplied with them. (The authors compared it with David Hubel and Torstein Wiesel's contemporary work on vision: Chapter 14.iv.b.) The paper in Feigenbaum and Feldman's collection described how the program learnt to recognize written letters and doodles (with respect to the latter, it surpassed human beings). But it had also learnt to deal with diagrams of speech acoustics, which aren't distinguishable by humans unless they're expert in the field (Uhr and Vossler 1961*b*). Its generality lay in the fact that a pixel is a pixel . . . that was all the program needed to know.

The second was Feigenbaum's (1961) EPAM: Elementary Perceiver and Memorizer, briefly mentioned in Chapter 6.iii.c. This had started out as an attempt to simulate aspects of decision making in business (Newquist 1994: 81). But it moved away from business towards psychology, and from decision making towards memory. The published paper described it as "an attempt to state quite precisely a parsimonious and plausible mechanism sufficient to account for the rote learning of nonsense syllables".

The program used its "elementary information processes" to build, and when necessary to revise, treelike "discrimination nets" representing the various syllable pairs. The details of the nets, and of the processes using them, were decided partly by general principles and partly by human psychology. For instance, *any* learning system needs to store more information about a stimulus if it has to reproduce it, as opposed to merely recognizing it. But only systems—such as EPAM—simulating human psychology need to notice the order of letters in syllables, since our tendency to be more influenced by end letters than by middle letters, and by first letters than by last letters, is presumably not a cognitive universal.

EPAM was more valuable for psychology than for AI. Feigenbaum (with Simon) had already used it to simulate some classic paired-associate experiments, when it had shown stimulus generalization and various types of interference (where later learning seems to undo earlier learning) even though these hadn't been specifically anticipated. And they soon applied it to explore the difficulties involved in learning serial lists where the same item occurs twice (Feigenbaum and Simon 1962). Even today, improved versions are still being reported in the psychological journals (e.g. Feigenbaum and Simon 1984; Gobet 1998).

The last of the *Computers and Thought* trio was valuable for psychologists and AI alike (Hunt and Hovland 1961; see also Hovland and Hunt 1960). Indeed, its ideas would eventually bear huge fruit in technological AI (13.iii.e). It was a model of strategies for learning from examples, based largely on Bruner's psychological work of the late 1950s (6.ii.b–c).

The senior author was Yale's Carl Hovland, whose mid-century analyses of the information available in concepts had inspired Bruner to work on that topic in the first place. The junior author was his student Hunt (1933–). Nowadays, their approach is normally attributed to Hunt, for Hovland died (in 1961) soon after their initial program was completed, and the work was taken forward by the younger man—who published an entire book on it just before *Computers and Thought* appeared (Hunt 1962).

Following Bruner, Hunt and Hovland defined strategies for learning both conjunctive and disjunctive concepts, given both positive and negative examples. And (again, like Bruner) they considered not only concepts whose instances share common properties, such as redness or triangularity, but also concepts defined by "common relationships"—such as having *a large figure on top and a small one below*, irrespective of the shape of either figure.

Broadly speaking, they modelled the inductive strategies informally described in *A Study of Thinking*—which they thought of as MGP's TOTE units (Hunt 1962: 184–5). In addition, they leant on the computer simulation of human memory that Hunt had presented for his Ph.D. in 1960, which featured—for instance—the limited size of STM. That was significant. For they were doing psychology, not technological AI:

> We have attempted to write a computer program which, when given as input coded representations of the stimuli, will give as output coded responses that can be used to predict the responses of a human subject. *Accurate prediction of the responses, not the development of a good hypothesis developer*, nor, solely, the reproduction of previously obtained protocols, is our goal. (Hunt and Hovland 1961: 146; italics added)

So, for instance, their model favoured "positive focusing", in which hypotheses are developed from positive instances rather than negative ones—even when this isn't the most efficient strategy. Why? Because BGA's experiments had found that human subjects favour positive focusing.

Hunt's (1962) book discussed a number of strategies—some implemented, others not. The way in which they were described was less 'logical', and more 'psychological', than in the Hunt–Hovland paper. Consider "conditional focusing", for example—a procedure for learning disjunctive concepts. In the *Computers and Thought* paper, this had been expressed as a recursive function operating on sets, an idea taken from McCarthy (1960). But Hunt described it as a method for building decision trees (see Figure 10.10). These two approaches were equivalent: logic had been re-expressed, not dropped. Indeed, Hunt still declared that "Concepts are essentially definitions in symbolic logic" (p. 8).

We needn't follow through this strategy in detail. But notice that step (3) involves a simple frequency measure, seeking the description *most commonly* applicable to positive instances. Later versions of this general inductive approach would employ increasingly sophisticated frequency measures (see 13.iii.e). Notice, too, that different random choices at step (3)—where no single description is "the most common"—would result in different decision trees for one and the same concept. Suppose, for example, that the training set was as follows (Hunt 1962: 234):

Positive instances	Negative instances
Large, black circles	Large, black triangles
Large, white circles	Large, white triangles
Small, black triangles	Small, black circles
Small, white triangles	Small, white circles

Two decision trees derived from those data are shown in Figure 10.11.

As Hunt pointed out, conditional focusing *as shown in Figure 10.10* was "an algorithm for producing answers", which "[clearly] will not do as a simulation" (p. 235). The Hunt–Hovland paper had already suggested limiting the number of recursions permitted, so restricting the maximum length of a path through the decision tree. Now, Hunt had done further experiments on human subjects, learning concepts of various kinds. As a result, he suggested changes to make the machine strategies more human-like—which sometimes, though not always, made them more efficient too.

For instance, a "positive focusing" modification could give quick solutions for conjunctive concepts (see Figure 10.12). This new routine was inserted between steps (0) and (2) of the procedure shown in Figure 10.10. It would seem at first sight, therefore, that conditional focusing had been made more complex. But that wasn't so. For the calculations shown in Figure 10.12 take less time (especially in list-processing languages) than those of step (2) in Figure 10.10.

Various other modifications were suggested as well. In short, this work was not only an achievement, but also an intriguing promise of further achievements. That promise would be fulfilled, for Hunt and Hovland's research led eventually to the ID3 algorithm (13.iii.e). (It was part-inspiration also for the NewFAI program that discovered geometric analogies, using the "common relationships" mentioned by H&H and by Minsky in 'Steps'—T. G. Evans 1968: 283, and 13.iv.c.) However, that didn't happen immediately.

Despite being honoured by inclusion in *Computers and Thought*, these early learners didn't set GOFAI learning afire at the time. In part, that was because they were so heavily biased towards psychology. Besides, NewFAI was concerned with other things: planning, theorem proving, and especially KR. McCarthy had said in his harbinger paper

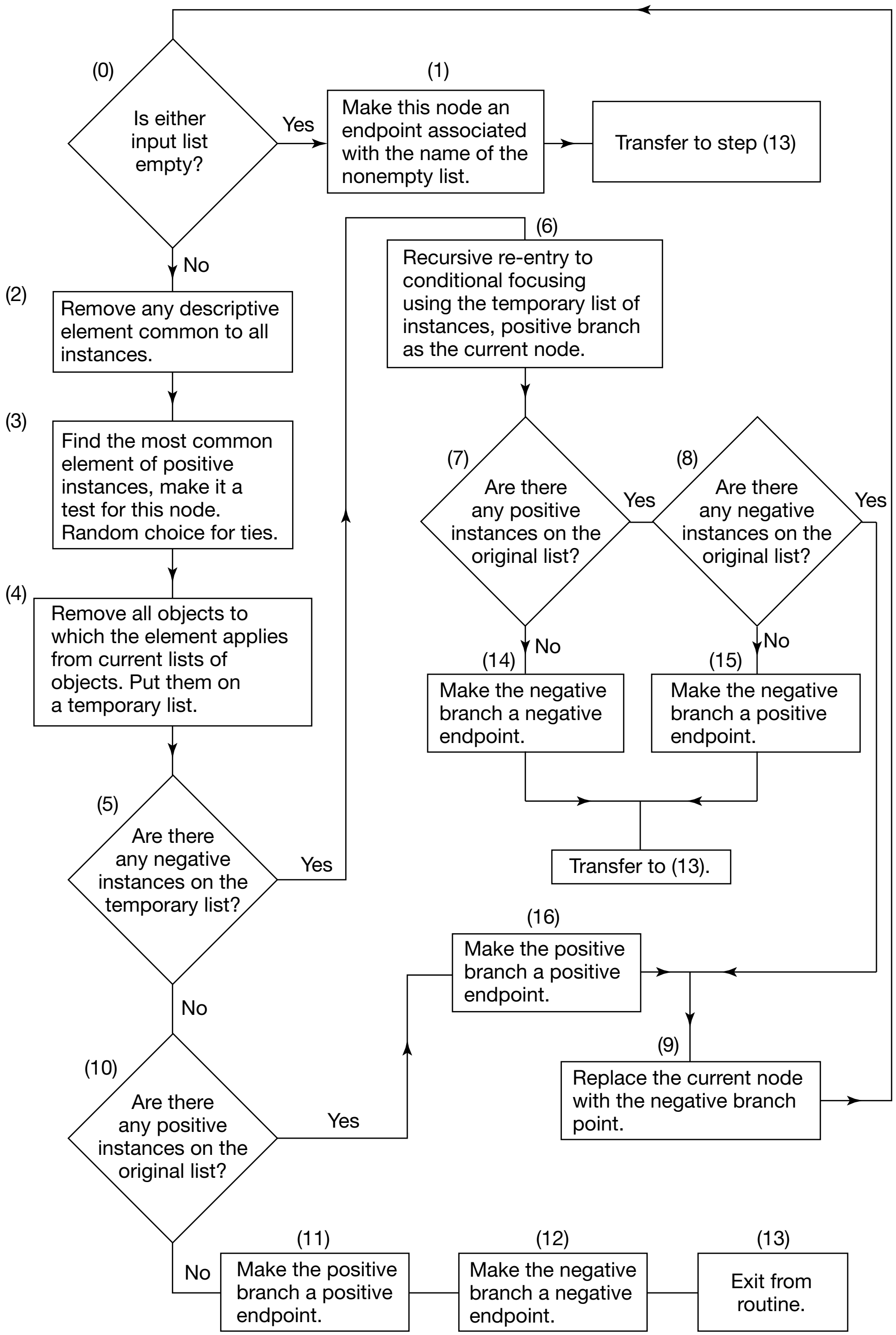


FIG. 10.10. Conditional focusing. Redrawn with permission from Hunt (1962: 232)

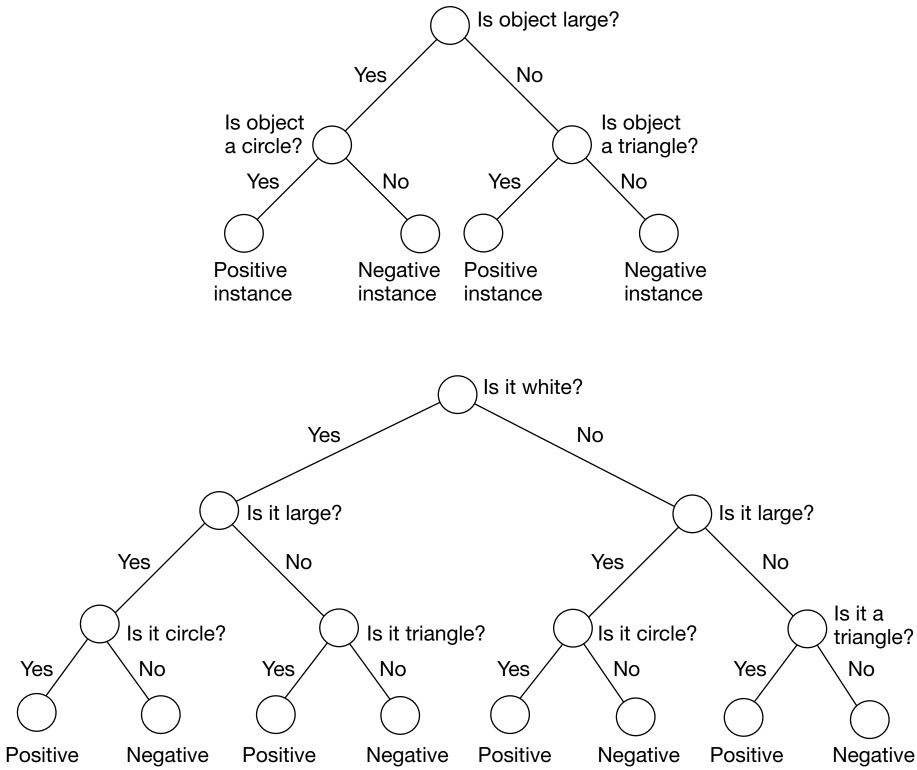


FIG. 10.11. Alternative decision trees defining the same concept. Redrawn with permission from Hunt (1962: 234, 235)

that if a machine were to learn something, it would first have to be able to represent that something—and the 1960s, as we've seen, were largely devoted to finding ways of representing things. Occasionally, creative efforts in KR were aimed at automatic learning: Michie's "Adaptive Graph Traverser", for instance, which extended Samuel's work from games to problem solving (Michie and Ross 1969). By and large, however, learning was put on the back burner.

In the early 1970s, a few pots were brought to the front of the stove. The first was Winston's concept-learner (1970*a*), the second Sussman's HACKER (1973/1975), and the third McDermott's TOPLE (1974). This trio addressed many interesting aspects of intelligence, for the programs learnt (respectively) by comparing examples, by doing, and by being told. They created some local warmth, especially at MIT. But they barely raised the temperature in the kitchen, as we'll see.

By the time Winston's program was officially published (in 1975), it was already well known. It had been developed during the late 1960s, presented to MIT for a Ph.D. in 1970, and circulated unofficially from then on—amid no little excitement. It was the first NewFAI learner since Samuel's to be widely lauded within the field (although I felt at the time, and still do, that its title to fame was largely due to the efficiency of the MIT publicity machine).

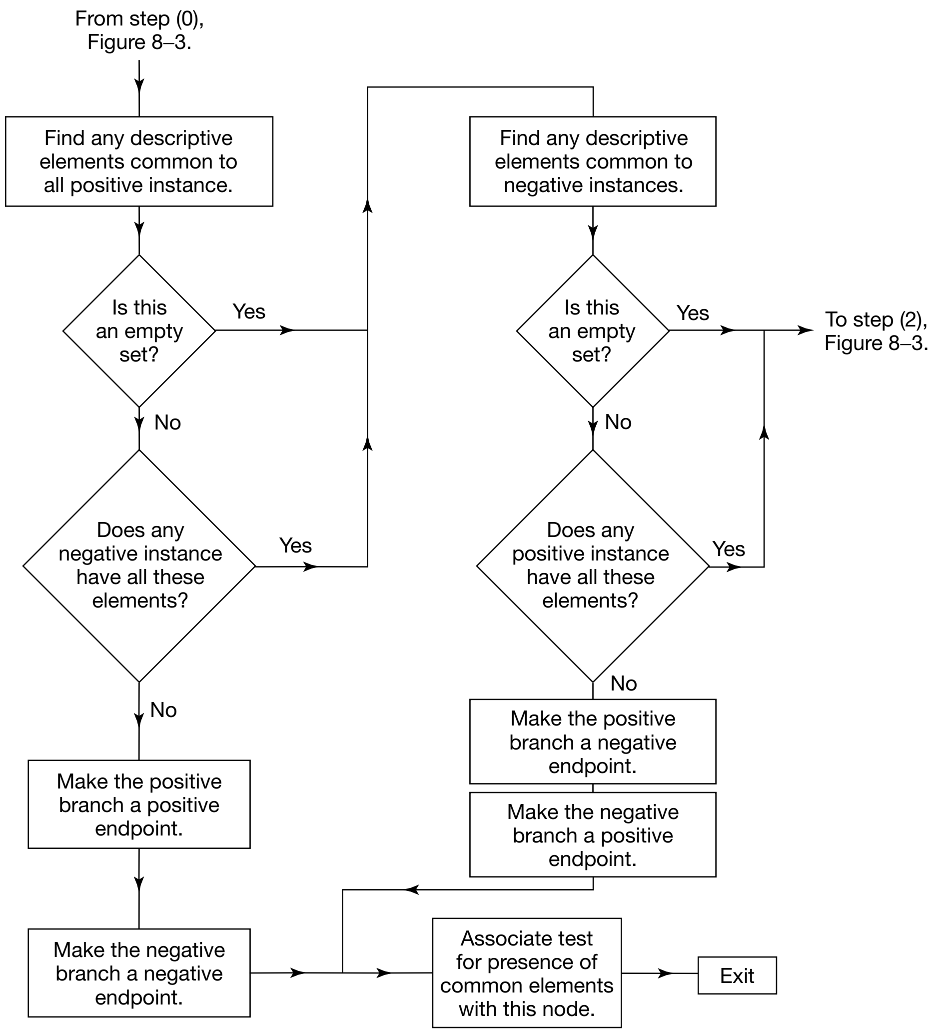


FIG. 10.12. Modification of conditional focusing for a rapid solution of conjunctive problems. Redrawn with permission from Hunt (1962: 236)

Winston, like Bruner and Hunt before him, assumed that concepts can be defined by lists of necessary and sufficient conditions (but cf. 8.i.b, 9.x.d, and 12.v–vi and x.b). His program learnt what counts as an *arch*, or an *arcade*, in the blocks world. On being presented with inputs labelled as examples or counter-examples (including near-misses), it generated an articulated description of each one: a hierarchical semantic network. Gradually, it modified the current candidate to construct a description matching *only* the examples.

That's easily said. In fact, the comparison and modification techniques were a significant KR achievement at the time (see Boden 1977: 252–67). Quillian had shown how to build semantic networks, and how to use them for simple inferences; but he'd said nothing about how to compare them. Winston's new network-matching techniques were designed to do just that. Indeed, they—not the program's power as a learner—were the main reason for people's interest.

An arch, for instance, consists of two vertical blocks with another block on top of them—not lying on the floor beside them. The shape of the top block is irrelevant: it may be a bulky cuboid, or a plate, wedge, or pyramid. Moreover, there must be a significant gap between the two vertical blocks: if they abut, it's not an arch but a near-miss. These facts wouldn't be evident on seeing just one arch. They'd become clear only after many different inputs—including some non-arches as well as various types of arch. The program's final concept is shown in Figure 10.13.

Much as Bruner and Hunt had found that the order of presentation matters when people learn concepts, so it mattered for Winston's program. And he had to avoid introducing several differences at once—hence the importance of the near-miss. But the inputs, and the list of candidate properties, were carefully tailored to suit the blocks world. Because this was so narrowly defined, Winston *couldn't* present the program with irrelevant information. For the same reason, it didn't have to decide which ones were *likely* to be salient. Real-life learning is different. It's possible only if one assumes that most possible descriptions are irrelevant, and (usually) that some relevant descriptions may be missing—or even false. Such issues were ignored by Winston. (And, to be fair, by most other AI workers until many years later: see 7.vi.d–f, 12.viii.c–e, and 13.iii.e.) Moreover, structural comparisons of complex semantic networks were difficult

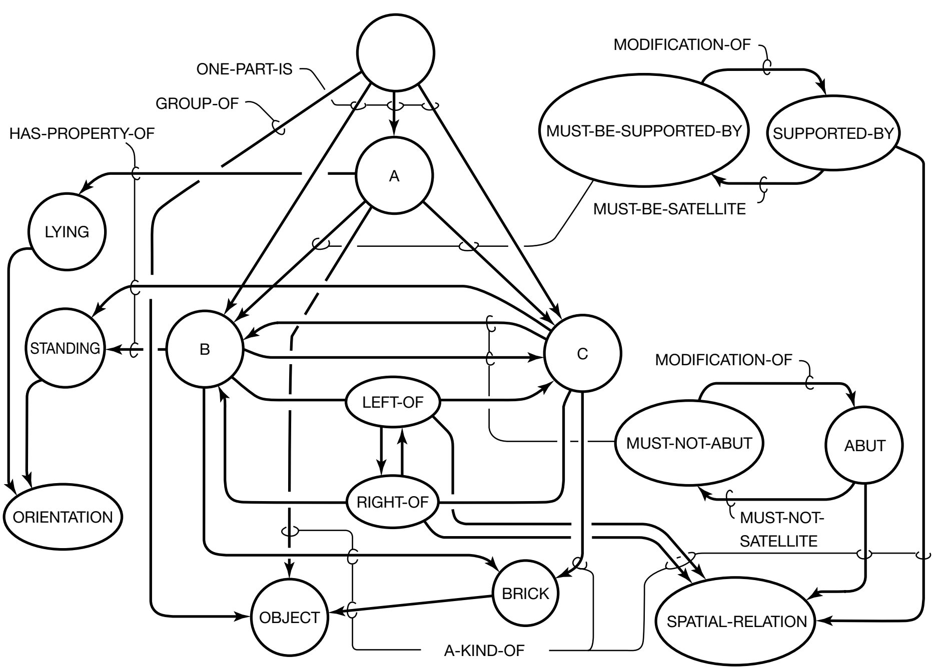


Fig. 10.13. Semantic network representing the concept of arch. Redrawn with permission from Winston (1975: 198)

to program, and to generalize (much more difficult than the property-counting done by Hunt's strategies). For both these reasons, Winston's program didn't hold much promise of practical applications.

Another issue ignored by Winston, and by most of his contemporaries, was *just why* certain programs did or didn't manage to learn. This question was an aspect of meta-epistemology, which most NewFAI scientists ignored—Minsky being an honourable exception (see i.g, above). It was highlighted in the mid-1970s, when researchers at Edinburgh not only proved that Winston's program had unsuspected limitations but also explained its successes in a principled way (Richard M. Young *et al.* 1977).

Their own concept-learning algorithm was a generalization of a focusing strategy earlier identified by Bruner. It could deal with hierarchical concepts (such as arch or arcade), and dealt uniformly with descriptive features (such as predicates, relations, and multi-valued dimensions) which Winston had treated as special cases. It worked irrespective of the order of presentation. And it explained the logical role of near-misses, which Winston had recognized intuitively. The key idea was to represent a candidate hypothesis by *two* nodes in the semantic network, one describing the specific instances already known to fall under the concept, the other holding the most general description known to be possible, given the instance encountered so far. So the two nodes set boundaries (identifying sufficient and necessary conditions) to the search-space within which the concept must lie. An efficient learning strategy would shift these boundaries until they met.

This Edinburgh algorithm was later used as the logical core of the widely used method of "version spaces" (T. M. Mitchell 1979; Mitchell *et al.* 1983). Its importance here, however, is as an early attempt to meet Drew McDermott's criticism of NewFAI in general: that it failed to analyse why programs were, or weren't, successful (see 11.iii.a).

Soon after Winston's 1970 memo weighed down the postmen's mailbags, his MIT colleague Sussman circulated a memo of his own (1973/1975). This described HACKER, a planning program engaged on a very different task: learning by doing. It wasn't the only NewFAI system that learnt by doing. Another was the Edinburgh pole-balancer (Michie and Chambers 1968), and yet another the STRIPS learner developed for the SHAKEY robot (Fikes *et al.* 1972*a*,*b*; Boden 1977: 280–6). But HACKER exhibited a capacity for deliberate self-criticism. Among other things, this suggested how Piagetian error-led constructive learning might be possible (see 5.ii.c, and Section vi.a below).

Much as Winston's program looked out for specific types of discrepancy, which it tried to reduce by relevant amendments to its conceptual models, so HACKER looked for discrepancies of particular kinds. As noted in Section iii.c above, it corrected its own mistakes by using a "teleological commentary" on its performance, couched in terms of various types of bug. But it didn't have to correct the same mistake repeatedly, because it learnt from its errors.

HACKER's learning took two forms. On the one hand, it stored its corrective "patches" in a central LIBRARY, indexing them according to the bugs they'd been written to correct. On the other hand, a patch devised in particular circumstances could be generalized to cover a general class of cases. (That had also been true of STRIPS, as we've seen.)

For instance, consider Figure 10.14. HACKER solved (*a*) instantly, using the primitive action for moving blocks. But (*b*) caused trouble, because the most nearly relevant primitive action required that the block to be picked up have no other block on top of it. However, HACKER classified this correctly as a prerequisite-missing bug, and patched the plan accordingly: A was moved off B, and then B could be put on C. This patch was stored in the LIBRARY, and brought out for problem (*c*)—which was therefore solved *immediately*, despite having the very same structure as (*b*). Moreover, problem (*d*) was also solved immediately, because the patch generated in response to (*b*) was sufficiently general to direct these steps (resulting in Figure 10.14.*e*):

Wants to put A on B
  Notices C and D on A
    Puts C on TABLE
    Wants to put D on TABLE
      Notices E on D
        Puts E on TABLE
      Puts D on TABLE
Puts A on B.

HACKER's learning-by-practice wasn't just a matter of repetition, where doing something several times makes it more likely to be done in future. (Think connectionism.) Rather, the program had learnt something about the teleological structure of what was being done, and used this to do something else—something better—next time. So when it used the primitive action to solve problem (*a*), it made a note that this was being done *in order to* get A onto B. Similarly, its comments on the patch noted that putting A on the table was done purely *in order to* clear B, *in preparation for* moving it onto C. In short, Sussman was following Papert's lead in proclaiming the "virtuous" nature of bugs (Sussman 1974; cf. vi.a, below).

As for learning by being told (McCarthy's dream for the Advice Taker), McDermott's TOPLE was intended to show that this is a highly active form of cognition. Besides interpreting the meaning of the input sentences, one has to make judgements on their truth. Indeed, the interpretation itself may depend on judgements about truth, as Winograd's SHRDLU had recently shown (see the discussion of *Put the blue pyramid on the block in the box*, in Chapter 9.xi.b). Those judgements are grounded in the cognitive system's world model.

So TOPLE didn't just add newly input sentences ("Advice"?) to a list, but interpreted them in the light of what it already knew—or believed—about the world being described. Ambiguities were resolved, and anaphora understood, by comparing the input sentence with the program's current world model. The model itself could be amended, if it conflicted with the facts provided in the input. For each input, a "tree of hypothetical worlds" would be constructed, capturing every possible interpretation. (That was done by using the CONNIVER programming language, co-developed by McDermott himself: v.d, below.) This tree would then be pruned, leaving only the "plausible" possibilities—i.e. those consistent with the world model. As McDermott put it:

> TOPLE is basically a very skeptical program. It wants to resist any change to the data base . . . so it forces itself to believe what it is told as cheaply as possible . . . [It] knows that some interpretations of the situations that it is told about are unlikely. It resists having to believe in such interpretations at all if more plausible interpretations of what it hears are available; and, if it must accept them, it demands some kind of compensating belief. (1974: 57)

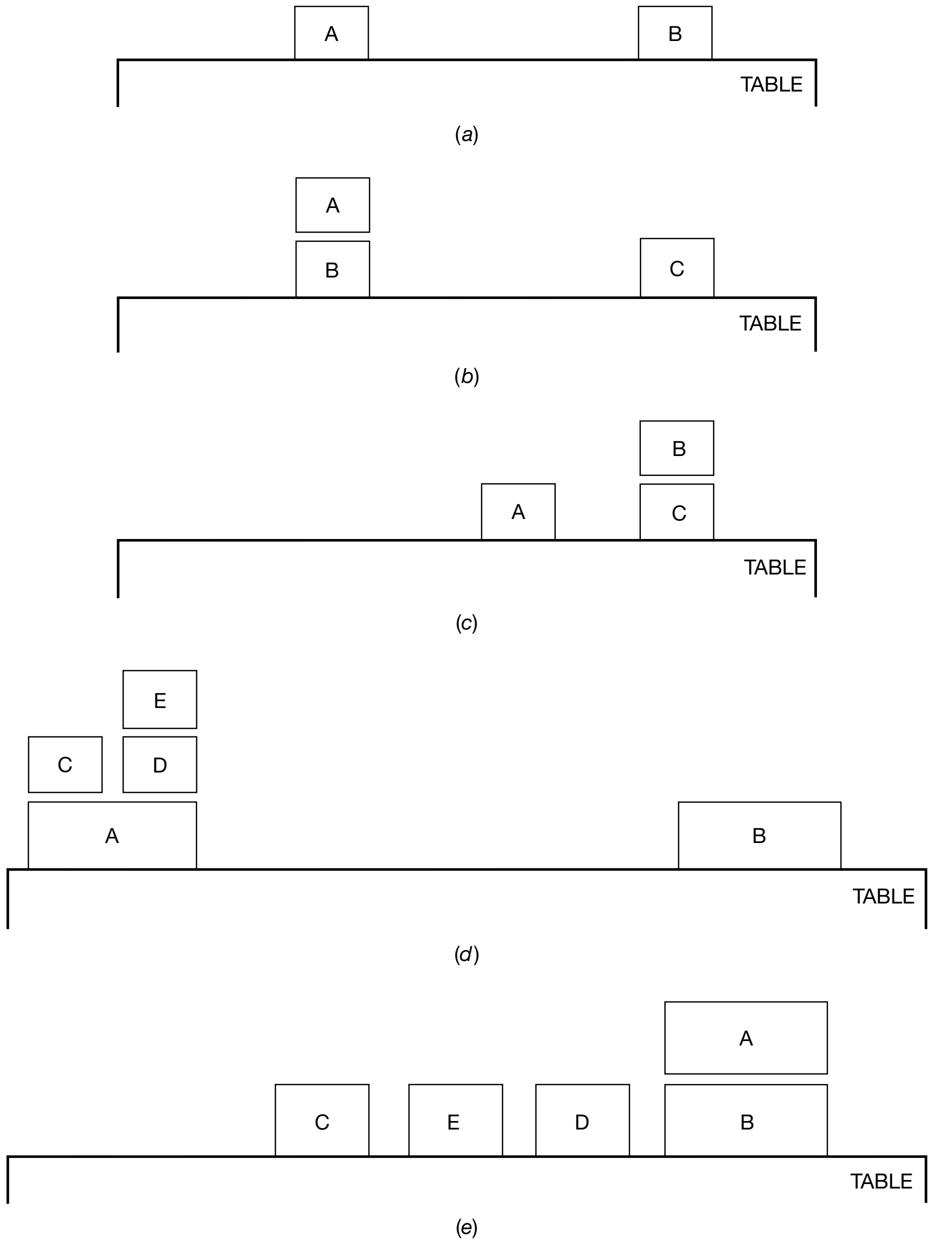


Fig. 10.14. Four problems solved by HACKER. Adapted with permission from Sussman (1975: 9–11) (*a*) Problem: Starting from this situation, put A on B. (*b*) Problem: Starting from this situation, put B on C. (*c*) Problem: Starting from this situation, put C on A. (*d*) Problem: Starting from this situation, put A on B. (*e*) HACKER's solution to problem (*d*).

The world model employed by TOPLE was a logical one, for McDermott was then still wedded to logicism (see 13.ii.a). But it was open to others to suggest non-logical versions, and the psychologist Philip Johnson-Laird soon did so (7.iv.d–e). He discussed the role of spatial mental models (and sentence–model comparisons, and even possible-worlds ontology) in reasoning and language understanding—not least, in learning by being told.

Despite these attempts to advance GOFAI work on learning, and despite the praise handed out to Winston's program (and, to a lesser extent, HACKER), the topic was still largely neglected. Ryszard Michalski, a key player in the revival of AI learning in the early 1980s, remembers that it was seen in the 1970s as "a 'bad' area to do research in (the dominant AI research was then on problem solving)" (R. S. Michalski, personal communication). What eventually brought it back to life was an exciting development of Hunt's work. But that's a story for a later chapter (13.iii.e).

e. 'Some Philosophical Problems'

In 1969 McCarthy and Patrick Hayes (1944–), then a Ph.D. student in Meltzer's Metamathematics Unit at Edinburgh, gave a paper at the fourth Machine Intelligence Workshop which very soon became a classic. Indeed, drafts had already been circulating for some time, and Hayes had been asked to give seminars on it in the months preceding the Workshop: "People were kind of primed for something important, and this was generally reckoned to be John's magnum opus in some sense" (P. J. Hayes, personal communication).

"John's" *magnum opus*, you'll notice. Why not "Pat's", too? Well, McCarthy was one of the three biggest names in NewFAI (alongside Minsky and Newell-and-Simon). Hayes was a mere research student. In fact, when he first impressed McCarthy with his ideas, he hadn't even achieved that far from exalted status: he'd just started a diploma in Machine Intelligence.

That was in 1966, when McCarthy came to Edinburgh for the second MI Workshop. Being too lowly a creature to be allowed to attend the sessions, Hayes begged Rod Burstall to arrange an audience with the great man. For McCarthy's grapevine-circulated memo on 'Situations, Actions, and Causal Laws' (1963) had excited him "hugely", as being "a natural way to put [my hero] Carnap into a computer". Burstall came up trumps, and scheduled the meeting:

> Rod warned me that John was a very busy man, I had 30 minutes at most, and not to make him cross. So I turned up at the appointed time . . . and I was so nervous that I had written out my questions on a piece of paper, in case I forgot them. So John snarled "Come in!" rather grumpily, obviously irritated at having to talk to this kid as a favor to Rod, and I sat down and started asking my questions. John tossed the first couple of answers off in a few words but then he started to get thoughtful, and by about the 5th question he figured I was reading from a list, and he suddenly stood, reached over the (tiny) desk and grabbed the list out of my hand, "Lemme see that!" And then he sat in total silence for several minutes reading it, and I sat in utter terror that I had committed some cardinal sin of intellectual discourse by having a list written out and would be banished from academic society for ever. And then he looked up and said "How would you like to come to California?" And I was so flabbergasted that I couldn't speak. And then I said "When?" and he said "Maybe in the summer" and I said "Wow! Great" and so on and then my 30 minutes was up and I didn't see him again . . . (Hayes, personal communication)

After several months of total silence, during which Hayes put it out of his mind as a dream, a telegram arrived summoning him and his wife and baby son to visit California—all expenses paid. That was in the spring of 1967, and Hayes remembers:

> While [I was] there we wrote the main draft of the paper. In fact, John had already written a lot of it, and my role was to be a kind of in-house critic/questioner, like a court jester, for most of the stuff; but I did write the section on situation calculus and modal logic (Section 4 of the paper) which, apparently, introduced the actual term "situation calculus" into the language though at the time this didn't seem like a big deal. (personal communication)

Their declared intention was to discuss some familiar philosophical problems *from the standpoint of* AI—and this, they did. But they also identified a huge philosophical problem *concerning* AI. Specifically, they defined the frame problem.

The roboticist Lynn Andrea Stein (1965–), some twenty years later, said that "a definition of the 'frame problem' is harder to come by than the Holy Grail" (L. A. Stein 1990). I'll offer one, nevertheless—or rather, two. To see why a dual definition is needed, one must first consider just how McCarthy and Hayes introduced the problem in their 1969 paper.

They didn't do this in a defeatist spirit. On the contrary, they located the frame problem in the context of a constructive discussion of how new forms of logic might be used for AI purposes. (Thirty-five years later, McCarthy's home page on the Web described it tersely as "the basic paper on situation calculus".) In other words, the generalities being sought by NewFAI were set alongside, and to a large extent identified with, the generalities sought by philosophical logic. And the McCarthyite faith in logic was undimmed: in their eyes, the frame problem was potentially solvable.

They weren't afraid of reaching for the skies. The philosophical chestnuts addressed in their paper were:

* causation;
* the nature and origins of knowledge;
* purpose, action, and ability;
* counterfactual conditionals;
* self-knowledge;
* and free will/determinism.

In addition, they discussed various modal (i.e. non-truth-functional) logics. These had been developed by philosophers in order to express certain highly general aspects of the world clearly, and to enable inferences about them to be deduced accordingly. The subject matter, ranging over metaphysics and mind, included:

* possibility and necessity;
* probability;
* ontology;
* knowledge and belief;
* tensed statements and time;
* decision and purposive action;
* commands;
* obligation and permission;

* interrogatives;
* and the conditions necessary for communication.

So McCarthy and Hayes were optimistic, not to say hubristic. Although modal logics were a twentieth-century invention (mostly pioneered after mid-century), the problems they were focused on were much older. As for the "chestnuts", these had puzzled philosophers for hundreds of years. Questions about the basic ontology of the universe (states, events, properties, changes, actions . . . ?), for example, had kept generations of grey-beard metaphysicians busy. Now, here were two NewFAI researchers, one of them a mere pipsqueak, brashly claiming to be able to illuminate them—even, *mirabile dictu*, to solve some of them. Causation and counterfactuals, for instance, could—so they said—be explicated by considering systems of finite automata interacting according to deterministic rules (McCarthy and Hayes 1969: 470–7, 479–80).

This was strong meat. As remarked in Chapter 16, philosophical problems don't get solved in a hurry. So their paper was nothing if not ambitious. Admittedly, their hubris had limits. For they were also claiming that AI "needs" philosophy. Even though most philosophy was declared by them to be "irrelevant", NewFAI scientists could learn a lot from some philosophers.

They weren't the only people who thought that philosophy and NewFAI needed each other. Others were saying that, too (Minsky 1965; Boden 1965, 1970, 1972; Newell 1973*b*). By the end of the 1970s this claim was even more common (e.g. Newell and Simon 1976). It was argued at length—and with no little passion—by Aaron Sloman, who declared that "within a few years philosophers . . . will be professionally incompetent if they are not well-informed about these developments" in AI (Sloman 1978, p. xiii). Moreover, some of the early cyberneticians—especially Craik (1943) and McCulloch (1948, 1961*a*; Pitts and McCulloch 1947)—had been equally bold in their claims to be able to help solve long-standing philosophical problems (see Chapters 4.iii.a–c and vi.b, and 12.i.c). So the coupling of philosophy with computers wasn't new.

However, McCarthy and Hayes were looking to philosophy in a more focused way. Their approach was distinctive in its heavy reliance on modal logics. Most of these had been developed very recently, in the 1960s, by philosophers such as Arthur Prior, Jaakko Hintikka, Georg von Wright, Saul Kripke, and Nicholas Rescher.

In recalling the many ancient philosophical disputes, McCarthy and Hayes reiterated the claim made in McCarthy's harbinger papers (1959, 1963), that natural language and common-sense knowledge could be represented in predicate calculus terms. And they promised that (their improved version of) resolution theorem proving could be used to explore the implications of all the highly problematic concepts listed above—and even to prove the correctness of complex action strategies. It followed that any philosophy of knowledge (epistemology) which could not, at least in principle, help AI "to construct a computer program to seek knowledge in accordance with it, must be rejected as too vague" (1969: 467).

For most AI scientists, however, what was especially interesting was the paper's relevance not for philosophy, but for the practice of AI. And there, despite the optimism, lurked a huge problem—one which would dominate the research of both men, and many of their AI colleagues, for decades. This was what they called "the frame problem" (p. 487).

McCarthy and Hayes introduced the frame problem by reference to their attempt to write an AI program that knew something about human communication: specifically, telephone conversations. When representing one person's ability to get into a conversation with another, they'd been "obliged to add the hypothesis that if a person has a telephone he still has it after looking up a number in the telephone book", and also to assume if someone looks for a number they'll know it, and that if they dial it they'll speak to their friend (pp. 485, 487, 489). Clearly, this spelled trouble.

Indeed, it spelled potentially endless trouble. It may not actually be the case that if Peter looks up John's number in the book, he will know it, or that if he dials the number he'll soon be talking to John. As the authors pointed out (on p. 489):

1. The page with John's number may be torn out.
2. Peter may be blind.
3. Someone may have deliberately inked out John's number.
4. The telephone company may have made the entry incorrectly.
5. John may have got the telephone only recently.
6. The phone system may be out of order.
7. John may be incapacitated suddenly.

Of course, they said, we could add terms contradicting any one, or even all, of these exceptions—"But we can think of as many additional difficulties as we wish, so it is impractical to exclude each difficulty separately."

They suggested three methods of escape, the first of which was to use the notion of a state vector, or "frame", associated with a theorem-prover. Here, various aspects of the (formally described) situation are listed, and the effect of any action is defined by stating which situational aspects are changed—all the others being presumed unchanged (p. 487). Earlier in the paper, for example, they'd mentioned a simple action sequence enabling a monkey to reach some bananas suspended from the ceiling: *Move the box under the bananas; climb onto the box; and reach for the bananas* (p. 481). A clear, though usually unstated, assumption of this strategy was that moving the box doesn't move the bananas. (That's usually true, of course. But others soon pointed out that it's not true in the situation depicted in Figure 10.15.)

Second, they said, modal terms like "normally" or "probably" could be introduced into the logic. This would act as a default assumption, yielding the expected conclusion unless there were a specific statement saying, for instance, that the phone system wasn't working (p. 489). (They rejected the idea that each sentence should be given its own probability measure, because in most cases we wouldn't know how to do that, and because it's not always clear how to match probabilities to people's subjective conviction: p. 490. Although they didn't say so here, this was a dig at fuzzy logic: Zadeh 1965—see 13.i.a.)

And third, they suggested using Rescher's (1964) recently published philosophical work on hypothetical reasoning and counterfactuals. His logic, they said, might enable a program to search for, and sensibly fix, inconsistencies arising from its (deliberately false) assumption that *nothing* would change as a result of a given action (p. 499).

None of these suggestions was spelt out in detail in their paper. And they made this telling confession:

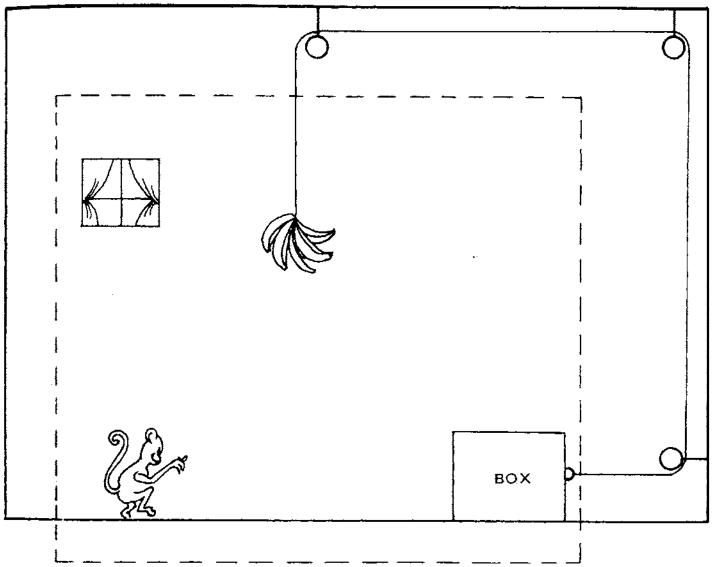


FIG. 10.15. Monkey and bananas problem: how does the monkey get the bananas? (The usual approach to this problem assumes, though doesn't necessarily explicitly state, that the relevant 'world' is that shown inside the dotted-line frame. In other words, nothing exists outside this frame which causes significant changes in it on moving the box.) Reprinted from Boden (1977: 387)

We hereby warn the reader, if it is not already clear to him, that these ideas are very tentative and may prove useless, especially in their present form. However, *the problem they are intended to deal with, namely the impossibility of naming every conceivable thing that may go wrong, is an important one for artificial intelligence, and some formalism has to be developed to deal with it.* (p. 490; italics added)

While they were writing these words, the first of their three escape routes was already being developed by some of McCarthy's Stanford colleagues. On the one hand, by Green (who described it at the same Edinburgh workshop: C. C. Green 1969); on the other, by the programmers of STRIPS, the triangle-table planner used by SHAKEY (Fikes and Nilsson 1971; Fikes *et al.* 1972*a*, *b*). And as soon as the Edinburgh paper was off the press, people started writing about "the frame problem" in AI (Raphael 1971; Sandewall 1972).

Roboticists in particular, who couldn't avoid real-world exigencies even in highly artificial environments, were well aware of "the impossibility of naming every conceivable thing that may go wrong". But common-sense reasoning was threatened too, as McCarthy and Hayes' telephone example had shown. The impossibility of giving cut-and-dried definitions of concepts, or natural-language words, were further sources of this general difficulty.

In fact, their find-the-phone-number example had identified *two* problems, not one. Because both of these were introduced in relation to the same example, they're often conflated—even confused. Both are called "the frame problem", although it's usually clear from the context which one the speaker has in mind.

The first was the problem of knowing just which aspects of a situation would be changed by a particular action, and which would not. For instance, whether it's true that "if a person has a telephone he still has it after looking up a number in the telephone book", or that moving the box doesn't move the bananas. That is "the frame problem" strictly so called, because that is the one addressed by the first of their three escape routes, which is where the word "frame" entered the discussion.

The second was the problem of reasoning with incomplete knowledge—where one doesn't know, for instance, whether the page has been torn out of the phone book, or whether the phone system is out of order. This problem (often called the frame problem, but sometimes the qualification problem: see 13.i.a) is a feature both of our inevitable ignorance about the facts of the real world, and also of the irredeemable vagueness of ordinary-language concepts. It's normally dealt with by some form of default reasoning—their second escape route. But of course the default doesn't always apply.

In general, the first version of the frame problem was relevant for robot planning, and the second for common-sense reasoning. Given the importance of both of these (especially the second) for AI in general, it's hardly surprising that the frame problem (dual sense) has been on the lips of AI researchers, not to mention AI's opponents, ever since it was first named (e.g. Raphael 1976: 146–52; Pylyshyn 1987; Dennett 1984*b*; Ford and Hayes 1991; Ford and Pylyshyn 1996; Sperber and Wilson 1996; Shanahan 1997)—nor that it's mentioned in various other chapters in this book (e.g. 7.iii.d, 8.i.b, 9.d–f).

McCarthy and Hayes themselves were no exception. They spent many years trying to defuse the problem, working respectively on non-monotonic reasoning and naive physics (Chapter 13.i.a–b).

(Today, Hayes feels that they were over-sanguine in the 1960s and 1970s, and that the frame problem is probably unsolvable: see 13.ii.b. Certainly, the claim—see Shanahan 1997—that it has already been solved is over-optimistic for the general case.)

The statement of the frame problem, and the suggestions regarding a situation calculus, were the most influential features of 'Some Philosophical Problems'. But a third was sometimes picked up too. This was McCarthy and Hayes' distinction between three sorts of representational "adequacy", whether in minds or in machines: metaphysical, epistemological, and heuristic. (They might have embarked on a philosophical discussion of reductionism here, but they didn't; Fodor 1968 had just used broadly comparable ideas to do so.)

A representation of a problem is metaphysically adequate "if the world could have that form without contradicting the facts of the aspect of reality that interests us" (p. 469). Such representations were mainly useful, they said, for constructing general theories. For example:

1. The representation of the world as a collection of particles interacting through forces between each pair of particles.
2. Representation of the world as a giant quantum-mechanical wave function.
3. Representation as a system of interacting discrete automata [a system which they went on to explore in their paper]. (p. 469)

A representation is epistemologically adequate "if it can be used *practically* to express the facts that one actually has about the aspect of the world [being considered]" (italics added). The language of particles-and-forces, they said, can't be used to express the facts that "dogs chase cats" or "John's telephone number is 321–7580", but ordinary language can. On the other hand, English can't express the information processing involved in recognizing a particular face. (Nor, one might add, can it express the NLP rules involved in interpreting Chinese—despite John Searle's famous claim to the contrary: see 16.v.c.)

With respect to philosophically problematic concepts such as cause, ability, and knowledge, a main aim of their paper was to find a way of representing/expressing these which would be epistemologically adequate for a computer—or, indeed, for a metaphysician. In that case, they would *no longer be* philosophically problematic.

Finally, a representation is heuristically adequate "if the reasoning processes actually gone through in solving a problem are expressible in the language". Only in very simple cases is the representation which is epistemologically adequate *also* heuristically adequate. They said virtually nothing more about this level of adequacy, but it underlay the already recognized need for high-level programming languages, and would soon aid the development of AI planning.

There may be more than three representational levels, for heuristic adequacy can exist on multiple levels of abstraction. High-level programming languages are needed because people can't think fruitfully in machine code when they're trying to solve relatively large-scale problems—in other words, machine code isn't heuristically adequate for such problems (Section v, below). So solutions found in the PLANNER programming language must be translated into LISP, and then into machine code, before they can actually be executed.

We've seen, for instance, that neither GPS nor early STRIPS could produce an overall strategic plan before starting on the detailed solution. In effect, they made no distinction between heuristic and epistemological adequacy. As problem complexity increased, they were soon scuppered by the combinatorial explosion. But that distinction was recognized in Sacerdoti's ABSTRIPS. This could ignore the low-level details while formulating the strategic plan, and also (during execution) translate that plan into detailed instructions about what needed to be done. Whereas STRIPS couldn't see the wood for the trees, ABSTRIPS could represent a hierarchy of woods, in which copses and trees of decreasing size could be successively specified in heuristically adequate terms, until an epistemologically adequate level was reached.

In sum, the logicist case was argued more explicitly, and (thanks to philosophical logic) more persuasively, in 'Some Philosophical Problems' than in McCarthy's harbinger papers. But the ever-hungry worm nestling at the heart of logicism had been made clearly visible.

10.iv. The Need for Knowledge

Minsky had said (in 'Steps') that work on AI "is concerned with using all we know to build the most powerful system that we can" (1961*b*: 446). But much of the newest NewFAI, as we've seen, was ignoring much of what we know. In other words, it was seeking *general* theories of intelligence. By the 1970s, however, Minsky's remark was being interpreted more seriously. For generality wasn't working as well as had been hoped. Increasingly, the 1970s saw a flight from domain-independent models. (This didn't apply to Minsky himself, who complained about this trend in his high-visibility Turing Award lecture: Minsky 1969.)

As Winograd put it (in March 1974), there had been a growing split between people "making programs do more and more intelligent things" and people studying "general issues of representation and problem-solving". But from now on, things would be different:

Today I see a movement towards a middle ground. The theorem-proving craze is slowing down. People are aware that very general systems are not going to be the basis of practical programs, and people who have been doing specialized programs are asking what these programs have to offer which can be brought to bear on more general problems. (Winograd 1974: 92)

a. A triumph, and a threefold challenge

Winograd was a mere graduate student in September 1970, and a neophyte postdoc in September 1971. Nevertheless, he was invited to give a talk at the second IJCAI Conference in London that autumn. This was no mere parallel session, fighting for attention among half a dozen others. On the contrary, it was the inaugural *Computers and Thought* Lecture, supported by the royalties donated by Feigenbaum and Feldman (ii.b, above). Those of us in the UK who were planning to attend the meeting were enthused by the rumours of his participation, for his work—available as a Technical Report from MIT's AI Lab since February of that year—was already a glowing beacon for the cognitive science community.

Why all the fuss? Well, Winograd's SHRDLU—written as his Ph.D. thesis of 1971 and officially published, to huge acclaim, a year later—was a triumph of NLP (9.xi.b). It was also a triumph of AI programming. But Winograd had done more than write an amazing program: he'd challenged three basic assumptions of the conventional NewFAI approach.

First, he abandoned generality and relied instead on detailed domain knowledge. Indeed, he was one of the strongest voices arguing for this move. Since he wasn't merely arguing but also achieving, and that in a spectacular fashion, his words carried weight. In interpreting the input sentences, SHRDLU relied heavily on its detailed knowledge of syntax. Its unprecedented ability to parse sentences as complex as *How many eggs would you have been going to use in the cake if you hadn't learned your mother's recipe was wrong?* was the result. And Winograd drew a general moral: whatever the problem domain happened to be, successful AI required that the relevant domain knowledge be provided to the program.

He wasn't the only one in the early 1970s to be saying this. Within NLP, HEARSAY combined syntactic, semantic, pragmatic, lexical, phonemic, and phonetic knowledge—all simultaneously available via the blackboard architecture (Reddy *et al.* 1973; Newell *et al.* 1973). Outside NLP, work in vision and expert systems was similarly relying on specific knowledge of the domains concerned (see subsections b–c, below). And Joel Moses (of MIT) had seen the writing on the wall for NewFAI generalism as early as 1967:

The word you look for and you hardly ever see in the early AI literature is the word knowledge. They didn't believe you have to know anything, you could always rework it all . . . In fact 1967

> is the turning point in my mind when there was enough feeling that the old ideas of general principles had to go [which is why, for instance, Danny Bobrow ignored GPS to work on STUDENT] . . . I came up with an argument for what I call the primacy of expertise, and at the time I called the other guys the generalists. I was antigeneralist for many years. (interviewed in McCorduck 1979: 228–30)

But Winograd was especially influential. Indeed, Moses went on to say that "it took some difficult doing" for knowledge-based AI to be recognized as necessary, and "I think what finally broke [the generalists'] position was Winograd" (McCorduck 1979: 229).

The second major challenge to NewFAI posed by Winograd was his criticism of "declarative" programming and his urging of "procedural" programming instead. The logicist dream was that intelligence could be modelled as theorem proving carried out propositions in predicate calculus. Such propositions are declarative: they state *It is the case that* . . . Even those NewFAI workers who favoured non-logical representations (such as semantic networks) thought of programs, in essence, as starting from facts and inferring other facts from them. (For instance, the *isa* network links between *cat/mammal/animal* meant that *A cat is a mammal* . . . etc.) So what AI programmers needed to do was to declare (*sic*) the facts relevant to the domain concerned, and then let some general-purpose inference engine—a theorem-prover, or a network-rambler—take over. In short, providing domain knowledge to a program meant telling it *what is the case* rather than *what should be done*.

Winograd disagreed. His definitions of words weren't 'factual' statements or logical axioms expressing the word's meaning (see 9.xi.b). Nor were they semantic networks, whose pattern of nodes and links defined the meaning. Rather, they were mini-programs ("procedures"), which would be run when the word was encountered. On encountering the definite article *the*, for example, SHRDLU would immediately start looking for one and only one thing which fitted the following description, e.g. *pyramid*. Similarly, SHRDLU's definition of *and* was a mini-program that interrupted the normal parsing sequence to tell the system to "look for another one [i.e. a syntactic category] like the one you just found" (Winograd 1973: 179). This enabled it to cope sensibly—and relatively quickly—with both *The giraffe ate the apples and peaches* and *The giraffe ate the apples and drank the vodka*. Sometimes, the definition of a word would include a program for examining *previous* sentences—as for the word "one" in *Pick up the green one*. (As Winograd pointed out, all this was possible largely because he was using a version of the new PLANNER programming language: v.d, below.)

A wide range of work in AI and computational psychology was profoundly affected by Winograd's recommendation of procedural programming (see 7.ii.d and iv.c). Throughout the 1970s, as we've seen (iii.a, above), knowledge representation was a key focus of GOFAI research. The already familiar question of whether predicate logic could represent all of human knowledge was now joined by the broader question of whether *any* declarative representation could do so. For a while, there were heated discussions between declarative and procedural camps. The former presented the latter as tinkerers, content to cobble programs together without worrying about logical consistency. At the same time, logicist approaches (and logic-based languages) were viewed with suspicion, not to say disdain, by the proceduralists. (Often, they drew an analogy with Gilbert Ryle's distinction between knowing *how* and knowing *that*: pulling such a famous philosopher on board seemed to raise their spirits significantly—see 16.i.c.)

By the late 1970s, however, most people had realized that the divide between the two programming styles wasn't absolute. After all, even LISP instructions (procedures) could be used as data by other instructions: see v.c, below. And Newell and Simon's productions, despite being based on a system of *logic*, were rules which *made things happen*. So whether one chose to regard a particular representation of knowledge as "procedural" or "declarative" was largely a matter of emphasis, or point of view.

Winograd himself eventually admitted that AI work should combine the two types of representation, so as to get the best of both worlds (1975). Indeed, a prime aim of the new programming language (KRL) he developed with Bobrow was to do just this (Bobrow and Winograd 1977). KRL made quite a stir at the time. It was given pride of place as the very first paper to be published in *Cognitive Science*, and was later described as "one of the more ambitious efforts in the history of AI representation frameworks" and "the high point of a certain style of Knowledge Representation" (Brachman and Levesque 1995: 263). (Not everyone was impressed, for it was also described as "a splendid edifice [constituting] a castle in the air": McDermott 1978.)

KRL tried to get "the best of both worlds", because the two approaches had complementary advantages and drawbacks. It's easier to add new knowledge to a declarative representation; for although simply *adding* an instruction may (sometimes) be just as easy, its implicit effects are less clear. In general, declarative languages are easier for people to understand, whereas procedurally embedded knowledge may be highly opaque to the human user. But adding knowledge isn't enough: one must also specify how it can be used. Often, it's easier to embed knowledge implicitly inside a procedure (which will actually get the relevant task done) than to provide it as a theoretical background from which the necessary procedures must be inferred. Imagine trying to give a theoretical justification of all the heuristics used in AI programs, for instance: even deducing *Protect your queen* wouldn't be trivial. For effective action, a set of domain-specific procedures may be more economical than long lists of facts to be manipulated by general deductive procedures. In short, the *advice* "If you know Turing's human, assume that he's fallible" may be more helpful than the *data* "Turing is human, and all humans are fallible".

All of that became evident, however, largely as a result of the discussion prompted by Winograd's unflinching support for procedural programming in his early work. The familiar Popperian point applied: your scientific theory doesn't have to be right, but it should lead to challenges and tests that show whether it may be right, and—if not—just what theory might be better.

The last of Winograd's three challenges was directed against hierarchical programming: "heterarchical" programming was recommended instead. In a hierarchical program, one part—the master program, or central executive—has overall control and the others are subordinate to it, as mere subroutines in the service of its goals. Hierarchy had been crucial to GPS, and even to Pandemonium (whose demons could communicate only upwards, not laterally or downwards: 11.ii.d). Moreover, Simon (1962, 1969) had declared hierarchy to be necessary for both artificial and biological intelligence, despite certain disadvantages pointed out by his colleague Newell (1962). Winograd didn't deny its importance. But he did deny that inflexibly top-down control was the best way of exploiting it. In place of rigid hierarchy, he said, what AI needed was heterarchy.

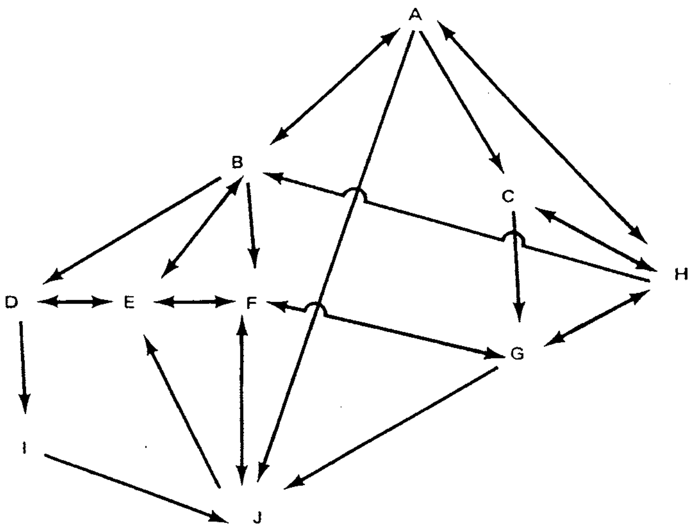


FIG. 10.16. Heterarchy. Reprinted from Boden (1977: 126)

In a heterarchical program, control is more equally distributed throughout the system, and internal communication between the various subroutines is increased. The component programs can call upon each other upwards, downwards, or sideways (see Figure 10.16). Moreover, they can do this at various points in their (potentially independent) functioning. SHRDLU, for example, could do it many times in parsing a single sentence (see 9.xi.b).

The added flexibility which heterarchy provided to AI problem solving was avidly welcomed for work on many domains. Fahlman, for instance, identified heterarchy as the key source of BUILD's ability to plan blocks-world structures. And an example applied to vision, for use by the MIT robot (for which SHRDLU had been written in the first place: see 9.xi.b), is shown in Figure 10.17.

The various parts of a heterarchical program were *thought of* very differently from the subroutines in GPS. Winograd's colleague Winston, for instance, put it like this:

> Communication among these modules should be more colorful than mere flow of data and command. It should include what in human discourse would be called advice, suggestions, remarks, complaints, criticism, questions, answers, lies [better: approximations], and conjectures . . . Note particularly that . . . programs normally thought to be low level may very well employ other programs considered high level . . . [In computer vision, for instance] line-finders that work with intensity points are low level but may certainly on occasion call a stability tester that works with relatively high-level object models. (Winston 1972: 444)

In short, a heterarchical program was conceptualized as a community of autonomous *agents*—although that terminology didn't become widespread in AI until the 1980s (see Chapter 13.iii.d).

The term "heterarchy" wasn't coined by Winograd. It had already been used by neurophysiologists such as McCulloch (1947). And they'd borrowed it from the political philosophers. Today, it's typically used—by sociologists, political scientists,

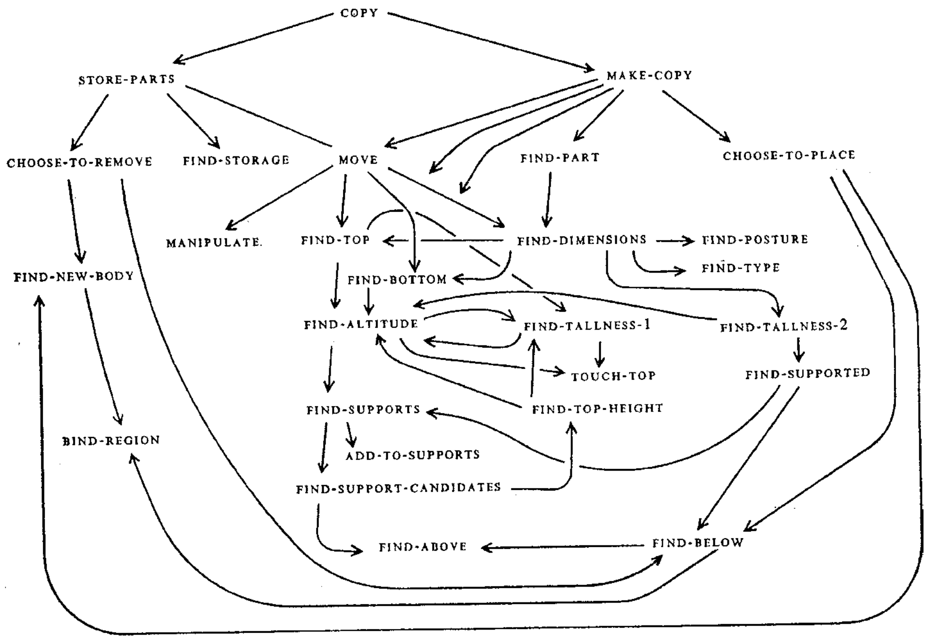


FIG. 10.17. Heterarchical organization of the MIT robot's vision system. Reprinted with permission from Meltzer and Michie (1972: 456)

and management gurus—to describe human organizations (e.g. Stark 1999). Within AI, it has fallen out of use, being replaced by "agents" or "distributed AI".

These terminological points are relevant because they highlight the human and political aspects of the concept—which were never far from the surface, even in technical treatises on computer programming. Autonomous agents deserve respect. Someone who's allowed (see the quotation above) to advise, suggest, criticize, and complain . . . is an equal, not a mere lackey. And so it was with the components in heterarchical programs. Winston, again: "the modules interact not like a master and slaves but more like a community of experts" (1972: 443). The new programming style was repeatedly described in terms of "committees" of "experts", bound by mutual respect and judicious negotiation rather than autocratic power relations.

In other words, socio-political metaphors were commonly used by AI scientists to recommend heterarchy over hierarchy—at the height of the Cold War, with resonant implications (see 1.iii.b–d). (Twenty years later, when "multi-agent systems" became all the rage, their popularity would be partly fuelled by similar political considerations, even though the Cold War had thawed by then: 13.iii.d.)

(As for the wunderkind Winograd, what happened to him? It's an interesting story, and not one which AI people like to hear. For in their view, their hero betrayed them: see Chapter 11.ii.g.)

b. Clearer vision

Vision is the prime example that shows why AI is no longer defined as Minsky originally defined it: "the science of making machines do things that would require intelligence if done by men" (Minsky 1968, p. v). We don't normally think of vision as requiring intelligence: after all, everyone who isn't blind can do it—and so can squirrels. Accordingly, the initial NewFAI attitude to vision was that it must be pretty simple. As late as 1966, Minsky asked a bright first-year undergraduate, namely Sussman, to spend the summer linking a camera to a computer and getting the computer to describe what it saw. This wasn't a joke: both Minsky and Sussman expected the project to succeed (Crevier 1993: 88).

In the very earliest days of NewFAI, "vision" had meant pattern recognition, modelled by highly general techniques (see Uhr and Vossler 1961*b*). This might conceivably have been enough for the bookreader-for-the-blind envisaged by Selfridge and McCulloch. But it wasn't enough to capture vision as described by the "New Look" psychology of Bruner and Richard Gregory, which posited top-down influences from perceptual hypotheses about the real world. Indeed, Gregory (1967) was arguing that visual computers would suffer from illusions just as humans do, and for similar reasons (6.ii.e).

The implication was that pattern recognition should make way for scene analysis, where the program's task was to interpret visual input *as depicting objects in real-world scenes*. (Most of the early scene-analysis programs are detailed in Boden 1977, chs. 8–9.) This, in turn, implied that the image should be articulated into distinct parts—which pattern-recognizers couldn't do. So some NewFAI pioneers started thinking about the *structure* of images. Narasimhan in Bombay, and (following Narasimhan's lead) Max Clowes in Oxford/Canberra, offered hierarchical "picture grammars" inspired by Chomsky's syntax (Narasimhan 1964, 1966; Clowes 1967, 1969). And Clowes, whose earliest work had been in technological character recognition (Clowes and Parks 1961), now drew inspiration also from psychology and neurophysiology: specifically, from Miller's 'Magical Number Seven' and retinal/cortical feature-detectors (Clowes 1967: 181, 195–6; see 6.i.b and 14.iv.a–b).

In particular, pattern recognition wasn't enough for robotics. An AI robot—as opposed to a fixed-routine *Unimate* bolted to a factory bench—needs to know where something is in 3D space; what size and shape it is; what the currently invisible parts of it are like (e.g. the occluded corners of the wedge numbered "11" and "12" in Figure 10.18); what the orientations of the various surfaces are; and how to distinguish one 3D something from another one in the first place. Accordingly, the people associated with the MIT robot, Stanford's SHAKEY, and Edinburgh's FREDDY tried to write programs enabling a computer to interpret 2D images in terms of 3D scenes.

Those last nine words were easy for me to write, and for you to understand: today, the distinction between (retinal) "image" and "scene" is clear. In the 1960s, it wasn't. With the 20:20 vision of hindsight, it's amazing how blind early NewFAI was to it. When AI workers first tried to write 2D-to-3D programs, they often made mistakes precisely because they hadn't taken this distinction properly on board. In other words, they hadn't realized the need to include *knowledge of the image-forming process* in their programs.

The image-forming process held to be most relevant at that time was projective geometry. 2D-to-3D algorithms based on physical optics would be written later, in the mid- to late 1970s (14.v.f and 7.v.b–d). But before then, optics was used only in 2D-to-2D line-finders (Roberts 1965; Shirai 1973) and region-finders (Tenenbaum *et al.* 1974; Tenenbaum and Weyl 1975). Computer vision used hand-drawn line drawings, or line drawings generated from grey-scale camera input by line-finders (Winston 1975).

Because the geometrical knowledge embodied in the first scene-analysis programs wasn't made fully explicit, they succeeded for largely mysterious reasons. Likewise, they suffered apparently inexplicable failures.

The prime example of that type of blindness was due to Adolfo Guzman (1943–), a Mexican graduate student at MIT (today, working at Mexico's National Polytechnic Institute). On one level, Guzman's SEE program (1967, 1968, 1969) was a triumph. For it could interpret a complex image as representing many separate physical objects: eleven, in Figure 10.18.

It did this by first finding the *vertices* (classified into nine basic types), and then using them to guide linkages between line-bounded *regions*. A link between two regions assigned them to one and the same physical thing. Linked regions were usually neighbours, but not always (see Figure 10.19). So seven non-adjacent regions in Figure 10.18 (labelled 3, 21, 22, 23, 24, 28, and 29) were all assigned to "one" object, as were regions 1, 2, and 33.

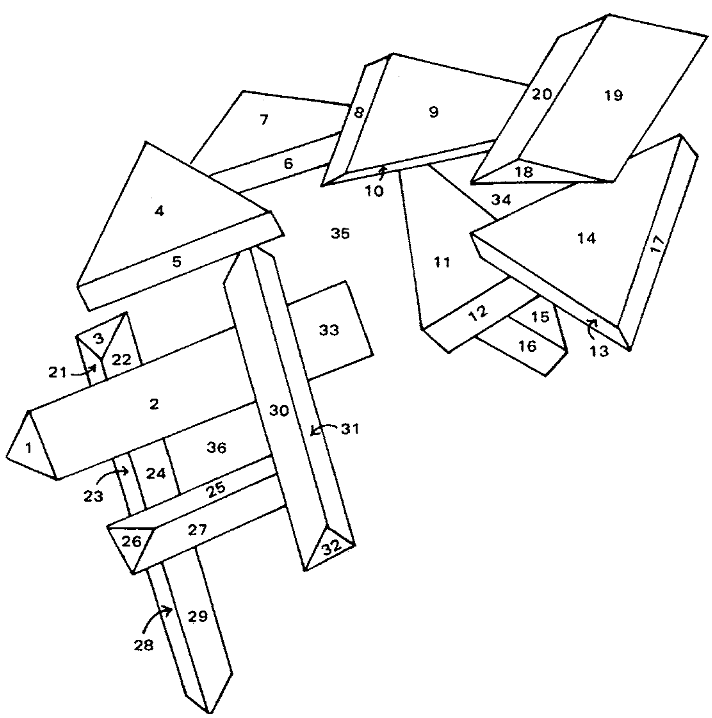


Fig. 10.18. Guzman's "HARD". All bodies were correctly identified by SEE, even though one (regions 6:7) has no "useful" visible vertices. The background (34:35:36) also was correctly found. Reprinted with permission from Guzman (1969: 273)

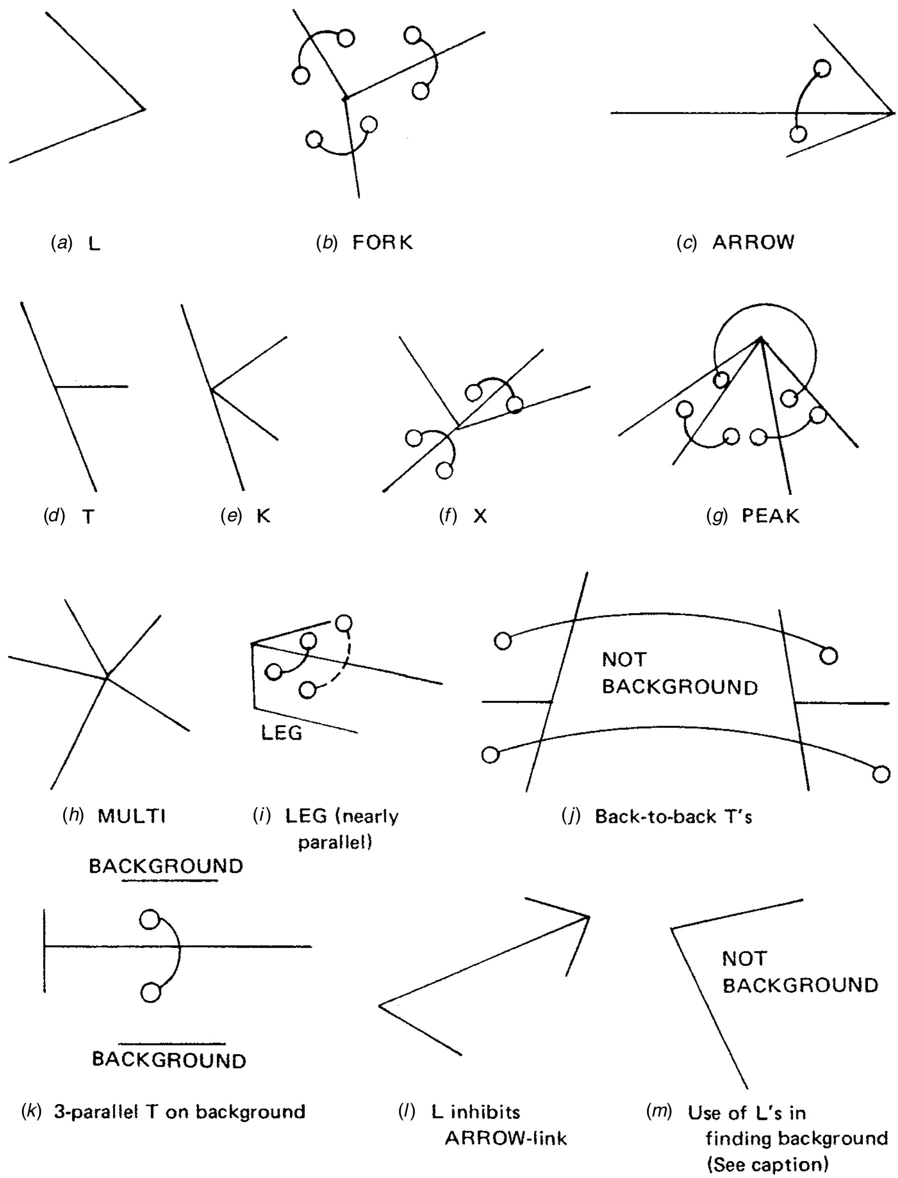


FIG. 10.19. Guzman's vertices and their associated links. Items (*b–c*), (*f–g*), and (*i–k*) show strong region-links. The absence of links in items (*a*), (*d–e*), and (*h*) means that these vertices provide no reliable cues about physicality. Item (*i*) shows a weak link, placed on ARROW by an adjacent LEG. Items (*j–k*) show strong links placed on complex patterns. Item (*l*) shows link-inhibition by an L-vertex (assumes convexity). And (*m*) shows the convexity assumption in the use of Ls to identify the background. Adapted with permission from Guzman (1969: 258; 1968: 87)

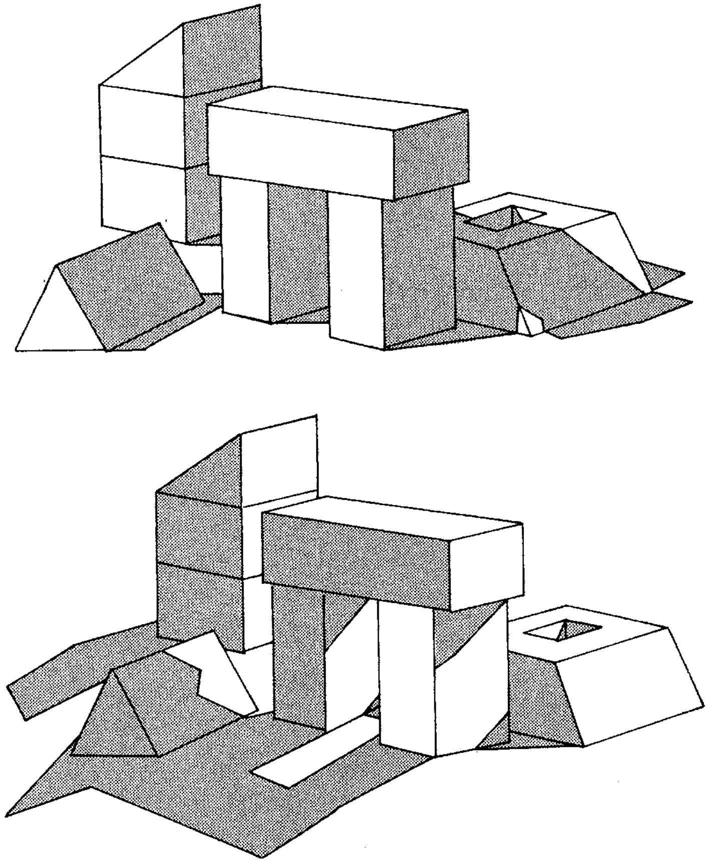


FIG. 10.20. Two differently shadowed images of the same scene. Adapted with permission from Waltz (1975: 20)

It turned out, much to Guzman's puzzlement, that there were some things which SEE *couldn't* see. Holes, for instance. Even without the shadows (which SEE couldn't cope with), Guzman's system wouldn't have recognized the object on the far right of Figure 10.20 as a single thing.

His colleague Winston (1970*b*) was as puzzled by this as Guzman, and tried to extend SEE so as to cope with holes—without success. Nor could SEE have recognized images of impossible objects as impossible (see Figure 10.21). Moreover, even when it did see things correctly Guzman didn't really understand why. He didn't know *why* a fork normally allows one to place region-links across all three lines, nor *why* the few link-inhibition rules were helpful yet not foolproof: see items (*b*) and (*l*) in Figure 10.19. Even more to the point, he didn't ask.

Admittedly, another NewFAI scientist had already produced a program that could articulate an image in terms of distinct 3D objects. Larry Roberts (1937–) had cut his teeth at MIT by trying to speed up the perceptron (J. A. Anderson and Rosenfeld 1998: 100). But he'd turned to scene analysis for his doctoral thesis, written in 1961 but

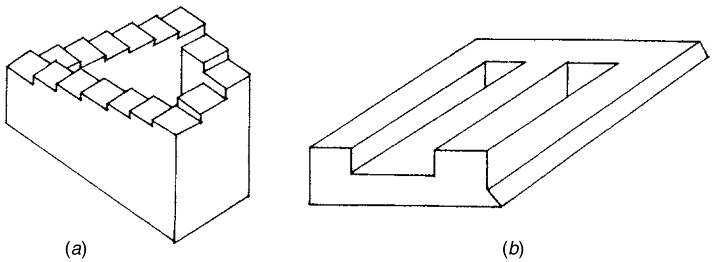


FIG. 10.21. Two impossible objects. Item (*a*) adapted with permission from Meltzer and Michie (1971: 296); item (*b*) reprinted with permission from Clowes (1971: 105)

not officially published until later (Roberts 1963, 1965). Indeed, it was his early 1960s research which initiated the polyhedral blocks world. (He initiated something else, too: appointed head of the information-processing group at ARPA in 1966, he was largely responsible for the creation of the ARPAnet.)

In some ways, Roberts's program was more powerful than SEE. Besides picking out individual objects it could also identify them as cuboids, or wedges, or . . . which SEE couldn't do. In addition, it could discover precise sizes, locations, and surface orientations (by number crunching guided by information about the picture plane). And it could accept camera input. The hope was that some future version, besides being used in robotics, would be able to interpret aerial photographs—like those which had shown the Soviet missiles on Cuba just as Roberts was embarking on his doctorate. But line drawings were what the program was really about, since the grey-scale image was immediately converted into a drawing. (The line-finder looked for edge fragments in four orientations, inspired by David Hubel and Torstein Wiesel's recent discovery of cortical feature-detectors: 14.iv.b.)

However, this system was less general than Guzman's. It relied on internal geometrical models of three types (cuboids, wedges, and hexagonal prisms), each carrying rules about how to interpret specific 2D cues in 3D terms. Anything that couldn't be analysed by applying those rules was, in effect, invisible. In dealing with the line drawing, the program's first move was to find the lines bounding separate *regions*. Using Roberts's list of "approved polygons" (polygons that could depict a face of one of the three basic polyhedra), it then assigned the regions to distinct physical objects. It could also cope with "compound" polyhedra, some of whose faces wouldn't be approved polygons (see Figure 10.22). At base, however, it had to know precisely what it was looking for in order to find it.

Both these scene-analysis programs had a strong affinity with the New Look. For they both went "beyond the information given" (Bruner 1957*a*), by using top-down perceptual hypotheses about the objects they expected to find. But only Roberts identified the theoretical source of those hypotheses, namely projective geometry. Guzman, by contrast, based his interpretative heuristics in intuition. He didn't ask *why* they worked, *why* a corner of a certain type would appear in the drawings as a vertex of a certain type.

Indeed, he didn't distinguish clearly between "corner" and "vertex" in the first place (and sometimes used these terms interchangeably). The former was a denizen of the

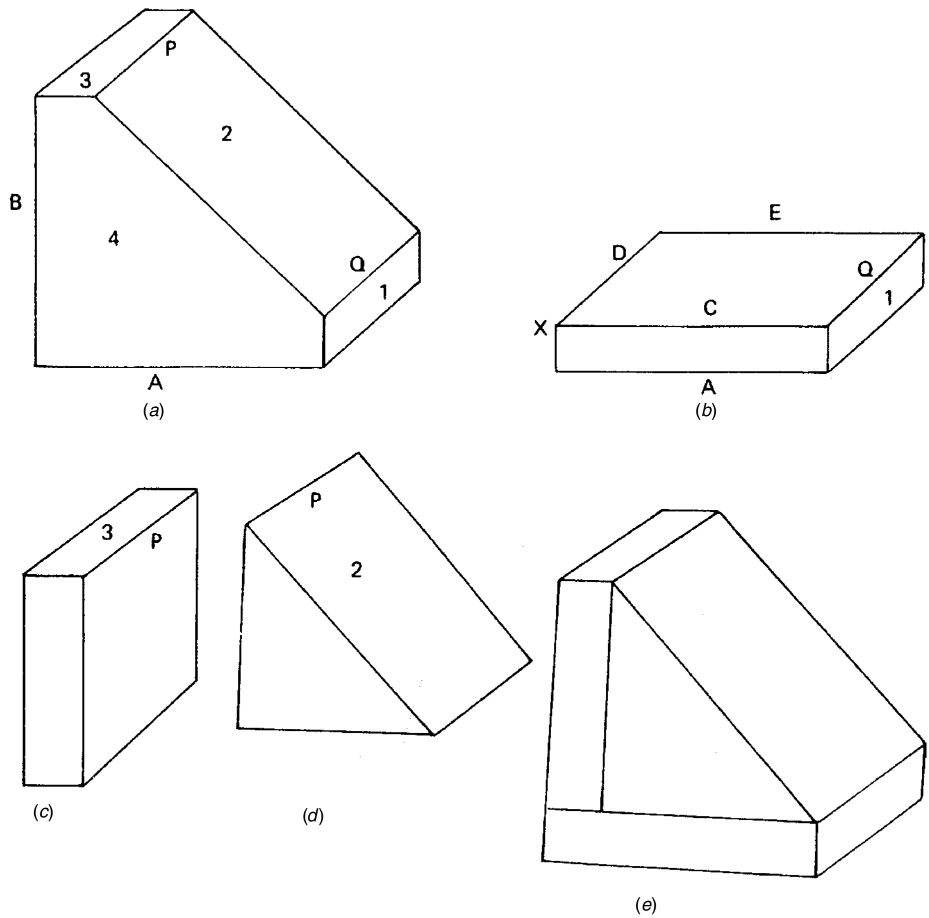


FIG. 10.22. Item (*a*) represents the information passed by Roberts's line finder to his picture interpreter, which searched for significant picture fragments (suggesting "approved polygons"). It found the polygon bounding region 1 with line A connected to it, and constructed and analysed out solid (*b*) accordingly. Similarly, (*c*) and then (*d*) were found. No picture lines remained unaccounted for. The program's final reconstruction of the target compound object is shown in (*e*). Reprinted with permission from Roberts (1965: 182)

scene, the latter of the image. But that was never clearly stated, because Guzman didn't explicitly consider (as Roberts had done) the geometry of the 3D-to-2D projection involved. Instead, he implicitly embedded some of his unorganized spatial intuitions in the region-linking rules. SEE's successes were due to the nuggets of geometrical gold buried inside those intuitions, and to the (atheoretical) improvements that resulted from trial and error with the program.

In the 1960s, it wasn't only Guzman who confused image and scene. Most NewFAI people did. That's why Winston's (1970*b*) treatise on holes added piecemeal procedural patches to SEE, instead of asking systematic questions about 3D/2D representation. It was left to two other people to do that.

One was Clowes (1933–81), by then at the University of Sussex. The other was the mathematician David Huffman (1925–99), who'd recently left MIT to found the Computer Science Department at UC Santa Cruz. Both Clowes (1971) and Huffman (1971), independently, suggested labelling each line according to its *physically possible* interpretations, and then finding the set of *mutually consistent* interpretations across the image as a whole. Clowes recommended breadth-first search to do this, Huffman depth-first search—but their core ideas were so similar that people soon spoke of "Clowes–Huffman labelling". Huffman's labels were the ones most widely borrowed (for his improved version, see Huffman 1977*a*,*b*). However, Clowes's paper was the more interesting for AI, because he reported a scene-analysis program showing how the shared theoretical insights could actually be used.

Clowes and Huffman pointed out that there are four possible interpretations of any line in pictures of polyhedra: either a convex or a concave edge with both associated body surfaces visible, or a "hiding" edge that obscures a surface to one or other side of it. Figure 10.23 shows these four edges, labelled respectively by plus and minus signs and arrows (the visible surface lies to the right of an ant crawling in the direction shown by the arrow). However, no line can be labelled in different ways at either end, because no (non-curved) edge can be convex at one point and concave at another, nor hiding an adjacent surface at one end but not at the other. This allows for tests of mutual consistency between line labels, given that vertices can be sensibly interpreted in only a few ways.

Clowes had originally been puzzled about why SEE worked at all, not only why it sometimes didn't (personal communication). That puzzlement was now solved. He and Huffman had distinguished clearly between image and scene, and analysed the possible relations between them in an orthodox 3D/2D projection. They'd shown why Guzman's "arrow" was so useful: the shaft *must* show an edge, convex or concave, with both adjacent surfaces visible and both belonging to the same object. And they'd shown why his "Ls" were so unhelpful: there are six possible line–label combinations for an L, so Ls have no reliable region-linking rules—see item (*a*) of Figure 10.19. They'd explained why SEE had been defeated by holes, and even by concavities, and they'd offered ways of recognizing these physical features. And both had shown that their methods (for Clowes, a program; for Huffman, a paper-and-pencil algorithm) could find *all possible* picture labellings—which meant that they could also identify *impossible* labellings, and even locate the impossibilities at specific picture points. (Guzman had instructed SEE

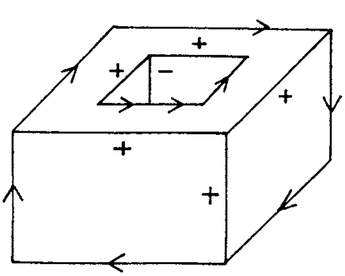


FIG. 10.23. The four physically possible line labels (see text). Reprinted with permission from Huffman (1971: 305)

to reject "illegal" pictures, but his criteria of illegality—e.g. lines not forming part of the boundary of a closed region—weren't properly understood. An illegal picture isn't at all the same thing as an impossible object.)

Two important additions to the domain knowledge used in scene analysis followed in the mid-1970s. One was Mackworth's (1973) gradient space. This was an extension of projective geometry that enabled vision programs to interpret some images that had previously defeated them: namely, drawings of polyhedra with skewed surfaces (see Figure 10.24). Also, by including explicit constraints relating the *relative slopes* of surfaces to their visibility or invisibility, Mackworth explained why Clowes–Huffman labelling worked. This is historically interesting not least because David Marr, despite his scathing criticisms of scene analysis, would later borrow gradient space for use in his own work (Marr 1982: 17, 240–3; see Chapter 7.v.b–d).

The other addition to visual knowledge was David Waltz's (1975) treatment of shadows and cracks. Building on Clowes–Huffman and Mackworth, he allowed lines to represent the boundaries of shadows as well as of objects, and to depict several types of "crack" between adjacent, but separable, objects (e.g. the lines separating the three stacked objects on the left of Figure 10.20). His program could interpret images such as those in Figure 10.20, and could also recognize that they represent one and the same scene.

Even before Waltz's thesis was officially published, further domain knowledge had been added by others. For instance, Kenneth Turner (1974), of the FREDDY team in Edinburgh, extended Waltz's program to include certain classes of curve: see Figure 10.25. (Turner's program started out with a TV image, which it converted to a line drawing, and used hierarchical visual models of the objects that FREDDY might be expected to encounter: see Figure 10.26.)

However, Waltz had done much more than add extra domain knowledge. He'd provided a general method—for coping with multiple constraints—which could be applied in all areas of AI, from resolution theorem proving to connectionist pattern recognition. So today, what people remember best about his early 1970s work isn't the shadows, but the filtering algorithm.

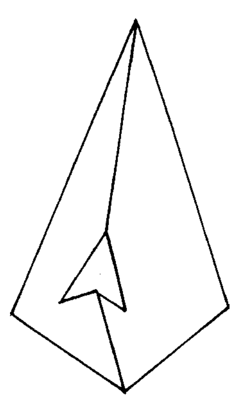


FIG. 10.24. An object with a skewed surface. Reprinted with permission from Mackworth (1973: 134)

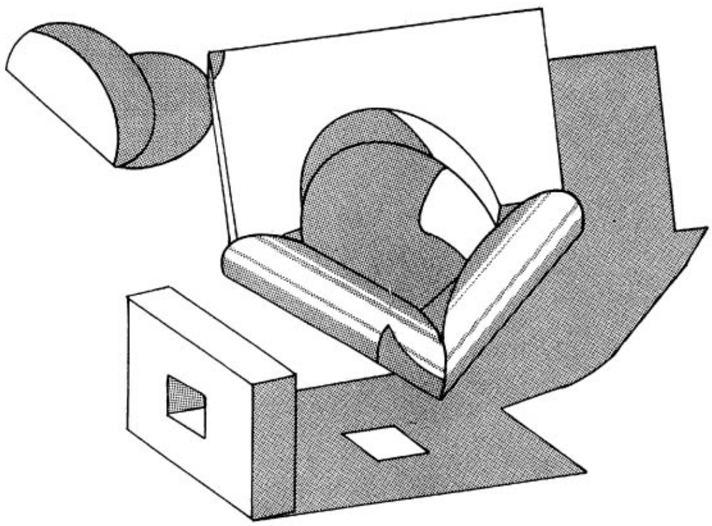


Fig. 10.25. Image of scene with curved (and shadowed) objects. Reprinted with permission from K. J. Turner (1974: 246)

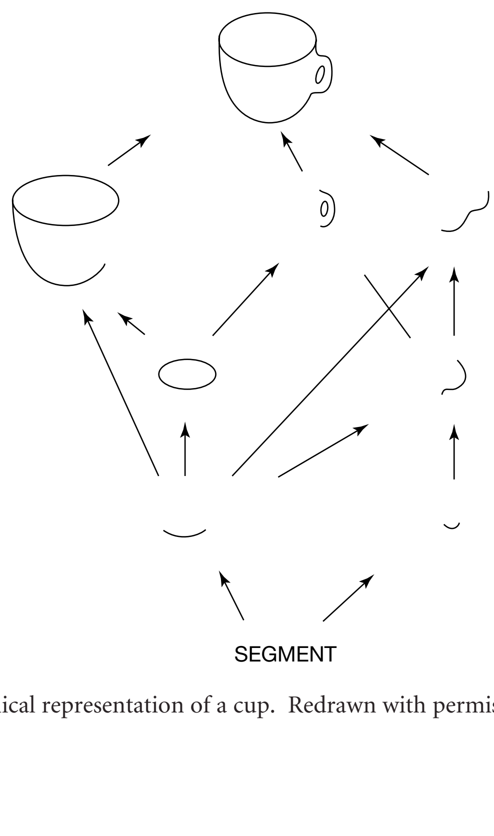


Fig. 10.26. A hierarchical representation of a cup. Redrawn with permission from K. J. Turner (1974: 244)

Waltz-filtering was a technique developed to overcome the combinatorial explosion when there were many possible interpretations of a single cue (in this case, of a single line). Because Waltz allowed for shadows and cracks, his program had eleven different line labels, as compared with the Clowes–Huffman four. This multiplied the possible vertex interpretations horrendously.

There were 500 sensible interpretations of FORK and T, and 100 each for MULTI, X, and K; ARROW and L both had about 75. Only PEAK was relatively manageable, with merely 10. (Things would have been even worse if Waltz hadn't used his *physical knowledge* about shadows, cracks, and separable edges to exclude impossible interpretations: there were no fewer than 6 million combinations for the "relatively manageable" PEAK.) This meant that provisionally assigning all possible labels to every line (as in the Clowes–Huffman approach), or even to every vertex, wasn't feasible.

Instead, his program chose a pair of neighbouring vertices at random, and provisionally assigned all—and only—the *mutually consistent* line labels to them. Then, it crawled along to the next vertex, and assigned *only those labels which were consistent with the ones already assigned*. To his astonishment, this simple iterative procedure, when applied to simple scenes, found a unique label for every line. That was also true for many complicated scenes. Even when it wasn't, the numbers of "possible" labels remaining were so small that the familiar Clowes–Huffman approach (comparing labels across the whole image) could be used to disambiguate them.

The overall interpretation was speeded up still further when Waltz decided to make the initial choice of vertex only *semi*-random. In doing that, he relied—again—on specific domain knowledge. Given that some vertices have fewer sensible interpretations than others, Waltz ensured that two relatively unprolific vertices were "randomly" chosen. Also, reminding his readers that one usually attacks a jigsaw puzzle by finding the edge-pieces first, he made the program pick vertices that probably contained *object-boundary* lines. (So PEAKS were shunned at this stage, because only two of the ten PEAK interpretations make sense on the scene–background boundary.)

His AI colleagues were quick to get the point:

> Waltz' work on understanding scenes surprised everyone. Previously it was believed that only a program with a complicated control structure and lots of explicit reasoning power could hope to analyze complicated scenes. Now we know that understanding the constraints the real world imposes at junctions is enough to make things much simpler . . . It is just a matter of executing a very simple constraint-dependent iterative process that successively throws away incompatible line arrangement combinations. (Winston 1977: 227)

Waltz-filtering, like gradient space, was another legacy of scene analysis which would be utilized by Marr, despite his harsh words about GOFAI computer vision. It was used by many others too, for it wasn't specific to vision. Any domain involving multiple constraints, and where some local interpretations were mutually inconsistent, could be tackled more efficiently by means of it.

There was a major problem, however. Namely, a single mistake in the Waltz filter's depth-first search might cut off the very branch of the search tree on which the correct solution lay. Geoffrey Hinton (1976, 1977) referred to this as "computational gangrene". As we'll see in Chapter 12.v.h, he formulated an improved constraint-filtering strategy, called "relaxation" (the term was borrowed from other work on scene analysis: Rosenfeld *et al.* 1976). Relaxation found the best overall interpretation *even though* this might contain some minor inconsistencies. In other words, the gangrene was prevented.

Initially, Hinton's ideas were developed in the context of a GOFAI vision system, the Sussex POPEYE project (see below). Soon, however, he (and others) would apply them in PDP models. Today, multiple constraint satisfaction by relaxation is a key pillar of connectionism. But the pillar's base, and even its name, lies in good old-fashioned scene analysis.

Good old-fashioned scene analysis was more varied than I've suggested so far. For instance, the SRI approach differed from MIT's: Jay Tenenbaum's group, which included the young Harry Barrow (1943–), started from photos rather than drawings and from regions rather than lines (Tenenbaum *et al.* 1974; Tenenbaum and Weyl 1975; Tenenbaum and Barrow 1976). Their program, with helpful input from the programmer, wasn't restricted to the blocks world but learnt to recognize real-world objects such as telephones. There were some other (non-interactive) early 1970s programs that started from regions rather than lines (Brice and Fennema 1970; Barrow and Popplestone 1971; Yakimovsky and Feldman 1973). But one of the most interesting examples of 1970s computer vision, albeit not widely noticed at the time, differed even more from MIT's blocks-world paradigm.

This was the POPEYE project, directed by Sloman at the University of Sussex (Sloman 1978, ch. 9). POPEYE modelled the interpretation not of fully connected drawings of perfect polyhedra (or even polyhedra-with-shadows), but of highly ambiguous, noisy, input—with both missing and spurious parts: see Figure 10.27. And it simulated the *complexity* of perception to an extent that was unusual at the time.

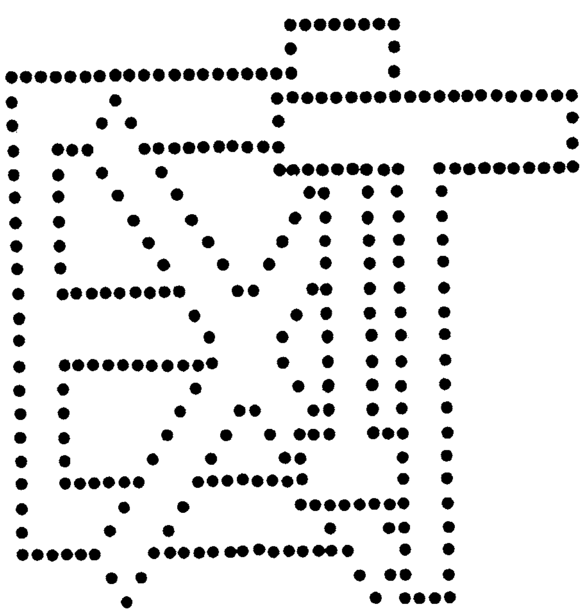


Fig. 10.27. Noisy visual input. Reprinted with permission from Sloman (1978: 219)

POPEYE's complexity had several roots. Many different sorts of background knowledge were combined in the one program. In addition, processing could occur concurrently in different domains, determining which sub-processes would dominate the scarce computational resources. This was different from the heterarchy so popular in the early to mid-1970s. In SHRDLU, for example, there was only one locus of control at any moment; and control was transferred to process X by an explicit call from process Y—as when the syntactic procedure for interpreting the word *the* passed control to a perceptual subroutine, to see whether there was *one and only one* thing fitting the predicate concerned. Each knowledge domain in POPEYE had its own priorities for finding/processing information, and these priorities could change suddenly as a result of new information arriving unexpectedly. Complexity (and flexibility) was increased also by the fact that some of the internal representations constructed by POPEYE were temporary, rather than provisional. (Something provisional may become permanent, but something temporary should not.) At the point when its funding was cut, the POPEYE program was about to be turned into a hybrid system (12.ix.b). A neural net was being designed (by Hinton) to replace the GOFAI spell-checker included in POPEYE. The idea was that the network would be trained on a collection of known words, and would then suggest the most likely word when presented with a "half-baked" letter sequence (A. Sloman, personal communication).

Considered as a practical visual system for a robot, POPEYE wasn't at all impressive. But Sloman wasn't trying to advance robotics, least of all robotics confined to toy polyhedral worlds. Rather, he was trying to advance Immanuel Kant's argument that the mind must provide some prior knowledge for even the "simplest" perceptions to be possible. Kant had talked about the mind's comparing representations, combining and separating them . . . and so on, but "What Kant failed to do was describe such processes in detail" (Sloman 1978: 230).

Whereas for Kant the principles of organization were very general, and innate (see Chapters 2.vi.a and 9.ii.c), for Sloman they also included highly specific examples. Citing recent work on how we avoid word-by-word parsing of commonly heard phrases and on how we learn visual "phrases" too (Becker 1973, 1975), and referring also to Bartlett (see 5.ii.b), he pointed out that visual schemata can aid recognition enormously.

For instance, the familiar upper-case sign "EXIT" helps us—and POPEYE—to recognize the four letters in Figure 10.27. Likewise, a head, or leg, in a photo of a human being not only speeds up our interpretation of that picture-part but also guides our recognition of other parts. In both cases, the computational complexity has a chicken-and-egg aspect: if one recognizes a particular set of dots in Figure 10.27 as colinear, that can help one to recognize an "E"; but if one has already recognized "EXIT", one will be much more likely to recognize *those very dots* as colinear (Sloman 1978: 228–32). POPEYE was an attempt to move towards a realistic degree of visual complexity. To do that, it needed to be fairly complex itself:

> Our program uses knowledge about many different kinds of objects and relationships, and runs several different sorts of processes in parallel, so that "high-level" processes and (relatively) "low-level" processes can help one another resolve ambiguities and reduce the amount of searching for consistent interpretations. It is also possible to suspend processes which are no longer useful, for example low-level analysis processes, looking for evidence of lines, may be terminated prematurely if some higher-level process has decided that enough has been learnt

about the image to generate a useful interpretation. This corresponds to the fact that we may recognize a whole (e.g. a word) without taking in all of its parts. (Sloman 1978: 229)

One reason why POPEYE had relatively little influence on AI vision research is that, in effect, it was killed in about 1978. By that time, Marr's exclusively bottom-up approach to vision had become popular with the referees advising the grant-awarding bodies, and POPEYE was marked down accordingly. Sloman's account of what happened is given in the historical note added to the online version of his 1978 book. (One of the many factors he mentions is the widespread move from AI languages such as LISP or POP-11 to more general, more efficient, languages such as Pascal or C/C++. These make it very difficult to express "complex operations involving structural descriptions, pattern matching and searching", and to permit "task-specific syntactic extensions . . . which allow the features of different problems to be expressed in different formalisms within the same larger program"—see Section v, below.)

However, POPEYE wasn't wholly forgotten. Quite apart from Sloman's own later work, it was cited by the neuroscientists who recently caused a sensation by positing two visual pathways in the brain—one for perception, the other for action (Goodale and Milner 1992; Milner and Goodale 1993: cf. Chapter 14.x.c). The dorsal pathway apparently locates an object in space relative to the viewer, who can then grasp it; the ventral pathway may (this point is contested) locate it relative to other objects, and enables the viewer to recognize it. The evidence lies partly in brain-scanning experiments with normal people, and partly in clinical cases: damage to these brain areas leads to visual ataxia and visual agnosia, respectively. For example, one patient can recognize an envelope but is unable to post it through a slot, whereas another can post it efficiently but can't say what it is.

These 1990s researchers, too, asked a host of questions about how the different pathways can be integrated in various circumstances. In Sloman's view (personal communication), even they didn't really get the point—which was that there are many visual pathways, not just two. Of course, he was talking about computation, not neuroscience: there may or may not be distinct neuranatomical pathways related to distinct types of function. (In low-level vision, it appears that there are: 14.i.c.) But the moral of POPEYE, as of Sloman's more recent visual work (1989), was that *many* types of background knowledge play their parts *concurrently* in perception.

In a sense, POPEYE wasn't really about vision. Rather, it was an exercise in architecture building. For Sloman saw computer vision as a way of keying into the computational architecture of the mind as a whole. (More accurately: as a way of keying into the space of all possible minds—1978, ch. 6.) Over the years, he would focus increasingly on the control structure of the entire mind, and—after a long period of relative neglect—his work on that topic is now attracting significant attention (Chapter 7.i.f).

A final word: GOFAI vision and robotics were typically pursued in terms of the Cartesian sensori-motor sandwich (see 2.iii.a). MIT's early Mars robot was a rare exception—and even there, the sandwich was ignored rather than challenged (13.iv.a). The slices of bread, one white (the sensory input) and the other brown (the motor output), enclosed the meat at the centre: vertices, line labels, models of "EXIT" . . . in other words, mental representations linked by theorem proving, planning, and other forms of inference. Marrian research, which eclipsed scene analysis in the 1980s

(7.v.b–d), fed on the same sandwich, for Marr too treated perception and motor action as separate aspects of intelligence.

This didn't sit well, for instance, with the finding that only *self-activated* locomotion helps kittens to develop visual discrimination (Held and Hein 1963), for that fact suggested a much more intimate relation between sight and movement. And it ran counter to the approach of the two mid-century Williams, namely Ross Ashby and Grey Walter (4.viii). But they were (temporarily) out of fashion. Although computer vision had become increasingly enactive, utilizing the changes in input caused by the movements of the system/camera itself (including 'foveal' focusing and continual 'saccades'), internal representations—and the action–perception split—had endured. Not until the "situated" and "dynamical" roboticists of the late 1980s (13.iii.b–c) would AI seriously question the sensori-motor sandwich.

c. Expert Systems

Expert systems showed the 1970s "need for knowledge" especially clearly. For these AI programs embodied highly specialist knowledge gleaned from human professionals. The first examples were written in the late 1960s, and more appeared in the 1970s (Michie 1979). The target areas were very strictly confined, and were originally drawn from science, medicine, or science-related businesses such as computer manufacture or oil prospecting. (Soon afterwards, law was tackled too: see 13.ii.c.)

Developed for *practical* use, the early expert systems were then the prime example of technological AI. As remarked in Chapter 1.ii.b however, technological AI is often based in psychological insights. Expert systems were intended to capture the knowledge and reasoning processes of human experts. In the 1970s, moreover, the venture capitalists hadn't yet moved in on the field. Lacking strong commercial pressures, expert-system builders still aimed, in part, to investigate intelligence as such. So at the close of the decade the co-directors of MIT's AI Lab wrote that "*most* of the people doing artificial intelligence", besides trying to make computers more useful, hoped to formulate theories applicable to "any intelligent information processor, whether biological or solid state" (Winston and Brady 1979, p. ix; italics added).

Throughout the 1970s, AI scientists developed what Feigenbaum (1977) dubbed knowledge engineering, defined as "The art of designing and building expert systems and other knowledge-based programs". This included the "art" of coaxing the real experts to make their human expertise explicit, so that the programmer could program it. As soon became clear, much of the expert's knowledge wasn't available as facts/theories in the domain textbooks. Rather, it consisted in informal heuristics developed over the years, rarely verbalized and almost never communicated. All agreed that making this knowledge explicit wasn't going to be easy—and some critics doubted whether it's even possible (see 13.ii.b).

In general, knowledge engineering required special methods of questioning (analogous to the "protocol analysis" of the Gestalt psychologists and LT/GPS authors), developed by trial and error over the early years (R. Davis 1976/1982). Sometimes, the questioning would be done by human beings. But sometimes, it was done by a program: an expert system whose expertise lay in knowing *how* another expert system (e.g. MYCIN) represents its domain knowledge, and *how* it reasons about it. Using this "meta-knowledge", the automatic knowledge engineer would be able, for instance, to criticize the format of a new item of knowledge added by the non-specialist user—and might also suggest a way out of the difficulty.

AI was notionally aimed at expert systems right from the start. Licklider (1988) had contributed to AI so generously, both intellectually and (via ARPA) financially, because he hoped for military and civilian applications—many of which would assist human experts, or occasionally replace them (see 11.i.b). NewFAI's search for generality, however, implied that expert systems weren't its prime concern. So even MIT's Project MAC, an ambitious (ARPA-backed) 1960s programme of mathematical research, was generally regarded more as NewFAI's close cousin than as its twin. But as NewFAI matured into GOFAI, AI projects focused increasingly on specific practical domains.

That focus wasn't due merely to people wanting to be useful and/or rich. Some were disillusioned with the failure of generalist programs to progress beyond the first, exhilarating, days. And some felt that the intellectual challenge would be greater as a result.

Feigenbaum, for instance, embarked on AI as blue-sky research:

> At that time, [as a student] in the fifties, I didn't think of practical applications at all. I was intrigued by the vision of a highly intelligent, maybe super-intelligent, artifact. (interviewed in Shasha and Lazere 1995: 213)

Some ten or fifteen years later, when visiting Newell and Simon at CMU in the late 1960s, he still had that vision. But now, he put it like this:

> You people are working on toy problems. Chess and logic are toy problems. If you solve them, you'll have solved toy problems. And that's all you'll have done. Get out into the real world and solve real-world problems. (Feigenbaum and McCorduck 1983: 62)

Naturally, Feigenbaum felt that this finger of scorn couldn't be pointed at himself. For he'd arrived to give his talk at CMU carrying the defensive shield of DENDRAL on his arm.

DENDRAL (the name came from the Greek for *tree*) was the first expert system. It was developed at Stanford by Feigenbaum with Bruce Buchanan (1940–), Georgia Sutherland, and the Nobel geneticist Joshua Lederberg (1925–). Soon after the start, they were joined by the chemist Carl Djerassi, who acted as the domain specialist. It was begun in the mid-1960s, as a result of conversations between Feigenbaum and Lederberg.

Lederberg had been sympathetic to AI for some years (Lederberg 1987: 17–19). He'd been captured by Minsky's infectious enthusiasm, had read his 'Steps Toward Artificial Intelligence' on its official publication in 1961, and had been impressed when Minsky showed him the interactive possibilities in Spacewar. He'd even tried to persuade Minsky to move to Stanford (the Medical School), in McCarthy's wake. He could use a computer, and had done some computer-based medical work—on genetic epidemiology. However, that was merely computerized card shuffling. (Often, using appallingly low-quality data—which implied, for instance, that some mothers give birth at three-month intervals.) He was already convinced that "computers [and especially interactive computers] were going to change the whole style of scientific investigation". Specifically, by 1963 he was hoping to use computers to study chemical structures. But he realized full well that "This was not going to happen with card deck data entry."

In short, when Feigenbaum first met Lederberg—introduced by Karl Pribram in 1963, when *Computers and Thought* was hot off the press (Lederberg treasures his signed copy)—and communicated his youthful hopes to the older man, he was pushing at an open door. The graduate student Feigenbaum wanted a tractable AI problem to work on, dealing with what he called "empirical induction in science" (Feigenbaum and Watson 1965). The Nobel prizewinner Lederberg had a suitable domain already in mind. He'd even sketched a structural taxonomy of organic molecules, and had outlined some problem-solving procedures. But he had "no idea how one would go about translating these structural representations into a computer program" (1987: 22). In 1965, when Feigenbaum arrived at Stanford, they agreed to collaborate on a specific chemical problem chosen by Lederberg—and the result was DENDRAL (so called, because it generated *trees* of candidate structures).

The DENDRAL program was completed by the end of the decade, when it was presented alongside "Some Philosophical Problems" at the fourth Machine Intelligence Workshop (Buchanan *et al.* 1969*a*). More accurately, the *first* completed program was presented there. Increasingly powerful versions soon appeared, and were described at the next three Edinburgh Workshops (Buchanan *et al.* 1969*b*, 1972; Feigenbaum *et al.* 1971; Buchanan and Sridharan 1973). The most interesting improvement was meta-DENDRAL, which was under development from 1970 to 1976 (Buchanan and Mitchell 1978). Besides learning domain-specific rules inductively, this could propose historically new rules itself, which could be included alongside the ones it had been given (so it was one of the few early "creative" programs—see Boden 1977: 327–32). The final version of DENDRAL, and a retrospective view, appeared in 1980 (Lindsay *et al.* 1980; see also Barr and Feigenbaum 1982, chs. VII–VIII).

DENDRAL's expertise lay in a particular corner of organic chemistry, concerning the steroids used in contraceptive pills—at that time, still a novelty. Analytical chemists often discover the structure of an unknown compound by using mass spectroscopy, in which an electron beam breaks the molecules into fragments. Having identified the fragments (by their spectrographs), chemical theory suggests how they could fit together in a whole molecule. DENDRAL was designed to do the same sort of thing, by combining chemical knowledge with AI techniques of heuristic search. A "toy" problem, this was not. Nevertheless, its performance compared well with that of human chemists.

Chemical theory played a large part in DENDRAL's success. The program could formulate probable hypotheses about an unknown compound's molecular structure (on the basis of its spectrograph), and then test these hypotheses by way of further predictions. Besides analysing specific molecules, it came up with inductive hypotheses based on the data (e.g. "IF the graph of the molecule contains the estrogen skeleton, THEN break the intramolecular bonds between nodes 13–17 and 14–15"). It didn't stop there: it selected the most likely hypotheses, by reference partly to chemical considerations (such as the number of bonds assumed broken) and partly to logical features such as simplicity, uniqueness, and evidential strength. Moreover, DENDRAL provided, for the first time, a complete list of the set of possible isomers of a given empirical formula within several families, including amines and thioethers.

As for meta-DENDRAL, this discovered mass spectrum fragmentation rules for several types of compound (such as aromatic acids) which hadn't previously been recognized by expert chemists. It did this by first seeking regular patterns in the experimental data, and then applying known concepts to see whether it could find any explanation of them. So it would ask, in effect, "What's so special about nodes 13–17 and 14–15? Why do the intramolecular bonds break there, rather than elsewhere?" Its answer would be couched in terms of some smaller, more general (sub-molecular) structure in the immediate environment of the broken bonds. The latest version could even accept mixed data drawn from several different molecular structures, and separate out the different sub-groups while finding a characteristic explanation for each.

A final advantage, for chemistry, of the DENDRAL research was that the process of extracting the human expert's knowledge led to some new insights. For example, one of the non-chemists involved remarked that it seemed to him that amines are something like ethers. That analogy hadn't ever occurred to the chemists themselves, but they agreed that there was an interesting similarity. This was a special case of the general point—which was becoming more evident with every NewFAI year that passed—that trying to express a theory (in chemistry, or psychology . . . whatever) clearly enough for it to be programmed can lead to new insights, both before and after the computer is plugged into the wall. That general point was illustrated also by the new psychological insights about expert reasoning *as such* which sprang from these exceptionally intensive interviews.

In short, DENDRAL was fulfilling Lovelace's prophesy that some future Analytical Engine might help scientists to "adequately express the great facts of the natural world" (3.iv.b). It was doing this, at the level of the virtual machine, not by what she'd called "mathematical" (algebraic) computation, but by logical-symbolic inference. However, Ada's mentor Babbage had believed that logic and algebra were essentially one. So one can see NewFAI research on KR as an attempt to justify his hunch *in practice.* DENDRAL had apparently done that—and had inched chemical science forward in the process.

Close on the heels of DENDRAL came MYCIN, also from the Stanford stable (Shortliffe *et al.* 1973, 1975; Buchanan and Shortliffe 1984; Cendrowska and Bramer 1984). The core programmers were Buchanan and Edward Shortliffe, who soon qualified in medicine. (One co-author, Stanley Cohen, later developed the basic technology for recombinant DNA.) A production system of 450 rules, MYCIN simulated a medical consultant specializing in the diagnosis and treatment of infectious diseases. Clinicians reported that it performed much better than junior doctors, and as well as some experts—a report that was later confirmed by blind testing (Yu *et al.* 1979).

MYCIN was much more than a table of bugs-and-drugs, for it employed then advanced AI techniques. It engaged in question-and-answer conversations, lasting twenty minutes on average, with doctors needing specialist help. The NLP was of little interest, but it was backed by complex domain reasoning. The physician would ask MYCIN for advice on the identification of micro-organisms, and on the prescription and dosage of antibiotic drugs. That advice would be based on blood tests and histology, and on the patient's symptoms and past medical history. If the relevant evidence hadn't been provided, MYCIN could ask the doctor for it. In addition, MYCIN could explain its advice, at the appropriate level of detail.

Whereas DENDRAL had had a scientific theory (as well as experts' reported intuitions) to go on, MYCIN didn't. Why? Because chemistry does, and this area of medicine doesn't. MYCIN was wholly dependent on rules (heuristics) gleaned from clinical textbooks and from the experts consulted by the knowledge engineers. Also, and again unlike DENDRAL, it had to express various degrees of uncertainty—in a way that reflected how doctors assess the impact of evidence on their diagnosis. So it could present *several* hypotheses to the user, prioritizing them according to the various confidence levels—which, in turn, could be explained.

In brief, this was a user-friendly interface designed for people knowing nothing about computers (cf. 13.v). It conversed in English. It asked questions when it needed to, and in so doing it often reminded the user about things as yet undone. It offered helpful advice, and explanations too (so it could be used as a tool for teaching, as well as clinical practice). It gave estimates of how reliable its advice was likely to be. And it even explained just why it couldn't be sure. Indeed, if providing incomplete medical data counts as "putting the question in wrong", then MYCIN (after requesting the missing data) could do what Babbage's lady visitor had required of the tiny Engine in his salon: have "the answer come out right" nevertheless (3.i.a).

Both DENDRAL and MYCIN were production systems (v.e, below). Feigenbaum and Buchanan had been inspired to use that methodology by their ex-colleague Newell, who'd visited Stanford to lecture about it in 1967. One of the advantages, for expert-system builders, was that each production rule was stated independently. So one didn't need to rewrite the whole program in order to add an *extra* item of knowledge gleaned from the human expert, including his/her explanation of the program's past failures. (This didn't mean, of course, that its interactions with existing rules could be foreseen: the combinatorial explosion was always lying in wait.)

Feigenbaum and Buchanan tried to extract the logical skeleton of the production systems they'd pioneered, so that others could use it to write expert systems in indefinitely many domains. In doing that, they developed EMYCIN: the first expert-systems "shell".

The "E" stood for Empty, for a shell was a content-free inference engine. (The term "inference engine" was coined by Randall Davis, in homage to Babbage—interview in Crevier 1993: 157.) It specified, for example, the basic blackboard architecture, and the method of conflict resolution to be used when more than one production was satisfied. Also, it provided methods for "forward-chaining" and "backward-chaining" inference. The former was used bottom-up to generate conclusions from data, the latter top-down to find evidence for a supposition (and to explain an item of advice by recapitulating the previously fired rules). Several expert-systems shells were being marketed by the close of the 1970s, for use in building new systems for commercial clients.

By the early 1980s, then, expert systems had become a recognized field within AI. At that point, the field was given an added boost by Japan's Fifth Generation project (Chapter 11.v). This prompted several popular publications from AI leaders, such as Feigenbaum's *The Fifth Generation* (1983) and Winston's *The AI Business* (1984). There was a burst of technical discussion, too (e.g. Stefik *et al.* 1982; R. Davis and Lenat 1982; F. Hayes-Roth *et al.* 1983; Merry 1985; Waterman 1985). Optimism reigned.

But there were problems lurking in the undergrowth. Some expert knowledge was highly elusive. And some of that tacit knowledge might elude capture for ever. By 1990, various insightful critiques had highlighted the difficulties, and the need to keep human beings somewhere inside the loop (see Chapter 13.ii.b–c).

10.v. Talking to the Computer

Abstract proofs about universal Turing machines (4.i.c) and universal programming languages are all very well. But in practice, as George Orwell might have put it, some languages are more universal than others. Indeed, a list of epigrams on programming, written by CMU's Alan Perlis (who led the team that designed ALGOL in the late 1950s), includes this salutary warning: "*Beware the Turing tar-pit in which everything is possible but nothing of interest is easy*" (Perlis 1982).

When the first computers were built, it was difficult even for experts to tell them to do anything interesting. For one had to use the machine code (or some very closely associated assembly language). That involved long sequences of 0s and 1s, which human beings find almost unintelligible. So J. Clifford Shaw, the person primarily responsible (aided by Newell) for the *programming* of the Logic Theorist and GPS, remembers:

> It was very painful [in 1954] to try to program anything, to make progress towards a chess-learning machine, because we didn't have an adequate language for communicating. [Newell and I] had done a number of programs in what was essentially machine language . . . but it was far too low-level a language to begin to specify the chess-playing program. *As programmers, we had a creative task each time with trying to invent a representation in the machine corresponding to what we were communicating fairly loosely in English.* (Shaw, interviewed in McCorduck 1979: 141; italics added)

For NewFAI to be practicable, then, it would have to escape the shackles of the binary code. To do that, it relied on three Ls: List processing, Logic, and LOGO. Each of these enabled people to avoid thinking of computers only as shufflers of 0 and 1, even though that's what they are at base.

List processing was the first of the three to rule the NewFAI roost, in the mid-1950s. Logic programming became prominent twenty years later. As for LOGO, this was less widely used by AI researchers. But it was much more interesting to other people. It wasn't merely a programming language, but embodied a specific psychology of thinking, learning, and teaching.

a. Psychology outlaws binary

There's a reason why binary code is near-unintelligible: namely, the "magical number seven" (Chapter 6.i.b). It's because of that limitation on human memory that computer science as we know it, NewFAI included, couldn't have got off the ground without programming languages.

When the psychologist Miller first identified the "magical number" he pointed out that it can be cheated by chunking. He also pointed out that since there can be no more than about seven chunks, we regularly compose larger chunks out of smaller chunks . . . on indefinitely many levels. Programming languages are chunkers: one term in the high-level language is defined by many in the machine code. That's why programmers can write complex software using these languages which they couldn't have hoped to write without them.

The need for programming languages had already become clear in practice even as Miller was writing his paper (published in 1956). And his colleague Licklider was well aware of this. In his famous call to arms on enabling "Man–Machine Symbiosis"

(11.i.b), he listed them as an essential prerequisite for that happy state of affairs (1960: 4). The lack of powerful programming languages, he said, was "the most serious obstacle to true symbiosis".

Why was Licklider so concerned? After all, by the mid- to late 1950s a few such languages were already available. The most widely used were IBM's FORTRAN (the acronym came from FORmula TRANslation); the algebra-like ALGOL, designed in Germany with some input from Zuse; and COBOL, created by Grace Hopper (who'd also written the first compiler, in 1952).

However, these weren't much use to NewFAI. Programming languages are like traps, designed to catch different animals in different ways: they store and process information differently, and are best suited to different computational tasks. FORTRAN and ALGOL had been custom-made (for scientists and engineers) to represent mathematical formulae or number arrays, not to model hierarchical problem solving or associative thought. (Weizenbaum used FORTRAN to program ELIZA, as we've seen: but he didn't try to *do* anything with it, besides superficial template matching.) Similarly, COBOL was designed largely to help businessmen and accountants to do their sums. But AI was focused on symbol processing *in general*, not just numbers. (Zuse's Plankalkul was non-numerical, but was still unknown: see subsection f.)

Since AI required new types of task to be accomplished, the AI community had to create new ways of telling computers what to do. That explains Licklider's concern, for he'd been closely involved with the NewFAI pioneers right from the start. It also explains why Minsky could justify the "slow" rate of progress in 1950s AI by saying that "Much of [our] time has been spent on the development of programming languages and systems suitable for the symbol-manipulation processes involved" (1961*c*: 215).

The design of such languages depended on three things: (1) how people find it easiest to think; (2) what NewFAI folk wanted to think about; and (3) how the new language could be implemented.

The last question was crucial. Someone can define a formal language and then use it 'intuitively'. That's what Bertrand Russell did when he proved theorems in his propositional and predicate calculi (Chapter 4.iii.c), and what Chomsky did when he applied his transformational grammar to English sentences (Chapter 9.vi). But to enable a machine to use the language, someone has to show how to represent the newly defined chunks in the computer's memory, and how to translate them into machine code so that the program can actually be run.

The same applies to the development of user-friendly computer interfaces (Section i.h above and Chapter 13.v). As Engelbart put it, in discussing the desirability of word-processing:

> [The] internal structure [of symbols in the computer for words, phrases, sentences, paragraphs, cutting, pasting, copying, etc.] may have a form that is nearly incomprehensible to the direct inspection of a human (except in minute chunks).
>
> But let the human specify to the instrument his particular conceptual need of the moment, relative to this internal image. Without disrupting its own internal reference structure in the slightest, the computer will effectively stretch, bend, fold, extract, and cut as it may need in order to assemble an internal structure that is its response, structured in its own internal way . . . [It] portrays to the human via its display [i.e. interface] a symbol structure designed for *his* quick

and accurate perception and comprehension of the conceptual matter pertinent to his internally composed substructure. (Engelbart 1962: 87)

These psychological facts of life affected the development of NewFAI very deeply. It required special programming languages right from the start, and couldn't have matured into GOFAI without continual (and still continuing) improvements in *how* to tell a computer what to do. Deciding *what* to tell it to do, which is what people normally focus on when they think about AI, was therefore just one side of the coin. NewFAI researchers had to originate both the head and the tail.

b. Entering the lists

When Newell died in 1992, the *AI Magazine* printed three full-page pictures of him, including one on the front cover. In addition, they ran a twenty-six-page obituary, a memoir by Simon, and a previously unpublished speech of Newell's—championing AI as a "fairy-story" full of "enchantment"—that he'd given nearly twenty years before his death (Laird and Rosenbloom 1992; Simon 1992; Newell 1976/1992). Fully one-third of the Winter number was given over to him.—Why?

For the obituarists, his long-time SOAR collaborators John Laird and Paul Rosenbloom, his research as a cognitive scientist was paramount (see Chapters 6.iii.b–c and 7.iv.b). The nature of mind, they said, was "the ultimate question", which they hoped wouldn't be lost in AI's rush to money-spinning commercial applications (p. 41).

Newell's own theory of mind was the paradigm of GOFAI, even including the claim of strong AI (Chapter 16.v.c and ix.b). His SOAR colleagues made that very clear. They didn't mention the fact that he'd encouraged other cognitive science approaches, too. The connectionist Hinton, recalling Newell's offering him a job at CMU, has said:

[Newell] was very eclectic. He was very broadminded. He realized that sooner or later there was going to be a connection between what went on in the mind and what went on in the brain . . . [He] was in favor of having people do all sorts of things [at CMU], so he was basically in favor of having someone who worked on neural nets there. He could see it coming back into fashion . . .

I was very impressed by the fact that Newell was open to getting somebody in an area that he didn't believe in. It's very rare to see that in academics. (J. A. Anderson and Rosenfeld 1998: 375)

But if the obituarists didn't praise Newell's eclecticism, they did laud his work on programming languages—which enabled him, and many others, to ask that "ultimate question" in computational terms. By inventing new programming languages, Newell—with Simon—revolutionized the potential of AI. One might even say that they *made AI possible*. For in the mid-1950s, they implemented the first list-processing language.

I say "they", but the younger man was the senior in terms of computing expertise. Most of the technical details were worked out by Newell, although the overall rationale was developed jointly. (The same was true of their second revolution in programming, namely, production systems: subsection e, below.) Presumably that's why Newell, not Simon, was elected as AAAI's first President. (His vision for AAAI was recently reprinted in their twenty-fifth anniversary number, and extensively quoted in the opening paper: Newell 1980/2005; Reddy 2005: 9.)

List processing enabled computers to handle problems of greater hierarchical complexity than had been possible before. And it enabled them to be more than mere number-crunchers. That is, it supported what Minsky (1968) termed "semantic" information processing, concerning matters normally expressed not in numerals but in logical symbols or even natural language.

In principle, of course, list-processing languages weren't necessary. Any universal programming language, in theory, can support any computation (Chapter 4.i.d). Indeed, Colby later switched from a Newell–Simon list processor to ALGOL (designed to represent numerical arrays) when writing the later versions of his neurotic program (7.i.a). What's possible in principle, however (the Turing tar-pit), may be very different from what's feasible in practice. And in practice, list processing was a huge advance.

Its ability to represent hierarchy, a prime feature of human thought and action, made it both easier to work with during the programming process and more apt for modelling thought. The second point was especially important to Newell and Simon, who were psychologists before they were computer scientists (Chapter 6.iii). When they first designed new languages for AI, they weren't doing an abstract exercise in computer science. Rather, they were trying to get the Logic Theorist off the ground (Newell and Simon 1956*b*; Newell and Shaw 1957).

Their initial attempt at going beyond machine code was the "Logic Language", which they used for paper-and-pencil simulations (Newell and Simon 1956*a*). They soon buckled down to implementation, however, with the help of a RAND colleague: the professional computer scientist Shaw. Through the late 1950s, they pioneered a six-sibling family of Information Processing Languages, or IPLs (Newell and Shaw 1957; Shaw *et al.* 1958; Newell 1960; Newell *et al.* 1964).

The first sibling wasn't actually implemented. For IPL-I was none other than the Logic Language, its name being "a label we put on retroactively" (Shaw, in McCorduck 1979: 142).

IPL-II, however, was implemented—and was powerful enough to be used for the Logic Theorist. It had no compiler, and had to be translated into machine code by hand. (This was a labour-intensive exercise: Newell and Simon worked out the relevant binary numbers simultaneously, saying them out loud to ensure that they agreed.) IPL-III, Newell said many years later, was "the best one of the bunch", because it had "no syntactic structure at all" (Laird and Rosenbloom 1992: 23). But it had to be abandoned, because it "was so space intensive that you simply couldn't run it on the machines"—which, in those days, were still tiny. IPL-V was more tractable, and was used for writing GPS.

In these IPL programs, each component ("statement") was a list structure. In general, a list needn't be a list of single items: it may be a list *of lists*. Just as a shopping list can be hierarchically organized (Grocer: sugar, tea, biscuits; Greengrocer: lettuce, carrots, courgettes; Draper: ribbons . . .), so could an IPL routine. So an IPL program wasn't best thought of as a sequence of instructions all conceptualized at the same level. Some instructions were simple (compare: "Get money from bank machine"), others more complex (compare: "Go to the grocer—and while you're there, buy sugar, tea, and biscuits and order some coffee beans").

Since *everything* was a list, statements could be treated either as data (information, facts . . .) to be noted, copied, or even changed, or as routines to be executed. So routines could be altered during execution, by the same processes that altered lists of data. These processes included substituting one symbol for another, adding an item to the end of a list, and inserting one list into another.

The last facility was necessary for solving problems whose goal/sub-goal complexity wasn't known beforehand. Think of going downstairs to fetch some milk from the fridge: it may turn out that you first have to search for the key of the kitchen door. Even in theorem proving, which was Newell and Simon's concern at this time, one doesn't usually know how many levels of means–end analysis will be necessary.

This required creative thinking about basic aspects of computer science. I implied, above, that if one could implement lists then hierarchy (lists of lists) would come for free. But this wasn't going to happen by magic: AI wasn't such a "fairy-tale" as that. How, then, were lists to be implemented?

For instance, how could a lower-level list be contained within a higher-level one? To do this (which was needed for means–end analysis, as we've just seen), memory access and allocation had to be handled in a new way. Specific sections of the memory couldn't be pre-assigned to this or that function, as was usual, because the programmer couldn't foresee how much memory would be needed when the program was run, nor just which items would need to be associated with (or included within) each other.

Newell and Simon's solution was to label each item with the memory address of the next item on the list—where 'adjacent' items needn't be close together in the physical memory store. The listed items could then be accessed in the right order *despite* being scattered across the memory as a whole. Functional ordering was liberated from physical ordering. Similarly, list insertion could be effected not by 'pushing other items aside' (compare making room in the crockery cupboard for a new set of bowls) but by changing a couple of memory addresses. As Simon put it later:

> [The] entire memory could be organized like a long string of beads, but with the individual beads of the string stored in arbitrary locations. "Nextness" was not determined by physical propinquity but by an address, or pointer, stored with each item, showing where the associated item was located. Then a bead [or a sub-string of beads] could be added to a string or omitted from a string *simply by changing a pair of addresses*, without disturbing the rest of the memory. (Simon 1991: 212; italics added)

Another fundamental contribution was the 'push-down stack'. The IPLs were recursive languages, meaning that one procedure could be nested inside another. To achieve that, Newell and Simon implemented the push-down stack. (An earlier form of push-down stack had been suggested, but not implemented, by Turing in the ACE report: Chapter 3.v.c.) This is analogous to a stack of unpaid bills placed on a spike, to be dealt with one by one and removed accordingly. If instruction A includes the instruction to do B, then A is temporarily stored ('pushed down') in such a way that it can be refound ('popped up'), and control passed back to it, as soon as B has been completed. This is possible even if instruction A is nested inside *itself*: the push-down stack ensures that the computer won't 'lose its way' by forgetting which level should be in control at any given stage.

That's how GPS was able to represent, and be efficiently controlled by, means–end hierarchies with goals and sub-goals on various levels. And it's how computers were able to accept self-reflexive procedures such as this (expressed here in English):

```
CLIMBSTAIRS:
  push right toe forwards
  IF toe meets obstacle
     THEN raise right foot onto next step
        AND raise left foot onto same step
     CLIMBSTAIRS
  ELSE stop.
```

In such cases, *repetition* (of reaching the next step) was implemented as *recursion* (of CLIMBSTAIRS).

A third innovation concerned memory loss. Earlier computer programming had followed the principle that when one routine hands over control to another it instantly forgets everything it had needed to know about the local context while it was working. After all, why waste memory space? However, instant memory loss on the relinquishing of control would make recursive routines impossible. One clever feature of IPL-V was that the programmer didn't need to state explicitly, every time a different goal took over the control, that the relevant context should be remembered (for push-down) or recalled (for pop-up). Nor did he/she need to state that when a given routine had been completed its contextual information could be deleted to save space, or that if it was abandoned then *everything* currently in the push-down stack could be removed simultaneously. The programming language took care of all that automatically.

As it turned out, IPL-V was soon dropped like a hot potato (see below). So why did Newell's obituarists make such a song and dance about it? The reason was that Newell and Simon hadn't merely invented a new type of programming language. They'd also developed a new approach to programming *in general*—and here, Simon was at least as important as the more technically skilled Newell.

Simon summarized his general ideas about hierarchical complexity soon after the birth of IPL-V (Simon 1962). He expanded them as the Karl Compton Lectures on 'The Sciences of the Artificial' in 1968—since when, they were revised and reprinted many times (Simon 1969). But the seeds had been evident in the first IPL-V manual, Newell's (1960) RAND memo on the language.

That memo was widely read within the NewFAI community—indeed, it helped to *create* that community. After circulating within the core of insiders, it was put out by a trade publisher in 1961; and a second edition appeared a few years later (Newell *et al.* 1964). By that time, IPL-V had been well-nigh abandoned. So the interest wasn't primarily in that particular language. Rather, it was in the manual's general advice on programming: e.g. recommending top-down design, short subroutines, and the avoidance of GOTO instructions. This was valuable to all NewFAIers, irrespective of their choice of programming language. As Newell's obituarists put it, he and Simon had introduced "*a design philosophy for programming* that years later would be reinvented independently as structured programming" (Laird and Rosenbloom 1992: 24; italics added).

In short, IPL-V was an important contribution to computer science, to software engineering, and (via GPS) to cognitive science too. That's partly why the *AI Magazine* honoured Newell so generously (see also subsection e, below).

c. LISPing in 'English'

Newell and Simon weren't the only ones to program in IPL-V. Colby, for instance, did so too, when writing the first version of his neurotic program (7.i.a). Nevertheless, it wasn't easy to use.

It was slow, because it had no compiler to translate instructions *directly* into machine code (it depended on an interpreter). It forced the programmer to keep track of some boring 'housekeeping' details, such as declaring whenever an unwanted list could be deleted to make room for a new one. And it was defined at a very low level, more like an assembly language (which is just one step up from the machine code) than what people think of today as a programming language. It therefore looked more like algebra than English—and not pretty algebra, at that. Indeed, Sloman (personal communication) describes it as "a horrible inelegant mess, [which] had no hope of surviving".

It didn't follow that the IPL-V *manual* was destined for the dustbin, as we've seen. But despite their respect for the programming strategy spelt out there, most computer scientists were loath to use IPL-V as their programming medium. Several other list-processing languages were soon designed accordingly.

MIT's Bobrow and Raphael compared some of them (Bobrow and Raphael 1964), as J. Michael Foster of Aberdeen University did a few years later (1967, ch. 7). Their differing advantages and disadvantages included speed, computational potential, and ease of use. For example, were the instructions interpreted or compiled, or both? Did the user have to oversee the 'garbage collection' (tidying up the used/unused memory locations)? Could lists be represented in only one fixed way? Was sharing of sub-lists allowed? And what about circular lists? These questions guided the very early AI researchers' choice of language, given that some form of list processing was desired.

So IPL-V soon had rivals. Gelernter, for instance, developed FLPL to write his geometry program (Gelernter *et al.* 1960*a*; see Chapter 6.iv.b). And Victor Yngve's (1958) COMIT was designed to deal with words, for the purposes of machine translation. COMIT was more easily intelligible than either IPL-V or FLPL—indeed, even beginners (such as myself: see Preface, ii) could use it. This was due not least to its relatively user-friendly programming manual, which stood out from the crowd in those days of aggressively 'macho' technicality (Yngve 1962*a*,*b*). (In fact, COMIT remained in use outside AI well into the 1970s: Yngve 1972.)

Within AI, the rival which won out was LISP—an acronym for LISt-Processing language. This was developed by McCarthy, and was one of his most important contributions to the field.

He'd already declared the need for a new, and English-like, language in his funding application for the 1956 Dartmouth Project (see i.h, above). Following discussions there with Newell and Simon, he started work on LISP that autumn (Chapter 6.iv.b). (He recalls: "I wasn't much influenced by Dartmouth—unless, perhaps, Newell and Simon pushed me into logic as a way of expressing information": personal communication.)

McCarthy was then teaching at Dartmouth, which didn't yet have an IBM 704 machine, so he was confined to using pencil and paper (Gabriel 1987: 520). When he went to MIT, he "concentrated on doing LISP" (J. McCarthy, personal communication). He wrote his first LISP program, for differentiating algebraic expressions, while visiting

IBM in summer 1958: it had if–then conditionals, and recursive use of conditionals (personal communication).

He outlined his ideas for the language publicly two years later, both in the very first MIT AI Lab memo and at Albert Uttley's November 1958 NPL meeting in London (McCarthy 1958, 1959). More detailed descriptions soon followed (McCarthy 1960; McCarthy *et al.* 1962). (The implementation details, and many subsequent improvements made by McCarthy and others, are described in Gabriel 1987.)

Introducing the famous CONS and CDR, McCarthy described a new way of implementing lists. This owed something to IPL-V and its recent cousins, but went far beyond them too. LISP quickly became the natural language of choice for AI (Bobrow 1972). Three decades later, when NewFAI had long given way to GOFAI, it was still being described as "the most widely used language in AI programming" (Gabriel 1987: 527).

The first version of LISP had several technical strengths. It was up to two orders of magnitude faster than IPL-V (and less demanding of memory space), because it had a compiler as well as an interpreter. And it provided automatic garbage collection. Besides tidying the memory store, this kept track of the partial results of various computations without any need for human monitoring.

LISP made the activity of programming easier, too. Because it offered built-in arithmetical functions, the programmer who happened to need them didn't have to define them anew. And it enabled one to write conditional (*if–then . . .*) instructions easily, as well as straightforward imperatives (*do this*). This was due to its intellectual origins in logic: specifically, Alonzo Church's lambda calculus (Chapter 4.i.c) and Gottlob Frege's higher-order functions (2.ix.b). (The first of these was later commemorated in the name of the hugely popular VR environment LambdaMOO: see 13.vi.d.)

Since functions could be arguments in other functions, recursion could be made explicit in the program, instead of being handled purely behind the scenes by the push-down stack. (Strictly, a LISP program wasn't a sequence of *instructions* but a set of *function calls*: one function call could include arguments returned as values from other function calls, and so on.) Like the IPLs, then, LISP allowed the definition of a term to include that term, so that processes could invoke themselves: CLIMBSTAIRS, all over again.

With respect to *what one could talk about* when talking to the computer, however, the most important innovation was its syntax. Recursion was represented more 'naturally' in LISP than in IPL-V, by nesting one bracketed expression inside another. Compare: *(The cat (I saw on Saturday night (when I came to supper)) sat on the mat)*. And there were no arcane symbols. The simplest LISP expressions consisted of up to thirty capital letters and digits, with commas and brackets as well: e.g. ((ABCDEF,239),(MNO,835)) or ((FATHER,MAGGIE),(HUSBAND,DOROTHY)). This syntax carried a liberating—but potentially deceptive—advantage, and an annoying disadvantage too.

The disadvantage was that the more complex LISP expressions could have many, many nested brackets. This meant that structures of significant hierarchical complexity could be represented. So far, so good. But typing errors were common. One of SHRDLU's sentence parsings, written in a LISP-based language, ends with a sequence of eleven brackets, thus:))))))))))) (Winograd 1972: 176). These are difficult to count accurately, even if one uses a pencil tip to do so. (Try it!) Today, the computer editor could count the brackets for you. But in those days, the brackets had to be matched by hand. As a result, bracket bugs were common in LISP programs. (And the name was sometimes glossed as "Lots of Inane Stupid Parentheses".)

The advantage was that LISP allowed the user to employ symbols visually identical with English words—as in the (FATHER,MAGGIE) example, above. Indeed, McCarthy's (1959) introductory paper had stressed this fact. LISP's origins were in logic, as we've seen. But McCarthy wanted to have his cake and eat it, by combining the deductive power of logic with the representational power of natural language. So in his talk at Uttley's meeting, he'd proposed the Advice Taker: a (notional) program that would accept statements in English and draw inferences from them (Section i.f, above).

The option of using English terms made it much easier to program complicated procedures in LISP than in IPL-V. For each step or sub-procedure could be named and/or expressed by symbols that looked like semantically appropriate, and therefore easily remembered, English words. In terms of ease of use, then, LISP was a liberation.

For this reason (plus the speed, arithmetic, garbage collection, and conditionals), AI professionals in the USA overwhelmingly opted for McCarthy's language. IPL-V was dropped, as were its other rivals. LISP ruled supreme.

(UK researchers used LISP too, in the early days. By the late 1960s, however, the Edinburgh-based POP2 was widely preferred, and remained dominant there—spreading to other European centres—for many years: Burstall and Popplestone 1966. Indeed, its successor POP-11 is still used by some AI researchers east of the Atlantic. Unlike LISP, which was designed for offline use, POP2 was a pioneering online computer language. That is, it was intended for use "by a person communicating directly with a computer *via* a typewriter"—Popplestone 1966: 185. It combined facilities offered by ALGOL and LISP: arrays, as well as lists. And its syntax avoided the plethora of brackets that plagued LISP programmers, making bugs due to typing mistakes less likely.)

However, there was a sting in the tail. LISP's major advantage—that it appeared to be using English words—was a mixed blessing. Too often, programmers unthinkingly assumed that the wordlike symbols in the program meant much the same as the corresponding words in natural language. As a result, they deceived themselves and others about the power and psychological relevance of their research.

They'd be rapped sharply over the knuckles for this by an AI colleague in 1974 (11.iii.a). But despite the knuckle rapping, the temptation to over-interpret procedure names and data labels persisted. Twenty years later, leading AI researchers were still being accused of believing that their programs were doing something (e.g. finding/recognizing analogies) in a 'human' way largely because this was implied by the mnemonic labels they'd chosen (Hofstadter and FARG 1995: 275–91).

Another way of putting this is to say that LISP made it easy for AI workers to fall into the trap described by Searle (16.v.c). Even though (*pace* Searle) a program *is not* all syntax and no semantics (see 16.ix), it can be so regarded for certain purposes. That is, it can be seen as an uninterpreted formal system. Purely formal systems can sometimes be mapped onto structural relations between meaningful concepts. For instance, a simple letter-shuffling or necklace-building game may map onto the rules of arithmetic—something one realizes spontaneously, as one plays the game (Hofstadter 1979: 46–54; Boden 1990*a*: 42–6). Where AI programs are concerned, there will always be *some* degree of match to the domain being modelled. The more insightful and/or competent the programmer, the closer the mapping will be. So people, programmers included, will be tempted to understand LISP's 'English' symbols as though they were the equivalent English words.

In general, this will be a mistake. Setting aside the philosophical question of whether there can be any real understanding there (Chapter 16), a program's quasi-understanding will inevitably be less rich than the human's. For in practice, not all of the relevant semantic associations will have been provided. Even arithmetical calculations by a computer can fail to map onto the human equivalent (13.ii.b). And natural-language words are interpreted by us in terms of types of relevance undreamt of by NewFAI (7.iii.d and 9.x).

This wasn't clear to AI workers in 1960, despite Bar-Hillel's recent critiques of MT (9.x.e) and Ludwig Wittgenstein's just-published *Philosophical Investigations* (9.x.d). Nor was it clear to 1960s cognitive scientists in general. Their common assumption, which guided Bruner's work on concept formation for instance, was that words could be defined by necessary and sufficient conditions. Indeed, the programme of logical atomism was still close to many people's hearts, from McCulloch (4.iii.c) to McCarthy himself. Small wonder, then, that 'English' was commonly assumed to be English.

In sum, what looks like gobbledygook isn't likely to be misread as good plain sense, but what *looks like* good plain sense is easier to write than gobbledygook is. If IPL-V hadn't given way to LISP, there'd have been less misunderstanding of AI. On the other hand, there'd have been much less AI to be misunderstood.

d. Virtual cascades

Even LISP had its weaknesses. There were certain things that late 1960s AI programmers would have liked to be able to do which simply couldn't be done, or which could be done only with great difficulty. So several people at MIT started asking themselves whether they could design better languages.

They might have started from scratch: *Forget LISP! Let's invent a fundamentally new language!* After all, that's what Robin Popplestone (1938–2004) had done when he designed POP. And it's what Kay at Xerox PARC did in 1971–2 when he designed SMALLTALK, the first object-oriented programming language (A. C. Kay 1993).

Object-oriented languages are 'natural' in the sense that they allow the programmer to think in terms of objects, or hierarchical concepts (think *frames*), without always having to worry about *just how* those objects can/will interact. In effect, the object (as defined in the programming language) already 'knows' how it behaves, and/or how it relates to other objects. Often, it can 'tell' the user what it can do, and ask just which feature the user wants to focus on. (That's partly why object-oriented languages are useful for designing computerized *agents*: see 13.iii.d.) The reason is that object-oriented languages have an "inheritance" mechanism, whereby properties already defined with respect to one concept are automatically inherited by any concept defined as a subclass of it. (Compare: if I tell one of my grandsons that a lynx is a type of cat, he immediately knows a great deal about lynxes because he already knows a great deal about cats.) Java is a well-known modern example, but there are others besides. (SMALLTALK itself is still used occasionally: Shasha and Lazere 1995: 49.) Licklider had remarked the need for object-oriented languages in his 1960 paper, pointing out that computers at that time had only two elementary symbols (0 and 1), and "no inherent appreciation either of unitary objects or of coherent actions".

But although Kay was using SMALLTALK for his own research (see 13.v.d), it was too far ahead of its time to be effectively implementable. (Today's Java is partly inspired by SMALLTALK.) At MIT, moreover, LISP ruled the roost. It was so well established there that the natural option was to use it as the basis for designing more powerful languages.

It was already known that one programming language could be defined in terms of another. (Gelernter's FLPL, for instance, had been a list-processing language embedded in FORTRAN—hence the "F" in the acronym.) In other words, there could be a *cascade* of computational environments.

Just as computer scientists weren't restricted to working in machine code, so they weren't restricted to working in a 'base-level' programming language such as IPL-V or LISP. The facilities made available by a programming language on level n could be used to define new facilities in language $n + 1$, which in turn could support the computational environment of language $n + 2$ and so on. In the terminology used in Chapters 7.i.f and 16.ix.a, each language provides a particular *virtual machine*, and one virtual machine may be implemented in another . . . until we reach the machine code—which alone can make something happen in the electronic entrails.

In such cases, successive levels of internal translation have to be done by the computer. Because this costs time and memory space, multilevel languages are inefficient. Eventually, new languages and/or hardware would be developed in which functions previously considered as high-level (needing one or more layers of compilation/interpretation) were directly implemented in the hardware. For instance, in the late 1970s MIT developed the LISP Machine, in which basic LISP functions were embodied in the machine code (Bawden *et al.* 1977), and also the Connection Machine (Hillis 1985), in which parallelism—instead of being simulated by a von Neumann computer, as is still usual today—was built in. (Neither was a commercial success: 11.v.a.) But that was for the future. Meanwhile, NewFAI was still struggling to find better ways of telling computers what to do.

At MIT, several virtual cascades were defined, of which the two best known were PLANNER and CONNIVER. Indeed, the former became world-famous as a result of Winograd's thesis, which electrified the AI community—and cognitive scientists in general—early in the 1970s.

Recall, for instance, Winograd's opening remark to SHRDLU: "Pick up a big red block" (iv.a, above). This wasn't a straightforward task, because the green block had to be cleared away first—which involved noticing it, picking it up, finding empty space for it on the table, and putting it down. But Winograd didn't have to say any of that: he left SHRDLU to work it out for itself. If he'd been using LISP, he couldn't have written such a vaguely specified instruction. He'd have had to say not only what he wanted done, but precisely how it was to be achieved.

Instead of LISP, Winograd had used (a cut-down version of) Carl Hewitt's new language, PLANNER (Hewitt 1969, 1972; Sussman *et al.* 1970). PLANNER was the first "goal-directed" language, in which one could specify a goal in general terms without having to identify all the particular objects and operations involved in attaining it.

This was possible because procedures were indexed not by some individual descriptor or name, but by general patterns specifying the type of data on which (or towards which) they were supposed to work. So procedures could be called indirectly, using (unbound) "open variables" instead of (bound) "closed variables".

To see the difference, compare asking a friend to bring you a copy of today's newspaper with asking them to walk to the third counter on the right in the newsagent's shop on Middle Street with the purple awning, riffle through the newspapers until they find *The Independent*, and then buy it, handing over the money to the shop assistant and bringing back the change. The first version of your request is much easier to give. But you have to assume that your friend has enough nous to be able to execute it. They need to know already, for example, that if one wants to buy a newspaper the best place to do so is a newsagent's, that there's a newsagent nearby (on Middle Street or elsewhere), and that all the newspapers will be laid out together.

So it was with PLANNER. Winograd's instruction "Pick up a big red block" activated the PICKUP function by name. But the PLANNER procedure defining this function included within itself the general information: "If you wish to pick up X (whatever it is), and there is (any) Y on top of that X, then you should call CLEARTOP to get rid of Y before you try to move X". In this example, the first occurrence of X is open, as is the first occurrence of Y, but the subsequent occurrences of both are closed. When PICKUP was called, *the program itself* could be relied on to fill in the requisite values of the variables. In LISP, by contrast, every procedure call had to include only closed variables (otherwise, the LISP compiler would spit it out, with an error message asking the programmer to bind the open variable).

As this simple example illustrates, a 'single' PLANNER procedure could include calls to other procedures within it (these calls might be recursive, hierarchical, or heterarchical: Section iv.a, above). More complex PLANNER procedures could include heuristic advice about which other procedures *might* be relevant, and which of these should be tried first. When no heuristic advice was specifically included, the PLANNER procedure would search the database for *any* item whose goal matched the desired pattern, trying each one in turn.

All of this involved hidden processing. A PLANNER program would *automatically* remember what was the last decision, and which alternatives had remained at the time. If the current attempt failed, it would backtrack to the last decision point and try another alternative—or, if none remained, it would back up to the next-highest level in the search tree and try any alternative remaining there. This processing strategy was implicit in the programming language, whereas a LISP programmer (or in the case of GPS, an IPL-V programmer) would have had to specify it explicitly. Since such details were hidden, PLANNER programs were relatively 'chunky' and so relatively easy to understand. The "magical number seven" had been cheated, again.

By the same token, however, the hidden details could include unsuspected inefficiencies and dead ends. Consider the housekeeping involved in the automatic backtracking. PLANNER handled its push-down stacks in such a way that the details of the local environment at each previous decision point were saved, to be reinstated when failure caused the program to pop up to some higher level. However (as in the earlier list-processing languages), abandonment of a goal on level n would automatically delete the contextual information at level n. Human thinking isn't like that: when someone decides to return to a previous decision point so as to restart the search from there, they'll usually want to remember *why* the previous attempt failed. For if they don't, they might try a second alternative (and a third, and . . .) that's doomed to failure for precisely the same reason. Also, they might want to remember *how the world changed* while they were making the unsuccessful attempt. For if they can do that, they may even be able try the same alternative again—this time, making sure that a specific troublesome change is either prevented or immediately remedied.

To avoid PLANNER's stupidity in situations of failure, Hewitt's MIT colleagues Sussman and McDermott designed yet another LISP-based language, called CONNIVER (Sussman and McDermott 1972*a*,*b*). A failed CONNIVER procedure could not only tell its higher-level module why it failed, but also pass on information about the sequence of world changes it encountered on the way. This was a prime reason why HACKER (written in CONNIVER) could learn from its mistakes, whereas SHRDLU couldn't (see iii.d, above).

A prime reason why CONNIVER programs could do things which no PLANNER program could do was the language's economical (and automatic) storage of contextual information. A series (or tree) of local contexts was stored in such a way that the *shared* information didn't have to be repeated. After the first context frame, any information that was explicitly coded represented only environmental *change*. So a robot wouldn't have to reiterate the fact that a square box *stays square* no matter how many times it's moved. Or rather, it wouldn't have to do so if the programmer had had the foresight to build that proviso in. (This was one way of trying to avoid, or anyway lessen, the notorious frame problem.)

Another strength automatically provided by CONNIVER was the ability to choose *the best* of a set of alternative procedures, instead of (as in PLANNER) making an unordered list of candidates and trying them out one by one. To be sure, some PLANNER programs could make such choices too—but that ability had to be specifically written in by the programmer. In CONNIVER, it was tacitly provided by the language itself.

PLANNER and CONNIVER were, in effect, LISP with knobs on. Other AI languages were developed later which weren't based on LISP. But the point of general importance here is that a new programming language, even if it's defined in terms of a previous one, can hugely increase the ease of programming—and the power of the ensuing computation.

e. NewFAI in parallel

Having pioneered list processing, Newell and Simon could have been forgiven for resting on their laurels—at least so far as the technological aspects of AI were concerned. But they didn't.

In the mid-1960s, they (again, mostly Newell) developed yet another way of talking to computers: by using production systems, or PSs. Strictly, these were a type of computational architecture rather than a specific programming language. Like semantic networks, or ATNs (9.xi.b), they could be defined in many different languages (see below). In other words, they were a new class of virtual machine for AI's armoury—providing a form of *parallel* processing, of a richness far surpassing Pandemonium (Chapter 11.ii.d).

Thanks to the ever-present grapevine, NewFAI's inner circle knew about this development from the start. In 1967 Newell's lectures on PSs at Stanford had inspired Buchanan and Feigenbaum (who'd collaborated with Newell on IPL-V at RAND, ten years earlier) to use them as the basis of DENDRAL, officially reported in 1969. But it wasn't until the early 1970s that Newell and Simon themselves published on their new approach (Newell 1972, 1973*a*; Newell and Simon 1972).

Through the rest of the 1970s, that approach was widely adopted within both technological and psychological AI. Encouraged by DENDRAL and its cousin MYCIN, PS-based expert systems burgeoned. And psychological modelling flourished too, following Newell and Simon's massive tome on *Human Problem Solving* (Chapter 7.iv).

Like LISP, PSs grew out of logic. In 1947 the Polish-born logician Emil Post (1897–1954) had defined "rewrite production systems" as a way of representing recursion. As formally defined by Post (1943), a production system was a set of logically independent condition–action rules, or "productions". As implemented by Newell and Simon, a production system was a set of *if–then* pairs: *if* the condition is satisfied, *then* the action is executed. In effect, then, the whole system consisted of demons (or of ants: 7.iv.a–b). This was a huge liberation from the recursive programming languages, in which each procedure had to be explicitly invoked by some other procedure. Productions, by contrast, were triggered by the current state of affairs.

Post's logic had already entered cognitive science before Newell and Simon took an interest in it, for Chomsky (1957) had based the rewrite rules of his phrase-structure grammar on it (Chapter 9.vi.c). It had also affected *computer* science, where it was being used in the early 1960s to design compilers. Indeed, Simon first heard about production systems from a young computer scientist called Bob Floyd, who was working in a Boston software company before Simon gave him a job at CMU (Crevier 1993: 150). But it hadn't yet spread to AI. Although Chomsky had done important theoretical work in computer science (including a proof that push-down stacks can handle context-free languages: 9.vi.a), he wasn't interested in AI—not even computational linguistics. His only venture into this area was in collaboration with a psychologist colleague (G. A. Miller and Chomsky 1963: 464–82). Newell and Simon, by contrast, had been committed to computer modelling since the mid-1950s (6.iii.b). When they discovered Post's logic, they showed how it could be implemented for AI purposes—and how it could be *put to work*.

Since sets of IF–THEN rules can in principle be used to represent any computation, PSs were in effect a universal (very high-level) programming language. Indeed, Newell's student Michael Rychener (1976) reimplemented many classic NewFAI systems as PSs. However, they're usually thought of not as a programming language but as a general type of program. For PSs are a *class* of control architectures, which have been implemented in a variety of (lower-level) programming languages.

Some of these languages were designed with PSs specifically in mind. One of the first widely used examples was PSG. This was the final version of a series of possible languages outlined by Newell (1973*a*) to implement different control systems for PSs—"different", for instance, in the ways in which conflict resolution and/or memory matching and/or backtracking were handled.

PSG, in turn, gave rise to successive versions of OPS, or Official Production-System language (Forgy and McDermott 1977; Forgy 1981, 1984). These were all developed at Newell and Simon's home base in Pittsburgh, and partly by Newell himself. One of the first commercially successful expert systems, John McDermott's VAX configurer (11.v.b), was written in OPS4 and OPS5.

The main aims behind OPS were to support Newell and Simon's claims that PSs could function in complex environments (which provide many interruptions), and could learn general lessons from experience while so doing (Forgy and McDermott 1977: 933). In other words, OPS was a departure from the early expert-systems approach, in which a PS would be tailored for a single task: specialized, uninterrupted, and fixed. (Likewise, Newell and Simon's PS models had been a departure from GPS—which also was specialized, uninterrupted, and fixed.)

In general, PSs used a "blackboard" architecture. Although production systems, like GPS, were sequential (only one rule could be executed at a time), they weren't pre-ordered instruction lists: first do *this*, then do *that* . . . Instead, all the currently active conditions were made simultaneously visible to the whole system, by being placed on a central blackboard. The PS progenitors, Newell and Simon, allowed no more than nine symbols on the blackboard, so as to match the "magical number seven" of human psychology (Chapter 7.iv.b). But not all PS designers limited their systems in that way. Blackboard architectures were pioneered in the early 1970s HEARSAY speech system, a project led by Newell himself. The blackboard was helpful here because it enabled the program to integrate evidence drawn from widely different areas—namely, phonetics, syntax, and semantics (Chapter 9.xi.g). (Eventually, blackboard PSs were used as the basis of most expert systems.)

In essence, if a production found its condition on the blackboard, then it would fire automatically. Because that was so, and because (as Post had specified) the individual productions were logically independent, a PS was in effect a parallel-processing system. Like Selfridge's (1959) Pandemonium (11.ii.d), the virtual machine was parallelist even though the basic implementation wasn't. And since hundreds, or even thousands, of productions could be involved, most PSs were immeasurably richer than Pandemonium.

In practice, however, this richness presented a problem. For two (or perhaps many more) productions might find their conditions satisfied simultaneously. So some way—preferably sensible, not random—had to be found of enabling the system to choose *just one* of these for execution. Various conflict-resolution methods were used. For instance, the unfired productions might remain visible, as in SOAR, or they might be repressed—as in the earlier PSs (see Chapter 7.iii.b).

In other words, one of the major architectural dimensions on which PSs could differ was their method of dealing with simultaneously satisfied productions. Another was their method of backtracking, and yet another was memory matching (Newell 1973*a*). Memory matching was problematic, for instance, because there might be tens of thousands of individual rules in a single system: how can the blackboard be efficiently inspected, given this fact? Partly because of differences in memory matching, forward and backward chaining (iv.c, above) could be done in various ways.

In sum, production systems offered a rich store of virtual machines implemented in other virtual machines (including languages specially designed for PSs) . . . and differing among themselves not only in low-level detail but also on several broadly defined computational dimensions. The potential for both psychological and technological AI had been hugely increased. Those twenty-six pages of obituary were well deserved.

f. It's only logical!

LISP was designed so that the programmer could tell the computer what to do, when. So the general form of a programmed procedure was *do this, then do that, followed by the other*... Even the 'knobby' LISPs, such as PLANNER and CONNIVER, required the programmer to lay down a sequence of goal-oriented actions, although the details would be decided by the program itself at run time.

Given the sequential architecture of the von Neumann machine, this made sense. But AI workers sometimes wanted to shrug off the sequential straitjacket. This was so, for example, if their programs had to deal with situations arising at unexpected times. Indeed, it would in general be nice—so they thought—not to have to trouble themselves with issues of temporal order. How much simpler if the machine could be given the facts and then left to do what it had to do, in whatever order it pleased.

As we've seen, one way of avoiding specifications of task order was to provide anticipatory demons, which would lie in wait until aroused by their input cue. Another was to use a 'parallelist' PS. And a third was logic programming.

(A word of clarification: programming *in* logic—i.e. logic programming—must be distinguished from programming *inspired by* logic, and from programming a computer to *do* logic. Moreover, one logic must be distinguished from another. Three have given rise to hugely influential AI languages: Russell's predicate logic, Post's production systems, and Church's lambda calculus. However, when people mention "logic" in the context of programming languages, or "logicism" in cognitive science, they usually mean predicate logic—perhaps supplemented by modal logics of various kinds: see 13.i.a–b.)

Logic programming was PROgramming in LOGic—hence the acronym of the best-known language of this type, PROLOG. But PROLOG wasn't the only example, nor even the first. Zuse had designed a logic-based programming language, the Plankalkul, as early as 1946. However, this wasn't published until 1972 (and translated in 1976). It didn't catch on: despite being better than its rivals in some ways, they were already well established (Bauer and Wossner 1972). Adopting a new programming language is almost as difficult as abandoning the QWERTY keyboard. It does happen, but only if the advantages are significant.

In Anglo-American computing, theoretical work on 'declarative' programming languages had started in the early 1960s (e.g. Laski and Buxton 1962), and the turn of the decade saw the first fully implemented example. This was ABSET (with its compiler ABSYS), named after ABerdeen and SET theory (Foster and Elcock 1969; Elcock *et al.* 1971).

The authors' motivation was clear. In a preliminary paper tellingly subtitled 'Programs Written Without Specifying Unnecessary Order', Foster had said:

> The basic elements in this system are not instructions to do something, as are the statements of ALGOL, but assertions about the data, such as $a = b$ or $a + b = c$. The evaluation of a program of assertions *is not* the obeying in specified sequence of a set of instructions, but *an attempt to find data which satisfies the assertions*. (Foster 1968: 387; italics added)

At that time, his "Assertions" language (like Ted Elcock's work on 'Descriptions': 1968) was only partly implemented. But within a year or so the ABSET/ABSYS version had been completed. Again, the aim was to avoid the sequential straitjacket:

The overall design aim of ABSET was to devise an interactive programming language in which it is possible, at will, to take or defer decisions about a program: we therefore require that *decisions which are logically separable can indeed be taken separately and in any order*. (Elcock *et al*. 1971: 467; italics added)

We have concentrated on more primitive ideas [including notions taken from set theory], *the distinction between an ordering of decisions and an order of evaluation*, and the manipulation of partly-evaluated program. (pp. 467–8; italics added)

Two themes run through [our work]: that it should be clear what a program says, and that *the language should not force the programmer to commit himself to decisions he would prefer to postpone*. (p. 469; italics added)

Like set theory in mathematics, ABSET/ABSYS was focused on the primitive logical concepts underlying more familiar notions—here, not *numbers* but *list processing, matrix multiplication, text processing,* and so on. That is, it was intended as a general programming language. By 1980, however, it—and respected logic-based rivals such as SYNICS, named for SYNtax and semantICS (Edmonds and Guest 1977*a*,*b*; Edmonds 1981: 406–17)—had been overtaken by PROLOG.

In a retrospective review, two of the Aberdeen authors showed that "anything expressible in PROLOG was capable of straightforward translation into a subset of ABSYS" (Elcock and Gray 1988: 1; see also Elcock 1988). Indeed, they quoted Robert Kowalski's (1988) admission that "ABSYS, a declarative programming language . . . anticipated a number of PROLOG features"—including resolution, unification, backtracking, and the notorious 'negation by failure' (see iii.b, above).

Kowalski's word, here, was significant. For it was he who'd made PROLOG work. Or rather, it was he who'd made PROLOG work best. PROLOG was a language based on Russell's predicate calculus, and using an efficient form of resolution defined by Kowalski in 1971 (Kowalski and Kuehner 1971). It was first conceived—also in 1971—by Alain Colmerauer (1941–) and Philippe Roussel at the University of Aix–Marseille, for use in NLP. It was implemented by them in ALGOL a year later (Colmerauer and Roussel 1993). Kowalski's more efficient implementation was done at Edinburgh (until 1975) and then at London's Imperial College (Kowalski 1979).

One of the advantages of PROLOG was that it provided "for free" much of the inference machinery which had to be specifically programmed in LISP. By the mid-1980s, it was being widely employed in Europe (van Caneghem and Warren 1986), and introductory accounts had been written for AI students (Clocksin and Mellish 1981; Clocksin 1984). Not the least spur to its success was its use by the Japanese in their Fifth Generation project (11.v.a).

Like PSs, a PROLOG program—according to the blurb—would provide the machine with a set of items, and leave it to cope with them in whatever order it pleased. Unlike PSs, the items in question weren't expressed as conditionals (*if–then* rules) but as declaratives (assertions). The program would use general methods, such as logical *unification* and *resolution* (J. A. Robinson 1965), to draw whatever inferences it could from the items available.

PROLOG attracted a lot of attention in its early days partly because it was seen as a protagonist in the then raging 'procedural/declarative' controversy (iv.a, above). Logic, as such, says nothing about the *order* in which assertions are to be tested (or, in a Post production system, the order in which rewrite rules are to be applied). That's decided by the human logician. Indeed, logically independent assertions (such as the various conjuncts within one long conjunction) could in principle be considered simultaneously. That's why many PROLOG programmers insisted that PROLOG, as an implementation of logic, provided declarative program specifications—not procedural programs. (Hence my reference to "the blurb".)

It gradually became clear, however, that it had to be thought of in both these ways. In trivially simple cases, to be sure, a PROLOG program could be seen as purely declarative. When there are only a few assertions to be tested, the order of testing won't matter. But a large PROLOG program (like a large PS) will include many individual items—and may require long chains of inference too. Random testing could take for ever and a day. Some ordering preferences must be followed—which is to say that some *procedural* information must be provided in the program. Eventually, as people realized the futility of this dispute, the language was judged more by a 'horses for courses' criterion. (Logic programming was *combined* with list processing in POPLOG, a programming environment widely used in the UK and Europe which integrated PROLOG with POP2: Hardy 1984.)

With respect to horses for courses, there was an embarrassment lurking in the undergrowth. PROLOG was logically elegant—indeed, logically reliable. But whether it was *practically* reliable was less clear. For as we saw in Section iii.b, resolution theorem-provers can't distinguish between *proving that X is false* (i.e. genuine negation), and *failing to prove that X is true* (i.e. negation by failure). The same applied, then, to any program written in PROLOG. However, there's a huge difference between these two types of negation (if you doubt this, read X as *my partner is having an affair*). If the program is doing theorem proving in a closed logical system, this may not matter. But the case is different for reasoning in 'open' systems, where one can't assume that all the relevant information is present. The use of PROLOG for real-world expert systems was therefore problematic.

This was pointed out very early on (K. L. Clark 1978). Even so, the Japanese adopted PROLOG enthusiastically in their Fifth Generation programme a few years later. Their assumption, apparently, was that every essential feature of the relevant specialist domain could be adequately represented in a PROLOG-based expert system—"adequately" for technological purposes, if not for Marcel Proust or for jealous lovers. (The adequacy of PROLOG for various *legal* purposes was discussed at length by Kowalski himself: Chapter 13.ii.c.) Soon, various people tried to provide 'real' negation, combining goal-directed user questioning with an underlying three-valued logic: *true*, *false*, and *don't know* (Aida *et al.* 1983; Edmonds 1986).

When Kowalski first implemented PROLOG, he used von Neumann machines. But the virtual parallelism of PROLOG—i.e. the logical independence of individual assertions—was in principle well suited to hardware parallelism. So when the Japanese committed themselves to PROLOG, they committed themselves also to developing a PROLOG machine, where the inferential methods of unification and resolution weren't programmed in, but implemented in the hardware. (In effect, logic was the *machine code* of the computer.) Today, PROLOG is still used—with or without PROLOG machines—by many people in AI.

10.vi. Child's Play

LOGO wasn't just yet-another-programming-language: it was more like a philosophy of life. And it had a fascinating life story of its own. It started in the 1960s, in a form so simple that it was easy for AI researchers to ignore. Around 1990 it metamorphosed into something radically different, and very exciting.

From the start, it brought Piagetian psychology into the world of programming, and (soon afterwards) programming into the world of child education. Eventually, it even helped to bring computing to Everyman, in personal computers like the one you may have sitting on your desk at home.

a. The power of bugs

In 1967, while his MIT colleague Hewitt was struggling to sophisticate LISP as PLANNER, Papert (1928–) was defining a new language, LOGO. This was specifically intended for use by children as young as 4 or 5 (Papert 1972, 1973). As such, LOGO needed to be both simple and intuitively accessible.

Accordingly, the virtual world it defined contained a "turtle" moving about on a CDU screen, according to the child's instructions. These were expressed in familiar terms such as FORWARD, BACK, STOP, TURN, RIGHT, LEFT, 5, 10, PEN UP, and PEN DOWN. And the human–computer interface had to be designed anew: instructions weren't given by typing, but by button pressing (how would a 4-year-old manage to type "FORWARD"?).

Very soon, Papert took a leaf out of W. Grey Walter's book (Chapter 4.viii.a–b) and implemented LOGO as a language for controlling robot turtles moving around the floor. (The first turtles were made in the AI Lab's workshop, but they were commercially manufactured from 1977, by Terrapin Software.)

Whether in the virtual (CDU) or the real world, LOGO turtles carried a "pen" which they could lower so as to leave a trace on the screen/paper as they moved. The possibility arose, then, of the child's defining routines and subroutines for drawing a SQUARE, a ROOF, a HOUSE, a MAN, a FACE, and so on.

Usually, the first attempts wouldn't work (see Figure 10.28). Perhaps the child, trying to draw a square, would tell the turtle to TURN. But should it turn LEFT or RIGHT? Often, the child wouldn't say, assuming that this was obvious (especially if the first turn had already been made). And how far should it turn? In a program for drawing a square, "TURN 90" was needed; for starting to add a roof, some other angle would be required. And PEN-UP would be needed if, having drawn the house walls, the child now wanted to add a door or a window. Otherwise, an unwanted line would mar the drawing.

All pretty trivial . . . ? For Papert, absolutely not. The object of this playful exercise was education, not meaningless play—and education not in drawing houses but in self-critical, creative thinking *in general*. As he put it, he was concerned less with mathematics than with *mathetics*: "the set of guiding principles that govern learning" (Papert 1980: 52). In other words, he was doing cognitive science, not technological AI.

His five years with Jean Piaget in Geneva had convinced him that learning comes about by *construction*, not *instruction*. That is, the child's interactions with objects in

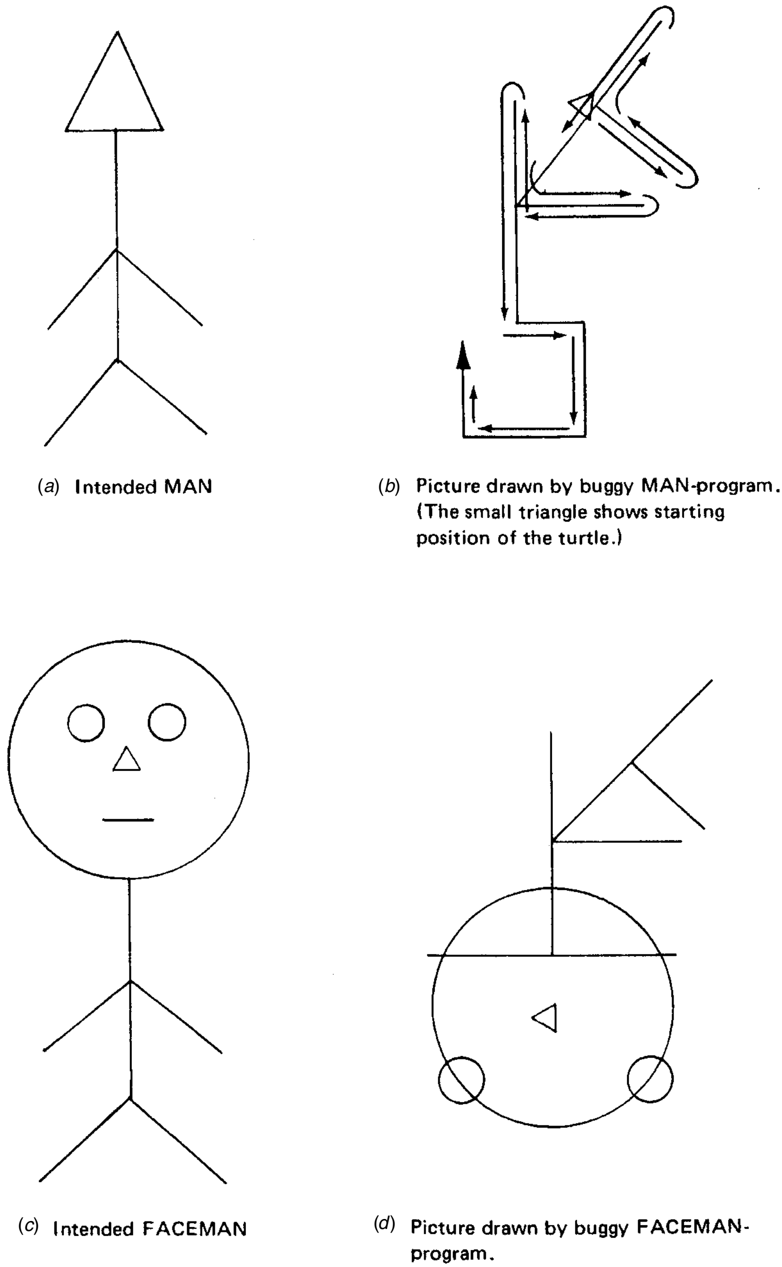


Fig. 10.28. Drawings done by children's LOGO programs. Reprinted with permission from Goldstein (1975: 250)

the world (and virtual worlds) enable him/her to construct problem-solving schemata, which become increasingly well adapted to the task as a result of active reconstruction prompted by errors (Chapter 5.ii.c). So Papert relied on the child's own exploratory activities to foster progress through constructive (*sic*) self-criticism, perhaps aided by a sensitive teacher.

A key idea was "powerful ideas": concepts that would help the child (and his/her teacher) to understand what's involved in thinking. Perhaps the most powerful idea of all was *bugs*.

Bugs are errors. But they're errors of a very special kind. Because they occur in programs, they're in principle both recognizable (as Lovelace had realized: see 3.iii.b) and correctable. In addition, they're displayed by the (non-judgemental) computer, rather than being pointed out by a (possibly scornful) human teacher. As illustrated in Figure 10.28, Papert enabled children to use their own buggy programs to gain insight into their thinking processes, and thereby to improve them.

One mid-1970s MIT project aimed to devise a computer program to play the role of the teacher. So Ira Goldstein's MYCROFT used general ideas about debugging, like those already being used in HACKER, to evaluate and correct *drawings done by computers programmed to draw by children* (Goldstein 1975). Given a mismatch between the intended and actual drawings, MYCROFT would inspect the child's program to locate the bugs. But it wouldn't merely spit out a corrected program. (The child probably wouldn't learn much if it did.) Rather, it would analyse the faults in relation to specific bugs, which the child might learn how to correct—and how to avoid—in future. For instance, if one starts to draw a man (as in Figure 10.28*b*) by drawing the two legs splayed out from the starting point, one will have to insert an extra orienting step before starting on the body. Otherwise, as in this buggy picture, the body will be aligned with the second leg instead of being at an angle to it.

Reorientation bugs *in general* involve the failure to restore some previous state of affairs so as to continue with the next main step of the procedure. They occur in all sorts of contexts, not just in drawing. Papert's hope was that LOGO-based experience, whether with MYCROFT or with a human teacher, would help children to recognize—and avoid—common bugs in many different domains, as well as increasing their self-confidence by 're-educating' their attitude to error.

His own writings, and those of his adherents, abounded with anecdotal evidence that this was so (e.g. Papert 1980; Howe *et al.* 1980; Weir 1987). As a result, LOGO in some form (including LEGO/Logo: see below) was being used "in about one-third of the elementary schools in the United States" by the mid-1990s (Resnick 1994: 26).

However, some careful 1980s attempts to assess the educational value of LOGO programming were ambiguous. The effects were there, to be sure—but they were neither so strong nor so generalizable as Papert had hoped (Pea and Kurland 1984; Kurland *et al.* 1986; Pea *et al.* 1987).

Today's verdict, after many empirical studies (focused on other programming languages as well as on LOGO), is that positive effects depend on the extent to which the instructions, and the teachers' comments, stress *metacognition*, or thinking about thinking. Merely teaching children to program, without such metacognitive encouragement, doesn't necessarily help them to develop transferable thinking skills.

The Harvard educational psychologist David Perkins, who'd been familiar with Papert's work from the start, reviewed the literature and concluded:

> Originally, one might [like Papert, and others too] have thought of programming as a kind of cognitive playground; mere engagement in the activity of itself would exercise the mind as real playgrounds exercise young bodies, without any need for *instruction finely tuned* to provoke such consequences. Unfortunately, the research argues against such a vision.
>
> Instead . . . certain inconvenient conditions must be met. [These include huge time commitments for practice, and significant programming expertise on the part of the teacher.] Taken together, these points suggest that the *straightforward* teaching of elementary programming is not a very good way to foster cognitive skills. (Salomon and Perkins 1987: 163–4; italics added)

Some twenty years later, his judgement remains unchanged (D. N. Perkins, personal communication).

b. Complication and distribution

Even in the 1970s, LOGO wasn't restricted to drawing-with-turtles. For instance, Harold Abelson and Andrea diSessa (1980) showed how LOGO could be used to teach children about other types of mathematics, such as spherical geometry and algebraic vectors—and the laws of physics, too. Part of the interest here was that a law of physics could be deliberately varied, or omitted, and the result would be visible on the CDU screen.

In today's terminology, the children were constructing a virtual reality whose behaviour one could actually *see*. But because the computer graphics used by Abelson and diSessa were so minimalist, these imaginary physical worlds couldn't begin to compare with what we now think of as VR (13.vi).

The 1980s saw the addition of list processing to LOGO. This suited it, for instance, for helping children to learn writing skills (Friendly 1988). Also in the 1980s, Papert added *real* bells and whistles. In other words, he helped the LEGO toy company to design special sensors and actuators to be connected to LEGO components (Resnick 1994: 24–31). The sensors could respond to touch, infra-red, and light; and the actuators included gears, pulleys, wheels, motors, and light switches. Using this LEGO/Logo construction kit, children could build/program their own machines. These ranged from toy cars through ovens to robots—with Valentino Braitenberg's lifelike "vehicles" in between (15.vii.a).

Still more was to come. In the 1990s, LOGO was hugely improved/expanded so as to be suitable for 'real' AI programming (B. Harvey 1997). In addition, a parallelist version called StarLogo was developed towards the end of the century (Resnick 1994: 31–47; Colella *et al.* 2001).

StarLogo provided a high-level language whereby *thousands* of turtles (agents) could be simulated simultaneously. Each one had its own "program", in the form of a set of rules about what to do in various situations. These rule-sets could be identical in every agent, or there might be several different rule-sets distributed (equally or unequally) across the population. Think of a beehive: there's only one queen, but there are many workers and drones. Queen, worker, and drone follow very different rules, live very different lives. And the entomologist can study the overall organization within the beehive, as well as studying the behaviour of individual bees.

In a StarLogo program, each little local environment ("patch"), too, could execute StarLogo commands. So it could grow more 'food' in certain circumstances, for instance, or release 'chemicals' towards neighbour patches at various rates. As a result, the user could model *interactions with and within* the environment, as opposed to *agents' actions on* a passive environment.

In short, StarLogo gave people a way of studying *distributed* systems. These are made up of many autonomous agents—which may be very simple, or relatively mindlike (see 13.iii.d–e and 15.viii–ix). They're all around us: not just beehives, but traffic jams too. But being ubiquitous doesn't make them easy to understand.

StarLogo might even be called the first postmodernist programming language. It's not that postmodernists are enamoured of traffic jams, or less anxious than the rest of us when near a beehive. But, as we saw in Chapter 1.iii.d, they eventually progressed beyond their counter-cultural predecessors of the 1960s–1970s (whose rejection of computer-grounded psychologies was total) by warming to certain types of computational psychology and AI/A-Life: namely, those focused on distributed systems. Much as heterarchy was more politically congenial than hierarchy (see iv.a, above), so cooperation between autonomous individuals was more acceptable—and also more veridical—than rigidly other-directed behaviour.

c. Pointers to the future

The children hadn't dropped out of the picture. Far from it.

On the one hand, they could use StarLogo too. And in playing (*sic*) with it, they could, for the first time, get an intuitive sense of the behaviour of distributed systems.

On the other hand, Papert's passion for child-friendly computing had helped lead to technologies without which many children's lives today—and probably yours, too—would be very different. Namely, user-friendly interfaces and virtual reality. In other words, Papert's work was crucial in turning the futuristic visions of Bush and Engelbart into everyday realities.

But that's a story for a later chapter (13.v–vi).

11 OF BOMBS AND BOMBSHELLS

NewFAI didn't excite only its practitioners: it electrified plenty of other people as well.

Some were cognitive scientists: psychologists, linguists, and philosophers enthused by NewFAI's promise of turning *mind-as-machine* from a metaphysical slogan into a scientific research programme. Their excitement was raised almost to fever pitch by the consciousness-raising meetings of the late 1950s (see Chapter 6.iv). But some were vociferous opponents, fiercely scornful of any such idea—and worried about AI's technological potential, to boot.

This wasn't a private fight: virtually everyone could join in. In other words, the critiques came largely from journalists, social commentators, and the general public. As we'll see, their passionately held views had important (and usually damaging) effects on the intellectual advancement of AI and related disciplines.

Early connectionism, too, met opposition—but this came only from AI insiders. It sometimes aroused passions, to be sure. Indeed, a major scandal ensued within the field when Marvin Minsky's mid-1950s criticisms in 'Steps . . .' were brought to a head in 1968 (Chapters 10.i.g and 12.iii). But outsiders knew little or nothing of that. Their sights were set on the symbolic footpath, not the cybernetic one (1.ii.a). In essence, that was because it was symbolic AI which claimed to represent specific propositional content, and rational thought (4.v.d and 10.iii.a).

(In the 1980s, when connectionist AI became more widely known, some of the public opposition to NewFAI was generalized to cover that as well. Only "some": many members of the public were seduced by the connectionist propaganda machine—see Chapter 12.vi–x.)

The opposition to AI, and to cognitive science in general, was both theoretical and political—sometimes, closely interrelated. Mostly, it came from critics proud to be outsiders, and anxious to distance themselves from computational ideas in any way possible. But basic criticism occasionally came from insiders as well—arousing no little glee in the outsiders, of course.

Between the mid-1960s and the mid-1980s, a number of fundamental challenges arose which affected symbolic AI's practices and public relations—and which spilled over to some extent onto cognitive science as a whole. Each of them, to a greater or lesser degree, illustrates the claim made in Chapter 1.iii.a, that the Legend of wholly disinterested science is a myth. We can think of these challenges in terms of (literal) bombs and (metaphorical) bombshells.

"Bombs", because the US military's view of the psychology of decision making and rationality (Chapter 6.iii.a–b), and especially its sponsorship of AI technology for weapons and strategic planning, troubled many people. As the bombs got nastier, the opposition got more pointed. In the 1980s, when the bombs had become very nasty indeed, and the governmental decisions about their use increasingly irrational (by common-sense standards), the AI researchers themselves made concerted efforts to counter these lunacies. The bombs are discussed in Section i.

As for "bombshells", by my count there have been nine of those. For a criticism to qualify as a bombshell, it didn't have to be correct: some were, some weren't. But it did have to be so widely accepted by the general public and/or those holding the purse-strings for research funding that the field was undermined, or at best temporarily held back. The one exception to this was the early 1980s bombshell from Japan, a challenge which invigorated AI instead of damaging it (Section v, below).

The earliest bombshell of all has been described already: a US government committee report of 1960 that paralysed the funding of machine translation (MT) in the West, recovery coming only years later (see 9.x.f). (AI *as a whole* wasn't severely damaged by that particular blast, because the pioneering AI labs at MIT, Stanford, and Carnegie Mellon weren't doing MT: it's no accident that none of the "harbingers" of Chapter 10.i discussed it.) The latest, namely "situationism" AI, will be discussed in Chapters 13.iii.b–e and 15.vii–viii and xi. (The dust from that one is still settling.) So that leaves seven to be covered here.

Three philosophical bombshells, only one of which was detonated by a philosopher, are highlighted in Section ii, below. They were aimed at symbolic AI and cognitive science. Their original target was NewFAI-based work, but each was—and still is—directed also at later research.

Next, in Section iii, we'll consider an admonishment demanding better intellectual hygiene in AI—again, initially aimed at NewFAI but still sometimes needed today. Sections iv and v outline the effects, for both GOFAI and connectionist work, of an explosive charge from 1970s England, and of another (ten years later) from Japan: namely, the Lighthill Report and the Fifth Generation project.

Finally, Section vi identifies the early 1980s development that devastated some parts of the field (at least in the public eye) while clearing obstacles from others. That ultra-brief section heralds Chapter 12, in which this parcel of connectionist dynamite (whose fuse had been smouldering since mid-century) is described at length.

11.i. Military Matters

Mathematical research has long been exploited for military purposes. Even Archimedes was persuaded by King Hieron of Syracuse to put his beloved geometry to practical use (Chapter 2.i.a). That's why he designed the ship-destroying crane, and other weapons aimed against the besieging Romans in 212 BC. Nearly 2,000 years later, much the same thing was happening. But now, as well as geometry, there was AI.

AI's precursor, cybernetics, had already been used for urgent military purposes in the Second World War (4.vii.a). The techniques made available by AI as such could serve an even wider range of military purposes, involving reasoning as well as missile tracking.

This section outlines how the military helped give rise to NewFAI, and how the ups and downs of defence spending were reflected in changes in support for AI in general. Connectionism wasn't immune: as we'll see in Chapter 12.vii.b, DARPA's mid-1980s decision to fund it was driven by their hope for "next-generation, fire-and-forget, autonomous weapons" (DARPA 1988, p. xxv).

(Now, in the post-9/11 world, the military are expecting even more from AI. Reporting a significant rise in technical papers submitted to the 2005 'Innovative Applications of AI' (IAAI) conference, a journalist noted that "Various strains of AI are viewed as critical to homeland security, antiterrorism, and emergency response. Governments are allocating huge amounts of funding for AI research"—Hedberg 2005: 12. And in Europe the journal *IEEE Intelligent Systems* devoted a special issue to 'Artificial Intelligence and Homeland Security' in autumn 2005. One ludicrous result of this situation was a widely cast military request for predictive models of the emotions of terrorists: see Chapter 7.i.f.)

Cognitive science was often affected too. By the early to mid-1960s, for instance, even 'straight' linguistics and cognitive psychology were being generously funded in the USA for defence-related reasons, including the national loss of face caused by the 184 pound Sputnik in 1957, and the Soviets' space dog Laika, sent into orbit in Sputnik-II a few months later (6.iv.f; see also Koerner 1989). Of course, Sputnik didn't represent only loss of face. Potentially, it presented a military threat (so prompting the formation of ARPA, as we'll see): spies, and worse, in the sky.

As we saw in Chapter 2.ii.c, science depends on wealthy philanthropists of some kind. Today, AI is supported by many different institutions. These include academic grant-givers such as the USA's National Science Foundation and the UK's various research councils, and major charities like the Rockefeller and Nuffield foundations. But such is the wide range of AI applications that funds are available also from many governmental departments, and from private businesses—some of which have in-house AI teams.

Military and space-related sources (such as RAND, NASA, and Lockheed) are of course included. But war-related funding was even more important half a century ago. Indeed, *all* post-war scientific research was heavily in debt to the military. In 1947–50 the USA's Office of Naval Research alone was funding half of all the sponsored science being done at MIT (Mirowski 2002: 200). Ten years later, NASA too was a prime sponsor. Formed by President Eisenhower in July 1958 as the Aeronautics *and Space* Administration (replacing NACA: the National Advisory Committee on Aviation), this was a speedy—not to say panicked—response to the two Soviet Sputniks of a few months earlier. Where NACA's budget had been a mere $117 million, for 8,000 people, NASA soon received $6,000 million (for a staff of 34,000). Inevitably, some of that munificence would reach the universities—perhaps not for research in medieval poetry, but certainly for engineering and computing, and for cognitive psychology too.

This fact not only made AI possible, but to some extent affected the topics it chose to study. Only *to some extent*: even Noam Chomsky, no friend of the military, has allowed that "When MIT was funded maybe 90 per cent by the military it had no constraints on what it should do. As it has moved from the Pentagon to corporate funding there are more and more constraints" (quoted in Swain 1999: 28–9). (After 1973, this liberal attitude on the uses of military funding was tightened up: see below.)

In addition, AI's dependence on military funding, and the military support for computational psychology too, influenced public attitudes towards both fields.—Rarely, of course, for the better.

a. Nurtured in war

Warfare was part of the context even when NewFAI was merely a twinkle in Alan Turing's eye. Computer technology as such, and cybernetic control-systems too, were first developed for military ends (Chapters 3.v and 4.vii.a).

The Harvard Mark 1, for example, was largely funded by the US Navy. Howard Aiken was a commander in the US Naval Reserve, and Grace Hopper a lieutenant—later, an admiral. And the ENIAC was built for the US Army, via their Ballistic Research Lab in Maryland. (Unusually, the military resources were limited, for resistors were very hard to find; the ENIAC was ready on time only because someone discovered that one of the manufacturers had bins full of rejected samples, rejected not because they were physically faulty but because the colour markings were wrong—M. V. Wilkes 1982: 54.)

(Today, things haven't changed as much as one might think. "The most powerful publicly admitted [*sic*] computer in the world" in 1997 was mainly used for "*in silico* nuclear weapons testing"—J. M. Taylor 2001: 168. As for software, a single project was said by DARPA's director to have paid back "the entire investment that ARPA had made in artificial intelligence since the beginning"—namely, the conversion of an ARPA-sponsored program written for factory scheduling into a logistics aid for moving soldiers, tanks, etc. in the first Gulf War—Shasha and Lazere 1995: 220. And one doesn't have to be employed by the military to join in. In 2003–4 DARPA announced a cash award of $1 million for the first team whose autonomous vehicle could cross Death Valley, from Los Angeles to Las Vegas, within a specified time—DTI 2004: 34. If the prize is won by a gaggle of pacifist garage tinkerers, so be it: DARPA's intent is to use whatever ingenuity they can find, to accelerate a technology that could be used for military purposes.)

The one exception to the military dominance in early computing was the series of computers designed in pre-war Germany by Konrad Zuse (Chapter 3.v.a). By 1941, these included an operational version of the Z3: in effect, a stored-program von Neumann machine, capable of floating-point arithmetic—and potentially of 'semantic' computing too.

Fortunately, the German military (although it had partly funded Zuse's research) turned out to be interested in neither. Soon after being conscripted into the army in 1941, Zuse asked for leave to work on his machine (which could be used to calculate matrices predicting wing flutter in aircraft flying at different speeds). His application was supported by a letter from a respected professor. In his autobiography, he recalled the result:

> The Battalion commander, a major, summoned me, first informed me that as a completely new soldier I had no right to take leave anyway, and continued "What does it mean here [in the letter] when it says that your machine has applications in aircraft construction? The German *Luftwaffe* is top-notch, what needs to be calculated there?"—What was I to say to this? How could I have responded to this? Permission denied. (Zuse 1993: 56–7)

Zuse's cryptographic skills went to waste too: his work on coding wasn't needed, he was informed, because the Germans had the Enigma machine. (If he'd been told that Turing was already breaking the Enigma code, he'd have been as surprised as the major—not least, because he hadn't yet heard of Turing.) Instead, he was put to work on weapons development: the successors to the flying bombs that ravaged London in the last years of the war.

It was said later that when Adolf Hitler was told of Zuse's invention "he replied that he didn't need any computing machine, he had the courage of his soldiers" (Zuse 1993: 81). Zuse never learnt the truth about that titbit: "Fact or fiction?—It was a time of rumors." But it's clear that computing wasn't high on the German military's agenda.

Things were very different across the Channel, and across the Atlantic too. After the Second World War ended, the urgency had gone. And so had much of the money—but by no means all.

Whereas US defence spending after 1918 dropped to a minimum, in the post-1945 period it dropped only to just under half (see the graph in Molina 1989: 11). Money was still needed, the powers-that-be decided, to deal with the military challenges posed by the Cold War: e.g. the Berlin blockade of 1948, the Chinese revolution of 1949, the first Soviet A-bomb test (also 1949), and the Korean war in 1950–1. That war caused defence spending to peak again, and although it did sometimes decrease it never returned to the low post-1945 level.

During the 1950s, AI was getting off the ground. War-related funding was crucial (Edwards 1996, chs. 2 and 8; Mirowski 2002: 161–90). Some of the money was spent on obviously military applications. As early as 1953, for example, the US military asked a budding NewFAI scientist (Alex Bernstein) to try to simulate Washington DC's missile air defence system (McCorduck 1979: 154). But much of it was spread more widely.

For instance, mid-1950s MT research in the UK, USA, and Soviet Union was being funded by military bodies. The US Navy, as we've seen (Chapter 9.x.a), was supporting British MT work focused on such blamelessly pacific sentences as *agricola incurvo terram dimovit aratro*. That may seem odd—but if one could translate sentences about farmers and ploughs, one could probably translate sentences about admirals and battleships as well. And while the Navy was sponsoring descriptions of the ploughing farmer, and Frank Rosenblatt's perceptrons too (12.ii.f), one of its sister services was developing "techniques of air warfare" at the RAND Corporation.

RAND—the acronym stands for Research ANd Defence—had been founded in 1946 by the US Air Force (with Douglas Aircraft, who dropped out two years later). The aim was to apply game theory and systems analysis (developed during the Second World War) to weapons design and to nuclear strategy. Throughout the 1950s, its computation and software development centre was one of the largest in the world. For computers were *essential* to RAND's purposes: planning for nuclear warfare could be done only by simulation. In addition, RAND employed psychologists and other social scientists to improve the efficiency of the human beings in warfare's technological loops. So Allen Newell and Herbert Simon were (separately) hired to focus on air defence simulations and game-theoretic/bounded-rationality models of warfare, respectively (Chapter 6.iii).

At the end of the 1950s Paul Armer, then heading RAND's Computer Science Division, wrote an influential survey-cum-prospectus of AI (Armer 1960/1963). This was published not by an academic journal, but by the Wright Air Development Center

(Wright, as in Wilbur and Orville). That wasn't surprising, given RAND's Air Force roots. A full third of the paper was devoted to Russian attitudes to AI (these were divided, as they were in the West), and to the Russian research that was going on in the area.

Clearly, and unsurprisingly (given the global reach of the Cold War mentality), there was a good deal of that. One Soviet scientist had remarked (speaking in English) that simulating the brain with a computer was "*the* number one problem"—and, said Armer, the emphasis was clear in his intonation (p. 402). The Russians were already discussing "chess playing by machines, and the deciphering of ancient Mayan manuscripts" (p. 404). Trivialities, perhaps, like the ploughing farmer. But Armer made it very clear that he agreed with the President of General Dynamics Corporation that "if the area has real military or psychological value to them, they'll put massive concentration on it" (p. 404).

In that, of course, the Soviets weren't alone. Defence spending in the USA tracked the national politicians' view of military threat. After Korea there were several more peaks:

* one in the post-Sputnik and Cuban missile era,
* one linked to Vietnam (where US involvement lasted from 1965 to 1973),
* and one—in 1992, still rising—in connection with President Reagan's military programme of the early 1980s (see Figure 11.1).

Some of this money went on building more bombs. But much of the R & D tranche was spent on the development of information technology—primarily hardware (including $3 million from DARPA for the ill-fated Connection Machine: Hillis 1985), but also

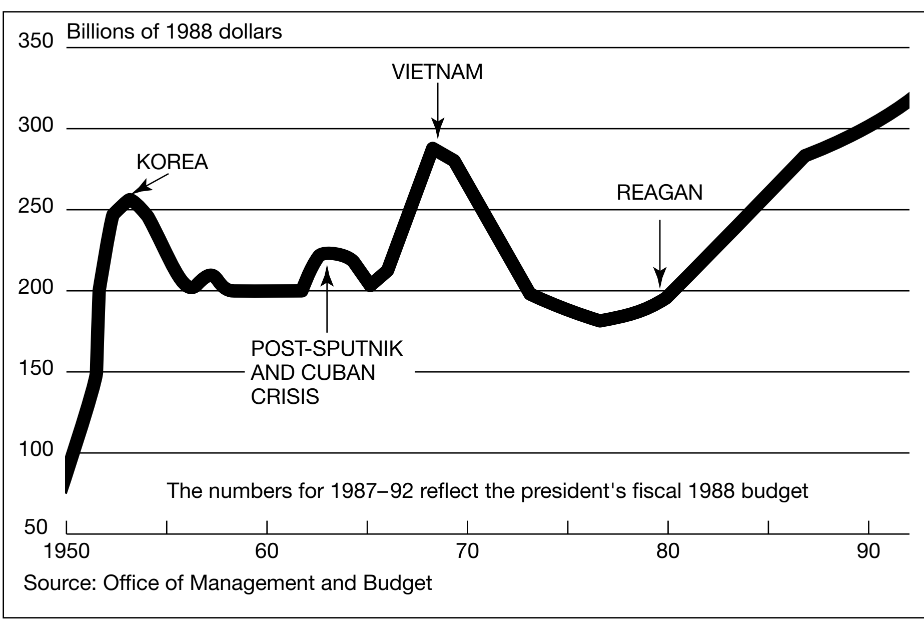


FIG. 11.1. Total volume of defence outlays in the USA (1950–early 1990s). Constant 1988 dollars. Redrawn with permission from Molina (1989: 17)

software. Indeed, creating new sorts of bomb required a huge investment in new computers, and in new ways of using them. (For some fascinating, if frightening, details, see MacKenzie 1991*a*,*b*.) In effect, the microelectronics industry we take for granted now was developed by the military. It was RAND's 1950s efforts in computer building, for instance, which led IBM—despite Thomas Watson's early scepticism (2.iv.c)—to decide to manufacture digital computers (Edwards 1996: 122).

The vicissitudes of the Department of Defense's (DOD's) budget, and especially the early 1980s funding rise (subsection c, below), also influenced the development of AI. One change—not in the amount of money, but in its favoured recipients—was due to the Mansfield Amendment of 1970: ARPA's annual budget of $26 million for information-processing research was now directed only to *military* applications (Chapter 6.iv.f). But various other changes affected the *amount* of money available. The course of US government funding for mathematics and computer science (i.e. excluding the bombs, as such) is shown in Figure 11.2.

b. Licklider as a military man

In the 1950s and early 1960s, military aims—and therefore funding—were already suspect to some people. Probably most, however, considered them respectable—after all, military efforts had recently saved the world from Nazi/Imperial-Japanese domination.

AI benefited hugely from that fact, not least because Joseph Licklider was in the second group:

I had this little picture in my mind of how we were going to get people and computers really thinking together. [And I thought that] "command and control essentially depends on interactive

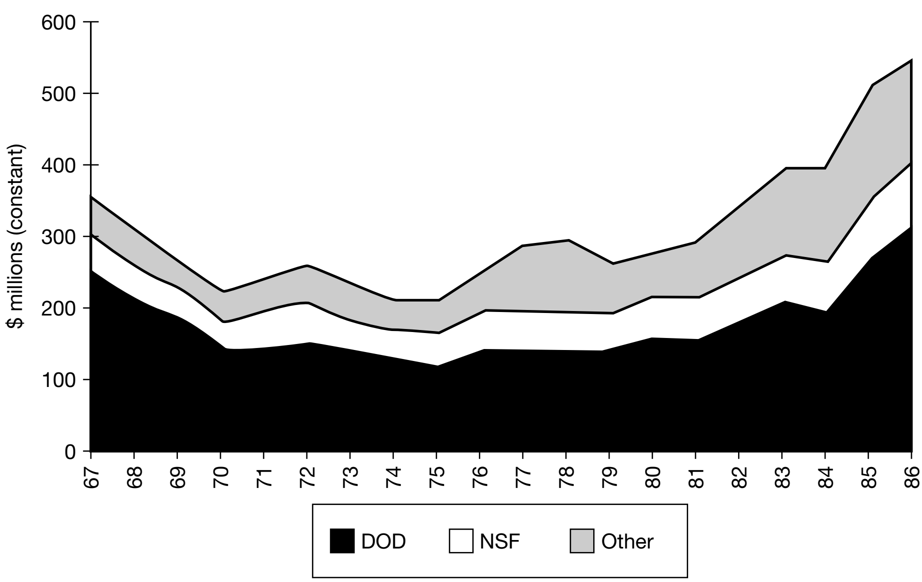


Fig. 11.2. Federal funding for mathematics and computer science, 1967–86. Data: Flamm (1987: 46). Redrawn with permission from Edwards (1996: 283)

computing [i.e. time-sharing] and there isn't any interactive computing so the military really needs this". *I was one of the few people who, I think, had this positive feeling toward the military.* It wasn't just to fund our stuff, but they really needed it and *they were the good guys.* (Edwards 1996: 267; italics added)

Today, Licklider is widely remembered as the prime sponsor of the technological advances in computer communication that led from the pioneering ARPAnet (first suggested in 1967) to the Internet (Waldrop 2001). Besides providing the money, he'd also provided the vision—in his influential essay on 'The Computer as a Communications Device' (Licklider and Taylor 1968). But what's more relevant here is that he had a crucial formative influence on AI in its earliest days. So his attitude to the military mattered.

ARPA had appointed him the first director of their information-processing section in 1962 (see Chapter 5.iv.f) largely because of his visionary 'Man–Computer Symbiosis'—which cited several early AI programs, both symbolic and connectionist (Licklider 1960). One historian has said that this paper "rapidly achieved the kind of status as a unifying reference point in computer science (and especially in AI) that *Plans and the Structure of Behavior* [see 6.iv.c], published in the same year, would attain in psychology" (Edwards 1996: 266). It wasn't a detailed review, orienting future AI research. That place was held by Minsky's 'Steps'. Rather, it was a vision of some potential *applications* of AI.

The 'Symbiosis' paper forecast the use of interactive (time-sharing) AI systems as assistants to human beings in many situations—explicitly including military command and control, where real-time functioning is often crucial. The idea of time-sharing had come from John McCarthy. Indeed, Licklider confessed to having undergone "a religious conversion to interactive computing" as a result of McCarthy's lectures at MIT (Edwards 1996: 264).

He'd been influenced also by McCarthy's harbinger paper of 1959 (see 10.i.f). Although the computers would provide speed and accuracy and the humans flexibility and common sense, he insisted that the contributions of the two sides couldn't easily be teased apart (hence, "symbiosis"). For instance, he said, theorem-proving programs could learn from experience (as McCarthy had suggested), and the SAGE program could suggest courses of action which the human being may not have considered.

SAGE was an ambitious *military* program, whose interface had been designed by Licklider himself (Edwards 1996, ch. 3). The interface was almost the first in which information was shown on a VDU; and different graphic displays could be brought onto the screen by touching it with a lightpen. Licklider's experience as a psychologist of perception was being put to good practical use.

SAGE was the first large computerized system for "C3": command, control, and communications. The "SA" in the acronym stood for Semi-Automatic, marking the man–machine symbiosis he'd recommended. (The "GE" stood for Ground Environment.) The machines, in this case, included a line of radar stations in Canada—and used as much power as a small city (J. McCarthy, personal communication). In other words, SAGE was the 1950s predecessor of the fully automatic "Star Wars" of the 1980s (see subsection c, below).

When Licklider joined ARPA, it was only 4 years old. President Eisenhower had set it up early in 1958, in a near-panic response to the Soviet Union's Sputnik (launched just a few months earlier, in October 1957). Some scientists feared that this political panic would sideline non-military research, or even squeeze it out of consideration—and funding—altogether. For instance, the Stanford biologist Joshua Lederberg recalls:

> Starting with the observation of Sputnik . . . I had set out [in late 1957–early 1958] to assure that fundamental biological science was properly represented in the programs of space research that were just emerging. *The danger was that scientific interests would be totally submerged by the international military and propaganda competition.* They have never gained first priority; they might have been totally excluded. (Lederberg 1987: 17; italics added)

Given Licklider's background (not to mention the benefit of hindsight), it's not too surprising that he also appreciated "fundamental" scientific interests. And where Licklider led, ARPA (and ARPA's money) followed.

ARPA's newness was one reason why Licklider was able to have such a seminal effect on the organization's policy. Another is that, as a member of MIT and as one of the Macy participants, he was already close to many other cyberneticists and budding cognitive scientists. Besides Donald Hebb, whose *magnum opus* he'd commented on in draft (Chapter 5.iv.f), these included Norbert Wiener, Jerome Lettvin, Warren McCulloch, George Miller, McCarthy, and Minsky (and many others too—Edwards 1996: 263–4). Indeed, his long-standing links to Minsky would be wryly remarked by many people when DARPA funds for connectionism dried up in the late 1960s (Chapter 12.iii.e).

On his appointment to ARPA, Licklider funded not only time-sharing (which he'd helped design: 10.i.h) but also AI—including psychological AI (Licklider 1988). He'd started out as a psychologist himself, working alongside Miller on speech and psychoacoustics; and he helped to design the HEARSAY speech-processing project (Newell *et al.* 1973). Indeed, he'd set up MIT's first Psychology programme in the late 1940s. If that hadn't been so, early AI would probably have been very much more "technological" in aim than in fact it was.

He soon sponsored MIT's ambitious Project MAC, launched in 1963. The acronym was spelt out sometimes as Man And Computer, sometimes as Machine-Aided Cognition, and sometimes as Multi-Access Computing—i.e. time-sharing. Project MAC's funding was exceptionally generous: $2 million as the start-up sum, and $3 million for each of the following years. The young AI Lab at MIT benefited to the tune of nearly $1 million. (All in early 1960s values.)

In addition, Licklider funded the first four US graduate programmes in AI—at MIT, CMU, Stanford, and SRI. For the next fifteen years, he continued to provide large amounts of money to these four groups, his attitude being that ARPA money should go to the people he knew to be excellent scientists (see 10.ii.a). Unlike the more 'academic' National Science Foundation (NSF), ARPA didn't have to function via peer review. In effect, this was an old-boy network from his days at MIT and Bolt, Beranek & Newman: the favoured few—notably, Minsky and McCarthy (and Newell and Simon)—were given the money and left to get on with it.

If Licklider retained his faith in the military as "the good guys", many members of the general public didn't. The military's respectability was soon undermined, in their eyes. This was one aspect of the counter-culture that emerged in the late 1960s, and which was strengthened by the psychological aftermath of warfare and US defeat in Vietnam (1.iii.c).

Although the US politicians stopped well short of wearing flowers in their hair, they did listen. In the mid- to late 1970s, there was less money available for R & D in America. MIT, for example, suffered financial difficulties across the whole institution as a result (see 14.vi.a).

To be sure, this situation was largely due to OPEC's sudden tripling of the oil price in 1973. Further price-hikes followed: in a ten-month period in 1974, the price of a barrel of oil rose 228 per cent (Boal *et al.* 2005: 14). But in addition, a greater proportion of the R & D tranche was socially directed. For a "new set of demands and values" was in play, in reaction against those which had previously driven technological development (Molina 1989: 24). An important part of the counter-cultural movement was:

> [a] critique of the social consequences of unfettered technological development, ranging from the environmental damage caused by the side-effects of modern science-based production processes to the use of sophisticated electronics in the war in Vietnam. (Dickson 1984: 30)

As a result, defence expenditure declined (as a percentage of GNP, it dropped from 2.9 to 2.3—Molina 1989: 24). This meant, among other things, that there was less money available for AI research. A very cold wind was blowing: ARPA's cutbacks to AI funding in 1974 had several "two-million-dollar-a-year contracts cut to almost nothing" (Hans Moravec, interviewed in Crevier 1993: 117). And ARPA cut back its support of *basic* research still further, accordingly (*THES* 1975).

But that state of affairs didn't last. By the late 1970s, the "new set of values and demands" had been sidelined by the economic crisis that followed the recent oil shocks. One historian of technology says:

> *[The counter-cultural] demands for greater social responsibility and accountability in the development of science and technology were superseded.* Such demands could not provide an impetus similar to war or international competition—either politico-military or economic. This period of socially-aware demands was a time of little growth for the R&D system. As soon as the spectre of the Vietnam defeat began to fade, replaced by alarm at the economic crisis, war and international competition began to reassert themselves and, with them, the same dominant social interests [namely, those of the scientific, corporate, and military elites]. (Molina 1989: 26; italics added)

By the early 1980s, then, American R & D in computing was rising steeply again, largely due to increased defence spending (see subsection c).

The AI community benefited, not least because part of the DOD's agenda was to advance US information technology *in general* so as to meet the Fifth Generation challenge (Molina 1989: 76–7; see v.a–b, below). The politicians were persuaded to authorize the vast sums involved partly because of fears that the USA's economy would be outstripped by Japan in the coming information age (Roland and Shiman 2002). The $4 million provided to NASA in 1985 for space-related AI (which they'd been supporting at a lower scale since 1982) was justified not only by national pride with respect to the space race, but also by the expectation of technological spin-offs. The law passed by Congress to authorize this stated that "the development of [advanced automation and robotics] shall be estimated to cost *no less than 10 per centum of the total space station cost*" (Montemerlo 1992: 51; italics added).

By the late 1980s, the annual budget for NASA's AI research programme had risen to $13 million, and it stayed at this level until at least 1992. But not all of this new money had come in response to the Fifth Generation. Nor was it attributable to Licklider's efforts. Rather, it was the result of a highly controversial political decision.

As such, it wasn't welcomed by the AI community as a whole—as we'll now see.

c. Star Wars and AI qualms

Some members of the AI community had been wary of accepting military money from the start. Wiener had stopped all his military work in 1946, at the end of the Second World War (so Theodore Roszak's impassioned attacks on him had been partly misguided: see 1.iii.c–d). Unlike Licklider, they didn't see the military in uncomplicated terms as "the good guys".

Vietnam provided further ethical complications. And the burgeoning cruise-missile technology added yet more. Benjamin Kuipers (1949–), for instance, had been funded as a graduate student by NSF and "didn't think much about military funding and AI research at that time" (Kuipers 2004). On graduating in 1974, he accepted one year's funding from DARPA before changing from AI vision to AI in medicine: for DARPA, the only agency willing to fund his vision work, wanted to use it for developing better-targeted cruise missiles.

Kuipers (a conscientious objector during the Vietnam war: see ii.e, below) may have been more scrupulous than most. But ten years later, the ambivalence had spread—and deepened. The AI profession of the mid-1980s was highly uneasy about the project then gobbling up most of the generous AI funding. This was the "Star Wars" programme, or Strategic Defense Initiative.

Star Wars was announced on national TV by President Reagan himself, in March 1983. Immediately, DARPA was allocated $600 million—later rising to $1 billion—over five years to develop the computing required (Roland and Shiman 2002). (The similarly named Strategic *Computing* Initiative had already been planned by DARPA, as its response to Japan's Fifth Generation project. In the event, SCI was swallowed up by SDI: Edwards 1996: 298–9.)

Most of this huge sum of money, DARPA decided, would be devoted to five specific aims, each involving AI at its core (Edwards 1996: 296). These were an intelligent image-processing system, for battlefield reconnaissance; two AI battlefield management systems; an autonomous land vehicle for use by the US Army; and an intelligent fighter pilot's assistant, which would possess 10,000 words of English and be able to understand the pilot's speech even in a noisy jet cockpit.

You may smile, realizing the many impracticalities being blithely ignored. But in the geopolitical circumstances of that time, it wasn't funny. The least funny aspect of all was that man–computer *symbiosis* had virtually disappeared. The goal of Star Wars (or so people were told: see below) was to provide an AI shield to defend the USA—if necessary, by triggering machine-made attacks. So Star Wars computers would not only detect an imminent Soviet attack but would also deploy an *automatic* nuclear response. (Hence the DARPA director's assurance to a US Senator that "we might have the technology so he couldn't make a mistake", where "he" was the President of the United States, faced with the awesome decisions required in a nuclear confrontation—Edwards 1996: 71.)

This removal of the human being from the loop caused explicit opposition from AI scientists around the world. And from many others too—including Hollywood directors, literary theorists, sociologists, and philosophers. For instance, the hugely successful 1984 film *The Terminator* played on people's fears of failures in early-warning systems, as well as their distrust of AI technology in general (Edwards 1996: 22–6). And Donna Haraway's monitory *Cyborg Manifesto* was written at much the same time (Chapter 1.i.b).

But one didn't have to be a counter-culturalist to be sceptical. The historian Paul Edwards states that Reagan "did not bother to consult Pentagon analysts before announcing the plan", but that "had he done so, *most would have refused to support it*" (1996: 289; italics added).

Computer scientists, of course, were in a special position to understand the technical problems. They knew only too well that a program consisting of 10 million lines of code (which was what the Star Wars directorate was proposing) couldn't, in practice, be bug-free.

What's more, they knew that even a bug-free program won't always do *precisely* what it was intended/expected to do. All the standard programming languages were ambiguous. This applied even to the supposedly mission-critical Ada (named after Ada Lovelace), which the DOD in 1975 had insisted be used for any real-world military application. That is, a given instruction in Ada (or any other source code) could be translated in different ways by different compilers when it was run. And *concurrent* execution was often ambiguous too, in that the programming language left it open for the compiler to choose which instruction should be followed first (E. Robinson, personal communication). In time-critical real-world applications, this might matter.

Later, a very carefully defined subset of Ada, called SPARK, would avoid ambiguity entirely (Chapman *et al.* 1994*a*,*b*; Chapman 2001). In the 1980s, however, it was rife. Admittedly, this rarely (if ever) caused problems in practice. But many felt that launching nuclear weapons wasn't a context in which to take that for granted.

Over and above those highly general points, AI professionals had by then learnt some lessons. The days of the "ten years" hype were (mostly) over. They knew that many of the AI problems assumed by Star Wars protagonists to be solvable were intractable in practice, and perhaps even in principle. These included real-world examples subject to the frame problem, and the (proposed) automatic programming of complex and bug-free code. Moreover, they'd learnt that AI successes could often be derived only by heuristic programming, not by success-guaranteed algorithms. Where an inappropriate decision could have such dire effects, the essentially fallible nature of heuristics was more than a little troubling.

As if all that weren't enough, several failures in early-warning systems *had already happened*. In one case, an unusual (so hard-to-spot) fault in a 46 cent computer chip had caused the North Atlantic Defence (NORAD) computer to report a Soviet attack, and 100 B-52 bombers had been readied for take-off (Edwards 1996: 285). In another, a major alert had been caused by someone's forgetting about leap years when programming the calendar, needed for predicting when and where the rising moon would appear in the sky (H. Thompson 1984). If even something so straightforward as the calendar could be misprogrammed, how much greater was the potential for mistake lurking in the arcane science and mathematics built into nuclear warning devices. In one eighteen-month period (after NORAD installed new computers), there'd been no fewer than 147 false alerts—four of which had moved the US defensive forces one step closer to a nuclear response.

The public concern was so great that the US House of Representatives held two days of hearings in May 1981 on "Failures of the NORAD Attack Warning System". These hearings "revealed a long and mostly secret history of spectacular failures in the computerized . . . Ballistic Missile Early Warning System" (Edwards 1996: 284).

All this explains why, when US Vice-President George Bush (senior) visited England in 1985, he was greeted by a media campaign criticizing the technical assumptions implicit in the Star Wars project. That campaign included a letter signed by seventy British computer professionals, including some mentioned elsewhere in this book. At the same time, the British Computer Society organized several meetings to look into the military implications of computing.

Bush didn't have to go abroad to encounter such resistance. There was plenty of it at home:

> A pledge not to take SDIO [Star Wars] funding, in order not to support *the impossible fantasy* of effective nuclear defense, circulated widely on [US] university campuses. Thousands of scientists signed. (Edwards 1996: 289; italics added)

On both sides of the Atlantic, the recently founded CPSR (Computer Professionals for Social Responsibility) was one of many sources of professional criticism. CPSR gave strong backing, and publicity, to the computer scientist David Parnas (1941–). Besides running several pieces by/about him in their own newsletter, they sponsored a volume on *Computers in Battle: Will They Work?* (Bellin and Chapman 1987), in which one chapter was written by Parnas.

Parnas had been invited to give evidence on this question to the US Senate. In December 1985 he assured the senators that Star Wars *couldn't* be made to work. Moreover, he said, since mistakes were so likely and their effects so horrendous, it shouldn't even be attempted.

His evidence was especially weighty because he'd been deeply involved in defence-sponsored projects. He'd done eight years' study of real-time software to be used in military aircraft, and from 1979 to 1982 he was head of software engineering research for the US Navy. So far was he from having any *political* axe to grind in these matters, that in June 1985 he'd accepted the US government's invitation to join their top advisory group on the computing aspects of Star Wars (the SDI Committee on Computing in Support of Battle Management).

Soon after the committee's first meeting, however, he'd resigned. He published his letter of resignation (with eight brief explanatory appendices), which later formed the basis of his Senate testimony (Parnas 1985*a*,*b*, 1986, 1987; cf. D. B. Davis 1985). So Parnas's protest wasn't a principled anti-militaristic, or even anti-nuclear, one. Rather, it was pragmatic: war is sometimes necessary, and so are nuclear weapons—but *this* military strategy was just too dangerous to use.

(This all seems so obvious. So why didn't Reagan listen? Why didn't he halt Star Wars forthwith? Indeed, why had he suggested it in the first place?—One common answer is that its being "an impossible fantasy" didn't matter, because the primary aims weren't military at all. Rather, they were political and ideological: Reagan intended to bankrupt the Soviet Union, which would be bound to try to respond in kind even though it couldn't afford to do so. Whether it was his original intention or not, that's what happened. With the fall of the Berlin Wall in 1989, and the break-up of the USSR soon afterwards, the political "need" for Star Wars disappeared. The fear lessened—and, as one might expect, the Federal money available for AI lessened too.)

d. *Les mains sales?*

Given the insuperable technical difficulties detailed by Parnas and others, and the huge risks to humanity implied by the nuclear dimension, it was easy for AI scientists to disapprove of Star Wars. As in the case of Parnas himself, one didn't have to hold an especially subtle and/or left-wing moral–political position to want to have nothing to do with the project. And it was relatively easy—at least for those employed by universities, and/or those outside the USA—to refuse to accept grants for work that was directly, explicitly, related to Star Wars. But deciding on one's position with respect to military work *in general* was more tricky (Ladd 1987).

Some individuals accepted money only for military research that was relatively benign in intent. After all, autonomous vehicles could prevent human soldiers from getting killed, as bomb-disposal robots do. (Several decades later, the title chosen for the 2006 International Conference on Automation and Robotics, which dealt with "demining, search/rescue missions, homeland security . . . etc.", was 'Humanitarian Robotics'.) Some, such as Kuipers (a Quaker), refused to work on any military projects. However, others objected that for non-pacifists to do that was as squeamishly hypocritical as for meat-eaters to refuse to work in abattoirs. The British AI scientist Yorick Wilks declared that if our soldiers had to risk their lives on our behalf then he, safe at home in his laboratory, was glad to support them. And to refuse *any* military funding was to be cut off from the most lucrative grants (so one could employ only graduate-student assistants, not research fellows: Kuipers 2004). In short, AI people faced dilemmas like those facing the hero of Jean-Paul Sartre's *Les Mains Sales*.

One could be ambivalent about this, even in the midst of a terrible war. Zuse, for instance, had had mixed feelings about his work on flying bombs:

> Technically speaking, our assignments were very interesting. The fact that the sophisticated weapons we were working on in the final analysis served war and death, was easily forgotten. And yet we all would have preferred working on civilian projects. (Zuse 1993: 60)

(Tom Lehrer might have taken that claim with a pinch of salt, for when Zuse's workplace was bombed in 1945 he spent a few weeks working with the Lehrer-immortalized rocket scientist Werner von Braun.)

AI researchers varied widely in their preparedness to accept/seek military funding. In Edinburgh, for instance, Bernard Meltzer was wholly against accepting grants from military sources whereas Donald Michie baulked only at doing *classified* research. MIT in the 1960s adopted a policy of doing no classified research on campus, although its special laboratories, such as the Draper and Lincoln labs, did do military work. (It wouldn't need a cynic to say that that's why they were founded in the first place: the Lincoln lab was set up in 1953–4 to design SAGE—Rheingold 2000: 142–3.) McCarthy and Edward Feigenbaum, by contrast, had no problem with accepting money for classified purposes (Fleck 1982: 206).

Feigenbaum even argued—both in a best-selling book and in evidence to a Congressional hearing—that the Fifth Generation project threatened the USA's military superiority, which the government should take urgent steps to preserve (Feigenbaum and McCorduck 1983: 215–20). He became a national leader in this area of applied AI (and today, specifically mentions it on his web site). He was also more up front than most in acknowledging the military's role in getting AI off the ground:

> When no corporation or foundation chose to take AI seriously, or could afford to, the Advance [*sic*] Research Projects Agency (ARPA) of the Department of Defense supported it through two decades of absolutely vital but highly risky research. Since the Pentagon is often perceived as the national villain, especially by intellectuals, it's a pleasure to report that in one enlightened corner of it, human beings were betting taxpayers' money on projects that would have major benefits for the whole human race. (Feigenbaum and McCorduck 1983: 215)

And in urging an immediate US response to the Fifth Generation project, he said, "It is essential that the newest technological developments be made available to the Defense Department . . . [which] needs the ability to shape technology to conform to its needs in military systems" (p. 217). Perhaps it's not too surprising, then, that he eventually became chief scientist for the US Air Force.

His Stanford colleague Terry Winograd (who'd founded CPSR with Brian Cantwell Smith) had a very different attitude—and avoided military funding as a result. He couldn't deny that military research may have beneficial spin-offs. (In applying for government funds, DARPA had made a point of stressing the likely commercial benefits from Star Wars research.) But he was very uneasy at the extent to which *everyone* receiving DARPA money—including about 85 per cent of the AI research done at the four major centres (MIT, CMU, SRI, and Stanford)—was now fairly directly implicated in military research (Winograd 1991). For DARPA had announced in 1983 that even basic research now had to be related to one of the five military aims specified above. (The mid-1970s policy shift mentioned below had merely required that it be relevant to military use in general.)

AI researchers' consciences were troubled also by the fact that even seemingly innocent AI work could be used for military purposes. That's why ARPA, which wasn't engaged in the disinterested pursuit of truth, had been prepared to fund it in the first place. Their mandate had specified the responsibility to fund research which the DOD wouldn't otherwise have supported "because the feasibility or military values of the new capabilities were not apparent at the beginning" (Edwards 1996: 260).

Consider, for instance, NLP research on anaphora, such as the little—but decidedly tricky—word *it*. MIT's Eugene Charniak (1972, 1973, 1974) wrote a program to analyse stories about children's birthday parties. One sentence was *He will make you take it back*. How could this be interpreted? Suppose the gift-giver had just said *I put the toy train on the table*: for sure, the table (mentioned nearer to *it* than the train was) would not be what the birthday boy would want his guest to take back. But how was the program to know that? When ARPA turned more mission-oriented in the relatively cash-strapped mid-1970s, eschewing basic research in favour of projects with military relevance (*THES* 1975), some NLP researchers encountered funding difficulties for the first time. A friend told me that he'd solved the problem by resubmitting his proposal, now using examples like *The battleship was spotted North-East of the rock. The gunner aimed at it*. This man was no more concerned with battleships than Charniak had been with toy trains: both were interested only in *it*. However, anyone writing an NLP-based battle management system would need to be able to cope with *it*.—Morally, then, there were no easy answers.

Specifically military research proceeded, regardless. By the mid-1980s, an impressive—or depressing, depending on your point of view—roster of (non-secret) applications had been developed. Some of these had advanced AI *as such*. That's not surprising, for real-world problems in general provide a useful discipline to the programmer. And military applications had some specially challenging features:

> Military operations . . . possess significant characteristics that have not always been prominent in other AI application domains. One such characteristic is the time-critical nature of tactical decision making—the need for appropriate, real-time response to dynamic situations. (J. Franklin *et al*. 1987: 605)

Typically, the "dynamic situations" weren't merely rapid (like the movement of a guided missile), but also highly complex. That is, they involved battles, not just single weapons. As a result, much military AI focused on increasing the efficiency of search and pruning techniques, and of *distributed* reasoning under uncertainty (see 13.iii.d)—where the data were not only uncertain, but also vast in quantity, multi-sourced, and used for different purposes by different types of personnel (J. Franklin *et al*. 1987: 606).

Among the many programs developed in this general area were one (called BATTLE) for deciding what weapons to send where, before and during a battle, and another (ANALYST) for integrating sensory information betraying the current location of enemy units (Slagle and Hamburger 1985; Bonasso 1984). Others were aimed at building (for example) "smart" bombs, mine-laying robots, and (non-intelligent) radio sensors. The latter were disguised as twigs, jungle plants, and animal droppings; and they were used for detecting movement, body warmth, the noise of truck engines, or even the scent of human urine (Edwards 1996: 3).

Clearly, then, the computer scientists of the 1980s weren't short of ingenuity. But political critics already saw them as morally compromised. Some of Chomsky's fiercest polemic, for instance, had been directed against the use of high-tech weapons on an undeveloped country halfway across the world (Chapter 9.vii.a). In his eyes, indeed, DARPA/RAND were doubly guilty. Not only had they invented these fearsome devices, but their game-theoretic strategists had advised that it was "rational" to use them (cf. 6.iii.a).

Today, twenty years later, robot planes and tanks are only too familiar. Indeed, the US Congress has approved a programme to make fully one-third of all US ground attack vehicles and deep-strike aircraft unmanned by 2010 (Almond 2005).

As for robot soldiers (i.e. robots used instead of soldiers), they're normally justified in terms of lessening the mortality rate of human soldiers—which, no doubt, they do. But one especially cynical (or especially honest?) military expert, the Director of America's "Global Security" research body, was recently quoted as saying this:

[Robots] will kill without pity or remorse. Historically, only about 1% of soldiers in real war are like that. They are sociopaths who these days are used as snipers. Half of the rest just spray bullets around and the other half never fire at all. (John Pike, quoted in Almond 2005: 21)

Given the cold bellicosity behind those words, it would be comforting to think that this newspaper quotation was as inaccurate as many of them are. But I wouldn't bet on it.

In sum, Archimedes and King Hieron live on: the link between AI and the military is as strong as ever. And still, expectations of AI are sometimes so unrealistic as to be risible. A US Army general in 1970, on being shown SHAKEY, asked whether a bayonet could be mounted on it—apparently assuming that it could skip across grassy meadows and crawl through ditches and tunnels . . . whereas in fact it could barely stand up (B. Raphael, personal communication). Three decades later, a US-government employee asked an AI researcher whether he could model the emotions of terrorists—something that's way beyond the state of the art (although the other AI workers he'd approached hadn't admitted this: see Chapter 7.i.f). Any opportunity to put AI to martial use, however fanciful, is eagerly explored. *Plus ça change, plus c'est la même chose.*

11.ii. Critics and Calumnies

Alongside the bombs, there were the bombshells. Between the mid-1960s and mid-1970s three waves of intellectual criticism washed over NewFAI, and the cognitive science projects associated with it. These were wide-ranging and widely disseminated critiques, whose influence is still strong today (see Chapters 13.ii and 16.vii).

The first, and most famous, came from an outsider: the phenomenological philosopher Hubert Dreyfus (1965, 1972). The next best known came from a neighbour: the computer scientist Joseph Weizenbaum (1976). And the third came from a highly respected AI insider (see Section iii, below).

Dreyfus's charge was twofold: that GOFAI wasn't succeeding, and that it never could. Weizenbaum's was that even if it could appear to succeed (which he, too, doubted), it shouldn't be used—at least, not in 'human' contexts.

Criticism, and the debate it engenders, isn't always good-tempered. These two critics repeatedly hurled calumnies at their opponents, who were tempted to answer in kind. The more passionate the writing, the less effective it was in changing the minds of the enemy, i.e. NewFAI-influenced cognitive scientists. Points of criticism that were both just and important received less attention than they deserved.

Since many supporters of Dreyfus and/or Weizenbaum were members of the general public already disposed to dismiss AI, these heartfelt criticisms spread like wildfire. The 1970s critique of GOFAI was no mere intellectual exercise, confined to the academy: it was a passion-ridden cultural phenomenon too.

a. The outsider

Hubert Dreyfus's first extended critique of AI bore the RAND Corporation imprimatur, and he was employed by MIT at the time, a formative point in his career (he was born in 1929). Nevertheless, he was an outsider.

He was a professional philosopher (with a first degree in physics), not an AI researcher. In the mid-1960s he was teaching the subject at MIT, but he soon moved to Berkeley's Philosophy Department (alongside John Searle). Even more to the point, his philosophy—a form of phenomenology taken from Martin Heidegger, Maurice Merleau-Ponty, the later Ludwig Wittgenstein, and Michael Polanyi (see 16.vi–vii)—was deeply antipathetic to GOFAI.

The RAND connection was due to his brother Stuart, a mathematician working there (and at MIT) at the time. He'd persuaded Armer, then head of RAND's Computer Science research, to invite Hubert to visit their Santa Monica centre for two weeks in the summer of 1964 (S. E. Dreyfus, personal communication). This sojourn on the Pacific coast would enable him to observe the AI work being done there by Newell, with a regular visitor from the other side of the continent—namely, Simon (Chapter 6.iii.b–c). So far as the siblings were concerned, he was going there to meet the enemy at close range.

It wasn't a friendly meeting, and it didn't lead to a truce. On the contrary, after his fortnight sojourn at RAND Hubert wrote a stinging ninety-page critique of GOFAI, based on a talk he'd given there in August 1964. Provocatively, he called it *Alchemy and Artificial Intelligence* (H. L. Dreyfus 1965).

Alchemy argued two different, though closely related, points. On the one hand, it declared the overall project to be in principle impossible for philosophical reasons—in essence, because the "higher" forms of intelligence are *necessarily* derived from "lower" forms concerned with bodily action (this claim is discussed in Chapter 16.vii.a). On the other hand, it mocked the performance of the programs that had actually appeared thus far.

Dreyfus accused NewFAI of four general performance failings, each missing out some "essential" aspect of human intelligence. These were reliance on the fringe of consciousness; discrimination between the essential and the accidental; tolerance of ambiguity; and perspicuous grouping. The lack of those aspects, he said, resulted in the two glaring weaknesses of NewFAI: crudity and brittleness.

Some types of intelligence, he allowed, were in principle programmable: "associationist" and "simple formal" thinking. But "complex formal" and "non-formal" intelligence weren't. The latter types included chess and "intuitive" theorem proving, and riddles and translation. Whatever successes NewFAI had achieved so far, and might achieve in future, lay within the first two classes of problem. These could be tackled by formalized "counting out" and "trial and error". Heuristic search wasn't a general answer, because AI heuristics were derived by human insight—and (in people) heuristics often need to be applied insightfully, too.

Moreover, he said, intelligence *did not* constitute a "continuum", as assumed by Armer and by Feigenbaum and his co-editor Julian Feldman (see the quotes given in 10.ii.b). On the contrary, there were fundamental qualitative differences between the four classes. In any event, it was likely that "the body plays a crucial role in making possible intelligent behavior" (H. L. Dreyfus 1965: 59). If so, then computers simply couldn't qualify.

One of the specific criticisms he made was that GPS relied crucially on its programmers' human ability to distinguish between the essential and the accidental, in choosing how to represent the problem. Another pointed out that pattern recognition programs belied the fact that human concepts are networks of Wittgensteinian family resemblances, not definable by necessary and sufficient conditions. Yet another rejected Rosenblatt's vision of a mechanical secretary: no one knew how to begin making such a device, Dreyfus said, because acoustically identical speech signals are heard as different phonemes depending on the "global" factor of expected meaning (but see 9.xi.g). Some others were directed at MT in general, Yehoshua Bar-Hillel being repeatedly quoted.

In essence, these criticisms weren't new. Quite apart from the by-then-familiar complaints about MT (9.x.d–f), the relevant features of human thinking had been stressed also by the Gestalt psychologists. Merleau-Ponty (one of Dreyfus's three major influences) had borrowed extensively from the Gestaltists, and had also highlighted the importance of having a *body* (16.vii.a; cf. H. L. Dreyfus 1967).

These aspects of thought had even been noted by the AI researchers themselves. Newell and Simon owed much to the Gestaltists (6.iii.c). As for the frame problem, which exemplified the 'essential/accidental' difficulty, this had been pointed out by NewFAI people in the 1950s and named by them in the 1960s. Moreover, ambiguity was being confronted (in as yet unpublished work) by Yorick Wilks (9.x.d), and perspicuous grouping by Adolfo Guzman (10.iv.b).

But although Dreyfus failed to acknowledge the extent to which NewFAI had already *recognized* such issues, his main claim was correct: attempts to *deal with* them, when they weren't simply ignored, had been crude. This, he said, wasn't a question of AI's being a very young science. Rather, the crudity was inevitable, because the last two of his four "classes" of intelligence were immune to AI methods. Famously, he compared NewFAI claims about progress to a man climbing a tree in order to reach the moon (1965: 17, 86). The climber would move a tad nearer to his goal, to be sure. But clambering up even the Californian redwoods wouldn't ever get him there.

Besides criticizing specific programs, Dreyfus scornfully quoted various wildly optimistic predictions made by NewFAI enthusiasts. Chief among these was Simon's forecast, in a talk given in 1957, that a computer would beat the world champion at chess within ten years (Simon and Newell 1958: 7). The prospects of that happening were already dim when he wrote *Alchemy*:

> As described in their classic paper, [Newell, Shaw, and Simon (1958*b*) admitted that] their program was "not yet fully debugged" . . .
>
> In fact, in its few recorded games, the NSS program played poor but legal chess, and in its last official bout (October 1960) was beaten in 35 moves by a ten-year-old novice (xxxi). Fact, however, had ceased to be relevant. [Their] claims concerning their still bugged program had launched the chess machine into the realm of scientific mythology. (H. L. Dreyfus 1965: 6)

The notorious chess forecast was one of many examples of hype in AI (both symbolic and connectionist) that were used by opponents to damage the field. Simon himself produced another gem a few years later: the hubristic "ten years" had been doubled to a more modest twenty, but the substance of the prediction had multiplied a thousandfold—"machines will be capable, within twenty years, of doing any work that a man can do" (Simon 1965: 96). Even if "any work" actually meant only office work (he was writing about his old love, management), this was optimism gone wild.

Dreyfus wasn't the first opponent to mock such claims made by AI researchers. Bar-Hillel, for instance, had preceded him (9.x.b. and e). Nor was he the last: Sir James Lighthill (iv.a, below) would target several of them.

There were plenty of less highly publicized criticisms over the years, of course. There's some evidence, for instance, that DARPA killed their programme on speech understanding in the mid-1970s not simply because the allotted five years had ended, but because they felt let down—even deceived—by the fact that the grammar used when speaking to the system had to be very highly constrained. I'm tempted to say that this should have been obvious to them right from the start (although SHRLDU may have led them to expect better), except that the research director himself has protested,

> But nobody had ever said anything about not constraining the grammar! Nobody even understood that that was a parameter that could be adjusted at the beginning of the project! (Raj Reddy, interviewed in Crevier 1993: 116)

That example reminds us that there are two types of hype. In honest hype, the optimists themselves are misled. In irresponsible or dishonest hype, they know that they're exaggerating (see iii.b, below). Dreyfus's arrows of contempt were aimed primarily at the first.

b. Scandal

Alchemy was no dry intellectual critique, but a passionate attack on a project perceived as the enemy. And it engendered responses in kind.

Dreyfus's mockery of the 1957 chess hype soon came back to plague him. In 1966, only a year after *Alchemy*, he himself was beaten by Richard Greenblatt's MacHack program in a match organized by Seymour Papert. (Greenblatt was still an MIT undergraduate when he wrote the program—in response to Dreyfus's earlier remarks.) The match was reported in the *New Yorker*, and in less exalted publications too.

Dreyfus's defence—that he hadn't claimed that no program would ever play a competent game, and that he was a "rank amateur" anyway—was true enough (1972, pp. xxxii–xxiii, 223 n. 45). (Legends are long-lived: he's still falsely accused of making "the blunt assertion that no computer program would be able to play a good enough game of chess to beat a ten-year-old"—Levy 1994: 89.) But he *had* said that any program play would be inhuman and mechanical. And Simon—not a disinterested observer, of course—described the 1966 game as "wonderful" and "a cliffhanger" (McCorduck 1979: 199).

So, in the eyes of the NewFAI community, Dreyfus had egg on his face. The incident delighted them, and prompted gleeful comments and witticisms in several issues of the SIGART newsletter.

Their satisfaction wasn't drily professional/technical, but all too human. Most of SIGART's readers loathed Dreyfus. That's hardly surprising, for the feeling was mutual. His contempt for their activities was unplumbed. Even his choice of title was insulting, intended (as his text made clear) as *Alchemy alias Artificial Intelligence*. Significantly, he had nothing good to say about alchemy. Years later, he'd mellowed enough to agree with Winograd's (1977) remark that "it was the practical experience and curiosity of the alchemists which provided the wealth of data from which a scientific theory of chemistry could be developed" (H. L. Dreyfus 1979: 2). His RAND fireball, however, *didn't* allow this. According to what he said there, chemistry had been substituted for alchemy—not developed from it.

The way in which Dreyfus had presented his arguments was at least as important in triggering the ensuing scandal (not too strong a word) as were the arguments themselves. This cat wasn't merely stalking the pigeons, but teasing and sneering at them unmercifully. The sharpness of his tongue is illustrated by this remark:

> This output of confusion [i.e. various NewFAI predictions about chess machines] makes one think of the French mythical beast which is supposed to secrete the fog necessary for its own respiration. (1965: 8)

NewFAI researchers, and people doing "cognitive simulation", were described (in Dreyfus's words) as credulous, smug, unscientific, casual, dogmatic, and naive. They were accused (again, Dreyfus's words) of self-delusion, methodological confusion, and absurdity; of spreading intellectual smog; and of preferring adventure to patience.

Newell and Simon (especially Simon) got the worst of it. Besides the epithets just quoted, they were said to show "either a will to obscure the issues or a total misunderstanding" of the Gestaltists' position, and to have "surreptitiously" introduced their own insight into their programs. And Simon's hugely over-optimistic chess prediction of 1957 was mocked over and over again. There were only two people for whom Dreyfus had a good word to say: "Only Shannon", he declared, "seems to be aware of the true dimensions of the problem" of ambiguity in language and perception (p. 75). And only Donald MacKay, he said, had seen that *analogue* processing, and/or "wet" engineering, might be needed for successful AI (cf. 4.v.b and 12.ix.b).

The mockery—and the philosophy too—alienated Armer, already an influential champion of AI (see 10.i.f). (Years later, he said that if he'd known about the brothers' 1962 attack on AI he wouldn't have invited Hubert to RAND in the first place—McCorduck 1979: 194.)

He wanted to suppress Dreyfus's paper, and had "a big squabble" with some RAND colleagues who liked it (interview in McCorduck 1979: 195). It's pretty certain that they weren't paid-up phenomenologists. So perhaps they liked it largely because they were 'pure' computer scientists, who saw AI as a betrayal of the proper standards of theoretical rigour (see iii.a, below). Or perhaps they were merely speaking out for academic freedom: let a hundred flowers bloom, and so on. (Unlikely: academic freedom wasn't high on RAND's agenda, and many of its reports were classified.) In any event, Armer faced a mini-rebellion in the RAND ranks.

After about nine months of this—while rumours about the draft spread from the Pacific to the East Coast, and across the Atlantic too (I remember being all agog to see it)—Armer allowed it to come out as a RAND memo in December 1965. This mimeographed paper was eventually followed by a properly printed version, in 1967. A few changes had been made, and some abusive remarks removed.

But by that time, of course, the first—venomous—version had been widely circulated. Indeed, it had already achieved a mention in the *New Yorker's* Talk of the Town column of 11 June 1966, which repeated Dreyfus's scornful remarks on the weakness of computer chess. Naturally, the AI community weren't pleased. Simon, the chief target of attack, was even more incensed by Armer's decision to publish than by the journalist's high-profile follow-up. RAND's imprimatur, he said, gave Dreyfus a credibility which was "really false pretences" (McCorduck 1979: 194).

Alchemy is usually thought of as Dreyfus's first public assault on the field. In fact, the earliest was an attack—co-authored with his brother—prompted by a 1961 meeting convened by MIT's School of Industrial Management to celebrate the Institute's Centennial Year. Among the interdisciplinary delights in that conference were an opening speech on scientists and policy making by C. P. Snow, and a paper on digital libraries that would have made Gabriel Naudé's hair stand on end (Kemeny *et al.* 1962). But NewFAI was featured too.

Besides a paper on EPAM by Simon and Feigenbaum, there was a general talk on AI given by John Pierce (of ALPAC fame: 9.x.e). This was followed by invited comments from Claude Shannon and Walter Rosenblith (introduced by Vannevar Bush), and then a General Discussion. Not liking what they heard, the two brothers wrote a note that was eventually published as part of the official record (H. L. Dreyfus and Dreyfus 1962). Ink was already being mixed with pepper: AI wasn't merely opposed, but scorned. (What later became the tree-to-moon argument appeared there as mountain-to-moon.)

However, an editor's footnote (on p. 321) admitted that their remarks "were not made at the Pierce session itself, but were submitted in writing a short time thereafter". Simon was enraged by that too, as he admitted later to Pamela McCorduck:

> It was not a discussion that took place during the meeting, it was an afterthought they had. And it was a nasty little diatribe about this preposterous stuff that was being peddled. (interview in McCorduck 1979: 193)

His rage would increase on publication of *Alchemy*, wherein the pepper had grown even hotter.

The brothers Dreyfus weren't the first to attack NewFAI, and the nascent cognitive science associated with it, in strongly emotional terms. As we saw in Chapter 9.x.f, Mortimer Taube (1910–65) had done this a few years before, in his book *Computers and Common Sense: The Myth of Thinking Machines* (1961). And the ratio of abuse to factual/analytic discussion had been even higher there. The psychologist Walter Reitman had commented:

> This book is the work of an angry man.... [Taube] concludes that an uninformed, science-worshipping public is being deceived, hoodwinked, and bilked of millions of dollars by electrical engineers and computer enthusiasts. (Reitman 1962: 718)

Reitman wasn't imagining things, for Taube's disdain near-scorched the pages. His remark that Turing himself displayed "the tendency of computer experts to be pontifical about subjects in which they have no competence" was a relatively mild example (Taube 1961: 51). He'd accused MT researchers of "writing science-fiction to titillate the public and to make an easy dollar or a synthetic reputation". And he'd specifically said that "the nation's monetary resources" were being wasted when used to support AI research.

Armer had bridled then, too—not least, because Taube's book led to a "[negative] climate of research in the field" (Armer 1960/1963: 389). Nor was he the only one. Besides the hostile review penned by Reitman, another attack was mounted by Richard Laing (1962), then a member of Arthur Burks's team at the University of Michigan and doing work on neural networks and automata theory (1961*a*,*b*). (Like several others in

Burks's group, he was later recognized as an early worker in A-Life: see Chapter 15.v.b, and Richard Laing 1975, 1977, 1989.)

Stuart Dreyfus (1962), by contrast, had approved of the book. (Simon "blew up at me in his office" at RAND, as a result: S. Dreyfus, personal communication.) In a review written for a friend's literary magazine, he did distance himself from it in some degree, saying that "not all of Dr. Taube's arguments are convincing" and allowing that "some innocent researchers are doubtless encompassed by some of Taube's sweeping condemnations". However, he described chess-playing machines as "a haven for frauds for centuries". (True, but hardly tactful.) And he continued:

> [The] state of the artificial intelligence art has been grossly exaggerated. It is further clear that the natural desire of the bored public for believable science fiction, of the newspapers for sales, of computer companies for publicity, and of hard-working scientists for adulation contribute to this systematic delusion. (S. E. Dreyfus 1962: 54)

He poured scorn on the "multitudinous, disorganized, helter-skelter projects" of AI, and on LT/GPS (and Simon's chess prediction) in particular. And he described Taube's volume as the first book on AI childcare, remarking: "Taube may not be a wise or sympathetic parent, but at least he is willing to stand up to the pampered child."

One sentence was prophetic. He didn't have a "non-existence proof" of intelligent machines, but: "Conceivably, someday, a philosophic study of positive attributes of intelligence might [provide one]." In short, his and (still more) his brother's major intellectual project was already being flagged.

If even Stuart Dreyfus remarked on Taube's sweeping condemnations, and his lack of wisdom and sympathy, it's not surprising that the book cut little ice with the AI community. Despite his international eminence, Taube's over-the-top rhetoric led most of them to overlook his criticisms. The brothers Dreyfus each had a quiverful of insults too, as we've seen. Naturally, these were resented by NewFAI folk:

> [Dreyfus's attack on AI contains] a set of almost defamatory personal allegations. (Papert 1968: 0–8)

> Dreyfus' mission does not end with showing that people associated with Artificial Intelligence are wrong or even foolish. He is called to expose them as obscurantists and liars. (III–1)

The infamous RAND memo, then, expressed—and aroused—so much antagonism that it wasn't taken as seriously by NewFAI people as it might have been. (And, at that time, its author had no international eminence to command respect.) AI-leaning philosophers, too, were largely unimpressed. That's evident in Daniel Dennett's "little scalpel job" (personal communication) of 1968 (his first publication), and in my own critiques as well (Boden 1972: 146–50, 222–3; 1977: 435–41).

The situation wasn't helped by the fact that Dreyfus had made many mistakes, and even some disingenuous remarks—and omissions. The errors were ruefully acknowledged even by Weizenbaum, who shared many of Dreyfus's worries about the AI project: "It was terribly incompetent. [Dreyfus] knew so little about computers, and made so many mistakes!" (interview in Crevier 1993: 123). But the disingenuousness was even worse than the errors, and both were noted by Papert (1968), in a paper mischievously called 'The Artificial Intelligence of Hubert L. Dreyfus: A Budget of Fallacies'.

Papert being the AI star that he was, and Dreyfus already a notorious intellectual gadfly, this was a high-visibility contest. And likely to be a fierce one, too. Papert's flair for combativeness was evident from the tone of the already well-known draft of *Perceptrons* (see 12.iii.b). He would pull no punches.

And so it turned out. Papert accused Dreyfus of "questionable honesty". He showed, for example, that Dreyfus had deliberately omitted a brief sentence from a passage by W. Ross Ashby (about Herbert Gelernter's program: 10.i.c) which would have made nonsense of his ill-informed criticism (Papert 1968: III-8–9). Similarly, he'd stopped short of quoting a sentence from Wiener which confessed to a huge weakness of chess programs (III-4). And he'd claimed that Simon said in 1962 that his 1957 chess prediction was "almost realized", whereas Simon had said no such thing.

As well as bad faith, there was a failure to do his homework:

> The most astonishing feature of Dreyfus' texts [the two versions of *Alchemy*] is their bibliography. His references to experiments on Artificial Intelligence are almost entirely confined to early work that has filtered into anthologies of "classical papers" [i.e. *Computers and Thought*]. (Papert 1968: 0–11)

One of the missing items was Guzman's work, which though still unpublished was in the air, and being done by "people who live within ten miles of him" (II-9). Dreyfus's ignorance of the literature was compounded by his ignorance of what the literature was trying to achieve. On that topic, *A Budget of Fallacies* outdid even *Alchemy* in its level of calumny. For instance:

> [His] discussion is irresponsible. His facts are almost always wrong; his insight into programming is so poor that he classifies as impossible programs a beginner could write . . . (Papert 1968: 0–2)
>
> [His] comments on actual projects [show] that he systematically misunderstands their purpose, their methods, and their difficulties. The reason is simple. He knows nothing about the technical issues and barely understands the language used. In addition he is sufficiently suggestible to take people as meaning what he thinks they must. (0–7–8)
>
> [He ludicrously alleges that Ashby misled people.] That "naive" readers might be misled is undeniable. But scientific writing is not addressed to the ignorant and the naive (even if they are professors of philosophy). (III-9)

(*Ouch!*)

If *Alchemy* was as bad as this, why bother with it? Surely Papert, as one of the half-dozen top people in AI at the time, had better things to do? He answered:

> I have been told that it is a waste of time [to rebut *Alchemy* in detail]. I have been told that only a pedant would object to the technical nonsense that pervades every paragraph of Dreyfus' papers about Artificial Intelligence since his real purpose is to provide insight into the rich subtlety of human intelligence. I have been told that his arguments must be read as literary conceits with rich "humanistic" content.
>
> I think it does matter. I sympathize with "humanists" who fear that technical developments threaten our social structure, our traditional image of ourselves and our cultural values. But there is a vastly greater danger in abandoning the tradition of intellectually responsible and informed enquiry . . . The steady encroachment of the computer must be *faced*. It is cowardice to respond by filling "humanities" departments with "phenomenologists" who assure us that the computer [cannot encroach further] into areas of activity they regard as "uniquely human".

Our culture is indeed in a desperately critical condition if its values must be defended by allowing muddled thinking to depose academic integrity. (Papert 1968: 0–2–3)

The protest about "filling" humanities departments with phenomenologists was aimed at the postmodern turn in Western society: *les événements* were already spreading far beyond Paris (cf. 1.iii.c–d, 6.i.d, and 8.ii.b–c). As we'll see below (subsection e), had Dreyfus written his RAND squib a decade earlier Papert might not have been so concerned to demolish it.

Papert could have given a more literal answer to the question "Why bother?" He could have said, "I wrote it because Armer asked me to." As it turned out, Armer was disappointed: RAND's lawyers forbade publication for fear of slander suits (McCorduck 1979: 196). MIT's lawyers were less nervous, or perhaps they weren't consulted. Papert's paper, after circulating widely in draft, eventually appeared (still unfinished) as a Project-MAC Report.

c. After Alchemy

RAND Corporation memos aren't found in airport bookstores—although *Alchemy*, despite Armer's attempt at censorship, was the most widely read RAND memo of all time. But Dreyfus soon started work on a book-length statement. On its publication in 1972, it became a best-seller overnight. Many members of the general public got their first extended vision of AI from Dreyfus's book.

What Computers Can't Do (1972) contained much more philosophical discussion than *Alchemy*, and was graced by a Preface written by Harvard's Anthony Oettinger (1929–), a well-known critic of MT. But the technical aspects were virtually unchanged. Indeed, most of the text of *Alchemy* was reproduced verbatim. (This explains why Dreyfus remarked that "the funny thing is, there was practically no additional response when the book was published"—McCorduck 1979: 195.) Some extra examples were added, to be sure; for instance, he pointed out (contra McCarthy) that being "at home" isn't simply a matter of being in a particular physical location but of owning (or renting . . .) the real estate in question—which can't be defined in logicist terms (1972: 150). But McCarthy's logicism wasn't new.

In the book's discussion of 'Ten Years of Research in Artificial Intelligence (1957–1967) [*sic*]', the AI programs that had been written since the original RAND squib were almost all ignored—even those trying to address the very problems which Dreyfus had highlighted in the mid-1960s. Apart from McCarthy and Newell–Simon, Dreyfus focused only on MIT. (Even so, Winograd, whose work had electrified the AI grapevine and was about to electrify the wider world, wasn't mentioned. This may have been because SHRDLU wasn't featured in Minsky's *Semantic Information Processing*, from which Dreyfus drew heavily.) AI work had been going on elsewhere too—but from his new pages, you'd never think so. As Bruce Buchanan put it:

One can only speculate why the author fails to acknowledge recent AI work. To this reviewer, and other persons doing AI research, programs developed in the last five years seem to outperform programs written in the tool-building period of 1957–1967 . . . One would hope that a criticism of a growing discipline would mention work in the most recent one-third of the years of work. (Buchanan 1972/1973)

The anti-GOFAI rhetoric, too, was as virulent as before. The ever-courteous Buchanan chided Dreyfus gently, but firmly:

> It is lamentable that the critique of AI in this book has taken the form of a popular-press attack on AI work. The author's phrases are damning but his arguments are not convincing. As the author mentions, the popular press has often given over-enthusiastic impressions of AI work; this book is written in the same vein, but with a negative sign. (Buchanan 1973)

A few leading GOFAI workers admitted that he'd said some things worth saying, and the GOFAI psychologist Zenon Pylyshyn (1974), while rebutting him at length, did so too. For example, Wilks (1976) allowed that "the naive, and from this distance in time, absurd over-optimism of many in early AI is now sad reading", and that—as Dreyfus had insisted—what Wittgenstein called the human "form of life" must underlie/inform natural language. He did say: "Dreyfus's empirical arguments are not sound, they disprove nothing." But he added:

> Whether or not its survey of research is fair, or its arguments are sound, the book has done a great deal of good in a field whose lack of self-generated criticism is scandalous. Dreyfus should review the field every five years and change his empirical arguments each time—AI workers should then be grateful and ask no more of him, for he would be doing them a considerable service. (Wilks 1976: 184)

In effect, Wilks got his wish. For Dreyfus's critique would run and run. Quite apart from other books and papers attacking AI (e.g. H. L. Dreyfus and Dreyfus 1986, 1988), *the* book turned out to have many lives. (It's still being reprinted roughly once a year.)

Dreyfus's book lived on in other outsiders' writings, too. Most of these authors were in his home discipline, philosophy (Chapter 16.vii). But a few had studied the psychology of human skills, and/or had experience—if only as observers—of GOFAI in practice.

One person inspired by him was Harry Collins, whose late-century critique of expert systems will be outlined in Chapter 13.ii.b. Another, who was initially inspired by Heidegger but who later took Dreyfus's work willingly on board, was David Sudnow (1978/2001). His subtle descriptions of the phenomenology of learning a motor skill, namely piano playing, were mentioned in Chapter 7.vi.h. In general, the descriptions of skills offered by the Dreyfus brothers and their followers often provided interesting data. What was missing, and what's required by cognitive science, was an *explanation* of those data. (In the last quarter-century, some such explanations were attempted: see 14.viii.b, 15.vii–viii, and 16.vii.)

An unrepentant second edition of *What Computers Can't Do* appeared in 1979, consisting of the first version unchanged but with an added Introduction, in which some post-1967 AI work featured. Winograd—who hadn't yet come out as a lapsed GOFAIer (see subsection g, below)—was now discussed, with respect to both SHRDLU and KRL (pp. 5–15, 48–55). Scene analysis and robotics made an appearance (pp. 15–25). And frames, conceptual-dependency theory, and KRL were explored in relation to early to mid-1970s research on knowledge representation (pp. 27–55)—see Chapters 9.xi.d and 10.iii. But the verdict was unchanged: all were declared valueless.

(Dreyfus, like Licklider and Bar-Hillel, had predicted from the beginning that a "symbiosis" of man and computer might achieve more than unaided human intelligence can—1965: 83. Now, he admitted that specialist applications such as DENDRAL were useful, but insisted that these had nothing to do with general intelligence—1979: 5.)

Dreyfus's ink was still peppery. Citing an MIT report that had described research on flexible motor action by robots as "somewhat dormant", he tartly remarked, "[We] can only take 'dormant' as a polite synonym for stagnant or even comatose" (1979: 25). And having praised Winograd for being "admirably cautious" in his claims about modelling language as a whole, he commented drily:

> [Everyone] interested in *the philosophical project of cognitive science* will be watching to see if Winograd and company can produce a moodless, disembodied, concernless, already adult surrogate for our slowly acquired situated understanding. (1979: 55; italics added)

In brief, cognitive science was chasing a philosophical mirage. GOFAI programs were "not at all promising as contributions to psychology" (p. 18).

This new edition opened with a mini-history of the field, which boiled down to *I told you so!* During the previous decade, he said, his critique "has been more or less acknowledged", and "the wishful rhetoric characteristic of the field has been recognized and ridiculed by AI workers themselves [specifically, McDermott: see Section iii below]" (Dreyfus 1979: 1). In sum:

> Almost everyone now agrees that representing and organizing commonsense knowledge is incredibly difficult, and that facing up to this problem constitutes the moment of truth for AI. (1979: 3)

d. Dreyfus and connectionism

If 1979 was "the moment of truth" for GOFAI, it was also the moment for the first multidisciplinary meeting on distributed connectionism (Chapter 12.v.b). If Dreyfus knew anything of the work reported there, he didn't mention it in his new edition.

Indeed, in a Panel Discussion held in New York five years later, he declared:

> The ability that people have . . . to have the right thing pop into their head . . . requires having images and having memories since it involves seeing the current situation as resembling earlier situations, where resembling is a tricky notion because resembling doesn't mean identical with respect to any particular features, *which is the way machines always have to analyze resemblance*, but simply overall similarity. (Pagels *et al.* 1984: 341; italics added)

He was right about the "trickiness" of the notion of resemblance. But if he'd known anything about PDP connectionism at that time (which in all conscience he should have done), he would never have said that machines "always" have to analyse it as sharing identical features.

Two years after that, it became abundantly clear, even to outside observers, that GOFAI was no longer the only AI game in town (12.vi.a–c). The American Academy of Arts and Sciences therefore organized a wide-ranging debate on AI. The brothers Dreyfus contributed a paper giving guarded approval to connectionism, and saying of GOFAI: "The rationalist tradition [in philosophy] had finally been put to an empirical test, and it had failed" (H. L. Dreyfus and Dreyfus 1988: 34; cf. Dreyfus 1972: 215). The editor of the trade book that carried this debate evidently agreed with them, for it bore the tendentious subtitle *False Starts, Real Foundations* (Graubard 1988).

Accordingly, Dreyfus started work on another 'revision' of his book, defiantly called *What Computers STILL Can't Do* (1992). Like the earlier editions, this attracted a great deal of attention. The journal *Artificial Intelligence* devoted almost an entire number to it, including reviews by McCarthy, John Haugeland, and Collins (among others), and Dreyfus's reply (Stefik and Smoliar 1996).

Dreyfus's new Introduction provided some predictable mockery of Douglas Lenat's CYC project (1992, pp. xvi–xxx; see Chapter 13.i.iii), and an equally predictable salutation to the now reconstructed Winograd and to Philip Agre and Lucy Chapman (pp. xxxi–xxxiii: see 9.xi.b and 13.iii.b). Mainly, however, it focused on PDP connectionism. Again, there came the *I told you so*:

> In retrospect, the stages of my critique of attempts to use computers as physical symbol systems [which a connectionist system is not] to simulate intelligence now fell into place. My early appeal to holism, my concern with commonsense understanding as know-how, Stuart's phenomenology of everyday skills, and the capacities of simulated neural networks all added up to a coherent position—one that predicted and explained why GOFAI research should degenerate [*sic*] just as it had. (1992, p. xv)

("Degenerate", because GOFAI was said to be "a paradigm case of what philosophers of science call a degenerating research program": p. ix.)

But Dreyfus's new essay was less a matter of *Three cheers for connectionism!* than of *So much the worse for AI!* AI of some (non-GOFAI) sort, he said, might not be impossible: "no one has been able to come up with such a negative proof" (p. ix). But the prospects didn't look rosy.

Dreyfus admitted that neural networks were superior to GOFAI in some ways (1992, pp. xiv–xxxvi; cf. H. L. Dreyfus and Dreyfus 1988). For instance, they allowed for family resemblances in concepts (12.x.b). They showed that skilled behaviour needn't involve abstraction of a *theory* of the domain (12.vi.e). And they demonstrated that learning could take place without all the relevant 'beliefs' being explicitly represented beforehand (12.vi.c).

But, he continued, "The commonsense-knowledge problem resurfaces in this work and threatens its progress just as it did work in GOFAI" (1992, p. xxxvi). So neural networks can make inappropriate generalizations, such as learning to recognize the presence/absence of shadows instead of tanks; and they'd probably "stupidly" fail to learn *our* generalizations, or to adopt our priorities, such as symmetry (cf. Chapter 8.iv.b). Perhaps only a neural net with cell numbers, architecture, and initial connectivities identical to the human brain could ever model our intelligence.

As for learning, supervised PDP learning is dependent on the human trainer. Even unsupervised (reinforcement) learning, if the network were to be intelligent like us, would require—he said—that it have needs and perspectives like ours, and a comparable sense of relevance. And, of course, artificial neural networks lack bodies, just as GOFAI programs (and robots) do.

In short, he wasn't hopeful: "it looks likely that the neglected and then revived connectionist approach is merely getting its deserved chance to fail" (p. xxxviii). Even technological AI, if aimed at implementing *general* intelligence, was futile. As for psychological AI, this was a waste of time.

e. The neighbour

Weizenbaum (1923–) was a neighbour not only in the sense that he was located in MIT's Computer Science Department, but also in the sense that his ELIZA program was regarded by the public as an example of NewFAI. That was a mistake, for ELIZA hadn't been an effort in AI (see 10.iii.a). However, this wasn't widely known. The publicity afforded to his mid-1970s attack on AI was therefore all the greater.

Weizenbaum's book *Computer Power and Human Reason: From Judgment to Calculation* (1976) was memorable for three things. It provided perhaps the best introductory account of a Turing machine (cited in Chapter 4.i.b). It gave a superbly funny description of the priorities and social (or antisocial) habits of a new subspecies of *Homo sapiens*, the dedicated "hacker" (pp. 115–24). (The hacker lifestyle had already been described in *Rolling Stone*, but with more appreciation and less humour: Brand 1972.) And it argued at length that GOFAI could never replace human judgement—and that even if it could, it shouldn't.

Weizenbaum didn't discuss actual AI programs in any detail, wanting "to avoid the unnecessary, interminable, and ultimately sterile exercise of making a catalogue of what computers will and will not be able to do, either here and now or ever". He complained that Dreyfus was too concerned with "the technical question of what computers can and cannot do" (1976: 12). This, he said, wasn't the key issue:

> [If] computers could imitate man in every respect—which in fact they cannot—even then it would be appropriate, nay, urgent, to examine the computer in the light of man's perennial need to find his place in the world. (Weizenbaum 1976: 12)

As for that "which in fact they cannot", this was a matter of principle. Man-as-machine was an absurdity:

> Whether or not [the programme of AI] can be realized depends on whether man really is merely a species of the genus "information-processing system" or whether he is more than that. I shall argue that an entirely too simplistic notion of intelligence has dominated both popular and scientific thought, and that this notion is, in part, responsible for permitting artificial intelligence's perverse grand fantasy to grow. I shall argue that an organism is defined, in large part, by the problems it faces. Man faces problems no machine could possibly be made to face. Man is not a machine. (p. 203)

His insistence that AI systems, even if they could be hugely improved, should not be substituted for human judgement was especially fierce with respect to contexts involving interpersonal respect or emotions. In such cases, the very idea of replacing human judgement with computer calculation was "obscene": merely contemplating such projects "ought to give rise to feelings of disgust in every civilized person" (1976: 268). So Weizenbaum denounced his rival Kenneth Colby's plans to use ELIZA-like programs for interviewing mental patients (see 7.i.a).

In part, he was (understandably) worried by the crudity of the NLP involved in Colby's programs, and the consequent crudity of the computer's performance. In part, however, he was concerned that the patients' naivety would lead them to believe that Colby's computer was really intelligent. As evidence for this concern, he reported that his secretary had once asked him to leave the room when she was interacting with ELIZA. In truth, this was no cause for alarm: if *you* were playing around with ELIZA and had decided to answer its quasi-questions honestly, would you want your boss reading over your shoulder? Nevertheless, it was possible that some programs might fool some people into attributing genuine intelligence to them.

Weizenbaum also poured scorn on the idea that AI programs might be used in the law courts, to aid the judges in their deliberations. He'd already crossed swords about this with McCarthy, for his views were already familiar to "the artificial intelligentsia"—both from face-to-face meetings and from the pages of *Science* (Weizenbaum 1972). McCarthy hadn't shared his horror at the prospect of AI-in-the-law-courts. Weizenbaum recalled:

As Professor John McCarthy once put it to me during a debate [IJCAI-1973, at Stanford: Kuipers *et al.* 1976: 13], "What do judges know that we cannot tell a computer?" His answer to the question . . . is, of course, "Nothing." And it is, as he then argued, perfectly appropriate for artificial intelligence to strive to build machines for making judicial decisions. (Weizenbaum 1976: 207)

Weizenbaum gave an answer to McCarthy's question, but was horrified that it should have been asked at all:

What could be more obvious than the fact that, whatever intelligence a computer can muster . . . it must always and necessarily be absolutely alien to any and all authentic human concerns? *The very asking of the question, "What does a judge (or a psychiatrist [see 7.i.a]) know that we cannot tell a computer?" is a monstrous obscenity.* That it has to be put into print at all, even for the purpose of exposing its morbidity, is a sign of the madness of our times. (pp. 226–7; italics added)

(No one, then, was seriously suggesting that AI programs might be seated on the bench: McCarthy had merely been responding to a rhetorical question from Weizenbaum—see below. But people were already working on expert systems representing relatively clear-cut laws, as we'll see in Chapter 13.ii.c–d. And it was the philosophically sophisticated Buchanan who'd first suggested that this be done: Buchanan and Headrick 1970.)

In short, expert systems were (potentially) all very well for dealing with geology for oil prospecting, or medical diagnosis of bodily diseases. At a pinch, they might even be used as aids, though not as substitutes, in searching legal databases for precedents. But where decisions about human lives were concerned, AI programs should be forever eschewed.

Various NewFAI leaders would make fierce ripostes. That's not to say that they rejected every one of Weizenbaum's warnings. But not all were willing to spend time in planning how to pre-empt them. Take McCarthy, for instance. His answers to *What worries about computers are warranted?* showed that he thought there were some (Kuipers *et al.* 1976: 10). But he hadn't lost much sleep in worrying. He hadn't thought seriously, for example, about *What do judges know that we cannot tell a computer?* He didn't even remember saying it, although he was happy to reply to it (in one brief sentence) as a "rhetorical" question. His reply was typically recalcitrant: "I'll stand on that if we make it 'eventually tell' and especially if we require that it be something that one human can reliably teach another" (p. 9). In other words, he didn't take Weizenbaum's question about AI judges as a *practical* issue, but as an attack on the philosophical principle (which he'd been among the first to adopt) that all human knowledge can be formally represented by AI.

Nor had McCarthy thought seriously about any other social implications of the widespread use of AI. Indeed, he'd said in the early 1970s that it wasn't worth speculating about these ahead of time. Once the technology was actually being used in society, it would be clearer what the real problems were. On those grounds, he'd opposed Donald Michie's setting up a meeting of a few carefully selected participants to discuss these issues (The Serbelloni File 1972, letter from J.M. to D.M.M.).

Michie's meeting did eventually take place, at the Rockefeller Foundation's Villa Serbelloni in Italy. That fact alone indicated that some NewFAI people disagreed with McCarthy on this matter. (My own view was that it was irresponsible *not* to consider the possible social implications, despite the unavoidable uncertainties at that early stage: Boden 1977, ch. 16.) They felt that Weizenbaum had been right to raise these concerns, even if he'd done so in an unnecessarily emotive, near-hysterical, fashion. One didn't have to go all the way with him to be worried about such things.

In particular, one didn't have to share his socio-political views. But as we'll now see, many did. Some of the feeling in his favour, on the part of professionals and public alike, was grounded in the cultural context of the time.

f. A sign of the times

Both Dreyfus and Weizenbaum were important voices in the counter-culture (Chapter 1.iii.c–d). But the counter-culture itself had already prepared the general public to pay attention to them.

Let's consider Dreyfus first. His book sold well, in all three versions, not only because he had some important things to say, nor only because many people agreed with him. In addition, they *wanted* to agree with him, and empathized with the virulence of his attack. Indeed, that's largely why Papert had taken such pains to rebut *Alchemy*. Had the RAND memo been published only ten years earlier, when the most powerful McCarthy in the land was Senator Joseph, he'd probably have agreed with his friends that to criticize it in detail was "a waste of time" (see above). In the late 1960s, however, it wasn't.

The first edition of Dreyfus's book fitted well with the emerging counter-culture. One aspect of that cultural emergence was that Dreyfus's background position, namely neo-Kantian phenomenology, was enjoying a renaissance in the Anglophone world. By the time that *What Computers Can't Do* appeared, even Hilary Putnam, the founder of functionalism, was already starting across the philosophical divide (Chapter 16.vi).

The philosophical movement gained strength—or anyway, numbers. By 1990, several prominent cognitive scientists (including Agre and Chapman, and Brian Cantwell Smith) were regularly quoting Merleau-Ponty and, especially, Heidegger. The MIT roboticist Rodney Brooks even felt the need to say of his own seminal paper in the technical journal *Artificial Intelligence* that "It isn't German Philosophy," though he did admit to "certain similarities" (1991*a*, sect. 7.5; cf. M. W. Wheeler 2005, ch. 1). According to that stream of philosophy, as we saw in Chapter 2.vi, *no* naturalistic account of human thought is possible, not even a neuroscientific one: AI is just the worst of a bad bunch.

Another counter-cultural aspect was that AI was closely associated with military-industrial concerns. Dreyfus himself didn't stress this. Nevertheless, most young readers in the 1970s were very happy to see AI denounced.

Unreconstructed aficionados of the old philosophical/political order, of course, weren't. Feigenbaum, for instance, had little patience for Dreyfus:

> What artificial intelligence needs is a *good* Dreyfus. The conceptual problems in AI are really rough, and a guy like that could be an enormous help . . . We do have problems, and they could be illuminated by a first-class philosopher. But Dreyfus bludgeons us over the head with stuff he's misunderstood and is obsolete anyway [see above] . . . And what does he offer us instead? Phenomenology! That ball of fluff! That cotton candy! (interview in McCorduck 1979: 197; italics added)

(Buchanan, whose Ph.D. had been in philosophy, was less dismissive of phenomenology: "If there is any reason to read the book at all, it is to become acquainted with this current view of man and the world which is different from the traditional scientific view"—1973: 21.)

The "cotton candy" is considered at length in Chapter 16.vi–viii and ix.d–e, together with attempts, even on the part of the Dreyfus brothers (1990), to reconcile phenomenology and cognitive science—but not GOFAI. Here, what's relevant is that outsiders in general were readier to consume it in the 1970s than they would have been at any previous time.

That's largely why the young Dreyfus, who didn't have international (or even national) eminence when he wrote *Alchemy*, very soon achieved it.

Similar remarks apply to the historical context of Weizenbaum's book, published just four years after Dreyfus's. Indeed, he himself saw it in this light (1976: 10 ff.). So he thanked the counter-cultural figures Lewis Mumford, Steven Marcus, and Chomsky: not just for their constructive advice on the drafts, but for their encouragement when he "despaired" at others' having said what he had to say more eloquently:

> [As] Lewis Mumford often remarked, it sometimes matters that a member of the scientific establishment say some things that humanists have been shouting for ages. (Weizenbaum 1976, p. x)

Specifically, he was firing a salvo in the "science wars" that caused havoc in social psychology and, especially, anthropology (Chapters 1.iii.b–d, 6.i.d, and 8.ii.b–c). Having said that "This book is only nominally about computers" (p. ix), he declared "scientism" to be the true enemy—of which psychological AI was a special case (pp. 222–3).

It's hardly surprising, then, that his views were widely welcomed by the general public. Many of them were already highly sceptical about (and frightened by) AI, so were delighted to receive support from someone in the know. Dreyfus, after all, had been a mere philosopher: Weizenbaum, by contrast, knew which side of a computer was which. Many also cheered his bitter critique of the use of computer programs—often incomprehensible to the user—for destructive military purposes in Vietnam and elsewhere (1976: 236–43).

A proviso: with respect to Weizenbaum's strictures on computer judges, the "general public" didn't include young black males in inner-city USA (Turkle 1995: 292). Their view of judges was very different from that of white middle-class students. Whereas the latter usually agreed with Weizenbaum (saying, for instance: "Judges weigh precedents and the specifics of the case. A computer could never boil this down to an algorithm"), their black contemporaries said things like:

> This is a pretty good idea. He [the computer judge] is not going to see a black face and assume guilt. He is not going to see a black face and give a harsher sentence.
>
> [From another young man:] I know that if I ever had to go before some judge, there is a good chance that he is going to see my face and he is going to think "nigger".
>
> The computer judge would have a set of rules. He would apply the rules. It might be safer. (both speakers quoted in Turkle 1995: 292–3)

Those interviews were done in 1983. By 1990, PDP had come on the scene. An MIT student (race unspecified, but the context suggests that he was black) now pointed out:

> If you were training a [neural net] computer to be a judge and it looked at who had been found guilty and how they were sentenced, it would "learn" that minority people commit more crimes and get harsher sentences. The computer would build a model that minorities were more likely to be guilty, and it would, like the human judges, start to be harder on them. The computer judge would carry all of the terrible prejudices of the real ones . . . Of course, you would never be able to find a "rule" within the system that said you should discriminate against minorities. The computer would just do it because in the past, people had done it. (quoted in Turkle 1995: 293)

In short, a PDP judge would have racist flaws similar to those of the NewFAI expert system designed to select candidates for a London medical school (see 13.ii.c).

Weizenbaum hadn't simply attacked specific applications of AI, such as military or legal programs. AI in general, he said, depended on a "corruption" of language whereby intentional terms such as *problem*, *understanding*, and *knowledge* were tacitly redefined to fit the mechanistic mould. Minsky, Newell, and Simon were specifically named. But this wasn't a matter restricted to the laboratory:

> [The] computer, as presently used by the technological elite, is . . . an instrument pressed into the service of rationalizing, supporting, and sustaining the most conservative, indeed, reactionary, ideological components of the current *Zeitgeist*. (p. 250)

Such remarks went down well on campuses across the world, and with sensation-hungry journalists. Within the profession, however, they were to some extent counter-productive. They antagonized the AI cognoscenti, many of whom were themselves out of sympathy with US policy, above all in Vietnam. His MIT colleague Kuipers, for instance, complained:

> I had some strong reactions to Joe Weizenbaum's book *Computer Power and Human Reason*. The book mentions some important concerns which are obscured by harsh and sometimes shrill accusations against the Artificial Intelligence research community. On the whole, it seems to me that the personal attacks distract and mislead the reader from more valuable abstract points . . . *I see [many of his ideas] as being quite current in the AI community, so I was quite puzzled by Weizenbaum's vehement attacks on us for not sharing them*. (Kuipers *et al.* 1976: 4; italics added)

Kuipers may have been more annoyed than most, being a conscientious objector who did alternative service (before going to graduate school) instead of submitting to the

Vietnam draft. But he certainly wasn't the only NewFAI researcher to be offended by Weizenbaum's accusations of lack of moral/political integrity.

Nevertheless, the book couldn't be ignored by the NewFAI community. It was the subject of many debates, both formal and informal. McCarthy, for one, saw it as a threat to AI research in general, being all of a piece with the counter-culture of the time:

> This moralistic and incoherent book uses computer science and technology as an illustration to support the view promoted by Lewis Mumford, Theodore Roszak, and Jacques Ellul, that science has led to an immoral view of man and the world. I am frightened by its arguments that certain research should not be done if it is based on or might result in an "obscene" picture of the world and man. Worse yet, the book's notion of "obscenity" is vague enough to admit arbitrary interpretations by activist bureaucrats. (Kuipers *et al.* 1976: 5; cf. p. 9)

McCarthy wasn't one of those who, like Kuipers, sympathized with Weizenbaum's politics. He complained that "Certain political and social views are taken for granted," accused Weizenbaum of "political malice" and "new left sloganeering", and—by implication—objected to his description of US policy in Vietnam as "murderous" (Kuipers *et al.* 1976: 6, 7, 9).

But a significant number of his colleagues did share Weizenbaum's concerns. Partly due to the efforts of Winograd, a new society and newsletter were founded in Palo Alto in 1981: Computer Professionals for Social Responsibility, or CPSR (see <http:www.cpsr.org>). Offshoots soon sprang up at other US locations, at the universities of Edinburgh and Sussex, and in France, Germany, and Italy.

Within a few years, CPSR was lobbying against the technical inanities of the USA's Star Wars programme (see i.c, above). One prime concern was the issue already mentioned by Weizenbaum: programs so complex that they're incomprehensible to the layman (including military personnel) and sometimes even to computer professionals. A new journal, *Computing and Society*, was founded in 1988 by CPSR's executive director, Gary Chapman, to draw attention to these matters. Across the ocean, several attempts were made in the 1980s to warn the public *and AI professionals themselves* about the risks of using certain types of AI system (H. Thompson 1984, 1985; Whitby 1988; Council for Science and Society 1989). Sometimes, these warnings were placed in the context of a specific, though unorthodox, philosophy of science—as we'll now see.

g. The unkindest cut of all

Another bombshell of this general type was due, believe it or not, to Winograd. In the mid-1980s, he publicly accepted the arguments already put forward by Dreyfus and Weizenbaum, and added those of the Chilean biologist Humberto Maturana.

All three of these men were seen as highly maverick by GOFAI folk, and were mostly ignored by them accordingly. (Maturana was rarely even read, partly because he wrote in such an opaque manner; his work became popular only much later: see 15.viii.b.) Winograd, by contrast, couldn't be ignored.

What caused most consternation in GOFAI circles wasn't the force of his newly borrowed arguments. Rather, it was his offering the enemy the glamour and authority of his hugely famous name. For this was the man who, only a couple of months after getting his Ph.D., had been invited to give the inaugural *Computers and Thought* Lecture at IJCAI-1971. His youthful achievement in NLP, and his influence on 1970s symbolic AI in general (and on cognitive science too), had been nothing short of phenomenal (see 9.xi.b and 10.iv.a).

A person of lesser intellectual honesty could have rested comfortably on those early laurels for a lifetime. Through the 1970s, however, he'd become increasingly sensitive to the difficulties inherent in NLP—already emphasized by Bar-Hillel and Dreyfus. And he said as much in print, writing alongside Minsky and Papert (and others) in an official national report:

> The AI programs of the late sixties and early seventies are much too literal. . . . This gives them a "brittle" character, able to deal well with tightly specified areas of meaning in an artificially formal conversation. They are correspondingly weak in dealing with natural utterances, full of bits and fragments, continual (unnoticed) metaphor, and reference to much less easily formalizable areas of knowledge. (Winograd 1976: 17)

By the early 1980s, he'd long abandoned SHRDLU, and put his major efforts into founding CPSR instead. He was disenchanted also with his programming language KRL, which was "collapsing perhaps under the weight of its own features" (Brachman and Levesque 1995: 263). His long-heralded treatise on *Language as a Cognitive Process* had a promising title, from the point of view of orthodox cognitive science. But it never progressed beyond the first instalment, on syntax (Winograd 1983).

Semantics, he now felt, was very much more difficult. He'd hinted as much in a talk he gave at IJCAI-1973 in Los Angeles. But there, he was saying in effect, "When I wrote SHRDLU, I hadn't really thought much about semantics. I'll have to do so"—and he praised Roger Schank, as someone who was already attempting the taxing task of bringing the communication of meanings into AI (see 9.xi.d). At that point, he hoped to emulate, even surpass, Schank's efforts. But by the mid-1980s, when his *Syntax* book appeared, he'd given up. He'd decided that the task wasn't just taxing, but impossible. And he soon said so, at length, in a book co-authored with Fernando Flores (Winograd and Flores 1986).

Flores had been the key player in leading Winograd to change tack. It was he who'd introduced Winograd to his compatriot Maturana's writings—and to Maturana himself, who later gave them feedback on the draft of their book (Winograd and Flores 1986, p. xiv). Flores didn't go in for nitty-gritty linguistic theory, still less the highly abstract semantics of Richard Montague, then gaining popularity on another part of the Stanford campus (9.ix.c). Indeed, he was less interested in language as such (i.e. sentences) than in the forms of human communication it made possible.

Winograd was drawn to him by political sympathies as well as intellectual ones. Deeply influenced (as a student of engineering) by Stafford Beer's cybernetic work in the 1960s, Flores had been largely responsible for inviting Beer to design the Cybersyn network for the newly elected Allende government in Chile (Chapter 4.v.e). He'd been the minister responsible for nationalizing Chile's industry, and after Colonel Pinochet's CIA-led coup in 1973 he spent three years in prison. He was released in 1976, thanks to the good offices of the San Francisco chapter of Amnesty International—which brought him to Stanford, three years after Winograd's arrival there.

The two men's shared intellectual sympathies eventually went much further than a dissatisfaction with formalist NLP. For, to his previous colleagues' bewilderment, Winograd followed Flores (and Dreyfus) and turned to hermeneutics and phenomenology (9.xi.b and 16.vii), and to autopoietic biology (15.viii.b and 16.x.c). The co-authored book of 1986 bitterly disappointed most of his long-time admirers as a result.

Even Winograd himself hadn't foreseen this volte-face. When he went to Stanford in 1973, he'd known of Dreyfus only through the scandal described in Section ii.a–c above. He says now that he "wasn't pre-inclined" to him (personal communication). I take this remark to be code for something very much stronger: benignly non-competitive though he was, Winograd could hardly have escaped infection by the virulent hostility to Dreyfus then current at MIT. But his inclinations changed. For Dreyfus turned out to be one of a small group, also including Searle and Daniel Bobrow, attending "a very interesting series of informal private lunch seminars". (If Winograd had adopted the macho 1980s motto "Lunch is for wimps", maybe things might have turned out differently!)

One person whose life was changed by the Winograd–Flores book, which he read while it was still in preprint form, was Randall Beer. Then, he was a graduate student in GOFAI, happily writing expert systems in LISP. Now, he's a renowned A-Life roboticist and dynamical systems theorist (15.vii.c and viii.d).

For Beer, Winograd's new book was an epiphany. This wasn't because of its views on language as such, but because it introduced him to the biological ideas of Maturana and Francisco Varela (M&V). Their theory of autopoiesis (15.viii.b and 16.x.c) had persuaded Winograd and Flores to think of 'Language as a Biological Phenomenon' (pp. 38–53). On reading their book, and then M&V's (Maturana and Varela 1972/1980), Beer was convinced that cognition couldn't be understood except as a property of life, and that both these concepts had to be theorized in dynamical, embodied, terms. He now sees his own work as "an attempt to concretely express, illustrate and apply some of the insights that [M&V's ideas on autopoiesis] bring to understanding biological behavior and cognition" (R. D. Beer 2004: 310).

But if the new publication was a welcome wake-up call for this young AI student, for most of its older AI readers it was a very nasty shock. Like M&V themselves, who avoided all talk of "representations" and even of "feature detectors" (15.vii.b), Winograd was rejecting the computational approach wholesale. Considered as a philosophical position, functionalism and "strong AI" were being declared intellectually bankrupt (16.iii–iv, v.c, and ix.b). Considered as a practical proposition, GOFAI-based NLP was being dismissed as a waste of time.

In the eyes of the GOFAI community, this was betrayal. Winograd had started as a heretic and rapidly became a high priest of the orthodoxy. Now, he was an apostate. That in itself was enough to qualify as a bombshell. And GOFAI's philosophical enemies, naturally, made the most of the ammunition.

11.iii. A Plea for Intellectual Hygiene

Neither Dreyfus nor Weizenbaum, nor even the newly reconstructed Winograd, had been asking for AI to be done better. They didn't really want it to be done at all.

Admittedly, they were happy for AI to be applied for highly limited technological purposes. But they didn't want it to be used as the intellectual core of cognitive science.

Indeed, they didn't want cognitive science: *mind as machine* (they believed) was absurd, and *mind as formalist/symbolic machine* was anathema.

Our next bombshell, by contrast, was specifically asking for AI to be done better (McDermott 1976). The person who dropped it in the mid-1970s believed that human intelligence could be explained by symbolic AI in principle, and that logicist programs might model much of it in practice. (He later changed his mind about logicism: see 13.ii.a.) But there was no chance of that, he said, if AI research carried on in the way it had started.

He accused the early AI programmers of being scientifically ill-disciplined and often self-deluding. To turn over-excitable NewFAI into respectable GOFAI, they'd need to mend their ways.

a. The insider

Drew McDermott (1949–) was a well-respected AI researcher at Yale (previously at MIT) when he first threw a cat among the AI pigeons. He was also an accomplished philosophical logician, with a good grasp of work in epistemology and metaphysics. He'd won his GOFAI spurs by helping develop CONNIVER (Chapter 10.v.c), and by modelling some of the automatic inferences made when we hear familiar words (10.iii.d). So he was 'one of the boys'.

However, while the AI community was still basking in the reflected glory of SHRDLU, he wrote a squib about the field for SIGART, provocatively called 'Artificial Intelligence Meets Natural Stupidity' (1976). This was a critique (even, as he put it, "self-ridicule"), not an attack. But it was so cutting, and so well aimed, that when Haugeland later included it in his collection *Mind Design* (1981) it was sandwiched between David Marr's (1977) strongly anti-GOFAI 'Personal View' and Dreyfus's (1981) characteristically dismissive 'AI at an Impasse'.

McDermott charged his NewFAI colleagues—and, disarmingly, himself—with systematic self-deception and obfuscation, various types of sloppy thinking, and lack of persistence in improving first-effort programs. In short: he diagnosed an overall lack of scientific self-discipline, which compared unfavourably with common practice in other areas of computer science.

It was no accident, he said, that many professional computer scientists looked down their noses at AI. It wasn't simply that, as *mathematicians*, they preferred proven theorems to unreliable heuristics. In addition, they deplored the lack of intellectual discipline that was all too common in the NewFAI days. (Resentment at the glamour and media publicity attending AI probably played a part too: I certainly got that impression in several conversations with computer scientists in the 1970s.) "If we are to retain any credibility", he declared, "this should stop" (1976: 143).

Today, 'pure' computer scientists still look down their noses at AI (see 13.vii). But, partly because AI now is technically so much deeper and more rigorous than it was then, McDermott sees this as a primarily methodological difference:

> AI people [today] are willing to try an algorithm and test it empirically even though theorists have proven it won't work. I've had more than one theorist tell me that the value of AI is in its practitioners' willingness to try things. When they work better than expected, often lots of interesting theoretical issues are created as a byproduct, and the theorists move in.

> I'm not saying that AI shows the theory was *wrong*. [We know, for a problem that's NP-complete, that] there's no hope of finding an algorithm that solves every instance of the problem. The question is whether there are interesting clusters of instances where better algorithms can be found, and there's no way to know without empirical exploration. (McDermott, personal communication, 2004)

NewFAI workers, said McDermott in his bombshell paper, weren't being irresponsible at random. A systematic source of self-deception was their common habit (made possible by LISP: see 10.v.c) of using natural-language words to name various aspects of programs.

These "wishful mnemonics", he said, included the widespread use of "UNDERSTAND" or "GOAL" to refer to procedures and data structures. In more traditional computer science, there was no misunderstanding; indeed, "structured programming" used terms such as GOAL in a liberating way. In AI, however, these apparently harmless words often seduced the programmer (and third parties) into thinking that *real* goals, if only of a very simple kind, were being modelled. If the GOAL procedure had been called "G0034" instead, any such thought would have to be proven, not airily assumed.

The self-deception arose even during the process of programming: "When you [i.e. the programmer] say (GOAL . . .), you can just feel the enormous power at your fingertips. It is, of course, an illusion" (p. 145). Indeed, he said, one of the advantages of CONNIVER over PLANNER was that it was *harder* to program in CONNIVER, because FETCH & TRY NEXT was used instead of GOAL, METHOD instead of THEOREM, and ADD instead of ASSERT (144–5). (But he too, he confessed, had been guilty of obfuscation: CONNIVER's so-called "CONTEXTS" were no such thing.) The rot had also affected AI thinking about *deduction*. The meaning of this familiar, and apparently unambiguous, word had been unwittingly changed (by AI work on resolution theorem proving: 10.iii.b) to become "something narrow, technical, and not a little sordid" (p. 145).

Self-deception was rampant in the names given to subroutines. Among the countless examples McDermott could have cited were Colby's FINDANALOG (7.i.a), and Robert Abelson's REINTERPRET FINAL GOAL, ACCIDENTAL BY-PRODUCT, and FIND THE PRIME MOVER (all procedures for "rationalization": 7.i.c). As for the names of entire programs, the famed GPS, with its spurious promise of being a *general problem-solver*, would have been better called LFGNS—"Local-Feature-Guided Network Searcher". And Ross Quillian's (1969) 'Teachable Language Comprehender' would have caused less excitement had it been—honestly—named 'The Teachable Language Node Net Intersection Finder'.

Indeed, whole sub-fields had been optimistically misnamed, with unfortunate effects:

> And think about this: if "mechanical translation" had been called "word-by-word text manipulation", the people doing it might still be getting government money. (p. 148)

Here, McDermott was referring to the disastrous effects of the ALPAC bombshell of 1964 (Chapter 9.x.e), and to the general disenchantment with machine translation seeded in 1960 by another insider, Bar-Hillel (9.x.b and e). He might have added that a similar reproof could have been made—and, already, often was—against McCarthy's name for the field as a whole: artificial *intelligence*. Some AI sympathizers had foreseen this from the start, and had recommended other names accordingly (6.iv.b).

Quillian (1961, 1968) had pioneered another near-ubiquitous example of self-deception, namely, work on semantic networks (10.iii.a). These, said McDermott, were neither semantic nor networks (p. 160). For instance, the apparently simple IS-A link (aka *isa*) was fraught with misinterpretation—usually, over-interpretation. McDermott's worries about IS-A (146 ff.), and about the handling of "a" and "the" (152–3), were broadly similar to those of William Woods (see 9.xi.e). In a nutshell, highly limited—though precisely defined—program procedures were being *interpreted* in a much vaguer, and richer, way.

The sloppy thinking connected with the IS-A link was largely due, McDermott argued, to the lazy assumption that natural language provided both problems and solutions. In other words, language-like internal representations in the computer were felt to be intrinsically superior to more abstract ones. (This was implicit in Quillian's little note of 1961, for example.) To the contrary, any really illuminating (and workable) representation of a familiar concept or an English sentence would be "a directly useful internal representation, probably as remote as possible from being 'English-like' " (p. 150).

Moreover, natural language was being underestimated. Not only is it often/usually used for purposes other than transmitting information, but its meaning can't be easily corralled. For instance, the common NewFAI assumption that words such as *the*, *a*, *all*, *or*, and *and* are expressible in something like predicate calculus terms was an illusion. Russell and the logical atomists had believed it, to be sure (Chapter 4.iii.c). But their philosophical critics had been legion. Even the exercise of "examining two pages of a book for ten-year olds, translating the story as you go into an internal [programmed] representation" (p. 153), said McDermott, would show the task to be horrendously difficult. (Here, he was thinking of Eugene Charniak's work: Charniak 1972, 1973, 1974.)

In general, he added, people should distinguish clearly between what their program actually did and what some other version of it might do in the future. As it was, "Performance and promise run together like the colors of a sunset" (p. 157). What the program *didn't* do should also be made clear:

> AI as a field is starving for a few carefully documented failures. Anyone can think of several theses [including some highly publicized ones] that could be improved stylistically and substantively by being rephrased as reports on failures. I can learn more by just being told why a technique won't work than by being made to read between the lines. (p. 159)

(It's worth remarking, by the way, that one of the many commendable—and often commended—aspects of Winograd's Ph.D./book had been his unusual honesty in pointing out the *limitations* of his program.)

Last but not least, McDermott identified a "common idiocy" in AI research: the abandoned-program syndrome. Far too often, people supposed "that having identified the shortcomings of Version I of a program is equivalent to having written Version II" (p. 156). (Again, he identified himself as one of the culprits: McDermott 1974.) There was some sociological excuse for this, in that the culture of awarding Ph.D.s in AI prioritized theoretical comment over effective programming, and sought new work on new problems rather than improvements on research already done (whether by Self or Other). But scientific excuse, there was none. What true scientist would happily report only his initial, half-baked, ideas, without carrying them forward in later work?

Colleagues, too, should be able to carry them forward. But if AI was to benefit from normal scientific cooperation (Chapter 2.ii.b–c), programs should be properly debugged so that other people could run them, test them, and develop them. Although McDermott didn't say so (because he didn't know it at the time: personal communication), Winograd was at fault here. The man–machine conversation he'd reported was the only one of that length ever produced (even so, it may have been 'cobbled together' rather than being one continuous interchange), and it turned out that SHRDLU couldn't be effectively run by walk-up users because of the bugs remaining in it (Winograd, personal communication—see 9.xi.b). McDermott admitted, however, that a first-effort program may be very useful—and surprising—in showing what can be done, and what with persistence *might* be achieved.

This was strong stuff: McDermott didn't mince his words ("stupidity", "idiocy" . . .). But he didn't suffer the calumnies that the AI profession was directing against Dreyfus and Weizenbaum. Not only was he an insider, and a self-confessed culprit to boot, but he was undeniably right. His arrows had hit the mark. Moreover, he wasn't saying (as they were) that AI was a worthless enterprise. Even logicist AI was a respectable project (so he believed at that time: but see 13.ii.a).

b. Natural Stupidity survives

Some others, too, were complaining that AI's "lack of self-generated criticism is scandalous" (Wilks 1976: 184). Their remarks, and McDermott's, shamed some of their colleagues into being more self-critical. Even so, complaints about "natural stupidity" still had to be made in the 1980s . . . and occasionally have to be made now.

(To be fair, natural stupidity wasn't monopolized by GOFAI. It was a feature of some *connectionist* AI as well. "Why think when you can simulate?", as James Anderson put it—see Chapter 12.ii.a.)

A decade after McDermott's squib, two similar complaints were made on either side of the Atlantic. One came from McCarthy, then President of the AAAI (McCarthy 1984).

His brief President's Message (constantly threatening, he said, to turn into a paper) argued that we need better standards for AI work. Not that AI people were any less motivated than other scientists to do good work: they weren't "fooling" the funders, or the public. But in any science evaluative standards have to develop, and this takes time. In AI, he said, they "are not in very good shape" (p. 7). He identified seven principles that should help AI to progress faster, some of which (such as repeatability) had been anticipated by McDermott. And, by the way, he pointed out that although the Turing Test can be an interesting "challenge" for AI research, it *isn't* a "scientific criterion" of AI value (8)—see Chapter 16.ii.

The other complaint published in that year came from Edinburgh's Alan Bundy (1947–). He criticized GOFAI work as—still—insufficiently analytical and (therefore) non-cumulative (Bundy 1984; Bundy *et al.* 1985). Although learning was the particular domain he considered, his conclusion was directed at AI in general. A more scientific, not to say mathematical, approach was required if the field was to progress.

With respect to learning, Bundy showed that a powerful "focusing" algorithm defined by some of his Edinburgh colleagues in the year following McDermott's 'Natural Stupidity' tirade subsumes many AI programs modelling superficially different types of learning in apparently different ways (discrimination, generalization, version-spaces . . .). That is, the principled core of these seemingly diverse programs was the same. Moreover, it helped explain the differences in the ways in which they modify their rules on the basis of positive or negative information, and in learning conjunctive or disjunctive concepts.

The explanation of *why* these programs worked, as opposed to mere reports *that* they worked, lay in such theoretical analysis. In Chomsky's language (which Bundy borrowed), AI research shouldn't merely describe a program's performance, but should identify its competence as well (Chapter 7.iii.a).

By that time, Marr too had asked for more systematic theoretical analysis in AI (D. C. Marr 1977). Indeed, he'd gone further than either Bundy or McDermott, claiming that AI programming is a waste of time if the abstractly definable task underlying the domain concerned hasn't been identified (7.iii.b).

Minsky and Papert, of course, had provided an abstract computational analysis of one type of AI—namely, simple perceptrons—twenty years earlier (12.iii.a–c). Indeed, ten years earlier still Minsky had shown (in 'Steps': 10.i.g) that analytical questions and comparisons were crucial. But the point hadn't sunk in. For far too long, the Minsky–Papert exercise remained the Lone Ranger of *theoretical* AI, or what McCarthy called meta-epistemology. If McDermott's advice had been truly taken to heart when he wrote his paper, companions might have turned up sooner.

As it is, most of McDermott's complaints still ring true of much AI work. That's clear from these two late-century remarks—one astringent, the other defensive:

> We need to discipline ourselves so that our tests are both proper in themselves and connectible, through common data or strategies, with those done by others. Because if we do not seek the standards [that] doing information science should imply we will be open, correctly, to the claim that we're just inventing copy for the salesmen. (Sparck Jones 1988: 26)

> Computer science is really like physics in 1740. People are so happy for any results, that more than haphazard repeatability and duplication of other people's results are too much to ask. It's still at the stage where people are discovering rather than colonizing. (D. B. Lenat, interviewed in Shasha and Lazere 1995: 229)

It's clear, too, from the fact that AI hype still occurs.

For instance, the official web site for RoboCup, the ongoing competition in robot soccer, opens by declaring this as the aim:

> By the year 2050, [to] develop a team of fully autonomous humanoid robots that can win against the human world soccer champion team. (<http://www.robocup.org>, accessed 9 Apr. 2004)

In 1999, soon after RoboCup was launched—at the IJCAI-1997 meeting in Nagoya, Japan—with ninety AI teams competing (Asada *et al.* 1999, 2000), one of its initiators quoted those words and commented:

> The current technology is nowhere near the accomplishment of that goal, as most robots use wheels instead of legs, and have serious difficulties seeing a ball and other robots. Nevertheless, we believe that the goal can be accomplished by 2050. (Kitano 1999: 189)

One of the main issues, he said (p. 190), was collaboration: How can robots learn to collaborate? How could teamwork emerge? How can we program it? Do we even need to program it? (One of the papers representing the then state of the art described an entirely behavior-based approach: Werger 1999, and cf. 13.iii.d–e.)

The competition's spokesman Mats Wiklund apparently hadn't read the first sentence of that last quotation. For he was reported by Reuters in 1999 as predicting man-sized soccer automata by 2002, "although we don't expect the robots to stand a chance against humans at that stage". If Reuter's quotation was accurate, Wiklund's words "at that stage" were more lunatic than cautious.

A few years later, more than 300 research groups around the world were involved with RoboCup; and the 2003 competition in Padua registered 243 teams and 1,244 participants. By that time, much of the more successful (i.e. least embarrassingly unsuccessful) efforts were going into small puppy robots, not large two-legged ones. (Most of these were AIBO robots bought off the shelf from Sony: see <http://www.eu.aibo.com>. The challenge was in the programming, not the engineering.) And the dreaded offside rule was still nowhere to be seen. But the overall aim hadn't changed.

There's no doubt that something of interest will ensue (has already ensued) from this exercise, which involves real-time world-modelling and real-time decisions about cooperation and strategy. That's argued persuasively in a recent paper co-authored by the eminently sane Alan Mackworth (Sahota and Mackworth 1994). It was he who first recommended soccer as a fruitful domain for AI. Although he's now given up on soccer (because it doesn't allow for long-term planning), he's still working with ball-kicking robots relying on recognition and guidance—i.e. not on lasers or sonar (13.iii.c). Indeed, he suggests replacing "GOFAI" with "GOFAIR", where the "R" stands for *situated* or constraint-based Robotics (Mackworth 1993, 2003). (Unlike the more radical situated roboticists described in Chapter 13.iii.b, however, he doesn't deny representations or planning: GOFAI is part of GOFAIR.) But he bemoans the lack of scientific discipline in most of the RoboCup work:

> Our original robot soccer-players were the world's first . . . We did enter the simulation track [of RoboCup] three times. We were focussed more on the science than performance (at least, that was our excuse)—we won the odd game, but never a competition. I tried as a member of the Board to get people to accept a standard hardware platform but failed. So much of the effort went into hacking a better kicker etc., not into smart controllers and planners. Although it has had a tremendous stimulating effect on students and good PR, not a lot of great new science has come out of it. That disappointed me since I started it all. But in retrospect soccer has lots of great features as a test bed but it lacks longer term time horizons and hence planning and the like. (A. K. Mackworth, personal communication, 2004)

Most RoboCup competitors are less judicious than Mackworth. There are occasional signs of caution in the PR material: the report on the latest bout of the competition, for instance, opens by saying, "We do not promise that this [officially stated] goal will be reached by 2050" (Pagello *et al.* 2004: 81). Nevertheless, the mind boggles. The defence given to me in 2004 by one of RoboCup's winning team leaders (tact forbids . . .) that "It's not a prediction, it's a challenge. No one takes it seriously!" was disingenuous. For this admission came only after repeated needling from me. At first (and second, and third . . .), this person had insisted that the declared goal was achievable. I was repeatedly given the PR party line, obviously compiled for communication to journalists, with no attempt to engage with the scientific questions I'd raised.

In short, RoboCup isn't unambiguously "good PR". It may well have brought more students into AI, as Mackworth says. It's even been named by Paul Cohen (also an eminently sane individual) as one of the "new challenges" to replace the Turing Test as a measure of advance in AI (P. R. Cohen 2005: 63–4, 67). And it undoubtedly gets AI onto our TV screens. But as *the very first item* on the RoboCup web site, the ludicrous prediction cited above is potentially damaging. For such attention-seeking irresponsibility can lead people to devalue AI as a whole—as McDermott pointed out over thirty years ago.

McDermott, here, should have the last word. In a recent interview (in the ACM's first electronic publication), he was asked how he feels about being an AI researcher today:

> I'm thrilled with my station in life now. I think AI has matured a lot and has developed into a much more hard-edged discipline. I'm glad just to be a part of that. (McDermott 2001*a*)

Even so, he said in the same interview that youngsters entering AI should beware of the superficially "exciting" projects, and question their supposedly technical vocabulary so as not to fall into "fantasy":

> If somebody starts talking about meta-rules and you're thinking "Wow, this is really neat, this is like something that is self-conscious, it's able to think about its own structure, etc.", think again. I cringe when I hear people talking about meta-knowledge representation of something. It seems to me there are still a lot of people in AI who are living in a fantasy world, and it ultimately hurts the field. (2001*a*)

In sum, his rules of intellectual hygiene for AI are still valid—and sometimes still needed.

11.iv. Lighthill's Report

As René Descartes foresaw long ago, progress in scientific research requires not only intellectual creativity but also financial support (2.ii.b–c; cf. 6.iv.f). So the historian must sometimes consider money: where it came from, why it occasionally dried up, and what—sometimes—made it start flowing again.

GOFAI suffered four funding bombshells. These were especially damaging, not least to overall morale, when they were grounded not in fiscal/military policy in general (as recounted in Section i) but in the funders' disaffection with AI in particular. That applied to the first and second financial bombshells: two government-initiated reports which, in essence, declared AI to be a waste of time—and money.

The first was the ALPAC Report of 1960 (9.x.f), the second the Lighthill Report some ten years later. The former was launched in the USA, and virtually stopped MT funding on both sides of the Atlantic. The latter originated in the UK, and caused significant damage there—with shock waves spreading across the Pond.

It's worth remarking that the explosive force of the second of these bombshells was largely due to the personality of one man. His personality, you'll notice: not his research. As such, it's a telling objection to the Legend of wholly disinterested science (Chapter 1.iii.b).

a. A badly guided missile

The Lighthill Report was twenty-one pages of dynamite, sent from on high to wreak havoc on the ground. It caused a recurring nightmare for AI in Britain, a bad dream that was eventually ended by dawn trumpets from Japan. It even engendered a few restless nights in the USA. This extraordinary episode wouldn't have happened but for the personal characteristics of a single individual: namely, Michie.

Michie was the founder of symbolic AI in Britain (see 6.iv.e). He was clever, literate, charming, self-confident, persistent, energetic, entrepreneurial . . . and each of these, in spades. In a word, charismatic. But he was also, for such a suave man, surprisingly brash in his claims about his own, and AI's, achievements and potential.

Before turning to AI, he was already well known—and, significantly, well connected—as a geneticist (a protégé of Conrad Waddington at Edinburgh: see 15.iii.b). In the 1950s, with his first wife, Anne McLaren (now Dame Anne, FRS), he'd published research that helped lay the groundwork for today's reproductive technology—and later earned him a Pioneer Award from the International Embryo Transfer Society. So although AI became his passion, it wasn't his first scientific success.

As for his achievement in AI, this was significant. Indeed, he was recognized by IJCAI's Reward for Research Excellence in 2001. For example:

* His pole-balancer, written on his MENACE-inspired visit to Stanford in 1961 (see 6.iv.e), was the first reinforcement-learning program (Michie and Chambers 1968). It wasn't a simulation, but controlled a real pole on a real cart.
* The pioneering Graph Traverser (Doran and Michie 1966; Doran 1968*b*; Michie and Ross 1969) provided ideas that are now standard in heuristic search, and it still lives on as the core of a widely used planning program developed by Austin Tate (Michie 2002: 4).
* His later work (with Ivan Bratko) on chess endgames was an important contribution to chess programming—and to our knowledge of chess (e.g. Bratko *et al.* 1978; Bratko and Michie 1980).
* And his recent StatLog project, which ranges from statistics through tree building to dynamical systems, has been described as "by far the most exhaustive investigation into the comparative performance of learning algorithms" (Russell and Norvig 2003: 675; cf. Michie *et al.* 1994).

For present purposes, however, what's most relevant is his sociological influence as a research leader and highly visible AI guru. In those roles, he was hugely constructive, on the world scene as well as nationally. But he also sowed some destructive seeds.

His Department of Machine Intelligence and Perception was the first in the world to be dedicated to AI. (For the story of how it came to be founded, see Chapter 6.iv.e.) Among the AI leaders who came from that stable were Patrick Hayes, Robert Kowalski, Aaron Sloman, Michael Brady, Rod Burstall, Robin Popplestone, James Doran, Bundy, and Tate. In addition, many US scientists spent brief visits there—and some recorded their gratitude for Michie's guidance and suggestions (e.g. Quinlan 1983: 481). His Edinburgh colleague Meltzer was the first editor of the *Artificial Intelligence* journal (founded in 1970). And Michie's international Machine Intelligence meetings and publications, initiated in the mid-1960s, were the first regular series in the field.

All that was highly commendable—and hugely influential. So much so, that when Donald Broadbent and others in 1970 lobbied to get some much-needed government funding to support the maverick AI being done by Gordon Pask's team in Richmond, they lost out to Michie's team in Edinburgh (Mallen 2005: 87). But there was trouble in the offing.

By the time of the break-up of the Edinburgh triumvirate in 1970 (Chapter 6.iv.e), two of Michie's colleagues, Meltzer and Christopher Longuet-Higgins, had become his bitter enemies (P. J. Hayes 1973: 36–7, 43–4; Howe 1994). Indeed, Meltzer (1971) had recently regaled their international peer group, the readers of the *SIGART Newsletter*, with a squib containing some caustic remarks about the Edinburgh robot FREDDY. (His subtitle, 'Bury the Old War-Horse!', referred to the Turing Test, wrongly seen by the public as a criterion of success in AI—but one might have been forgiven for interpreting the title in another way!) Since he was the first Editor of the *Artificial Intelligence* journal, Meltzer's criticism of his colleague's pet project carried weight as well as venom.

The discord had spread way beyond Edinburgh. Michie's tireless lobbying for and publicizing of AI had offended many people. In part, they were annoyed by (even jealous of?) the huge public attention his department attracted: so many TV crews and other visitors made their way there that such visits eventually had to be restricted. In part, too, they were upset by his unrealistic optimism. Domestic robots, according to him, were just around the corner. (That's not quite fair: sometimes, he was more realistic—e.g. Michie 1968*b*.)

Worse, some doubted his reports of the present almost as much as his hubristic hopes for the future. They suspected that his glowing descriptions of FREDDY were, to put it delicately, inaccurate (see below).

The squabbling in his department was the most important of three factors which prompted the Science Research Council (SRC) to commission Sir James Lighthill (1924–98) to write a "dispassionate" report on AI research in the UK. They could hardly have chosen anyone more high-profile. For Lighthill, a world leader in hydrodynamics, was the holder of the Lucasian Chair of Applied Mathematics at Cambridge—previously held by Isaac Newton and Charles Babbage (and today by Stephen Hawking).

The second factor behind the decision was a recent SRC review of computer science. This had found a troubling division in professionals' attitudes towards the centrality/marginality of AI (SRC 1972: 19). And the third was Michie's recent (failed) application for a seven-year grant, which had included a request for a PDP-10, a machine larger than any the SRC had ever provided for research in any discipline (Michie 1972).

The Report, submitted in September 1972, had been intended as an internal document for the SRC's eyes only. But it was so controversial that the Council decided (uniquely, so far as I can discover) to publish it—and with four commentaries alongside it (SRC 1973).

The original version had contained corrosive personal comments about Michie, who'd been *very* generously funded by SRC. One member of the relevant SRC committee (who prefers to remain anonymous) recalls, for instance, that they were shocked to discover that Michie's widely shown film of FREDDY was speeded up by sixteen times, and that the assembly being filmed wasn't fully autonomous but was being controlled by someone at a keyboard. The defence, he tells me, was (1) that the measurements being used by the human controller were correct, so they *would* work in an autonomous robot; and (2) that the slowness was due to the human's being unable to tell the arm/gripper to move *up 7*, *left 21*, *down 3*, but having to type in each step incrementally: *UUUUUUULLLLLLLLLLLLLLLLLLLLLDDD*. That defence might have been grudgingly accepted if these shortcomings had been mentioned up front, but they weren't.

In short, Michie hadn't delivered what he'd promised, nor even what he'd announced. To be fair, he wasn't the only person playing this sort of game. Hans Moravec, a graduate student at Stanford when grant reports/applications were being prepared for the SHAKEY robot, remembers a similar deception:

> An entire run of SHAKEY could involve the robot going into a room, finding a block, being asked to move the block over to the top of a platform, pushing a wedge against the platform, rolling up the ramp [formed by the wedge], and pushing the block up. SHAKEY never did that as one complete sequence. It did it in several independent attempts, which each had a high probability of failure. *You were able to put together a movie that had all the pieces in it*, but it was really flaky. (interviewed in Crevier 1993: 115; italics added)

Moravec added: "Not that it fooled anybody: the DARPA people who were reading those reports had been students in AI a few years before!" So perhaps Michie's 16-accelerated film might have been viewed more indulgently had it not been for the composition of the committee. My informant tells me that the panel included highly influential theoretical computer scientists who would have had scant time for AI, which is largely experimental, even if it had been done by the Angel Gabriel.

The Lighthill Report fizzed with bubbles of contempt. The fiercest criticisms were excised before publication, although they'd already spread on the grapevine to some extent. However, everyone in the AI community was able to decode the expurgated text, considered as a *roman-á-clef*.

What caused the major scandal (as opposed to the insider gossip) was Lighthill's dismissive judgement of AI in general. Or rather, *what he took to be* AI in general (see subsection b).

He divided AI into three categories. Category A was Advanced Automation for specific purposes—including expert systems, speech recognition, and MT. Category C was Computer-based CNS research—including models of cerebellar function, associative memory, language, and vision. These two categories, he said, had only "a minor degree of overlap of interest" (Lighthill 1973: 6). They were closer to already established (code: respectable) disciplines—computer science and control engineering for A, neurophysiology and psychology for C—than to each other. The third was category B: the Bridge Activity of Building Robots, where "aims and objectives are much harder to discern but which leans heavily on ideas from both A and C and conversely seeks to influence them" (pp. 2, 17). It sought to "mimic" hand–eye coordination, visual scene analysis, use of natural language, and common-sense problem solving (p. 7).

Although the Report started by including all three categories within AI, it went on to treat category B alone as "AI" properly so-called. Broadly speaking, Lighthill approved of work in categories A and C. Indeed, in 1973 he told MIT that Stephen Grossberg was doing, as Grossberg has put it, "exactly what AI should have done" (Chapter 14.vi.a). "Bridging" projects, however, were lambasted.

The only AI person for whom Lighthill had a good word to say was Winograd—who'd clearly charmed him, as he charmed everyone else, with his refreshing modesty. But even Winograd's work, which he classified as C, was used (because of its reliance on detailed world knowledge) as a stick to beat category B (pp. 16–17).

Lighthill's tendentious definition of category B could be decoded even by outsiders as *What's valuable in B has almost all been pinched from A or C, neither of which have benefited from B—and they never will.* Insiders could just as effortlessly decode it as *The work done at Edinburgh that was directed by Michie, rather than by his colleagues Longuet-Higgins, Meltzer, and Richard Gregory*. And just in case someone hadn't already worked it out, there came a passage which—in the British context—pointed the finger straight at Michie:

> [Disappointments about category B arose because] claims and predictions regarding the potential results of AI research had been publicized which went even farther than the expectations of the majority of workers in the field, whose embarrassments [*sic*] have been added to by the lamentable failure of such inflated predictions. (p. 8)

So Lighthill said, for instance:

> There is . . . a widespread feeling that progress in this bridge category B has been even more disappointing [than in A and C], *both as regards the work actually done and as regards the establishment of good reasons for doing such work* and thus for creating any unified discipline spanning categories A and C. (p. 3; italics added)

> *[The] whole case* for the existence of a continuous, coherent field of Artificial Intelligence research (AI) depends critically on whether between categories A and C *there exists* a significant category of research that may be described as a "Bridge" category, B, as well as on the strength of the case for *any* researches in that category. (p. 6; italics added)

And again, there came a remark that pointed straight to Michie:

> Here, letter B stands not only for "Bridge activity", but also for *the basic component* of that activity: Building Robots. [This is] seen as an *essential* Bridge Activity justified primarily by what it can feed into the work of categories A and C, and by the links that it creates between them. (p. 7; italics added)

The motivations of Robot Builders were sneered at, being said to include "to minister to the public's general *penchant* for robots [from medieval fantasy to science fiction]", and perhaps "to compensate for the lack of female capability of giving birth to children" (p. 7). (One of Edinburgh's small team of roboticists, Pat Ambler, was a woman: perhaps, to this very Establishment male, she was invisible?)

He listed the "past disappointments" in all three categories, and especially in B, with some relish. The vast amounts of money spent on MT and general speech recognition had been "wholly wasted". (This, remember, was advice to a funder.)

Lighthill allowed that B-research on high-level programming languages had been useful. (These included Edinburgh's POP, used for most AI work in Britain for many years: 10.v.c.) He also allowed that computational modelling had encouraged "a new set of attitudes to psychological problems", by distinguishing "possible candidates for consideration and theories that simply cannot be made to work" (p. 12). But he classified this research (including SHRDLU, and early connectionist AI in general) as category C—i.e. not AI at all.

And his diagnosis? Here he pulled rank, using masterly understatement. "As a mathematician", he said blandly—his readers would know that he held Sir Isaac Newton's Chair—he was inclined "to single out one rather general cause" (p. 9). Namely, "failure to recognize the implications of the 'combinatorial explosion' ". (That was unfair: as we saw in Chapter 10.iii, the combinatorial explosion had been recognized since the late 1950s. Waltz-filtering, for instance, was developed to avoid it.)

The rubble created by this missile was widespread. Summaries and titbits from the Report appeared in the media: *Sun* readers might not be assailed by it, to be sure, but opinion-formers taking *The Times* or other broadsheets were. Moreover, Lighthill's eminence (besides holding Newton's Chair, he'd amassed twenty-four honorary doctorates by the time he died—Mialet 2003: 443) ensured that his judgements would be taken as authoritative by the general public, and by the scientific community at large. They'd even be *welcomed*, since—as explained in Section ii.f, above—most people were suspicious and/or fearful of AI in the first place. (A close parallel was Sir Roger Penrose's ill-informed but widely popular attack on AI, around 1990: Chapter 14.x.d.)

In short, the picture looked grim.

b. Clearing up the rubble

Such was the concern at Lighthill's conclusions that, besides publishing the Report (largely at Michie's insistence, I'm told), the SRC Chairman Brian Flowers announced in the Preface that "The Council would welcome readers' comments on the importance of artificial intelligence research, and the extent of the support the Council should plan to give to it." As a start, the SRC publication itself included four sections of commentary written by people with differing views.

The most important objection, which was made by many people, was that Lighthill's (regrettably vague) A–B–C classification, and his tendentious definition of category B, betrayed fundamental misunderstanding of the field. As the psychologist Stuart Sutherland put it:

> Area B [best interpreted not as Building Robots but as "Basic" research] has clearly defined objectives of its own. Its aim is to investigate the *possible* mechanisms that can give rise to intelligent behaviour, to characterize these mechanisms formally, and to elucidate general principles underlying intelligent behaviour. These seem to be valid scientific aims and are clearly different from those of work of types A and C. (N. S. Sutherland 1973: 22)

Similarly McCarthy:

> If we take [his] categorization seriously, then most AI researchers lose intellectual contact with Lighthill immediately, because his three categories have no place for what is or should be our main scientific activity—*studying the structure of information and the structure of problem solving processes independently of applications and independently of its realization in animals or humans.* (McCarthy 1974: 317)

Sutherland, Michie, McCarthy, and Hayes all pointed out that many examples of AI work couldn't be readily fitted into *only one* of Lighthill's three categories. All agreed that B-research didn't cover only robotics. (Lighthill's use of "Robot" was unclear in any case, for he sometimes included chess-playing programs—P. J. Hayes 1973: 39–40.) And Hayes remarked that the A–B–C classes "fit altogether too neatly" with the "idiosyncratic" views of three mutually hostile personalities at Edinburgh: Meltzer, Michie, and Longuet-Higgins (P. J. Hayes 1973: 43). Their interests, as well as their personalities, differed. (Meltzer was a theoretical computer scientist, and the first Editor of *Artificial Intelligence*; Longuet-Higgins, who'd won his FRS as a chemist, had already done seminal research on associative memories, language, and the perception of music: see Chapter 12.v.c and Longuet-Higgins 1962, 1972, 1976, 1979; Longuet-Higgins and Steedman 1971.) (An intriguing snippet: Lighthill and Longuet-Higgins had been separated by only one year at Winchester public school, where they used to work on maths conundrums together—H.C.L.-H., personal communication.)

Definitions can always be nit-picked, of course. But as Sutherland said (1973: 23), definitions *matter* when what's being discussed is which areas should be supported by a funding council, and which should not.

Lighthill's misunderstanding wasn't too surprising, for his "independence" was close to ignorance. He'd spent only two months investigating AI, and hadn't contacted some whom one would have expected to be approached. His fifty-one names included many non-AI people, and were mostly British—although McCarthy, Minsky, Winograd, David Hubel, Bertram Raphael, and Ira Pohl were also listed. Indeed, it appeared that he'd spent most of his time talking to people at Edinburgh (Hayes 1973: 37). That was too bad. Even Lucasian professors need to do their homework.

The main set of Comments published alongside the Report was written by Sutherland, Professor of Experimental Psychology at Sussex. This was a masterly demolition of Lighthill's paper, explaining why psychological AI is the intellectual core of cognitive science.

The other comments came from Longuet-Higgins (by then, also at Sussex), Roger Needham, and Michie. Longuet-Higgins (1973) praised Lighthill's paper as "shrewd", "penetrating", and "comprehensive", and (predictably) endorsed his positive valuation of category C. But, he said, Lighthill had been too concerned with the brain's hardware, as opposed to its software. Even Marr's abstract models of the neocortex (14.v.c–e) would "for some time to come" be less valuable for the cognitive sciences (*sic*: see 1.ii.a) than non-neurological AI theories of cognition would be.

The most like-minded response came from Needham, a leading computer scientist at Cambridge (see Preface, ii). But even he admitted that the A–B–C classification was "contentious" (SRC 1973: 32).

The commentary didn't stop there. The UK's/Europe's AI Society ran two reviews in their newsletter. One was a delicious two-page spoof supposedly written by "Sir Grogram Darkvale FRS" (the ogre's name having been turned upside down), but actually authored by Sussex's Max Clowes (Anon. 1973). The other was a more serious piece, by McCarthy's collaborator Hayes—who defined AI as "the development of a systematic theory of intellectual processes, wherever they may be found" (1973: 40).

Meanwhile, across the seas, McCarthy (1974) reviewed Lighthill for the *Artificial Intelligence* journal. He allowed that AI faced great difficulties, and had been "only moderately successful" (p. 322). Nevertheless, he concluded that "Lighthill has had his shot at AI and missed" (p. 321). This was hardly surprising, since the missile's goal had been so badly defined.

(In private, of course, the sparks flew even higher. Hayes was heard to remark that accusing roboticists of having repressed maternal instincts was like accusing hydrodynamicists of suffering from premature ejaculation.—Lighthill, you'll remember, was a hydrodynamicist.)

Certainly, Sutherland, Hayes, and McCarthy were all *partis pris*. Even so, their reviews would astonish the innocent reader. Rarely can such an important piece of advice on scientific funding, and by such a hugely eminent author, have been so shoddy. (An Edinburgh friend, who shall be nameless, told me that someone he knew phoned Flowers to say: "You idiot! If you wanted to bury Michie, why didn't you pick someone who'd do the job properly?")

Michie, of course, was the *parti* most *pris* of all. He didn't lie down quietly. Indeed, it was largely due to him that the Report had been made public in the first place. His official reply filled eight pages in the SRC's Lighthill booklet. This concentrated on the substantive issues, and on the implications for funding needs in the UK (for a PDP-10, for instance, and an interface to the ARPAnet). To reach a much wider readership, he also wrote four papers timed to coincide, more or less, with the publication of the Report. One was for the popular *Computer Weekly*, for whom he was soon writing a regular column ("Michie's Privateview") in which AI was often lauded (Michie 1973*a*). Another, comparing the vast sums spent on largely irrelevant nuclear physics with the much smaller amounts needed for practically useful AI, appeared in the equally popular *New Scientist* (1973*d*). A third, specifically defending Edinburgh's AI work, graced the pages of the business-oriented *Management Informatics* (1973*b*). And the last, aimed at his scientific peers, was for *Nature* (1973*c*).

He didn't stop there. Within a twelvemonth, he republished some of his less technical articles (written from 1961 onwards), aimed not at AI specialists but at administrators and scientists in general (Michie 1974). Other trade books on AI would eventually follow (Michie 1982; J. E. Hayes and Michie 1983; Michie and Johnston 1984). True to form, these pulled no punches: the question of the potential of AI "is believed by some to be the most consequential ever posed" (J. E. Hayes and Michie 1983, p. ix).

But the damage was done: journalists, businessmen, and even scientists were more aware of Lighthill's scandalous conclusions than of the detailed rebuttals. The Lighthill Report thus undermined the morale of AI researchers in the UK. Not only did they fear the SRC funding stop which Lighthill had recommended, but their scientific reputation in the eyes of outsiders (including alternative funders) had been lowered.

Among those who left for the USA as a result were Hayes and Brady, both then at Essex. (After helping to run the MIT AI Lab for some years, Brady eventually returned; he's now "Sir Michael" and Professor of Information Engineering at Oxford.) They didn't escape entirely, however, for the psychological shock waves were felt across the seas.

This is evident from contemporary issues of the *SIGART Newsletter*. Indeed, one of the best-attended sessions at IJCAI-1973, held in August at Stanford, was a hot-off-the-press video of the Royal Institution's (July) debate between Lighthill, Michie, Gregory, and McCarthy. (I remember the atmosphere's being more like a pantomime than an academic occasion: the dramatis personae drew repeated hisses and cheers from the Stanford audience.) And although various US colleagues tell me that they can't recall research funding being affected, there was a cutback in US money for robotics (Fleck

1982: 192). In addition, as noted above, ARPA started favouring mission-directed research. The historian Fleck grants that this happened in a social context of *general* cutbacks for 'irrelevant' scientific research (see i.b and c, above), but he believes that Lighthill was an additional factor (ibid.).

Whether the Report hugely delayed the advance of AI in the UK is controversial. The administrative leader of the Alvey Programme (see v.c, below) stated that Lighthill "dealt a heavy blow to AI research in Britain from which, in 1981, it had yet to recover" (B. Oakley and Owen 1989: 15). So did the post-Alvey evaluation commissioned by the government:

> Prior to Alvey [i.e. in the early 1980s] the UK AI community was small and fragmented. Government support for AI was minimal. The Lighthill Report of 1973 *had resulted in a significant reduction in AI funding and led to a brain drain of many key figures from the UK*. By 1980 skilled personnel were concentrated in a handful of academic centres. (Guy *et al*. 1991: 16; italics added)

The SRC itself had said much the same, in 1979:

> The Panel [on Proposed New Initiatives in Computing and Computer Applications] *has no doubt* that the reluctance of the present community to take up the challenge (of industrial robots research) is due at least in part to *the general discouragement of Artificial Intelligence* which took place in this country several years ago and that it is now up to SRC to take steps to remedy the situation. (quoted in Fleck 1982: 215 n. 87; italics added)

But Sloman disagrees:

> In the UK, it certainly did not nearly (even temporarily) kill off AI, as some people often say. The main effect was to facilitate the spread of funds for AI to other parts of the UK, so that it became less heavily concentrated in Edinburgh. (personal communication)

Although the funding for Edinburgh became less generous, they did get the PDP-10 and ARPAnet access that Michie had asked for. And other centres, Sussex included, were given access to it for SRC-funded AI research. So work didn't grind to a halt.

It's true that AI funding in 1973–83 didn't reach the level that had been recommended in the earlier SRC review (SRC 1972). But science funding in general during that period, in the UK as in the USA, fell as a result of the OPEC-initiated oil crisis. Much of the UK's 1970s AI was funded not as "Computer Science" but via the new SRC Panel on Cognitive Science. This, as it happens, was set up largely as a result of the anti-Lighthill arguments from Longuet-Higgins and Sutherland.

As for Michie, he was officially sidelined. A new Department of Artificial Intelligence (headed by Meltzer) was formed in 1974, which took over most of the resources, including the robotics equipment, built up under his leadership. He was shunted off into an independent Machine Intelligence Research Unit, and forbidden to work on robotics (Fleck 1982: 189).

But he was irrepressible. Through the rest of the 1970s he accepted many visiting appointments abroad—mostly in Virginia (with his ex-Bletchley colleague Jack Good) and Illinois, but also Canada and the USSR. At home, he spent much of his time in publicizing expert systems. Besides his contributions to the media, he set up a British Computer Society special interest group on expert systems, which ran its own conferences and newsletter.

By the early 1980s, he'd even achieved some distance from the crisis. He now explained the Lighthill incident as a "mishap of scientific politics" due to all-too-human frailties:

> Officialdom has subsequently indicated eagerness to repair the damage. Bodies such as the Science Research Council may find it hard to accept some of the remedies required—notably *the return from abroad and re-habilitation of some of those whose work was pilloried*.
>
> At the time, I felt amazement. Ecclesiastes, that incomparable analyst of the dark side, has warned about this feeling: "If you see in a province . . . justice and right violently taken away, do not be amazed at the matter; for the high official is watched by a higher, and there are higher ones over them . . .".
>
> A number of officials attempted at the time to palliate my wrath by explaining in just such terms some of the actions which their jobs had obliged them to take. I concluded that nothing but ignorance at the top could be the cause of the abuses, and that no good could be accomplished until the minds of men were better informed [and so I wrote many "popular" articles about the achievements and promise of AI]. (Michie 1982: 247; italics added)

How had he regained his urbanity? The "re-habilitation" had been made possible when the UK's AI nightmare was abruptly ended by loud noises from Japan. As we'll now see, this clarion call was shamelessly economic/political, not Legend-arily scientific.

11.v. The Fifth Generation

The third funding bombshell was constructive rather than destructive, rescuing non-military AI from the doldrums—and, to some extent, from other scientists' scepticism. It can be called a "bombshell" nevertheless, because its advent was unexpected and its immediate results explosive. For it came in the form of a challenge from Japan, one so frightening that it reinvigorated official sponsorship of GOFAI (and relevant aspects of cognitive psychology) on both sides of the Atlantic.

a. A warning shot from Japan

In October 1981 the Japanese used AI to fire a warning shot over the bows of the Western economies. It had huge repercussions, for it led to an injection of governmental and industrial funds on a huge scale. It was this which revivified the field in the UK, and which took it up to a new level of public interest in the USA and elsewhere. AI was suddenly ubiquitous: it was featured at length in *Fortune*, *Forbes*, and *Time* magazine (where a computer was chosen as "man of the year"), and even made the front cover of *Newsweek*. By 1985, the biennial IJCAI was attracting some 6,000 participants (twelve years earlier, there'd been only about 250).

Had the Japanese made some great intellectual advance? An AI equivalent of the genetic code, perhaps? No. What caused the excitement, and the frissons of fear, was Japan's announcement in October 1981 of a ten-year national plan for developing "Fifth Generation" computers.

This came to most people, including journalists, as a bolt from the blue. The worldwide AI grapevine, however, had had fair warning. The project's overall director, Tohru Moto-Oka (1929–85), later recalled:

[Before the conference] there had been workshops and so forth to hear the opinions of international scholars; through [Japan's IT association] there were discussions with research institutions of the principal countries of Europe and America, and some information was obtained from a survey visit to America.

In addition, there were invitations to participate from [Japan] to American and European governments, and because the project had already become well known abroad, more than eighty overseas participants attended the Fifth Generation Computer Conference.

Before this conference I spent a month . . . touring America, Germany, France, and Great Britain, in preliminary discussions with participants [including Colmerauer and Feigenbaum]. (Moto-Oka and Kitsuregawa 1985: 6)

What caused the huge public—and political—excitement was less the ambitious technological project than Japan's economic motivation for launching it. They weren't seeking to appropriate just another market, to be added to cameras and video equipment. Their aim, baldly stated to the Western politicians and industrialists they invited to the "Announcement" meeting in Tokyo, was world domination in information technology (Moto-Oka 1982). Pre-eminence here, they argued, meant economic pre-eminence *tout court*.

Indeed, as the opening speech from Moto-Oka made clear, for Japan it meant economic survival:

Japan . . . cannot attain self-sufficiency in food [or energy]. On the other hand, we have one precious asset, that is, our human resources. Japan's plentiful labor force is characterized by a high degree of education, diligence, and high quality. It is desirable to utilize this advantage *to cultivate information itself as a new resource comparable to food and energy*, and to emphasize the development of information-related, knowledge-intensive industries which will make possible the processing and managing of information at will. (quoted in Feigenbaum and McCorduck 1983: 12; italics added)

Similarly, an official of the Ministry of International Trade and Industry, the body overseeing the Fifth Generation programme in Japan, told an American journalist:

Until recently we chased foreign technology, but this time we'll pioneer a second computer revolution. If we don't, we won't survive. (Feigenbaum and McCorduck 1983: 135)

In other words, industrial economies needed AI—and Japan, lacking rich agricultural or oil/mineral resources, needed it most of all.

Fifth Generation computers were thus defined not (like the first four generations) by their hardware components, but as computers designed to be suitable for supporting AI applications (Moto-Oka and Kitsuregawa 1985). This included not only parallel computers but also powerful machines dedicated to a specific AI language, such as LISP or—especially—the recently developed PROLOG. (For several years, PROLOG's implementer Kowalski virtually lived on the plane from England to Japan.)

As it turned out, LISP machines didn't catch on widely—not even as exotic workstations for the laboratory bench. Designed at MIT's AI Lab in the mid- to late 1970s (Greenblatt 1974; Bawden *et al.* 1977; Greenblatt *et al.* 1984), they were eventually launched—amid much excitement, fanned by the goings-on in Japan—by two companies set up by MIT personnel (Symbolics and LMI). But they were very expensive, and—one Symbolics employee believes—suffered guilt by association when the over-inflated early 1980s expectations for AI failed to be satisfied (Withington 1991). The same was true of the massively parallel, and massively expensive, Connection Machine—also designed at the MIT AI Lab (Hillis 1985).

But that was for the future. Meanwhile, there was the breathtaking announcement in Tokyo. This was astonishing in terms of both the scale and the nature of the Japanese commitment.

The Fifth Generation plan was to be jointly funded by the country's government and electronics industry, to a minimum of $810 million—some observers were already speaking of billions. (As it turned out, the Japanese government provided less money than any of the eight companies was devoting to its own R & D programme, and vastly less than the $1 billion per year that IBM was spending on its research—Newquist 1994: 212.)

What's more, the project had been allowed to overturn the traditional Japanese attitude to seniority. The director of Tokyo's newly formed AI institute (ICOT) was a surprisingly young man, in his early middle age. He insisted that ICOT be staffed only by *very* young computer scientists, under 30 years old, whose training was up to date and whose creativity was still comparatively high. Each of the Japanese companies involved would be expected to provide four or five of their best young researchers, free of charge to ICOT.

Not the least surprising aspect of the Fifth Generation project was that it was officially announced to an *international* audience. The declared reason for this was that, in this very expensive R & D area, competitors in the capitalist marketplace needed to *cooperate*.

Such cooperation would soon be initiated in Texas. August 1982 saw the foundation of MCC (the Microelectronics and Computer Technology Corporation) in Austin. The AI section was headed by W. W. (Woody) Bledsoe (1921–95), and the overall director was Vice-Admiral Inman—previously Deputy Director of the CIA, but described by Douglas Lenat as "the most charismatic person I'd ever met" (Shasha and Lazere 1995: 234). (In 1993 Inman would be nominated as Secretary of Defense by President Clinton; a few weeks later, however, he decided against accepting the post, which was given to William J. Perry instead.) The good impression was evidently mutual: MCC would soon provide the tens of millions of dollars required for Lenat's CYC project (see 13.i.c).

b. Self-defence in the USA

Capitalist cooperation—and in Texas, of all places!—was just one example of the huge effect this announcement had on AI research funding in the USA, and in the UK and Europe too. The 6,000 participants of IJCAI-1985 included venture capitalists as well as ordinary businessmen, all prepared to provide money for AI research.

The money reached both 'pure' and 'applied' AI workers. Quite apart from the funds coming directly or indirectly from the US government, the newly founded AAAI benefited hugely. Feigenbaum, having been a core AI researcher for many years and having also got wind of the incipient challenge from Japan, was a leader in establishing AAAI in 1980 (Feigenbaum 2005; Reddy 2005). Unlike its long-time UK/European predecessor, AISB (see 6.v.c), it found itself showered with money. Hayes, a refugee from the Lighthill debacle across the Atlantic (who'd be elected President of AAAI a few years later), was impressed—and not a little envious:

> I remember having a kind of distant amazement that the USA could find so much money in such a short time. AI societies in Europe were run on shoestrings, almost entirely by volunteer academics, but AAAI had piles of cash from day one (well, actually maybe something like day three) thanks to the AI explosion and the trade fair at the early meetings. Nothing like that had happened outside the USA. (Hayes, interviewed in 2005—Reddy 2005: 11)

Even inside the USA, however, nothing like that would have happened if it hadn't been for the Fifth Generation. In short, AAAI blossomed not just because the USA was rich, but because it was frightened.

Two AI pioneers, Feigenbaum and Buchanan, founded Teknowledge, the first commercial expert-systems company in the USA. (A much smaller enterprise had been started by Michie in the UK, but Teknowledge was on an altogether more ambitious scale.) They had no trouble finding eager recruits:

> [We] hired as many smart people as we could afford. At one point, I had taken a leave from Stanford to manage the company and a friend at IBM Watson labs complained to me that IBM couldn't hire enough talented AI scientists because Teknowledge was hiring them all. (Buchanan 2003: 3)

The task of this new company was to write expert systems to commission and/or to develop off-the-shelf shells for an indefinitely wide range of clients. At much the same time, Feigenbaum also co-founded a specialist medical company called IntelliGenetics. This was based on Stanford's MOLGEN expert system, which advised biochemical researchers on how to clone specific DNA sequences. Other software, and hardware, AI companies soon followed them.

(Few of these companies prospered to the expected degree. The author of one history of these events puts this down to a number of reasons, not least the "egos, frailties, and foibles" of scientific researchers with no head for business—Newquist 1994, p. xiii.)

Some commentators compared the Fifth Generation's influence in stimulating scientific research in the USA to that of the Soviet Sputnik in 1956. (In part, this was because the USA—unlike Japan—saw the Fifth Generation largely in terms of its military significance.)

Hard-headed businessmen and ambitious politicians, naturally, had scant interest in psychological AI: technological AI was what they cared about. Nevertheless, AI in general shared the benefits: not only would more young people need to be trained in this new technology, but basic AI research would be needed too.

Moreover, Japan's stated goals—which included MT, speech processing, intelligent assistants, advanced problem solving, and robotics—would require advances in psychological AI, even if the human example was to be dropped as soon as it became inconvenient. By the same token, they'd require advances in other areas of cognitive science, especially psychology.

AI, at last, had been shown to be *useful*. And it was being widely *said* to be useful. Feigenbaum wrote a trade book, which sold over 200,000 copies in the English-language edition (plus several translations), outlining 'Japan's Computer Challenge to the World' and arguing that hugely increased funding for AI was needed if the USA were to retain its economic success (Feigenbaum and McCorduck 1983).

His AI colleagues weren't greatly impressed, although they'd be among the beneficiaries if his wake-up call succeeded. For even if one ignored the vulgarity of the tone (which wasn't easy to do), many of the judgements were questionable. However, that didn't worry most of the readers, who didn't know what questions were relevant.

The book was hugely influential—if not within the field, then outside it. It swiftly propelled AI onto the TV screens, and Feigenbaum onto the chat shows. Quite apart from his (well-deserved) status as a pioneer of expert systems, he had special access to the Japanese developments. For his wife, the computer scientist Penny Nii, was Japanese herself. Five years later, they'd cooperate in a book brashly proclaiming the business successes recently achieved by AI technology (Feigenbaum *et al*. 1988).

One can't say that the hype had ceased: far from it. (McDermott's lessons hadn't been fully taken on board.) Both of Feigenbaum's volumes, not to mention the journalists' offshoots, needed to be taken with a pinch of salt.

So did Winston's *The AI Business*, which appeared hot on the heels of Feigenbaum's *The Fifth Generation* (Wilson and Prendergast 1984). The book's nicely ambiguous title should have warned readers that it was a piece of advertising, not a mere report of scientific advance. Besides straightforward accounts of existing expert systems (including one for configuring VAX machines uniquely to suit clients' needs, which had already saved its parent company DEC a fortune: Kraft 1984), this collection contained its fair share of shaky predictions. It even offered a tendentious editorial defence of past NewFAI claims, such as Simon's notorious vision of a chess program beating the world champion by the mid-1960s (see Section vii.a–b). The people making those predictions, said Winston, were conscientious scientists "simply trying to fulfill their public duty" by preparing people for something that seemed quite plausible at the time (Wilson and Prendergast 1984: 3).

Even NASA, whom one might have expected to be more hard-headed, fell foul of the demon of hype. The manager of NASA's AI research admitted as much, a few years later:

> [We held] a week-long workshop in April 1985 in which each NASA center got to describe the AI and robotics projects that it had under way. By the end of the second day, the total number of expert systems *that had been described as having been developed* was about 100. However, *none of these expert systems ever became operational*.
>
> NASA's AI research program *was not immune to overselling and overpredicting*. The objectives stated [in an official "white paper" of 1986] for the program during its inception [were hugely ambitious]. These goals were to be achieved by 1995! (Montemerlo 1992: 51–2; italics added)

The hype of the early 1980s (especially at NASA) was largely due to inexperience. As work on automation and expert systems progressed, people's expectations matured and their predictions became more realistic. By the mid- to late 1980s, NASA was turning "from revolution to evolution", and devising many useful systems—partly by picking the "easy" problems first, and partly by including in-the-loop human control (Montemerlo 1992: 53–7). (An insightful defence of the need for humans to remain in the loop was published in 1990 by Dreyfus's disciple Collins: 13.ii.b.)

NASA wasn't alone. The VAX configurer (XCON) and its successor XSEL (J. P. McDermott 1980, 1982) was providing a crucial service to DEC's clients *and* saving the company over $40 million a year (Russell and Norvig 2003: 24). From 300 rules (and very low accuracy) in 1979, it had grown to more than 3,000—and could now reliably configure ten different DEC computers, not just one (Bachant and McDermott 1984).

XCON/XSEL was the top success story. But there were now many other proven money-savers. At last, AI was successful enough, and visible enough, to attract huge financial support from both government and industry. The AI industry in the USA burgeoned from a few million dollars in 1980 to billions of dollars in 1988.

It wasn't all good news: as usual, the hype became counter-productive. The boomerang effect peaked around 1990. Misled by the optimism, not to say disingenuousness, of AI's vociferous champions, the industrialists expected too much—and met with inevitable disappointments. The period of 1980s largesse was followed by a brief "AI Winter", in which "many [AI] companies suffered as they failed to deliver on extravagant promises" (Russell and Norvig 2003: 24). Eventually, the promises grew less extravagant—and were very often delivered.

(Today, AI is—in practical terms—even more successful. But it's much less visible. Both GOFAI programs and neural networks are hidden inside a myriad of everyday products and services, from cars to refrigerators, call centres to credit checks. That's partly why, when people mistakenly say that GOFAI has "failed" as a technology, they're often believed: see 13.vii.b.)

For all the obvious cultural reasons, the main effect in the West of the ICOT initiative was seen in the USA. Indeed, Feigenbaum's first book relentlessly exploited American fears that US economic and political interests, and ascendancy, were threatened.

Describing the Fifth Generation as "comparable in human intellectual history to the invention of the printing press, with the certainty of making even greater changes in the life of the mind than books did", he chided counter-cultural American intellectuals for seeing the computerization of university campuses not as an opportunity but as "the new barbarism" (p. 210). "Most self-styled intellectuals", he complained in disgust, "don't even recognize what's happening" (p. 211). As for more explicitly political comments, he said:

> The Japanese plan [for AI applications] is bold and dramatically forward-looking. It is unlikely to be completely successful in the ten-year period. But to view it therefore as "a lot of smoke", as some American industry leaders have done, is a serious mistake . . .
>
> We now regret our complacency in other technologies [small cars, videos, computer chips]. Are we about to blow it again? The consequences of complacency . . . will be devastating to the economic health of our most important industry. The Japanese could thereby become the dominant industrial power in the world. (p. 2)

> *We believe that Americans should mount a large-scale concentrated project of our own; that not only is it in the national interest to do so, but it is essential to the national [military] defense.* (p. 216)

> A superiority in knowledge technology provides whoever holds it with . . . an unequivocal advantage—whether we are speaking of personal power, national economics, or warfare.
>
> The Japanese understand this perfectly . . . Other nations recognize the soundness of the Japanese strategy—and, of course, its inevitability. In response to the farsighted Japanese, ambitious national plans are being drawn up in many places. But the United States, which ought to lead in such plans, trails along in disarrayed and diffuse indecision. (p. 239)

He got his wish. The US government, via ARPA in the Department of Defense, committed $1,000 million over five years—twice as much as ARPA's total expenditure on computing over the previous twenty years.

c. Lighthill laid to rest

In the UK, the response to the Japanese initiative was less up front in tone. But it happened. However, it couldn't happen as quickly as it did across the Atlantic. Quite apart from cultural differences in attitudes to innovation and entrepreneurship, the UK was still carrying the baggage of the Lighthill Report.

If this had been an embarrassment before, it was doubly so now. Perhaps triply. For Michie, as he often said (e.g. Michie and Johnston 1984: 38), had written the very first commercially marketed expert-system shell, back in the 1960s. (Called AL/X, Ray Reiter wrote his Illinois MS thesis on it in 1981—and see Harmon and King 1985: 109.) The prospect of hearing "I told you so!" from him was not one his former detractors found agreeable.

Accordingly, yet another report was commissioned by the SERC—the new name for the SRC (Alvey Committee 1982). This one, too, was entrusted to an eminent mathematician: Sir Peter Swinnerton-Dyer—although the criteria he was given ignored scientific importance, being based only on how information technology could aid UK industry in general (Alvey Committee 1982: 5–6; cf. Cornwall-Jones 1990). And this time, the intended verdict was delivered: a green light for AI.

Given the hangover from Lighthill, however, the term "artificial intelligence" was declared inadmissible (a further reason why, in the UK at least, AI has low visibility today). The powers-that-be insisted on "Knowledge-Based Systems" instead. (So when Sussex received new money for "technology transfer", in the form of thirty guaranteed M.Sc. studentships a year, we had to change the name of the already existing course: AI became IKBS.)

The key strategy group involved, which included Michie, had no illusions about this. A couple of months before the Japanese announcement, they'd met to discuss the future of AI in Britain:

> "A bunch of us had felt for quite a time that we really ought to try to do something about rehabilitating AI in the U.K. after Lighthill." . . . [The aim was to set up a special SERC programme in AI.] "We were all sitting around a table wondering what to call this new area", says John Taylor [a big wheel in the Admiralty's Weapons Establishment, and in the computing section of SERC]. "Should we call it artificial intelligence? *We didn't want to call it artificial intelligence because of all the Lighthill connotations*, and we didn't want to call it expert systems, and we came up with this awful phrase 'intelligent knowledge-based systems' or IKBS." (B. Oakley and Owen 1989: 15; italics added)

For public consumption, Lighthill discreetly wasn't mentioned. (The official evaluation of the Alvey project would be less coy—Guy *et al*. 1991: 15–16.) The explanation given for the change in terminology was that "AI" was overambitious and unnecessarily provocative, whereas "IKBS" was not.

As a psychological observation about people's prejudices and mental associations, this was doubtless correct. But a moment's clear thought would have shown that the term "knowledge" was no less philosophically problematic than "intelligence"—which anyway hadn't been dropped, but merely transformed into an adjective. Some people, accordingly, preferred the term "inference computing" (B. Oakley and Owen 1989: 270). But it didn't catch on. (Feigenbaum now favoured KIPS: knowledge information processing systems, of which expert systems were a special case.)

After the Japanese fired their public warning shot two months later, this budding SERC activity was merged into the national Alvey Programme, which spent £350 million over five years on advanced information technology (B. Oakley and Owen 1989: 294). Business and war were both represented: besides the SERC, the money came from the Department of Trade and Industry and the Ministry of Defence.

Of the four main areas of IT involved, one was AI (the others were man–machine interfaces, VLSI, and software engineering). It was agreed at governmental level that £21 million should go via the SERC to universities for AI and computer science (half each)—plus more money to work done, and 50 per cent funded, by industry (Alvey Committee 1982: 2). This would be partly for immediate/short-term demonstrations of IKBS systems with clearly commercial uses, and partly for longer-term, including highly general, AI research (Alvey Committee 1982: 17). (This was the first time that the SERC, supposedly an autonomous body, had been instructed by government to set aside a certain sum of money for university research in a particular area.)

In addition, the UK was a contributor to the European ESPRIT Programme, which had a budget of $650 million from governments and another $650 million from industry. Other European countries, too, supported their own AI research more generously—in some cases ensuring that most of the increase went on basic, not applied, research (Dickson 1986).

Looking back on the five-year Alvey Programme, its director, Brian Oakley, asked 'Was It All Worthwhile?' (B. Oakley and Owen 1989: 265–94). His answer wasn't an unqualified "Yes". For the hoped-for cooperation between industry and academia, always a problematic area in Britain, had been less fruitful than expected. In some cases—the work on speech technology, for example (a dozen projects, the largest of which was at Edinburgh)—the major industrial funder had pulled out after deciding that progress wasn't fast enough (p. 291).

Nevertheless, AI applications—in connectionist pattern recognition as well as GOFAI-based data handling and expert systems—had burgeoned. So had work on human–computer interfaces (13.v): "for many years to come human intervention, and so the significance of the human interface, will remain essential for most applications of AI" (B. Oakley and Owen 1989: 271). Hardware issues had progressed, and software engineering had benefited too. Market awareness had been hugely raised, which had been one of the aims of Alvey from the start. "It remains only a matter of time", said Oakley, "before inference-computing applications far exceed today's so-called conventional uses of computers" (p. 284).

Finally, the need for interdisciplinarity had been underlined. The Alvey directorate, Oakley reported, had exerted "a bit of pressure" to foster cooperation in the face of "the rigid departmental structure of universities" (p. 292).

Despite the call for interdisciplinarity, Oakley—and Alvey—was primarily concerned with AI as an economic force, not as basic and/or psychologically oriented research. The same was true of the effort in Japan, and of the responses in Europe and the USA.

Indeed, Minsky complained that the success of the current expert systems "bodes ill for making further progress", because commercial companies weren't prepared to look ahead—to incorporate learning, for instance. In particular, he said:

> There is no significant increase in the number of people working on the ideas that we will want to use in ten years. The number of people doing *basic* research in Artificial Intelligence is probably under one hundred people and maybe under fifty. (Minsky 1984*a*: 245; italics added)
>
> [Large companies who boast about their AI groups are loath to provide small amounts of money to support a few students.] They do not seem to understand where the ideas came from and where the new ones will come from in another decade. (p. 251)

For all that, the effects on AI in general had been broadly beneficial, especially in the UK where (thanks to Lighthill) Establishment scepticism had reigned. By the late 1980s, Oakley saw a change in the "fashion" for public support of research in the West, with "a renewed emphasis on basic research in academia and a weakening of academic/industry ties" (B. Oakley and Owen 1989: 292–3). Lighthill had been laid to rest at last.

11.vi. The Kraken Wakes

John Wyndham's sci-fi novel *The Kraken Wakes* narrates the havoc caused when a sea-monster, asleep in the depths of the ocean for many years, suddenly becomes active again. From GOFAI's point of view, the Kraken was connectionism. This (eighth) bombshell comprised an intellectual watershed for AI, as well as a financial one.

a. Small fry and sleeping draughts

Connectionist AI was spawned in the 1940s (Chapter 12.i). And some of the small fry had swiftly grown appreciably larger. So in 1958 Rosenblatt published his seminal paper on "Perceptrons"—in the very same volume of *Psychological Review* which, a few months earlier, had carried the GPS authors' 'Elements of a Theory of Human Problem-Solving' (Newell *et al.* 1958*a*).

Minsky and Papert, however, were (at that time) less eclectic than the editor of *Psychological Review*. From 1956 (i.e. 'Steps') onwards, they expressed fundamental doubts about the value of perceptrons. The small fry swam on, regardless—until the MIT duo administered a powerful sleeping draught in the late 1960s (12.iii).

But the ensuing slumber was a doze, not a coma: important connectionist research was still going on beneath the waves. In 1979 connectionism raised its nose above the waters, at an interdisciplinary meeting in La Jolla (12.v.b), and the whole of the Kraken's head became visible soon afterwards (12.vii.a).

b. Competition

Worse was to come. The monster leapt high out of the ocean in 1986, on publication of the two-volume connectionist 'bible' (12.vi).

Almost immediately, the USA's main research sponsor had second thoughts about its generous funding of GOFAI. Late in 1987, DARPA sponsored a detailed two-month study to reconsider their policy (12.vii.b). The result was a shift in their priorities. To be sure, symbolic AI wasn't put in the dire position that connectionist AI had been in before. (Since the late 1960s, ARPA/DARPA had favoured the former almost to the exclusion of the latter—not least, because of Licklider's closeness to Minsky.) But the limited research money now had to be shared.

Perhaps even more to the point, the flow of research students into GOFAI slowed. That was no accident. The connectionist bible had been written, priced, and marketed as a deliberate attempt to induce youngsters onto the connectionist pathway through the AI field (12.vi.a).

Outsider interest shifted as well. Many psychologists and (especially) philosophers transferred their concern from GOFAI to connectionism, which promised a more plausible account of concepts (12.x). Symbolic AI was commonly said to have failed. As remarked above (i.d), one high-status collection on the symbolic/connectionist AI debate was subtitled *False Starts, Real Foundations* (Graubard 1988)—hardly a neutral way of putting it.

Coincidentally, Haugeland's patronizing definition of GOFAI dates from this general period. His critique was driven by phenomenology, not connectionism—which wasn't even discussed (the bible was still in press when his book appeared). But his readers would be even more ready to accept his criticisms in the pro-connectionist climate.

The journalists, too, swam with this tide. They saw only the connectionist Kraken, dismissing the still-advancing GOFAI as a wounded minnow floundering in the shallows. Symbolic AI was no longer glamorous, no longer sexy. And cognitive science in general, if it was to retain respect and gain new adherents, had to follow in the Kraken's wake.

The history of this intellectual watershed, and of GOFAI philosophers' attempts to meet it in a principled fashion (12.x.d), is detailed next, in Chapter 12. Then, in Chapter 13, we'll see how symbolic AI fared in the aftermath.

12

CONNECTIONISM: ITS BIRTH AND RENAISSANCE

"Only connect", said E. M. Forster as the motto of *Howards End*. Connectionists say this too. But whereas the novelist was talking about people, they're talking about neurones—or, more accurately, abstract computational units inspired by neurones. This chapter explains the "connect", and casts doubt on the "only".

If connectionism reminds one of *Howards End*, it also reminds one of *The Sleeping Beauty*. For the infant field, spawned in the 1940s by cybernetics, went into hibernation in the late 1960s. Ironically, the sleeping draught had been forced down the Beauty's throat in a no-holds-barred attack by one of her earliest suitors—an extraordinary story, told in Section iii.

Her heart was still ticking over during dormancy, however, and some important thinking was done during the twenty-year sleep. She stirred visibly in 1979, aroused by an interdisciplinary get-together on the Californian coast. The return to full activity happened, amid a blaze of publicity, in the mid-1980s. She rose from her bed and danced around the world, posing for the newspapers and entrancing almost all who met her.

The awakening of connectionism invigorated many philosophers and psychologists, but it constituted a bombshell from the point of view of symbolic AI (see 11.vi). Indeed, the few who remained unentranced by the merrily dancing Beauty were long-time devotees of GOFAI (x.d, below). But a bombshell, despite delivering a very nasty shock, need not end one's life. It may leave certain strengths unharmed—and even unsurpassed. The sections below (viii–ix) which cast doubt on the "only" show why GOFAI didn't leave the floor. It would continue steadily on its way during the next quarter-century (a story told in Chapter 13), achieving results which connectionism couldn't—and still can't—match.

That human minds *do have* rich associative powers had been obvious for centuries. And two early twentieth-century writers had provided a host of intriguing examples illustrating the subtleties involved. One was Sigmund Freud, whose work on dreams and slips of the tongue suggested countless strings of surprising, but psychologically possible, associations of ideas (see Chapter 5.ii.a). The other was the literary scholar John Livingston Lowes (1930), a specialist on Samuel Taylor Coleridge's poetry. In a masterly detective story hunting out the sources of the imagery in *The Ancient Mariner* and

Kubla Khan, he gave highly detailed, and intuitively convincing, hypotheses about just where Coleridge's ideas had come from. But in answer to the question *How?*, all he could say was to repeat Coleridge's own metaphor of "the hooks and eyes of memory" (cf. Boden 1990*a*, ch. 6).

In other words, by 1940 *the fact that* our memory is richly associative was clear, but *how it manages* to associate wasn't. Indeed, things had been made worse by Karl Lashley's recent failure to find localized memories (5.iv.a). As he put it, "I sometimes feel, on reviewing the evidence . . . that learning just is not possible. *It is difficult to conceive of a mechanism* which can satisfy the conditions set for it" (Lashley 1950: 477–8; italics added).

The connectionists tried to answer that *How?* question, in scientific rather than merely intuitive terms. They hoped to explain not only the maverick associations found in poetry or dreams, but also everyday perception, language, learning, and rational thought. And they showed how, in principle, a memory could be distributed over a whole network of cells, rather than being stored in one place.

Connectionists thought of the hooks and eyes of memory in two different, but closely related, ways. On the one hand, they wanted to define idealized computational networks, made up of many simple interconnected units, which could in principle underlie this or that psychological phenomenon. On the other, they wanted to identify the computations actually carried out in the brain, and to understand how the relevant brain mechanisms implement them.

The first is the aim of connectionist AI/psychology, the second of computational neuroscience. The two disciplines are discussed here and in Chapter 14, respectively. Some people's research was guided by both aims, so is mentioned in both chapters. Walter Pitts and Warren McCulloch in 1947, for example, defined two novel types of information processing *and* related them to specific neural structures. And Albert Uttley, in the early 1950s, focused on real neurones as well as artificial ones—and turned towards neuroscience even more strongly in his mature work.

Connectionism seeks to explain (and to model) psychological capacities that intrigue almost everyone: perception, memory, analogy, creativity, language, learning, and development. It puts computational flesh onto the associative bones sketched by Freud and Livingstone Lowes, suggesting how haunting images can arise—whether neurotic or poetic. And it carries a strong whiff of the brain. So far, so good.

At base, however, it's the study of two highly abstract topics: the configuration (anatomy) of neural nets and the functioning (physiology) of learning rules. Inevitably, then, much of this chapter concerns the mathematics of various learning rules and nets. I'll spare you (and myself) the equations, for this isn't a practitioners' manual. Even so, some paragraphs may be tough going for some readers. The overall story, however, should be readily intelligible—and exciting, too. For connectionism has aroused heady hopes, passionate controversies, huge media interest, and intense philosophical debate.

The first two sections below focus on the opening quarter-century of connectionism: first, the initial ideas, whose implementation—if any—could only be highly schematic; second, some pioneering computer models. Section iii deals with a notorious 1960s critique of some of that early work, which is widely (though perhaps unjustly) accused of having set the field back for almost twenty years. What was actually going on in those years of hibernation is described in Sections iv and v.

The heyday of parallel distributed processing (PDP)—which is what most people today take "connectionism" to mean—is discussed in Sections vi and vii, as is the explosion of the "bombshell". Sections viii and ix consider how PDP research later tried (and failed) to recover some of the lost strengths of GOFAI. Finally, Section x explains why many philosophers were excited by PDP, and how some contributed to its development.

12.i. Lighting the Fuse

Connectionism's fuse was laid down in the mid-eighteenth century, and patted into the ground more firmly several times over the next 200 years. It wasn't until the 1940s, however, that it began to smoulder. That happened when McCulloch and Pitts (1943) defined idealized neural nets based on logic (see 4.iii–iv).

During the 1930s, neural feedback loops, or reverberating circuits, had often been posited to explain psychopathology of various kinds (4.iii and v). But by the early 1940s, McCulloch was thinking of them mostly as the brain's *normal* activity. As for how they might be acquired, he and Pitts suggested in their 'Logical Calculus' paper (1943) that learning could—in principle—be modelled by cyclic nets.

Even then, there was no intellectual explosion—only a dull glow. Initially (as we saw in Chapter 4.iii.f), their paper faced hostility or indifference from most psychologists. One reason was its neurological implausibility. Another was its rebarbative abstractness. And a third was its blue-sky character: at that time, its ideas could be implemented only in a very primitive fashion (with soldering irons to the fore). The dull glow wouldn't brighten until computers came on the scene.

a. A long gestation

Connectionism was conceived almost three centuries ago (in the 1740s), but had a long gestation. In its embryonic form, it was merely biologized introspection: David Hartley's view that thinking is grounded in associative mechanisms in the brain (2.x.a). These, he said presciently, make two things possible. First, they link different concepts so that the one comes to mind when one thinks of the other. Second, they enable a mere fragment to recall a whole stimulus. (Hum the first three chords of Beethoven's Fifth Symphony, or of Three Blind Mice—and see whether your neighbours can stop themselves from thinking of the fourth.)

Hartley was unusual in thinking about brain mechanisms at all. His contemporary David Hume, an immeasurably greater philosopher, theorized about mental associations but not about any detailed goings-on in the brain (2.x.a). His supposed Newtonian "attractions" were even more mysterious than Hartley's hypothetical "vibrations".

A hundred years later, Hartley's hints were echoed by the father-and-son philosophers James and John Stuart Mill (2.x.a). They, too, supported their mentalistic focus on the association of ideas with speculations about the brain. They argued that learning could be due to an increased likelihood of brain activity at certain points, as a result of simultaneous activity earlier. But *just what* this activity was, and *just how* its probability could be modified, remained mysterious.

In the late nineteenth century, some then unorthodox biologists ventured that the mechanism might involve links between separate units, or brain cells—as opposed to a continuous network, or reticulum (2.viii.c). And in 1890, thanks to William James's widely read *Principles of Psychology*, that general idea became familiar in the new discipline of psychology.

Yet familiarity didn't bring clarity, still less proof. For nearly 200 years, Hartley's hunch remained no more than intriguing.

By the 1940s, however, it had begun to look highly plausible. The neurone theory had been vindicated. "Animal spirits" and "vibrations" had given way to action potentials. And various synaptic properties—facilitation, inhibition, thresholds, and refractory periods—had recently been established (see Chapter 2.viii).

These ideas led McCulloch and Pitts to describe psychology as the study of precisely defined neural nets (4.iii.e). Lashley and Donald Hebb proposed that psychologists should also think about the activity of less precisely defined *populations* of neurones (5.iv). And in 1949, Hebb put the central idea into a more tractable form, suggesting that a "connectionist" (his word) learning rule for modifying synapses is the neural mechanism underlying memory and conceptual thought (5.iv.b–e).

By that time, too, ideas about computation, and about "self-organization" (as cybernetic control) in animals *and* machines, had arisen (Chapter 4). Since it was now conceivable that some artefacts might work in a way somehow similar to the brain, connectionism was often expressed in computational terms—drawing sometimes on logic, sometimes on statistics.

Some of these early systems managed to show how a whole group of (artificial) units could store a 'single' representation—and access it, as Hartley had guessed, when given only a partial cue. And the 1950s saw the development of models that could learn concepts as well as associating them. Typically, these used some version of Hebb's 'ft/wt' (fire together, wire together) rule.

Connectionism was no longer just a twinkle in Hartley's eye. The baby was now well and truly born.

b. Turing and connectionism

The prototype of the first modern computer, MADM, wasn't completed until five years after the 'Logical Calculus' paper. And the von Neumann computer, whose design rested in part on McCulloch and Pitts' 1943 theory, became available later still, in the early 1950s (Chapter 3.v.e). Only then could functioning models of the mind—whether symbolic or connectionist—be based on their ideas.

Before that time, various people had predicted that the advent of digital computers would make exploratory modelling possible. They were mostly engineers, or psychologists with a background in physics or engineering.

Belmont Farley and Wesley Clark, engineers at MIT's Lincoln Laboratory, had even sketched potential simulations of trial and error learning (Farley and Clark 1954). Describing "self-organizing networks" based on Turing machines, they distinguished "conditioning" and "serial learning" in terms of short-performance and long-performance units, respectively. But these were merely suggestions: in the early 1950s, functioning models were still thin on the ground.

One of the very first was due to Alan Turing. He'd already implemented a learning network by 1947, on an embryonic version of MADM. He reported observing "the sequence of externally visible actions for some thousands of moments" (Turing 1947*b*: 20). Most of his connectionist experiments, however, had been pencil-and-paper simulations.

It may seem strange to refer to Turing and connectionism in the same breath. For he's usually thought of as the guru of symbolic AI, thanks to the Turing machine (4.i) and the Turing Test (16.ii.c)—not to mention his early efforts in programming (3.v.b). It's widely assumed (by non-specialists) that he said nothing about connectionism. But that's not so. His oft-repeated aim "to build a brain", which dated at least from 1944 (Hodges 1983: 290), *isn't* relevant here, for it didn't necessarily imply a brainlike (micro-)anatomy. But in a speculative report written in September 1947, he discussed a range of computational networks consisting of standard binary units, each one linked to (zero or more) others (Turing 1947*b*).

Such networks could be specifically designed to perform a pre-set task, as McCulloch and Pitts had remarked. Alternatively, they could be "unorganized". In the latter case, their links were randomly assigned. This, said Turing, is "about the simplest model of a nervous system with an arrangement of neurons [such as the cortex] whose function is largely indeterminate" (1947*b*: 10, 16).

For learning to occur in an initially unorganized network sounded, on first hearing, like magic. But he suggested that some training procedure, analogous to the education of a child, might be imposed which would gradually alter the linkages. A random network could be converted into one that behaves in systematically adaptive ways, he said, if the positive and negative influences on the links ("pleasure and pain") were set appropriately. His observations of "thousands of moments" of computer activity were focused on a pioneering model of this type.

Turing's remarks here were, as ever, intriguing. But they had no influence on the field. The paper was an internal report for NPL (the UK's National Physical Laboratory), and remained unpublished until 1969.

Certainly, he sometimes mentioned these ideas to compatriots and foreign visitors. For instance, at a seminar held by the Manchester philosophers in 1949, at which luminaries from various disciplines were present (see 16.ii.a), he speculated about "a machine using neuron-models" (Manchester Philosophy Seminar 1949). But just how this might be achieved wasn't spelled out. Even when lecturing to the London Mathematical Society in the year of the NPL report, he spoke only about ACE (3.v.b), without mentioning the possibility of more brainlike machines (Turing 1947*a*).

He himself failed to develop these ideas further because, by the late 1940s, his own interests were turning from learning to embryology. He was still concerned with the gradual organization of an initially unorganized substrate. But he was now asking how it might happen *without* the "external interference" of a teacher (A. M. Turing 1952: see 15.iv.a). In other words, he'd turned to self-organization.

c. 'How We Know Universals'

While Turing was still writing his NPL report, a very different suggestion about brainlike machines was being published on the other side of the Atlantic. This was contained in an essay on ‘How We Know Universals: The Perception of Auditory and Visual Forms’ (Pitts and McCulloch 1947), and it did make connectionism’s fuse glow brighter.

The new paper by Pitts and McCulloch was rooted in cybernetics. One of its sources was a statistical theory of heart fibrillation, in terms of the spread of impulses within a network of excitable elements. This had been developed in the mid-1940s by Norbert Wiener and Arturo Rosenblueth, and Pitts had recently worked on it.

Pitts and McCulloch had now switched from logic to statistics, from single units to collectivities, and from purity to noise. Although they didn’t use the modern vocabulary, it’s clear that their 1947 paper was describing the brain in terms of what’s now called distributed, cooperative, computing (Chapter 14.ii.b and iii.a).

Whereas their 1943 networks hadn’t been intended as a biologically plausible theory, their 1947 networks were. For they now proposed neurological hypotheses to match specific neural nets (see 14.ii.b). One of the three reasons (mentioned above) for the former indifference to their work had therefore vanished—and indeed, their second paper got a warmer welcome than the first (Bishop 1946). Another obstacle would soon vanish too, when computer technology became available.

The two authors didn’t disclaim their earlier, logic-based, work. On the contrary, they saw the new approach as an “extension” of it, and as “a systematic development of the conception of reverberating neuronal chains”. They still insisted that “any theoretically conceivable net” based on neuronal properties demonstrates that the computation concerned could be physically effected. But they were now ready to be more realistic about what those (anatomical and physiological) properties are.

They allowed that one can’t assume that the brain is made up of such neat and tidy connections as they had previously discussed. Nor can one assume that actual neurones have such constant, reliable, thresholds. Plausible neural nets therefore have to be error-tolerant:

> It is wise to construct . . . these nets so that their principal function is little perturbed by small perturbations in excitation, threshold, or details of connection within the same neighborhood. Genes can only predetermine statistical order, and original chaos must reign over nets that learn, for learning builds new order according to a law of use. (Pitts and McCulloch 1947: 46)

Moreover, perception itself is error-tolerant, in the sense that it involves the recognition of categories—what philosophers call “universals”—whose particular instances may differ in many details. Each cat is somehow unlike every other, but they’re all recognized as cats. A theory of perception should therefore explain how a physical mechanism such as the brain (or, by implication, a computer) could do this.

What they sought, then, was “general methods for designing nervous nets which recognize figures in such a way as to produce the same output for every input belonging to the figure” (p. 47). And “output”, here, covered both *classification* (*recognition*) and *action* (*movement*). For instance, we’ll see below that Pitts and McCulloch asked how the eye muscles can be instructed to lock one’s gaze on the centre of attention.

Instead of strings of precisely positioned logic gates, they now considered parallel-processing, statistical, devices. These were first defined as abstract mathematical systems, and then related to possible implementations in the brain.

“Parallel”, here, meant logical independence rather than precise temporal simultaneity. Time (all but ignored in the 1943 paper) was assumed to be an important factor.

But the real-time details were deliberately ignored. For instance, their system might compute a moving average over the preceding five synaptic delays, but these were unrealistically assumed to be equal and constant (p. 47). As for "statistical", this meant not probability theory but some sort of averaging, or assimilation to a norm.

In general, they argued, a universal is recognized by identifying an invariant under some group of mathematical transformations. They defined two types of mechanism capable, in principle, of doing this.

In the first, an input organ (such as the retina or cochlea) made up of many individual receptor units is "scanned" to find the average of all the independent measurements, or mini-decisions. This could enable the system to recognize (for example) shapes of any size, or chords of any pitch. Their mathematical analysis highlighted a general principle they called *the exchangeability of time and space*. This said that the delay in scanning any stimulus dimension corresponds to the number of distinct places on that dimension. In other words, the fewer discriminable points, the less time needed for discrimination—and vice versa.

In the second, a group of distinct perceptual inputs is "reduced" to a standard form. The "uniform principle of design" here is to select some *one* of the many appearances (transformations) of the category as the norm, and then to define parameters enabling one to locate all the others with reference to it. If any of the transformational equivalents is perceived, then the category will be recognized as such. All triangles, for instance, are topologically equivalent and have straight sides. (Compare John Locke's puzzlement over how a triangle can be thought of as "neither oblique nor rectangle, neither equilateral, equicrural, nor scalenon; but all and none of these at once"—see 2.x.a.)

The example they gave was the gaze reflex, in which the eye centres on some stimulus in the visual field. The norm, here, was defined as the "center of gravity of the distribution of brightness". They showed how a neural mechanism could compute this centre of gravity, move the eyes towards it, and then—through negative feedback—keep them there. The mechanism they suggested, based partly on work done on cats by the neuro-anatomist Julia Apter (1946), involved two point-to-point mappings: one from retinal cells to the superior colliculus, and one from the colliculus to the eye muscles.

This paper aimed for a high standard of theoretical rigour. Its approach was "basically simple and completely general, because any object, or universal, is an invariant under some groups of transformations" (W. S. McCulloch 1961*b*: 10). (Identifying all the relevant transformations was, of course, the $64,000 question.) It even included proofs that a certain net was *the simplest one possible* for effecting a certain computation. But it tried not to substitute abstraction for practicality. Some conceivable systems (designs for brains or computers) were defined only to be rejected, because of the combinatorial explosion.

However, it dealt with neither learning nor randomness. Its topic was how we know (recognize, represent) universals, not how we learn them. The "small perturbations" in thresholds and connectivities weren't supposed to result from the processing history of the system, but were taken as given. (They corresponded to the spontaneous neural firings discovered in the 1930s, and caused by chemical changes at the synapse: see 2.viii.f.) Moreover, they had to be small if the mathematics was to fit: massive changes or randomness would significantly alter the computed average and/or distance from the norm.

Randomness was widely recognized by cyberneticians as an important challenge, given the behaviourist anti-nativism prevailing at the time (see 5.i.a and 7.vi). If there was little or no prior structure to the newborn's cerebral cortex, how did interaction with the environment manage to produce it? Or if there *was* significant prior structure at birth, could this be ascribed to universal mechanisms of self-organization working in the womb, rather than to individual heredity? (see Chapter 14.viii.a). The encephalographer William Ross Ashby, for example, was working on self-organization in random systems, and would soon unveil his adaptive Homeostat (4.vii.c–d).

Likewise, McCulloch and (especially) Pitts were already trying to generalize their ideas to random nets. But they'd achieved suggestive analogies rather than definitive theorems.

So in a lecture given in the previous year (but not published until 1952), McCulloch had summarized their approach in terms of each neurone's being surrounded by "a nest of surfaces such that the chance of connection with our neuron is the same for all neurons on one and the same surface" (W. S. McCulloch 1946: 270). If only the connections are important, the nest will be spherical. If the directions and/or positions also matter, it will be egg-shaped or somehow lopsided.

In the same lecture, he'd compared learning to the physics of magnetizing a bar of steel. The myriad tiny magnets, initially positioned at random, end up (because of many local interactions) all pointing in the same direction—and, having reached equilibrium, they stay there. After spelling out the analogy between the various stages of magnetization and the formation of synapses, he said:

> It is not too much to hope that with these things in common the mathematics for one may be shaped to fit the other. If it will serve, then we may someday state how random nets may learn by taking on this or that local structure . . . *This may be done in some few years.* (W. S. McCulloch 1946: 273; italics added)

Those years turned out to be many, not few. Pitts had done some work on the mathematics of the three-dimensional "nests" mentioned by McCulloch. But he didn't persist, and never published his results. McCulloch (1961*b*: 110) later said this was "because we could not make the necessary measurements". But part of the problem was the dreadful mental decline that struck Pitts in 1952 (see Chapters 4.iii.d and 14.v.b). He burnt his manuscript on 3D nets, and no trace was ever found—despite major efforts by his friend Jerome Lettvin in searching for it.

Pitts described his new ideas in public only once, at the second Macy conference on cybernetics (see 4.v.b). That lecture wasn't published, either. However, as McCulloch remarked later, "[It] was enough to start John von Neumann on a new tack" (1961*b*: 110).

d. From logic to thermodynamics

This new tack—on which McCulloch, too, was working—was a search for a probabilistic logic (von Neumann 1956). That's one in which the functions, not just the arguments, are more or less probable.

Von Neumann did sketch a probabilistic network of formal neurones. But he wasn't satisfied with it, because the units were far more reliable, and far simpler, than real neurones. So instead of focusing on that network, he generalized his research to cover a diversity of "complicated automata"—now called cellular automata (15.v).

The result, in effect, was invisibility. These highly abstract ideas reached few mathematicians or computer scientists, and even fewer neurophysiologists. Von Neumann did publish a lecture he gave at the Hixon Symposium in 1948 (von Neumann 1951). But otherwise, his work—including five lectures given at the University of Illinois in 1949—remained unavailable until the mid-1960s, well after his death (15.v.b). Again, light was being hidden under bushels.

If von Neumann's work on cellular automata didn't influence the nascent connectionist community, his Silliman Lectures did (von Neumann 1958). These were intended for delivery at Yale, but were still unfinished at his death. They'd grown, at McCulloch's instigation, out of a talk von Neumann gave to the American Psychiatric Association in 1955.

There, he'd stressed the computational complexity of individual neurones as well as of collectivities, and argued that mere redundancy (large numbers of units) couldn't account for the brain's power and reliability. In the Silliman Lectures, too, he argued that brains ("natural automata") are so different from digital computers that computational neuroscience can't be grounded in logic.

The differences he mentioned included:

* the statistical (imprecise, yet reliable) character of the nervous message;
* its implementation as periodic pulse-trains;
* its anatomical restriction (often) to only a few synaptic steps;
* the continuous (analogue) properties of the synapse;
* the rich connections of individual neurones;
* the huge numbers of neurones;
* their functioning largely in parallel;
* and the possibility that neural thresholds and/or connections may change over time.

All these, he said, implied a need for a very different approach:

> [Whatever] language the central nervous system is using, it is characterized by less logical and arithmetical depth than what we are normally used to . . . [The] language here involved may well correspond to a short code [i.e. a basic machine language], rather than to a complete code [i.e. an assembly code or programming language]; when we talk mathematics, we may be discussing a *secondary* language, built on the *primary* language truly used by the central nervous system . . . [Whatever] the system is, it cannot fail to differ considerably from what we consciously and explicitly consider as mathematics. (von Neumann 1958: 81–2)

As for what the new approach should be, however, this unfinished text didn't say.

To be sure, there'd been a hint already. For in the 1948 lecture, von Neumann had suggested that "thermodynamics, primarily in the form it was received from Boltzmann" might be a good analogy for cerebral processing. (Compare McCulloch's earlier remarks on "magnetization", above.) But this idea couldn't yet be explored by computer modelling. The special-purpose differential analysers he'd used since 1940 weren't suitable, and the infant von Neumann machine wasn't sufficiently powerful.

It wasn't even obvious that a more powerful version could fit the bill, until McCulloch proved (in 1959) that a digital computer could implement a probabilistic logic (W. S. McCulloch 1961*b*: 16). But this was a point of principle, not practice. Only very much later would "thermodynamic" connectionism be feasible (see Sections v.e–f and vi.b, below). Meanwhile, von Neumann's fellow Hungarian Andras Pellionisz was inspired by his book to develop a "geometrical" form of connectionism (see 14.viii.b). Tucked away in Budapest, however, his ideas weren't taken up.

Von Neumann's scepticism about logic-based approaches had some influence, nevertheless—not least, because it was seen as a recantation by the master, the man whose eponymous machine had made GOFAI possible. Right from the start, a close friend recalled later, "one of [von Neumann's] motives for pressing for the development of electronic computers was his fascination with the working of the nervous system and the organization of the brain itself" (Ulam 1989: 19). Indeed, he'd altered his own computer design to incorporate the McCulloch–Pitts neurone (see Chapter 4.iv). The fact that he now went to such pains to stress the differences between brains and computers attracted attention accordingly.

Many readers of his posthumous book inferred that GOFAI (still NewFAI at the time) was doomed to failure. They assumed that *brainlike* computer models were needed instead. But just what might that mean?

12.ii. Infant Implementations

Several early cyberneticians had already tried to build brainlike models, some of which were theoretically interesting (see Chapter 4.vi–vii). But they'd had to rely on their own engineering skills, working with the proverbial biscuit tins and string to build special-purpose connectionist *hardware*.

This might involve a monstrous apparatus, like the analogue associative-memory machine constructed at University College London in the early 1950s by Wilfred Taylor (1956, 1959). This machine could recognize patterns, such as alphabetic letters, written in varying tones of black and white, and presented in varying orientations. In effect, it learnt its own feature detectors (see 14.iii). And it worked fast: it could find the pattern edges (high-intensity gradients) in less than a microsecond.

The apparatus had a nine-cell artificial retina. This involved lateral inhibition between neighbouring input units, and a "maximum amplitude filter" that passed on—to the sixteen-cell associative layer—*only* the strongest signal within a given area, provided that it was larger than the others by some minimal amount. (Later, Taylor applied these ideas to the brain, suggesting that specific cortical neurones and connections act as maximum-amplitude filters: W. K. Taylor 1964.) The light intensities were measured discontinuously, as ten distinct levels. Patterns were coded in terms of the most useful sensory input features, such as vertical, horizontal, and oblique lines. These features weren't built in, but were identified (from around 500 possibilities) by means of lateral inhibition.

By the mid-1950s, however, general-purpose computers were becoming available in the leading research centres. These were huge monsters too, of course. But each one could be used for many *different* purposes. This meant that (small) connectionist models could now be implemented more conveniently, by being simulated in digital computers—as Turing (1947*b*) had foreseen.

Not everyone switched from analogue to digital. Taylor, for instance, continued to develop his hardware model through the early 1960s, because the data couldn't be fed into it quickly enough in simulated form. But many others did take advantage of the new technology. The scene was now set for an explosion of work in both symbolic *and* connectionist AI.

a. B24 bricolage

While MADM and EDSAC (see 3.v.b) were still being put through their pioneering paces, Marvin Minsky, then an undergraduate at MIT, was puzzling about trial-and-error learning. He was largely inspired by McCulloch's ideas and by Hebb's then recent book, both of which he'd learnt about from his teacher George Miller.

On graduating in 1951, and before starting work on his Ph.D., he turned his puzzling into practice. With a $2,000 grant from the US Navy (officially, for Miller), he built a machine that simulated four rats in a maze learning to avoid each other:

> In the summer of 1951 Dean Edmonds [a physicist] and I went up to Harvard and built our machine. It had three hundred tubes and a lot of motors. It needed some automatic electric clutches, which we machined ourselves. The memory of the machine was stored in the positions of its control knobs, 40 of them, and when the machine was learning, it used the clutches to adjust its own knobs. We used a surplus gyropilot from a B24 bomber to move the clutches. (J. Bernstein 1981*a*: 69)

Each of the forty "neurons" had a probability of firing, implemented by the machine's potentiometer. The reward/punishment clutch was fired by a gas tube. And when the reward/punishment button was pushed, the shaft would turn clockwise or anti-clockwise, thus increasing/decreasing the probability of passing a signal next time.

(Where is this swords-to-ploughshares machine now? Well, after the 1956 meeting McCarthy's mentor at Dartmouth, John Kemeny, said that he wanted to have it there. Minsky and McCarthy got it up to Hanover, in full working order. However, it was never used for anything else. "Maybe it's still there, in some basement": J. McCarthy, personal communication.)

The contraption's success—up to a point, it worked—had much the same effect on Minsky that NewFAI was to have on many people a few years later:

> *We sort of quit science for a while to watch the machine.* We were amazed that it could have several activities going on at once in this little nervous system. Because of the random wiring it had a sort of fail safe characteristic. If one of the neurons wasn't working, it wouldn't make much difference and with nearly three hundred tubes, and the thousands of connections we had soldered there would usually be something wrong somewhere . . . I don't think we ever debugged our machine completely, but that didn't matter. By having this crazy random design it was almost sure to work no matter how you built it. (J. Bernstein 1981*a*: 69; italics added)

However, he soon abandoned this happy-go-lucky approach. Not only was he unable to interest his friend Burrhus Skinner in the project (he'd been trying to implement Skinner's ideas—Crevier 1993: 35), but his mathematical instincts overpowered his engineering ones.

The switch away from gizmo-gazing was triggered by his meeting someone else who'd worked on learning. Specifically, someone studying the induction of Miller's artificial grammars (Chapter 9.v.d and x.b):

> [In around 1955] I met a young man named Ray Solomonoff who was working on an abstract theory of deductive inference . . . He had worked on a learning machine . . . that was pretty formal. I was so impressed I decided this was much more productive than the neural net system, in which you built a piece of hardware and hoped it would do the right thing. With [this new] approach, *you tried to make theories of what kind of inferences you wanted to make, and then asked "How would I make a machine do exactly that?"* (Minsky, interview in Crevier 1993: 37; italics added)

In other words, bricolage was no longer enough. Nor were intriguing but ill-understood 'successes'.

Nor, significantly, was it enough to try "to understand how the brain works" as opposed to "understanding what it does" (interview in McCorduck 1979: 84). Ray Solomonoff, in short, had made him realize that McCulloch-and-Pitts wasn't sufficient. He also needed *a better theory of the task*.

A few years later, in his seminal 'Steps' paper (Chapter 10.i.g), Minsky would apply this insight right across the AI board. But in this early case, it was being applied to learning. To be sure, his Ph.D. of 1954, part-supervised by von Neumann, described similarly tinkered machines—constructed from "SNARCs": Stochastic Neural-Analog Reinforcement Calculators. But it discussed some of their limitations, as well as their potential (cf. also Minsky 1956*a*). For example, he speculated about how one network could be enabled to control another (a prime focus of research today: Section ix.a, below).

Paul Werbos's diagnosis, years later, was that Minsky—like many others "in the learning-business"—simply "didn't understand numerical analysis [or] the concept of numerical efficiency" (J. A. Anderson and Rosenfeld 1998: 344). However that may be, by the mid-1960s Minsky favoured careful mathematical analysis of the computational power of AI models in general. He dropped connectionist research for a while, disappointed by his own pioneering machine. Indeed, his abstract analysis of early connectionism's limitations would lead to a major scandal in the field (see Section iii).

It wasn't only Minsky who'd matured by the 1960s. Throughout the previous decade, some other connectionists had progressed from isolated tinkering towards systematic analytical research. (Some, but not all: "Why think when you can simulate?" was still a common attitude—J. A. Anderson and Rosenfeld 1998: 245.)

These influential early modellers included the cyberneticians and psychophysiologists mentioned in Chapters 4.v–vi and 14.i–ii. Others, discussed below, were Uttley, Raymond Beurle, Oliver Selfridge, Frank Rosenblatt, and Bernard Widrow.

b. Self-organizing networks

One swallow doesn't make a summer, and half a dozen neurones don't make a network. It was accepted in the 1950s that McCulloch–Pitts neurones could implement logic gates, and many strings and simple loops for performing and/or learning specific tasks were modelled accordingly. But other questions concerned the properties of *whole groups* of neurones, as involved in the reverberating circuits posited by neurophysiologists—and by Hebb (1949).

Turing had recently described embryogenesis in terms of interacting waves (Chapter 15.iv.a), and others now used similar ideas to explain *neuropsychological* self-organization. One such approach was pioneered in 1954 by Beurle at Imperial College, London, and developed by Clark and Farley at MIT (Beurle 1956, 1959; Clark and Farley 1955). They studied the dynamical properties of two-dimensional arrays of randomly connected cells, or neurones. Those properties were conceptualized as waves of activation: building, spreading, persisting, dying—and sometimes meeting and interacting.

The randomness was important not just as a mathematical challenge but as an attempt to match the biological facts. The increasingly common comparisons between computers and organisms, said Beurle, were "interesting when made in relation to abstract fundamental concepts, but less productive when details are considered" (Beurle 1956: 83). For whereas the components of computers are "connected to some exact specification", many parts of the cortex show a "very large random factor in the interconnexions between neurons". In short:

> The aim of this paper [is] to show that some of the basic forms of behaviour of living organisms can be simulated by a mass of simple units *without the necessity of postulating a large degree of specific organization* of these units. (Beurle 1956: 83; italics added)

Among those "basic forms of behaviour", said Beurle, were chains of conditioned responses and memory in general (1956: 81–3).

Beurle made constant references to "servo-control", but his 1956 paper was a theoretical one. In the implementations of his wave-interaction theories, B24 bombers were left far behind. In some of Farley and Clark's later work, a model network was built out of 1,296 analogue units, with as many as thirty connections each (Farley and Clark 1961).

By that time, even larger networks were being *simulated*, not built with soldering irons, by Richard Laing at Michigan—up to 10,000 neurones, with up to 200 synapses per neurone (Laing 1961*a*). But Beurle and Clark-and-Farley were earlier. Moreover, MIT—Clark and Farley's home base—had much more visibility in the AI community than Michigan did (see Chapter 15.v.b). Light under bushels, yet again.

That's not to say, however, that the Imperial and MIT hardware implementations were so primitive as to be a waste of time. Far from it. This work remained influential long after it was done, as we'll see.

The Beurle-inspired networks were guided, to some extent, by mid-century neurophysiology (2.viii.d–e). For instance, although the connections were random, the probability of two units' being connected decreased with their distance from each other. This was based on (admittedly shaky) evidence about the distribution of cell bodies and axons in the cat's visual cortex.

One physiologically unrealistic assumption made by Beurle was that there was a fixed synaptic delay, and a fixed refractory period—after which a cell would suddenly recover its excitability. These timings were important, because the behaviour of the whole network would depend on the proportion of currently excitable cells. Some of his followers, such as Farley and Clark, tried to keep closer to the biology. That is, they allowed for exponential recovery, and for variations in the thresholds and connectivities involved. Their waves could be visually displayed, and even 'frozen' for inspection (Farley and Clark 1961).

Beurle claimed that if several cells at the centre were simultaneously stimulated, the activation would spread out over a proportion of the net. If the stimulus was weak, the wave would eventually extinguish. But a strong stimulus would generate a wave that spreads out over the whole sheet: "[The wave] will increase in amplitude until it 'saturates,' when it uses all the cells in the medium through which it passes, and the amplitude cannot increase further" (Beurle 1956: 63). By contrast, stimulation of a single cell wouldn't spread.

In certain circumstances, a wave of excitation could leave a process of self-excitation behind it:

> Fully saturated waves may follow each other at intervals equal to the period taken by the cells to recover sensitivity completely, and unsaturated waves may follow even closer. This makes it possible for a wave to pass through a region, and return again to the same region some time later when the majority of the cells has recovered. This would allow a relatively local circulation of activity which might be of some importance. (Beurle 1956: 65)

If two waves of excitation were generated independently, specific types of interference pattern would result when they met.

In some of Beurle's computational experiments, neighbouring units mutually inhibited one another. In others, a unit's sensitivity was slightly increased with each activation, so that the collectivity learned to respond differently as time passed. These results were used to ground hypotheses about the difference, and the interaction, between short-term and long-term memory.

Beurle's hypotheses were not merely qualitative, but quantitative too: he offered a mathematical theory describing the origin of wave-interference patterns. It turned out later that some of his mathematics was mistaken. But many of his ideas remained interesting, especially his demonstration that a *part* of a wave could regenerate the rest.

The neurophysiologist Jack Cowan (1933–) came across Beurle when he was at Imperial College in 1956. He now says that "a lot of the basic ideas about the dynamical properties of networks of neurons are sitting in Raymond Beurle's work", and "I found what Beurle had done was really interesting mathematically," despite the mistakes (J. A. Anderson and Rosenfeld 1998: 105).

In the early 1960s, this part-to-whole result would be cited in developing holographic theories of associative memory (see Section v.c). And when Cowan took over Nicholas Rashevsky's then flagging group at that time, he started by trying to develop Beurle's work—hiring the young chemist Hugh Wilson to help him (alongside Stuart Kauffman, "right out of medical school": see Chapter 15.viii.b).

In the early 1970s, Cowan and Wilson formulated an influential theory of neural dynamics that included Beurle's system as a special case. Later still, Cowan realized that they'd created the neural analogue of Turing's (1952) work on morphogenesis, which was a *universal* model of pattern development (J. A. Anderson and Rosenfeld 1998: 111, 118). Small wonder, then, that he regards Beurle's research as mathematically interesting.

c. Connections with the Ratio Club

Uttley (christened Albert, but nearly always called Pete) had outlined an associative memory even before Beurle did. Indeed, Beurle explicitly acknowledged Uttley's help, in the 1956 paper. Uttley grounded his pioneering models in current ideas about the brain—and in McCulloch and Pitts' neuronal logic (Sholl and Uttley 1953). And he tried very much harder than Beurle to match the biological details (see Chapter 14.ii.b).

Uttley was one of the seventeen founder members of the Ratio Club (see Chapter 4.viii). Indeed, it was he who'd chosen the name—because its etymology covered reasoning, relations, and numbers. Among his fellow founders were Ashby, Horace Barlow, Patrick Merton, William Grey Walter, and Donald MacKay. Other members included the visual psychologist William Rushton, the neurophysiologist John Pringle, and the AI pioneer Jack Good (Husbands forthcoming). (At least two of these men are widely regarded as being unlucky not to have won a Nobel Prize: namely, Barlow and Rushton.)

The Ratio members had got used to cross-disciplinary research during the Second World War, but Uttley was even more interdisciplinary than most. Originally a mathematician, he was involved in some early developments of computer technology. During the war, and afterwards at the Royal Radar Establishment, he developed analogue devices for target-tracking. He designed an early digital computer called TREAC (short for Telecommunications Research Establishment Automatic Computer) in the 1950s. This was the UK's first parallel processor. He also designed a dynamic computer memory, using feedback implemented by mercury delay lines—an idea anticipated by Turing (see 3.v.c). His prime interest (and graduate training), however, was in psychology. He left RRE for NPL in the mid-1950s, where he founded the "Autonomics" division to work on learning in brains *and* machines.

Combining mathematical expertise with a healthy respect for experimental data (on both brain and behaviour), his early ideas attracted various people's attention. Computer modellers were interested of course, but so were psychologists and, especially, neurophysiologists. His Ratio colleagues Barlow and Merton, for instance, were strongly influenced by him: the former for his application of information theory to neurones; the latter for his work on the automatic aiming of gun turrets in aircraft, which informed Merton's servo-theory of muscle control (H. B. Barlow, personal communication).

Uttley's reputation was international, too. He was hailed as the founder of "a revolutionary mechanical logic" by a French cybernetician (de Latil 1953: 280). And one of his papers was included by Claude Shannon and John McCarthy in their seminal collection on *Automata Studies* (1956).

In addition, Uttley co-organized the select London (NPL) seminar at which Selfridge and Rosenblatt first presented Pandemonium and perceptrons, respectively (see below), and Barlow first announced his coding theory of perception (14.ii.a). This NPL meeting, like the Dartmouth Project just before it, was an important event in the history of cognitive science (see 6.iv.b). Its participants included many people mentioned in this book.

For all the respect in which he was held, Uttley's ideas weren't always explained clearly. Barlow (personal communication) recalls that "he was not a great communicator", and remembers him "trying, and failing, to get across the idea of what he called 'unitary coding' at the Ratio Club". (Barlow now thinks this may have been a form of "sparse" coding: see 14.x.e.) Nevertheless, Uttley undoubtedly helped foster the nascent interest in cognitive science.

In his early days at NPL, Uttley developed a range of "conditional probability machines" (Uttley 1956, 1959*a*,*b*). These were intended to simulate various neural mechanisms underlying the conditioned reflex (see Chapter 2.viii.b) and innate releasing mechanisms (see Chapter 5.ii.c). One design, implemented by hydraulics as well as electronics, enabled a computer to count coincidences, and then to estimate the probabilities that two distinct inputs would occur together. He saw such probability estimations as the essential ground of all learning (14.ii.b).

Other models were focused on positive and negative conditioning, neural inhibition, memory decay, and time of onset. Uttley showed that if the temporal order of two inputs of equal strength were recorded, twelve different patterns could result—and could be detected automatically. In general, he explained how a single neural circuit used for inductive learning could distinguish several classes and subclasses, depending on the connectivities and thresholds involved.

These mid-century models are still recognized as important early studies of "reward-modified learning" (Minsky and Papert 1988: 282), and of the role of redundancy and probability estimations (Barlow 2001*b*, acknowledgements). Uttley's mature work, by contrast, is rarely cited today (see Section iv.a, below).

Quite apart from his specific models, his general approach—and his hugely exciting 1956 meeting—inspired a number of people in the 1950s to take computer models of intelligence seriously. As Richard Gregory (personal communication) has put it: "The idea that mind is due to physical mechanisms was still quite alien to almost everyone. Uttley saw very clearly that this was the way to go, so he shone out as a pioneer."

d. Pandemonium

One of Uttley's near-contemporaries on the other side of the Atlantic was Selfridge. (Yes, it was his family—his grandfather—who had founded the famous Oxford Street store.)

In today's publication-obsessed research evaluations, he would hardly figure. (His eight first-author entries in this book's bibliography span forty-seven years.) So much the worse for bibliometrics as a measure of intellectual value. For Selfridge was a hugely important figure in the history of cognitive science. His ideas, in conversation as much as publication (and in the circulation of unpublished drafts), would influence not only connectionism but several other areas besides (see Chapters 6.ii.c and iii.b, 10.i.b, 13.iii.d, and 14.ii.a).

As a young man at MIT, Selfridge was at the heart of the cybernetic community. He was close to both McCulloch and Wiener (who acknowledged his help in the second edition of *Cybernetics*). And his near friends included his contemporaries Jerome Lettvin and Pitts, who were his room-mates for a while. By 1947, he was revising Wiener's mathematical theory of heart flutter (with Pitts working alongside, on fibrillation), and already knew of the ideas on stimulus generalization in Pitts and McCulloch's universals paper.

Ten years later, he was publishing on pattern recognition and learning (Selfridge 1955, 1956)—and enthusing Allen Newell and Herbert Simon to embark on simulating thought (see 6.iii.b and 10.i.b). In addition, he was acting as the patron for Minsky's SNARC machine.

But above all, he was implementing his own "paradigm for learning": a program called Pandemonium, named after John Milton's vision of a demon-packed hell (Selfridge 1959). As remarked in Chapter 10.i.b, this could well have been included in the list of AI harbingers there. Its influence was incalculable.

McCulloch described the Pandemonium paper (written in July 1958) as "the outstanding American contribution" to Uttley's seminal NPL symposium on 'The Mechanization of Thought Processes' (W. S. McCulloch 1961*b*: 224). But that's not to say that he was surprised by it.

The fundamental idea—that perception involves a hierarchy of specialized feature-detectors, or "demons"—had been much discussed between himself, Wiener, and Selfridge, and he'd already applied it in designing a tonal 'reading machine' for the blind. It was familiar also to Minsky, Lettvin, and Ulrich Neisser (all acknowledged in an endnote). Indeed, Selfridge had outlined the core ideas several years earlier, at a meeting on learning attended also by Clark, Farley, Newell, McCulloch, Miller, and Selfridge's Lincoln colleague Gerald Dinneen (Selfridge 1955, 1956; cf. Dinneen 1955).

Pandemonium was part-program, part-programmatic. Selfridge had implemented a simple version on an IBM computer, and would describe its progress in the pages of *Scientific American* a few years later (Selfridge and Neisser 1960). The main excitement, however, lay in his vision of its *potential*. This vision included speculative glimpses of mindlike software "agents", capable of cooperation and communication not only with each other but with a human user too (13.iii.d–e).

The program was a NewFAI achievement. (That's why it could have been listed as a NewFAI harbinger.) However, it was conceptualized as a multilevel parallel-processing network (see Figure 12.1). In spirit, then, it was a connectionist system. It used a *localist* representation, in which one unit corresponded to one feature, or concept. From the bottom up, Selfridge distinguished four levels: "data" demons (cf. a cochlea or retina), "computational" demons, "cognitive" demons, and one or more "decision" demons. In effect, the higher-level demons were grandmother cells (14.x.e), each one looking out for a particular feature, or set of features, in the level below.

If a demon found what it was looking for, it "shrieked" (as Selfridge put it) to the demon/demons on the level above. When the input demons responded to specific sensory data, such as sound or light, the computational demons measured their activity—and some computed *compound* features (such as equality, greater than, less than, maximum, and average). The cognitive demons looked out for more complex patterns, such as the letters A or B. Their decisions were continuously graded: the more confident the demon was, the louder it shrieked. Since the image was simultaneously on view to all the cognitive demons, they all shrieked in parallel—hence, pandemonium. The top-level demon identified which one of these was the loudest, and its output represented the network's final decision about what the input pattern was.

Pandemonium could have been programmed to discriminate clearly defined patterns, or classes. But the aim was for it to *learn* to recognize new ones, and to do so even if they *could not* be precisely defined beforehand. Selfridge assumed—as Jerome Bruner

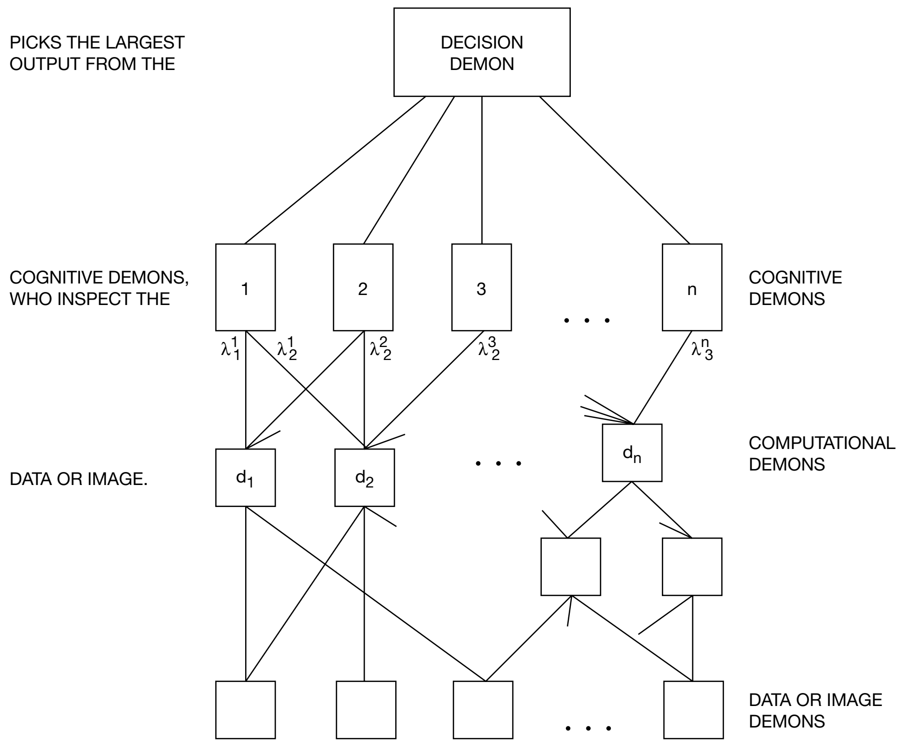


FIG. 12.1. Schematic Pandemonium. Redrawn with permission from Selfridge (1959: 514)

was doing, too (6.ii.b)—that people's classifications (concepts) approximate logical functions of the features concerned. He designed the data demons and computational demons accordingly, and Pandemonium's task was to learn (via the cognitive demons) just which features are involved in this or that class.

It started out with a set of cognitive demons already defined in plausible terms:

For the [cognitive level], we collect a large number of possibly useful functions, eliminating a priori only those which could not conceivably be relevant, and make a *reasonable* selection of the others, being bound by economy and space. We then guess *reasonable* weights for them. *The behavior at this point may even be acceptably good*, but usually it must be improved by means of [several kinds of adaptive changes]. (Selfridge 1959: 516; italics added)

In other words, each cognitive demon computed a weighted sum of the outputs of all the computational demons, but the weights for individual features differed. In recognizing an A or an H, for instance, slanted and vertical lines were weighted differently.

The system was shown examples of various patterns, labelled as such, and had to guess the label for a newly presented example. It was then told whether its guess was right or wrong.

It learnt by hill climbing, a technique introduced in Arthur Samuel's checkers-player (10.i.e). So it continually computed the "score" of its overall performance. On each trial, the weights of some relatively highly weighted feature were slightly altered, in random directions, and the new score was compared with the previous one. This process was repeated until the overall score was maximized—or rather, until every such change reduced it. Selfridge realized that the system might get trapped on a local maximum, or "false peak", but could suggest no way of avoiding this. He did point out, however, that different learning rules would suit statistically different problem landscapes.

The network's method for learning was similar to what Selfridge saw as the basic principle of adaptive movement (see Section iv.b). But there was a crucial difference, which made him distinguish "supervised" and "unsupervised" learning. In these two types of learning the success is monitored, respectively, by the human trainer or by the system itself.

This was an important distinction, and (with the caveat mentioned below) a lasting one.

Today, the three major computational paradigms for learning are *supervised, unsupervised*, and *reinforcement* learning. (And there's some evidence that different brain structures and neurochemicals, such as dopamine, may be involved in the different types: e.g. Schultz *et al.* 1997; Kitazawa *et al.* 1998; Doya 2000.) Each type may result in sequences of input–output pairs (compare chains of conditioned reflexes: 5.iii.a). But this is achieved in different ways:

* In the clearest cases of supervised learning, the human programmer "trains" the system by defining a set of desired outcomes for a range of inputs, and providing continual feedback to the system about whether it has achieved them. When it fails to do so, it has made an "error"—and error messages of various sorts are crucial in the learning rules concerned. In other cases, the target is specified by the system itself, based on sensory signals and higher-level goals (Wolpert *et al.* 2001: 489). Examples include the "emulator systems" discussed in Chapter 14.vii.c, in which the brain predicts the sensory consequences of current motor activity and adjusts the relevant motor control if those predictions aren't borne out.
* In unsupervised learning, there are no desired outcomes or error messages—nor any reinforcers either (i.e. punishments or rewards). Rather, learning depends merely—as Hebb had suggested—on simultaneous activations of the pre-synaptic and post-synaptic neurones. Put crudely, cells that fire together, wire together (ft/wt). As we saw in Chapter 5.iv.b, Hebb himself gave two versions of the ft/wt rule; since then, there have been many further variations. If this seems to favour sequences built up by mere happenstance, perhaps it does: "The main problem with purely unsupervised learning is that *there is no guarantee that the representations learned will be useful for decision making and control*" (Wolpert *et al.* 2001: 490; italics added).
* In reinforcement learning, no desired outputs are specified beforehand. However, each actual output receives either reward or punishment from the environment. This may be the biological environment, or a human "teacher". Sometimes, whole sequences of individual outputs are strongly reinforced at their endpoint, in which case a problem of "credit assignment" arises: which of the individual outputs were most responsible for the happy ending? (Credit assignment is a common problem for non-connectionist AI too: see Chapter 15.vi.a.)

Returning to Selfridge, the promised caveat is that, according to the modern terminology, he sometimes conflated both supervised and unsupervised learning with reinforcement learning. He pointed out that animals, unlike Pandemonium, possess intrinsic measures of success, or innate reinforcers. (Even John Watson had allowed that a young baby will be frightened by loud noises: 5.i.a.) Moreover, he said, reinforcement tells the learner whether it has done well or badly, without providing detailed information on *just which* aspects of its decision were correct/mistaken (i.e. without what was later termed credit assignment).

Since Pandemonium couldn't judge whether one decision was "better" than another, it needed to be told. That is, it was restricted to supervised learning. Unsupervised learning might be possible, said Selfridge, but only if it rode on the back of supervised learning. For instance, if Pandemonium were to discover that the classes were very clearly divided, it might then reject classifications based on more ambiguous evidence.

Selfridge made three intriguing suggestions about how the functions, as opposed to the weights, of the computational and cognitive demons might be adaptively altered. This would enable the network to define its own operators.

(Pandemonium couldn't do this, but a daughter program did so a few years later: Uhr and Vossler 1961*a*/1963. That system initially learnt to recognize alphanumeric characters, and arbitrary doodles, by spotting—defining—their characteristic features for itself. But it was soon used also to learn to recognize spectrograms of spoken digits. These are complicated visual diagrams of acoustic output; non-experts can't distinguish them, but the program learnt to do so, even when the word was spoken by different people: Uhr and Vossler 1961*b*.)

His first suggestion, here, was that a relatively useless demon might be removed, or disconnected. Second, a new demon might be produced by "conjugating" two sub-demons, so that it coded the simultaneous presence of *two* specific features, or *one of them only*, or *one or both*, or *neither*, etc. (He was being influenced here not only by McCulloch and Pitts' 1943 paper, but also by Bruner's work on concept learning: 6.ii.b.) And third, a single demon might be randomly "mutated", and the best survivor retained.

(Simultaneous mutations were specifically excluded, because Selfridge didn't know how to attribute any ensuing improvement to *this* new demon rather than *that* one. In other words, the credit assignment problem again. We'll see in Chapter 15.vi.a that this nut was eventually cracked by John Holland—whose early modelling had located hidden contradictions within Hebb's verbal theory: Rochester *et al.* 1956.)

The implemented Pandemonium program was the first step in the construction of a machine to convert hand-keyed Morse code into a typewritten message. Its task was to learn to discriminate dots and dashes. This is more tricky than one might think: although dashes are in theory exactly three times as long as dots, in practice their length varies. Accordingly, there were only two cognitive demons: one for *dot*, one for *dash*.

But an automated Morse typewriter would need to do more than that: it would have to group the dots and dashes in appropriate ways. So Selfridge envisaged future programs having extra levels of cognitive demons, for computing "symbols" (e.g. *dot–dot–dot*), letters (e.g. *S*), and words (e.g. *SOS*). Beyond that, syntactic and semantic interpretation would require yet more levels.

The implication was that networks with many hierarchical levels exist in adult human brains, and might one day be modelled in computers. Perhaps all these levels could be learnt/constructed from scratch (by iterated conjugation, for instance), or perhaps they're largely built in—by evolution or by the programmer. Selfridge didn't address that question in this paper. But he soon would, as we'll see.

e. The perceptron

Exciting—and influential—though it was, Pandemonium was overshadowed by the near-simultaneous *perceptron*. Perceptrons were described in a leading psychological journal in 1958, shortly before Selfridge gave his NPL talk. (They were also described at the NPL meeting, so reached a widely interdisciplinary group: Rosenblatt 1959.)

The perceptron paper was a distillation of several years' work at Cornell, where the psychologist Rosenblatt had borrowed the Aeronautical Laboratory's IBM-704 computer to do his modelling. This arrangement had started as an inconvenience, necessary because his own department didn't have a suitable machine along the corridor. But it eventually helped spread his ideas. For while psychologists read about them in the *Psychological Review*, physicists—thanks to his borrowed colleagues—encountered them in *Reviews of Modern Physics* (H. D. Block 1962; Block *et al.* 1962).

It was Rosenblatt, even more than Selfridge, who fanned the smouldering fuse of connectionism into a shining flame. A perceptron's task is the same as Pandemonium's: to learn to discriminate input patterns. Indeed, Rosenblatt's diagram of a simple perceptron was highly similar to Selfridge's of Pandemonium (see Figures 12.1 and 12.2). But Rosenblatt's work was both more ambitious and more systematic than Selfridge's.

It was more ambitious, in that Rosenblatt assimilated learning to fundamental self-organization. Self-organization is the emergence of order from a relatively unordered state (see Chapter 15), so *all* learning exemplifies it, in a sense. But this terminology is especially appropriate when the starting point is highly unordered, and Rosenblatt focused on initially random systems. That is, he didn't start out—as Selfridge had done—with networks provided with "reasonable", or "appropriate", features and weights.

Selfridge soon argued that this focus was *too* ambitious. Random networks, he said, are useful only for small local tasks, such as correlating or classifying inputs (Minsky and Selfridge 1961). As we'll see in Section iii, his co-author here would eventually publish a mighty roar of disapproval at Rosenblatt's random-network approach. But that was for the future. Meanwhile, this new work was very well received.

It was more systematic than Pandemonium, in that Rosenblatt compared many different types of perceptron, involving a variety of learning rules. Some of these

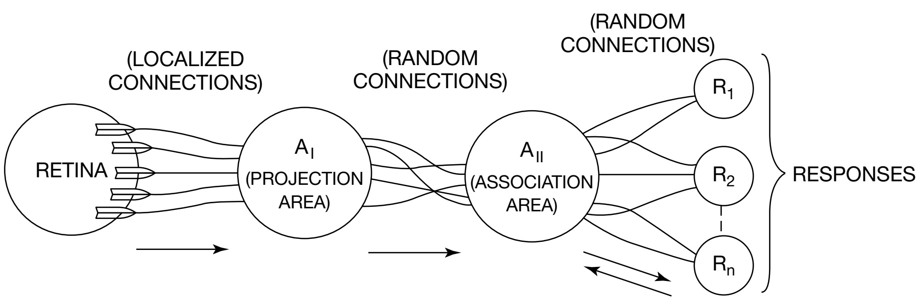


Fig. 12.2. Organization of a perceptron. Adapted with permission from Rosenblatt (1958: 389)

were defined in his paper for *Psychological Review*, others in his eagerly awaited book, previously available only as a lab report (Rosenblatt 1962). So although, as he admitted, "the mechanism for pattern generalisation proposed by Clark and Farley is essentially identical to that found in simple perceptrons" (1962: 24), he took their approach further.

Rosenblatt's ideas were expressed—and compared—in mathematical terms. However, the behaviour of the more complex perceptrons was described qualitatively, not numerically. In many cases, he backed up his theoretical discussion by precise measurements on experimental simulations.

His book included a speculative chapter on "back-coupled" networks. These contained at least one level of what are now called hidden units, plus "back-connections" whereby error messages could be passed backwards from level to level. Such error messages, he said, would enable "layers of units which are relatively remote from the sensory end of the perceptron [to] modify the activity of layers which are relatively close to the sensory end" (1962: 471).

He didn't give an algorithm by means of which this could be done. Nine years later, in 1971, Werbos did so—but no one realized this at the time. The first *recognized* algorithm for back propagation had to await the mid-1980s (see Section vi.c–d, below). Back in the early 1960s, however, Rosenblatt had explored how various ways of doing it—by magic, so to speak—would modulate the overall behaviour of the network.

For example, he argued that a particular back-coupled network illustrated "the simplest conditions under which 'selective attention' might be said to occur in a perceptron" (Rosenblatt 1962: 479). He outlined an experiment in which this back-connected perceptron would learn to distinguish not only triangles and squares and/or the top and bottom halves of the field, but triangle-in-the-top and square-in-the-bottom. It would be able to pick out (pay attention to) one of these stimuli at a time:

> [The] system should give a consistent description of one of the two stimuli, in terms of shape and location, and ignore the other stimulus; it will not name the shape of one and the position of the other, even though both shapes and both positions are simultaneously present. (p. 478)

Like Uttley, Rosenblatt was aiming for biological realism—hence the title of his book: *Principles of Neurodynamics: Perceptrons and the Theory of Brain Mechanisms*. But he did this in much more general terms than Uttley. A perceptron, he said, is:

> a hypothetical nervous system, or machine . . . designed to illustrate some of the fundamental properties of intelligent systems in general, without becoming too deeply enmeshed in the special, and frequently unknown, conditions which hold for particular biological organisms. (Rosenblatt 1958: 387)

So Pitts and McCulloch's 1947 attempt to model detailed neuro-anatomy (see 14.ii.b) wasn't an exemplar for Rosenblatt.

Nor was their 1943 paper. Quite the contrary. Rashevsky's followers in general, he complained, had produced "a profusion of brain models which amount simply to logical contrivances for performing particular algorithms . . . in response to sequences of stimuli" (1958: 387). Admittedly, they'd been more concerned with possibility proofs than with brains as such. But they assumed that it would require "only a refinement or modification of existing [logical] principles to understand the working of a more realistic nervous system" (p. 388). (This criticism was unfair. Certainly, the fashion at the time was logicist; but Rashevsky's biophysics group had long suggested using differential equations to describe neural networks: see Chapter 4.iii.c.)

Rosenblatt took a fundamentally different approach: no "idealized wiring diagrams" for him. Indeed, he was closer to the cybernetic tradition. Declaring Ashby (4.vii.c–d) and von Neumann as his intellectual ancestors, he aimed to show how an initially random network could organize itself.

(As for how it could identify the *relevant* stimuli in the first place, he argued that perceptual similarities aren't objective features, like McCulloch and Pitts' geometrical properties. Rather, they depend on the nature and history of the perceptual system itself: cf. Chapters 15.vii.c and 16.vii.c. In a sense then, the fundamental input sensitivities aren't random at all.)

The theoretical language needed, Rosenblatt said, wasn't Boolean logic but—as the cyberneticians had suggested—probability theory. Some network-modellers had already recognized this, as we've seen. However, "it is frequently hard to assess whether or not the systems that they describe could actually work in a realistic nervous system, and what the necessary and sufficient conditions might be" (p. 388). This two-pronged criticism was a clue to his own approach. He used both experimentation and mathematical analysis to study a wide range of machines.

In the simplest cases, a perceptron is a parallel-processing system comprising an input layer of sensory units (a "retina" of S-points), an output layer of response cells (R-units), and a single layer of associator neurones (A-units) between them. Once a classification has been learned, some individual R-unit fires if and only if the relevant pattern is input. In the ultra-minimalist version, there is only one R-unit, hence only one possible classification.

Each A-unit receives excitatory connections from several S-units, and each R-unit from several A-units. Between S-units and A-units, there are only feed-forward connections. But (usually) each R-cell has inhibitory feedback links to all the A-cells that *don't* excite it. This helps make the responses mutually exclusive: if this R-cell fires, then that one doesn't.

The units are all-or-none McCulloch–Pitts neurones: if the (fixed) threshold is reached, the cell fires. However, the activity of an A-unit has some graded (and alterable) "value", which makes it more or less effective in its influence on the relevant R-cell. The information in the system is thus stored, or represented, as a set of connections of varying strength.

Learning takes place by some form of negative feedback, which Rosenblatt termed "reinforcement". He usually ignored positive reinforcement: learning in simple perceptrons follows error, not success. (This fitted some theories of learning, such as Jean Piaget's, but was controversial nonetheless: see Chapter 7.vi.h.)

The paper described more complex perceptrons, too. And the complexities involved were many and various. I'll list some of them here (without discussing them), so as to convey a sense of the visionary scope of Rosenblatt's thinking at the time—and so as to allow specialist readers to compare his visions with today's achievements. For in effect, he was outlining a long-term research programme for connectionism:

* A network, he said, might contain a larger number of cells,
* and/or several associative layers.
* There might be lateral (inhibitory) connections between A-units.
* Or there could be R-units coding "discriminating features" instead of mutually exclusive responses (so seven R-units could represent 100 patterns).
* Another possibility was to have mini-networks of A-units, specialized to detect discriminating features.
* Further complications included changeable thresholds in the A-units,
* and continuously graded activity in the S-units, proportional to stimulus intensity.

That wasn't all:

* Complex perceptrons might employ a (probably more realistic) form of feedback, in which each R-unit excites its own A-units.
* There might be positive feedback (increase in connection strengths) as well as negative feedback.
* And the training might "force" some specific initial connections, the rest of the system organizing itself around them.
* Also, there might be a "projection" layer placed before the first associative layer.
* There might be systematic connections between S-units and the projection layer, to reflect "focal points" on the retina.
* And the system could be sensitive to temporal, as well as spatial, order.

The crucial point, irrespective of the complexity of the network, was that learning involves some change in the A-units or in their connections. For instance, an A-unit might output a weaker impulse; or there might be a change in the probability that its output will actually reach the relevant cell in the next layer.

Rosenblatt pointed out that there are various ways in which successful connections might be encouraged, and several different criteria for judging success. Usually, he relied on error correction. A connection would be strengthened if the R-unit failed to respond when it should have done, and weakened if it fired when it shouldn't have done. But various types of learning rule could achieve this:

> In the alpha system, an active cell [A-unit] simply gains an increment of value for every impulse, and holds this gain indefinitely. In the beta system, each source-set [all the cells that feed forward to a particular R-unit] is allowed a certain constant rate of gain, the increments being apportioned among the cells of the source-set in proportion to their activity. In the gamma system, active cells gain at the expense of the inactive cells of their source-set, so that the total value of a source-set is always constant. (1958: 392)

Different systems could be specified by substituting different numbers for the variables concerned.

When Rosenblatt did this, and measured the learning curves observed in his computer implementations, he found systematic differences across learning rules. What's more, these differences could be predicted from six quantities: the number of excitatory/inhibitory connections per A-unit, the threshold of an A-unit, the proportion of R-units to which an A-unit is connected, and the number of A-units/R-units in the system.

Connectionism, it seemed, had come a very long way from playing around with bits of abandoned B24 bombers. The B24 bricoleur himself would soon declare this apparent progress an illusion (see iii, below). But meanwhile, in describing his achievements and predicting their future, Rosenblatt didn't hold back. He even designed a secret weapon (not kept secret for long): a mathematical proof that perceptrons had astonishing potential.

f. Excitement, and overexcitement

Rosenblatt made bold claims for perceptrons. "*Each of [the six] parameters*", he pointed out, "*is a clearly defined physical variable, which is measurable in its own right*, independently of the behavioral and perceptual phenomena which we are trying to predict" (1958: 406). It followed, he said, that his system far surpassed behaviourist learning theories—including Clark Hull's, which he saw as the best of a bad bunch (Chapter 5.iii.b).

His theory, or so he claimed, was superior in respect of parsimony, verifiability, generality, and explanatory power. It enabled precise quantitative predictions of learning curves from neurological data, and vice versa. It could be used both to analyse the behaviour of biological organisms, and to guide computer modelling ("the *synthesis* of behaving systems, to meet special requirements"). Hebb had hinted at the organic substrate underlying behaviour, and at a plausible bridge between biophysics and psychology (5.iv.b–d). But, Rosenblatt declared, "[my] theory represents the first actual completion of such a bridge" (p. 407).

This was exhilarating stuff: Edward Tolman's schematic sowbug had been well and truly outdone (see 5.iii.c). To cap it all, Rosenblatt soon (in 1959–60) proved the "perceptron convergence theorem".

The convergence theorem applied only to simple perceptrons, with only a single layer of units between the input and output layers. It stated that a particular learning rule is *guaranteed* to find the correct set of connection values for any pattern that is in principle learnable by a perceptron. That is, a perceptron using this rule can learn to do anything that it's possible to program it to do.

The relevant rule was a form of error correction, in which the A-units' values are slightly raised or lowered whenever their R-unit fires incorrectly. It's true that, eventually, it would always converge on a solution. But that wasn't entirely clear at the time. For Rosenblatt's proof was very demanding, and so too was the simpler version produced by a physicist colleague (H. D. Block 1962, sect. 9b). Ironically, the theorem would later be proved more elegantly by Rosenblatt's fiercest critics (see Section iii.b).

No less exciting than the mathematics and the experiments was the long list of things that perceptrons—considered as a general class—could apparently do. Their capabilities, Rosenblatt reported, had already been *seen* to cover the following:

* both spontaneous and trained pattern recognition;
* tolerance of changes in size or rotation;
* recognition of examples never yet encountered;

* responsiveness to patterns in any sensory modality (or combination thereof), and in both space and time;
* sensitivity to visual contours;
* selective attention and recall;
* distributed memory (stored not, as in the localist Pandemonium, in a single high-level unit but in the collective activity of many units);
* damage tolerance;
* associative learning;
* trial-and-error learning;
* and learning of ordered sequences of response.

Was there anything perceptrons *couldn't* do? Yes, said Rosenblatt:

> The question may well be raised at this point of where the perceptron's capabilities actually stop . . . [Is it] capable, without further modification in principle, of such higher order functions as are involved in human speech, communication, and thinking? Actually, the limit of the perceptron's capabilities seems to lie in the area of relative judgment, and the abstraction of relationships. In its "symbolic behavior", the perceptron shows some striking similarities to [certain] brain-damaged patients . . . As soon as the response calls for a relationship among stimuli (such as "Name the object left of the square") . . . the problem generally becomes excessively difficult for the perceptron . . . Some system, more advanced in principle than the perceptron, seems to be required at this point. (1958: 404–5)

In his book a few years later, and especially in the final chapter, Rosenblatt admitted further limitations. For instance, a single-layer perceptron can't deal with multiple simultaneous inputs, such as a circle next to a square (although a multi-layer version could: see above). And no perceptron of the types he had considered was capable of using a "temporary memory", which is needed for many tasks (1962: 577).

Despite these caveats, however, many people were deeply impressed by Rosenblatt's claims. (Perhaps some who read the paper didn't read the book?) His ideas were soon widely applied (e.g. David and Selfridge 1962).

The flame he had lit ignited the journalists, too. Rosenblatt received "wall-to-wall media coverage" (J. A. Anderson and Rosenfeld 1998: 304). The media interest was caused partly by the 'brainlike' nature of perceptrons, and partly by his special-purpose hardware, the "Mark 1" perceptron (H. D. Block 1962, sect. 8).

This machine—which learnt, for instance, to recognize letters from the alphabet—used photoelectric cells for the retina, and (like Minsky's earlier gizmo) motor-driven potentiometers for varying the weights. It had a 20×20 input array, 512 associator units, and eight binary response units, with each S-unit having up to forty connections to the A-units. The eight R-units communicated and competed with each other, eventually agreeing on a (winner-take-all) single response. Rosenblatt found, as Minsky had done before him, that his hardware needn't be perfect. Change a few connections, knock out some units, or use somewhat unreliable components, and the Mark I might still give an acceptable result.

He himself was ambivalent about the publicity. Obviously, it had its advantages. But it usually involved a fundamental misunderstanding of what he was up to.

For the journalists, he was building a machine. It might be only the first in a long line of improved machines, but the object of the exercise was to engineer a gizmo. For

Rosenblatt, of course, that wasn't the point at all. In explaining why he'd coined the term "neurodynamics" (and used it in his book title), he said:

> The term "perceptron", originally intended as a generic name for a variety of theoretical nerve nets, has an unfortunate tendency to suggest a specific piece of hardware, and it is only with difficulty that its well-meaning popularizers can be persuaded to suppress their natural urge to capitalize the initial "P". On being asked, "How is Perceptron performing today?" I am often tempted to respond, "Very well, thank you, and how are Neutron and Electron behaving?" (Rosenblatt 1962, p. v)

He regretted the scornful scepticism aroused among other scientists by the media attention. The Office of Naval Research, who had funded his work (cf. 11.a), probably regarded it less as an exercise in neurodynamics than as a prelude to militarily useful pattern-recognizers. But scientists in general, reading the press reports, might think his aims were far more grandiose than either:

> [In] the first public announcement of the program in 1958 by the popular press, [they] fell to the task with all the exuberance and sense of discretion of a pack of happy bloodhounds. Such headlines as "Frankenstein Monster Designed by Navy Robot That Thinks" (Tulsa, Oklahoma *Times*) were hardly designed to inspire scientific confidence. (Rosenblatt 1962, p. v)

He was wise to be wary. The brouhaha helped trigger a hugely damaging attack from two critics at MIT (Section iii, below). In effect, Rosenblatt's research programme was killed off by it—or anyway, put into lengthy hibernation.

That wouldn't be the last time when "scientific confidence" in an entire area of research was undermined by overenthusiastic reports composed by, or even *for*, the press. Given the public's (and other scientists') tendency to see computer modelling as hubris, this is an occupational hazard of AI in general. A telling example of what can happen if one is too ready to boast to the press (and to everyone else) was described in Chapter 11.iv.

g. Enter the Adaline

Rosenblatt wasn't alone at that time in being fêted by the newspapers for building an intriguing adaptive machine. Widrow's Adaline (ADAptive LINear Element) and Madaline (a group of Many Adalines) also caused much media excitement.

One widely publicized example of Widrow's pattern-learning hardware, which he called "Knobby Adaline", was an electrical circuit with sixteen input switches and a manual threshold control. Another worked by electrolysis. The connections were made of copper-plated pencil leads, whose resistance varied with the thickness of copper gradually laid down on the graphite core.

You may be reminded of Gordon Pask's electrochemical "concept machine" mentioned in Chapter 4.v.e—which, like Pandemonium, had recently been described at Uttley's 1958 conference. This is no accident. The cybernetic movement had experimented with many adaptive machines based on physical (as opposed to programmed) functions, even including chemistry. And Widrow, as an electrical engineer trained at MIT in the years around 1950, was firmly rooted in the cybernetic tradition.

His special interest was digital filtering, wherein a signal is strengthened by compensating for the noise. Originally a topic for engineers, signal detection theory had been applied to psychophysics by David Green and John Swets at the famous 1956 symposium at MIT (see Chapter 6.iv.b). Widrow, however, was especially enthused by his experience of the 1956 summer school at Dartmouth.

Widrow questioned Wiener's (then orthodox) theory that one has to know the statistics of the signal in order to design the best filter. Widrow wanted an adaptive filter, which would optimize itself *no matter what* the relation of signal to noise.

Wiener had used "mean square error" to measure a filter's success. (*Mean*, because it's the average error that matters; *square*, because squaring a number always gives a positive number, so the direction of error can be ignored.) Widrow had to find a way of minimizing this error measurement automatically. This would require some form of supervised learning, wherein the machine could gauge the difference between actual and desired outputs. In other words, it would require something broadly similar to a perceptron. (This isn't a remark about Widrow's thought processes, for he didn't know about Rosenblatt's work until it hit the newspapers.)

Widrow's first solution resembled the perceptron convergence procedure, for it involved repeatedly making small changes in the filter's parameters and measuring the resulting error each time. Those changes which were found to give the greatest average improvement were then followed until improvement ceased. However, this method of gradient search by "steepest descent" required many successive measurements and calculations. In 1959 (having just moved from MIT to Stanford), Widrow and Marcian (Ted) Hoff together discovered a much more efficient way of finding the error gradient: the "LMS", or least mean square, algorithm (Widrow and Hoff 1960) (see Section v.a, below).

Within only half an hour, they had implemented the LMS algorithm on the department's analogue computer. The next day, they cobbled together an adaptive digital circuit with a 4×4 array of binary input switches—which they used as a demonstration model to amaze their colleagues, not to mention the press. "You'd be surprised how many different geometric patterns you can make with a little four by four array of switches," Widrow said later (J. A. Anderson and Rosenfeld 1998: 54).

That was the first hardware Adaline. The pencil-lead version—as it happens, the beginning of liquid-state electronics—was devised and commercially marketed soon afterwards. It was much more efficient than its predecessor, and also than the Mark I perceptron. Indeed, Widrow regarded the latter as "a disaster", because of its unwieldiness, slowness, and unreliability (J. A. Anderson and Rosenfeld 1998: 59).

One obvious commercial application of Adalines was in adaptive telephone modems, to filter out the noise in the telephone line. But *noise* is a generic concept, so these "adaptive neurons" were potentially useful for many different purposes, including some of interest to cognitive science. Clearly, they might be valuable in technological AI. Whether biological organisms can compensate for noise by using some equivalent of LMS was less clear, not least because LMS involved supervised learning. But the question seemed worth pursuing.

Where adaptive networks were concerned, then, at the turn of the 1960s all systems were *Go!* The interest aroused was phenomenal, and "probably sucked away 10 or 15 years of all the smartest people in cybernetics" (Minsky 1984*b*: 122).

Admittedly, as Widrow recalled many years later, the publicity hype that he and Rosenblatt received "infuriated" many of their colleagues (J. A. Anderson and Rosenfeld

1998: 65). But this annoyance could be dismissed, at least by enthusiasts and outsiders, as being driven by professional jealousy rather than reason. By the end of the 1960s, over 100 research groups in AI and psychology were focusing on perceptrons and similar systems.

Yet only ten years after that, a science journalist wrote: "[The perceptron] had fundamental limitations and has, in recent years, pretty much been abandoned" (J. Bernstein 1981*b*: 100).

"*Abandoned? Surely not! (Whatever happened to the shining flame?) And if so, why?*"—The "Surely not!" will be addressed in Section iv, where we'll see that the flame had been damped, but by no means extinguished. First, let's discuss the "Why?".

12.iii. Attack Without Apology

The journalist just quoted had interviewed Minsky at length. It was he who reported Minsky's story about being beguiled by his B24 bricolage before returning to "science". This probably explains his dismissive remark about connectionist research.

For in so far as perceptrons were "abandoned"—which they weren't, not entirely (see Sections iv–v)—it was largely Minsky's doing. If an outsider got the impression that connectionism was finished, talking to Minsky could have been responsible. This presumably explains also why the science journalist Pamela McCorduck, who knew Minsky personally but wasn't an expert in the field, wrote in her late 1970s history of AI that connectionism had "died" (McCorduck 1979: 47).

The extent of Minsky's responsibility for the Sleeping Beauty's going into hibernation is arguable, as we'll see. But that he had something to do with it is certain. He was one of the devilish duo—for so the connectionist community viewed them—who published a radical attack on Rosenblatt's methodology (Minsky and Papert 1969).

a. The devilish duo

That Minsky jeopardized network research may seem ironic. For he'd built one of the very first connectionist learning machines (see Section ii.a). In fact, Minsky's approach to the two main types of AI swung like a pendulum.

His initial work on learning was connectionist in spirit, as we've seen. But his early paper on neural networks was logicist, not probabilistic. Then, partly due to the influence of John McCarthy, he turned to GOFAI (property-list) methods, making them the focus of research in the AI Lab they co-founded at MIT (Chapter 10.ii.a).

Soon afterwards, he embarked on his first principled critiques of connectionism. These were early exercises in meta-epistemology (10.i.g). For example, he and Selfridge pointed out some intrinsic limitations of the work on perceptrons outlined above (Minsky and Selfridge 1961). And in his seminal paper 'Steps Towards Artificial Intelligence' (Minsky 1961*b*), he explicitly favoured symbolic AI above adaptive networks for modelling learning (10.i.g). In the late 1960s, he even described perceptrons as "sterile" (Minsky and Papert 1969: 232).

By the 1980s, however, his interest had swung back towards parallelism. His "K-lines" account of memory explained it as the partial reactivation (by single units) of *sets* of simple units, comparable to cell assemblies, working in parallel (Minsky 1980; 1985, ch. 8).

But he hadn't abandoned GOFAI (see subsection d, below). On the contrary, his intellectual pendulum had come to rest in an attempted integration of symbolic processes with parallel architecture (Minsky 1985). This hybrid position had been anticipated, in outline, a quarter-century before, in the 'Steps' paper.

Minsky was a hugely influential figure in early AI, as we saw in Chapter 10. He was respected by the wider computing community too. Not only was he the first AI person to receive the ACM's high-profile Turing Award, but his three predecessors were the pioneering 'pure' computer scientists Alan Perlis, Maurice Wilkes (3.v.b), and Richard Hammond. So when he attacked perceptrons, people sat up and listened. It's no wonder that he was so deeply resented by the connectionists: for them, it was a public relations disaster.

Minsky's partner in the devilish duo was Seymour Papert, a South African who'd left home in the 1950s to study mathematics at the University of Cambridge. They were brought together by McCulloch in autumn 1963, at a party in his house. (I was there—sitting on the carpet with Papert!) Papert had come straight from the airport, on his arrival from Geneva where he'd been working with Piaget for five years.

As a Piagetian, Papert saw learning not as the gradual self-organization of an initially random system, but as a process structured by a succession of internal schemata (see Chapters 5.ii.c and 10.v.f). So although he was already thinking about thinking in computational terms (he'd been commuting to London to use the NPL computer, no machine being available to him in Switzerland—Crevier 1993: 86), he was doing so in a way that was at odds with the connectionist approach. As for Minsky, he was already directing the NewFAI research dominant at MIT.

Besides these shared grounds for scepticism about perceptrons, the two men had already ventured into the relevant mathematical theory:

> We had both been interested in the perceptron since its announcement by Rosenblatt in 1957. In fact we had both presented papers related to its "learning" aspect at a symposium on information theory in London in 1960. (Minsky and Papert 1969: 239)

Eighteen months later, with McCulloch's encouragement, they were together at MIT—where they started a "serious" collaboration.

b. The opening salvo

Their joint critique was highly abstract. They didn't build and test actual computer models, still less "quit science for a while to watch the machine". They focused instead on the underlying principles, to determine the scope and limits of the methodology's computational power.

This wasn't a new departure: Minsky's 'Steps' had been written in that way too. In other words, they were doing what Drew McDermott and Alan Bundy had criticized NewFAI workers for not doing (11.iii)—and what Rosenblatt himself had been doing when he formulated the convergence theorem.

It's clear, however, that they were far from disinterested:

> Our first formal presentation of the principal results in [our] book was at an American Mathematical Society symposium on Mathematical aspects of Computer Science in April 1966 . . . We were pleased and encouraged by the enthusiastic reception by many colleagues at the A.M.S. meeting and no less so by the doleful reception of a similar presentation at a Bionics meeting. However, we were now involved in establishing at MIT an artificial intelligence laboratory largely devoted to real "seeing machines", and gave no attention to perceptrons until we were jolted by attending an IEEE Workshop on Pattern Recognition in Puerto Rico early in 1967.
>
> Appalled at the persistent influence of perceptrons (and similar ways of thinking) on *practical pattern recognition*, we determined to set out our work as a book. (1969: 242)

Even this passage gives little hint of the passion that had perfused the early drafts. According to Robert Hecht-Nielsen (1947–), these dripped with "vitriol" and amounted to "an unseemly personal attack" on Rosenblatt (J. A. Anderson and Rosenfeld 1998: 305).

Competition for money was certainly one factor. The DOD "was seriously thinking about funding Rosenblatt's work on the Perceptron" (Newquist 1994: 72). This alarmed Minsky and Papert, because every ARPA dollar devoted to Cornell would be one less for MIT. In the early 1960s that might not have mattered too much, given the depth of ARPA's coffers at that time. But "rumors swept through the [AI] research community around 1968 and 1969 that ARPA was going to cut back on its seemingly unlimited largesse" (Newquist 1994: 139). That may be why, having circulated the book in draft for some years, they now decided to publish it more widely.

There may well have been personal jealousies too. Minsky would understandably be annoyed by Rosenblatt's superman image in the media, especially the mistaken assumption that the Mark I was the first learning network ever built. Possibly, there was some long-standing rivalry: Rosenblatt and Minsky had been fellow pupils at high school in the Bronx. And it probably didn't help that, in 1957, Marshall Yovits of the Office of Naval Research—a friend of Minsky's, who had provided much of the early funding for AI—"had publicized Rosenblatt's work and made a big splash about it" (J. A. Anderson and Rosenfeld 1998: 99).

However that may be, and even though the later printings (after Rosenblatt's death) were dedicated to Rosenblatt's memory, it's pretty clear that "the whole thing was intended, from the outset, as a book-length damnation of Rosenblatt's work and . . . neural network research in general" (Hecht-Nielsen again: ibid.). Eventually, Minsky and Papert followed "the strong and wise advice of colleagues", and expunged almost all of the vitriol.

Those wise colleagues weren't trying to protect Rosenblatt. On the contrary, they wanted to ensure that the damning critique was all the more effective for being seen as objective. For Minsky and Papert weren't the only ones at MIT to doubt Rosenblatt. Cowan remembers attending a "terrible" lecture he gave there in 1958, at which the audience "went after him and really attacked him". "Almost everyone" at MIT, says Cowan, was very sceptical: "by and large, it was clear that the perceptron wasn't doing the things that Frank claimed it could do" (J. A. Anderson and Rosenfeld 1998: 99–100).

The scepticism was experienced with passion. Many of these critics were "infuriated" by Rosenblatt's media popularity partly because they feared that the irresponsible claims involved might cause a backlash against neural networks in general.

Their fears were justified. The VLSI engineer Carver Mead (1934–) would later attribute the "twenty-year famine" in neural networks to the "overhype" about perceptrons (J. A. Anderson and Rosenfeld 1998: 141). Even if Mead was exaggerating, there was much truth in his charge (see subsection e, below). And ARPA (soon to be renamed DARPA: see 5.iv.f), who had funded early 1960s neural-network research but withdrew their support because of the criticisms emanating from MIT, repeatedly warned against similar "hype" when they considered funding connectionism again (see Section vii.b, below).

In the final version, then, Minsky and Papert's critique had lost the invective that had peppered the drafts. It was expressed almost entirely in the ascetic terms of computational logic—or, as they put it, computational geometry.

So they defined various types of simple perceptron: all with a single associative layer, and all locally connected, with no feedback loops. And they compared their computational properties. For instance, they considered the effect of altering the number of S-units leading into an A-unit, and/or the size of the A-unit's receptive field. Mostly, they considered the behaviour of a single (output-layer) neurone, as opposed to the joint activity of several such neurones.

They saw their work as relevant not only to assessing the potential for practical applications, but also to theorizing about the brain. For if perceptrons can't do some of the things that brains do, then some neural processes must be different. Primarily, however, it was an exercise in the general theory of computation: a study of self-organizing machines that make decisions by weighing many independent items of evidence. As such, they suggested, it might throw light not only on the brain but even on "how the genetic program computes organisms" (Minsky and Papert 1969: 1).

The interest and optimism aroused by Rosenblatt's work had been due in part to his convergence theorem (see ii.f, above). Minsky and Papert agreed that it was "seductive" (1969: 14), even "amazing" (1988, p. xi). But they cautioned against taking it at face value.

A theorem, of course, is a theorem—and they even produced new (more elegant) proofs of their own (1969, ch. 11). They argued, however, that certain relevant questions had rarely, if ever, been addressed. These included:

* Is the perceptron an efficient form of memory?
* Does the learning time become too long to be practical even when [learning] is possible in principle?
* How do the convergence results compare to those obtained by more deliberate design methods?
* What is the perceptron's relation to other computational devices? (1969: 243–4)

The last of these questions became "more and more important" in their eyes. It prompted comparisons, for instance, with Ashby's Homeostat (Chapter 4.vii.c–d) and with GOFAI's techniques for problem solving and learning. Minsky and Papert detailed the threat of combinatorial explosion, arguing that scaling up simple perceptrons to deal with higher-order problems wouldn't be feasible.

As they pointed out, the existing successful perceptrons relied on operators specially designed for the task—such as Selfridge's low-level demons, or feature-detectors. This, they said, was no accident. In general, "significant learning at a significant rate presupposes some significant prior structure" (p. 16). This was a version of the lesson already learnt the hard way in GOFAI, that "general" problem-solvers are highly limited in power (see Chapter 10.iii.c and iv). In sum, perceptrons could be useful only for simple, pre-specified, problems.

One might think that the connectionists would have been grateful. After all, GOFAI was thriving despite the threat of the combinatorial explosion, and despite the fact that digital computers are only *approximations* to universal Turing machines. To warn against over-optimism about perceptrons isn't necessarily to dismiss them as a waste of time. Rosenblatt himself had suggested important limitations, as we've seen.

But Minsky and Papert went further. They concluded that this type of research could lead only to a dead end, since even idealized perceptrons aren't equivalent to Turing machines. In other words, the combinatorial explosion was only part of the difficulty. What, above all, put the sceptical cat among the connectionist pigeons was their proof that (simple) perceptrons cannot do certain things which, intuitively, one might have expected them to be able to do.

One intractable problem was the computation of parity. Although a "diameter-limited" perceptron (where each operator can inspect only a small part of the retina, or stimulus, at one time) can tell whether any particular part of an image contains a dot, it can't tell whether the number of dots overall is odd or even. Indeed, Minsky (with Selfridge) had already noted this point in 1960, and repeated it in 'Steps'. Similarly, McCulloch had already noted that a perceptron can't compute *exclusive-or* (so some of the disjunctive concepts studied by Bruner—see 6.ii.b—were out of reach).

Now, Minsky and Papert elaborated on these limitations, and identified more. In general, they showed that perceptrons can discriminate only between classes that are "linearly separable". (Suppose two classes are each represented by a set of points in some mathematical space: if a line can be drawn between the two sets, the classes are linearly separable.)

Most of the specific examples they discussed were clearly relevant to pattern recognition. For instance, they proved that although simple diameter-limited perceptrons can distinguish concave and convex lines, they can't compute spatial connectedness (pp. 12–13, 70–95). They can't say, for example, whether a scribble consists of one continuous line or several. (People sometimes fail too: the two doodles printed on the cover of their book were visually equivalent, but tracing-by-finger shows that one is connected and the other isn't.) Nor can such perceptrons recognize connectivities in line drawings of 3D solids, as the current GOFAI scene-analysis programs could (10.iv.b).

To many people, this was highly counter-intuitive. If perceptrons can cope with convexity, then surely they should be able to deal with connectedness too? Besides referring to existing scene-analysis programs (pp. 232–9), Minsky and Papert gave principled arguments to show that serial, not parallel, computation was required for recognizing connectedness (and parity). And they pointed out that, since all the biological receptor cells identified thus far were diameter-limited (see 14.iii.a–b), an animal needs "more than neurosynaptic 'summation' " to perceive connectedness.

Allen Newell (1969) hailed their critique as "a great book", not least because it furthered "the appropriate shaping of computer science into a disciplined field of enquiry". Many GOFAI scientists welcomed it for less laudable reasons, seeing it as a powerful weapon in a turf war for funding and prestige. But if people in symbolic AI welcomed it, connectionists understandably didn't.

Psychologists who had been excited by perceptrons saw the book as "a major disaster" (R. L. Gregory, personal communication). The network modellers themselves countered it by saying that their research had only just begun, and that Minsky and Papert had considered only very simple systems. Rosenblatt's co-worker Herbert Block (1970), for instance, argued that multi-layer perceptrons would be able to do everything they had said to be impossible.

The critical duo had anticipated this riposte. On their view, more complex, many-levelled, networks would offer no significant advance. They admitted that this was merely a hunch, meriting further research:

> The perceptron has shown itself worthy of study despite (and even because of!) its severe limitations. It has many features to attract attention: its linearity; its intriguing learning theorem; its clear paradigmatic simplicity as a kind of parallel computation. There is no reason to suppose that any of these virtues carry over to the many-layered version. Nevertheless, we consider it to be an important research problem to elucidate (or reject) our intuitive judgment that the extension is sterile. (Minsky and Papert 1969: 231–2)

They even allowed that "some powerful convergence theorem" might be discovered for describing learning in multi-layered machines. Clearly, however, they weren't holding their breath.

c. Intransigence

Twenty years later, it would seem—to many—that their judgement had been mistaken. The late 1980s saw an explosion of interest in connectionism, both from AI researchers and in the media. Besides countless intriguing applications, the results included parity recognition (Rumelhart *et al.* 1986*a*), a "powerful convergence theorem" for multilevel networks, and a multilevel learning rule of great generality (see Section vi.b–c). It was even proved that a three-layer network can in principle solve *any* problem (Hornik *et al.* 1989).

On top of all that, DARPA started funding neural networks again, explicitly admitting that their previous disdain had been "undeserved" (DARPA 1988: 23). In short, many people felt that Minsky and Papert had ended up with egg on their faces.

Not so!, they replied. For their judgement remained unchanged. Minsky made this clear in his contributions to the DARPA study, and also more publicly in a second edition of *Perceptrons*—the text unchanged, but with a new Prologue and Epilogue. He and Papert were unrepentant and unbowed:

> [We] were tempted to "bring [our] theories up to date". But . . . we found that little of significance had changed since 1969 . . . Our position remains what it was when we wrote the book. We believe this realm of work to be immensely important and rich, but we expect its growth to require a degree of critical analysis that its more romantic advocates have always been reluctant to pursue—perhaps because the spirit of connectionism seems itself to go somewhat against the grain of analytic rigour. (Minsky and Papert 1988, p. vii)

Their main complaint concerned that central topic of GOFAI research, the representation of knowledge (see 10.iii.a):

> What we discovered was that the traditional analysis of learning machines—and of perceptrons in particular—had looked in the wrong direction. Most theorists had tried to focus only on the mathematical structure of what was common to all learning, and the theories to which this had led were too general and too weak to explain which patterns perceptrons could recognize. As our analysis . . . shows, this actually had nothing to do with learning at all; it had to do with the relationships between the perceptron's architecture and the characters of the problems that were being presented to it. The trouble appeared when perceptrons had no way to represent the knowledge required for solving certain problems. The moral was that one simply cannot learn enough by studying learning by itself; one also has to understand the nature of what one wants to learn. No machine can learn to recognize X unless it possesses, at least potentially, some scheme for *representing* X. (Minsky and Papert 1988, p. xii)

That final sentence, of course, was a near-verbatim rendering of McCarthy's statement about learning in the early days of GOFAI (10.i.f), and a restatement of Minsky's concern with knowledge representation in 'Steps' (10.i.g).

They did make one concession. They answered the question "How much, then, can we expect from connectionist systems?" by saying "Much more than the above remarks might suggest, since reflective thought is the lesser part of what our minds do" (1988: 280). They even said: "We see no reason to choose sides . . . [And] we expect the future of network-based learning machines to be rich beyond imagining" (p. xiv).

But they were far from endorsing the purist type of self-organization envisioned by Rosenblatt and most, though not all, PDP workers (see Sections vi–viii, below). High-level intelligence, they said, does not and cannot arise from utter randomness, nor from a wholly non-sequential system:

> Most probably, the human brain is, in the main, composed of large numbers of relatively small distributed systems, arranged by embryology into a complex society that is controlled in part (but only in part) by serial, symbolic systems that are added later. (p. xiv)

d. The hybrid society of mind

Innocent readers might have been bemused by the passage just quoted, which was given no explanation. But it tacitly reflected a wide range of work done by cognitive scientists in the twenty years after the first edition. This included research on nativism and modularity (see Chapter 7.vi). Above all, however, it reflected Minsky's ideas on *The Society of Mind* (Minsky 1985). Strictly, one should rather say "Minsky and Papert's ideas", for they'd cooperated very closely in developing them.

The *Society* book had appeared (from a trade publisher, not an academic press) only recently, almost simultaneously with the PDP bible (see Section vi.a). But most cognitive scientists already had some idea of its main themes. Minsky had been mentioning them in conversation since the early 1970s, had given many people drafts from the beginning, and had published a "taster" ten years before (Minsky 1979). Indeed, it "was already cited in detail in a dozen books that came out before it did" (Brand 1988: 102). What they didn't expect was the rhetorical simplicity and structure of his text.

As for simplicity, the book was deceptively lucid. Minsky later said that during the ten years of drafting it, he'd ruthlessly removed jargon: "Whenever someone asked me what a term meant, I explained what it meant and took it out" (interview in Brand 1988: 102).

But the real surprise was the textual structure. The book consisted of 270 half-page to two-page snippets, each discussing a particular psychological and/or computational topic. These ranged from grasping blocks, through recognizing chairs, to emotion, jokes, self, and consciousness; and (as AI topics) from means–end analysis, through frames, demons, and defaults, to reinforcement in neural nets. The relevance of one snippet to another was largely tacit, the overall picture emerging only when the links had been activated in the reader's mind.

This apparent disorganization was deliberate. (It was widely rumoured that Minsky had wanted to present it in a loose-leaf folder, so that the pages could be endlessly rearranged, but that the publisher had refused; however, none of that is true: Minsky, personal communication.) The unusual structure of the book reflected Minsky's unorthodox ideas about the structure of the mind:

> My explanations rarely go in neat, straight lines from start to end. I wish I could have lined them up so that you could climb straight to the top, by mental stair-steps, one by one. Instead, they're tied in tangled webs.
>
> Perhaps the fault is actually mine . . . But I'm inclined to lay the blame upon the nature of the mind: much of the power seems to stem from just the messy ways its agents cross-connect. (1985: 17)

The questions he was addressing, and the general tenor of his reply, were broadly similar to those addressed in Aaron Sloman's theory of motivation and intelligence (see Chapter 7.i.e–f). For Minsky, too, was concerned with the mind as a whole, not just cognition—still less, a specific subset of cognitive abilities. And he too was concerned with architecture, not algorithms.

To be sure, when he could gesture towards an existing or plausible AI algorithm, he did so. ("Gesture", because he gave no references within the text: he left his specialist readers to decode his remarks using their previous knowledge, and provided a glossary to supplement the common sense of his general readers.) But the crux of his discussion was how the algorithms would fit together.

The fit he described was far from snug. Instead of likening the mind to a carefully designed top-down program, he compared it to a loosely organized collective, or society. A society is a distributed system made up of many different agents (see 13.iii.d). Each one has different skills, knowledge, and interests. And all of them do their own thing in parallel, though some may be dormant while others are active.

So it was for Minsky's theory of mind. Various types of communication between agents allowed for the emergence of ordered sequences, hierarchies, conflicts, negotiations, dominance, and cooperation. For example, different instincts or motives would compete for control of the hands, or of attention. Some "supervisor" agents would be able to prioritize one goal over another. But many motives—to eat or sleep, for instance—would spontaneously build up in strength until they broke through such top-down controls.

The GOFAI research of the past three decades, including work on hierarchy, planning, and learning, was a major source of ideas for Minsky's book. His Acknowledgment mentioned many well-known names in symbolic AI (1985: 322–5), and diagrams based on Patrick Winston's blocks-world arches adorned the chapter on learning (see 10.iii.d). But GOFAI concepts such as demons, production systems, blackboard architectures, and heterarchy (all parallelist in intent) were stronger influences than single-minded top-down programs such as GPS.

Connectionist AI was a strong influence too. So were Minsky's years of friendship with (among others) Pask, Hebb, Ashby, von Neumann, and especially McCulloch and Selfridge—all acknowledged in the text. And he leant heavily on his own theory of K-lines: a discussion of associationist memory (both semantic and episodic), and of the hardware likely to be best suited to it (Minsky 1980; cf. 1985, ch. 8).

Despite his long-standing infamy in connectionist circles, the K-line theory had already been greeted as "an important advance" by the organizers of the meeting that heralded the connectionist revival (see Section v.b, below). As they had put it:

> The central idea of the model, that partial mental states are re-created by activating particular agents that designate them, is an interesting intermediate position with respect to the issue of local versus distributed representations . . .
>
> The real value of Minsky's model will only be known when the model is specified in sufficient detail for it to be simulated, but the general approach of trying to implement sophisticated computational processes in parallel neuron-like hardware seems extremely promising. (Hinton and Anderson 1981: 23)

Another intellectual source (via the book's effective co-author, Papert) was Piagetian psychology. This highlighted the importance of developing new ways of organizing existing knowledge, as opposed to acquiring utterly new knowledge (see Chapter 5.ii.c and Section viii.c–d, below). Yet another was Sigmund Freud's psychodynamics. Unlike Kenneth Colby, Minsky didn't present a 'Freudian' program (7.i.a). But he used many Freudian ideas in his discussions of intelligence, motivation, emotion, humour, and self.

Not least, the book drew on evolutionary biology, with its emphasis on opportunistic bricolage rather than anticipatory design. As Minsky put it, the "messy ways [the] agents cross-connect [are] only what we must expect from evolution's countless tricks" (1985: 17). This supported his claim that the mind is a more or less well-integrated collection of separately identifiable systems (cell assemblies, networks, programs, procedures . . .) on many different levels.

That claim wasn't new. William McDougall (1923, 1926) had described the mind as "a colony of monads" (mindlike subsystems) interlinked by functional relations broadly similar to those posited for Minsky's "society" (see Chapter 5.ii.a). More to the point (since McDougall was by then almost forgotten), Hebb had stated this view clearly in 1949, and Herbert Simon had recommended hierarchy as a general principle of complex "design" (including evolution) some years later (1962). In addition, it had recently been stressed in various areas of cognitive science, for instance by David Marr and—as "mental modularity"—by Noam Chomsky and Jerry Fodor (Chapters 7.vi.d–e, 9.vii, and 16.iv.c–d, respectively).

But Minsky was especially interested in subsystems (modules) that were *not*, or not wholly, innate and *not* informationally encapsulated in Fodor's sense. How could they be learnt, and how could they come to interact and control each other? How could modules provided "by embryology" be honed by experience? In particular, how could cultural practices and beliefs (such as religion: Chapter 8.vi) develop, and enter into motivation and emotion as well as problem solving?

Minsky seemed to share Fodor's view that there was no hope of a detailed predictive science of higher mental processes (7.iii.d). But (like Sloman) he was aiming for a scientific understanding of the sort of computational architecture that can make such phenomena possible.

(Minsky's next book, *The Emotion Machine*, would develop these ideas further: see Chapter 7.i.e. As I write these words, it's not yet published. Or rather, it's not published *in print*. Draft chapters have been available for some time, however, on Minsky's web site: Minsky in preparation. Instead of dog-eared mimeographs or fiddly microfilms of "faint purple typewriting with handwritten annotations"—see Chapter 9.vi.a and ix.e—we have clearly legible and easily searchable text. And comments are welcomed, to be sent to Minsky's email address. In short, a good example of Solomon's House in cyberspace: see Chapter 2.ii.b. As for programs grounded in *The Emotion Machine*, he says: "It has almost enough (hidden) technical detail to program it. In fact we already have an early running version of its architectural structure, in which two robots cooperate to solve problems in a simple world, by communicating with some natural language"—personal communication, December 2004.)

The long-awaited *Society* book was rich in insightful comments, questions, and suggestions. But its hybrid spirit, sharing both GOFAI and connectionist insights, didn't fit either orthodoxy. (The need for hybrid systems had been hinted at in 'Steps', as we saw in Chapter 10.i.g; now, it was being made fully explicit.)

Even more to the point, it offered no mathematical analyses, no new techniques for instant application, and no new programs. As such, it was more likely to appeal to non-practitioners than to hands-on AI technologists. Daniel Dennett, for example, was strongly influenced by it (Chapter 16.iv.a).

However, Minsky being Minsky, the book couldn't be ignored by the AI community. It was given eighty pages of reviews/reply in the *Artificial Intelligence* journal (Stefik and Smoliar 1991).

One AI reviewer felt it "unfortunate that cognitive scientists have, for the most part, reacted to Minsky's book as though it were light reading or a minor conversation piece, to be relegated to a coffee table" (Stefik and Smoliar 1991: 321). But another, while allowing that the book was "fascinating and provocative", was less sympathetic:

> How, *exactly*, is this mediation [between agents in conflict] to be accomplished? Without an answer to this, the concept of an agent is an idea with no practical value; it is so general that it might encompass anything. The science, the engineering, the *value* of ideas lie in specifications and the associated falsifiable claims that Minsky chooses not to make.
>
> It is all well and good to invent interesting theories and speculate about their consequences, but such an approach almost inevitably leads to an examination of the strengths of these theories and not of their weaknesses—and Minsky's inability to choose between conflicting agents is indeed a weakness of his theory. A formal approach would have forced him to focus on this weakness until some resolution was obtained; his informality allows him to deal with it by sticking his head in the sand . . . All told, *The Society of Mind* is as Minsky describes it: an adventure in speculation, and not substantive science. It is well worth reading, but only in that light. (Stefik and Smoliar 1991: 338–9)

That was the polite way of putting it. I myself heard many exasperated complaints, even contemptuous dismissals. These were voiced by AI workers (including callow

MIT graduate students, hardly fit to lick his boots) more frustrated by Minsky's lack of technical detail than inspired by his subtlety and vision. His notorious disinclination to programming, and the lack of programs to illustrate his architectural ideas, didn't help (Chapter 10.i.a).

The complaints were understandable. But the dismissals were undeserved. The Popperian appeal (above) to "falsifiable claims" appeared to forget that Karl Popper himself had defended speculative metaphysics as a fruitful predecessor of "substantive science" (K. R. Popper 1935). In short, *Society* was setting an agenda for future AI much as 'Steps' had done over twenty years before (Chapter 10.i.f), albeit now in an even more informal way. The immediate difficulty was that neither symbolic nor connectionist AI was ready to get very far in working on it.

Minsky and Papert, clearly, were using connectionist ideas in their society-theory of mind. The "coffee table" book of 1985 emphasized K-lines, associative memory, and distributed systems. Why, then, were they so unrelenting in the (near-simultaneous) second edition of their notorious critique of Rosenblatt?

The reason was that, even by the late 1980s, most of the new perceptrons were conceptualized as initially unstructured self-organizing systems, doing only one thing—recognizing or expressing only one pattern—at a time (see Sections v–vi). The importance of domain-specific multilevel structure, whether emergent or inbuilt, was being acknowledged by some of the field's leaders. But it was a goal for the future rather than an achievement of the past (see Sections viii–ix). In respect of the *intellectual substance* of their earlier attack on connectionism, then, Minsky and Papert offered no apology.

e. Were they to blame?

As for their *historical influence* on the field, they insisted that—despite the demonology—they had nothing to apologize for. Yes, interest in connectionism had plummeted. But they weren't to blame. ("*Not us, guv!*")

Many readers were surprised by this. For it was—and still is—commonly believed that their 1969 book called a halt to connectionist research, which (with a few honourable exceptions) lay latent until the PDP-led renaissance of the late 1980s. Indeed, some commentators thought that such research had ceased completely.

For instance, a history of AI published in 1979 declared that GOFAI now dominated cognitive psychology, while "the attempt to imitate cell behavior [had] produced only trivial results, *then withered and died*" (McCorduck 1979: 47; italics added). Similarly, the Dreyfus brothers have said that "Everyone who knows the history of the field will be able to point to the proximal cause" of GOFAI's becoming "the only game in town": namely, the mid-1960s circulation of drafts of *Perceptrons* (H. L. Dreyfus and Dreyfus 1988: 21).

This descent into the "wilderness years", it's often added, was due not least to Minsky's unparalleled access to key personnel in the governmental funding bodies. Above all, he was close to Joseph Licklider (5.iv.f, 10.ii.a, and 11.i.b), whose good offices he'd procured for the type of AI being done in his laboratory at MIT. Hecht-Nielsen, for example, says:

Minsky and Papert's book, *Perceptrons*, worked. The field of neural networks was discredited and destroyed. The book and the associated conference presentations created a new conventional wisdom at DARPA and almost all other research sponsorship organizations that some MIT professors have proven mathematically that neural networks cannot ever do anything interesting. The chilling effect of this episode on neural network research lasted almost twenty years. (J. A. Anderson and Rosenfeld 1998: 305)

Certainly, most of the excitement, and the money, throughout the 1970s and early 1980s was in GOFAI rather than connectionism. There's even some suggestion that Rosenblatt himself could no longer get his papers published in the technical journals (Newquist 1994: 74). DARPA later admitted publicly that they had "largely abandoned" neural networks, seeing symbolic AI as "apparently more promising" (DARPA 1988: 23).

Nevertheless, the role of Minsky and Papert's critique is disputed. Widrow's comment, for instance, is very different from Hecht-Nielsen's:

[They] were able to prove that [the perceptron] could do practically nothing. Long, long, long before that book, I was already successfully adapting Madaline, which is a whole bunch of neural elements. All this worry and agony over the limitations of linear separability, which is the main theme of the book, was long overcome.

We had already stopped working on neural nets. As far as I knew, there wasn't anybody working in neural nets when that book came out. I couldn't understand what the point of it was, why the hell they did it. But I know how long it takes to write a book. I figured that they must have gotten inspired to write that book really early on to squelch the field, to do what they could to stick pins in the balloon. But by the time the book came out, the field was already gone. There was just about nobody doing it. (J. A. Anderson and Rosenfeld 1998: 60)

Their own assessment of their book's historical influence on the AI community explicitly rejected the received view:

How did the scientists involved . . . react to [our analysis]? One popular version is that the publication of our book so discouraged research on learning in network machines that a promising line of research was interrupted. Our version is that progress had already come to a virtual halt because of the lack of adequate basic theories, and the lessons in this book provided the field [i.e. AI in general] with new momentum—albeit, paradoxically, by redirecting its immediate concerns. (Minsky and Papert 1988, p. xii)

They (or anyway, Papert) admitted to a degree of "hostility" in their critique:

Did Minsky and I try to kill connectionism . . . ? Yes, there was *some* hostility in the energy behind the research reported in *Perceptrons*. . . . [Part] of our drive came, as we quite plainly acknowledged in our book, from the fact that funding and research energy were being dissipated on what still appear to me (since the story of new, powerful network mechanisms is seriously exaggerated) to be misleading attempts to use connectionist methods in practical applications. (Papert 1988: 4–5)

But the funding collapse that followed on the book was blamed by Papert not on their own quest for money but on the schismatic nature of 1960s AI (see Chapter 4.ix) and the "universalist" tendencies of their AI colleagues:

The desire for universality . . . was nurtured by the most mundane material circumstances of funding. By 1969, the date of the publication of *Perceptrons*, AI was not operating in an ivory-tower vacuum. Money was at stake. . . . [That's largely why our book was misinterpreted as claiming] that neural nets were universally bad. (p. 7)

Even more important, he said, was the computational weakness of the connectionist approach: "We did not think of our work as killing Snow White; we saw it as a way to understand her" (pp. 7–8). Moreover, he and Minsky pointed out that work on neural networks had become "virtually dormant" twice: once in the late 1950s, and again in the late 1960s (Minsky and Papert 1988, p. xi). The implication was that their 1969 attack wasn't to blame. The work had "stalled" because of its inherent limitations.

There's some evidence for their view in Widrow's comment, above, and there's more in Sections iv–v, below. Even so, their disclaimer is only a half-truth.

Drafts of their work had been circulating for at least eight years before the book appeared. What's more, they'd attacked perceptrons at several high-visibility meetings from the late 1950s on, such as the Chicago conference on self-organizing systems in 1962 (organized by Yovits: Yovits *et al*. 1962), and the Wiener Memorial meeting (in Genoa) in 1965. They'd also shared these critical ideas with the funding agencies: not just Yovits at the Office of Naval Research, but their contacts at ARPA too (J. A. Anderson and Rosenfeld 1998: 109). Those "contacts", of course, included Minsky's long-time friend Licklider, the supremo in charge of AI funding at ARPA/DARPA (Chapter 5.iv.f).

And people had listened. Cowan, who attended the Chicago meeting, says: "There's no question that after '62 there was a quiet period in the field" (J. A. Anderson and Rosenfeld 1998: 108). In short, if many researchers in 1969 were already disillusioned, the book's authors—if not the book itself—were largely responsible.

As for what they meant by giving "new momentum" to the field of AI, this involved a swing towards the central problem then being addressed by GOFAI:

> It seems to us that the effect of *Perceptrons* was not simply to interrupt a healthy line of research. That redirection of concern was no arbitrary diversion; it was a necessary interlude. To make further progress, connectionists would have to take time off and develop adequate ideas about the representation of knowledge. (Minsky and Papert 1988, p. xiii)

"In any case", they added, "the 1970s became the golden age of a new field of research into the representation of knowledge [see Chapter 10.iii.a]. And it was not only connectionist learning that was placed on hold; it also happened to research on learning in [symbolic AI]."

Their comment on symbolic AI was justified. Although Winston's model of learning had some influence, most of his GOFAI colleagues were working on the representation of knowledge for problem solving, planning, perception, and parsing (see Chapters 10 and 9.x–xi). Indeed, Minsky later admitted that "In all modesty, we were unduly influential" in dissuading GOFAI people from working on learning (Minsky 1984*b*: 122).

But whether connectionist learning really was "placed on hold" is questionable, as we'll now see.

12.iv. Lamps Invisible

The 1970s and early 1980s were the time of the Sleeping Beauty's sleep. Indeed, they're often called the Dark Ages of connectionism. But Cowan, for instance, has protested:

"I don't think it was the dark ages at all. There was a lot going on" (J. A. Anderson and Rosenfeld 1998: 110).

That's true. If the funding famine caused by the Minsky–Papert critique prevented connectionism from running riot, it didn't stop it in its tracks entirely. Nor did it prevent important neuroscientific work on connectionism, which didn't depend only on DARPA for financial support (see 14.v.a–b and x.a).

Most of the 1950s pioneers continued working throughout the 1970s. (Rosenblatt didn't: he died in a boating accident in 1971.) Some younger folk focused on networks as associative memories. (These generate pattern B on being shown pattern A; they include "content-addressable" memory, where A is a fragment and B the whole pattern.) And Marr and others, by the mid-1970s, diverted interest in AI vision towards connectionist models (see Chapter 7.v.b–d).

In short, by the time connectionism became publicly visible again in the mid-1980s, it rested on three decades of continuous research (J. A. Anderson and Rosenfeld 1998; Widrow 1990). The many enthusiastic outsiders who regarded it as "new" were thus sadly—or rather, happily—misguided.

Some of what went on in those "Dark Age" years excited fellow hibernators at the time, and those responsible would eventually bask in the limelight of the connectionist renaissance (see Section v). But some, discussed here, received scant attention even from like-minded colleagues. Uttley and Selfridge continued working but would never play a leading role again; and the pioneering John Andreae was unappreciated by his peers. (Werbos wasn't appreciated either: his contribution, an important learning rule, was recognized only in the post-hibernation period—see Section vi.d, below.)

a. Relegation to the background

Uttley, in the 1970s, was no longer regarded as an important force in cognitive science. On leaving NPL in 1966, he'd joined N. Stuart Sutherland's psychology group at the University of Sussex, where he implemented a large number of neural networks and moved increasingly towards computational neuroscience (see 14.ii.b). But, for reasons explained below, his ideas now fell on relatively stony ground.

By the standards of the time, Uttley's computer models were more complex than most. His numeral-learner, for example, contained over 200 units ("informons"), each with up to fifty variable inputs. And these might represent many more actual neurones: Uttley allowed that whenever he spoke of a "neurone", one could probably substitute "neurone pool". (Some of the more well-known connectionists said much the same thing.)

His mature theory, which described synapses in informational terms, was summarized in the *Journal of Theoretical Biology* (1975) and detailed in a large book soon afterwards (1979). One of the central ideas was that a single neurone can become a "classifier", not through external reinforcement or training but by "subset reinforcement". That is, the neurone calculates the input correlations, and disconnects itself from the uncorrelated inputs—thereafter responding only to the relevant subset. "Calculates" is the right word, here, for Uttley gave mathematical equations defining which probabilities were supposedly assessed, and how. (He didn't treat *all* learning as unsupervised: the neurones between cortex and muscle, he said, do need a teacher. In other words, they need conditioning.)

Uttley's work was nothing if not ambitious. For instance, he sketched networks explaining why Bach's music is perceived differently today from how it was appreciated in his own time. These ideas, in turn, rested on his more detailed account of how successive layers of neurones in auditory cortex may spontaneously (i.e. with attention, but without training) discover deeper and deeper relationships between musical notes. These were assumed to be hierarchical classifications, defined across time and in harmonic space.

He admitted that his approach threw little light on language. But he claimed that it helped us understand [take a deep breath, here] perception, motor skills, generalization, hierarchical concepts, memory, imagination and hallucination, music, art and craft, and purposive behaviour in general—even including birdwatching and religious belief. (He was a keen birdwatcher, and a committed Christian.)

Given these wide-ranging claims, one might have expected Uttley's *magnum opus* of 1979 to make a splash in the cognitive science community. However, it didn't.

Besides his failings as a communicator in face-to-face conversations (see Section ii.c), and the fact that he "was not someone who went out of his way to interact with other people" (A. Baddeley, personal communication), his highly abstract writing style didn't help. Even his shorter book, written a few years later to show "the enquiring layman" how adaptive neural networks can have mindlike properties, was beyond the capacity of most laymen, enquiring or otherwise (Uttley 1982). Valentino Braitenberg's (1984) contemporaneous account of mindlike "Vehicles" grounded in interacting reflexes was far more accessible (14.vi.a and 15.vii.a).

The main problem, however, was the change in popularity of his theoretical approach. Information-theoretic psychology and psychophysiology had fallen out of favour since around 1960. Even within behind-the-scenes connectionism, Uttley's circuit-based approach had been pushed aside by studies of random networks, and of single impulses rather than spiking frequencies. The focus was on trainer-led (supervised) rather than spontaneous learning, and conditioning had been replaced by pattern recognition as the centre of attention. (Interest in models of conditioning would revive in the late 1980s.) Indeed, the first—and highly influential—PDP conference was held in the very year that Uttley's technical book was published (see Section v.b).

Moreover, network models *in general* were still out of fashion in 1979. Most of Uttley's Sussex colleagues in cognitive science (myself included) were more interested in GOFAI approaches, and were concentrating on vision and knowledge representation rather than learning. This applied even to Geoffrey Hinton (1947–), who in the early to mid-1970s at Sussex was still using GOFAI methods (see below). The general feeling was shared by Uttley's postdoc William Phillips, hired to work with him at Sussex for four years on a reading aid for the blind (which wasn't successful):

> I didn't study Uttley's theories very closely, even though I had to help him give a presentation on the "Informon" to the Royal Society. At that time I was very much under the spell of Max Clowes's arguments for the necessity of a syntactic approach to cognition in general, including image processing and scene understanding. As Uttley's work did not seem relevant to those issues it didn't seem worth while spending much time on it. (W. A. Phillips, personal communication)

Ironically, Phillips—currently Professor of Psychology at Stirling's Centre for Cognitive and Computational Neuroscience—now favours information-theoretic and other essentially statistical perspectives on cognition. So he now appreciates Uttley's work better than he did then. But it had no overt influence on his development.

In sum, various factors combined to make Uttley's book invisible—even at very close range.

b. Run and twiddle

Selfridge, too, now received less attention than he deserved—especially from AI scientists. To be sure, he was still a respected voice within the core AI community. (He'd left academia for Bolt, Beranek & Newman but retained his links with his fellow pioneers.) And cognitive psychologists valued his earlier research. Pandemonium was highlighted in two early textbooks (Neisser 1967; P. H. Lindsay and Norman 1972), and fetchingly illustrated in one of them (see Figures 6.5 and 6.6). But his own theory of learning was so different from the then current GOFAI paradigm that most AI people ignored it. (Minsky didn't, but he was always a maverick.)

Even the embattled connectionists paid little or no attention. This was partly because Selfridge didn't make his new work easily available. A few brief accounts appeared in out-of-the-way publications, and drafts of his future book were circulated to friends—including myself (Selfridge 1981). But it was never officially published.

Focusing on *the whole animal* rather than isolated systems such as vision, Selfridge argued that the fundamental mechanism of all adaptive movement is a form of unsupervised trial-and-error learning. He called this "RT", or "run and twiddle", identifying the core principle as "if things are getting better, don't change what you're doing" (Selfridge 1984: 23). Even *E. coli* can engage in a random walk, follow a positive value gradient, and then stop if things don't get any better. In other words, he said, even simple creatures—unlike man-made perceptrons (which he criticized for this reason)—"want" something.

What *E. coli* wants is an optimal concentration of certain chemicals, which can be achieved by simple chemotaxis. Animals with more complex wants, or purposes, need more complex mechanisms to guide and structure their behaviour. That is, they need hierarchies of adaptive control loops—but how can such systems, and their development, be conceptualized? As he admitted, it was hard to know how to pose the relevant questions, never mind answer them.

(His answers today are posed in terms of "EAMs": Elementary Adaptive Mechanisms: Selfridge and Feurzeig 2002. Now based at MIT's Media Lab, Selfridge is working on DARPA's "ABC" programme. The abbreviation stands for Agent-Based Computing, an area currently of great interest to GOFAI too—see Chapter 13.iii.d–e.)

Another reason for the lack of interest in Selfridge's 1970s ideas was that unsupervised learning had become unfashionable. Thanks to perceptrons, Adalines, and—ironically—Pandemonium, most connectionism in the 1970s involved a human trainer. Those few people who were still modelling reinforcement were swimming against the current.

c. Reinforcement and purpose

One of the people still modelling reinforcement was Andreae (1927–), an Englishman based since 1988 at the University of Canterbury, New Zealand. He'd started in the early 1960s, with a model of noise-tolerant trial-and-error learning called STeLLA (Andreae 1963, 1969*a*; Gaines and Andreae 1966). (The name, and its typography, indicated Andreae's former post: Standard Telecommunications Laboratories Learning Automaton.)

STeLLA was interesting not least because it was an attempt to embrace both sides of the symbolic–subsymbolic and virtual–interactive dichotomies. That wasn't always recognized by Andreae's contemporaries, however. For example, Edinburgh's James Doran (1940–), who'd already simulated a "pleasure-seeking" rat robot inspired by the Grey Walter tortoises (Doran 1968*a*), said this:

> [It] is far from clear how such network systems can be persuaded to yield really complex behaviour. The results of the work [by Minsky and Papert: 1969] on perceptrons and other such self-organizing systems do not encourage optimism. (Doran 1969: 524)

Andreae described STeLLA as constructing, and being guided by, an internal plan of action and model of the environment. These were implemented as matrices coding probabilistic transitions between various pattern–action pairs. He soon realized, however, that probabilistic transitions weren't going to be enough. His associative robots needed to be able to learn fixed sequences too. They couldn't be programmed (for instance) to recite *one, two, three, four, five* ... flawlessly. But they should be able to learn to do so: even a 99 per cent probability of coming up, next, with *six* wouldn't be acceptable. So he redesigned STeLLA in the early 1970s, to produce a fundamentally different machine: PURR-PUSS (Andreae 1977, 1987; Andreae and Cleary 1976).

The acronym stood for Purposeful Unprimed Real-world Robot with Predictors Using Short Segments. The "PURR" coded Andreae's guiding aims: that the robot should set its own goals; learn patterns and behaviours that he hadn't anticipated (even though some "innate" biasing might sometimes be needed); and have real-world capacities comparable with ours, such as motor control, 3D vision, hearing, and even language. The "PUSS" coded the complex associations that were crucial in the robot's processing and performance.

For example, this new system was able to construct temporary memories on various hierarchical levels. These were broadly equivalent to the pushdown stacks and "contexts" used in GOFAI planning (Chapter 10.iii.c and v.b–d), in the sense that they enabled the system to monitor its own performance, keeping track of where it was in a plan structure. Instead of being stored as symbolic expressions, they were stored as associations with environmental changes (compare: tying a knot in one's handkerchief) or, more often, trivial bodily actions (compare: raising an eyebrow). Fixed sequences, by contrast, require long-term memories—but these too were stored as complex associations constructed by the system.

One of Andreae's papers (1969*a*) was chosen as the first item in the newly founded *International Journal of Man–Machine Studies*, whose readership included psychologists, and he contributed to an interdisciplinary encyclopedia co-edited by a linguist (Andreae 1969*b*). More recently, he has been cited in historical remarks in the reinforcement-learning literature (Sutton and Barto 1998: 19, 84, 109).

Nevertheless, and although he was respected by control engineers, he had relatively little influence on cognitive scientists.

One reason was place of publication. Most of his papers appeared in journals aimed at engineers. And the forty technical reports on PURR-PUSS that were lodged in leading libraries, including the Library of Congress, were easy to find only if one already knew about them. (Modulo information overload, the Web should decrease this sort of invisibility.)

A closely related reason was timing. Andreae's earliest work appeared when people excited by connectionism were looking in Rosenblatt's direction. By the time he described his second system, connectionism in general was out of fashion. One reviewer of his 1977 book (Schubert 1978), for example, wheeled out John McCarthy's GOFAI objection that one must understand a cognitive skill well enough to program it before one can enable a computer to learn it (Chapter 10.i.f). Andreae's position, by contrast, was that complex associative robots probably won't be fully intelligible, even to their designers. (The same may apply to detailed neuroscientific models: see Chapter 14.v.d.)

Only very recently, thanks to the renewal of interest in this area, has his later research been made widely available—although it remains to be seen how many people will read it (Andreae 1998). Previously, it was neglected: "*Unfortunately*, [Andreae's] pioneering research was not well known, and did not greatly impact subsequent reinforcement learning research" (Sutton and Barto 1998: 19; italics added).

12.v. Behind the Scenes

Although no behind-the-scenes connectionist was attracting much attention in the cognitive science community, some were making significant theoretical advances nonetheless.

Very few were talking to each other: their shared project didn't become truly communal until the end of the 1970s (see subsection b below, and Section vii). Meanwhile, they were working in very different disciplines, and on rather different problems.

a. Left alone to get on with it

If Andreae was unfortunate, Widrow—in his own estimation—wasn't. Not only did he (unlike Andreae) receive enormous attention in his early career, but he managed to escape the near-inevitable consequences.

On his view, the anti-perceptron backlash largely passed him by simply because he'd called his networks by a different name:

> I looked at [Minsky and Papert's] book, and I saw that they'd done some serious work here, and there was some good mathematics in this book, but I said "My God, what a hatchet job." I was so relieved that they called this thing the perceptron rather than the Adaline because actually what they were mostly talking about was the Adaline, not the perceptron. (J. A. Anderson and Rosenfeld 1998: 60)

Another reason for Widrow's lucky escape, perhaps, was that—unlike Rosenblatt—he hadn't provocatively presented his Madalines as models of human psychology.

He had, however, suggested in the paper on the LMS algorithm (Widrow and Hoff 1960) that AI could be hugely advanced by using "computers built of adaptive neurons". This suggestion may have looked like an afterthought to the contemporary readers of that paper, but AI was already close to Widrow's heart. He'd attended the Dartmouth Summer Project in 1956 (Chapter 6.iv.b), and decided then and there "to dedicate the rest of my life to that subject" (J. A. Anderson and Rosenfeld 1998: 49). Adaline-based computing, he believed, could do things that NewFAI couldn't:

> [Samuels's checkers-player] would be considerably more powerful if it were possible to extract from the memory previous situations that are *similar* and not necessarily *identical* to the current situation. Far less experience and storage would be needed to adapt to a given level of competence. (Widrow and Hoff 1960: 133)

In general, he said, AI needed not "rote memory" but "recall-by-association parallel-access memory systems"—which could be implemented in "Memistors". These were Adaline-based devices, providing a simple form of content-addressable memory.

A third reason why Widrow was less affected by the backlash was that, as a control engineer, he could demonstrate practical applications that undeniably worked.—But *how* did they work? The answer lay only partly in the special-purpose hardware. Even more important was the mathematics. If the journalists had been turned on by the switches and the pencil leads, the scientists were more excited by the learning rule:

> [The key to the LMS algorithm] was to get the gradient not by taking many samples of data and measuring mean square error over a long period of time. The idea was to be able to get the gradient from a single value of error—a single number, square it, and say that's the mean square error. Then when you work out the gradient of that error with respect to the weights, it's really simple. You get an algebraic expression and you realize that you don't have to square anything; you don't have to average anything to get mean square error. You don't have to differentiate to get the gradient. You get all this directly in one step. Not only that, but you get all components of the gradient simultaneously instead of having to make measurements to get one gradient component at a time. *The power of that, compared to the earlier method* [i.e. steepest descent], *is just fantastic.* (J. A. Anderson and Rosenfeld 1998: 53; italics added)

(*Plus ça change*... It became clear years later that the "fantastic" Widrow–Hoff adaptation rule was essentially the same as Hull's early 1940s equation for calculating habit strength: Sutton and Barto 1981; see 5.iii.b.)

Widrow wasn't the only one to address his work mathematically. In general, the behind-the-scenes research of the 1970s–1980s which—unlike that discussed in Section iv—*did* eventually receive recognition was mathematical in spirit. It aimed to find more powerful training procedures, especially for multi-layer nets (which Rosenblatt had found intractable).

The problem with multi-layer nets was that they contained "hidden" units. These weren't directly connected to *both* input and output units, and might even be directly connected to *neither*. And that fact carried unavoidable problems with it:

* How could an individual hidden unit be reached by a learning rule (which monitors the output, given a certain input)?
* Could one define rules that would enable multi-layer networks to adapt efficiently?
* Was there any multi-layer equivalent of the single-layer convergence theorem?

* What were the limits, if any, on the patterns or structures that specific kinds of rule and/or network could learn?
* How few hidden units would suffice for any given task? (Too many, and the system would in effect be a look-up table.)
* What could be said about the potential storage capacity of associative memories with different numbers of cells?
* And what were the functional merits of distributed memories as compared with localist networks, or with GOFAI?

Answers to these abstract questions were sought by a wide variety of people. Some were control engineers, seeking a multi-layer equivalent of the LMS algorithm. Some were computer engineers, keen to implement their answers in VLSI chips. Others were electrical engineers or physicists, interested in dynamical models of associative memory. Yet others were computer scientists already working in AI.

And many were mathematically minded psychologists, usually with experience of GOFAI modelling and/or interests in neuroscience. A few of them tried to model the nervous system in some detail (see 14.iv–v).

b. A problem shared . . . ?

One shouldn't assume that all these people communicated with each other, or even knew of each other's existence. During the Dark Ages most of them were working in isolation, separated not by monastery walls but by disciplinary boundaries. This is why priority disputes in this area are so common. Indeed, a recent collection of interviews abounds with coded and not-so-coded priority claims, backed up by hard-luck stories about lack of recognition in the wilderness years (J. A. Anderson and Rosenfeld 1998).

To be sure, GOFAI-based cognitive scientists already had a thriving interdisciplinary community (see Chapters 6–10), and many influential connectionists had strong links to that. Some had even developed GOFAI-inspired psychological theories.

For instance, David Rumelhart at UC San Diego had developed formal models of story plots, or "story grammars" (Rumelhart 1975). And he had used both ATNs and ideas from HEARSAY to model reading errors (see Chapters 10.v.e, and 9.xi.b). Indeed, HEARSAY was later acknowledged by the leaders of PDP as having "played a prominent role in the development of our thinking" (Rumelhart, McClelland, *et al.* 1986: 43). Rumelhart had also been excited by NewFAI's "semantic networks" (Chapter 10.iii.a), which had helped lead him and others to work on associative memories (Findler 1979).

Similarly, Christopher Longuet-Higgins had produced GOFAI models of musical perception (see Chapter 2.iv.c). Moreover, there was a small but growing community of connectionist workers on vision, including Marr, Tomaso Poggio, Stephen Grossberg, and Hinton. Although they were wary of GOFAI approaches, most of them had started from that base; and a few, such as Hinton, had worked on GOFAI projects for some years (see below, and Chapters 10.iv.b, 14.ii–iii).

However, specialists in AI and psychology, even if they communicated with each other, had little or no contact with engineers and physicists. If "A problem shared is a problem halved", there were no helpful interdisciplinary half-measures available.

This situation would begin to change in the summer of 1979, when many connectionists (including supporters of *non*-distributed memories) met for the first time. The occasion was a meeting on 'Parallel Models of Associative Memory'. It was hosted in the Cognitive Science Center in La Jolla, by Rumelhart and Donald Norman—key members of the PDP group (see below). (Ironically, 1979 was the year in which connectionism was announced to have finally "withered and died": Section iii.e, above.)

This was one of the many projects funded around 1980 by the Sloan Foundation, who were then switching support from neuroscience to cognitive science (see Chapter 8.i.c). The organizers were the psychologist Hinton and the neurophysiologist James A. Anderson (1940–), at Brown. Hinton was then based in the Cambridge University research unit that had housed Kenneth Craik, but he'd visited UCSD's Cognitive Science group and was about to return there. As for Anderson, he'd been at UCLA in the early 1970s but was now based at Brown.

(NB. One shouldn't confuse James A. Anderson with John R. Anderson, whose ACT* system was discussed in 7.iv.c. It's easy to do so, however, because they both did important work on associative memory. Indeed, Hinton and J.A.A. described J.R.A.'s ideas on this topic as "seminal"—Hinton and Anderson 1981: 15. In this chapter, "Anderson" refers to J.A.A. unless otherwise noted.)

Like Christopher Langton's "Call for Papers" for the seminal conference on A-Life (15.ix.a), Hinton and Anderson's invitation had been sent to a very varied group:

> We deliberately chose people from fields as diverse as neurophysiology, cognitive psychology, artificial intelligence, mathematics, and electrical engineering. *Most of the participants had not met each other previously and in some cases were unaware of each other's work.* Yet as things progressed, it became clear that there were large areas of agreement, as well as strong disagreement about details. (Hinton and Anderson 1981, p. vii; italics added)

The new-found community, however, wasn't destined for sweetness and light. Within a few years, the "strong disagreement about details" would help engender what Bart Kosko later described as "many feuding factions", some of whose leaders "literally hated each other's guts" (J. A. Anderson and Rosenfeld 1998: 402–3). Human beings being what they are, this lack of collegiality was largely due to jealous resentments caused by the *success* of the field, with its attendant publicity and commercial promise (see Section vii).

However, all that was yet to come. At the end of the 1960s, success seemed very far off. Quite apart from the Minsky–Papert fusillade, there were many practical difficulties.

c. How large is your memory?

One difficulty concerned multiple storage in associative memories. Both Rosenblatt and Widrow had found that their machines could store more than one pattern at a time—but the more patterns involved, the less efficient the storage: interference effects came to the fore.

This made sense, given that any one pattern was distributed across many different units, and that each unit might contribute to several patterns. Indeed, it raised the spectre of a rapid descent into chaos as the number of stored patterns increased. What was needed, beyond these empirical observations, was some *principled* (mathematical) estimate of the potential storage capacity of connectionist systems, and of the efficiency of distributed memories.

Such questions had been asked before, in relation to punch card systems as well as brains. Turing himself had been asked by John Z. Young "whether one can make an estimate of how much information can be stored in a brain with a given number of nerve cells", and had sketched the bare bones of a reply (S. S. Turing 1959: 145 ff.).

Moreover, Peter Greene, in the Chicago group founded by Rashevsky, had recently developed that earlier work. He'd shown how random superimposed ("hash") coding could be used both to store and to recall associative memories (Greene 1965). (Cowan regards this as "virtually identical with David Marr's cerebellum model, except it's not applied to the cerebellum"—J. A. Anderson and Rosenfeld 1998: 110; see Chapter 14.iii.d.)

But Greene's paper wasn't widely read. The first to reach an appreciable audience for these matters were Longuet-Higgins and his students David Willshaw (1945–) and Peter Buneman, at Edinburgh University (before Longuet-Higgins's move to Sussex in 1974). In the late 1960s, Longuet-Higgins outlined a quasi-holographic model of memory (1968*a*,*b*), and then collaborated with his two students on a non-holographic model (Willshaw *et al.* 1969).

He presented some of these ideas informally at one of the renowned Serbelloni meetings on theoretical biology run by his Edinburgh colleague Conrad Waddington (15.v.b). On that occasion, Cowan made him "absolutely furious" by saying they were "virtually identical with Steinbuch's learning matrix" in the late 1950s (J. A. Anderson and Rosenfeld 1998: 110). Longuet-Higgins in "absolutely furious" mode was, as all his friends know, fearsome to behold. But this storm in a teacup didn't matter. For the main audience wasn't the select group overlooking Lake Como, but the wide readership of the *Proceedings of the Royal Society*, Series B, and the even wider readership of *Nature*.

Later recalling the critical reception of his 1968 *Nature* paper, Longuet-Higgins said it "attracted altogether too much attention when it first appeared in print". But he had a ready defence:

> I should have known better than to speculate so shamelessly about the neural mechanism of temporal memory; the idea of a neuron's "learning" a particular frequency has scarcely a leg to stand on. But the discussion the paper provoked—in both physical and physiological circles—testified to the fact that ideas, however far-fetched, are the life blood of scientific research, and that a demonstration of how something could possibly happen (in this case, cued temporal recall) may be almost as welcome as the discovery of how it actually does happen. (Longuet-Higgins 1987: 367)

Quite apart from any second thoughts about that paper, he had other reasons for moving on to a non-holographic theory. Several people in the 1960s had likened the brain to a hologram, a newly invented device for storing optical images. Patterns in a hologram are not stored at specific points but are distributed across the whole system. They can be reconstructed from a damaged hologram or from a small part of it, so the device acts as a content-addressable memory.

The physicist P. J. van Heerden, when he originated the brain–hologram analogy in 1963, had compared holograms to Beurle's models of neural nets, which were inspired by current ideas about the cortex (see Section ii.b). And the neuroscientist Karl Pribram, co-author of *Plans and the Structure of Behavior* (6.iv.c), had likened them to brains in some detail (Pribram 1969). Later, in a memorial volume for his ex-teacher Lashley, he said:

> The properties of holograms are so similar to the elusive properties that Lashley sought in brain tissue to explain perceptual imaging and engram encoding, that the holographic process must be seriously considered as an explanatory device. (Pribram 1982: 176)

Willshaw and Longuet-Higgins, however, were sceptical.

Although the analogy was suggestive, they said, it breaks down on closer examination. They had recently generalized the hologram to deal with sound, in the "holophone" (Willshaw and Longuet-Higgins 1969*a*). But their main aim hadn't been to widen the technology (from optics to acoustics). Rather, they wanted to estimate the computational power of holograms *considered as a general class of device*. Since temporal patterns are one-dimensional, the holophone's mathematics would be relatively tractable.

Now, they argued that some crucial physical properties of holograms can't be attributed to brains (Willshaw *et al.* 1969). Moreover, the quality of the regenerated patterns is *in principle* unnaturally low: they'd wryly noted the "rather distressing noise characteristics" of the holophone (Willshaw and Longuet-Higgins 1969*a*: 356). As for computer modelling, holographic systems would be excessively slow in simulation: the holophone needs N^3 separate multiplications for a signal of length N. This was a theoretical point, but had been borne out by experimental simulations.

In short, holographic models of memory, although admittedly suggestive, didn't fit the biological facts. (Some neuroscientists, including Pribram, retained faith in them nonetheless: Pribram *et al.* 1974.) And they weren't computationally tractable either.

Instead of holographs, Willshaw and Longuet-Higgins offered a simple matrix schema of an associative network having the relevant functional properties (Willshaw *et al.* 1969). This schema was both easier to simulate and more biologically plausible. They predicted that it "may well find application in computing technology, especially when parallel computation techniques become generally available" (Willshaw and Longuet-Higgins 1969*b*: 351). And although their approach was primarily "a mathematical investigation", not an empirical hypothesis, they outlined how it might apply to actual synapses (Willshaw *et al.* 1969: 962; cf. Willshaw 1981).

Their matrix model could store more information than a hologram of comparable size, and could reconstruct patterns more faithfully. Indeed, "in optimal conditions [the network] has a capacity which is not far from the maximum permitted by information theory" (Willshaw *et al.* 1969: 960). Specifically, the informational capacity was 69 per cent of the theoretical maximum, and proportional to the number of possible connections.

This was encouraging: it meant that interference between superposed representations was less of a threat than had been feared. Admittedly, simulation had shown that "[even] in the absence of damage there is a fairly sharp limit to the number of associations that can be stored; if this number is exceeded the performance of the system degenerates rapidly" (Willshaw and Longuet-Higgins 1969*b*: 358). Nevertheless, the high potential efficiency (within that limit) of distributed systems was a strong point. The more patterns a memory model can store, the better.

Or so it seemed. At the turn of the next century, the ex-Ratio Club neurophysiologist Barlow would show that this assumption was mistaken (Barlow 2001*a*: 603–4). He'd already suggested as much, many years earlier (Barlow 1961). Now, he provided mathematical arguments to prove that *learning*, as opposed to mere *storage*, requires "sparse" representations—which use many more units to store each pattern (see 14.x.e).

At the turn of the 1970s, however, Willshaw and Longuet-Higgins's analysis seemed to be very good news.

d. Disillusion on distribution

The bad news was that learning in multi-layer nets was still intractable. When Willshaw and Longuet-Higgins published their work (and Minsky and Papert their broadside), disillusionment had begun to set in. Indeed, Widrow later said: "[The] field was already gone. There was just about nobody doing it" (J. A. Anderson and Rosenfeld 1998: 60)—see Section iii.e.

Widrow himself had given up in disgust. He'd returned to his early interests: "We stopped doing neural nets because we'd hit a brick wall trying to adapt multilayer nets. On the other hand, in adaptive filtering and adaptive signal processing, we were making great strides" (J. A. Anderson and Rosenfeld 1998: 61).

Some connectionists avoided these problems by focusing on localist memories, instead of distributed ones. In a localist system, a concept or pattern is represented by a single unit. An early 1980s example, published in a leading psychological journal (largely because it was closely compared with experiments on human subjects), was a model of word recognition broadly similar to the localist Pandemonium (McClelland and Rumelhart 1981; Rumelhart and McClelland 1982). In this model, letter strokes, letters, and (finally) words were recognized by three separate layers.

Localist networks have certain processing advantages, and were defended by Julian Feldman—co-editor of *Computers and Thought*—in the core journal *Cognitive Science* (J. A. Feldman and Ballard 1982). Moreover, the 1970s saw increasing experimental evidence for "grandmother cells" in the brain, and Barlow (1972) had proposed an influential theory that emphasized them (Chapter 14.x.e). So even if the cerebral cortex was a distributed system, it might have some important localist features.

Nonetheless, distributed systems seemed more interesting to most people. The authors of the word recognition network, for instance, were already moving firmly in that direction (see Section vi).

Gradually, the disillusionment lessened, as light became visible at the end of the distributed tunnel. The exciting new results included linear associative memories (in 1972), Hopfield nets (in 1982), the Boltzmann machine (in 1985), and above all back propagation (in 1986). Widrow, again: "We would have given our eye teeth to come up with something like backprop" (J. A. Anderson and Rosenfeld 1998: 60).

These ideas, all discussed below, provided complex networks with increasing computational power and efficiency. They also—up to a point—offered increasing biological plausibility.

(Only "up to a point". For one thing, back propagation is unrealistic: brain synapses don't transmit backwards. For another, connectionist theories and artificial networks were/are drastic oversimplifications of real neurones: see 14.i.)

e. Linear associative memories

Linear associative memories were independently defined in 1972 by James Anderson of UCLA (who had developed a simpler memory model in the late 1960s), and by Teuvo Kohonen (1934–) of the Helsinki University of Technology. Both men had long-standing interdisciplinary interests, but they came into cognitive science from diametrically opposite directions.

Anderson was an experimental neuroscientist whose teenage passions had been amateur radio and science fiction, whereas Kohonen was an electronic engineer whose "special hobby" at high school had been psychology—not least, Gestalt psychology (J. A. Anderson and Rosenfeld 1998: 242, 147). Whereas Anderson studied memory in a neuroscientific context, even developing a detailed "cortical model" of its implementation in the brain (J. A. Anderson 1972: 190 ff.), Kohonen entered from the engineer's corner. He was intrigued by various recent analyses of holographic memory, and by some suggested alternatives—including that of Willshaw and Longuet-Higgins (Kohonen 1972: 174).

Coming from distinct professional communities, Anderson and Kohonen knew nothing of each other's work for several years (although both attended the La Jolla meeting in 1979). Indeed, Kohonen had often found his professional background to be an obstacle in biological circles.

To be fair, even his fellow engineers were initially sceptical. He originally became interested in neural networks in 1962, and in the late 1960s submitted his first two papers on the topic to two very different journals: *Nature* and the *IEEE Transactions on Computers*. Both submissions "came back like a boomerang", because his views were considered so "weird" by the editors and referees (J. A. Anderson and Rosenfeld 1998: 150).

His theory of associative memory was eventually published in 1972. But it wasn't widely accepted until the 1980s. Part of the problem was that although he was "desperately trying to relate [his] networks to biology", he presented himself as "an inventor" (p. 160). He attended some neurophysiological meetings in the 1970s, but found it "very difficult to get an engineer's point of view accepted" (p. 155).

The engineer's point of view is more mathematical than the biologist's, even though neurophysiology had been moving in this direction ever since Rashevsky (see Chapter 4.iii.c). Anderson himself, whose own paper appeared in the successor to Rashevsky's journal, has said:

> It is interesting to see how differently the two authors develop the same basic idea: Kohonen is concerned with the mathematical structure, while Anderson is concerned with physiological plausibility . . . Kohonen does not make the neural analogies as explicitly as does Anderson, but the mathematics is identical. (J. A. Anderson and Rosenfeld 1988: 171)

The mathematics was not only identical, but also simple (linear): at base, multiplication and addition. These new systems—which Anderson called "interactive" memories and Kohonen "correlation matrix" memories—were in effect complex perceptrons, in which a *group* of *analogue* (not McCulloch–Pitts) units is connected to another such *group*. The crucial biological analogies were thus neuronal firing frequency, which *varies* with the strength of the stimulus, and cell assemblies (5.iv.c). Grandmother cells were tacitly denied, or anyway ignored (see Chapter 14.x.e).

The point to grasp, for those not mathematically inclined, is that these systems boiled down to *arithmetic*. Each cell group was represented by a vector (a matrix of many individual values) defined over its component units. And its activity level was expressed by adding the activity levels (summing the weights) of all the units. As for the synaptic weight changes that effected Hebbian learning, these were proportional to the product reached by multiplying the pre- and post-synaptic activity levels.

At the 1979 La Jolla meeting, Kohonen wowed the audience by presenting a "spectacular" example of content-addressable memory (Hinton and Anderson 1981: 9). To near-universal amazement, an entire face was reconstructed by inputting either a mere fragment, or a smudged version, of the image. Moreover, this was "unsupervised" learning, done without any feedback from the programmer.

Kohonen stressed that 100 different faces had 100 *distinct* representations in the network. But these were distributed ("holological") matrices, not localized units. More important, since even Simon had allowed that nothing in GOFAI-based psychology required that memories be stored at a specific point, the computational operations were *structured interactions* correlating these collective states (Kohonen *et al.* 1981: 107).

At one level, simple correlation matrix memories could be seen as "the extreme and purest cases of S–R mapping", since there is either no feedback whatever from response to stimulus, or (in certain recurrent systems) feedback involving *all* the stimuli (Kohonen *et al.* 1981: 112). At another level, they could be seen as models of real neural structures. Indeed, Kohonen made various comparisons between 3D matrices and laminar cortex, and between his auto-associative memories and synaptic learning.

Similarly, he suggested that his account of topographical mapping (Kohonen 1982) explained why there are various types of *interneurone* in the brain (see Chapter 14.x.a). (This work on topological mapping was akin to that of Willshaw and Christoph von der Malsburg, who were cited several times in Kohonen's paper: see 1.iii.h and 14.v.b.)

f. The physicists have their say

Ten years later, in 1982, a very different model of associative memory burst onto the scene: Hopfield nets. These are named after the Caltech physicist John Hopfield (1933–), who intended them as "a form of general (and error-correcting) content-addressable memory". They'd actually been described many times by others, earlier. But for various reasons, explored below, it was Hopfield who got the credit.

Hopfield nets were globally connected, in that every unit was linked to every other. So there weren't any separate layers: each unit acted as an input, output, and processing unit.

To be set up for an application, the inputs would be coded as (for example) sensory feature detectors, while the final output was the relevant concept, or pattern. The units were McCulloch–Pitts neurones. The (initially random) connection weights were continuous, and symmetrical (each unit would excite its 'pair' to the same extent). At each change point—which were random, not time-stepped—the unit would sum the weights of its synaptic inputs, and then 'fire' only if its threshold had been reached.

In physical systems made of many simple elements, said Hopfield, stable collective phenomena (such as vortex patterns in fluids) emerge spontaneously. The explanation is dynamical:

> The equations of motion of the system describe a flow in state space. Various classes of flow patterns are possible, but the systems of use for memory particularly include those that flow toward locally stable points from anywhere within regions around those points. (Hopfield 1982: 460)

In other words, the system undergoes myriad local changes until it settles into equilibrium. (Compare Steve Wolfram's four dynamical patterns in cellular automata, only one of which is suitable for computation: see 15.viii.a.) Memory storage is analogous to the stability of the relevant points (attractors). Error correction and generalization correspond to the fact that one and the same attractor will be reached from anywhere within the relevant region. And content addressability is comparable to the reconstruction of the attractor state from an approximation to it.

As for how this happens, in physics the settling-down process involves energy minimization. So Hopfield defined an algorithm, based on the theory of spin glasses, that enabled a simulated network to reach equilibrium in a comparable way. This is reminiscent of von Neumann's hunch that the logic of the brain might be closer to thermodynamics than to formal logic (see Section i.c).

The global "energy" of the network as a whole was defined by summing across all units. And the effect of (binary) state changes in the thresholded units was either to decrease this global measure or to leave it unaffected. When there were no more unit changes (i.e. when the network had settled down), this measure would have reached a minimum.

Having defined this class of networks, Hopfield ran a large number of simulations varying the numbers of units and the starting points (i.e. the binary values assigned to the units before the system was allowed to run). He asked (for instance):

* how many memories a network of a given size could store before producing retrieval errors, or even being totally overwhelmed;
* how many units needed to be set "correctly" at the start in order for the *closest* attractor to be reached;
* and how dissimilar two inputs must be if they were to be stored as distinct memories (distinct attractors).

Rosenblatt, too, had combined theoretical analysis with systematic simulation experiments (see Section ii.e–f). But Hopfield saw his networks as an advance on perceptrons in three ways:

* They involved backward as well as forward connections (this was implied by the symmetry condition);
* they focused on abstract emergent properties, as opposed to the specific results of encountering a particular pattern in the real world;
* and they didn't require synchronous change.

Similarly, the brain involves backwards connections (though not two-way synapses), and it doesn't work in time steps. However, the brain isn't made of McCulloch–Pitts neurones. Two years later, Hopfield generalized his results to cover more biologically realistic units. The paper's title said it all: 'Neurons with Graded Response have Collective Computational Properties Like Those of Two-State Neurons' (1984).

Hopfield's 1982 paper contained two exciting claims. One—echoing the comparable memory-network research described above—was that "the ability of large collections of neurons to perform 'computational' tasks may be in part a spontaneous collective consequence of having a large number of interacting simple neurons" (Hopfield 1982: 460). That is, intelligence could arise without the need for highly specific circuitry to be designed by evolution—or by technologists. The other was more novel, and more provocative: that "the model could be readily implemented by integrated circuits hardware" (p. 460).

It's not surprising that many people were enthused. For there was promise here for psychologists and technologists alike. Whereas Kohonen's first paper on neural nets had come back "like a boomerang", Hopfield's work was immediately acclaimed—and was soon featured in the *Los Angeles Times*.

When, only six years later, DARPA considered funding neural networks again, they would identify it as a watershed:

> From the mid-1950s to the mid-1970s, a wide variety of associative memory models were studied . . . Widespread interest in such models seems to have waned by 1975. Although many application areas were explored, these early memory models had little technological impact. The available technology favoured the use of [the digital memories used in GOFAI] . . .
>
> Hopfield rekindled widespread interest in associative memory when he proposed his 1982 energy minimization model based on the outer product storage rule. *While outer product memories had been studied extensively, Hopfield's version captured attention* because of its timeliness from a technological standpoint and the theoretical appeal of energy minimization. (DARPA 1988: 81; italics added)

Anderson agrees: "As far as public visibility goes, the modern era in neural networks dates from the publication of this paper" (J. A. Anderson and Rosenfeld 1988: 457).

The public, here, included not only the *Los Angeles Times* but also many of Hopfield's professional colleagues—a fact tartly remarked by the neurophysiologist Cowan:

> The theoretical physics community is like a swarm of locusts. There are far, far more theorists around than there are problems. There are only two or three problems, and whatever problem gets hot, a lot of them swarm onto it. That's what happened with the Hopfield paper. (J. A. Anderson and Rosenfeld 1998: 113)

Undeniably, however, the "locusts" helped advance the field. (And some stayed within it, rather than flying off to pastures new.) They had exceptionally keen mathematical skills, and a pre-existing body of theory (the physics of spin glasses) that could be applied in analysing these networks. The increase in mathematical expertise would be a lasting benefit, even if more neuroscientific data were also needed (see Chapter 14).

As for Hopfield himself, his careful analysis of the computational properties of 'his' networks was highly influential. It was more easily intelligible, for example, than Grossberg's many earlier remarks on such topics (see below, and 1.iii.h). It helped, of course, that Hopfield's paper had been published in the highly respected *Proceedings of the National Academy of Sciences*. So had some of Grossberg's (e.g. Grossberg 1971). But, characteristically, they'd covered a host of neuropsychological details that many readers would find distracting.

In addition, Hopfield himself was no shrinking violet. He "travelled all over the country talking about his results", even reaching itinerant visitors by speaking in small rooms located in major airports (Barrow 1989: 14). In other words, he was deliberately taking his ideas into the scientific marketplace (1.iii.h). As we'll see below, this was just one reason why many of his colleagues became deeply irritated by him.

But "the public" also included the many people in business, the military, and academia who hoped to gain from putting associative memories onto commercially marketable VLSI computer chips. Indeed, Mead—at the California Institute of Technology—achieved this within a few years.

By 1990, Mead had even designed silicon chips replicating a 2,500-cone human retina (Sivilotti *et al.* 1987; Mead and Mahowald 1991). These multi-layer networks—the first examples of what he termed "neuromorphic engineering"—provided not just photoreception but also various types of low-level visual processing, including automatic light adaptation. He also produced a VLSI cochlea, and a sound localizer based on the auditory brainstem (Mead 1989). Had Hopfield published the same paper ten years earlier, when VLSI was still confined to the research lab, it wouldn't have made such a splash.

All this is *not* to say that Hopfield was the first to have the core idea of energy minimization. A number of researchers interviewed by Anderson and Edward Rosenfeld (1988) for their fascinating "oral history of neural networks" insisted—with some force, not to say venom—that several people had already had much the same idea. Some had even explicitly mentioned spin glasses.

At least eight such individuals were named in these interviews, including two as far back as the 1950s. And two interviewees picked out Shun-Ichi Amari (1936–), of the University of Tokyo, as the real pioneer.

Amari had been modelling associative networks since the late 1960s. In 1977 he defined "learning based on potential" in terms of a mathematical function identical to Hopfield's (Amari 1977: 275). Although he gave only fourteen lines to this topic, he situated it within an elegant (and equally condensed) mathematical discussion of various learning rules, of the differing effects of noise, of two new "convergence theorems", and of recurrent networks in which the output is fed back as the input. Hopfield cited Amari (alongside Kohonen, Willshaw and Longuet-Higgins, Anderson, and Marr—but not Grossberg) as a precursor. But whereas Amari had given only fourteen lines to the idea, Hopfield discussed it at greater length.

Hecht-Nielsen, in these same interviews, mistakenly accused Hopfield of stealing Amari's idea *without* citing him (J. A. Anderson and Rosenfeld 1998: 301). This mistake, I assume, was an overflow from his general irritation at the attention, indeed the publicity, that Hopfield received. Hecht-Nielsen's lasting irritation, and the venom of the other interviewees' remarks, was due in part to Hopfield's relentless self-promotion, and in part to his repeated failures to cite other people's work, as a result of which he got the credit for priority.

A glaring example was his paper comparing networks composed of binary (McCulloch–Pitts) and continuously graded (sigmoid) neurones. The opening page announced "new" computational results, "unexpectedly" discovered (Hopfield and Tank 1986: 265). This appeared in the high-prestige journal *Science*, whose referees evidently didn't know that these results weren't new at all. But others did, and wrote in to complain vociferously. Most unusually, the editor agreed to publish several Technical Comments pointing out not only that Hopfield and Tank's contribution wasn't original but also that others had already taken them further.

One brief comment ended by saying drily that "The application of neural network theory to technology would be expedited by further consideration of known results," and gave a footnote citing several reviews of "early [*sic*] and recent" relevant work (Carpenter *et al.* 1987: 1227). That was for public consumption. What was said in private on the grapevine, which was part-surfacing in those oral-history interviews, was much less tactful. (So much, again, for the Legend: 1.iii.b.)

g. The power of respectability

The main reason why Hopfield's 1982 paper had attracted so much more attention than its predecessors was his reputation *as a physicist*. As Anderson has put it:

> John Hopfield is a distinguished physicist. When he talks, people listen. Theory in his hands becomes *respectable*. Neural networks became instantly legitimate, whereas before, most developments in networks had been the province of somewhat suspect psychologists and neurobiologists, or by those removed from the hot centres of scientific activity. (J. A. Anderson and Rosenfeld 1988: 457; italics added)

What can happen when someone isn't perceived as "respectable" is indicated by Werbos:

> So he [Grossberg: see Chapter 14.v.a] handed me his papers. I can honestly say, based on those papers—this was really early, like '71,'72—I know that he was talking about what we now call Hopfield nets because that's what was in those papers. He was having trouble getting it published. It may be that what had happened with Grossberg is in part what happened with me [with respect to backprop: see Section vi.d]—namely, we had the exact same idea in the exact same form [as someone else did later], but people weren't willing to publish it. He had to dance it through and change it and modify it and screw it up before people would allow it to get through the system. *And then after the screwed-up version got through, then people would allow the full form of it to come in from places that they trusted. We were not people they trusted, neither Steve nor I.* (J. A. Anderson and Rosenfeld 1998: 343; italics added)

Grossberg first wrote about these ideas—his "Additive Model"—as a freshman (at Dartmouth) in 1957, and had published "at least fifty papers on it" by the time the current label was coined. (One of those "fifty" papers was a review article, which one might expect to have been widely read: Grossberg 1974.) Naturally, he resents Hopfield's high visibility: "I don't believe that this model should be named after Hopfield. He simply didn't invent it. I did it when it was really a radical thing to do" (J. A. Anderson and Rosenfeld 1998: 172, 176).

One of the problems with priority claims is that if an idea is very new, it may not be intelligible to other people even if they do come across it. That's especially so if it's presented alongside many other novel ideas, and a range of highly disparate empirical data that the writer is seeking to explain. As remarked above, Hopfield's abstract, data-free, work was more influential than Grossberg's "equivalent" for these very reasons.

Werbos apparently understood Grossberg's ideas on first reading. But Rumelhart has confessed that he (as a research student in mathematical psychology), *and* his more senior colleagues at Stanford (including William Estes), didn't:

> [In about 1966, Grossberg] sent them a copy of his big, fat dissertation, which had all of these differential equations and everything, and this big cover letter in which he explained how he had solved all the problems in psychology. Of course, the people at the Institute for Mathematical Studies in Social Sciences were a little perplexed about what this could all be about. I remember *we spent a great deal of time and effort trying to figure it out and failed completely, I should say, to understand* what it was that Grossberg had actually done. In the end, I guess I gave up on it. (J. A. Anderson and Rosenfeld 1998: 271; italics added)

As we'll see in Chapter 14.v.a, the people at Stanford were sufficiently impressed to invite Grossberg there as a graduate student. Nevertheless, even first-rank mathematical psychologists were "perplexed".

I shan't attempt a definitive priority judgement here (Grossberg? Amari? Hopfield? . . .). As remarked in Chapter 1.iii.h, identifying "the same" idea, even in mathematical contexts, is often a highly subtle matter. *The very same equation* may be presented in two different ways, so that the 'second' presenter is understandably, and perhaps even justifiably, regarded as the 'first'. I suggested, above, that this applies in the case of Hopfield and Amari, respectively. It may well apply in the case of Hopfield and Grossberg too, but mathematical skills way beyond my own would be needed to say this with any confidence. It's undoubtedly true, however, that Hopfield was heard immediately whereas Grossberg, a quarter-century before, wasn't.

Whomever one credits with having had the idea of energy minimization first, the great advantage of Hopfield's presentation—and of his respectability—was that it attracted the "locusts". Their analytical skills helped people to understand the fundamental principles of the networks they discussed.

However, there were two problems, one sociological and one theoretical. First, most physicists knew nothing about psychology (and many couldn't have cared less). Occasionally, they looked at neuroscience: Hopfield himself had regularly attended neurobiological conferences and Princeton seminars in the 1970s. But Kohonen was exceptional in choosing psychology as a "hobby". So they couldn't simulate the experimental data, nor identify theoretically significant psychological problems on which to test out their ideas. It wouldn't have occurred to them, for example, to model the formation of the past tense (Section vi.e, below), or the visual perception of colour and form (see 14.v.d).

Second, Hopfield faced the difficulty that Selfridge had faced with Pandemonium (see ii.d, above): the danger of getting trapped in a local minimum. (*Minimum*, this time: in hill climbing the relevant quantity is increased, whereas in energy minimization it is decreased.) As remarked above, his algorithm never allowed the overall energy measure to rise, no matter what changes took place in the units. There was therefore no way in which the system could escape from a local minimum, even if a much deeper "valley" existed nearby in the state space. For Hopfield's immediate purposes (namely, storing memories), this didn't matter: all that was required was that *some* attractor be found. But if, for any reason, one wanted to locate the attractor with the lowest minimum of all, Hopfield's method would usually miss it.

As we'll see in Section vi, both these problems would be solved a few years later by Hinton and his colleagues in the PDP group—to some extent inspired by Grossberg's recent papers (especially 1976*a*,*b*, and 1980).

Between them, the members of the PDP group had many years of experience in AI modelling and/or computational psychology, drawing on both GOFAI and connectionism. For example, in the early to mid-1970s Norman had co-authored the textbook that used Pandemonium as a theoretical motif (see Figures 6.5 and 6.6), Rumelhart had developed computational theories of reading errors and story grammars, and the two had co-edited a collection on computational models (D. A. Norman and Rumelhart 1975).

Hinton's experience was especially relevant, however. For even before joining the PDP group he'd devoted several years to Hopfield's core question—and had offered a similar answer.

h. Hinton relaxes

The "core question" was how a computational system making many different local decisions could reach an internally coherent (and stable) state. Hinton, being a psychologist not a physicist, thought of it in terms of a specific example: how can a vision program simultaneously satisfy many different constraints when interpreting a line drawing? Where Hopfield would minimize overall "energy", Hinton had minimized overall "inconsistency".

Hinton first worked on this issue in the early to mid-1970s, as a student of Longuet-Higgins (in Edinburgh and then Sussex) and a colleague of Sloman and Max Clowes, both AI vision experts in the Cognitive Studies Programme at Sussex. Having been told about neural networks by his schoolfriend Inman Harvey (now a key researcher in A-Life: see 15.vi.c), he got interested in distributed memories. That's why he sought out Longuet-Higgins (who was by then working on GOFAI models of music and language). While still at Edinburgh, he decided to address some of the basic issues in the context of vision. Later, he worked as a Research Fellow on Sloman's POPEYE vision project at Sussex for a year or two (see 10.iv.b).

He started from an idea drawn from scene labelling, a method of interpreting line drawings pioneered by Clowes: the Waltz filter. This was a form of depth-first search that involved the sequential propagation of constraints (10.iv.b). However, there was a danger of what I remember Hinton calling "computational gangrene". A contradiction at any point would lead to the permanent abandonment of search below that point—so if one of the labels happened to be mistaken, the solution would be blocked. In short, noise was disastrous.

But noise is also ubiquitous. In particular, it's inescapable in grey-scale images: a light-intensity gradient in the image may or may not represent an edge in the scene. David Waltz had already added new labels for line drawings, representing shadows for instance. And Marr (1975*a*,*b*) had recently suggested a set of constraints (in effect, labels) involved in interpreting grey-scale images, corresponding to edges, textures, reflectances, and orientations (Chapter 7.v.b–d). Hinton wanted to find some way of maximizing the mutual consistency of any such set of low-level labels.

He was aiming for optimal, though not necessarily perfect, constraint satisfaction. This required parallel processing and alterable decisions, guided by a measure of inconsistency that was minimized as the system moved towards the optimum interpretation.

The first published version of such a method was called "relaxation" (Rosenfeld *et al.* 1976), and Hinton used this term in his own work (1976, 1977). His contour-finding program provisionally assigned continuous numerical values (between 0 and 1) to hypotheses about various areas in the image: that this is an edge part, for instance. If it is an edge part, then colinear edge parts are to be expected nearby. They aren't guaranteed, however. The edge part may be the endpoint of a sharp corner, or its continuation may be prevented by some other object obstructing the view—or it may not correspond to a real edge at all.

Whereas the first relaxation technique had allowed individual hypotheses to interact (support or inhibit) directly, Hinton's didn't. Instead, the logical relations *between* hypotheses were also expressed as numbers, which acted as feedback loops guiding the local decisions. The overall measure of logical consistency could be calculated, and was used to achieve the optimal overall value in the circumstances. At that point, each hypothesis would be announced "true" or "false", and contradictions (by now, minimized) would be ignored.

In other words, whereas Waltz had dealt only with *strong* constraints (which must be satisfied), Hinton focused on *weak* ones (which need be satisfied only so far as possible). That was a significant advance.

But this method, like Hopfield's, was threatened by extremity traps. And now it mattered, since the aim was to find the best *global* interpretation. Suppose the image were ambiguous—not in the sense that two alternative interpretations are equally consistent (as in the vase/faces of Figure 5.1), but in the sense that one of two plausible interpretations is better than the other. For instance, consider a photo that could be of either Jill or Mary: which is the more likely? If Hinton's system happened to reach the less consistent solution first, it would never find the other one. (Eventually, Hinton would show how to escape such traps: see Section vi.b.)

One counter-intuitive finding of Hinton's early work on vision had nothing to do with relaxation labelling. This was his proof that the most efficient way of coding for features (to recognize shapes, for example) is to use a few very coarsely tuned units, not a large number of finely tuned ones (Hinton 1981; cf. Hinton *et al.* 1986: 90–4). Overlapping groups of coarsely tuned receptive units, within specified size constraints, can compute spatial properties with specifiable gains in efficiency. So the fact that biological neurones don't respond to a perfectly defined narrow class of stimuli may be a strength, not a weakness.

As he pointed out, this is a mathematical result. As such, it applies to distributed processing in general, not just to vision.

i. Passing frustrations

If the main problems facing Dark Ages connectionism were mathematical, there were technical obstacles too. In a nutshell, the computers weren't up to it.

In the 1970s, computers were still relatively limited. Procedures requiring many cycles of simultaneous calculations weren't feasible. As a result, some good theoretical ideas couldn't be implemented by AI modellers, or only in a drastically cut-down form. And some were even ignored by them.

This applied to Holland's work on evolutionary computing, for instance. Although he'd long solved the problem that had defeated Selfridge, namely how to deal with *multiple* mutations, his theory couldn't be implemented and wasn't yet widely known (see Chapter 15.vi.a–b). Similarly, it was clear from the early 1970s that linear associative memories were promising. But they couldn't yet be used to deal with complex patterns, because large vectors need very many multiplications and additions. Connectionism couldn't flourish without much-increased computer power. This finally became available in the 1980s.

Implementation aside, however, in the wilderness years plenty had gone on behind the scenes. By the early 1980s, the actors were waiting in the wings—and their voices were beginning to be heard, at least by members of the AI community.

For example, Douglas Hofstadter (personal communication) gave a well-attended seminar at MIT in 1983–4, in which he covered a wide variety of parallelist models. These were HEARSAY, PDP (including a recent simulation of typing errors: Rumelhart and Norman 1982), simulated annealing, sparse distributed memories, and his own computer models of cognition—which included a network of nodes with spreading activation (see Section x.a, below). And Hinton's work was exciting many members of AISB in the United Kingdom, not just his erstwhile colleagues in Sussex.

Outside certain sub-groups of the cognitive science community, however, even PDP was still unknown. Moreover, connectionism in general was widely thought to have been *proven* useless (see Section iii). The money and public attention were directed elsewhere. AI was constantly featured in the media, thanks to Japan's Fifth Generation project (11.v). But it was expert systems and chess programs that were discussed, not connectionist AI. Similarly, most philosophers discussing AI, whether pro or con, considered only symbolic computation (see Chapters 11.ii.a–d and 16.iii–vi).

Hubert Dreyfus was no exception. In 1979, the very same year as the La Jolla conference, the revised edition of his book *What Computers Can't Do* contemptuously declared:

> Since those pursuing this course [the study of self-organizing systems], sometimes called cybernetics, have produced no interesting results—although their spokesman, Frank Rosenblatt, has produced some of the most fantastic promises and claims—they will not be dealt with here. (H. L. Dreyfus 1979: 130)

To make it worse, he was so confident in this dismissal that he even quoted his own arch-enemy, Minsky, in support.

The frustrated neural networkers could be forgiven for resenting this cultural invisibility—and they did. Passions ran deep, and sometimes surfaced in surprising ways. In 1981 for instance, in an otherwise highly convivial twenty-person weekend on adaptive systems organized by Selfridge, a leading cybernetician (never mind who . . .) expressed the most extraordinary personal hostility at my merely *mentioning* symbolic AI.

By the end of the 1980s, however, the shoe would be on the other foot.

12.vi. Centre-Stage

Connectionism exploded into the public consciousness for the third time in 1986. Granted, Douglas Hofstadter's hugely popular writings had already alerted his readers to the ideas of "subcognitive" parallel processing and "active" memory (see Section x.a, below). But there'd been no accessible account of *just how* this could be implemented. When one finally appeared, the fuss was even greater than had attended Rosenblatt and Widrow, or Hopfield.

It reached across the general public, from professional philosophers to the tabloid-reader on the Clapham omnibus. In the late 1980s it seemed that one could hardly turn on the television without seeing some reference to the magical new machines.

"Magical", in the popular view, because they had no clearly identifiable memory traces, because they could learn without being explicitly taught, and because some could start out from a merely random state.—And impressive, too. For these network machines really did seem to be a sort of "artificial brain". In fact, this 1950s phrase was now widely resurrected.

The type of connectionism concerned was the newly named "parallel distributed processing", or PDP for short. Localist and circuit-based connectionism were largely ignored. Many people didn't even know of their existence.

Moreover, the subtext—not to say the propaganda—changed. Previously, the focus of connectionism had been wholly positive: to discover the nature of associative memory. Now, it was largely negative: to counter GOFAI's influence on theoretical psychology and the philosophy of mind. In this context, the central message was that effortless intuitive thinking is *not*, as GOFAI had assumed, just like conscious inference but without the consciousness.

The downplaying of GOFAI was one reason for PDP's popularity with the public. At last, it seemed (but see Sections viii–ix, below), the 'dehumanizing' image of mind-as-von-Neumann-computer had got its come-uppance.

The leading connectionists themselves were less thoroughly dismissive of symbolic AI:

> It would be wrong to view distributed representations as an *alternative* to [GOFAI] representational schemes like semantic networks or production systems . . . It is more fruitful to view them as one way of implementing these more abstract schemes in parallel networks, but with one proviso: Distributed representations give rise to some powerful and unexpected emergent properties. These properties can therefore be taken as primitives when working in a more abstract formalism. [Examples are] content-addressable memory, automatic generalization, and the selection of the rule that best fits the current situation. (Rumelhart, McClelland, *et al.* 1986: 78)

But those wise words were largely ignored. Most outsiders, and many insiders too, evidently felt the humbling of GOFAI to be far too satisfying to pay attention to them. (Some exceptions are discussed in Sections viii–ix, and especially ix.b.)

a. The bible in two volumes

The new name, and the sudden surge of interest, arose with the publication of the two-volume PDP bible in the autumn of 1986 (Rumelhart, McClelland, *et al.* 1986; McClelland, Rumelhart, *et al.* 1986). This was PDP's manifesto, a clarion call to join the new movement.

Unlike the cognitive science manifesto (*Plans and the Structure of Behavior*: see 6.iv.c), its hand-waving was relatively restrained. Most pages reported work that had actually been done—some (in volume I) defining general methods, some (in volume II) applying those methods to specific topics. This made the argument more persuasive than an informal account would have been, but it also made for fairly dry reading. Fortunately, many readers had been prepared—and hugely excited—already. A Pulitzer-prizewinning book published seven years earlier had introduced the *idea* of distributed processing by a host of memorable, and intuitively intelligible, examples (see x.a, below). Now, the bible was adding mechanism to idea.

Like Norman and Rumelhart's GOFAI collection *Explorations in Cognition* (1975) and Minsky's *Semantic Information Processing* (1968), the PDP bible was a collection of papers largely written by research students or only-just-postdocs. If that sounds boring, it wasn't: these two volumes were even more influential than their GOFAI predecessors.

The editors were Rumelhart and James, or Jay, McClelland (1948–) at UC San Diego. (Almost immediately after the bible was published, Rumelhart moved to Stanford, where he worked from 1987 to 1998.) Hinton had originally been an editor too, but withdrew to concentrate on the Boltzmann machine. He hadn't been a mere dogsbody editor: Terrence Sejnowski described him, years later, as "the seed that led to PDP" (J. A. Anderson and Rosenfeld 1998: 323).

Further members of "the PDP Research Group" were officially based both in and outside San Diego. They included (among others) Norman, Sejnowski, Ronald Williams, Paul Smolensky, Jeffrey Elman, Michael Jordan, and Francis Crick.

Crick's name implies an interest in biology, and the PDP bible did contain several neuroscientific papers—one of which stressed the very *un-neural* character of artificial networks (Crick and Asanuma 1986) (see Chapter 14.i). The book even declared that the PDP group accepted "the brain metaphor" in place of "the computer metaphor". But although the (limited) biological plausibility "definitely enhanced" their approach, the neuroscience was strictly secondary:

> We are, after all, cognitive scientists, and PDP models appeal to us for psychological and computational reasons. They hold out the hope of offering *computationally sufficient* and *psychologically accurate* mechanistic accounts of the phenomena of human cognition which have eluded successful explication in *conventional computational formalisms* [i.e. GOFAI]; and they have radically altered the way we think about the time-course of processing, *the nature of representation*, and the *mechanisms of learning*. (Rumelhart, McClelland, *et al.* 1986: 11; italics added)

The italicized phrases are the keys to why other cognitive scientists were so interested. *Computational sufficiency* seemed to be provided by the Boltzmann machine and back propagation (see below). These alleviated two of the major mathematical difficulties of the Dark Ages.

Psychological accuracy was the aim of PDP models of experimental data, some of which had great theoretical interest. For instance, the past-tense learner described below challenged Chomsky's nativism (Chapters 7.vi.a and 9.vii). More broadly, the PDP perceptrons—unlike Rosenblatt's simple versions—allowed a role for success. There was "extensive experimental evidence" suggesting that "learning when the organism is correct usually appears to be more important than learning when a mistake is made" (Hinton and Anderson 1981: 15).

The criticism of *conventional computational formalisms* cast doubt not only on the psychological relevance of GOFAI, but also on philosophies of mind that shared the same conceptual roots (see 16.iii–iv). The suggestion that *representation* could be thought of in a new way was exciting not only for psychologists but also for philosophers (Sections viii–x, below). And the notion—not new, but resurrected—that machines could *learn* without being explicitly taught added the promise of commercial applications.

Having these intellectual delights to offer, plus the increased computer power of the 1980s that made persuasive demonstrations possible, the PDP group had been hopeful about prompting interest in connectionism. However, they realized that this would need some special effort on their part.

Their report on the 1979 conference had kicked off with the warning: "This is a difficult book" (Hinton and Anderson 1981: 1). It was indeed—not least because state vectors specifying holistic activity patterns were a mathematical nightmare for many people. They were unfamiliar to non-physicists, and very different from GOFAI instructions in LISP's 'English' (10.v.c). Now, they decided to provide not only a research report but a tutorial too.

The PDP bible was intended to fulfil both functions. Because they especially wanted to get the youngsters interested, they wrote it in an unusually accessible way. Moreover, they reduced the cost by doing all the typesetting and proof-reading themselves, and even paying for the preparation of the camera-ready copy. They hoped to have well-thumbed, coffee-stained, volumes on students' desks—not pristine pages sitting on library shelves.

But even they were amazed by the scale and immediacy of their success. There were so many advance orders for the PDP books that the second printing was started even before the first had been released. Forty thousand copies were sold in the first seven years.

The scale of the advance orders suggests that the rumour-mill in the final years of the Dark Ages had primed many people to be receptive. Sejnowski recalls that even a few months before publication "very few people knew what was happening by word of mouth" (J. A. Anderson and Rosenfeld 1998: 324). Nevertheless, enthusiastic students "from everywhere" turned up at the pre-publication Connectionist Summer School he and Hinton organized at CMU. Some may have been drawn in by Hinton's easy-to-read description of Boltzmann machines in the popular magazine *Byte*, in 1985. And some may even have found out about it when "somebody at MIT put out a spoof [announcement] advertising a connectionist cooking summer school" (J. A. Anderson and Rosenfeld 1998: 324).

Primed or not, what readers found on opening the books was a fascinating mix of abstract theory and psychologically significant models. As Rumelhart recalled later, the former had had a long gestation:

> [In the late 1970s, Jay McClelland and I developed our interactive model of word recognition that] turned out to very much like these models that settle into stable states. Indeed, it was those features that we eventually worked out. But what I remember are hours and hours and hours of tinkering on the computer. *We sat down and did all this in the computer and built these computer models, and we just didn't understand them. We didn't understand why they worked or why they didn't work or what was critical about them. We had no important theory yet.* We struggled and

struggled and began to get insights into the nature of these interacting systems and became totally obsessed by them. (J. A. Anderson and Rosenfeld 1998: 275; italics added)

The "important theory" they'd been hoping for was eventually achieved. It included, for instance:

* new methods of analysing hidden units (see below);
* Hinton's proof about the efficiency of coarsely tuned units (Section iv.h);
* McClelland's recasting of GOFAI's HEARSAY architecture in PDP terms (Section iv.b);
* and Smolensky's harmony theory.

But the most exciting theoretical advances, and the ones that received the most attention, were

* the Boltzmann machine, and
* back propagation.

b. Bowled over by Boltzmann

Boltzmann machines were announced informally in 1984, in a Carnegie Mellon Technical Report. This was written by Hinton (then at CMU) and the CMU computer scientist David Ackley, together with Sejnowski—an ex-physicist turned neurobiologist, whom Hinton had met at the 1979 La Jolla function.

Sejnowski (1947–) was first drawn to neurobiology by Charles Gross's research on feature-detectors (14.iii.b), and had been won over (in 1978) by studying electroreception in fish:

[Seeing the molecular receptors with the electron microscope] changed my life. After that I wasn't interested anymore in just abstract understanding of the brain. I really wanted to understand how it was made. I was committed to the idea that you had to understand the actual substance that the brain was made from if you're going to understand how it works. (J. A. Anderson and Rosenfeld 1998: 321)

By the mid-1990s, his prime interest would be "using techniques and tools from network modelling to understand different pieces of the brain" (J. A. Anderson and Rosenfeld 1998: 329). In the early 1980s, however, he was still doing 'abstract' connectionism. There was no neuroscience in NETtalk, for instance (see subsection f, below)—nor in Boltzmann machines.

The title of the CMU report described these machines as "constraint satisfaction networks that learn", so highlighting the topic that Hinton had been researching for a decade (see Section v.h, above). But three new features had been added. These were energy minimization, simulated annealing, and learning.

The first of these was borrowed from Hopfield. In the summer of 1982, Hinton and Sejnowski—already cooperating on relaxation models of vision—attended a small meeting of connectionists at the University of Rochester. There, they heard Hopfield give a talk on his about-to-be-published work. As Hinton remembers it,

As soon as Hopfield gave that talk, I realized that what you wanted to use these nets for was to solve constraint-satisfaction problems. You wanted to let the energy function correspond to the objective function you were minimizing. I'm not sure Hopfield had really understood that. He was using these nets for memories at that point, 1982. (J. A. Anderson and Rosenfeld 1998: 372)

In other words, "energy" could be made to stand for "inconsistency". And Sejnowski recalls:

Both Geoff and I within seconds realized that this was the convergence proof we needed to show that our constraint-satisfaction scheme in vision could actually be implemented with hardware. That's how the Boltzmann machine was born. (J. A. Anderson and Rosenfeld 1998: 322)

But translating relaxation into energy minimization wouldn't be enough: the problem of local minima would remain. It was Sejnowski who saw the way out.

He recalled some recent work by Scott Kirkpatrick (at IBM) on optimization by simulated annealing (Kirkpatrick *et al.* 1983). As long ago as 1953, physical chemists had used the Boltzmann equations of statistical mechanics in a method (the Metropolis algorithm) that modelled collections of atoms in equilibrium at various temperatures. Kirkpatrick had recently broadened the notion of equilibrium to cover optimization in general, including computer circuitry design and the travelling salesman problem (i.e. *How can one find the shortest route for visiting several scattered towns?*). Moreover, he had added simulated annealing (see below) to find the equilibrium more quickly.

Now, Sejnowski suggested doing the same thing. The Boltzmann machine "was the result of a week of solid thinking about what would happen if we had a noisy Hopfield network" (J. A. Anderson and Rosenfeld 1998: 322).

Annealing is a technique used by metallurgists to cool metals evenly. The quickest way is to start off at a high temperature, and cool the metal down gradually. Analogously, in Hinton and Sejnowski's networks equilibrium was reached by allowing the changes in the first few cycles to be relatively large ('noisy'), becoming smaller as time went on (Ackley *et al.* 1985; Hinton and Sejnowski 1986). This made it unlikely that the system would be trapped in a sub-optimal attractor.

Hopfield would have left it at that. Indeed, Hopfield would have spared the words and gone to town on the equations. To him, and to his fellow "locusts", the mathematics would be obvious. Kirkpatrick had offered more explanatory text than Hopfield. But he too had addressed his paper to people familiar with the mathematics: namely, computer scientists and engineers. The PDP group were aiming for a much wider audience and, thanks largely to their rhetorical skills, they reached it.

For example, in the PDP book (though not in their paper for fellow professionals, in *Cognitive Science*) Hinton and Sejnowski took pains to describe simulated annealing in intuitive terms. They imagined a ball-bearing on a two-dimensional landscape, and asked how one should shake the whole system to get it to lie still at the bottom of the deepest valley (see Figure 12.3).

Violent shaking, they pointed out, wouldn't do. For the ball-bearing would constantly jump from one valley to another—often, upwards. Gentle shaking would do the trick—but it might take a very long time. "A good compromise", they said, "is to start by shaking hard and gradually shake more and more gently" (Hinton and Sejnowski 1986: 288). With that strategy, the ball-bearing would probably find the bottom point and stay there.

Admittedly, in a landscape with hills and valleys at sufficiently awkward heights and locations, it might not. But they remarked that interesting problems have many

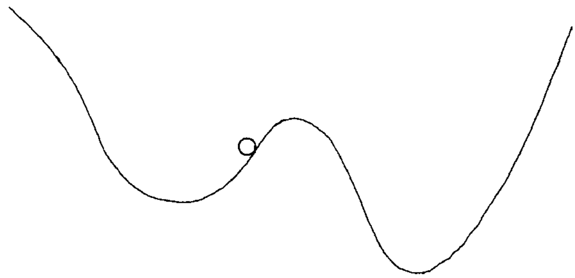


Fig. 12.3. Ball-bearing on an (energy-)landscape. Reprinted with permission from Rumelhart and McClelland (1986: 287)

dimensions, which implies many more potential pathways between one valley and another—especially when one shakes hard. (They predicted that simulated annealing would work well only for high-dimensional state-spaces.)

They spelt these things out because their intended audience was unfamiliar with neural networks, and perhaps even with physics. Hopfield's fellow locusts were very welcome to read the books too, but they weren't the primary target. The PDP bible was an educational enterprise aimed at cognitive scientists in general—and, as such, it worked. It even reached the more intrepid members of press and public.

The Boltzmann machine was—dare one say it?—a *general* problem-solver. Given suitable input units, the system could discover the appropriate consistency relations for itself. Or they could be provided beforehand, in the connection weights.

For example, the authors pointed out that one could use a Boltzmann machine for visual interpretation (Hinton's long-standing interest). The input units would fire when the relevant features (light-intensity gradients, texture cues, etc.) were present, and the weights would ensure that optimal equilibrium corresponded to the correct interpretation (cf. Ballard *et al.* 1983).

In principle, any 'environment' of interacting constraints—visual, linguistic, logical, financial . . . —could be implemented. So this approach could be applied to many issues in theoretical psychology. What's more, it could be applied to many practical problems in industry and commerce.

These networks were multi-layer perceptrons, whose units were not deterministic (as in Hopfield nets) but stochastic. The relative probabilities of the possible states of the system, and of the activity of the hidden units in different contexts, were defined by Boltzmann equations. And where Hopfield nets had modelled memory but not learning, the Boltzmann machine modelled both. The network was said to have learnt a pattern when the probabilities in the net matched those in the environment. In reaching this match, the system would in effect have identified (mimicked) the higher-order structure of the environment—which need not have been known beforehand by the programmer.

The new learning rule which brought about this match was simple (though slow), but far from obvious. It estimated and compared the probabilities of co-excitation of unit pairs in two equilibrated situations: with the environmental input present, and in the free-running state. ("Having the input present" meant clamping the input and output units into the appropriate states, before allowing equilibration across the hidden units.) Using these statistics, it gradually altered the connection weights so that the output layer would spontaneously adopt the relevant activity pattern even without any input, and complete the pattern if only part was provided.

The equilibria were reached by simulated annealing. In principle, the learning rule could have involved annealing too, but Hinton and Sejnowski couldn't implement that. However, the noise in the probability measures sometimes enabled it to escape from a local minimum anyway.

The Boltzmann learning algorithm provided what Minsky and Papert had asked for in 1969: a powerful convergence theorem *guaranteeing* successful learning in multi-layer nets (see Section iii.b). This was the aspect of their work of which Hinton and Sejnowski were most proud:

> Perhaps the most interesting aspect of the Boltzmann Machine formulation is that it leads to a *domain-independent* learning algorithm that modifies the connection strengths between units in such a way that the whole network develops *an internal model which captures the underlying structure of its environment* . . . [When a network with hidden units] does the wrong thing it appears to be impossible to decide which of the many connection strengths is at fault. *This "credit-assignment" problem was what led to the demise of perceptrons* [Minsky and Papert 1969; Rosenblatt 1962]. The perceptron convergence theorem . . . could not be generalized to networks of [decision] units when the task did not directly specify how to use all the units in the network.
>
> This version of the credit-assignment problem can be solved within the Boltzmann Machine formulation. (Ackley *et al.* 1985: 641; italics added)

The underlying reason for this guarantee was that the Boltzmann equations in physics guarantee thermal equilibrium. Some day, sun and moon and everything else will have the same temperature—but there's a very long time to wait. Analogously, this new convergence theorem (like Rosenblatt's) showed what's inevitable in principle, not what's feasible in practice. Even if one could be sure that one was dealing with a problem for which a satisfactory set of weights in principle exists, one couldn't be sure of finding them.

In practice, the learning rule was very slow, and useless for large networks. That's why, despite the huge interest aroused on publication, relatively few people besides the authors themselves actually used Boltzmann machines for psychological research (but see Plaut and Shallice 1993). As Hinton and Sejnowski had pointed out, even small networks could be problematic:

> [One] difficulty is that there is nothing to prevent the learning algorithm from generating very large weights which create such high energy barriers that the network cannot reach equilibrium in the allotted time. Once this happens, the statistics that are collected will not be the equilibrium statistics required for [the relevant equation] to hold and so all bets are off. We have observed this happening for a number of different networks. They start off learning quite well and then the weights become too large and the network "goes sour"—its performance deteriorates dramatically. (Hinton and Sejnowski 1986: 298)

There were other difficulties, too—not least, that more training would make the network "go sour", its performance getting worse and worse.

In brief: Boltzmann machines were mathematical magic. But they cast their spells unreliably in the real computational world.

c. Backprop hits the headlines

A more trusty piece of mathematical magic announced in the PDP volumes (and simultaneously in the high-prestige *Nature*) was back propagation (Rumelhart *et al.* 1986*a*,*b*). This was the idea which Widrow "would have given [his] eye teeth to come up with" (see v.d, above).

Backprop, too, solved the credit assignment problem for multi-layer networks, and although it was still slow it was much faster than the Boltzmann machine. (In the ensuing years, a lot of effort was put into speeding it up.) Although it couldn't be *proved* to converge, it was certainly applicable to a wide range of problems. At the end of the 1980s, it was judged to be "the most effective current learning algorithm for complex, multilayer systems" (J. A. Anderson and Rosenfeld 1988: 674).

Back propagation was a generalization of the long-familiar LMS algorithm, or "delta rule" (see ii.g, above). But it corrected errors on several levels, instead of just one. It did this by tracing responsibility back from the output layer into the hidden layers, identifying the individual units that needed to be adapted *even though* many units had been acting simultaneously. It was thus analogous to Holland's contemporaneous bucket-brigade algorithm (Chapter 15.vi.a).

Backprop requires the programmer to know the precise state of the output layer when the network is giving the right answer. A unit-by-unit comparison is then made between this exemplary output and the output actually obtained from the network being trained. Any difference between an output unit's activity in the two cases counts as an error. (Unlike the Boltzmann machine, the units here were deterministic.)

So far, so familiar. But the (current or desired) state of the hidden units isn't known, for they can't be directly inspected. The backprop algorithm therefore *assumes* that the error in an output unit is due to error(s) in the hidden units connected to it.

It attributes a specific amount of error to each hidden unit, depending on the connection weight between it and the output unit. Blame is shared between all the hidden units connected to the mistaken output unit. If a hidden unit is linked to several output units, its mini-blames are summed.

Proportional weight changes are then made to the connections between the hidden layer and the preceding layer. That layer may be another (and another . . .) stratum of hidden units. But ultimately it will be the input layer, and the weight changes will stop. This process is iterated until the discrepancies at the output layer disappear.

The resulting overall state of the hidden units was taken to be the "internal representation" of the input. Such representations were necessary, the authors said, whenever "the similarity structure of the input and output patterns are very different" (Rumelhart *et al.* 1986*a*: 318). "A classic example of this case", they pointed out, is the *exclusive-or* (XOR) problem, which single-layer perceptrons had been notoriously unable to solve (see Section iii.b). Pandemonium could solve it, because (as pointed out in Section ii.d) it could embody demons looking out for just that logical feature. But there had been no learning rule—no convergence theorem—that *guaranteed* success in this general class. Backprop, by contrast, did.

Backprop aroused enormous interest. The need for it had long been recognized:

* Rosenblatt himself had speculated about "back-coupled" perceptrons twenty years earlier, but he hadn't defined an algorithm for computing and communicating the error messages (see Section ii.e, above).
* His arch-critic Minsky, in his hugely influential AI prospectus of the same period (1961*b*), had named credit assignment as one of the key problems that would have to be solved (10.i.g).
* And Widrow had "hit a brick wall trying to adapt multilayer nets" in the early 1960s, largely because he couldn't solve it.

Now, someone had mastered it at last.

Those words "at last", however, are highly misleading. The adulation—not to mention Widrow's eye teeth—might have been offered long before. For backprop wasn't strictly new. Quite apart from Holland's analogous work, it had been independently discovered several times already.

d. Backprop anticipated

Even within the PDP group, backprop had been several years a-brewing. Rumelhart had prepared a talk on it for a meeting in 1983, but chose "at the last minute" to give another talk instead (J. A. Anderson and Rosenfeld 1998: 279).

A full year later, he mentioned the idea to Sejnowski. He "immediately understood" and was the first to use it for a large task (in NETtalk: see below). Rumelhart himself had attempted only toy problems.

When Rumelhart first mentioned it to Hinton, by contrast, Hinton declared that it could never work. Nevertheless, and despairing of getting the Boltzmann machine to work efficiently, a few months later Hinton thought he might as well try backprop. His research students were so sceptical that none was willing to program it, even though he assured them that "It would only take a day or so" (J. A. Anderson and Rosenfeld 1998: 377). So he (with Williams) implemented it himself, and then spent some months improving it. Their description of the 'new' technique was added as an extra chapter just as the PDP volumes were going to press. This was a full three years after Rumelhart's phantom talk.

Even more to the point, there were others outside the PDP group who had come up with much the same idea.

The PDP authors themselves cited two "similar" learning rules. These had been published very recently (in 1985) by David Parker and Yann le Cun. Indeed, in 1982 Parker had tried to get it patented by Stanford, but was rejected because they thought it had no commercial value (S. Grossberg, personal communication).

Moreover, several people had defined it long before that. Grossberg had done so, as we saw in Chapter 1.iii.h (and his then current version allowed for many brain/behaviour facts ignored by the PDP group—Grossberg 1987*a*: 232–40). Several non-biologists had done so too. Its mathematical core (the chain rule of differentiation) had been discussed in the 1960s by control theorists (Bryson and Ho 1969), and by Amari also (1967). And, even more significant, it had been *implemented* in the early 1970s, as a routine statistical technique for automatic calculation.

The person who did that, and who (so far as is known) was the true originator of backprop *as a computerized algorithm*, was Werbos (1947–). And he did it for the most surprising of reasons.

Werbos was a highly interdisciplinary animal: a mathematician interested in the intersection of psychology, economics, and political science. While a new Ph.D. student at Harvard in 1970 or so, he had suggested using "this little method for adapting multilayer perceptrons". But he didn't want to use it for modelling supervised learning. Rather, he wanted to translate Sigmund Freud's ideas on psychic energy into mathematics.

Werbos's overall aim had been to model intelligence. His elders and betters weren't impressed:

> I had an adaptive critic backpropagated to a critic, the whole thing, in '71 or '72. I actually mailed out copies of that paper to some people out at Stanford and certain people around Harvard.
>
> The thesis committee said, "We were skeptical before [i.e. when he had mentioned the mathematics but not the AI], but this is just unacceptable. This is crazy, this is megalomaniac, this is nutzoid." (Werbos, interviewed in J. A. Anderson and Rosenfeld 1998: 342)

Even allowing for the verbal embroidery, Werbos's frustration (remember "We were not people they trusted, neither Steve nor I": v.g, above) is understandable. Not until he'd presented his ideas as statistics rather than neural networks, and as applied political science rather than AI, did Harvard (in 1974) agree to pass the thesis. The blandly respectable title was *Beyond Regression: New Tools for Prediction and Analysis in the Behavioral Sciences*—but Werbos used the ideas to make some highly influential predictions about the conduct of the Vietnam war (J. A. Anderson and Rosenfeld 1998: 339–40, 349).

Meanwhile, finding himself penniless, Werbos had got a full-time job on a joint MIT–Harvard computing project. This sustained him while he was doing the Ph.D. He generalized his algorithm to deal with time-varying processes, and put it into the "Time Series Processor" as part of MIT's standard software. Besides being included in the official report to the military funding agency, a description of the algorithm (which he called dynamic feedback) sat on many people's desks: "It was part of the computer manual, so the first publication of backpropagation was in a computer manual from MIT for a working command for people to use in statistical analysis. I was a second author of that manual" (J. A. Anderson and Rosenfeld 1998: 348).

Computer manuals, of course, aren't high-profile publications. DARPA reports are. But—most unusually—Werbos's mid-1970s research report to DARPA containing details of his method was never published, nor even officially listed. The reason was that it also contained politically embarrassing results (J. A. Anderson and Rosenfeld 1998: 351).

Moreover, Werbos's 1978 paper (in an IEEE journal) announcing that he'd defined a powerful method of "dynamic feedback" was printed without the Appendix, because of length limits. Without the details, why should readers get excited, or even believe him?

Another publication, in 1982, even added discussions of neural nets and various parts of the brain. It provided diagrams of multi-layer perceptrons, doing reinforcement learning by back propagation. But although this paper got Werbos some job offers, backprop was still left on the shelf. His new employers tried to get MIT's engineers to implement it as a useful analytical tool: they wouldn't.

So much for the vicissitudes of Werbos's dynamic feedback. The sources for Parker and le Cun were similarly inaccessible. They were (respectively) a Technical Report of MIT's Center for Computational Research in Economics and Management Science, and a low-profile journal and edited collection.

Rumelhart and colleagues, by contrast, published simultaneously in the hugely influential journal *Nature* and (at much greater length) in a book written in a highly accessible style, with a provocative overall theme. That book was an instant best-seller, as we've seen. And everyone who was a scientific anyone read *Nature*. Their version of back propagation became famous virtually overnight. Until Werbos was remembered/discovered by the neural network community some years later, the PDP group were mistakenly given the credit for defining and/or implementing the "first" backprop algorithm.

e. Wonders of the past tense

There was another reason, besides the elegant mathematics, why the PDP approach became famous overnight. Namely, the 'bible' described simulations of a number of important psychological phenomena. In other words, it seemed to be describing the real world, not just doing exercises in fancy mathematics. And the "real world", here, was the world of human beings—not spin glasses.

Perhaps the most widely read chapter in this category was Rumelhart and McClelland's (1986). It described a Kohonen associative memory (with 460 input units and 460 output units), equipped with Rosenblatt's perceptron convergence procedure. This network modelled how a child learns the past tense of English verbs—which most of the bible readers, of course, would have heard in their own infant children or siblings.

Outsiders (and locusts) might have asked "Who cares? What's important about that?" But cognitive scientists did care, because this model presented a fundamental challenge to two closely related orthodoxies: nativism and rule realism. (By rule realism, I mean commitment to the psychological reality of processing rules, of which grammatical rules are a special case.)

Both of these orthodoxies were prominent in Chomsky's linguistics and Fodor's philosophy of mind (see 9.vii and 16.iv.c–d). And the second was a linchpin of Allen Newell and Simon's psychology (6.iii, 7.iv.b, and 16.ix.b). These four men had been hugely influential in the formation of the field. So it's not surprising that nativism and rule realism were accepted, even assumed, by most cognitive scientists at the time.

Rumelhart and McClelland questioned all that:

> We propose an alternative to explicit inaccessible rules. We suggest that lawful behavior and judgments may be produced by a mechanism in which there is no explicit representation of the rule. Instead, we suggest that the mechanisms that process language and make judgments of grammaticality are constructed in such a way that *their performance is characterizable by rules, but that the rules themselves are not written in explicit form anywhere in the mechanism*. (Rumelhart and McClelland 1986: 217; italics added)

The italicized claim had been made years earlier by Dreyfus, as we saw in Chapter 11.ii.a. But whereas he couldn't say anything positive about what such mechanisms might be like, beyond a vague reference to "bodily skills", Rumelhart and McClelland could.

They even claimed to have demonstrated success. If they were to be believed, Chomsky, Fodor, Newell, Simon, and almost everyone else in cognitive science were barking up the wrong tree.

Rumelhart and McClelland started from the fact remarked in Chapter 7.vi.a, that young children go through three stages in forming the past tense. First, a few past-tense verbs, and no mistakes; then, more past tenses, plus frequent over-regularization (*he goed* or *he wented* instead of *he went*); finally, lots of past-tense verbs, without mistakes. This development is gradual, and also includes more detailed changes—in the relative frequencies of *he goed* and *he wented*, for instance.

Their PDP model apparently produced the very same sort of behaviour. ("Apparently", because this claim was later challenged: see Section x.d, below.) But it was neither provided with, nor did it develop for itself, any explicit rules about past-tense formation. The patterns in its behaviour were based in memories (sets of connection weights) implicit in the network as a whole—and which reflected the statistical structure of its environment.

(Sets of connection weights were often referred to as "memories", largely in order to stress the radical difference between PDP and GOFAI representations. However, that could be misleading, because the word normally suggests something that is closely related to the inputs presented—which the weight sets produced by PDP learning rules are not. In some connectionist systems, inputs—sentences, for instance—are stored verbatim, and processing is done on the collection of examples when the system is presented with a query.)

Here, the "environment" was the language that the system—child or computer—experienced as input. The input to the computer wasn't continuous speech. Rather, it was (phonological representations of) a set of stem/past-tense pairs.

Rumelhart and McClelland claimed that the number of recognizable past-tense verbs heard by the infant is at first so small that there's little scope for spotting statistical regularities. Moreover, many everyday (i.e. frequent) words (*go, come, give, take, get* . . .) are irregulars, so the very early corpus includes a relatively large proportion of past-irregulars, such as *he went*. So the first input they provided consisted of only ten verbs: eight irregular, two regular; later, another 410 would be added. Present–past pairs (for instance, *go–went*) are evidently learnt by rote, as one-offs. As more speech is heard, however, the regular verbs massively outnumber the irregular ones.

In other words, the addition of *-ed* (and other past-tense rules) is increasingly prominent in the input statistics. The PDP model—and, its authors suggested, the child—picked up this statistical fact, and reflected it in its own performance. Only much later, after numerous repetitions of *went* had made it a statistically recognizable exception, did the network generate it reliably again.

The past-tense learner was theoretical dynamite:

* It cast doubt on nativism and modularity, for it suggested that a domain-general learning rule—namely, Rosenblatt's perceptron convergence procedure—can suffice for the acquisition of grammar.
* And if grammar, why not other cognitive structures too? (Twenty years later, McClelland would argue much the same point in respect of semantic categories such as *animal* and *plant*, which are often described as innate: see Chapter 8.i.d.)

* It undermined belief in the psychological reality of explicitly represented processing rules,
* and of explicit (symbolic) representations.
* And it threatened the then popular forms of philosophical functionalism (Chapter 16.iii–v).

Little wonder, then, that people flocked to read Rumelhart and McClelland's chapter.

f. Escaping from the black box

Very soon after the past-tense learner, other 'sexy' psychological simulations were described, and hit the media in their turn. Among the most visible—or perhaps one should say "audible"—was NETtalk (Sejnowski and Rosenberg 1986, 1987).

This network, which used phonetic representations that could be fed into a speech synthesizer, gradually learnt to pronounce English words. Listening to its increasingly human-like output, one was reminded irresistibly, and eerily, of an infant passing from babbling to speech. In short, it was guaranteed to *épater les bourgeois*—and in 1986 it did so, on the *Today* show.

But it captured the attention of cognitive scientists, too. For it addressed a methodological problem that faced PDP in general: namely, how to analyse the behaviour of a complex network, instead of treating it as an unintelligible black box. (Compare Minsky's confession "we sort of quit science for a while to watch the machine", and Rumelhart's: "[We'd] built these computer models, and we just didn't understand them . . . why they worked or why they didn't work or what was critical about them.")

Of course, one could always specify the learning rule being used. But what, exactly, had been learnt? Or, to put it another way, what exactly were the hidden units doing? How, for example, was the network representing the hard 'c' in 'cat' and the soft 'c' in 'city'?

NETtalk was developed at Johns Hopkins University in 1985–7, by Sejnowski and the psychologist Charles Rosenberg. Using backprop, it learnt the correct pronunciation of printed English words (from a 20,000-word dictionary) after only three months of work on their part. Indeed, to their amazement it took only one day to master the very first (100-word) version of the task. Three months later, it could learn a 2,000-word corpus in the same time. But just how it was doing it, and just how one might find out, were mysteries.

The inputs weren't letters or sounds, but phonetic representations called Wickelgren features. These are trios coding the target phoneme and its immediate predecessor and successor. So NETtalk didn't have to learn to distinguish the many English vowel sounds, for instance. Much of the power of the system was due to this choice of representation.

That fact illustrated an important point about PDP applications as a class. Just as GOFAI's "General" Problem Solver relied on the programmer to choose and articulate the problem, so a "general" connectionist learning rule works on a representation selected by the designer. One can't simply take a problem and set the learning rule to run on it, any more than one can state the missionaries and cannibals puzzle in English and expect GPS to solve it. This had been true ever since Rosenblatt's first perceptron, but—perhaps partly because "knowledge representation" was a prime focus of the rival, GOFAI—it wasn't much discussed.

Though small by today's standards, the network was very large for its time. It had 309 units, 18,629 connections, and about 30,000 weights. There were 26 output units, 80 hidden units, and 203 input units. Given this complexity, explaining its final performance—as opposed to stating the learning rule—wasn't easy. It took the designers not three months, but three years.

In the end, Sejnowski and Rosenberg recommended three different methods for analysing NETtalk's behaviour in terms of what the hidden units were doing. These analyses were based on network pathology, records of unit activation, and cluster analysis.

Network pathology involved systematic "lesioning" of the network, either before or after it had learnt its task. The authors suggested that this method could be used to throw light not only on artificial networks but on clinical neurology too. For instance, NETtalk might help in studying acquired dyslexias.

Soon afterwards, the British clinician Timothy Shallice (one of the dyslexia experts they'd cited) worked with Hinton on a connectionist model of reading, parts of which they deliberately disabled in a variety of ways. Forty stimulus words, from five semantic classes, activated twenty-eight (position-specific) letter-detecting units; in addition, sixty-eight semantic units represented features such as size, texture, typical location, use, and so on. On average, each input word was supposed to activate fifteen semantic units, with much overlap between words of similar meaning (such as *cot* and *bed*) and less between semantically different words (such as *cot* and *cat*). Hinton and Shallice found that the system exhibited a range of "dyslexias" that are qualitatively similar to those seen in human patients. Various neuroscientific hypotheses were suggested, accordingly (Hinton and Shallice 1989/1991).

Unit activation, they suggested, was related to neuroscience too. After learning, any one of NETtalk's hidden units seemed to be responding to several different input characters, so had no obvious coding or single interpretation. A few years later, follow-up studies found that some units responded more strongly to certain phonetic classes (vowels, for example), but that strict localization didn't develop (J. A. Anderson and Rosenfeld 1988: 662).

As for cluster analysis, this identified the associations most often used by the system in partitioning the "space" of its performance. It turned out—to the authors' surprise—that the network had developed an implicit hierarchy of (nearly eighty) distinctions like those explicitly defined in phonetics: vowels and consonants, voiced and unvoiced consonants, and so on. A GOFAI speech system might have employed an explicit representation of this phonetic structure, but NETtalk didn't. Rather, it implicitly reflected the statistical structure of its environment (English words, each one input as a series of trio-phonetic representations). And this, as we've seen, is what the past-tense learner had done, too.

Statisticians (such as Werbos) might have accused them of reinventing the wheel, for cluster analysis is a common method in statistics. Indeed, it became increasingly clear through the 1990s that many 'insights' of connectionism were differently named versions of statistical techniques. (And of behaviourist equations too: we saw in Section v.a that the Widrow–Hoff algorithm was equivalent to Hull's "habit strength".)

This was pointed out by Donald Michie, a long-time worker in AI learning—mostly of the GOFAI variety (Michie *et al.* 1994). It was also remarked by an outsider, Warren Sarle of the SAS Institute, North Carolina (Sarle 1995).

SAS produces software for statistics, artificial neural networks, data mining, etc. It's not surprising, then, that Sarle remarked that networks learning from noisy data are doing data analysis—which is what statistics is all about. Neural net researchers concentrate on the algorithms, statisticians on their results. And neural networkers tend to ignore the distributional assumptions they have made, whereas statisticians explore their consequences. Nevertheless, there are many close parallels.

Sarle listed over seventy near-equivalences between connectionist and statistical concepts. Let's pick out just two examples. First, feed-forward nets with one hidden layer are closely related to what statisticians call projection pursuit regression. And second, probabilistic neural nets are identical to what's called kernel discriminant analysis. In general, he said,

> many results from the statistical theory of nonlinear models apply directly to feedforward nets, and the methods that are commonly used for fitting nonlinear models . . . can be used to train feedforward nets. (Sarle 1995)

Sarle's analysis confirms Gerd Gigerenzer's (1994) view that "new ideas" in science commonly arise from pre-existing tools, and that experimental psychology in general has long exploited statistics in this way. It also confirms the central claim of this section and the preceding one: that connectionism needed mathematical advances to progress. The growth in computer power helped. Indeed, it was essential. But it would have been useless without the mathematical *ideas*.

12.vii. The Worm Turns

The explosive response to the PDP bible, and to the first summer schools at CMU and San Diego, revived the flagging fortunes of connectionism. It wasn't just intellectual appreciation that was involved, but financial support too.

a. Joyful jamborees

Much as a family gathering can be all the more joyous if it brings together people long parted, so an intellectual meeting will be all the more appreciated if it unites people from many different directions. If some or all of these can be cast in the role of the prodigal son, so much the better for fostering satisfaction all round.

The first connectionist conferences are remembered with great pleasure by many of the contributors to the oral history of connectionism (J. A. Anderson and Rosenfeld 1998). They played an important role, not only in raising the confidence of the field but in showing that it *was* a field—and a large one—in the first place.

Connectionism had always included two streams. There were those (like Rosenblatt and the PDP group) who leant towards psychology and/or neurobiology. And there were those (like Widrow and Hopfield) who came from engineering, physics, computing, and/or mathematics. In the 1970s and early 1980s, as we've seen, the two streams rarely mixed.

In the late 1980s, however, several international conference series were initiated which deliberately involved both. The first connectionist jamboree had been in 1979 at La Jolla (see Section v.b, above). But that was a mere taster. These new initiatives were more meaty.

The first "fairly large" neural network meetings were held in Santa Barbara in 1985, and at Salt Lake City's Snowbird Conference Center in 1986 (J. A. Anderson and Rosenfeld 1998: 300). The Snowbird meeting, organized by researchers at AT&T Bell Labs, is fondly recalled by several of the contributors to the *Talking Nets* oral history (a fascinating set of reminiscences and hindsighted judgements, arising in conversations between leading connectionists: J. A. Anderson and Rosenfeld 1998).

Primarily, it's remembered for the exciting mingling of people and sharing of ideas. Indeed, it later gave rise to the NIPS (Neural Information Processing Systems) series of meetings, now regarded by many as the premier scientific conference on neural networks. But it's remembered also, at least by the electrical engineer Robert Hecht-Nielsen, for the organizers' bizarre ideas on how to win friends and influence people:

> They had created this obnoxious conference registration form, where you wrote a little paragraph justifying why you should be invited to this meeting, and then underneath there were two boxes that they could check. One was "accept" and one was "reject". And as I understand the numbers, there were approximately five hundred of these forms returned, and something on the order of 125 of them were accepted. The others got them back saying "rejected". To say the least, there were a lot of angry, disenfranchised people. (J. A. Anderson and Rosenfeld 1988: 300)

Quite apart from his disgust at the crass way in which the Snowbird selection had been carried out, Hecht-Nielsen regretted that it had been done at all. Given the "sudden enthusiasm" for the topic, it was clear to him—and to his friend Bart Kosko—that "having a major meeting with unrestricted attendance was an absolute prerequisite for further growth and progress in the neural net field".

Accordingly, the two electrical engineers organized another, larger, meeting in 1987: the first IEEE International Conference on Neural Networks, for which Grossberg was the general chairman. This was intended as an educational exercise, rather than a straightforwardly scientific one. For the newly visible field was riven by priority disputes, and by worrying examples of ignorance and resentment that threatened its future. As Hecht-Nielsen later put it:

> There was already [in 1987] a very large clash under way between people who had been in the field for a long time and who had really prepared the foundation in the field and had all the good ideas, and new people who had come along and really didn't add much to the field, if anything, except perhaps an infectious enthusiasm and a following . . . There was this body of individuals who really had created the field and really had all the original ideas at that point, who didn't even exist as far as maybe a majority of the new people interested in the field were concerned. (J. A. Anderson and Rosenfeld 1998: 301)

One of the main aims of the meeting, then, would be "to firmly and unequivocally establish the history of the field in people's minds in a more correct sequence of events"

(p. 301). This meant inviting as many as possible of the half-forgotten originators, as well as the newly fashionable ones. But, as the co-organizer Kosko remembered:

> By the fall of 1986 we . . . went around to the many feuding factions in neural networks. The feuds were really beginning to heat up now. The PDP books were out. There were different camps, and we felt the only thing to do was have a level playing field . . . Many of the leaders of the neural field [whom we invited] literally hated each other's guts . . . (J. A. Anderson and Rosenfeld 1998: 403)

Given this unhappy background, Kosko and Hecht-Nielsen were far from confident that all the invitees would accept (they didn't), or that many other people would bother to turn up. But they were willing to put their money where their mouths were. Together with the local branch of the IEEE, they took on a million-dollar risk to underwrite the conference. (Sheraton hotels don't come cheap.) In the event, however, 2,000 people signed up.

Almost as many attended the summer 1987 meeting on neural networks at San Diego. A few months before that, Grossberg, Kohonen, and Amari had co-founded the International Neural Network Society (INNS), with Grossberg as its first President. They also invited papers for a new journal, *Neural Networks* (Grossberg was Editor-in-Chief). INNS held its first conference in Boston in 1988, co-sponsored by six IEEE societies. As for IEEE itself, it ran a 750-person scientific meeting (on 'Neural Information Processing Systems—Natural and Synthetic') in Denver in November 1987; and the second IEEE 'Neural Networks' conference—no longer a source of financial nail biting—took place in San Diego in July 1988.

Very soon (in 1989), these two series of meetings (INNS and IEEE) would unite as the International *Joint* Conference on Neural Networks. There were over 1,800 participants, including many from industry, at the initial assembly. The atmosphere of the first IJCNN reminded one participant of the movie mythology of *Star Wars*:

> There was very much a sense of excitement, almost euphoria. The Neural Network Jedi had returned and a rebellion was in progress. Bernie Widrow, one of the pioneers of the field, was perfectly cast in the role of Obi-wan Kenobi. The Evil Empire of AI, which had wrongfully suppressed and oppressed the innocent, was already declared to be dead. A new millennium was about to dawn. (Barrow 1989: 6)

One didn't need to be in Hollywood's home nation to share the sense of excitement. Other meetings were springing up all around in 1986–7, in the UK/Europe and Japan as well as in the USA.

The PDP group, in particular, put a lot of effort into stirring the pot. They instituted a series of highly successful summer schools, to introduce both students and fellow cognitive scientists to hands-on experience of connectionist computing. These were equivalent in intent to RAND's summer 1958 "Research Institute" and the other GOFAI summer schools (see Chapter 6.v.a). Just as in RAND-1958, a number of already established cognitive scientists welcomed the opportunity to be trained in the new methodology—and the new way of thinking about mind and psychology.

In addition, there were various attempts at commercial application. As Mead said later, these were interesting because they involved "actually looking at what [neural networks] do rather than making up stories about what they might do" (J. A. Anderson and Rosenfeld 1998: 142).

b. DARPA thinks again

At much the same time as these sudden and worldwide developments, DARPA began to have second thoughts about their current funding—or rather, no-funding—policy. ("No-funding" isn't quite fair: they'd supported a few selected projects from 1982 onwards, and had funded Hecht-Nielsen in building neurocomputers.)

The key trigger of their rethinking was the series of lectures given by Grossberg at MIT's Lincoln Laboratory in 1986–7, videos of which were circulated in the lab and reached the Director. He found them impressive, but there was no unanimity: some scientists and DARPA offices still held to the Minsky–Papert critique. He therefore persuaded DARPA to set up an urgent five-month enquiry into neural networks (DARPA 1988). Grossberg (personal communication) was invited to head it, but had to decline because of his other commitments. (As we've seen, besides doing his own research he was then the founding President of INNS, and editing the new journal *Neural Networks*.) Widrow was appointed as Director instead.

The enquiry was kicked off, in October 1987, by a two-day symposium at the Lincoln Laboratory. The task of the symposium was to outline the study's central themes. The fourteen speakers included several cognitive scientists and neurophysiologists. These were Feldman, Rumelhart, and Sejnowski; Grossberg and Gail Carpenter, both of whom spoke on vision; and David Hubel, Nobel prizewinner for research on visual cortex (see 14.iii.b).

The team who undertook the subsequent work, and eventually wrote the 650-page Report, were drawn mainly from computer science and engineering (in industry as well as academia). They included Hopfield and Hecht-Nielsen, as well as Widrow. But there were cognitive scientists, too: Grossberg, Carpenter, Selfridge, Waltz, Feldman, Williams, Rumelhart, and Richard Sutton.

In addition, the six Panels consulted many individual researchers, some of whom were invited to give a talk to the official DARPA team. Not the least of these was Minsky, who soon used his response as the core of his intransigent comments in the second edition of *Perceptrons* (Minsky and Papert 1988) (By this time, of course, he was both criticizing connectionism and using it in his own work: see Section iii.c–d, above.)

The reason behind this particular choice of speakers and Panel members was simple: DARPA's concerns were military. Neuroscience and psychology were included not for their own sake, but because they might help in producing useful working systems—where the military decided what was "useful". (There was little new in that. AI had been largely funded by military money from its inception: see Chapters 4.vi.a, 11.i, and 9.x.a.)

The Report baldly stated that any money that might be forthcoming would support "advancement of technologies critical to the development of next-generation, fire-and-forget, autonomous weapons" (DARPA 1988, p. xxv). Hence the emphasis on vision—for instance, Hubel's involvement. But any research topic might be relevant, if it advanced the understanding of connectionist systems in general. So, for instance, Sejnowski was invited to speak on 'Analysing the Hidden Units in Multilayered Neural Networks', one of the methodological issues raised by NETtalk.

As Hecht-Nielsen's remarks (above) clearly show, there were social sensitivities to be respected here. Widrow, as Study Director, noted that "there are many different and even conflicting views of what neural networks are, what they can do, and how they should be implemented", and disarmingly pointed out that "representatives of every school of thought in neural networks were consulted" (DARPA 1988, pp. xxix, xxx).

There was also the delicate matter of why DARPA had seemingly been so blind, in their near-refusal to fund connectionist AI for the last twenty years. Largely, no doubt, because—like Yovits—they'd been persuaded by Minsky in the 1960s (see Section iii.e, above). But there was another reason too, which was mentioned repeatedly in the Report: counter-productive hype on the part of connectionists themselves.

As Widrow put it in his introduction,

> Among scientists, the presence of hype and extravagant claims casts a dark shadow and makes the work controversial in scientific circles and amongst the world at large. (DARPA 1988, p. xxix)

The study specifically asked whether connectionism was "just a repackaging of old ideas and promises", as "some" (notably, Minsky and Papert) were claiming, and whether it was more "hyperbole" than achievement (DARPA 1988, pp. xxvii, xxix). Connectionist hyperbole, both past and present, was sternly noted. Media publicity and consorting with venture capitalists were decried too. Widrow tacitly defended DARPA's long-standing absence by complaining that "involvement is difficult . . . in the presence of hype" (p. xxv).

For all their disapproval of hype—plenty of which was now being spread by the PDP groupies, if not by the PDP group themselves—the scientific Panels didn't react as Sir James Lighthill had reacted to GOFAI sixteen years before (see Chapter 11.iv). As a result of their detailed assessment, DARPA decided to initiate "a major new program in neural networks beginning in 1989" (p. xxv).

As though this revenge wasn't sweet enough, Minsky and Papert were given a coded rebuke: "Neural network research is not new—it is, rather, newly revived from an obscurity and even disrepute which is now understood to have been undeserved" (DARPA 1988: 23). What Mead had called the "twenty-year famine" was over.

12.viii. A la recherche . . .

In the late 1980s, work on the new perceptrons ballooned. Much was largely 'more of the same', in the form of an ever-lengthening list of commercial applications driven by the venture capital remarked on by DARPA.

Examples ran all the way from mineral prospecting in the field to banking in the City of London or Wall Street. (Physics graduates were snapped up by the money men, to use 'thermodynamic' networks for financial predictions of various kinds.) These applications exploited PDP's power of pattern recognition, which Mead soon affirmed in uncompromising terms:

> [Recognition problems are] an area where it's very hard to do anything without a neural net. The AI people tried for years. With a neural network, you can do better in a week than people have done fooling around with AI programs for years . . . With real-world data, where you don't know what the information is, neural network paradigms can pull out that information and make it useful. That is very, very clear. It wasn't clear five years ago [in the late 1980s]. Then it was a gleam in all of our eyes, but at that time it wasn't clear to anybody who was objective about it that

it was going to be better than sitting down with a smart guy and writing a program. Now it's clear that there's no contest at all. That's been a big change. (J. A. Anderson and Rosenfeld 1998: 141)

It was also clear—within the field, if not in its press releases—that "Every silver lining has a cloud, and PDP is no exception" (A. J. Clark 1989: 127). Specifically, there's more to intelligence than pattern recognition. It followed that PDP wasn't the answer to every maiden's prayer—nor to every businessman's, either.

Relying not on madeleines but on arguments, some critics exhorted the new enthusiasts to remember, and try to recover, some key abilities of symbolic AI. For various things that had been easily achieved by GOFAI—notably, serial order and hierarchical planning—weren't yet possible using PDP.

Hierarchy couldn't be ignored, for it wasn't merely (merely?) a characteristic of intellectual thought. As Lashley had pointed out, it's a feature of skilled behaviour in general:

There is a series of hierarchies of organization; the order of vocal movements in pronouncing the word, the order of words in the sentence, the order of sentences in the paragraph, the rational order of paragraphs in a discourse. *Not only speech, but all skilled acts* seem to involve the same problems of serial ordering, even down to the temporal coordination of muscular contractions in such a movement as reaching and grasping. (Lashley 1951*a*: 121–2; italics added)

Propositional reasoning was impossible too, but the new connectionist modellers didn't even try to recapture that. (As you'll remember, the superiority of symbolic AI for representing propositions was one reason why it overtook cybernetics/connectionism in the 1950s: see 4.ix.b.) Even today, it's still an unconquered obstacle (but see Section x.e, below).

It wasn't clear that these things ever would be achievable by connectionist means. Perhaps only GOFAI could do certain sorts of problem solving, and/or perhaps the new networks would have to simulate GOFAI systems if they were to do so too. If one wants accurate reasoning or precise rule following, the famed noise tolerance of PDP may even be a disadvantage: two plus two really does equal four—not "probably 4", "maybe 3", or even 4.0001. Even "binding" different predicates (e.g. colour and shape) together to apply to *one-and-the-same* object—child's play in GOFAI—is problematic in connectionism.

These doubts were similar, at base, to Walter Reitman's doubts about the capacities of purely "Hebbian" programs (see Chapter 7.i.b). Now, some thirty years later, they engendered a new generation of PDP systems, doing things the ancestor networks couldn't do.

Admittedly, a few connectionists had already addressed these problems. Andreae had been doing so in working systems since the early 1970s (see Section iv.c, above). Minsky, in his "society" theory, had been attempting this too (iii.d, above). And John R. Anderson had combined localist networks with GOFAI insights about goal-directed thinking (7.iv.c). But the high-visibility (i.e. PDP) connectionist modellers had focused primarily on pattern recognition, not on hierarchy or sequence. Now, they made those the aim of their research.

(Meanwhile, two rather different things were going on. From around 1990, some young members of the recently named A-Life community developed a new form of neural net, which worked in continuous time and could simulate smoothly changing dynamical systems: see Chapter 15.viii.d. In addition, the now unfashionable GOFAI workers carried on, pushing their achievements still further: see Chapter 13.)

a. Emulating the ancestors

The first of these descendant networks had been conceived in 'biblical' times. For the PDP group themselves shared some of Minsky and Papert's continuing doubts about perceptrons. Indeed, they agreed with Minsky's hunch in his 'Steps' paper of 1961, that some *combination* of neural networks and GOFAI would be needed (see Section iii.b).

Even in their biblical volumes they admitted that, in order to do many kinds of problem solving, a PDP network would have to emulate a von Neumann machine. And Hinton, for example, had already tried to enable a PDP system to perform logical inference (Touretsky and Hinton 1985), and to implement a production system (see 10.v.e) (Touretsky and Hinton 1988).

Unlike some of their followers, then, the PDP group didn't claim that GOFAI had been a "false start" (Graubard 1988) or that it had "failed" (H. L. Dreyfus and Dreyfus 1988: 34; Dreyfus 1992, p. ix). Still less did they see "fooling around with AI programs" as a waste of time. On the contrary:

> [The] idea of distributed representations is consistent with some of the major insights from the field of artificial intelligence concerning the importance of structure in representations and processes. (Hinton *et al*. 1986: 104)

> [Mental] processes that take longer [than 0.5 seconds], we believe, have a serial component and can more readily be described in terms of sequential information-processing models. For these processes, a process description such as a production [see Chapter 7.iv.b] would, we imagine, provide a useful approximation. (Rumelhart *et al*. 1986*c*: 56)

The mental processes in question were characteristic of human thinking—the main focus of traditional AI (and of many group members' early research). Rosenblatt himself had surmised that perceptrons couldn't embody "such higher order functions as are involved in human speech, communication, and thinking" (see ii.f, above). Similarly, the PDP group admitted almost thirty years later that:

> The PDP system is fine for perception and motor control, fine for categorization. It is possibly exactly the sort of system required for all of our automatic, subconscious reasoning. But [we] think that more is required—either more levels of PDP structures or other kinds of systems—to handle the problems of conscious, deliberate thought, planning, and problem solving . . . People interpret the world rapidly, effortlessly. But the development of new ideas, or evaluation of current thoughts proceeds slowly, serially, deliberately. People do seem to have at least two modes of operation, one rapid, efficient, subconscious, the other slow, serial, and conscious. (D. A. Norman 1986*b*: 541–2)

The *microstructure* of the serial mode, they believed, is connectionist. This provides welcome emergent properties such as content addressability and graceful degradation, which sequential processing can exploit though not explain. Nevertheless, they recognized that PDP work would have to build on the "major insights" gained by years of GOFAI research.

Even in 1990, they weren't expecting any quick success. Hinton, by then based at the University of Toronto, edited a special issue of *Artificial Intelligence* on how connectionists might add "symbol processing" to pattern recognition. But the lost strengths of GOFAI weren't going to be resurrected overnight:

> [In future, if current learning] techniques can be applied in networks with greater representational abilities, we may see artificial neural networks that can do much more than just classify patterns. But for now, the problem is to devise effective ways of representing complex structures in connectionist networks without sacrificing the ability to learn the representations. *My own view is that connectionists are still a very long way from solving this problem* . . . (Hinton 1990: 3; italics added)

As for just how one can make a PDP network pretend to be a serial machine, the PDP books offered a number of ideas. For example, one chapter suggested that sequential thinking might be modelled by pattern matching in *multi-level* networks (Rumelhart *et al*. 1986*c*).

It described a simple computer model that used two mutually influential self-equilibrating networks to produce successive moves in a game of noughts and crosses (tic-tac-toe). The relaxation constraints of both networks embodied the rules and strategy of the game, but one network represented "player's move" and the other "opponent's move". The perceptual (input) model of each opposing move could drive equilibration in the player network, so as to produce an appropriate counter-move. In that case, the seriality would depend on the environment. But if the perceptual models were stored, or internalized, they could then be used by the player network to plan (and evaluate) hypothetical moves—in other words, to deliberate.

The ensuing years, to the end of the century, saw important advances such as recurrent nets, developmental minimalism, representational trajectories, input histories, multi-networks, hybrid systems, and constructive networks.

Each of these approaches lessened the distance between PDP and traditional AI. And each engaged with fundamental issues in computational psychology and philosophy of mind—not least, hierarchical structure and nativism. The first four are closely linked, and are described here; the last three will be discussed in Section ix.

b. Recurrent nets

Lashley had pointed out long before that serially ordered behaviour, such as speech, requires anticipatory (planning) mechanisms that define the 'place' of a behavioural unit in the overall sequence—typically, by imposing a hierarchical structure on it (see Chapter 5.iv.a). Planning had been a prime focus of GOFAI, and of GOFAI-based psychology. But whereas list processing had been ideally suited to represent both sequence and hierarchy (10.v.b–c), it wasn't obvious how—some doubters even said "whether"—a PDP system could do so.

Jordan (1956–) took up the challenge, and confronted the doubters with a recurrent network that could plan (Jordan 1986/1989). The general idea of recurrent networks, in which the output from some later layer is fed back as input to some earlier layer, wasn't new. It had been stressed by the cyberneticians, in their focus on "circular causation" and self-stimulating circuits (see Chapter 4.iii.c and v). But the early network-modellers had avoided feedback loops, because of the complex mathematics involved (the input to a given level changes continually). Now, Jordan provided a functioning model that could be used to generate ordered sequences of behaviour.

Constant feedback from output units to input units enabled Jordan's net to keep track of where it was, with respect to the sequence concerned. For instance, if a quickstep were represented as *slow, slow, quick, quick, slow* the net would know whether it had just output a *slow*, and which one of the three possible *slows* this had been. This information could affect the next signal from the input layer to the hidden layer, which in turn would determine the next output step.

So far, so rigid: a particular starting state would always trigger the same sequence. So Jordan added "plan units" to the input layer, which sent their own signals on to the hidden layer. Whether a hidden unit fired depended on the input it received from *both* the ordinary input units (receiving continual feedback) *and* the plan units. Hence: different plans, different behaviour—even though the perceptual input at the start might be identical.

As described so far, this required the researcher to specify all the unit thresholds and weights involved. In the toy networks (some less than a dozen units) described by Jordan, this was feasible. But pre-setting a network to execute 'at will' the plan of either 'Three Blind Mice' or 'The Death of Cock Robin', or any one of dozens of nursery rhymes (as humans can do) would be an impossible task. Clearly, what was needed was for the network to learn how to produce those sequences for itself. In other words, it had to learn to predict the next item in the behavioural sequence—and to reflect, in its own behaviour, any hierarchical structure in the input domain.

The pioneer in making this possible was Elman (1948–), who was working on speech processing (1990, 1993). This had been a key topic for connectionism since its beginnings. McCulloch, Pitts, and Selfridge had been involved in MIT's early project of developing an automatic speech-typewriter; and Rosenblatt, Longuet-Higgins, and Willshaw had all considered temporal input patterns, as we've seen.

Classical AI had modelled speech processing too, notably by HEARSAY in the early 1970s (see 9.xi.g and 10.v.e). In principle, this GOFAI model was a parallel-processing system, using multiple constraint satisfaction to interpret its auditory input. Over a decade later, and partly inspired by HEARSAY, Elman's PDP networks learnt to do much the same thing.

Elman's recurrent networks differed from Jordan's in using constant feedback from the hidden units (not the output units) to a special group of "context" units (which replaced Jordan's predetermined plan units). But the general principle was similar. An Elman network kept track of what the immediately preceding state of the hidden units had been. Indeed, it implicitly stored information about *all* its earlier states, because—given that the context units sent signals back to the hidden units—the current activity of a hidden unit would indirectly reflect its previous state, which in turn would have reflected *its* previous state . . . and so on. The context units (unlike Jordan's plan units) could learn, for the signals they sent to the hidden units were automatically adjusted by back propagation at each cycle.

These networks learnt to predict temporal dependencies, including dependencies within dependencies. For example, they learnt to predict phonemes so as to pick out individual words from continuous auditory input (Elman 1990). Sometimes, they'd get it wrong: *anelephant* would be segmented as *a nelephant*—a mistake that children often make too.

Given strings of individual words (Elman 1993), they distinguished verbs from nouns, from adjectives . . . and even animate from inanimate objects. And they learnt to reflect nested grammatical hierarchies. A plural noun at the beginning of an output sentence, for instance, would be matched by a plural verb later, even if there was an embedded relative clause in between; and a *subject-noun* would always be followed by some *verb*, and sometimes preceded by *adjectives*. (Strictly, this is an exaggeration: the longer the sentence, the less reliable the network's performance.)

All this was achieved in virtue of the distributional statistics of the various words. The Bloomfieldians, then, would have been happy (see Chapter 9.v). But whereas they had measured (timeless) coexistence within a whole corpus, Elman's networks were assaying the temporal statistics of sentences presented to them word by word and one by one.

The grammatical structures weren't represented explicitly, just as the rules of the past tense hadn't been stored *as such* in the past-tense learner. Network analysis showed, however, that the activities of the hidden units differed according to the grammatical role of a given word: e.g. *cat*, used as subject or as object.

In other words, Elman's networks posed yet another challenge to Chomsky's nativism. As already remarked, they were closer to the structuralist linguists who had preceded him. They had no inbuilt English grammar, nor any domain-specific Language Acquisition Device. Where SHRDLU and LUNAR (see Chapter 9.xi.b) had been provided with procedural definitions of English syntax, these PDP systems seemed to be able to pick it up for themselves by relying on domain-general principles of learning.

This was a "challenge" rather than a refutation, not least because backprop isn't biologically plausible. Synapses don't run backwards, nor are brains provided with error corrections by some outside supervisor. Moreover, Elman didn't ask whether there are types of 'grammar' that can be learnt by unstructured PDP systems but *can't* be learnt spontaneously by humans. If there are—which later turned out to be so (N. V. Smith 1999: 134–5)—then his results didn't disprove Chomsky's claim that humans depend on innate linguistic structure.

Nevertheless, his work revived questions about the development of language which Chomskyans—which is to say, most cognitive scientists at that time—had thought already settled.

c. Start simple, develop complex

Elman's work also raised other questions about children's language learning, which led to a general idea that one might call developmental minimalism. This sees development as passing from simple types of representation to more complex ones, where each of the higher levels is constructed 'on the shoulders' of the one below. (As remarked in Chapter 7.iv.h, it's a special case of the boundedness of rationality being a Good Thing.)

For example, Elman (1993) soon found that his networks failed to learn satisfactorily if the initial input set contained complex structures. By experimenting, he discovered that if the first inputs were all simple, more complex sentences could be learned later. It was as though the basic syntax was distinguished first, initiating a series of stepping stones leading to the full grammar.

This result fitted well with the view of some psycholinguists at the time, that parents use a special dialect ('motherese') when talking to infants (see Chapter 9.vii.c). The syntactic simplicity of motherese, it was said, enables the child to acquire grammar. However, those claims were by now controversial; and Elman, in particular, wasn't convinced. One problem was that even if parents do use motherese, children overhear adult conversation too—so why aren't they confused by that? Is the mere lack of active engagement enough to protect them?

Accordingly, Elman changed his strategy. Instead of minimalizing the network's input (environment), he now minimalized its memory. That is, he added a procedure that automatically 'emptied' the context units after every third or fourth input word, by resetting their activity to zero. So whereas these units had originally been designed to save traces of *all* of the network's previous activity (see above), they now held the impress of only the previous one to four words.

Clearly, the revised network had a good chance of learning *John punched Jack*, but not *John punched Jack hard on the nose*. And so it turned out: the new network could learn only sentences within the memory span of the context units. Once that had been achieved, however, Elman gradually increased the memory span, and found that this enabled the system to learn increasingly complex sentences. This strategy wasn't wholly 'artificial': evidence already existed suggesting that limits on human memory span affect the complexity of parsing, and explain 'garden-path' sentences (Chapter 7.ii.b).

The moral Elman drew was that if a recurrent network—whether artificial or biological—is learning something from scratch then a weaker, smaller-span, memory isn't a flaw (as he had earlier assumed). On the contrary, it's an advantage. Hence the subtitle of Elman's 1993 paper: 'The Importance of Starting Small'.

Babies and infants, he assumed (he wasn't an unreconstructed nativist: see 7.vi.g), learn virtually everything from scratch. Suppose there were some maturational process—myelinization of nerve fibres, perhaps?—that gradually extends the child's memory span. If so, it could afford domain-general assistance, and would result in abilities developing from simpler to more complex forms.

Developmental minimalism had started (for Elman) as an ad hoc computational trick, designed to avoid disappointing empirical results. But it was soon recognized as relevant to learning and development in general. The central principle was the need for repeated recoding, or representational trajectories.

d. Pathways for representation

The term "representational trajectories" was due to Andy Clark, a philosopher at the University of Sussex (1993, ch. 7). But the basic idea had been stated in the 1970s by Annette Karmiloff-Smith, then working with Piaget in Geneva, and formed the core of her theory of "representational redescription", or RR (1992). As we saw in Chapter 7.vi.h, RR theory posited the spontaneous development of increasingly powerful and flexible representations in the infant's mind.

Now, in the 1990s, she and Clark related it to the constraints faced by *any* learning system (A. J. Clark and Karmiloff-Smith 1993; Clark 1993). And soon afterwards, Clark and Chris Thornton (an AI colleague at Sussex) outlined a method whereby a PDP system could use representational trajectories to learn increasingly complex structures (A. J. Clark and Thornton 1997; Thornton 2000).

Their key insight was that the new perceptrons were limited in principle much as the old ones had been. Instead of linear separability, the focus now was on statistical regularity. Some regularities in the input are so clear that even an 'uninformed' learner can spot them. (Hence Mead's triumphalist remark quoted in the preamble of this section.) Others are more subtle, more elusive. In these ("Type-2") cases, the learner needs to know something about what regularities to look for if he/she/it is to have a realistic chance of finding them.

In effect, this was a generalization of Chomsky's anti-empiricist argument about the need for some innate grammar to support language learning (Chapter 9.vii). But unlike Chomsky, Clark and Thornton allowed that a suitably 'informed' learner may *either* be built/evolved to possess such guidance from the start, *or* may somehow learn, or construct, it for itself. What is needed in either case is a recoding of the input data, to make the marginal regularities stand out. A sequence of such recodings is a representational trajectory.

One example is Elman's research on increasingly complex grammars; indeed, Clark and Thornton described their work as an extension of Elman's.

* Another is the sequence of redescriptions posited by Karmiloff-Smith, whereby an automatic skill—such as drawing, or speech—develops into a flexible, generalized, and consciously self-reflective activity (see Chapter 7.vi.h).

* Yet another is the series of visual computations involved in constructing the Marrian Primal Sketch (Chapter 7.v.b–d).

* And yet another is the history of scientific theories and analogies used in developing current optics (P. M. Churchland 1995: 271–86) or astronomy (Thornton 2000, ch. 3). If Aristotle, or even Isaac Newton, were to come back today, we couldn't teach them modern physics by simply *telling* them: they'd need a sequence of conceptual bootstraps.

In general:

> [A] wide variety of superficially distinct ploys and mechanisms can be fruitfully understood in these terms. Such ploys and mechanisms range from simple evolved filters and feature detectors all the way to complex cases involving the use and reuse of acquired knowledge. The goal, in every case, is to systematically reconfigure a body of input data so that computationally primitive learning routines can find some target mapping—that is, to trade representation against computation. (A. J. Clark and Thornton 1997: 57)

The "ploys" used by *Homo sapiens* include a host of examples based in language, writing, technology, and cultural institutions. These are all "adaptations enabling this representation/computation trade-off to be pursued on an even grander scale" (p. 57), and are essential for, even intrinsic to, distinctively human thinking. So far, so psychological—but Clark would soon use this point to ground an unorthodox philosophy of the self (see Chapter 16.vii.d).

Non-human animals can't generate these language-based "cognitive technologies" (Clark's term, borrowed from Bruner: 6.iii.c). But even they needn't possess inbuilt modules suited to every structure they can learn, provided that they can recode the current information. If we want to understand why different species have different learning limits, we must ask how their representational trajectories differ, and why.

At the end of the century, Thornton told a fairy story about a Machine That Can Learn Anything—let's call it MTCLA (Thornton 2000, ch. 1). According to its designer, and to "no fewer than three TV stations", this machine never failed to make the correct association (to pick up the regularity concerned) in the majority of cases, *no matter what* problem it was presented with. What's more, the designer wasn't lying. Nonetheless, it turned out as the story proceeded that MTCLA was a confidence trick—or rather, three.

* First, its achievements were too few to be useful. A success rate of 50.01 per cent would satisfy the "majority of cases" criterion, but for any practical purpose it needs to be much higher. (Whether 100 per cent reliability is required, or even possible, is a question discussed by philosophers of science as well as by stockbrokers: Thornton 2000, ch. 9.)
* Second, the users of the fabled machine had to specify its learning tasks in a highly restricted way. In short, the power lay in the input representation. (Compare the so-called *General* Problem Solver: see Chapter 6.iii.c.) If no suitable input could be found, the machine wouldn't actually *fail*, because it couldn't be presented with the problem in the first place.
* And third, every problem that could be presented was trivial: a simple statistical trick sufficed to learn the regularity.

In fact, this wasn't a mere fairy story. The networks of Rosenblatt, Widrow, Hopfield—and now, PDP—had involved comparable hype, and comparable con tricks. The moral of Thornton's mocking tale was that, even after forty years of connectionist research, the core of (non-trivial) learning had hardly been touched.

If we really want a machine that can learn anything, he said, we must provide a domain-general method whereby the learner can produce useful truths from what looks, initially, like "trash". It looks like trash because of what Clark had called "snowblindness" (1993: 149), in which the system is (at first) so overwhelmed by detail that it can see neither the high-level regularities nor even the lower-level ones. Building on their joint 1997 paper, Thornton described an incremental process of recoding ("recursive relational learning") that provides a series of "scaffolds" for learning.

On this account, learning is continuous with creativity:

> Without the ability [for] relational learning, an agent is . . . effectively *trapped* within its immediate sensory space. Its only recourse is the establishment of simple partitions within that space. But [with] relational learning, the agent acquires the ability to escape its immediate embodiment—to interact with properties that are not explicitly manifest in the world to which it has direct sensory access. (Thornton 2000: 181)

As the number of recoding levels rises, the highest ones are "substantially the creative artefacts of the learner's *own* processing . . . [i.e.] their properties are [less] severely constrained by the source data" (p. 193).

Thornton didn't claim that his method could learn literally *anything*, nor that the human brain can do so. There may be inescapable constraints on the recodings that are possible in human thought. It's sometimes suggested, for example, that we won't ever understand the relation between brain and consciousness, much as dogs won't ever learn arithmetic (see Chapter 14.x.d). But if we want to understand systems that even *approximate* an MTLCA, we must allow for representational trajectories.

e. The importance of input history

As for just what these trajectories will be, it became increasingly clear that the input history—the temporal sequence of the trash—matters. For instance, how (and whether) a PDP system learns to emulate rule-governed syntactic processing depends on its training history.

Elman's decision to present simple sentences before complex ones was an early illustration of this point. But in the early 1990s, the effects of more subtle variations of the training set were explored by the Oxford developmental psychologist Kim Plunkett, with Virginia Marchman and Chris Sinha (Plunkett and Marchman 1991, 1993; Plunkett and Sinha 1992). Their test bed was an improved version of the past-tense learner described in Section vi.e (they'd added a hidden layer, and backprop).

Instead of artificially 'splitting' the input, they presented a fixed vocabulary set in varying quantities and proportions, and in varying orders. They found that both gradual and sudden (stagelike) changes in performance are driven by internal reorganizations that depend crucially on the input history. For instance, a training sequence can sometimes inhibit later learning that would have been possible if a different input sequence had been used.

In doing this study, they had to analyse the network's performance in greater detail than Rumelhart and McClelland had done. Beyond considering the overall statistics, they tracked the learning of individual verbs, and of distinct sub-groups of regulars and irregulars. They found many differences in the pattern of errors and error recoveries concerned, often including "regressions" in which old errors returned and/or new ones appeared as learning progressed. Another interesting result was contamination by irregulars, as certain regular verbs were apparently assimilated to subclasses of irregulars.

In all these cases (and others), developmental linguists had recently observed similar error patterns in children. In both humans and PDP systems, then, the learning of the past tense is a more complex—and contingency-driven—phenomenon than had previously been suspected.

But why stop at the past tense? Plunkett found that the effects of different input histories were often predictable, given the general properties of the PDP system concerned (the learning rule being used, and the constraints necessarily involved in storing many distributed patterns in a single network). The moral, then, was that *in general* input histories matter. One might say this insight wasn't new: as we've seen, Hopfield had asked how many units should be set correctly at the start to give optimal results. But no one had studied the effects of *extended* training histories in such detail.

12.ix. Still Searching

The four advances considered so far—recurrent nets, developmental minimalism, representational trajectories, and input histories—were closely linked, as we've seen. The other three were more diverse.

Research on multi-networks, hybrid systems, and constructive networks used very different methods to increase the power of connectionist systems. One even risked betraying the faith by resurrecting GOFAI, though admittedly only in combination with networks. By the turn of the twenty-first century, however, these too still left much to be done.

a. Assemblies of cell assemblies

An early instance of a multi-network was described in the PDP books, namely, the noughts-and-crosses player mentioned in Section viii.a. An even earlier example had been outlined by McCulloch in the late 1960s, in his controller for a Mars robot (Chapter 14.iv.a). Others were sketched or implemented later.

For example, Clark and Karmiloff-Smith (1993) suggested that a new network might come to represent the inner structure of an older one in a more "symbolic" way as a result of skeletonization (cf. Chapter 7.g). This technique copies an initial net into another one, and then prunes the connections that correspond to inessential, relatively inactive, connections in the first (Mozer and Smolensky 1989).

But two-network systems, such as these, are unrealistic. Hebb had argued long ago that adult human brains must involve numerous cell assemblies; and if Minsky and Papert's "society of mind" theory was on the right lines, then complex intelligence requires many interacting networks (iii.d, above). The PDP group agreed. As Norman put it in their manifesto:

> [People] can do multiple activities at the same time, some of them quite unrelated to one another. So, in my opinion, a PDP model of the entire human information processing system is going to require multiple units. That is, the complete model requires that the brain consists of several (tens? hundreds? thousands?) of independent PDP-like systems, each of which can only settle into a single state at a time. (Norman 1986*b*: 542–3)

Moreover, each of these "independent PDP-like systems" would very likely be a modular hierarchy itself.

This claim may have seemed strange to readers over-impressed by the connectionists' constant references to representations being "distributed over the *whole* network". But the PDP group's earlier work in GOFAI had already alerted them to the need for hierarchical structure. What's more, backprop had been designed with this in mind:

> One of the best and commonest ways of fighting complexity is to introduce a modular, hierarchical structure in which different modules are only loosely coupled [here, Hinton cited Simon's *Sciences of the Artificial* (see Chapter 7.iv.a)] . . . Self-supervised back-propagation was originally designed to allow efficient bottom-up learning in domains where there is hierarchical modular structure . . . It is possible to learn codes for all the lowest-level modules in parallel. Once this has been done, the network can learn codes at the next level up the hierarchy. The time taken to learn the whole hierarchical structure (given parallel hardware) is just proportional to the depth of the tree . . . [although] it is helpful to allow top-down influences from more abstract representations to less abstract ones, and [this has been done in] a working simulation. (Hinton 1989: 228)

Hinton's recommendation of "top-down influences from more abstract representations" was yet another example of GOFAI footsteps preserved in the connectionist sand.

If PDP could provide massive modularity in principle, however, it didn't follow that it could do so in practice. Hinton's reference (above) to a "working simulation" didn't mean that large multi-network systems were practically feasible by 1990, for they weren't. But it was already clear that one of the most important aims of PDP research into the next century would—or anyway, should—be to master this problem.

That qualification ("or anyway, should") is necessary because there was surprisingly little work on this topic in the 1990s. To be sure, the anthropologist David Hutchins (1995) modelled the interacting roles within a ship's crew (a part-hierarchical assembly) by way of *networks of networks* (see Chapter 8.iii.b). And some researchers, including Jordan and Hinton, soon developed pattern recognition systems within which distinct sub-networks emerged that were sensitive to different aspects of the input—men's, women's, or children's voices, for example, or a particular vowel (Jacobs *et al.* 1991).

This was done by extending the 'winner-takes-all' approach from single units to groups of simultaneously active units. (In a winner-takes-all network, the learning rule doesn't increase the weights on *all* the units, but only on the most highly activated one.) A higher-level 'referee' sub-network was backprop-trained alongside the emerging experts, to decide which mini-network should be the winner when more than one was activated by a certain pattern.

In effect, this was a self-organizing PDP system that learnt to mimic a simple version of Pandemonium. But a multilevel Pandemonium, or the interacting "multiple unit" hierarchies mentioned by Norman, would be harder to implement—never mind to learn.

Much as the early computational psychologists had abandoned multi-motive systems, because getting a program to pursue one goal was difficult enough (see Chapter 7.i.b), so PDP workers found that getting a network to achieve a single task was more challenging than they had expected. This is just what Thornton's analysis would predict: many interesting tasks are Type-2 problems, requiring one or more levels of recoding—which 1990s networks couldn't deliver. Accordingly, hardly anyone tried to address the sort of multi-network complexity that Norman had in mind. (An interesting exception, based partly on his own psychological theory, is described below.)

Many focused instead on continuing the mathematical turn described in Sections iv–v, seemingly forgetting that the original purpose of perceptrons had been psychological. As Thornton put it in 2001 (personal communication):

> My perception is that connectionists are currently executing a kind of mass retreat into mathematics, perhaps in a subconscious attempt to deny operational reality. Certainly the kind of psychologically-aware approach pioneered by Geoffrey Hinton [in his models of semantic memory] doesn't get much of a look-in these days. (Thornton 2001, personal communication)

To be sure, the 1990s had seen significant work on 'real' learning, some of it deeply informed by psychological and/or neuroscientific data (O'Reilly and Munakata 2000). But, as yet, there was little overlap between the biologically oriented connectionists and their more analytically oriented cousins. The conferences described in Section vii.a (above) were driven by the hope that empirical and analytic workers would collaborate, the distinction between them becoming much less clear. That hope hadn't been fully satisfied—which is why most of the research described below is more mathematical than psychological, and more psychological than neurological.

b. Hands across the divide

The assumption—shared by the PDP group and society-Minsky—that much human thinking requires some *combination* of connectionist and GOFAI methods led to a new research field: hybrid systems. This term is sometimes used to mean programs/robots that combine situated and deliberative procedures (see 7.iv.b and 13.iii.c). More often, however, it's used to mean systems combining connectionist and symbolic architectures.

The general idea of combining two very different types of computation in one machine wasn't new. Indeed, it dated back to the earliest days. MacKay, with McCulloch's encouragement, had suggested developing part-analogue, part-digital computers in the late 1940s (MacKay 1949/1959). By the mid-1960s, he was even saying:

> We on the circuit side had better be very cautious before we insist that the kind of information processing that a brain does can be replicated in a realizable circuit. *Some kind of 'wet' engineering may turn out to be inevitable.* (Mackay 1965: 329; italics added)

Minsky had said in his widely read paper 'Steps Toward Artificial Intelligence' (available in draft from 1956 on, and published in 1961) that hybrid architectures would probably be needed (Chapter 10.i.g). A few years after that, Reitman (1965) had built Argus as an exercise in combining sequential and parallel processing (see 7.i.b). Since then, however, hardly any work had been done on it.

Some commentators in the mid-1990s expressed impatience that this combination hadn't already been achieved:

> The whole field has been hovering around this issue, pendulum-like, for over three decades and it is time that these two approaches are reconciled, so that we can begin to take advantage of and build on the strengths of both. (Honavar and Uhr 1994, p. xiii)

After all, the two types of AI were in principle equivalent, since both were using general-purpose computational systems. In practice, however, they were different. The *degree* of difference was a matter of taste, not to say self-regard or professional pride (I was reminded of the horse in *The Wizard of Oz*, whose colour changed with the lighting as it trotted along: Boden 1991). But there were differences nonetheless, and it wasn't clear how they could be surmounted. (For the record, it still isn't.)

The organizers of a 1995 conference on the topic located the core problem in the ontologies we use to describe the world. As they put it, we must somehow "reconcile the static ontologies of standard knowledge representation with a continuously changing world described using the ontologies of physics" (Hallam 1995, p. v). In other words, we must find a way to translate effectively between von Neumann's "primary" and "secondary" languages (see Section i.c). What is needed, they said, is "a theory able to treat continuous and discrete processes and their interactions in a uniform way"—and that was still distant.

Perhaps, other authors added, it was even impossible:

> There is no clear method for ensuring the preservation of a symbolic algorithm in a dynamical equation that does not itself constitute an algorithm; nor one for ensuring the preservation of a symbolic semantics (or function) in a connectionist account of the discipline of real world entities . . . There is no possibility of an exact mapping between the connectionist and the symbolic. (Franks and Cooper 1995: 69)

This question couldn't be decided, they argued, until we have a better way of individuating algorithms. The current definition relies on the intuitive notion of a "step", which might be interpreted in differing ways. This raises questions about all computer models, hybrid or not, and "the issue remains to be solved by the cognitive sciences" (p. 70).

Meanwhile, a useful distinction was made between three types of hybridization—only one of which reflects hybrid psychological theories (Franks and Cooper 1995). In the first, two *physically* distinct types of computer architecture are used to compute different functions. For instance, a rule-based expert system implemented in a von Neumann machine might use neural networks on VLSI chips to recognize some rule-triggering patterns. This may be fine for engineering purposes, but it doesn't reflect the physical construction of the brain.

In the second type, the hybridness is viewer-dependent, since it rests on how one chooses to *describe* the system. For instance, the past-tense network can be described either as a PDP model or *as if* it were a rule-learner. The psychological theory being modelled, however, was purely connectionist. Only the third type is "truly" hybrid, in that the system's behaviour is generated by both connectionist and symbolic functions, *and* by theoretically significant causal relations between them.

The PDP bible itself had suggested that, in doing paper-and-pencil arithmetic, one combines perceptual pattern matching with rule-governed mathematical reasoning. Most PDP modelling wasn't driven by psychological theories that featured strong interactions between symbolic and connectionist processing. But Hinton (1988/1990) made an early attempt, by combining PDP with localist connectionism in order to represent part–whole hierarchies—such as family trees, for instance.

The localist network was a reduced version of the distributed one, wherein the 'relevant' units could be identified and accessed more directly. In other words, "the very same object can be represented in different ways depending on the focus of attention" (Hinton 1988/1990: 65). Putting the same point the other way around, he said:

> [Some] patterns of activity in [the network] need to exhibit the double life that is characteristic of symbols. The patterns must allow remote access to fuller representations, but so long as the patterns are also reduced descriptions this remote access need only be used very occasionally (e.g. a few times per second in a person). Most of the processing can be done by parallel constraint satisfaction on the patterns themselves. (p. 49)

And rationality, as opposed to intuitive thinking, was shown *not* to be a matter of seriality. Its defining characteristic, rather, was that "the way in which entities in the domain are mapped into the hardware *changes* during the course of the inference" (p. 50; italics added). It followed that the crucial GOFAI assumption—that fast intuitive inference is similar in form to conscious inference, but without the consciousness—was judged by Hinton to be "a major psychological error" (p. 72).

Hinton's 1988/1990 approach was soon developed by others (J. B. Pollack 1990; Chalmers 1990; Plate 1995; Sperduti 1992; Sperduti and Starita 1994; Goller and Kuchler 1996; Goller 1999). So his *distributed reduced descriptors* later gave way to *recursive auto-associative memory* (RAAM), *holographic reduced representations* (HRRs), *labelling recursive auto-associative memories* (LRAAM), and *folding architecture networks* (FANs). In general, these new acronyms named methods by which vectors representing the individual components of a compositional structure were combined into a single vector standing for the whole structure. However, although they were more powerful than Hinton's family-tree program, they still couldn't deal with large-scale tasks: only small examples were tractable.

Hierarchy is important not only in linguistic behaviour, such as family trees or syntactic parsing. As Lashley pointed out (5.iv.a), it's an observable feature of motor skills in general. And if Simon was right in his *Sciences of the Artificial*, it's also near-inevitable, for reasons of processing economy. Indeed, recent neurological work shows that the spinal cord itself (not just the brain) stores a number of basic motor functions as *patterns* of individual muscle activations, which mini-hierarchies are combined in various ways to produce larger-scale action sequences (Mussa-Ivaldi 1999; Tresch *et al.* 1999). It follows that weakness in handling hierarchy is a severe drawback for any computational methodology that hopes to simulate even non-intellectual behaviour.

By the mid-1990s, there were several part-PDP working models that took psychological hybridization seriously. (The long-established family of ACT architectures outlined in Chapter 7.iv.c were hybrid in spirit, but didn't come from the PDP stable.)

One of these learnt to cope with simple arithmetic—specifically, the multiplication tables (J. A. Anderson *et al.* 1990*b*). The number-names were stored symbolically, but the numbers were also represented (according to their size) on a "sensory" topographic map. The justification for this was the fact that many (most?) humans seem to represent numbers spatially, especially when making judgements about relative size.

"Coping with" arithmetic, in this case, didn't mean conquering it. Like Dr Johnson's dog walking on two legs, the wonder was not that the system did its task well but that it did it at all. For it made many mistakes: 30 per cent errors in multiplying two integers. However, most of these were "intuitively reasonable", and matched a variety of error-types observed in human beings. Tongue-in-cheek, the authors declared: "It may seem remarkable to spend hours of supercomputer time to obtain the wrong answers to simple arithmetic, but that is cognitive science."

Another PDP hybrid was implemented by the neuropsychologist Shallice (now at UCL's Institute for Cognitive Neuroscience) and other London-based colleagues (Cooper *et al.* 1995). It was based on the theory of action that Shallice had developed in the late 1970s with Norman, a PDP group member who had worked for many years on the psychology of error (D. A. Norman and Shallice 1980/1986).

Whereas Norman had focused on everyday cases, such as typing errors (Rumelhart and Norman 1982), Shallice added clinical examples. Besides his interest in dyslexia (which he and his colleagues continued to model: Plaut and Shallice 1993; Plaut *et al.* 1996; Plaut 1999), Shallice was an expert in clinical ataxias, or disorders of action.

For instance, brain-damaged patients may apparently forget that the letter should be put in the envelope before the sticky flap is licked, or frequently get into bed on going upstairs to change their clothes, or pick up the kettle when they mean to pick up the teapot. Similar mistakes—errors of *order*, *capture*, and *object substitution*, respectively—occur occasionally in all of us (and Norman had asked "Why?"). But they're much more common in certain clinical syndromes. Moreover, brain-damaged patients can't easily recognize or correct them, whereas normal people can.

In 1977–8, on first reading about GOFAI planning, Shallice told me of his hopes that this approach might help him analyse such errors. But he soon decided that connectionist ideas were needed too. He and Norman argued in 1980 that over-learned action is generated by one or both of two types of control. Errors, of differing classes, occur when one or both of these controls breaks down at specific points.

The first control mechanism ("contention scheduling") is automatic. It involves competition between various interacting and hierarchically organized action schemata. Control goes to the one whose activation passes some threshold.

The second mechanism ("executive control") is attentional—that is, conscious. Because this involves deliberate supervision and modulation of the operation of the first, it's sometimes called the supervisory attentional system. (An action schema is defined by a goal, and a partially ordered set of sub-goals; each schema controls a partially ordered sequence of actions.) It's required in five types of situation:

* planning or decision making;
* especially difficult actions;
* repairing mistakes;
* inhibiting a strong already learnt response;
* and learning new actions.

The original theory had been purely verbal: the techniques available in 1980 couldn't produce a computer model. Fifteen years later, Shallice's team built a model that simulated both normal error-free behaviour and, with different parameter settings, patterns of error seen in various clinical syndromes, including Parkinson's disease.

Their prime example was coffee making. This wasn't a trivial, spur-of-the-moment, choice. They focused on coffee making partly because it's a familiar multilevel action schema. But they picked it also because it had been closely studied in neurological patients. In short, there was already a detailed 'library' of errors in coffee making, observed in both normal and brain-damaged people.

This study, which turned out to be very influential, was part of a more general programme of research in computational psychology (Cooper *et al.* 1996). One of the core collaborators was John Fox, of the Imperial Fund for Cancer Research—a long-standing leader of expert-systems work in Great Britain. That was no accident, for the team saw both GOFAI and PDP as essential:

> The implementation is hybrid in that it contains components which carry out continuous-valued operations using a broadly interactive activation approach [based on the late 1970s work of Grossberg, McClelland, and Rumelhart] and other components which carry out discrete symbolic operations. (Cooper *et al.* 1995: 28)

They pointed out that this connectionist–symbolic mix wasn't a mere implementation detail, or engineering 'fix', but crucial to the psychological theory being modelled. The discrete, serial, symbolic aspect concerned the *selection* of this or that action schema or goal (sub-schema, sub-goal . . .) at a given point in the action sequence. The connectionist aspects regulated the *levels of activation* of the schemata in varying circumstances.

To some extent, the activation levels depended on the simulation of neuroscientific factors, such as lateral inhibition and dopamine concentration. Future research, the team said, would consider a range of neurotransmitters; differing reaction times for different senses; finer-grained object representations; and the attentional level of control (the 1995 program concerned only the automatic level). Each of these additions should result in a more discriminating and more realistic model of errors.

For instance, suppose that the sugar sachet mentioned in the plan schema were represented not only (as in the 1995 model) by *is-sachet* and *contains sugar*, but also by other perceptual and semantic features. In that case, one would expect more "substitution" errors, in which one object is picked up instead of another which shares some features and/or associations with it.

Besides applying to everyday errors and to debilitating psychopathology, the Norman–Shallice theory has been applied to hypnosis (see Chapter 7.i.g). With the help of philosophical work defining various levels of "explicit" and "implicit" representation, hypnosis can be explained as executive control *without* the highest level of explicitness—at which the person not only has an intention but knows that, and can report that, they have it (Dienes and Perner forthcoming). The intention can be monitored and flexibly executed by the hypnotized person. But these processes go on without conscious attention.

In sum, there's a growing appreciation that we need both GOFAI and PDP to model the mind. Michael Arbib, when he was interviewed for the "oral history" of connectionism, put it like this:

> I don't believe that neural nets are a magic panacea. I think there's still a place for knowing how to add numbers exactly, rather than using a neural net. *I think in the future we'll see hybrid systems* where we have something like schema theory [see Chapter 14.vi.c] or modular design to understand how to take a complex problem, break it into pieces, and then for *some* of those pieces find that neural nets will do the best job. (J. A. Anderson and Rosenfeld 1998: 236; italics added)

c. Constructive networks

Quite soon, connectionists began to experiment with "constructive" networks. In these, the number of hidden units didn't have to be decided by the designer. For hidden units could be deleted or added during the running of the system. (A wide range of examples are compared in Fiesler 1994.)

Self-deleting networks were defined by Rumelhart as early as 1987. Starting with many hidden units, these were selectively pruned—weights were permanently reduced to zero—so long as this reduced the overall error. This approach was sometimes compared to 'neural selection' in the brain, in which profuse neuronal connections are progressively pruned as a result of experience (Chapter 14.x.d). In essence, however, it was a slight variation of the normal PDP network: some weights were eliminated once and for all, not merely (temporarily?) reduced.

The complementary type of constructive network, in which hidden units are added, is more interesting. One influential example was developed by Fahlman (Fahlman and Lebiere 1990). His "cascade correlation" networks started out with no hidden units at all: the input units were directly connected to the output units. But they added new ones incrementally until the error couldn't be further reduced. Each newly added unit would be randomly connected to the input layer, and would receive input also from the previously added hidden unit—hence the name, *cascade* correlation.

This network construction was a cycle of two phases. Even before the first hidden unit was added, some learning would take place, during which the (direct) input–output weights were adjusted in the usual way. Cascade learning would start when these weight changes had settled to equilibrium. A number of potential hidden units would be provisionally added, and their performance monitored. The one found to be most efficient in reducing error would then be permanently included. After another 'normal' learning phase in which the whole network (now carrying a new hidden unit) adjusted its weights, the cascading would kick in again—and so on, until no current provisional unit was able to improve performance.

Cascade correlation enabled networks to emulate certain aspects of GOFAI programs. For example, we saw in Chapter 7.vi.g that Piaget's developmental theory of seriation—the ability to produce rank order, as in building a staircase out of blocks—posited a final stage in which the child apparently plans his/her actions as a rationally ordered sequence. We saw, too, that his three-stage theory was first simulated by GOFAI programs (e.g. Richard M. Young 1976). This is perhaps just what one would expect, given the GOFAI-ish nature of the final behaviour. But we also noted that a connectionist model of RR for seriation was achieved twenty years later (Shultz 1991; Shultz *et al.* 1994, 1995). This was done by using cascade correlation. Two separate output layers were required: one to decide which block to move, the other to decide where to put it.

One might object, however, that this seriation network involved cheating. For the error assessment didn't use a perceptual test, but relied on Piaget's formal–operational rule for building a staircase: *Move the smallest block that is out of place into its correct place*. In other words, it's not clear that the connectionist system was generating GOFAI-type performance 'spontaneously'.

d. What had been achieved?

By the end of the century, as we've seen, the lost strengths of symbolic AI hadn't been fully recovered. Research, not to say *Recherche*, remained necessary.

In practice, many people decided to *combine* rather than to *recover*. As Arbib said in 1993:

> There is so much fast-computing hardware around that a lot of ideas which were purely theoretical in the '70s are now eminently practical in the '90s . . . A lot of work now has to be either very focused, on a very specific application, or you have to develop a hybrid system where you apply some standard signal-processing and some standard AI expert systems stuff, using a neural net for a module or two. I think the day of the magic single neural network has gone. (J. A. Anderson and Rosenfeld 1998: 235)

But there were now many more ways to skin a connectionist cat (and yet more arcane statistical mathematics used to do so). To mention just a few, most of which have been discussed above:

* Some networks used basic computing elements (as identified by the mathematics) that were implemented not as single units but as groups of interconnected units, or "neuron pools" (Amari 1977).

* Besides a variety of learning rules that adjusted weights on existing connections, there were others that added new links and pruned old ones.

* Some learning happened across generations instead of in individuals, using the newly popular technique of genetic algorithms (Chapter 15.iv); an example involving a PDP group member, namely Elman was (Nolfi *et al.* 1994).

* Some connectionist work led back to Hopfield rather than PDP: 'attractor' neural networks were completely connected nets, understood as dynamical systems converging towards various attractors (global equilibrium states) as a result of different inputs, or 'perturbations'.

* The puzzle of how to enable ANNs to deal with hierarchy was addressed by methods in which a network could contain representations of a whole as well as representations of its parts—although, as remarked above, only small-scale tasks were tractable.

* Some people, such as Shallice, developed truly hybrid systems, in which neural networks and GOFAI components were combined.

* Judea Pearl (1988) described how to use (non-recurrent) Bayesian networks to do approximate inference, or reasoning, not mere pattern recognition.

* And, building on Pearl's work, methods for "loopy belief propagation" (LBP) enabled recurrent networks containing many loops, of different sizes, to converge instead of running endlessly (McEliece *et al.* 1998; Murphy *et al.* 1999; Yedidia *et al.* 2000). LBP is now being used for various purposes in computer vision (e.g. Coughlan and Ferreira 2002; Sigal *et al.* 2003; Isard and MacCormick in preparation), and applied also to make inferences of a type that would previously have been handled by a GOFAI expert system—medical diagnosis, for example.

Moreover (as we'll see in Chapter 14), the distinction between neural networks as an AI technology and as a way of modelling the brain was becoming less clear. For example, attractor networks were studied in relation to Walter Freeman's dynamical theory of the neurophysiology of perception (see Chapter 14.ix.b). And in 1998, Hinton was appointed head of the new Gatsby Computational Neuroscience Unit, at University College London.

Hinton's reputation, already high in the 1980s, had soared still higher: Cowan said the best advice he could give to a young researcher would be to "try to work with Geoff Hinton" (J. A. Anderson and Rosenfeld 1998: 124). At the outset of the new century, he received the first Rumelhart Prize for "contributions to the formal analysis of human cognition". This wasn't, you'll notice, a prize confined to connectionists. Accordingly, it was awarded at the 2001 (Edinburgh) meeting of the Cognitive Science Society.

More generally, connectionists moved towards techniques inspired by neuroscience (see Chapter 14). Some new learning rules, for instance, took account of spiking frequencies and synchronies, while others modelled the effects of widely diffusing chemicals in the brain. One, based on the functioning of the cerebellum, was "two to four orders of magnitude faster" than backprop (McKenna 1994: 79, 82).

The influences went both ways. A persuasive computational argument for localism—recently swamped by the D in PDP (*pace* Hinton's localist–PDP mix: see above)—implied that neuroscientists should *expect* to find grandmother cells as well as cell assemblies. The localist resurrection (and grandmother's revenge), however, had to await the new millennium (Chapter 14.x.e). In the last twenty years of the old one, unalloyed PDP was the most high-profile form of connectionism.

12.x. Philosophers Connect

The high visibility of PDP, described in Sections 8.vii–viii, was largely due to its *philosophical* interest. At an informal level, this was true of its appeal to journalists and the general public. But the professional philosophers were involved too. PDP was widely perceived by them as a more human alternative to classical functionalism (Chapter 16.iii–v), and also as a counter to Chomsky's nativist philosophy of mind (9.vii). Most of the interest came from analytic philosophers already sympathetic to cognitive science—although, as remarked in Chapter 1.iii.d, some postmodernists saw connectionism as buttressing their own, very different, approach (Globus 1992; Canfield 1993; Wilson 1998). But the GOFAI classicists fought back, as we'll see.

The widespread debate involved not only card-carrying philosophers, but also leading model-builders on both 'sides' of AI.

a. A Pulitzer prelude

Many philosophers first became aware of (broadly) connectionist ideas in 1979–80, as a result of Hofstadter's book *Gödel, Escher, Bach: An Eternal Golden Braid* (1979). This extraordinary intellectual and rhetorical feast drew many people to cognitive science, including what would later be called A-Life, for the first time. In addition, it inspired some already within the field to think about computation in a fundamentally new way.

One example of the latter was Mitchel Resnick, whose work on StarLogo was the result (Resnick 1994, p. xvii, and Chapter 12.iii.d). Another was Randall Beer (personal communication), then a graduate student writing GOFAI programs. His vision of how one might approach AI problems was irreversibly broadened by reading the book (see 15.vii.c and viii.c).

Begun in the early 1970s, the manuscript was essentially complete when Hofstadter joined Indiana University in 1977, in his first 'real' job. (He remained at Indiana thereafter, apart from interludes at MIT in 1983–4 and at the University of Michigan from 1984 to 1988.)

Being utterly impossible to pigeonhole, and very long to boot, it didn't easily find a publisher. This was so even though Hofstadter himself would do the highly complex, and potentially expensive, 'typesetting'. Fortunately, Martin Kessler of Basic Books was prepared to fight for it, provided he could find a non-US partner to share the risk. As the co-founder of the British publishers (Harvester Press), I had to fight for it too. On being told (by the other co-founder) that it was "totally unintelligible", I said it would either "fall deadborn from the press" like Hume's *Treatise* or become a cult book—but that we shouldn't be publishing at all if we weren't willing to accept such a hugely original, and insightful, manuscript.

A cult book, indeed, it turned out to be. And the cult was worldwide: *GEB* was translated many times. It soon won a Pulitzer Prize, and is still much admired. In 1999 the *New Scientist* magazine asked the mathematician John Casti to invite several people to choose a science book from the last quarter-century to take to a desert island; three, including Casti and myself, chose this one. (I had to make a second choice, as a result. I picked Andrew Hodges' biography of Turing: a rich book for British social history, not just for science/mathematics.)

Hofstadter explored, and insightfully interrelated, many superficially diverse topics—enough for many years on a desert island. These included the art of fugue, visual metamorphoses, translation, analogy, creativity, paradoxes, Gödel's theorem, GOFAI, *Alice's Adventures in Wonderland*, ants and bees, life, DNA . . . and connectionism.

Connectionism was presented, here, less as an AI modelling technique than as a guiding idea expressed through metaphor and analogy. Ant colonies were as relevant as brains, life as mind, memory as mathematics. The fundamental notion concerned association and interaction within a highly fluid parallel-processing system, an active memory from which higher-level properties of adaptation and intelligence emerge.

Surprising as it may seem, Hofstadter hadn't been turned in this direction by the connectionist work discussed earlier in this chapter, but by the pioneering HEARSAY program:

> Perhaps the deepest influence on me personally . . . was the Hearsay II speech-understanding system, which was developed in the mid-1970's by a team headed up by Raj Reddy, and including Victor Lesser and Lee Erman, among others. Hearsay II was a highly parallel system (it actually ran on truly parallel hardware), and it was the original implementation of the famous "blackboard architecture". I still think, personally, that the ideas in the Hearsay project are every bit as deep and as important as those in the PDP volumes. (personal communication)

As this recollection shows, HEARSAY II wasn't a typical GOFAI program. Nor was the first version of HEARSAY, in so far as it was parallelist in spirit (Newell *et al.* 1973; Reddy *et al.* 1973; Reddy and Newell 1974). Nevertheless, the original research was done within the GOFAI stable, and headed by one of GOFAI's high priests: namely, Newell. This, then, is yet another reminder that the two types of AI aren't so wholly opposed as they're often assumed to be.

Soon afterwards, besides co-editing a volume on "self and soul" with a leading philosopher of mind (Hofstadter and Dennett 1981), Hofstadter wrote several pieces on analogy and creativity in the *Scientific American* (many of these are reprinted in Hofstadter 1985*a*). He'd been invited in 1981 to alternate with Martin Gardner in a regular column, and ran it alone for a while on Gardner's retirement in 1982. And in 1983 he wrote a paper for an interdisciplinary volume which, when it was reprinted—with a new Post Scriptum—alongside some of his *Scientific American* pieces two years later, was also widely influential. The message of 'Waking Up from the Boolean Dream' was this:

> [Until] AI has been stood on its head and is 100 percent bottom-up, it won't achieve the same type of intelligence as humans have. To be sure, when that type of architecture exists, there will still be high-level, global, cognitive events—but they will be epiphenomenal, like those in a brain. They will not in themselves be computational. Rather, they will be constituted out of, and driven by, many smaller computational events, rather than the reverse. In other words, *subcognition at the bottom will drive cognition at the top*. And, perhaps most importantly, the activities that take place at that cognitive top level will neither have been written nor anticipated

by any programmer. This is the essence of what I call *statistically emergent mentality*. (Hofstadter 1983/1985: 285/654; italics in 1985 original)

However, although this paper was more explicit in comparing traditional and connectionist AI than *GEB* had been, it was still rhetorical rather than technical—deliberately so, for Hofstadter was addressing a mixed, non-specialist, audience. *Precisely how* a parallel-processing system could implement concepts—and model their role in perception, memory, analogy, and thought—wasn't explained.

In the Post Scriptum of 1985, Hofstadter mentioned the PDP group, whom he'd recently visited in San Diego. He hailed their work with "delight", as "a hotbed of subversive PDP activity" (Hofstadter 1983/1985: 654). But he gave no details. He also dropped hints, in the *Scientific American* as in the Post Scriptum, about his own computer model of concepts and creative analogy (13.iv.c)—but again, no details.

He'd given seminars on his model at MIT in 1984, and a detailed lab report was made available to the cognoscenti a few years later (Hofstadter *et al.* 1987). But official publication was long delayed (Hofstadter and Mitchell 1993/1995). Indeed, actual implementation—as opposed to mouth-watering promises—had to wait until the end of the century (McGraw 1995; Rehling 2001).

In the event, then, this intriguing but difficult work made much less of a splash than *GEB*. It didn't even enthuse the professional community, in part because it didn't exemplify the now all-conquering PDP approach. Hofstadter's model of analogy was parallel and distributed, but only partially non-symbolic. So it was connectionist only in the broad sense.

Indeed, he never considered himself part of the "connectionist" movement, and avoided their conferences (personal communication). His deepest influences from cognitive science were the HEARSAY "blackboard" model and Rumelhart and Norman's work on errors (see Chapter 10.v.e and Section ix.b above, respectively).

In 1986, of course, the *Precisely how?* question was given an initial answer in the PDP manifesto (see Section vi.a). By that time, *Gödel, Escher, Bach* and 'Waking Up from the Boolean Dream' were already part of the general intellectual background. Without such hugely popular heralds, the two technical volumes from San Diego, reader-friendly though they were, might not have found such a wide and immediate welcome.

b. Connectionist concepts

The main philosophical interest of Hofstadter's approach had been his insistence that *concepts* are active, interactive, fuzzily defined (holistic), and constantly changing. This was clearly opposed to the GOFAI picture, in which "a passive memory [consists of] data-structures [that] simply wait around to be inspected or manipulated" (Hinton and Anderson 1981: 11).

Ludwig Wittgenstein had argued for a broadly similar view, rejecting the logicism of his own—and McCulloch's—younger days (Chapters 9.x.d and 4.iii.c). For him, and for his many followers, concepts marked overlapping "family resemblances" rather than lists of necessary and sufficient conditions.

In addition, philosophers in touch with cognitive psychology knew of the experimental evidence that concepts function as "prototypes" with an ill-defined penumbra of similar cases (see 8.i.b). (Few realized, however, that James Anderson, for instance, had been relating this evidence to connectionism since the mid-1970s.) So Hofstadter's ideas had fallen on well-prepared philosophical ground.

The PDP volumes endorsed such views. In general, their authors made a point not only of describing their models clearly (ball-bearings in valleys) but also of indicating their philosophical implications. In particular, they repeatedly argued that concepts aren't cut-and-dried, but inherently fuzzy. This rhetorical strategy helps explain why their books were so crucial to the culturally prominent connectionist renaissance described in Sections vi–vii. But the philosophy would have been less persuasive without the technical details.

These showed, at last, how various puzzling phenomena could happen:

* how a single concept could be distributed over an entire network;
* how several different concepts could be stored simultaneously;
* how multiple constraint satisfaction could tolerate unclarities, and even contradictions;
* how a single unit could contribute to the representation of many concepts, and
* how it could 'mean' different things in different contexts;
* how 'one and the same' concept used in two different contexts could involve different sets of units;
* how two different inputs could be classified under the same concept;
* how semantically similar concepts could be represented by similar activity patterns;
* and how a fragmentary memory could excite a whole concept.

PDP systems, then, seemed to do 'naturally' many things which brains can do—and which might underlie the conceptual abilities long highlighted by philosophers unsympathetic to GOFAI.

Strictly, the phrase "at last" (above) is unwarranted, for others had shown these things before, as we saw in Sections ii and iv–v. But very few philosophers knew that. In England, some of those at Sussex did, largely because of Longuet-Higgins's and Hinton's presence there (I myself, for example, had been writing about connectionism since the mid-1970s). But, on both sides of the Atlantic, philosophers who weren't on the AI grapevine didn't.

This applied even to Paul Churchland. He had argued since the early 1970s that the philosophy of mind, and of science, should be based on brain dynamics (16.iv.e). But while still at the University of Manitoba, and even for some time at Stanford (where he went in the early 1980s), he was unaware of connectionist AI:

> At the time [when I wrote my 1979 book] I would have guessed that a new paradigm [based on the brain] was at least twenty-five years away, and probably more like fifty.
>
> In this I was wrong, for in fact it already existed and had existed, at least in stick-figure form, since the late fifties. By 1959 F. Rosenblatt had developed the Perceptron paradigm of vector-to-vector transformations in a parallel network of neuronlike processing units . . . Unfortunately, that paradigm did not catch on, and for two decades it was almost forgotten . . . [and] faded to invisibility. (P. M. Churchland 1989, p. xiii)

Churchland first "stumbled across" ideas about massive parallelism and vector transformations in 1983, when reading about the cerebellum (see Chapter 14.viii.b–c). He used these neuroscientific ideas to buttress his long-standing philosophical views, in essays written early in 1984 and published two years later—one of them in *Mind*

(P. M. Churchland 1986*a*,*b*). Thanks to *Mind*'s high profile, that item was widely read by philosophers. But whether it convinced many of them is another question. Its account of the processes underlying conceptual thought (and sensori-motor integration) was highly speculative, and there was no mention of existence proofs from computer modelling. It was the near-simultaneous publication of the PDP bible which *proved* to Churchland's *Mind* readers that vector-transforming parallel systems really could do interesting things. In short, the PDP volumes were a revelation to most philosophers, even including those already committed to a science-oriented approach.

Functionalism seemed to have widened its grasp, to include mental—and computational—phenomena very different from the formalist Turing tables highlighted in its initial definition by Hilary Putnam (16.iii.b). The PDP group encouraged such views, saying that:

> [PDP models] in no way can be interpreted as growing from our metaphor of the modern computer . . . [and involve] a new form of computation, one clearly based upon principles that have heretofore not had any counterpart in computers. (Norman 1986*b*: 534)

Anti-logicist doctrines that had previously seemed vague now appeared to have clear empirical support, vindicating those Wittgensteinians—notably Dreyfus—who had argued for years that GOFAI misdescribed concepts (see 11.ii.a).

Indeed, Dreyfus (and his brother) soon hailed connectionism as a philosophical advance, because it allowed sub-conceptual units and didn't presuppose that there must be an explicit theory of every domain (H. L. Dreyfus and Dreyfus 1988). Their paper was one of a set of 'reviews' written for the *Artificial Intelligence* journal, but reprinted under the tendentious title *The Artificial Intelligence Debate: False Starts, Real Foundations* (Graubard 1988). This new-found sympathy for connectionism belied Dreyfus's 1979 opinion that it had produced "no interesting results" (cf. Section iv.i).

From the late 1980s onwards, then, many people saw PDP as offering a scientifically respectable philosophy of mind that made room for mental subtleties previously ignored. However, there were dissenters.

Dreyfus himself rejected connectionism, despite seeing it as less implausible than GOFAI, saying that "building an interactive net sufficiently similar to the one our brain has evolved may be just too hard" (H. L. Dreyfus and Dreyfus 1988).

Indeed, neo-Kantians in general held that a "scientifically respectable philosophy of mind" is a chimera, since there can be no naturalistic account of meaning or normative rationality (Chapter 16.vi–viii). They saw even neuroscience as philosophically irrelevant, never mind GOFAI or PDP. And John Searle, who disagreed with them on the latter point, was no more convinced by connectionist functionalism than he had been by the GOFAI variety (see 16.v.c).

Even within the functionalist camp, PDP didn't sweep the board entirely. For the leading GOFAI computationalists resisted this newly fashionable view. The central point of contention was the relation between connectionist and symbolic processing.

c. The proper treatment of connectionism?

According to Paul Smolensky (1955–), a member of the PDP group, "the proper treatment of connectionism" was to regard it as concerned with *subsymbolic*, or *subconceptual*, processing. That is: "the units do not have the same semantics as words of natural language" (Smolensky 1988: 6), and do not correspond to the categories we use "to consciously conceptualize the task domain" (p. 5). This conscious conceptualization includes not only everyday examples, but also the verbal protocols observed in problem-solving experiments (see Chapters 6.iii.b–c and 7.iv.b).

Typically, a PDP unit represents some tiny detail or microfeature, whose individual significance may be very difficult to express in non-technical terms and quite impossible to access introspectively. In a system-modelling stereopsis, for example, each unit codes a comparison between the light falling on corresponding points on the two retinae (see Chapter 7.v.d and 14.iv.f). To be sure, the system as a whole computes the *depth* of the object being looked at. But no single unit does so. Similarly, NETtalk units represent not whole words, but Wickelgren features—and very few people have heard of those.

Even when some units do code for familiar features, as in Hinton's (1988/1990) model of Jets and Sharks (coded as married, single, divorced . . .), they don't do so in a "symbolic" way. For—as Smolensky pointed out—a PDP unit's meaning may vary according to context (i.e. the simultaneous activity of other units), whereas the meaning of GOFAI symbols is fixed.

In sum, when people introspect they aren't aware of the computations actually involved: "serial, symbolic descriptions of cognitive processing are *approximate* descriptions of the higher level properties of connectionist computation" (Smolensky 1987*a*: 103; italics added). So folk psychology, though not wholly illusory, isn't the true picture of thought. For Smolensky, as for Hofstadter, concepts and cognition properly so called exist only as emergent properties of processes occurring at the subsymbolic level. For certain purposes, that processing can be ignored and the symbolic level taken for granted. But even symbolic thinking is, at base, a PDP phenomenon—and this must be remembered, if human thinking is to be understood.

Two philosophers who agreed with this wholeheartedly were Paul and Patricia Churchland (1942– and 1943–). They had discovered PDP on moving to San Diego in late 1984—by which time the neuroscientifically oriented *Mind* paper was already "in the system". Unsurprisingly, given their earlier philosophical views, they welcomed it with alacrity.

Those earlier views had included a concern with neuroscience. Indeed, Patricia Churchland was already moving towards philosophically informed neuroscience, as opposed to neuroscientifically informed philosophy (P. S. Churchland 1986). She soon collaborated with the neurophysiologist Christof Koch and with Sejnowski, of NETtalk fame, in defining "computational neuroscience" as a distinct research area (Sejnowski *et al.* 1988: see Chapter 14, preamble). Later, she and Sejnowski would plan neuroscientific experiments together, and co-author a textbook (P. S. Churchland and Sejnowski 1992). Paul Churchland, however—while keeping abreast with neuroscience—stayed closer to 'pure' philosophy.

As we'll see in Chapter 16.iv.e, he was already well known as a proponent of eliminative materialism. This doctrine holds that the mental states named by everyday psychological language have no scientific basis, and do not really exist: beliefs, desires, hopes, regrets . . . all these are as illusory as the medieval humours, or witches (P. M. Churchland 1979, 1981). On encountering PDP, he immediately appropriated it to reinforce his long-standing eliminativism, and to develop his early ideas about the philosophy of science (1989). Later, he would apply PDP modelling and neuroscience to moral and political philosophy too (1995).

Much as Hofstadter had spoken of perception, explanation, and analogy in terms of fluid and inter-associated concepts, so Paul Churchland gave a "non-sentential" account of science—and of thought in general. He'd already argued that perception is fundamentally imbued by scientific theories, and that cognitive representations aren't language-like (Chapter 16.iv.e). Now, he said that the classical view of science—theories-as-sentences, related by deductive rules of explanation—should be replaced by a PDP-based account in which explanation boiled down to analogy.

He saw this as a truer picture of scientific knowledge and explanation, and also as a way of avoiding certain notorious difficulties that attend the classical view. Citing PDP models such as NETtalk, he argued that concepts could be represented as prototype-plus-penumbra within a (tacitly) hierarchical activation space. And he related this account to various aspects of scientific observation, learning, skilled experimentation, and reasoning—including Kuhnian paradigm shifts, and the stubborn opposition that typically greets them (1989: 191 ff.).

Anticipating the objection that this was psychology, not philosophy, Churchland said that philosophers must respect the facts about how scientists think—not least, because only thus would certain seeming difficulties (about the role of simplicity in science, for instance) be resolved. Against Popper and the neo-Kantians alike, he argued that *even if* rationality and epistemology are irreducibly normative notions, facts about how concepts are implemented in the brain can be philosophically relevant, since normative claims have empirical presuppositions which may in fact be false (1989: 195–6).

Churchland insisted that when the system is learning some generalization, it is "*theorizing* at the level of the hidden units, exploring the space of possible activation vectors, in hopes of finding some partition or set of partitions on it that the output layer can then exploit . . ." (1989: 179). He even said:

> [It] is clear that no cognitive activity whatever takes place in the absence of vectors being processed by some specific configuration of weights. That is, no cognitive activity whatever takes place in the absence of some theory or other.
>
> This perspective bids us see even the simplest of animals and the youngest of infants as possessing theories . . . [Their] theories are just a good deal simpler than ours, in the case of animals. And their theories are much less coherent, less organized, and less informed than ours, in the case of human infants. Which is to say, they have yet to achieve points in overall weight space that partition their activation-vector spaces into useful and well-structured subdivisions. But insofar as there is cognitive activity at all, it exploits whatever theory the creature embodies, however useless or incoherent it might be. (pp. 188–9)

Many philosophers, not least the neo-Kantians (Chapter 16.vi–viii), would predictably disagree. But computational philosophers had been accustomed to the assimilation of perception to scientific theorizing ever since the "New Look" pioneered by Bruner and Gregory (see Chapter 6.ii). So one might have expected them to be sympathetic.

Some were. Paul Thagard (1988, 1989, 1990), for example, was already developing a philosophy of science similar to Churchland's, and implementing various computer models accordingly (Holland *et al.* 1986; Thagard *et al.* 1988; Holyoak and Thagard 1989). Indeed, he used these empirical studies to challenge the Fregean orthodoxy

(2.ix.b) about the relation between logic and psychology, and spoke of "revising logical principles in the light of empirical psychological findings" (Thagard 1982).

Others, however, were not.

d. The old ways defended

Fodor in particular was not. Although strongly influenced by Bruner in the 1950s, by the early 1980s he was arguing for "mental modularity" (Chapters 7.iii.d and vi.d, and 16.iv.d). This led him to reject Churchland's account of perception as theorizing (Fodor 1984).

Perception (Fodor argued), being modular, is *not* theory-laden through and through. Admittedly, evolution may be said to have built certain assumptions, even "theories", about the world into our perceptual apparatus. But these aren't changeable: our fundamental perceptual processing isn't cognitively penetrable by the theories of science. So the sorts of perceptual shift envisaged by Churchland even in the 1970s, and by Thomas Kuhn before that, are impossible. (For Churchland's reply, see his 1989.)

Fodor had other reasons, too, for rejecting connectionism. In a widely read critique co-authored with the psychologist Zenon Pylyshyn, he described PDP as old wine in new bottles—and fatally adulterated, at that (Fodor and Pylyshyn 1988; cf. Pylyshyn 1984).

The old wine, here, was associationism—which had been laid down in philosophy's cellar three centuries ago (see Chapter 2.x.a). Expressing a "gnawing sense of *déjà vu*", he claimed that Hebb and Hull had been "conclusively" refuted twenty or thirty years before, and that PDP had merely added frills to their ideas. Indeed, this seemingly modern high-tech approach threatened to lead us back over 200 years, to "a psychology not readily distinguishable from the worst of Hume and Berkeley" (Fodor and Pylyshyn 1988: 49, 64).

More specifically, Fodor (contra Churchland) accepted the psychological reality of belief, desire, and other folk-psychological categories—which he glossed, significantly, as "*propositional* attitudes" (Chapter 16.iv.c–d). As such, they involve sentence-like mental representations, composed of concepts and processed by unconscious formal rules. These rules aren't mere emergent patterns of behaviour, but have psychological reality in their own right. Similarly, concepts aren't inherently fuzzy patterns of activation or prototypes-plus-penumbra, but semantically interpretable formal symbols, whose meaning—unlike that of Smolensky's "subsymbolic" units—is fixed irrespective of context (see also Fodor 1998*a*: 22).

These disagreements were closely linked to another. Fodor (and Pylyshyn, who regarded even visual imagery as "propositional": Chapter 7.v.a) insisted that PDP, in principle, couldn't model sequence and hierarchy in general, or language and conceptual thought in particular. A holistic pattern of activation, they argued, doesn't have the sort of internal structure that's required by the compositionality, productivity, and systematicity of language.

Specifically, they said, the holism of PDP can't allow that the meaning of a sentence depends recursively on the meaning of its component parts, enabling the production of indefinitely many new sentences. Nor can it admit that a concept has the same meaning irrespective of context. And nor can it satisfy the generality constraint (G. Evans 1982), that one and the same concept can be used in indefinitely many systematically related thoughts. For instance, someone able to think *John loves the girl* must also be able to think *The girl loves John*; and someone who thinks that roses are red and violets blue must also be able to think that roses are blue and violets red. A PDP network, said Fodor and Pylyshyn, might represent any of these thoughts without being able to represent its permutational partner, because it treats them as holistic patterns—not as *thoughts*, at all.

Lastly, Fodor—like many others influenced by Chomsky—was committed to nativism (see Chapters 7.vi.d–e and 16.iv.c–d). And nativism had been challenged by PDP. The challenge based on representational trajectories was still in the future (see Section viii.d, and subsection e below). But the past-tense learner had already thrown the connectionist cat among the nativist pigeons. This network was taken by PDP enthusiasts to prove not only that explicit rules aren't needed for using grammar, but also that an inborn Language Acquisition Device isn't needed for learning it. *Pace* Chomsky, mere statistical analysis of the input seemed to do the trick.

The most influential Chomskyan reply, which Fodor in general endorsed, came from linguists Steven Pinker and Alan Prince (1988). They crawled over the past-tense learner with a fine toothcomb, devoting 120 pages to their critique. The connectionists had claimed that the network made the same errors as children do (errors which Chomskyans regarded as strong evidence for nativism). But Pinker and Prince showed that the network's errors were *not* precisely the same as children's, and they argued that some of the errors made only by children can be explained by GOFAI but not by PDP.

It turned out later that the psychological data on which both they and Rumelhart and McClelland relied were faulty. For example, psycholinguists had reported, or anyway implied, that—for a while—children always over-regularize all irregular verbs. But they don't: they over-regularize only 5–10 per cent of irregulars, and correct uses co-occur with the incorrect ones. Moreover, psychologists hadn't reported any *irregularizations* of *regular* verbs—which, indeed, a Chomskyan would never expect. However, they do happen. Both these unexpected phenomena, and many others, fell out 'for free' from the network statistics explored in the early 1990s by Plunkett and Marchman (see Section viii.e).

Some of Pinker and Prince's criticisms of the past-tense learner could be applied to PDP in general, at least as it existed around 1986. For instance, the training set had contained no sentences (made up of nouns, adjectives, and variously tensed verbs), but only associative pairs: the *stem/past tense* of 420 verbs. Moreover, these pairs were input in two stages, in the first of which only ten (including eight irregulars) were used. In effect, then, the network had been given grammatical structure for free—or anyway, pointed so firmly at it that the learning required was trivial (compare Thornton's MTCLA: Section viii.d, above). Real parents, by contrast, don't tiptoe around their children mouthing only ten verbs.

But if the past-tense learner was in this sense too weak, in another it was too strong. Pinker and Prince complained that a connectionist learning rule could in principle learn *any* linguistic regularity, whereas children *do not* (because natural languages share certain structures, and lack others) and *cannot* (because some pre-existing bias is needed to enable interesting structure to be picked out). Even non-Chomskyans had to admit that if people find it difficult to learn a grammatical structure (mirror-image reversal of word strings, for example), then a PDP simulation should do so too. What's more, said Pinker and Prince, all the microfeatures were treated equally: the statistical learning picked out the most frequent patterns, not the most important ones. Top-down influences could counter this—but these go against the spirit of connectionism.

The question arose, then, as to what connectionism *could* offer to cognitive science. Fodor and Pylyshyn's answer, in effect, was "Not much". They saw it as a theory not of cognition as such, but of its implementation. All very well, perhaps, for neuroscience. But, given the functionalist assumption of multiple realizability (Chapter 16.iii), facts about implementation have no relevance for philosophy, or even for cognitive psychology properly so called. Newell and Simon agreed. Connectionism deals with the microstructure of (the implementation of) thought, but nothing below 100 milliseconds of brain activity, on their view, is significant in the study of cognition (Newell 1980, 1990).

In short, these computationalists still insisted that language and thought must be explained in formalist terms. The philosophy of mind needs the language of thought (alias LOT: Chapter 16.iv.c) and/or Physical Symbol Systems (16.ix.b), not connectionism. As Fodor put it, both then and later, a GOFAI-based psychology is "the only [theory of cognition] we've got that's worth the bother of a serious discussion" (2000*b*: 1).

Worth the bother or not, serious discussions of PDP soon abounded. The disputes outlined above triggered a huge, and still growing, philosophical literature. Paul Churchland's work had been provocative from the start, and it continued to raise philosophical hackles. As for Fodor and Pylyshyn's critique, Smolensky published a counter-blast even before it was officially published—to which Fodor soon replied (Smolensky 1987*b*; Fodor and McLaughlin 1990). Other professional philosophers soon joined in.

e. Microcognition and representational change

The most important newcomer to the debate was Clark. Besides bringing a taste of PDP connectionism to a general philosophical audience in an Aristotelian Society paper (1990), he provided countless details in a highly influential book, *Microcognition* (1989). This described PDP technology in plain English, and related it to many issues in the philosophy of mind and language—including the disagreements mentioned above.

For instance, Clark countered Pinker and Prince by pointing out (among other things) that "importance" could be coded by assigning higher weights to some microfeatures than to others, and that Karmiloff-Smith had already outlined how GOFAI-like processing might develop from a PDP base (see Section viii.d).

He also rejected Churchland's eliminativism. Individual thoughts (beliefs, hopes, intentions . . .), he said, could conceivably map onto identifiable computational states in the brain, even though these states aren't sentence-like. And even if they don't, it doesn't follow that they aren't "real". The reason is that the categories of folk psychology are holistic interpretations of behaviour, logically independent of the underlying causal mechanisms (cf. 16.iv.a–b). Ironically, Clark's anti-eliminativism was directed also against the arch-realist Fodor:

> Fodor's approach is dangerous. If one accepts the bogus challenge to produce syntactic brain analogues to linguistic ascriptions of belief contents, he opens the Pandora's box of eliminative materialism. For if such analogues are not found, he must conclude that there are no beliefs and desires. The mere possibility of such a conclusion is surely an effective *reductio ad absurdum* of any theory that gives it house space. (A. J. Clark 1989: 160)

As for Fodor and Pylyshyn's arguments about systematicity, Clark wasn't convinced by these either—though he admitted that productivity was more problematic than sequential order (1989, ch. 8; 1991*b*). His main point was that compositionality needn't be built into the basic architecture, but could emerge from it.

The core idea, here, was mental modelling (see Chapters 4.vi, 7.iv.d, and 14.vii–viii), together with the PDP group's suggestion that our reasoning capacity may result from "our ability to create artefacts—that is, our ability to create physical representations that we can manipulate in simple ways to get answers to very difficult and abstract problems" (Rumelhart *et al.* 1986*c*: 44; cf. A. J. Clark 1989: 223). Given that words are external (publicly accessible) things, which can be modelled in the mind/brain just as physical objects can, Clark saw the compositionality of thought as derived from that of language. That is, the brain somehow creates a *virtual* machine with formalist properties—and conceptual thought is a property of this machine. It follows, said Clark, that GOFAI-explanations of thought and language aren't mere *approximations* to finely detailed subsymbolic accounts, as Smolensky had claimed, but are *true* (or in some cases *false*) descriptions of the relevant virtual machine. (The implication was that only humans have language-like internal states; for Fodor, by contrast, even animals—if they have representations at all—possess some innate language of thought: see 16.iv.c.)

As we saw in Section ix.b, Hinton had recently suggested a model that tallied with Clark's approach (Hinton 1988/1990; cf. A. J. Clark 1991*b*: 215). Each concept, Hinton had said, might have *two* representations: a localist (single-feature) "reduced description", and a distributed "expanded description" involving many microfeatures. The first might be manipulable in a GOFAI-like way, even though the second isn't. But the reduced description isn't an empty syntactic token, as a GOFAI symbol is (cf. Searle 1980), because it is linked to the expanded one—whose microfeatures can be accessed if the concept's *meaning* needs to play a role. As Clark put it: "Classical representations, minus such a sub-structure, threaten to be brittle and contentless shells" (1991*b*: 217). In sum, "even when a classical virtual machine is somehow implicated in our processing, its operation may be deeply and inextricably interwoven with the operation of various connectionist machines" (1991*b*: 215).

Clearly, then, Clark—despite being the leading philosopher of connectionism—was never committed to *pure* PDP, the view that "Every cognitive achievement is psychologically explicable by a model that can be described using only the apparatus of PDP" (1989: 128). This position, he said, was just as implausible as its GOFAI-equivalent. Even in his first book, he argued for mental "multiplicity":

> I am advocating that cognitive science is an investigation of a mind composed of many interrelating virtual machines with correct psychological models at each level and further accounts required for the relations between such levels. Only recognition of this multiplicity of mind, I suspect, will save cognitive science from a costly holy war between the proponents of PDP and the advocates of more conventional approaches. (A. J. Clark 1989: 141)

Whether any of these virtual machines is as closely GOFAI-like as Fodor claimed was another matter. But this, for Clark (as for Hinton), was an empirical question. As for the answer, he was agnostic: he closed his second book by saying "No one knows" (1993: 227).

If Clark's second book still didn't answer the question about the relevance/irrelevance of GOFAI, it did offer some important advances. Earlier, he'd described symbolic reasoning as "merely ingenious icing on the computational cake" (1989: 135). Now, he asked how that icing could have been concocted in the first place. In other words, his interests had shifted from established (static) conceptual abilities to representational change. He now tried to show *how* "mental multiplicity" could develop from a PDP base. And his answer described the scaffolding of learning by various recodings of existing representations (1993, esp. chs. 7–8)—see Section viii.d.

As we've seen, he'd recently collaborated on this issue with a leading *developmental* psychologist, Karmiloff-Smith. She was one of a group of developmentalists who were reinterpreting the idea of innateness (Elman *et al*. 1996). Specifically, they'd revived Piaget's biological concept of *epigenesis*: a series of self-organizing interactions between pre-existing mechanisms and the environment (both in and outside the organism), by which the embryo/infant bootstraps itself into maturity (Chapters 7.vi.g–i, 14.ix.c, and 16.vii–viii). Unlike Piaget, this group had the advantage of PDP techniques and developmental neuroscience. Like Clark, then, they asked how a connectionist system—whether natural or artificial—could undergo representational changes in epigenetic development.

In other words, "Is it innate?" was no longer being treated as a *Yes/No* question (cf. 7.vi.g–i and 9.vii.c–d). The new "Minimal Nativism", as Clark termed it (1993, ch. 9), drew the sting of the nativism debate, for it stressed both inbuilt and bootstrapped processing biases ("knowledge"), as well as environmental interactions.

By the 1990s, there was a wide range of empirical research (not all done from a computational perspective) suggesting what these biases and interactions might be. Some philosophers interested in mental development called on cognitive neuroscience (see Chapter 14.xi.b). Others stressed the mother–child interactions that naturally scaffold the development of pointing, turn-taking, and language, and/or culture-specific scaffolding in the form of language and artefacts—what Bruner (6.iii.c) had called cognitive technology (E. L. Hutchins 1995—cf. 8.iii; Hendriks-Jansen 1996; A. J. Clark 2001, ch. 8). By the end of the century, then, research on (lifelong) conceptual *development* was influencing the philosophy of mind and cognitive science.

f. Non-conceptual content

Interest in development was evident also in philosophical work on the relation between concepts and non-conceptual content. One might say that these correspond to Hinton's reduced and expanded descriptions respectively, but that would be misleading: the distinction arose within 'pure' philosophy, and—as we'll see—was linked to connectionism later.

Non-conceptual content is meaning (intentional content) carried by sensory-perceptual experience and/or action that's untouched by language. One familiar example is the content *bug* that's supposedly available to the frog in virtue of the 'bug-detector' cells in its retina (and the adaptive links to the muscles of its limbs and tongue): Chapter 14.iii.a.

The word "supposedly" is needed here because the notion of non-conceptual content is controversial. René Descartes had seen no need for an equivalent (and is widely reviled for callousness as a result: Chapter 2.ii.e). Even today, some philosophers argue that there can be no such thing: in their view, the very idea of non-conceptual content, or purely sensory experience, is incoherent (Chapter 16.viii.b). Many others disagree. Indeed, the suggestion that there is some middle ground between adult human experience and the mental vacuum of insensate beings has a strong intuitive appeal—not just to pet-owners and zoo-keepers, but to people wanting to understand the *human* mind.

The Oxford philosopher Gareth Evans (1946–80), for instance, argued that non-conceptual content is the base from which true concepts develop (G. Evans 1982). He recognized that it's difficult to pin it down. Does the frog really employ the content *bug*—or, rather, *bug-over-there* or *edible-object-to-upper-right*, or *black spot there*, or . . . ? The answer, he said, lies in the frog's sensori-motor skills. For to have access to non-conceptual content of a certain sort just is to have bodily skills of a certain kind. The skills aren't mere evidence for (symptoms of) the existence of the content, but are criterial of it. As for concepts, Evans defined these in terms of the generality constraint (see above). The frog lacks the concept of "bug", for it can't respond to bugs in many different ways, nor (use language to) think of them in many different contexts. In other words, it can't conceive of bugs *objectively*—which is to say that it can't conceive of them at all.

But the same applies to babies: as Piaget had pointed out long before, being able to shake and suck a rattle, or to pick it up and drop it, isn't the same as being able to think of rattles in indefinitely many ways. Evans, then, was less concerned to give dumb animals their due than to explain the origin of concepts in humans, whether in evolution or infancy.

Largely inspired by Evans's intellectual legacy (he died in 1980, aged only 34, and his book was published posthumously), Adrian Cussins (1960–) drew on connectionist ideas in trying to show how this conceptual development could take place (Cussins 1990). While still a Research Fellow (at Oxford and Stanford) in the late 1980s, he produced the C3-theory of cognition, so called because it dealt with the 'Connectionist Construction of Concepts'. Almost all of the C3 paper dealt with *just what counts* as content, and *just what counts* as a concept. Having discussed these two questions for sixty pages, he closed with a brief section arguing that connectionism might show how, in practice, concepts emerge from a non-conceptual base.

Theories based on GOFAI (such as Fodor's), he said, don't explain the origin of conceptual content but instead take it for granted. In this, he was echoing Searle, for whom GOFAI was "all syntax and no semantics" (Chapter 16.v.c)—and, for that matter, Fodor himself (in his discussion of methodological solipsism: 16.iv.d). Both Searle and Fodor had argued that GOFAI can't explain how concepts in someone's mind can refer to objective facts or events. Cussins suggested that his theory can do so, for it shows how there can be "organisms *in* the world which are capable of thinking *about* the world" (Cussins 1990: 368).

Following Evans, he took non-conceptual content to be the representation of the world that is implied by the possession of language-free bodily skills. These include visual navigation, knowing the position of one's limbs, and catching a bug with one's tongue. Our friend the frog has such skills—but no concepts. We need to explain both how non-conceptual content can arise and how concepts can progressively be constructed from it. Concepts and objectivity (the mind–world distinction) are essentially connected, and to explain objectivity we must show how conceptual representations can have semantic properties such as reference, truth, and falsity. As Cussins put it:

> We need to understand how there can be a principled, if not sharp, distinction between creatures like paramoecia and creatures like us, between infantile and adult cognition, perhaps also between normal and demented cognition. With a fair grasp of the principle of this distinction, we can *then* address . . . how best to model the computational processes that can transform a creature from lying on one side of the distinction to lying on the other side. (Cussins 1990: 414; italics added)

His answer (again, following Evans) was that "contents are non-conceptual in virtue of being perspective-dependent" (p. 425), and that concepts can be constructed from a non-conceptual base by learning increasingly perspective-independent abilities. In humans, this process culminates in the "emergence" of objectivity and the mind–world distinction. At that point, the person satisfies not only the generality constraint, but also objectivity conditions such as referring to specific objects in the world, assessing truth-values, making valid inferences, and—in general—making judgements and perceptual discriminations that are independent of their own perspective.

So far, so philosophical: this was a theory of C2 (the Construction of Concepts), not C3. Connectionism now entered the picture, albeit briefly. Cussins commended Smolensky's account of the "proper treatment" of connectionism, and rebutted Fodor and Pylyshyn's claim that connectionists can't explain systematicity. And he suggested that this methodology might be able to show how concepts arise in practice (pp. 429–37).

However, he didn't discuss any specific models illustrating the movement towards objectivity that he had in mind. He might have cited Marr's theory of vision (it was mentioned in a footnote, but in a different context). For this had indicated how a connectionist system might construct decreasingly perspective-bound representations (see Chapter 7.v.b–d). From the retinal image, through the Primal Sketch, to the $2\frac{1}{2}$D sketch and beyond, Marr had pictured a subjective-to-objective progression satisfying some of the criteria noted by Evans and Cussins.

He might have cited Hinton too, for Hinton had already outlined how a visual network could construct increasingly viewpoint-independent representations of arbitrary 3D shapes (Hinton 1981; cf. Boden 1988: 80–6). ("Outlined", because he'd modelled his abstract theory only in toy systems.) Indeed, Hinton would have been a better example for Cussins's purposes than Marr, because his objective representations didn't depend, as Marr's did, on top-down influences from ready-made concepts (object models).

An even better example was soon provided by Ronald Chrisley (1965–), a Sussex philosopher with hands-on programming skills who developed Cussins's suggestion about a connectionist base for objectivity (Chrisley 1990, 1993; Chrisley and Holland 1995). He designed a recurrent PDP network called CNM (Connectionist Navigational Map), which would enable a robot to learn to navigate (to reach "home"), gradually decreasing its perspective-dependence as learning proceeded.

A series of experiments showed how various types of spatial generalization were, or were not, generated (in the hidden units) by distinct representational codes. For example, how could CNM (first) learn to identify specific locations, and (then) to relate them to each other more or less systematically? (This wasn't straightforward, for the robot's "sensations" and "actions" weren't associated one-to-one: the same action sometimes yielded different sensations, and different actions might yield the same sensation.)

CNM could be seen as an exercise in robotics. Chrisley compared it also to empirical work on learning (artificial) grammars, on spatial maps in the hippocampus, and on the development of object permanence in children. But his primary aim was philosophical: to clarify what sorts of representational trajectory might count as "increasing objectivity".

Cussins and Chrisley were both firmly rooted in analytic philosophy (they were fellow graduate students at Oxford in the 1980s). But their work on objectivity exemplified the growing stress on *embodiment* that characterized cognitive science in the 1990s, and which was often expressed in terms of the very different phenomenological tradition (see Chapters 15.vii and 16.vi–viii). Indeed, Cussins—and Clark (1997, 2005), too—moved increasingly towards that approach (Cussins 1992).

By the end of the century, then, Dreyfus was only one of many proclaiming the importance of bodily skills. Even Evans had highlighted such skills, in his account of non-conceptual content. But neither Dreyfus nor Evans had discussed connectionist implementations of intelligence, and Cussins had done so only in the sketchiest terms. Chrisley, by contrast, tried to show how specific PDP networks might be used to illuminate the grounding of objective thought in the dynamic interplay of bodily action and perception.

Although Cussins was exploring a core assumption of connectionism—that concepts can emerge from a subsymbolic base—he had fewer readers than Fodor, Churchland, or Clark. He was a philosopher's philosopher, whose highly abstract and closely argued work bristled with technical terms. Indeed, his C3 paper had been rejected as "too philosophical" by the editor of the interdisciplinary *Behavioral and Brain Sciences*, himself a philosopher—namely, Stevan Harnad (Cussins, personal communication).

He influenced the field, nevertheless. Paul Churchland was so impressed by the C3 paper that, shortly after it appeared, he invited Cussins to join him at San Diego. *Mind* published his second paper on content and embodiment soon afterwards (Cussins 1992). Clark referred to C3-theory in developing his own account of representational change (A. J. Clark 1993: 73–6). Chrisley spelt out the third 'C' in C3 in some detail, as we've seen. And Brian Smith (1996) was strongly influenced by Cussins's publications on embodiment, and (even more) by his ongoing research on content, objectivity, and ontology. As we'll see in Chapter 16.ix.e, Smith was attempting a major upheaval in the philosophy of mind and computation, in which insights of the analytical and phenomenological schools would be combined.

g. An eye to the future?

I said above (viii, preamble) that the late-century connectionists didn't try to tackle propositional reasoning. However, Clark (2005, in preparation) has very recently outlined a research programme aimed at doing so—and at taking *embodiment* seriously in the process. As a philosopher, he won't be doing hands-on modelling. But he will be taking modelling techniques into account—and empirical psychology and neuroscience too.

He sees language as a form of cognitive technology, whose origin and use can be understood as an aspect of embodied cognition (Clark 2003*a*; and 16.vii.d). Its epistemic artefacts aren't axes, pencils, or computers but publicly audible/visible words—and internal representations of them, and of the motor actions we've learnt to associate with them. At base, these are forms of perceptual imagery, not language-specific types of representation.

Clark isn't the only philosopher, today, to stress the embodied, perceptual, aspects of language. A similar idea imbues Jesse Prinz's radically empiricist philosophy of mind (Prinz 2002). And cognitive linguists such as Larry Barsalou and Rolf Zwaan have recently developed "situation semantics", which holds that words don't *have* meaning so much as *prompt* it (Barsalou 1999*a*,*b*; Zwaan 1999; Feldman and Narayanan 2004).

The abstract aridities of Montagovian semantics are here left far behind (see 9.ix.c). For on this view, language works by eliciting specific perceptual and motor imagery in the hearer/speaker, who thus *simulates* the relevant semantic content. Situational linguists can call on psychological studies of visual imagery (e.g. Stanfield and Zwaan 2001), and on fMRI (brain scanning: see 14.x.b) data showing that words elicit activity in the neurones that are normally active when the person senses, or enacts, the relevant thing or action (e.g. Tettamanti *et al.* 2005). They can also call on seventy years' work on schema theory, thanks to Frederic Bartlett and his many successors (see 5.ii.b, 7.i.c, and 14.vi.c, and Fauconnier and Turner 2002).

The idea that the concepts named by words may be based in concepts derived from the body isn't new to cognitive science. For instance, George Lakoff and Mark Johnson suggested a quarter-century ago that abstract concepts, such as *justice*, may be metaphorical extensions of body-grounded concepts, such as *balance* (Lakoff and Johnson 1980; M. D. Johnson 1987); and some even earlier claims about the bodily origins of language, including syntactic structure, are cited in Boden (1981*a*). What's new is the greater attention to psychological *processes*, that is, the suggestions about *how* language works within our minds.

Applying this approach to the language of *belief*, *desire*, and *intention* (the core vocabulary of Theory of Mind), it may seem that "simulation theory" has won out over "theory theory", at last. However, this depends—as we saw in Chapter 7.vi.f—on *just what* sub-personal cognitive machinery is involved. And that's not easily discovered. The current empirical evidence for simulation semantics has resulted from presenting either single words or isolated sentences describing simple actions. But fMRI can't distinguish *Man bites dog* from *Dog bites man*, nor reflect *reasoned series of propositions*. One of Clark's aims is to grasp those nettles.

He reminds us that a biological niche containing self-constructed nests, or webs, enables birds or spiders to do things they couldn't do without them. Similarly, a cultural niche containing axes and pottery (and shipboard instruments: 8.iii.a) enables us to do things, and to think things, that would otherwise be impossible. In addition to specific artefacts such as these, the spatial organization of objects can be used as a memory-prompt enabling us to perform potentially difficult tasks with relative ease. Consider, for example, the physical arrangement of the instruments aboard ship (8.iii.a), or of unfilled glasses on a bartender's counter (K. Beach 1988); and remember the many times you've positioned the salt and pepper pots to make a *non-spatial* point.

But language, given what linguists call "the arbitrariness of the sign" (9.iv.c), is even more generally useful than is arrangement-in-space. Words can stand in for non-spatial and/or abstract items and relations, from sameness/difference to justice. It thus provides a "superniche", in which we can represent *all* external epistemic tools. Uniquely "poised between the inner and the outer, the public and the private", it acts to "stabilize, anchor, and scaffold individual thought and interpersonal coordination" and helps to create "our selves" (A. J. Clark 2004: 725).

This explains the special power of language. Words lead us to do things which we wouldn't, couldn't, have done otherwise. They do that by selectively *directing our attention* to other items in our minds—including other words, and representations of bodily actions. As Clark puts it, "Words aren't meanings, but clues to meanings." (Here, he's quoting Elman, who in turn was quoting a remark made in conversation by Rumelhart many years ago—Elman, personal communication, and 2004: 301; for Rumelhart's explanation of it, see Rumelhart 1979: 85.)

Associative processes can link internal representations directly with each other, without any current engagement with the external environment. For instance, we can remember something we did, said, or thought yesterday. In general, we can monitor and control our own thoughts (beliefs, intentions, moral principles . . .) by using language to focus our attention on them—an ability that makes human 'freedom' possible (7.i.g). In short, our high-level knowing can be 'decoupled' from our sensori-motor behaviour, despite being mediated by representations that are grounded in embodied perception and bodily action. It's this, Clark argues, which causes the undeniable discontinuity between *Homo sapiens* and other species, despite the continuity of the basic psychological mechanisms (i.e. perception, association, and bodily skills).

Clark mentions many examples of empirical work which support his view. For instance, chimps trained to discriminate between sameness and difference pairs (e.g. cup–cup and cup–shoe) by picking out a red or blue tag are able later to learn to recognize higher-order sameness and difference—as in cup–cup and shoe–shoe, or cup–cup and cup–shoe (Thompson *et al.* 1997). Chimps who'd learnt the lower-level discrimination but *without* using a separable token to mark it couldn't learn to recognize the higher-level property. The experimenters suggested that what the tag-trained chimps were actually doing was to compare *a memory of a red tag* with *a memory of a blue tag*—which is a lower-order problem. (So Thomas Evans's task for his ANALOGY problem, one might say, was to provide his NewFAI program with the relevant tags: 13.iv.c.)

Another example cited by Clark is work done by the Paris-based group led by cognitive neuroscientist Stanislas Dehaene. This shows that arithmetical reasoning involves both verbal and visuo-spatial representations (Dehaene 1997). Experiments on animals and infants indicate that a primitive visuo-spatial sense of number has evolved independently of language, but can be supplemented by language in mature human beings (Spelke 1994; Dehaene *et al.* 1998*a*). Prior to linguistic development, there seem to be two core visuo-spatial "codes" for counting: one exact (for numbers up to between three and five), and one approximate (for larger quantities). Exact calculation of larger numbers requires language (Pica *et al.* 2004).

Experiments with bilingual subjects display systematic differences between arithmetical reasoning that's exact or merely approximate, and which relies on over-learned associations in one language or the other (Dehaene *et al.* 1999). These differences concern not only the time taken to complete the task, but also the locus of fMRI-scanned brain activations. And they appear to depend on the extent to which the reasoner employs *words functioning as perceptual cues*. For exact calculations, the solution is faster if the sums are presented in the language in which they were taught—and the brain's language area is activated. For approximate calculations, the language-of-teaching is irrelevant, and a different (parietal) brain area is activated.

Clark sees these results as showing that what we normally regard as high-level reasoning may be grounded in visual perception, but improved by words—which are *essential* for certain reasoning tasks. Even for those tasks, however, words function by means of learnt associations to language-specific perceptual (auditory and visual) imagery.

All very interesting . . . not least, because it can be seen as a twenty-first-century version of Hume's talk of "impressions" and "ideas" (2.x.a). (It's also an updated version of the Whorfian hypothesis, at least where vocabulary is concerned: see 9.iv.c.) But there are two major, and familiar, problems: hamsters and hierarchy.

If language and reasoning rest on perception and association, or in other words what Elizabeth Bates called "sundry old parts" (see 7.vi.c), why can't hamsters learn English? Why have all the attempts to teach language to non-human animals foundered, just as Descartes predicted (2.iii.c and 7.vi.c)? We've seen that Clark rejects Chomsky's nativist answer, and Fodor's too. He also rejects Kim Sterelny's (2003) recent suggestion that the brains of newborn humans today are representationally and computationally deeply different from those of early hominids, due to some combination of genetic changes and the effects of our unique developmental cocoon. He admits (personal communication) that there must be *some* inherited difference between animals' brains and ours, but has nothing convincing, nor even intriguingly plausible, to say about just what it might be.

As for hierarchy, the rock on which Burrhus Skinner's theory of language foundered (9.vii.b) and—as we've seen—one still unconquered by PDP, Clark ignores it. His project focuses on individual words and over-learned phrases, not whole newly generated sentences. Apart from saying (correctly) that a representation of structure X doesn't have to be X-structured, he has nothing to add about hierarchy.

So why mention his work here? In particular, why mention a largely speculative project, still (in February 2005) in the process of grant application? Why not confine the discussion to already peer-reviewed, or anyway already drafted, research reports? This book, after all, is history—not futurology.

The reason is that Clark's chosen problem is highly important, with significant implications for every discipline within cognitive science. Maybe he won't achieve the breakthrough that he's hoping for. But he'll probably come up with something of interest—a better understanding of attention, for instance. Psychologists have done a huge amount of work on this, including much which today would be described as the study of consciousness (6.i.a–c and 14.ix.a), but the concept is still far from clear. Again, his work may help to fill some of the gaps in situation semantics: the currently popular "blending theory", for instance, abounds with hand-waving about *just what* computations might be involved, and *just how* they can be triggered (Fauconnier and Turner 2002). Other goodies may ensue too—though we can't be sure of that now (see 17.i). Science-as-she-is-done, and philosophy-in-the-making too, isn't always a matter of dotting the *i*s and crossing the *t*s. It also involves speculation, hunches, boldness . . . in a word, risk.

12.xi. Pointing to the Neighbours

Connectionist AI/psychology had two next-door neighbours, and another directly opposite. The next-door neighbours were experimental psychology and neuroscience. The one facing across the street was symbolic AI.

These fellow residents spoke to each other less than one might think. As we've seen, a large part of connectionism's project was *negative*: to deny GOFAI's claims to ground a science of the mind. So cosy chats on the doorstep weren't very likely there. But even neighbour neuroscience was largely neglected by the connectionists I've discussed in this chapter. Indeed, the neglect ran both ways: for many years, most neuroscientists were territorially suspicious of computer models brought onto their patch (see 14.iv.d and v.c).

There were some exceptions. Clark, for instance, clearly realized that if his philosophy of mind/language is to be vindicated, it must be consistent with the data of experimental psychology and neuroscience. And the Churchlands drew even more inspiration from neuroscience. However, very few philosophers of connectionism paid much attention to either. Indeed, many of the *hands-on* connectionists, especially those coming from physics or engineering, near-ignored these experimental sciences too. Even if they considered psychology carefully, which they didn't always do, they sidelined neuroscience. (Again, there were exceptions: some individuals, such as Sejnowski and J. R. Anderson, came into the field from neurophysiology.)

Their networks were inspired by the brain, to be sure. But they focused on mathematical analyses of general computational properties: pattern recognition, distributed representation, associative memory, learning, development, and the low-level grounding of high-level thought. They took on board some very general features of biological neurones, but didn't pay attention to detailed aspects of the nervous system. Their work wasn't much help, for instance, if one wanted to know *just how, in fact,* a frog manages to catch a fly with its tongue, or *just how, in fact,* someone recognizes their grandmother.

Potentially, the connectionism and neuroscience neighbours had much to teach each other. Both were concerned with the probabilistic properties of cell networks (cf. 14.ii.b); and hypotheses about what *computations* (what information processing) the nervous system is performing could guide neuroscientific questions about the bodily mechanisms involved. In some cases, the party wall between the next-door residences was pretty thin. In other words, the distinction between connectionism and computational neuroscience was fuzzy.

With advances in neuroscience, it's getting fuzzier. One indication of that is that a recent textbook written by two students of McClelland is significantly more neural/biological than the first PDP volumes were (O'Reilly and Munakata 2000). Indeed, it's boldly subtitled *Understanding the Mind by Simulating the Brain*. Published at the turn of the century, it presages a new phase of research for the new millennium. To be sure, associative memory isn't being explained directly in terms of ion channels.

But the brain—and even ion channels—is being given more attention than before. (In part, of course, this is a result of neuroscientists' having learnt more about it.)

Twentieth-century computer models that tried to cast specific spotlights on the brain will be considered in Chapter 14. Before talking to the next-door neighbour, however, let's look at what happened to the neighbour across the street.

The spectacular rise of PDP didn't cause GOFAI to collapse. If Fodor and Pylyshyn hadn't given up on the symbolic approach, neither had their GOFAI colleagues. And much of what they were doing had implications stretching beyond technology, into cognitive science.

13

SWIMMING ALONGSIDE THE KRAKEN

The long-submerged connectionist Kraken identified in Chapter 11.vi surfaced in 1979, and leapt into the air a few years later (see 12.v–vi). There's no denying that it posed a threat to symbolic AI. It commanded philosophical interest, stole public sympathy, and—after DARPA's U-turn in 1986 (12.vii.b)—competed for funds. But it didn't chase GOFAI from the seas.

While the monster frolicked in the limelight, symbolic AI carried on swimming steadily. It's been doing that for twenty-five years now, during which time a mini-Kraken has risen to visibility—situated, or embodied, AI. (That too, the ninth bombshell, was spawned around mid-century: Chapter 4.viii.)

This chapter describes some work from GOFAI's post-Kraken period. I've ignored natural language processing as such, because various examples of post-Kraken work in NLP were described in Chapter 9. However, it's worth mentioning that current AI models of planning and multi-agent cooperation, including virtual-reality set-ups in which a VR agent interacts with a human being (Section vi.b, below), owe much to earlier NLP research on the recognition of intentions and beliefs (see 9.xi.f).

Some of the chosen themes—VR, for instance—have a decidedly modern feel. But every one was presaged in the very earliest days of NewFAI. In short, the harbingers (10.i) were coming home to roost.

Not all, however, found a comfortable place to perch. Logicism's efforts to do so are described in Section i. Some post-Kraken criticisms of logicism, and of the explosion of expert systems dating from the early 1980s, feature in Section ii.

Next, I focus on five topics which despite being heralded in the harbinger papers attracted scant attention until around 1980. These are (in Section iii) diagrammatic reasoning; the situationist attack on planning; agents and distributed cognition; inductive learning; and (in Section iv) creativity. GOFAI's contribution to human–computer interfaces and VR is outlined in Sections v and vi, which also explore some potential psychological effects of VR.

Finally, I give a historical perspective on two common beliefs: that AI isn't a discipline so isn't worthy of respect, and that GOFAI has failed.

13.i. Later Logicism

Post-Kraken work on classical AI themes included the use of logic to represent knowledge. This research programme had been announced in 1959 by John McCarthy, and was given a huge boost ten years later by McCarthy and Patrick Hayes (10.i.f and iii.e). Since the late 1970s there have been many developments.

Some of these, as McCarthy and Hayes predicted, concern classical metaphysical questions. For instance, Judea Pearl's long-standing logicist research has focused on causation and counterfactuals, on probability, and on the nature of scientific explanation (Pearl 1988, 2000; Halpern and Pearl 2005*a*,*b*). It's hardly surprising that it has drawn the attention of professional philosophers. For causation was grist to the metaphysician's mill even in Aristotle's time; counterfactuals have vexed logicians ever since the Middle Ages; probability has perplexed philosophers for 200 years; and scientific explanation was much discussed in the twentieth century (e.g. Popper 1935; Salmon 1989; Lipton 1991).

Overall, GOFAI's logic-based advances have included work on at least three fronts. Namely, non-monotonic logic, naive physics, and an AI encyclopedia (not to be confused with an encyclopedia of AI). But whether these three (described below) should really be called "advances", as opposed to *continued research activity*, is highly controversial. For it's still not universally agreed within AI that logic is a suitable medium for KR.

It still has champions around the world. For instance, Germany's Wolfgang Bibel, asked to comment on "AI's greatest trends and controversies" for the millennial issue of a leading computer journal, insisted that "the language as well as the inferential machinery of logic [is] fundamental for the endeavor of realizing an artificial intelligence" (Hearst and Hirsh 2000: 8). But a recent President of AAAI devoted much of his Presidential Address to casting grave doubts on this (Waltz 1999). In short, by no means all of today's AI is imbued by the McCarthyite passion for logic.

a. Less monotony

"Monotony" was lessened by post-Kraken GOFAI in order to deal with the (second sense of the) frame problem—namely, situations involving incomplete knowledge (see 10.iii.e). Symbolic systems now have a flexibility way beyond what was achievable in 1980.

In ordinary (monotonic) logic, any conclusion that follows from a set of assumptions, or premisses, also follows from any *larger* set of assumptions. If the original assumptions were true, then the conclusion is true, no matter what other true premisses are added. In non-monotonic reasoning, by contrast, the entailment of the conclusion can in some circumstances be annulled.

Consider the common-sense decision to abandon one's plan to take the train, and to take the bus instead—because on reaching the station one discovers that the trains aren't running today.

Using a monotonic logic, one would have to avoid inconsistency by *revising* one's initial assumptions ("Drat! I thought the trains were running today, but they aren't. Delete that premiss") and then replanning the journey. (So-called *truth-maintenance* techniques use monotonic logic, and attempt to restore consistency whenever new information comes in which contradicts some previous assumption.) Using a non-monotonic logic, one could *add* the new premiss "No trains today", but allow it to override the older premisses so as to *avoid* drawing the conclusion that "I shall take the train today".

That's an illustration of what's sometimes called the "qualification" problem: the fact that reasoning from incomplete knowledge is unavoidably risky. If new information comes in, one may have to change one's mind in some way. Various attempts to compensate for incomplete knowledge had been discussed in the 1970s (e.g. A. M. Collins *et al.* 1975; Norman and Bobrow 1975). Among them was GOFAI work which, following McCarthy and Hayes (1969), aimed to do this without abandoning representation-by-logic.

Ways of doing that had begun to poke their heads above the water in the 1970s (Winograd 1980*b*). For instance, Uppsala's Erik Sandewall (1945–) extended predicate calculus logic by defining the *Unless* operator, which prevented the system from making inferences it would otherwise have drawn (Sandewall 1972; cf. Raphael 1971). If told both *A* and *A Unless B implies C*, Sandewall's program would set up a sub-goal to assess the truth of *B*, before deciding whether to infer *C*. (This could happen on several goal levels, as *Unless* was recursive.) Similarly, the default slot-fillers in AI 'frames' led programs to assign default values *only if* those slots hadn't already been given some other value (Hewitt 1969; Minsky 1975). And Earl Sacerdoti's ABSTRIPS prevented failures by postponing on-the-spot executive planning until the last minute (10.iii.c).

In all those cases, disaster was pre-empted by *avoiding or delaying* decisions. In "fuzzy logic" too, contradictions were pre-empted—in this case, by using probabilistic reasoning (Zadeh 1965, 1972, 1975; for an 'update', see Bouchon-Meunier *et al.* 1995). Since there was no firm commitment to the truth of the conclusion, there was no contradiction if it turned out to be false.

In non-monotonic reasoning, by contrast, decisions/conclusions were actually made—only to be withdrawn later. Work of this type began in the mid-1970s, and a small workshop on the topic was held at Stanford in 1978. By that time, several ways of enabling a logic-based program to 'change its mind' were being suggested.

McCarthy himself developed "circumscription" as a possible solution (1977, 1980*a*,*b*, 1986). The key idea was that the program should assume, at any given time, that it knows all the relevant facts. Consider the missionaries and cannibals problem, for example:

> Imagine giving someone the problem, and after he puzzles for a while, he suggests going upstream half a mile and crossing on a bridge. "What bridge", you say, "No bridge is mentioned in the statement of the problem." And this dunce replies, "Well, they don't say there isn't a bridge." You look at the English and even at the translation of the English into first order logic, and you must admit that "they don't say" there is no bridge. So you modify the problem to exclude bridges and pose it again, and the dunce proposes a helicopter, and after you exclude that, he proposes a winged horse or that the others hang on to the outside of the boat while two row. (McCarthy 1980*a*: 30)

Clearly, given the inventiveness of "this dunce", no additions to the axioms of the problem could ever defeat him.

The only strategy, said McCarthy, is to assume that all the relevant facts are known—so if bridges aren't mentioned they're not needed. In *puzzles*, like the missionaries and cannibals brain-teaser, that's actually so. But in real-life problems, *that all the relevant facts are known* is an assumption, not a datum. It may turn out to be false. If so, the problem-solver must be prepared to reconsider some of the previous conclusions. (Fuzzy logic wouldn't help, he said. For not only can we not assign meaningful probabilities to bridges or to Pegasus, but we don't even think about them. Nor should we: when would such thinking ever stop?)

As McCarthy pointed out, circumscription wasn't an example of non-monotonic *logic*, but of non-monotonic *reasoning* that augmented ordinary first-order logic. (So was Jon Doyle's "truth maintenance system": Doyle 1979. This stored the reasons for its conclusions, and used backward chaining to revise its beliefs if a reason statement turned out later to be untrue.)

McCarthy advised against altering the logic itself, because it's difficult to ensure that all such modifications are compatible (1980*a*: 37). Non-monotonic logics were suggested nevertheless, by Drew McDermott and Ray Reiter (McDermott and Doyle 1980; McDermott 1982*a*,*b*; Reiter 1980). Reiter's "default reasoning", for example, was extended by a complete proof theory for a large class of common defaults, which could be presented to a resolution theorem-prover.

It wasn't always easy to compare the various forms of non-monotonic reasoning. Reiter, for instance, said that a particular result "suggests some deeper relationship between closed world default theories and [McCarthy's] method of circumscription although I have been unable to discover just what this might be" (1980: 129). McCarthy, besides offering technical comparisons between his own approach and Reiter's (McCarthy 1980*b*), argued that default reasoning in general is less useful than circumscription, because it's less generalizable (1980*a*: 37). For example, a block *x* that isn't explicitly stated to be on block *y* is taken (by default) not to be on *y*; similarly, for block *z*; but there's no way of inferring what circumscription allows one to assume: that there are *no* (unmentioned) blocks on *y*.

In 1980 non-monotonic reasoning was the subject of a special double issue of the *Artificial Intelligence* journal. This heralded an explosion of work in the area. Some of it was soon to be featured in the widely read compendium of 1985 that also included Brian Funt's and Aaron Sloman's thoughts on analogical representation (see iii.a, below).

Many people—even McDermott himself (ii.a, below)—pointed out that none of these attempts to formalize non-monotonic reasoning could solve the frame problem in the general case. (Hayes agrees.) But not all reasoning concerns the general case. Just as expert systems can be useful if they're restricted to a certain topic, so programs can approximate common sense where the types of rethinking that may be needed can be specified beforehand. Accordingly, research on non-monotonic reasoning (and logics) continued despite the critics' strictures. And it still does (cf. Gärdenfors 1992; Besnard 1989; Antoniou and Williams 1997). Indeed, just as this book was going to press McDermott received the AAAI Classic Paper Award for an influential paper on the topic (Hanks and McDermott 1986), and for his work on "causal and temporal reasoning in a wide variety of fields" (Leake 2005: 3).

As for the frame problem, it's still exercising many billions of brain cells. Hayes himself co-edited a collection of sixteen papers on it in the early 1990s, and also took time out to write a summary for Stevan Harnad's online journal *Psycoloquy* (Ford and Hayes 1991; P. J. Hayes 1992). Unsolvable though it may be, the problem won't lie down.

b. More naivety

Just as non-monotonic reasoning was presaged in 'Some Philosophical Problems', so was Hayes' work on naive physics (1979, 1985*a*,*b*). Hoping to rescue AI from the "toy problems" so characteristic of NewFAI, his goal was to capture "a sizeable portion of common-sense knowledge about the everyday physical world: about objects, shape, space, movement, substances (solids and liquids), time, etc." (P. J. Hayes 1979: 242).

These highly abstract questions were relevant to a wide range of practical problems. Many robots then being envisaged would need some mastery of the answers; and some expert systems, as his past collaborator was saying (McCarthy 1983), would need it as well.

Hayes relied on his own intuitions about what naive physics is. He didn't run any experiments, such as those done by the psychologist Michael McCloskey (1983*a*,*b*). McCloskey asked, for example, how people envisage the trajectory of a stone (held at shoulder height) dropped by someone walking fast. He found that his subjects' expectations were usually mistaken. Most said the stone would fall straight down (although a few thought it would move backwards, to land behind the point of its release); even Ph.D. students of physics often failed to give the right answer—a parabola. In other words:

> [our misconceptions] appear to be grounded in a systematic, intuitive theory of motion that is inconsistent with the fundamental principles of Newtonian mechanics [but] resembles a theory of mechanics that was held by philosophers in the three centuries before Newton. (McCloskey 1983*b*)

McCloskey suggested that these false beliefs arise from specific visual illusions (to which the medievals were also subject), a perceptual base which makes them highly resistant to modification by reason ("many [students of physics] emerge with their intuitive impetus theories largely intact"). However, *everyone* in McCloskey's experiments assumed that the stone would fall to the ground *somehow*. Indeed, he didn't bother to ask whether or not they formed that expectation: it was too obvious to mention.

That shows why Hayes didn't run experiments. What he wanted to make explicit was *that the stone would fall*. Also, he wanted to spell out just what that means: that it would follow a smooth, roughly vertical, downward path ("parabola or straight line?" wasn't relevant), ending on the ground roughly where it was dropped ("ahead, behind, or underneath?" wasn't relevant). Likewise, he wanted to say just what it is to *drop* something. (If a piece of fluff falls from my coat, have I dropped it? If I release a bird from my hand and it flies away, have I dropped it? If I'm lying face down on the floor clutching a pencil and I unclench my fingers, have I dropped it? . . .)

His two 'Naive Physics Manifestos' were a valiant attempt to make explicit a myriad of things which even McCarthy's "inventive dunce" wouldn't have thought of mentioning. These taken-for-granted assumptions and inferences pervade our everyday action, thinking, and language. They're just as crucial for subtle NLP as for successful manipulation and flexible problem solving. (Indeed, Roger Schank's "conceptual dependencies" had been an attempt to capture some of what Hayes was trying to express more thoroughly: 9.xi.d.)

Hayes asked, for instance, what happens when liquid pours out of a tilted glass. Or, to put it another way, just what is it for liquid to pour out of a container? What generally causes it? And what generally results from it? What's the difference in meaning between *pour*, *flow*, *spill*, *drop*, and the like? How might a robot be enabled to see that a container was just about to spill its contents? And how could it select a specific action to prevent the spillage, or to pre-empt a particular unwanted result of it?

As Hayes pointed out, in our everyday thinking we don't get our answers to such questions from theoretical physics. Even when someone happens to know the physics, they don't usually rely on it (a fact which McCloskey's experiments confirmed). What we rely on is naive (intuitive) "qualitative" reasoning. What we need for AI purposes, then, is a formal representation of that. And predicate calculus, according to Hayes, should be a suitable formalism.

Naive physics, alias qualitative reasoning, became an important focus of post-Kraken research. The journal *Artificial Intelligence* published a special volume in 1980, and two brief retrospectives in the early 1990s (Kuipers 1993*a*,*b*). Two influential collections appeared within six years of each other (Bobrow 1984; Weld and de Kleer 1990), and work in this area has continued ever since (Kuipers 2001).

One question that received attention was whether one should assume, following Hayes, that naive physics is *consistent*. Andrea DiSessa (1988) said it wasn't. He posited a number of "phenomenological primitives" (not theoretical principles), such as an intuitive notion of *impetus* as an unanalysed combination of force and motion. His logically fragmented view of intuitive physics allowed different, and potentially conflicting, primitives to be activated in different situations. It was similar in spirit, then, to contemporary theories of "simple heuristics" in psychology (Chapter 7.iv.g).

Another general question, which became increasingly insistent over the 1990s, was whether a purely intellectualist approach, consistent or otherwise, can capture our everyday physical knowledge. Sceptics argued that it's our *embodied experience* of solids and liquids which gives us access to their properties. For these critics, it wasn't only naive physics which was under attack. The whole of "disembodied" GOFAI, including plan-driven robotics, was put in doubt (Chapters 15.viii and 16.vii, and iii.b–d below).

Tacit conceptual structures exist for naive psychology too (7.vi.f), and for animals, plants, and time (8.i.b and d). Their core aspects are universal, not culture-specific. And they underlie thoughts that go way beyond common sense: notably, myth and religion (8.vi). In short, all human thinking involves intuitive reasoning—what Hayes called naivety.

c. The AI en-CYC-lopedia

If all human thinking involves intuitive common sense, it follows that an encyclopedia can't be understood without the reader's relying on it. But if the "reader" is an AI program, that common sense has to be explicitly supplied. This fact (or fancy?) informed the mid-1980s hope of building an AI encyclopedia: an organized body of knowledge to be consulted not by people, but by other programs.

The AI encyclopedia was Douglas Lenat's (1950–) CYC project. With the enthusiastic backing of Edward Feigenbaum, by then the public champion of expert systems, CYC was launched in late 1984 at the newly founded Microelectronics and Computer Technology Corporation (MCC) in Texas, and officially announced in the business-oriented *AI Magazine* (Lenat *et al.* 1986).

Why would the business community be interested? Well, Lenat had found in his survey of expert systems (Davis and Lenat 1982) that time was often wasted by one AI team's effortfully representing knowledge already expressed by others. Moreover, naive physics was usually ignored, since the introspection encouraged by the interview techniques of knowledge engineering wasn't capable of capturing it. A central store of common-sense knowledge, accessible by many different expert systems, would thus be extremely useful. It was the hope of attaining this practical goal which led to CYC's hugely generous funding: $50 million, enough for "two person-centuries" of work, to be spread over ten years (Lenat and Feigenbaum 1991). (Later, the funding was upped to reach six person-centuries.)

The key idea was that concepts/facts would be represented in organized frames (10.iii.a), with just a million frames sufficing to store all (non-idiosyncratic, non-arcane) human knowledge. Lenat and Feigenbaum (1991: 210) allowed that their "million" was speculative, but pointed out that two AI leaders (Marvin Minsky and Alan Kay) and a group working on electronic dictionaries had all made comparable estimates. (These included the "back-of-the-envelope calculations" mentioned in Chapter 1.iii.g.) In other words, instead of building common sense into the logic, as default reasoning, it was to be expressed within the widely shared knowledge base (Lenat and Feigenbaum 1991).

Once the encyclopedia had reached a certain size, said Lenat, it would (like McCarthy's notional Advice Taker) be able to learn new things not by hand coding, but simply by being told. And the telling would involve not only semantic inheritance, but analogy and questioning too.

For instance, CYC might be told that *a tiger is a big, fierce, cat with black and orange stripes, living in the jungle*. The system's previous knowledge of *cats* would be wheeled in, to be modified only in respect of size, fierceness, stripes, and habitat. That a tiger is a carnivorous furry mammal, with four legs and a heart, susceptible to hunger and to gravity . . . wouldn't have to be stated anew. All that information would already be included in the 'cat' frame, and automatically inherited by the 'tiger' frame. If CYC chose to ask the human being what specific type of meat tigers eat, it could do so—and slot the answer into the relevant frame accordingly.

Moreover, jungles would have been represented as *large, hot, rainy, forests with abundant flora and fauna*. So CYC would know, without needing to be specifically told, that tigers like hot, rainy, places. Given appropriate inferential mechanisms to access such knowledge when needed, an expert system for zoo-keepers would be able to advise that the tigers shouldn't be housed on the artificial ice-floe with the polar bears.

All this, however, was easier said than done. To make CYC work would require powerful AI techniques for analogical thinking (see iv.c, below). Significant effort—and, ideally, psychological research—would have to go into choosing the central case of each class, what Eleanor Rosch had called the prototype (8.i.b). Otherwise, why not define a cat as a sort of tiger? A variety of inference mechanisms would need to be provided. Eventually, CYC ended up with thirty: some already familiar—such as inheritance, demons, and if–then rules—and others not, such as following metaphorically sensible links.

The database would inevitably contain global inconsistencies, so the CYC team would have to provide non-monotonic reasoning. They'd have to provide modal operators too, including some dealing with the non-truth-functional logic of intentions, knowledge, and belief. (For instance, if Mary knows that Jack lives at No. 5, it *doesn't* follow that she knows that Poppy's uncle lives at No. 5—even if Jack is, in fact, Poppy's uncle.) To do all that, they'd have to create "a powerful constraint language which is essentially predicate calculus" (Lenat and Feigenbaum 1991: 211–12).

The amount, and range, of knowledge to be entered into the system by Lenat's thirty staff members was daunting. It concerned "thousands of everyday activities like shopping, football, visiting doctors, and so on" (Brand 1995: 236). So Schank–Abelson scripts (7.i.c) had found a new home, expressed in CYC's new language. Last but not least, the tacit knowledge that Hubert Dreyfus had said protected humans from the frame problem was to be explicitly incorporated in CYC. That meant that all of Hayes' questions would have to be answered. CYC would need mastery of naive physics—and naive psychology and folk biology too.

Whether that "mastery" was to be conceived in logical or pragmatic terms, however, was controversial. Lenat himself leant towards the second option, and favoured it even more strongly after some years of experience in building CYC. In the early 1990s, he said this:

> We avoided the bottomless pits that we might have fallen into by basically taking an engineering point of view rather than a scientific point of view. Instead of looking for one elegant solution, for example, to represent time and handle all the cases, look instead for a set of solutions, even if all those together just cover the common cases. (interviewed in Brand 1995: 237)

The philosopher Arthur Prior (1914–69), who'd pioneered work on temporal logic, would have been turning in his grave. And the logicist proponents of "neat" AI were uneasy too. But the a-theoretical "scruffies" were more accepting. Their question, in general, was not *Is it logically consistent?* but *Does it work?*

The *neat/scruffy* distinction between thinking styles is an old one, although those labels didn't come into common use in AI until about 1980. Then, they spread throughout cognitive science as a rampant meme (8.v.c). (They originated with Schank, according to his closest colleague: R. P. Abelson 1981*b*.)

The distinction is often seen as separating those driven by respectable intellectual standards from those too lazy to be bothered. That "good guys, bad guys" dichotomy informed William Woods's published critique of Schank's NLP, for example (9.xi.e), not to mention the scathing remarks he made face to face on conference panels, and in private. But some self-confessed scruffies offered strong arguments in defence of their position.

Minsky (1994), for instance, gave various reasons for believing that in *any* highly intelligent system, "consistency and effectiveness may well be incompatible". And forty years earlier, in 'Steps', he'd already argued that various types of non-logical computation are in practice essential even for proving theorems in formal logic (see 10.iii.b). As co-author of the fiercely analytical *Perceptrons*, he couldn't be accused of being intellectually lazy or scared by logic. Neither could Alan Bundy, who outlined how to combine the strengths of the two methodologies in undergraduate teaching without being misled by "slogans", or trapped into narrow AI "apprenticeships" (1981).

As an indication of CYC's *effectiveness*, Lenat—in the early 1990s—mentioned that when asked *Show me an image of shirtless young men in good physical condition*, the system had delivered a picture captioned "Pablo Morales winning the men's 1992

Olympics 100-meter butterfly" (Brand 1995: 240). The caption was crucial here, for CYC had no visual ability. But how had that query been linked to that caption?

The annotated transcript showed that a variety of common-sense knowledge had been involved. Some items were logicist axioms, such as *If X won event Y, then X participated in event Y*. Some were property-inheritance frames, implying for instance that *All Olympic men's events are instances of men's sports competitions*. Some were frames including common-sense default assumptions, such as *Participants in sports competitions are young* and *Participants in sports competitions have an athletic physical build*. Some were axioms grounded in common-sense knowledge, such as *Men who are swimming wear swim trunks*. And some were McCarthyite circumscription assumptions, such as *If CYC can't infer that a person is wearing X, assume he isn't wearing X*. So although CYC couldn't *see* that Morales was athletic, or shirtless, it found his picture in response to the user's request nevertheless.

One may wonder just how far this proffered example was a set-up. As in Lenat's Automated Mathematician (AM) program (Section iv.c, below), cynics might suspect that certain heuristics or frame-slot fillers had been provided precisely *in order that* specific 'discoveries'—such as this one—would be made. Such doubts could be allayed by inspecting CYC's data and finding that it included very much more than sport and swimming trunks, and by testing it with unexpected queries about a wide range of topics.

Lenat's original plan had been for CYC to achieve self-learning by 1994, and "completion" by 2000. Two mid-term reports—a book, and a brief résumé for the AI professionals—appeared in the early 1990s (Lenat and Guha 1990; Lenat and Feigenbaum 1991, esp. 210–28). MCC technical reports gave further details, some too recent to be mentioned in Lenat's book: e.g. further aspects of CYC's ontology of causality; early work on its taxonomy of types of change, such as transfer, destruction, etc.; and suggestions on how it might communicate with programs having simpler ontologies—Guha and Lenat 1989. (Another brief paper aimed at his peers came off the press more recently: Lenat 1995.)

CYC was proudly announced to be "still on schedule", even though "almost ten times as many" frames were required as had been expected. The team still hoped to "finish" by 1994, where this meant:

> [reaching the point] where it will be more cost-effective to continue building CYC's knowledge base by having it read online material, and ask questions about it, than to continue the sort of manual "brain-surgery" approach we are currently employing. (Lenat and Feigenbaum 1991: 212)

As it turned out, by the end of the century CYC had a core ontology of 6,000 concepts with 60,000 facts—allowing the system access to about a million facts in all (S. Russell and Norvig 2003: 363–4). These included: trees are usually found in the open air; a person, or a robot, should hold glasses of liquid the right way up; and (a particular favourite of mine) when people die they stop buying things. Countless other examples were reported by CYC's programmers, but those three items alone indicate the range of applications they were hoping to aid and abet.

By that time, several other attempts at designing and filling large common-sense knowledge bases were under way (S. Russell and Norvig 2003: 364). And a conference series on 'Formal Ontology in Information Systems' had been established, plus various web sites describing examples of ontology research around the world (e.g. <http://ksl-web.stanford.edu/kst/ontology-sources.html>).

In part, this activity was driven by a shared vision of the so-called Semantic Web, an extension of the World Wide Web foreseen by its 1989–90 inventor Tim Berners-Lee (2000: 21, 30, ch. 13). This would be "a web of data that can be processed directly or indirectly by machines" (2000: 177). It would allow not only keyword search—by Google, for example—but also automatic communication between programs, eventually by means of the "intelligent agents" mentioned in iii.d, below. For computers on the Semantic Web will achieve "at first the ability to describe, then to infer, and then to reason" (Berners-Lee 2000: 184).

Although web pages contain much which can't be understood/processed by machines, there is "a vast amount of data in them, such as stock quotes and many parts of online catalogues, with well-defined semantics" (Berners-Lee 2000: 180). If the well-defined semantics can be standardized, and the ill-defined semantics upgraded, the possibilities for automatic data search will be hugely augmented. So "the desperate need for the Semantic Web" (ibid.), given the explosion of information present but not practically accessible on today's web pages, is behind much of the current work on KR ontologies.

It's difficult even to merge very simple databases (Doan and Halevy 2005). For instance, if one database on the Web uses three descriptors for houses (*location*, *price* ($), *agent id*) and the other uses four (*area*, *list price*, *agent address*, *agent name*), how can the merging system match these schemata so as to get a sensible result? It's even more difficult to enable knowledge sharing that will support/involve simple reasoning. Consider, for example, an automated version of searching the Yellow Pages, so as to match *the books I want to buy* with *the books available from various sources* (Burstein and McDermott 2005).

Even that task is relatively cut-and-dried. The sharing of general knowledge is more taxing still. If the basic ontology of concepts like *space*, *move*, *time*, *cause*, and *animal* (and perhaps *goal*, *intention*, *purpose*, and *choice*) differs from program to program, then knowledge sharing will be faulty or impossible. Similarly, if one wants robots (e.g. Mars rovers or autonomous vehicles) to be able to communicate with each other then they must have a common ontology. Otherwise, a suggestion from robot A that robot B follow plan X in order to achieve situation Y couldn't be properly assimilated by the intended recipient.

This partly explains why, by the end of the century, the construction of "formal encyclopedias of knowledge and methods and techniques for tailoring them to particular ends", ultimately covering "all human knowledge and methods", was being listed as one of the long-term goals of AI (Doyle and Dean 1997: 98). In short, the CYC team's claims, in a discussion of the foundations of AI (Kirsh 1991*a*), that something like their approach was essential for the advance of AI *in general* had been widely accepted (Lenat and Feigenbaum 1991).

Widely . . . but by no means universally, as we'll see (ii.a–b, below). In any case, these were promissory notes rather than reports of actual achievements. With respect to the latter, what verdict should one reach on CYC's contribution?

If the money men's hopes of providing expert systems with full common sense hadn't been realized, it didn't follow *even from the point of view of the neats* that nothing of interest had been learnt. CYC had taught AI workers a great deal about the problems, and some of the tentative solutions, involved in ontology building. And it had contributed also to AI thinking about KR-by-frames, non-monotonic inference, analogical reasoning, and prototype-based thought.

Something had been learnt, too, about the topics that concern metaphysicians. As was already evident from 'Some Philosophical Problems', AI logicists got some of their ideas from previous work done by philosophers. Where Warren McCulloch had been inspired by logical atomism, the later logicists drew on Prior's (1967) temporal logic; on Willard Quine's (1953*a*) account of natural kinds and Donald Davidson's (1980) of events; on Saul Kripke's modal logic and ontology of possible worlds (1963, 1980); and on Richard Montague's model-theoretic semantics (9.ix.c) . . . to name just a few. Two AI scientists who've worked in this area (and on CYC) for many years have concluded that

> The discipline imposed in AI by the need for one's theories to "work" has led to more rapid and deeper progress than was the case when these problems were the exclusive domain of philosophy (although it has at times also led to the repeated reinvention of the wheel). (S. Russell and Norvig 2003: 363)

Whether these results were worth the initial $50 million, never mind the larger sums provided later, is controversial. Minsky, for one, wanted an even larger investment:

> [One] can imagine a system that acquires most of its knowledge by experience. [However, in 1994] we still don't have suitable learning techniques. Lenat and I agree in the view that in order to learn as a person does, one will need to begin with a considerable body of built-in knowledge about a variety of effective ways to learn. The problem is we have not done enough research yet to know how to do this . . .
>
> It seems to me there is a tragic aspect to the present situation. Over the last 15 years, almost all theoretical effort has gone into seeking alternatives where none exist. Consequently, all those years have slipped by, with no project other than CYC under way. The result is, we're all waiting to see how Lenat's work comes out, while doing nothing to help or compete . . .
>
> I find it heartbreaking there still are not a dozen other such projects in the world, while there are thousands each of attempts committed to logical deduction systems, situated action and autonomous robot schemes, feed-forward neural networks, and rule-based expert systems. Each of these has particular virtues, but none of them show much promise of making inroads into that problem of scaling up the exploitation of large accumulations of knowledge. (Minsky and Riecken 1994: 27–8)

Those words "tragic" and "heartbreaking", unusual as they are in scientific discourse, show that Minsky firmly believes a much-improved CYC to be possible—if only we try hard enough, and dig deep enough into our communal pockets. Indeed, his sci-fi novel *The Turing Option* features a futuristic CYC much as Arthur C. Clarke's *2001* featured the futuristic HAL (A. C. Clarke 1968; H. Harrison and Minsky 1992). (For the record, Minsky had been a technical consultant for the film *2001*; since Clarke's book was based on the Clarke–Kubrick screenplay, one could even say that Minsky was part-responsible for that novel, too.)

Lenat may not see CYC as a close rival to HAL, but he does hope that a future version will (for instance) help Alzheimer's patients to remember things they used to know, or prevent someone who's just broken their leg from receiving online advertisements for running shoes (Brand 1995: 239, 242). What's more, he thinks it's already good enough to be useful. In 1994 he founded a company called CyCorp, whose officially declared aim is "to create the world's first true artificial intelligence, having both common sense and the ability to reason with it". He launched an open-source version (OpenCyc) early in the new century (later than originally planned, as the expected release date of April 2001 was delayed). Clearly, then, he expected many people to find it practically serviceable. (A wide range of potential applications are listed on the web site: <http://www.cyc.com>.)

13.ii. Choppy Waters

If GOFAI was still swimming, it was doing so in choppy waters. The late-century critics of logicism and applied AI were provoked, and emboldened, by the huge attention being aroused by CYC, and by the explosion of expert systems triggered by the Fifth Generation project. To some extent, their complaints had been prefigured in NewFAI days, by Dreyfus and Joseph Weizenbaum (Chapter 11.b). But there was now much more work to attack, and the new attacks were more detailed accordingly.

Predictably, perhaps, McCarthy was recalcitrant. He still insists that "the best hope for human-level AI is logical AI, based on the formalizing of commonsense knowledge and reasoning in mathematical logic" (2005: 39). But not all his AI colleagues agree. Even his co-author Hayes has had second thoughts, as we'll see.

Some post-Kraken criticisms came from social scientists who shared Dreyfus's philosophical sympathies. The British sociologist of science Harry Collins (1943–) was a prime case in point. Others came from legal researchers sympathetic to Weizenbaum, such as Philip Leith (1954–). But some came from within the AI community itself.

a. Apostasy

Logicism's opponents didn't need to send in any Trojan horses. For this long-standing area of technical research was being attacked from within.

One such assault was mounted by the computer scientist Brian Cantwell Smith. He wrote a bitingly critical paper on CYC for the multiple review in *Artificial Intelligence* (B. C. Smith 1991). Although CYC was at the bull's-eye, logicism in general was targeted in the surround.

However, Smith had always been a maverick. His radical views on the semantics of programming languages had received the accolade of being included in the major collection on KR (B. C. Smith 1985). But his further work on computation was a very different kettle of fish. Although it wasn't yet officially published (that would happen in 1996), it had been circulating in draft for some years. It was brilliant, or crazy . . . or maybe both. What it *wasn't*, was orthodox. Indeed, it was deeply counter-intuitive in a host of ways (see 16.ix.e). It was relatively easy, then, for logicists to ignore Smith's critique of CYC. If not a true outsider, he wasn't a typical insider either. His views could be more readily dismissed as a result.

More worrying, from their point of view, was the fact that the arch-logicist Hayes was growing more pessimistic about the logicist programme as a whole:

> [With respect to] the extent to which formal logical ontologies can be said to adequately capture human intuitive knowledge . . . I've become very cynical as I grow older. For example, remember

the Frame Problem? That was a warning flag: we should have thought about that a lot harder, and seen it as a critique of our inadequate ideas, rather than trying to solve it with a technical hack. (personal communication 2004)

His new-found "cynicism" covered more than the frame problem. It also applied to formalizations of common-sense notions such as knowing what one is doing; having a sense of oneself; understanding the instruction "Proceed with caution"; and making, keeping, and breaking promises. How could a program, or a robot, have such abilities?—"We don't have any idea of how to begin answering questions like this" (personal communication). He didn't (and doesn't) think that AI is impossible. But the problems of representation were more difficult than most logicists believe.

Accordingly, he now had doubts about the extent of CYC's relevance to AI. It was generally accepted that CYC would need mastery of naive physics and naive psychology. On Hayes' view, its mastery of such matters wouldn't need to match ours. It wouldn't need to know (for instance) how a robot could obey instructions to be "careful", or how it could make or break a promise. So much the better for Lenat: if CYC didn't need to know such things, he didn't need to worry about them. But so much the worse for the prospect of CYC's realizing the goal of logicist AI as a whole: "Even if CYC had succeeded beyond Doug's wildest dreams, it wouldn't be making a jot of progress towards answering questions like these" (Hayes, personal communication, 2004).

One might object that CYC *would* have to understand promises, if it were to include entries about contracts or international treaties, and carefulness if it were to include items about legal negligence. Lenat could point out, in reply, that it had never been intended as an encyclopedia of politics or law. But some logic-based expert systems had been—and are now—intended to deal with legal matters such as these (see subsection c, below). So Hayes' late-century views on the difficulty of formulating naive physics and, especially, psychology were very uncomfortable for committed logicists to hear.

At least Hayes stopped short of accusing logicism of *impossibility*. But to the consternation of many in the AI community, another leading figure in the development of logicism had now abandoned it entirely. This was genuine apostasy, and as such highly disturbing.

McDermott, who'd tried so hard to define a non-monotonic logic, was now describing the solution of the frame problem as impossible in principle. In a paper cheekily called 'A Critique of Pure Reason' (1987), first given at a small AI meeting at the end of 1985, he argued—against his own previous convictions—that the dream of representing everyday knowledge by predicate calculus (even with modal knobs on) is unattainable.

This 'Critique' was unexpected, and all the more shocking for that. For he'd only recently published several logicist papers on cognitive science, and an AI textbook recommending the McCarthy–Hayes approach to beginners (McDermott 1978, 1982*a*, 1985; Charniak and McDermott 1985). Now, he undermined it. His new paper criticized some well-known cases, offered a general diagnosis of the "meagre results" attained so far, and rebutted six familiar logicist "defences" (including reliance on non-monotonic logics).

So, for instance, he faulted Hayes' work on naive physics and Stanley Rosenschein's (1981) on planning. These were among the best exemplars of logicism but, McDermott argued, they'd failed. As one illustration, Hayes' (1985*b*) account of liquid pouring into a container such as an open bath or a closed tank included "[what] seems like a beautiful pair of arguments". On closer inspection, however, Hayes had deduced a *reductio ad absurdum* so as to refute an assumption, but had then inferred that an assumption not thus refuted was therefore *proved*.

As for the most highly visible, not to say notorious, example of logicism, namely CYC, this—said McDermott—was doomed to failure. It didn't follow that nothing of interest to AI (and psychology, and even philosophy) would be found out by Lenat's project. As he'd remarked in his 'Natural Stupidity' paper (1976), failures can be instructive. But Lenat was hoping for more.

For all that, McDermott remained committed to GOFAI in general. He *wasn't* emulating Dreyfus's attacks on the formalization of common-sense knowledge, for his faith in formalizability was intact. His doubts concerned axiomatizing knowledge, not programming it.

The crucial logicist fallacy, he said, is to confuse deduction with computation, so to assume that all thinking is essentially deductive merely because—as he still believed—it's computational. (In 'Natural Stupidity' he'd already accused AI of misusing the term *deduction*: 11.iii.a.) This explained the paradox of an expert philosophical logician such as Hayes making the apparently elementary mistake noted above. The various efforts by AI logicists, himself included, to define non-monotonic logics had failed to deal with everyday temporality, for instance (McDermott 1982*a*). Yet we, as normal human beings, deal with it successfully as a matter of routine.

The inferences actually made by people, or by programs, may of course be formally describable even if they're not strictly deductive. Indeed, McDermott still retained his faith that this is so. However, since axiomatic analysis is restricted to deductive domains, non-deductive AI programs can be *scientifically* understood only by means of a general theory of non-deductive inference. This, McDermott pointed out, had long been sought by philosophers but not found.

Admitting the troubling possibility that much of AI may be dead-end research "buoyed by simple ignorance of the past failures of philosophers", McDermott still hoped nevertheless that its concepts and techniques will help us discover the general theories which traditional epistemology has not. Until then, on his view, there can be no comprehensive science of intelligence.

Some people assumed that this meant that AI is mere pragmatism, a technology with no scientific pretensions and, in particular, no implications for cognitive science. In other words, all one could say in 'justification' of a non-deductive AI program was *Look, Ma: it works!* But that wasn't—and isn't—McDermott's view.

He's still convinced that AI can help us understand intelligence as a *computational* phenomenon, even if it isn't an axiomatic/deductive one. So in his recent book *Mind and Mechanism* (2001*b*), he argues that programs based on probability theory or heuristics aren't deductive. They can, however, be rationally justified, in that they prevent consideration of irrelevant possibilities, and/or prune these possibilities more efficiently than other programs do. Perhaps the human mind does the same sort of thing.

b. Can the fox catch the rabbit?

A rabbit may elude the fox because although the fox sees it, and wants it, he can't run fast enough to catch it. Or the fox may not realize that the rabbit's there, so doesn't even try to catch it. Both sorts of elusiveness found analogues in logicism, and in expert systems too. And both led to attacks on late-century GOFAI.

Consider the observant but slow-running fox, first. The reasoning used by NewFAI's DENDRAL and MYCIN had been restricted to theoretical chemistry and IF–THEN logic. By the end of the 1970s, however, some people were trying to include reasoning of other kinds—causal inferences, for example. So an expert system designed for cardiology included an early causal model of how the several components of the heart lead to distinctive ECG, or electrocardiogram, traces (Bratko and Mulec 1981; cf. Bratko *et al.* 1988). Such research was driven by McCarthy's harbinger-hope of giving AI programs common sense.

That was a tall order. Obviously, satisfying McCarthy's hopes would be a useful achievement. (The rabbit was clearly visible.) But even if it was in principle possible to formalize common-sense notions such as *cause*, which in itself was controversial, it certainly wasn't going to be easy. (The fox would have to run very, very fast.) A great deal of AI work in the final decades of the century was aimed at that goal, as we've seen.

And besides, there was the (second) question of the visibility of the rabbit. In other words, there was the question whether professional expertise is describable, even in *words*, in its entirety. A fundamental assumption of the expert-systems enterprise was that it is. (The rabbit could be seen if the fox looked hard enough—and if seen, it could eventually be caught.) However, that assumption had been roundly criticized well before expert systems came on the scene.

Sometimes, it was criticized merely by a telling example. For instance, as early as 1950 the computer scientist Lord Bowden had said:

> a machine is unlikely to be able to answer satisfactorily such a question as this. "If a man of twenty can gather ten pounds of blackberries in a day and a girl of eighteen can gather nine, how many will they gather if they go out together?" (Bowden 1972, p. vi)

As he went on to observe, "it is problems like this which dominate the thinking of ordinary human beings".

Sometimes, however, the assumption was dismissed more systematically, in terms of a specific epistemological theory. The chemist–philosopher Michael Polanyi (1891–1976) is a good example here. He had long stressed the importance of "tacit" knowledge in science (1958, 1966, 1967). He allowed that tacit knowledge, once identified, can be made explicit. But he held that some (unspecified) tacit knowledge will always remain:

> Tacit knowing is the fundamental power of the mind which creates explicit knowing, lends meaning to it and controls its uses. Formalization of tacit knowing immensely expands the powers of the mind, by creating a machinery of precise thought, but it also opens up new paths to intuition. (Polanyi 1966: 18)

Polanyi had used his critique to oppose philosophies of science, such as Karl Popper's, which took scientific knowledge to consist only in (explicit) theories, hypotheses, and empirical data. But it was later applied by his followers to expert systems, too.

One of the younger generation of scholars deeply influenced by Polanyi was Dreyfus. Another was Collins—who was led to consider AI largely by Dreyfus's critique of it.

Dreyfus had pointed out, for example, that if you tell an automated travel agent that you'd like a flight to San Francisco leaving "a little earlier" than its first suggestion of 6.30, you don't want it to come back to you with a flight leaving at 6.29 (Pagels *et al*. 1984: 340). Moreover, adding a rule saying *"Earlier" means at least fifteen minutes earlier, and at most two hours earlier* would often help—but not always. As Dreyfus put it, this expert system "doesn't contain any knowledge of human temporality—what spans of time are important to human beings".

Towards the end of the century, Collins explored that line of thought in writing at length about expert systems (1989, 1990; Collins and Kusch 1998). His prime motive was to attack cognitive science, to persuade his readers that *mind-as-machine* is a philosophical absurdity. That's not our main interest here (but see Chapter 16). Rather, our question is what worried him about post-Kraken expert-systems *technology*, and what AI could learn from him about its own practices.

What it *could* learn, not what it *did* learn. Partly because they associated him with Dreyfus, most AI workers didn't bother to read Collins, or didn't take him seriously if they did. He certainly wasn't a friendly voice. Nevertheless, he said some things worth hearing.

In comparing rule-following expert systems with human experts, including scientists, Collins showed that our knowledge is indeed largely tacit. So far, so familiar—from the point of view of the AI practitioner. For by the time Collins's book appeared, knowledge engineers—whether they'd read Polanyi or not—had long recognized that professional skills are largely unstated. Indeed, the interview techniques they'd established had been developed precisely in order to uncover normally hidden inferences and assumptions, including informal heuristics that wouldn't be found in the textbooks (see 10.iv.c).

But Collins had two more arrows in his quiver. One was that scientific understanding is largely a matter of practical, bodily, skills. As such, these skills can't be fully described in verbal/logical terms. Again, this was familiar territory to readers of Polanyi. Another sociologist of science, Donald Mackenzie, would soon make the same point—in a reassuring, not apocalyptic, spirit—with respect to nuclear-weapons technology (see 4.ix.a). But Collins made it with specific reference to expert systems.

He drew much fascinating evidence from his closely detailed case study of laboratory work in crystal growing, and of an attempt to write an expert system for this domain (1990, chs. 10–12). This evidence cast doubt on whether AI programs could be relied on to interpret new experimental data. "DENDRAL had done pretty well", an AI defender might say. But Collins would have replied that the novel spectrographic data were perceptually monitored by a human being, to check for noise, before being passed on to the program (1990: 128–32). Without such monitoring, DENDRAL could easily have been misled (cf. the discussion of BACON in H. M. Collins 1989).

In addition (the last arrow left in the quiver), Collins argued that *using* an AI system requires one to draw on background skills and assumptions that aren't normally made explicit. Indeed, he said, the same applies with respect to using a hand-held calculator, or even adding on one's fingers.

For example, in counting on one's fingers one must be able to recognize a finger, to remember when a finger has been used, and so on (Collins 1990: 48). Any or all of these skills might be lost after neurological damage. And in using a calculator to work out someone's height in centimetres, one must know what type of answer counts as "correct" (pp. 53–8). To say that a man who is 5 foot 9 inches tall has a height of 175.26 centimetres, because the calculator says so, is *not* to use the machine properly.

In that case, of course, the calculator does at least *do the arithmetic* correctly. But Collins described several intriguing examples where the computer gets the arithmetic wrong (pp. 66–70). For instance, 7 divided by 11, multiplied by 11, came out on one machine as 6.9999996. On another, the same sum resulted in 7; but *the very same machine, set to work more "accurately"* also delivered the answer 6.99999988079071. In such cases, an arithmetically sensible human being is needed to retrieve the situation. If they have some glimmering of understanding of the machine concerned, so much the better—but this isn't strictly necessary.

Moreover, using numbers, never mind using computers, is a community-specific practice. It's governed by norms of correctness that one learns—or not—from one's culture. These norms don't merely influence the numerical exactitude we deem suitable in describing someone's height, but also determine what doing arithmetic *is*. (The same applies to mathematical proof, and to its automation: Mackenzie 1995.) "In lands remote from Western culture", said Collins, "the calculator is no more an arithmetician than two sticks rubbed together are a slide rule" (p. 71).

Similarly, one must have some sense of the purposes and limits of an expert system to be able to use it properly. In other words, an expert system *purely by itself* doesn't, and can't, supply reliable answers. It can do so only when used by a person. And that person, besides having common sense and a vast store of worldly knowledge, is a member of a community, whose norms of "correctness" matter:

> The intelligent computer is meant to counterfeit the performance of a whole human being within a human group, not a human being's brain. An artificial intelligence is a "*social prosthesis*". In the Turing Test the computer takes part in a little social interaction. Again, when we build an expert system it is meant to fit into a social organism where a human fitted before. An ideal expert system would replace an expert, possibly making him or her redundant. It would fit where a real expert once fitted without anyone noticing much difference in the way the corresponding *social group* functions. (1990: 14–15)

Although he didn't say so here, it had been the express intention of many expert-systems builders that their programs be consulted *in lieu of* human experts. The resulting consultation might not happen "without anyone noticing much difference". But in the absence of the AI system, it might be that no consultation would happen at all. (Human experts are expensive, and geographically focused too.)

Collins didn't dismiss the AI enthusiast's claim that expert systems can be useful, even *very* useful. By 1990, after all, their usefulness had been demonstrated over and over again. And Polanyi himself had said that formalization "immensely expands the powers of the mind". For many practical purposes, then, Collins's argument could be ignored. But it came into its own in contexts where expert systems were being considered for purely automatic use. If Collins was right, the human being should never be taken out of the loop.

During the 1970s, very few people had been considering doing that. Even Kenneth Colby's plans for computer psychotherapy, already denounced as "obscene" by Weizenbaum, hadn't suggested that the psychiatrist be sidelined, nor (yet) that patients self-administer the computer therapy in their own homes (see Chapter 7.i.a). At that time, expert systems clearly weren't good enough to be left to run the show by themselves.

In the 1980s, however, expert systems entered deeply into the business world—and the military world too. Many in the AI community found this deeply worrisome, and some passionately resisted the US government's "Star Wars" plan for using automatic AI experts in defence (see 11.i.c). Most of that resistance drew its power from their expert realization that the *technical* correctness of huge AI programs couldn't be guaranteed. But some of it was based on considerations similar to those which Collins would publish a few years later.

For instance, Henry Thompson (an NLP specialist at the University of Edinburgh) told the salutary tale of a nuclear near-disaster which had been averted only because common sense—and a sense of human community—had led someone to doubt what the computer was saying (H. Thompson 1984). A nuclear red alert in the USA had been caused by an unknown object on the horizon. The reason why this frightening episode didn't escalate further was that someone ruminated that the Soviets hadn't been making especially threatening remarks recently. The norms of political behaviour, even during the Cold War, therefore made it highly unlikely that this mysterious object was a Soviet attack. And the same norms deemed it inadmissible to launch defensive nuclear weapons on the basis of such weak—i.e. *politically* implausible—evidence. Accordingly, the computer was overridden. (The unknown object eventually turned out to be the rising moon: 11.i.c.)

That's not to say that whenever someone uses their common sense to contradict the computer, they're right. Consider another salutary tale, the story of the 1979 Three Mile Island accident in Pennsylvania (Reason 1990, ch. 7). There, a nuclear meltdown very nearly happened (and all children and pregnant women were evacuated, by order of the Governor) when someone sensibly decided to ignore the computer's advice—"sensibly", because the advice would have been disastrous *in any circumstances other than the highly improbable situation which actually obtained*.

That particular decision might have been prevented by a better human–computer interface. For it happened largely because the operator was overwhelmed by myriad dials changing simultaneously: nearly 200 alarms went off within the first few minutes (Michie and Johnston 1984: 57). After the official inquiry into this crisis, the instrumentation in some manned control rooms was simplified accordingly. But there's no way of avoiding such mistakes entirely.

Nevertheless, if the person in Thompson's moonrise story, with their community-sensitive political antennae, had been taken out of the loop then he might not have been there to tell the tale, and you might not have been here to read it. Many less dramatic examples were discussed in warnings given by other AI-friendly writers besides Thompson. For instance, people working in or sympathetic with the field of expert systems gave various warnings about their use in psychotherapy (7.i.a), law (see below), medical diagnosis (Dennett 1986), and other socially relevant areas (Boden 1977, ch. 15; Council for Science and Society 1989, chs. 3–5).

Some Western AI scientists advised using the Japanese term "job assistant systems" instead, because it made the responsibility of the human user, and the (partial) inferiority of the program, more evident (Council for Science and Society 1989: 5). Some warned their fellow AI professionals not to mislead the public by hyping their programs, and asked that they try to prevent the marketers from doing so too—backed up, preferably, by an official "Code of Conduct" for the profession (Bundy and Clutterbuck 1985; Whitby 1988). And some even included specific warnings in their programs and/or the accompanying literature, to ensure that users didn't ascribe real intelligence or, worse, responsibility to the machine (Sieghart and Dawson 1987).

In sum, Collins's hostility to AI in general and expert systems in particular didn't prevent him from having some highly pertinent things to say. Whether many AI researchers (as opposed to Collins's philosophical sympathizers) actually bothered to read them is another matter.

c. Matters-in-law

As a special case of the type of worry raised by Collins, and one which raises many issues within other areas of cognitive science, let's consider expert systems in law. After outlining (in this subsection) how these have been developed over the years, we'll look (in the next) at some of the "human" problems regarding their use today.

When Herbert Simon modelled Supreme Court decisions as wiring diagrams in the 1940s (6.iii.a), he was doing this *post hoc*: there was no suggestion that the electronic circuits might be used by the judges themselves—still less, that they might take over. And when Lucien Mehl, at the famous NPL meeting in 1958 (6.iv.b), envisaged using automated logic for legal inference he was scolded in the discussion by Yehoshua Bar-Hillel for being "very premature, to put it mildly" (Mehl 1959: 783).

Premature, it certainly was. But in principle, not totally unrealistic. In 1976, when Weizenbaum published his impassioned critique of "AI judges" (11.ii.d), a few people were already starting to speculate about how legal reasoning might be modelled by AI.

Bruce Buchanan (with Thomas Headrick of Lawrence University) had written a speculative paper on this topic for the *Stanford Law Review* some six years earlier (Buchanan and Headrick 1970). And two years after that, in April 1972, the Stanford Law School had held a 'Workshop in Computer Applications to Legal Research and Analysis'. Most of the twenty-eight participants were lawyers (including one from the early-established Institute of Computing and Law in Toronto). But they also included Feigenbaum, Buchanan, and Stanford's L. Thorne McCarty (1944–).

Whereas Feigenbaum and Buchanan (and Headrick) talked about AI and expert systems in general, McCarty (1973) reported on the "pilot" for TAXMAN. For despite his modest disclaimers "I don't yet have any results . . . [only] tentative and incomplete [remarks]" and "Most of what I describe has not yet been implemented" (pp. 1, 4), he'd already started programming the first expert system in law.

TAXMAN's area of expertise was the taxation of corporate reorganizations. Its aim wasn't to do arithmetic ("You owe $350,000 in tax") but to identify tax-free, or relatively tax-free, reorganizations. In addition it should argue for *and also against* the case that a particular reorganization would be free of taxes. The data—including rules defining legal concepts such as *stock*, *voting stock*, *equity*, *control*, *acquisition* . . .—were represented in a semantic net, modifiable at any time. (Interaction with users, McCarty intended, would improve the system continually.) Every assertion was linked to an additional data structure giving its legal justification—and also "some indication of how it can be subsequently attacked" (p. 6).

McCarty saw TAXMAN as having wide potential. Part of his reason for choosing corporate taxation as the topic was that

> the law here has an interesting history: it shows a complex interplay between broad common-law judicial concepts and specific statutory declarations, and this developmental process is one aspect of the law that I want the TAXMAN program eventually to focus upon, however difficult that may be. (McCarty 1973: 2)

For example, the crucial concept of "business purpose" was probably *not* capturable by a set of legal rules, but was grounded in "a complex set of arguments based on a vast collection of knowledge about [corporations] and the purposes of the reorganization laws" (p. 10). It remained to be seen how far this could be captured by AI heuristics. If case law were ever to be included, this would require programs for dealing with analogies/disanalogies between cases: a problem "more important and interesting than any I have talked about so far", and "the really challenging part of the research" (pp. 12, 14).

But even in the case of failure, he said, everyone would gain. On the one hand, legal analysis would be as helpful for advancing AI in general as chess, blocks world, or even mass spectrometry (i.e. Feigenbaum and Buchanan's DENDRAL: 10.iv.c). On the other, the attempt to write legal AI programs would provide insights into jurisprudence and the nature of legal reasoning.

McCarty was right (Rissland *et al.* 2003). However, he was a lone voice at that time. Buchanan was interested, as we've seen. But his energies were focused more on chemistry (DENDRAL) than law. It would be some years before any serious attempts, other than McCarty's (1977, 1980), were made to design legal reasoners as opposed to speculating about them.

The most influential of these was due to the PROLOG-implementer Robert Kowalski. In the mid-1980s, he and his student Marek Sergot would be invited to participate in an IJCAI Panel Discussion on the legal implications of AI (Boden *et al.* 1985). Whereas some panellists focused on questions about legal responsibility for the use/non-use of expert systems in general, Kowalski and Sergot discussed computer representation *of the law itself*. Today, their influence is still strong: many current legal AI systems are based on their pioneering example (and Sergot has been a President of the International AI and Law Association).

Their first exercise in this area was a PROLOG version of the British Nationality Act (BNA) of 1981 (Kowalski and Sergot 1985; Sergot *et al.* 1986; Kowalski 1992). Soon afterwards, they produced comparable programs representing legislation in other areas: immigration, government grants to industry, pensions, and taxes; and sick-pay regulations (Sergot *et al.* 1986: 385).

They'd chosen to model the BNA for three reasons. Two are unsurprising: the BNA was an example of statute law, which is "basically definitional in nature" and so less difficult to handle than case law; and, being then so recent, it was still relatively free of "the complicating influence of case law" (Sergot *et al.* 1986: 370).

The third reason was less obvious—and less open to counter-cultural scorn. For they chose this piece of legislation partly because it was highly controversial. The civil rights movement in the UK was up in arms, because the recent Act had introduced several new classes of British citizenship. Instead of all citizens being equal, some were now more equal than others. "We hoped", said Kowalski, "that formalization of the various definitions might illuminate some of the issues causing the controversy" (ibid.).

His hope was reasonable. For even if—as critics of logicism would have said—the attempt to corral legal concepts of nationality within strict PROLOG definitions was doomed to failure, it might help clarify the concepts concerned. Indeed, trying to write laws in the form of expert systems often exposes unrecognized gaps or ambiguities, just as it does for psychological theories.

By the mid-1980s, for instance, one US state legislature was already drafting its regulations in programmed form for this reason (Council for Science and Society 1989: 23). Similarly, a program produced at much the same time to mimic the initial selection of applicants for St George's Hospital Medical School in London was exposing previously unsuspected racism in the human selectors' procedures (CRE 1988). The racism may have been (to borrow Weizenbaum's word) obscene, but the whistle-blowing program wasn't.

Kowalski and Sergot's early systems were never used as a matter of routine. They weren't installed in lawyers' (or civil servants') offices and let loose on legal/administrative decision making. But besides any help they may have provided to legislators in drafting, they did have a strong influence on many advice-giving systems—such as an adviser on social security entitlements (Browne and Taylor 1989).

Today, BNA-type programs are being used by companies and governments in the UK, Europe, Australia, and USA (M. Sergot, personal communication). These deal, for instance, with building regulations, safety rules, and tax laws. The human–computer interfaces are hugely better now than in the 1980s, but the logical core is broadly similar.

So Kowalski's vision has been vindicated? Well, yes and no. For Kowalski's main interest was/is in cognitive science, not technological AI. (The same applies to McCarty.) His main aim wasn't to produce AI programs for lawyers to use. Rather, it was, and still is, to understand and help to improve everyday thinking in general—even including conflict resolution where, as in the Palestine–Israel dispute, the two sides have logically incompatible goals (Kowalski 2001, 2003, in preparation; Kowalski and Toni 1996). He sees legal thought as a helpful halfway house between logic and full informality. The hardy perennial of default reasoning, for example, is somewhat clearer in legal/administrative contexts than in idle gossip about the day's latest news (Bondarenko *et al.* 1997).

However, technological AI has benefited too, for logic programming as such has been affected. Kowalski (2002) has recently suggested several extensions based on types of language and argument needed in legal and everyday contexts. One of these adds "ordinary negation" to "negation by failure" (see 10.iii.b and v.f).

Whereas Kowalski and Sergot came to AI-and-law from their expertise in GOFAI, others came to it from law and jurisprudence. Leith, now Professor of Law at Queen's University, Belfast, is a well-known supporter of the use of computers in law, but a long-standing critic of AI models of legal reasoning. He was among the first to criticize logicist programs such as Kowalski's (Leith 1986*a*).

His core complaint was, and remains, that law is not—or not usually—a matter of cut-and-dried rules, but involves social negotiations of various kinds on the part of the barristers and judges who interpret the law:

> It seems to me to be all very well to draw up a collection of rules from legislation; but, as lawyers all know intimately, a piece of legislation is but one thing in the legal world. (Leith 1986*a*: 551)

In principle, of course, certain interpretations can be favoured in an AI system. But if that's done, then other possibilities are tacitly excluded.

Leith follows up Kowalski's admission that common law is especially difficult to model in AI terms, and that a concern for precedents can complicate even statutory law. Precedents in law provide the human paradigm of "case-based reasoning", a technique whose AI versions are widely used in many different contexts (Kolodner 1992, 1993; Leake 1996). For example, it was incorporated in the MINSTREL storytelling program, enabling it to generate the concept of *suicide* from that of *killing* (S. R. Turner 1994; see iv.c below).

In case-based reasoning, a previous problem/solution is identified, somehow (methods vary), as being similar to the current problem, and is then used as a template for solving it. Various aspects of the previous case's structure, with modifications as necessary, are mapped onto the current situation. This isn't straightforward, for both the identification and the modification involve analogical reasoning.

Modelling case-based reasoning in law, as McCarty had forecast in the early 1970s, would require a very powerful theory of analogy (Rissland 1985). But analogy isn't a simple matter—which is why the initial judgement of one person (lawyer) can be altered by reasoned negotiation with another. Common sense, not to mention cognitive scientists' work on relevance (7.iii.d), suggests that programs will interpret the law in ways much more crude, and much less just, than humans do. That applies even to legal expert systems with a case-based component (e.g. Rissland and Skalak 1991; Skalak and Rissland 1992; O'Callaghan 2003; O'Callaghan *et al.* 2003*a*,*b*).

More recently, Leith (1992) has pointed out that law-in-use typically involves spoken argument, not only in court but also in preparation (identifying the "facts" and the core issues, and even negotiating with clients about their instructions). This adds further cognitive dimensions, over and above those relied on by readers of law books. And it's not only computer scientists who forget this. Academic lawyers forget it too:

> The history of academic thinking about law and legal analysis in the 20th century has been the history of the textual nature of law. This most solid strand of legal thought has almost completely dominated and blotted out all other views; it is the view that all that lawyers need to know about law sits on the bookshelves of law. (Leith 1992: 227)

Legal *practitioners*, said Leith, know better. But they don't write the law books. (They may, of course, write novels and screenplays, in which the narrative can turn on just what's said in the witness box, even on *just how* it's said; remarks in letters can have similar effects on juries: e.g. the insurance assessor's "You must be stupid, stupid, stupid" in John Grisham's *The Rainmaker*.) Deep analysis of rules is all very fine for the legal academic, Leith admitted, but "if we want to develop and encourage gifted lawyers, then [we should focus on] the communicative skill of rhetoric—as the essence of advocacy" (p. 233). This realization, he continued, "takes us far away from current research strategies in AI and law" (p. 234).

He was right: AI modelling of 'law as she is spoke' is a tall order. This is evident, for instance, from Bernard Jackson's (1995) subtle exploration of the numerous interpretative complexities. And although some current AI work does focus on the rhetorical structure of legal arguments, it's still relatively crude (e.g. Ashley 1990; C. Reed 1997; Reed and Norman 2004). Grisham needn't fear that an AI-advised rival will replace him on the best-seller lists.

Law, logic, analogy, even rhetoric . . . it's clear that this is a wide-ranging area. Both professional lawyers and AI researchers take part in the ongoing International Conference on AI and Law (the ninth in the series was held in Edinburgh in 2003). Similarly, both contribute to journals such as *Artificial Intelligence and Law* and the *International Journal of Law and Information Technology*. And very recently, in November 2003, the core *Artificial Intelligence* journal published a special issue on AI and law, with contributions from both sides of this disciplinary fence. In addition, the more general research is sometimes reported also at cognitive-science meetings (e.g. Ashley and Keefer 1996).

The technical discussions range from dedicated models of specific legislation to wide-ranging work on the psychology of case-based reasoning and argument structures. In short, insights from AI, law, and psychology contribute to what is an essentially interdisciplinary enterprise.

d. Judgements about judges

Not all of the discussions on AI and law are "technical", however. Some are ethical. Indeed, we've seen that some law programs, such as Kowalski's version of the British Nationality Act, were written partly in order to clarify morally troubling aspects of the law.

But that was clarification, not application. Now that various legal programs are actually being used, and more are in the Establishment-approved pipeline, arguments about their appropriateness abound (e.g. Susskind 1987, 1993, 2000; Leith 1986*a*,*b*, 1988, 1992; Whitby 1996*b*: 44–63). The disputants include lawyers, as well as AI and cognitive-science researchers.

Many of these disputes concern the reliability of expert-systems representations of statute and/or common law, including computational and jurisprudential questions about the relation between law and logic. Others spill over into by-now-familiar questions about how to model the flexibility of human thought. For instance, the psychologist Jerome Bruner has recently discussed the combination of system and messiness in legal reasoning; he sees this as being grounded in case-based reasoning, but stresses the *narrative* aspect of the "cases" concerned (Bruner 2002; Shore 2004: 148–9). Yet other disputes express worries about situations in which the human being might cede responsibility for judgment to the program.

In general, the concerns differ according to just who is (or will be) using the expert systems, and why.

Most law-related programs today are intended for in-house use in lawyers' offices, or by people giving advice to citizens on entitlement to benefits and other regulatory matters. Examples include the latest version of TAXMAN, and the regulatory programs modelled on the work of Sergot and Kowalski. "Merely information retrieval", you may say. But even these programs can raise Collins's worry about removing human discretionary judgement from the loop (Browne and Taylor 1989).

Others are intended for use in the law courts, to aid decisions on the verdict (in non-jury trials) or on sentencing by judges or magistrates. These are even more problematic.

For they revive the McCarthy–Weizenbaum question (11.ii.d): whether computers might ever replace judges—and if so, whether this is desirable.

Just in case you think that it so obviously *isn't* desirable that it's simply not worth discussing, you should remember the remarks of the several young black Americans quoted in Chapter 11.ii.f. For instance:

> I know that if I ever had to go before some judge, there is a good chance that he is going to see my face and he is going to think "nigger".
>
> The computer judge would have a set of rules. He would apply the rules. It might be safer. (quoted in Turkle 1995: 292–3)

The question, then, is whether such fairness could be achieved (whether "the rules" could be adequately expressed), and if so whether this would do significant harm to the human dimensions of the law courts.

Such topics are no longer maverick, and many people today share some of Weizenbaum's concerns. For instance, in 1989 the UK's Council for Science and Society, an opinion-forming group with close links to both Houses of Parliament (and other sectors of the Establishment), predicted that an expert system might in future be used to help a Crown Court judge pass sentence. So the judge might say:

> Norman Stanley Fletcher, you have pleaded guilty in this court to a series of offences. In the light of your previous record, my normal inclination would be to put you on probation. However, I have consulted DISPOSAL, the expert system [in the laptop] I have on the Bench with me. Having entered all the relevant particulars about you, it tells me that the prospects of your responding favourably to probation are only in the range of 14 to 17 per cent, and that the statistical significance of this result is very high. It also appears that the prospects of success of any other non-custodial sentence that I might consider would be even lower. Accordingly, I have no option but to sentence you to a term of imprisonment of seven years . . . (Council for Science and Society 1989: 26)

Whether a successful appeal in such a case could be based on the judge's admission that he overruled his own judgment because of "what his microcomputer 'told him' ", the Report continued, "is an intriguing question".

That wasn't a joke—even though Norman Stanley Fletcher was the lead character in a hugely popular TV sitcom set in one of Her Majesty's prisons. (You may also feel that it's no joke.) This particular section of the CSS Report was drafted by the prominent British lawyer Paul Sieghart, a few months before his unexpected death in 1988. Sieghart was the founder of CSS, and also of an influential group (Justice) for considering tricky juridical/constitutional questions, especially in the area of human rights. Several important law reforms have been instigated by him, and he's still honoured by an annual Memorial Lecture that's been given by some of the country's most renowned judges and legislators. In short, he was a provocative thinker whose provocations often ended up being accepted as common sense. Moreover, he can't be written off as someone with no human sensitivities. Sieghart may have been smiling when he wrote those words, but they were seriously meant.

Comparable worries have been addressed by Richard Susskind (1961–). He's the Gresham Professor of Law and Visiting Professor at Strathclyde University's Centre for Law, Computers, and Technology. But he's no cloistered academic. (Is anyone, nowadays?) Besides his academic posts, he's the official adviser on IT to Lord Woolf, Lord Chief Justice of England and Wales. As such, his opinions are more influential than most.

Susskind has long held that "there are no theoretical obstacles, from the point of view of jurisprudence, to the development of rule-based expert systems in law of limited scope [such as the Scottish law of divorce]" (1987, p. vii). Accordingly, he's broadly in sympathy with the BNA-based approach. Indeed, he's helped to develop several similar implementations (e.g. Capper and Susskind 1988).

With respect to the Weizenbaum–McCarthy debate, he said this (in the late 1980s):

> A detailed study of this matter is sorely needed both to dispel the profusion of misconceptions and to assure the public that while computers will no doubt provide invaluable assistance to the judiciary in the future, it is neither possible *now* (or in the conceivable future) nor desirable *ever* (as long as we accept the values of Western liberal democracy) for computers to assume the judicial function. (Susskind 1987: 249)

As for what one might expect a "detailed study" to show, Susskind was very clear about the difficulties involved:

> Moreover, any legal theorist will recognize that construction of an expert system in law that could solve *all hard cases* would require . . . the development (explicitly or implicitly) of theories of legal knowledge, legal science, individuation, structure, legal systems, logic and the law, and of (judicial) legal reasoning, all of a far greater degree of richness, sophistication, and complexity than those that have been generated by the most adept jurisprudents in the past. (p. 250; italics added)

In other words, *Don't hold your breath!* Without using inflammatory terms such as "obscene", Susskind was echoing Weizenbaum's plea for human judges.

He's repeated this plea recently, by reissuing an early paper (1986/2000) in which he expressed his "horror" at McCarthy's reply to Weizenbaum, pointing out that among the many capacities of judges which computers don't possess are "moral, religious, social, sexual, and political preferences" and "creativity . . . intuition, commonsense, and general interest in our world that we, as human beings, expect not only of one another as citizens but also of judges acting in their official role". (Consider the scorn meted out to the judge—perhaps apocryphal, or joking—who asked "Who are the Beatles?")

The 1987 quotation, above, mentions "hard" cases. But even "clear" cases may not be so clear as is sometimes assumed. Susskind, again:

> In any clear case, *according to positivists*, the judge is committed—perhaps by the value of legality—to making one particular legal decision (or else, we might say, the case is not clear). If the judge wishes to abide by the principle of legality, or for some other reason decides to remain faithful to the law, he has no choice but to make that decision, even if he finds it morally unacceptable . . . As a matter of logic, then, there is no need for judgment in clear cases; there is nothing required other than a conclusion/decision reached through the inexorable application of deductive inference procedures . . . (1986/2000: 286; italics added)

The positivist separation between law and morals, Susskind argues, is mistaken. However, many arguments intended to refute it do no such thing (pp. 282–92). In particular, arguments assuming that positivism, mistakenly, "tends to see judges as computers; as, at least in clear cases, simply certifying what in fact the law is, without necessary moral commitment" (p. 282) are often flawed. They fall foul of logical non sequiturs, to the author's failure to be "a natural lawyer" (p. 291), and—significantly—to ignorance of AI in general.

In sum, Susskind (like Collins) sees many problems regarding tacit judgements lying in wait here. Nevertheless, his views on jurisprudence don't outlaw AI from legal offices, nor even—as quasi-intelligent aide-memoires—from the judge's bench. The relatively clear cases, at least, are potential grist to the computational mill. Like Sieghart, Susskind is an influential voice in the British legal profession. So we can reasonably expect to see AI entering the law courts. Whether it will always be reasonably used there, and/or reasonably regarded by the lay members of the court, remains to be seen.

13.iii. Advance and Attack

If logicism was a key research focus right from the beginning of AI, many other themes mentioned by the NewFAI pioneers were not. They couldn't be fruitfully studied until other matters (e.g. KR) had been advanced. In the closing quarter of the twentieth century, however, they came into their own.

The result was both advance and attack. Some late-century advances delivered on promissory notes issued in the 1950s. Others went so far beyond the early efforts that they engendered a very different (i.e. non-psychological) research spirit. One development, rooted in old ideas, attacked the most cherished of NewFAI assumptions: this mini-Kraken, namely situated AI, is regarded as a 'hot' topic today. Similarly, "distributed" cognition, in which *multiple* agents (human and/or artificial) collaborate, is now "one of the main directions for AI research in coming years" (Doyle and Dean 1997: 95; cf. Grosz 1996).

a. Gelernter revivified

Herbert Gelernter's late 1950s implementation of Minsky's back-of-the-envelope sketch (10.i.c) turned out to be much more than a nine days' wonder. For the general idea of using *a model, or schema, of the problem* (of which a diagram is a special case) had a lasting effect on techniques for controlling search. Moreover, diagrammatic reasoning itself attracted increasing interest in the last two decades of the century.

For instance, in 1980 Funt described an ingenious way of implementing diagrams-as-analogue-spatial-representations (as opposed to Gelernter's diagrams-as-symbolic-line/angle-specifications). He used a simulated parallel processing 'retina' (a 2D visual array) made of many individual cells, wherein image transformations could be defined spatially rather than symbolically. One application modelled the image-rotation experiments that had caused such excitement in experimental psychology in the 1970s (Chapter 7.v.a); not only could the system decide whether one image was indeed a rotation of another, but the time it took to do so was proportional to the degree of rotation involved (Funt 1983). Another, even more relevant here, was the WHISPER program (Funt 1980).

WHISPER solved problems about stability and "chain reactions" in the blocks world. But unlike Scott Fahlmann's BUILD (10.iii.c), it didn't use abstract reasoning about

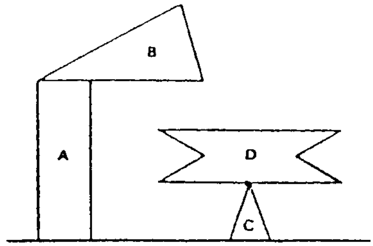


FIG. 13.1. Is block B stable? If not, what will happen when it falls? Adapted with permission from Funt (1980: 214)

physics to do so. Instead, it used qualitative knowledge about physics, and ways of continuously transforming one retinal image into another, to infer what would happen in certain situations. For instance, it could 'see'—or anyway, 'imagine'—that object B in Figure 13.1 is unstable, and that on falling it will hit object D, which will then tip over until the lowest point rests on the floor to the left of object C (Funt 1980, esp. 213–29).

WHISPER's method of representation was very different from most AI programs, and Funt's 1980 paper was chosen as one of those reprinted in the influential *Readings on Knowledge Representation* (Brachman and Levesque 1985). Given that this collection offered a thirty-page bibliography of relevant publications which hadn't been included, that was no small accolade.

Alongside it was a theoretical paper on analogical representation by Sloman (1975), whose earlier work on this theme had inspired Funt to build WHISPER in the first place (Sloman 1971; cf. 1978, ch. 7). As the editors of the *Readings* pointed out (p. 431), Sloman was one of the few people to have thought deeply about the difference between analogical and formal representations—a topic that was causing huge controversy in psychology at that time (7.v.a).

A main aim of Sloman's 1971 paper had been to clarify what's going on in "intuitive" reasoning. Since WHISPER's inferences were based on its imaginative transformations on the retina, they were intuitive rather than deductive. Indeed, this is true of diagrammatic reasoning in general. But how could logicians or mathematicians be confident that intuitive reasoning is valid?

Over the past quarter-century, Alan Bundy's research group at Edinburgh have addressed that question, seeking to justify intuitive reasoning in deductive terms. Specifically, they've worked on mathematical induction, in domains ranging from program synthesis, through arithmetic, to geometry (e.g. Bundy 1988; Bundy *et al.* 1990). In mathematical induction, of which diagrammatic reasoning is a subclass, specific results (achieved by intuitive "eureka steps") are used to justify general conclusions.

The Edinburgh group's general "theory of diagrams" enabled them to show how a diagrammatic proof can be generated, then schematized, and finally verified (Jamnik *et al.* 1998; Jamnik 2001). More recently, their interactive program "Dr. Doodle" has helped students to reason more easily about a mathematical domain that's usually represented algebraically (Winterstein *et al.* 2004). (One shouldn't assume that diagrams or visual images always aid problem solving; sometimes, they block it—Boden 1990*a*: 100–10.)

Many others, too, took up Gelernter's baton. In the 1980s, the journal *Artificial Intelligence* ran a 400-page special number on geometric reasoning (Kapur and Mundy 1988). By that time, too, researchers were able to knit new AI garments by wielding it alongside Ivan Sutherland's baton of computer graphics (v.c, below). Sutherland's pioneering ideas of the early 1960s had now been properly implemented, so that diagrams of three-dimensional structures could be generated, moved, rotated (with automatic deletion of hidden lines if necessary), and transformed. Plane geometry doesn't need such changes, but some diagrammatic thinking does.

In brief, diagrams have come in from the cold. Even McCarthy now allows (or "I would, if I had a definite idea of what a diagram is": personal communication) that they may be a more efficient form of representation for some purposes than logic is—although where logic can be used in a running program it's preferable, and it should in any case be used to specify what a program does. Today, there's a biennial interdisciplinary conference called 'Diagrams' (devoted to "the theory and application of diagrams in any scientific field of enquiry"), besides sessions and workshops scattered across various other meetings.

The 'Diagrams' series attracts people from AI and cognitive psychology, through education and human–computer interaction, all the way to architecture and cartography. So besides being highly interdisciplinary, it involves both psychological and technological AI.

b. Planning attacked—

The NewFAI researchers, one might say, had been committed Marxists. For Karl Marx had stressed the role of anticipatory planning in intelligent human labour:

> [Man] sets in motion the natural forces which belong to his own body, his arms, legs, head and hands, in order to appropriate the materials of nature in a form adapted to his own need . . . [So do animals—but human labour is different.] A spider constructs operations which resemble those of the weaver, and a bee would put many a human architect to shame by the construction of its honeycomb cells. But what distinguishes the worst architect from the best of bees is that the architect builds the cell in his mind before he constructs it in wax . . . Man not only effects a change of form in the materials of nature; he also realizes his own purpose in those materials. (Marx 1890: 283–4)

Simon, Fahlman, Sacerdoti, Allen Newell, Gerald Sussman, Richard Fikes, Nils Nilsson . . . all had agreed. Indeed, it would be difficult to name any NewFAI scientist, or any psychologist following MGP's manifesto (6.iv.c), who didn't. Many NLP researchers in the 1980s, for example, studied ways in which communicative (and other) plans can be recognized and/or put into effect (Cohen *et al.* 1990). And mainstream robotics continued to rely on planning (Brady 1985).

In the mid-1980s, that comfortable consensus was challenged. The attack came from several different research areas: behaviour-based robotics, interactionist AI, and situated automata theory.

The common theme uniting these "situationist" approaches was their focus on "[systems] having an intelligent ongoing interaction with environments that are dynamic and imperfectly predictable" (S. J. Rosenschein and Kaelbling 1995: 149). They emphasized embodiment, and rejected planning (and mental representations in general). In effect, they advised workers in AI/robotics to emulate Marx's spiders and to forget his architect. (It was even suggested—by David Cliff: 15.vii.b—that "AI" should be read not as "artificial intelligence" but as "artificial insects".)

Their work was soon dubbed nouvelle AI (which also covered animate vision, in which the creature's own movements provide crucial information: 15.vii.b). It sparked methodological changes in robotics research and philosophical changes in cognitive science. Eventually, it excited members of the general public too.

Situated automata theory was the first to make its mark. It was originated in the early 1980s by the computational linguist Stanley Rosenschein at Stanford's CSLI, and developed with Leslie Kaelbling at Brown (S. J. Rosenschein 1981, 1985; Rosenschein and Kaelbling 1986; Kaelbling 1988). The theory was used to design robots—the first, disarmingly named Flakey. But Flakey and SHAKEY, despite the rhyming similarity, were very different:

> [Situated agents] are very difficult to program because of their close interaction with the environment in which they are situated. Specifications of correctness for situated agents amount to specifications of their interactions with the environment: what action should the agent take when the environment is in a particular configuration? . . .
>
> The emphasis on an agent's connection to its environment is *an important change* from that of traditional theories of representation and control. [We] use high-level symbolic languages to describe the informational content of agents *without* requiring the symbolic structures to be implemented in the agent. (S. J. Rosenschein and Kaelbling 1995: 149–50; italics added)

Rosenschein used a logical specification of the task, and a model-theoretic semantics of motor action, to design robots driven by hardware circuits rather than programs. The circuits ensured that a specific action would take place in a specific situation, and the logic ensured that the overall task would be achieved. Flakey's designers later rejected the "folk wisdom" that representation and situated automata were incompatible (1995: 150). Nevertheless, it was clear that they understood *representation* in a way far removed from the GOFAI norm.

Their approach had some influence within AI, and was later used to design mobile robots for exploration and factory use (Kaelbling and Rosenschein 1990; S. J. Rosenschein and Kaelbling 1995). But it was too austere to excite outsiders. The most widely prominent situationists came from the other two groups mentioned above.

The most well known were the situated roboticists, hard-headed engineers building mobile (and media-friendly) gizmos to perform practical tasks. Some of these gizmos were dubbed "epigenetic robots", because their control systems and behaviour *developed* according to the features of their physical and social environment (see 15.viii.a). But intellectual waves were made also by the interactionists: maverick computer scientists who hoped to develop Heideggerian philosophy into a new way of doing AI. (Increasingly, some people fitted both descriptions: M. W. Wheeler 2005.)

The situated roboticists made four positive claims, and a negative one. Positively, they said:

1. that "intelligent" robots could be engineered rather than programmed, and would be faster, more reliable, and more ecologically valid as a result;

2. that modelling/understanding human intelligence will be possible, if at all, only if we have first modelled/understood simpler animals, such as insects;
3. that AI needs to model *complete* sensori-motor systems (what had been termed "the whole iguana": Dennett 1978*c*), as opposed to specialist AI sub-areas; and
4. that biology should be respected, not ignored.

The last claim reflected the fact that late-century neuro-ethologists were describing reflex mechanisms showing how Marx's spiders construct their delicate "operations" (Barth 2002), and how various other animals manage to live their lives.

In new-style robotics labs around the world, cockroaches and crickets inspired artificial creatures with ne'er a plan in sight (see Chapter 15.vi.c, vii.b–c, and viii.c). These were far less shaky than SHAKEY, being capable (as ever: up to a point) of walking on rough ground and climbing over obstacles, and of adaptive response in real time. By the end of the century, they would be used for applications ranging from Mars rovers to hugely popular children's toys (R. A. Brooks 1999). Some jokers, as we've seen, remarked that "AI" now stood for "artificial insects".

The negative claim was that intelligence doesn't require planning—indeed, that it doesn't need internal representations of any kind. (That was the extreme version. Sometimes, it was stated more ambiguously: the key situationist paper remarked that "much" human-level activity needs no representations, tacitly implying that logical/linguistic thought might do so—R. A. Brooks 1991*a*: 148–9.)

This claim was guaranteed to raise the hackles of functionalists (16.iii–iv)—which is to say, all traditional cognitive scientists. By contrast, it delighted people who were already hostile to cognitive science, and especially to GOFAI. Unsurprisingly, then, it spread apace. The seductive combination of telegenic robots and fierce attacks on symbolic AI attracted significant public attention—and sympathy.

Often, these new roboticists used a catchy phrase to express their view: *the cheapest store of information about the real world is the real world*. In fact, this remark was so catchy that they'd apparently caught it unawares from their predecessors. For that very phrase, and near equivalents, had been used long before by the now-scorned NewFAI folk: for instance, Feigenbaum (1969: 1011), Donald Michie and Richard Gregory (Michie 1970: 76), and even MGP themselves (G. A. Miller *et al*. 1960: 78). However, that didn't prevent its being true. GOFAI robots, such as SHAKEY, did indeed take an unconscionable time to update their world models, and to decide what to do next. Besides the huge computational expense, this put them at the mercy of events even in a sluggishly changing world.

The most high-profile iconoclast was MIT's Rodney Brooks (1954–), with Case Western's Randall Beer (1961–) as runner-up in the visibility stakes. Brooks was known to roboticists by the mid-1980s (R. A. Brooks 1986, 1987). But he opened the 1990s with a more generally noticeable splash, publishing two highly provocative papers previously available (as MIT memos) only to the cognoscenti. In them, he made the four positive claims distinguished above and drew the obvious implication: that symbolic AI was a waste of time—at least if one's "dreams" were for psychological understanding, rather than techno-dollars: "traditional Artificial Intelligence offers solutions to intelligence which bear *almost no resemblance at all* to how biological systems work" (1991*b*: 1; italics added). And he defended the negative claim too:

[In building several autonomous robots we] have reached an unexpected conclusion (C) and have a rather radical hypothesis (H):

(C) When we examine very simple level intelligence we find that explicit representations and models of the world simply get in the way. It turns out to be better to let the world itself serve as its own model.

(H) Representation is the wrong unit of abstraction in building the bulkiest parts of intelligent systems. (1991*a*: 139–40)

In a sense, this was a step back in time. For the anti-mentalist (fifth and sixth) tenets of behaviourism were being revived, even though the underlying physical/neurophysiological causes were now being considered (5.i.a). GOFAI-based computational psychology, by implication, was on the wrong track.

Despite having his home in an AI lab, Brooks's work was widely regarded by the mid-1990s as an example of A-Life, not of AI. Indeed, he often published in A-Life sources (e.g. Brooks 1992), and he co-edited some A-Life volumes (Brooks and Maes 1994; Steels and Brooks 1995). (That's why I've chosen to describe his—and Beer's—robots in the A-Life chapter, rather than this one.) Moreover, his negative attitude to GOFAI was shared by A-Lifers in general. Accordingly, the media-hyped success of situated robotics didn't persuade the general public that AI had advanced, but that it had failed (see vii.b, below).

The negative claim was argued also by the interactionist Philip Agre (1960–), initially at MIT and then at UCSD. He did much of his early work in cooperation with David Chapman at MIT, and the anthropologist (and ethnomethodologist) Lucy Suchman at Xerox PARC, who'd arrived at a similar position independently (Suchman 1987; Agre 1990).

In the 1990s, Agre would focus on human–computer interaction (Agre and Schuler 1997; Agre and Rotenberg 1997), and on human–human interaction too (his advice on the social conventions for Internet communication became a widely cited item on the Net: Agre 1998–2003). He even developed a "participatory" account of computation *as such* (see 16.ix.d). But the seeds had been sown in his interactionist research of the mid- to late 1980s.

In his MIT thesis written at that time, Agre had attacked planning even more explicitly than Brooks was doing. He said that, of all the non-classical AI work being done at the time, Brooks's was "by far the most similar project to my own" (Agre 1988: 247). But whereas Brooks started from robots and insects, Agre started from (largely introspective) human psychology and philosophy—especially Martin Heidegger. (Believe it or not, his thesis was supervised by none other than Michael Brady—not someone whom one readily associates with Heidegger.)

Calling for "an inversion of values" in both cognitive science and technological AI, he declared:

Faced with an empirical phenomenon to explain, our first explanatory recourse should be to dynamics, not to machinery. Faced with a technical problem to solve, our engineering should begin with dynamics, not with machinery. (1988: 27)

Dynamics, he said, "concerns the interactions between an individual (robot, ant, cat, or person) and the world" (p. 22).

The "ant", here, was Simon's ant, whose path across the beach was continuously determined by the sand grains it encountered (Chapter 7.iv.a). And the "person" was any one of us, engaged in our everyday activities. Most of these, said Agre, are routine, involving little conscious thought and no anticipatory planning.

Like the anthropologist Anthony Wallace (Chapter 8.i.a), Agre picked his daily journey to work as a prime example. But his approach was very different. Whereas Wallace had analysed "driving to work" in terms of Plans and TOTE Units, Agre described "walking from his home to the subway" as a routine consisting of a number of major steps, either run off automatically or triggered by interaction with (aka perception of) the environment (1988: 39–51). Even automatic actions, however, would be executed slightly differently each time, depending on the specific situation (the height of the kerb, the location of other pedestrians, and so on).

Agre's descriptions of routine activity were heavily influenced by the transactional psychologist Eric Berne (1964, 1972), the ethnomethodologist Harold Garfinkel (1967), and the anthropologist Pierre Bourdieu (1977). But he went beyond these speculative theorists by implementing his ideas in an AI program, called Pengi (Agre and Chapman 1987, 1991; Agre 1988: 189–233).

This was a LISP Machine reimplementation of a commercial video game called Pengo in which humans used a joystick and a button to control a cartoon penguin. The Pengi penguin controlled itself—or rather, it was controlled by the events happening in its simulated world. The penguin had a goal: to evade simulated killer bees, while trying to zap them by kicking virtual ice cubes at them. But it didn't Plan how to achieve that goal, nor represent it internally as a goal-state. Rather, it engaged in "an improvisatory interaction with continually evolving surroundings" (Agre 1988: 189). Instead of "elaborate world models", it used temporary "deictic" (situation-bound) representations—*the-ice-cube-I-am-kicking*, *the-direction-I-am-headed-in*, *the-bee-I-am-attacking*, *the-bee-on-the-other-side-of-this-ice-cube-next-to-me*—to organize its interactions with its environment from moment to moment (pp. 190–5).

Agre reported that the program played the game about as well as he did, although less well than Chapman. And he claimed that it played in much the same way as humans do—indeed, in much the same way as people engage in any routine activity. Planning, on his view, is neither necessary nor even possible. He remarked (as Karl Lashley had done long before him: 5.iv.a) that some skilled motor action is so fast that it simply can't be controlled by deliberate plans and decisions. Pengo experts, for instance, can push the right buttons amazingly quickly when responding to what's on the screen.

More accurately, he thought that Planning-with-a-capital-P (i.e. as envisaged by NewFAI and by MGP) is impossible. People do plan and make plans. But a plan, he said, is very different from a Plan:

> No plan could ever be so exhaustive that you could mechanically "execute" it. Carrying out a plan requires continual improvisation, interpretation, and fine judgement—especially about whether to revise or abandon the plan in mid-course . . . Real plans can be concise compared to Plans because they can rely on many aspects of how, where, and by whom they'll be used. (Agre 1988: 40–1)

ABSTRIPS, of course, had been an attempt to capture this run-time flexibility (10.iii.c). And as Agre pointed out, even "the very earliest definitions of Planning", by MGP and the LT–GPS team, had emphasized that the lower, tactical levels "can often be left unelaborated until it comes time to execute them" (1988: 235). But the elaboration, when at last it did come, was itself envisaged by GOFAI as a Plan.

Agre went even further: mental representations, the theoretical core of functionalist cognitive science, were denied. Deictic representations were admitted, but "objective" world models weren't. Mentalism was rejected:

> Mentalism refers to any psychology or philosophy organized around metaphors of Inside and Outside . . . Above all, mentalism leads one to make theories that posit objects and processes residing entirely within the head. (1988: 19)

> Mentalism provides a simple formula that provides plausible answers for all questions: put it in the head. If agents need to think about the world, put analogs of the world in the head. If agents need to act in situations, put datastructures called "situations" in the head . . . The tacit policy of mentalism, in short, is to reproduce the entire world inside the head: a "world model". (p. 20)

In arguing this position, Agre relied heavily on neo-Kantian philosophers such as Heidegger, Dreyfus, and Richard Rorty (Agre 1988: 36–7; see 16.vi–viii). But he was influenced also by cybernetics (4.v–viii) and what was about to be named A-Life (15.vii–x). That's clear from this list of "interactionist words" chosen to describe his own account: *interaction*, *conversation*, *involvement*, *participation*, *servocontrol*, *metabolism*, *regulation*, *cooperation*, *improvisation*, *turn-taking*, *symbiosis*, *routine*, and *management* (1988: 20).

"Mentalism and interactionism", he continued, "are incompatible . . . [Each] offers its own distinctive way of approaching every phenomenon of human existence." He made a seeming concession to mentalism: "Sure, perhaps some of these structures *are* entirely inside of agents' heads, but that's just an unusual special case with no particular privilege" (p. 20). However, whatever structures there might be inside our heads were assumed to be very different from the GOFAI norm. (A plan, remember, was not a Plan.)

Nouvelle AI quickly became a fashion, not to say a fad. It was reviving themes that had been neglected by most computationalists: notably, ecological validity (7.iv.g and v.e–f), animal intelligence (2.ii.d–e and 15.vii), and embodiment (16.vii). One of the people hugely influenced by its stress on reactivity and on dynamics was the young Beer (personal communication), who abandoned GOFAI partly as a result of Pengi (see 15.vii.c and viii.d).

Especially intriguing, to many observers, were the philosophical implications. The Cartesian ego engaged purely in thought (2.iii.a), and the sensori-motor sandwich (10.iv.b), were shunned—to be replaced by an alternative that had been waiting in the wings since the eighteenth century (2.vi).

Ironically, then, Brooks's (and Beer's) anti-mentalist emphasis on nuts-and-bolts engineering encouraged a renewal of interest in the type of philosophy favoured by the counter-culture. Heidegger and Maurice Merleau-Ponty, long wheeled in by Dreyfus to criticize GOFAI, were now names for cognitive scientists themselves to conjure with (M. W. Wheeler 1996, 2005). Even Brooks referred to Heidegger: he said of his own approach "It isn't German Philosophy", despite "certain similarities" to Agre's Heidegger-inspired research (R. A. Brooks 1991*a*: 155). Clearly, and whatever his private opinion (if any) about German philosophy, he realized that his name was being spoken in the same breath as Heidegger's by many of his fans.

c. —and defended

Not everyone, however, jumped on the situationist bandwagon. One who resisted it was the philosopher David Kirsh (1950–). He'd been at MIT for five years (before leaving for San Diego in 1991), so was familiar with Brooks's MIT memo on 'Intelligence Without Representation' (1987). Indeed, he organized a small Workshop on 'The Foundations of AI' at MIT's Endicott House, at which Brooks was an invited speaker.

When Brooks's MIT memo was officially published, in a special volume of *Artificial Intelligence* based on the Workshop, Kirsh's reply appeared alongside it (Brooks 1991*a*; Kirsh 1991*b*). Although specifically directed at Brooks, much of what he said ran counter to Agre as well.

Concentrating on the situationists' negative claim, Kirsh argued that any behaviour that involves concepts needs internal representations. He allowed, as Brooks had done too (1991*a*: 149), that "representation" is a weasel word, carrying multiple ambiguities. But *conceptual* representations were needed for a wide range of behaviours.

Full-blooded concepts, he said, enable a creature to recognize perceptual invariance, to reify and combine invariances (in referring predicates to names, for instance, or in drawing inferences), and to reidentify individuals over time.

They also allow creatures to engage in anticipatory self-control (i.e. planning), and to negotiate between, not just to schedule, potentially conflicting desires. (That robots might use reflex mechanisms to schedule conflicting motives had been pointed out twenty years earlier by the MIT team asked to design a Mars robot: see 14.v.a.) Moreover, they enable one to think counterfactually, to use the cognitive technology of language to create new abilities (6.ii.c), and—by teaching these abilities to others—to make cultural evolution possible (8.v–vi). Human adults possess all these capacities, chimps most of them, dogs some of them, and newborn babies hardly any (see 7.vi).

Kirsh wasn't arguing that non-human animals must possess symbolic, compositional, representations. He allowed that non-linguistic concepts and/or meanings may be implemented in other ways (see 12.x.f, 14.viii, and 16.iv.c). He even welcomed situated robotics, for having extended the domain of concept-free behaviour and for indicating the wide range of mechanisms that may be termed representations.

But he insisted that logic, language, and thoughtful human action all require symbolic computation. These may be, as Agre had put it, a "special case" of intelligent behaviour. But they're there. And they're significant. In sum, GOFAI might not be needed when studying cockroaches, but it was essential for modelling important aspects of human minds.

Simon, the high priest of GOFAI, also fought back against the situationist tide. As Brooks himself pointed out (1991*a*: 149), Simon had long allowed that intelligent systems are largely directed by the environment (7.iv.a). He'd already reformulated planning, now seeing it as a complex web of reflexes, or productions (7.iv.b). However, he thought of these as mental representations, some containing abstract variables to be instantiated before the rule could be run. (That's why Brooks, besides denying that his work was German philosophy, denied also that it was "production rules", and why Agre rejected production rules, ACT*, and SOAR—R. A. Brooks 1991*a*: 155; Agre 1988: 240–4.)

In a paper written with the psychologist Alonso Vera, Simon expressed the essence of situationism thus:

> In its extreme form, the SA [situated action] view argues that there is no need to include internalized world models in the equation. Such internal states, some proponents of this view have said, have no causal effect on behavioral output. (Vera and Simon 1993: 12)

Reiterating his Physical Symbol System approach (16.ix.b), he argued that "no one has described a system capable of intelligent action that does not employ at least rudimentary representations" (p. 38). He insisted that the situationists themselves used representations in their models. And, doffing his hat to his famous ant, he pointed out that GOFAI systems could be highly responsive to environmental control.

The GOFAI–situationist dispute aroused great interest. Kirsh's critical paper, for example, was soon assigned in AI seminars at MIT and elsewhere (personal communication). The special number of *Cognitive Science* carrying the Vera and Simon paper presented Replies by Agre (1993), Suchman (1993), and others. Agre allowed that *Pengi* had "internal state" (e.g. deictic representations), but disputed Simon's assumption that *any* internal state counts as a cognitive representation. And he claimed that Simon had missed the crucial point: "The critical issue is whether one's categories locate things in agents and worlds separately or in the relationship between them" (1993: 68–9).

The debaters were soon accused of having "shed more heat than light . . . with neither side yielding enough ground to reach a point of productive dialogue" (M. D. Byrne 1995: 118). But dialogue, productive or otherwise, continued. Two consecutive volumes of *Artificial Intelligence* were devoted to interactionist AI in the mid-1990s, and prompted so much attention that they were soon republished as a book (Agre and Rosenschein 1995). And the 1991 papers by Brooks and Kirsh were reprinted in various collections on the philosophy of AI and cognitive science (e.g. Boden 1996; Haugeland 1997; Chrisley 2000, vol. iii).

Today, the Brooks–Kirsh battle is still undecided, although it's less fiercely oppositional, on both sides, than it was initially (Chrisley 2000: iii. 6–7). And Kirsh is getting strong support from some philosophical colleagues. For instance, Richard Samuels (forthcoming) has written a robust defence of the "Standard Model" in cognitive science. Unfashionably, in these largely situationist times, he argues that symbolic representations are needed to support means–end planning and much of the flexibility of human behaviour. (Often—shock! horror!—he even cites SOAR: see 7.iv.b.)

AI professionals are usually more eclectic than philosophers, even if they happen to have been philosophically trained. So McDermott included "dynamic planning" (which he attributed to the ideas of Agre, Chapman, Rosenschein, and Kaelbling) as a subclass of planning when he co-edited a special volume of *Artificial Intelligence* (McDermott and Hendler 1995: 6). And although some roboticists today rely only on situationist techniques, others also employ symbolic (deliberative) planning. Indeed, a recent volume supposedly—according to its title—focused on situated, behaviour-based, robotics devoted an entire chapter to the topic of how to combine reactive and deliberative mechanisms (Arkin 1998, ch. 6).

Probably, the future lies in systems combining the two approaches. For many such systems already exist. By the late 1980s Flakey was being driven by a combination of an improved STRIPS-based, world-modelling, planner and a more reactive, situationist, system (D. E. Wilkins 1988, ch. 12). And even before that, the mind had been informally described by Minsky as a combination not only of symbolic and connectionist representations, but also of top-down GOFAI and largely autonomous, bottom-up, subsystems (Chapter 12.iii.d).

Symbolism and situationism needn't be clumsily bolted together, as they are in many current "hybrid" systems. Alan Mackworth has recently put it like this:

> The methodologies [for building integrated perceptual agents] are evolving dialectically. The symbolic methods of Good Old-Fashioned Artificial Intelligence and Robotics [GOFAIR] constitute the original thesis. The antithesis is reactive Insect AI. *The emerging synthesis*, Situated Agents, needs formal rigor and practical tools. A robot is a hybrid intelligent dynamical system, consisting of a controller coupled to its plant . . . Even though a robotic system is, generally, a hybrid system, its [overall control should be] unitary. Most other robot design methodologies use *hybrid models of hybrid systems*, awkwardly combining off-line computational models of high-level perception, reasoning and planning with on-line models of low-level sensing and control. We have developed a [unitary] testbed for multiple, visually-controlled, cheap robot vehicles performing a variety of tasks, including playing soccer. (Mackworth 1998; italics added)

Mackworth, then, has abandoned the sensori-motor sandwich—but without abandoning planning. Instead, his "Constraint Net" model is based on, and verified by, a formal specification of a *symmetrical coupling* between robot and environment (Sahota and Mackworth 1994). He's no longer entering his ball-kicking robots in the RoboCup competition that he helped initiate (with Hiroaki Kitano) in the late 1990s (see 11.iii.b). But his constraint-satisfying robots are a significant progression from both SHAKEY and Flakey, and also from Brooks's early menagerie of Allen, Herbert, Genghis, Attila, and Hannibal.

As for what Mackworth calls "the original thesis", this hasn't stood still: post-Kraken advances in symbolic planning have been significant (McDermott and Hendler 1995; S. Russell and Norvig 2003, chs. 11–12). They're still continuing. The 1995 edition of a widely used AI textbook described algorithms generating plans with dozens of steps, but the second edition only eight years later presented algorithms that "scale up to tens of thousands of steps" (S. Russell and Norvig 2003, p. viii).

One powerful GOFAI planner, for instance, controls the operations of a spacecraft, given high-level goals specified from Space Control on earth (Jonsson *et al.* 2000). It generates structured plans and also monitors their execution, diagnosing and (often) fixing problems as they occur. It's not HAL, to be sure (A. C. Clarke 1968). But, in its own (relatively predictable) domain, it's highly effective.

Up to a point, less predictable domains can be handled too, for some recent planning systems include ways of reasoning under uncertainty. In short, planning is here to stay. But whether it can survive as a *general-purpose* topic, as the NewFAI researchers had intended, is unclear. McDermott isn't optimistic:

> [Possibly, architectures and/or formalisms for general-purpose planning may be developed.] This would allow a recoupling of theory and practice, and could lead in exciting new directions.
>
> However, we're pessimistically forced to conclude that another solution is somewhat more likely. It may be inevitable for the field of planning to split into even smaller subfields, each with its own domain of interest (manufacturing, deliberation scheduling, logistics planning, etc.). After all, there may not be much in common between designing the behavior of a robot and

designing the behavior of a military logistics system. It would be a pity to see this happen, but if what is gained is a set of elegant and powerful theories coupled with useful implementations replacing the current elegant but weak theories coupled with toy systems, then maybe it will be worth it. (McDermott and Hendler 1995: 13)

d. Agents and distributed cognition

Both Rosenschein and Agre, in the quotations given above, referred to situated *agents*. Indeed, situationism helped spark a flurry of research on "intelligent agents", or "autonomous agents". The *Artificial Intelligence* journal devoted over 800 pages to the topic, which were also published as a stand-alone book (Agre and Rosenschein 1995); and the interest continued (Wooldridge and Jennings 1995; Huhns and Singh 1997; Wooldridge 2001). Likewise, situationism encouraged work on the closely associated topic of "distributed" cognition (Bond and Gasser 1988; Demazeau and Muller 1990; G. Weiss 1999).

In distributed cognition, the overall task is achieved by numerous agents acting in concert, no one of them having access to all the relevant information or skills. The individuals may be more or less complex, in psychological terms. But they can achieve the task only by (knowingly or unknowingly) cooperating. In short, the interactions *between the many agents* are at least as important as the nature of each agent as an individual.

Examples found in the social (*sic*) insects include termite nests and ant trails, in which the phenomenon (nest or trail) emerges from the behaviour of many individual agents. Ants, of course, *are not* mindlike, and the cause of ant trails is very simple: each ant automatically drops chemicals ("pheromones") as it walks, and automatically follows pheromones dropped by others (see Chapter 15.x.a). A human example, discussed in Chapter 8.iii, is the navigational knowledge aboard a modern ship, which is distributed across many crew members. (The "ultimate" distributed workforce, it's been suggested, would be the world community of seafarers, linked by the Internet to their employers, unions, weathermen, and families: P. Collins and Hogg 2004.)

By century's end, it had become clear that the interactions *between agents and their environment* were crucial too. The situationists had stressed embodiment and environmental triggering, of course. But they'd seen "agent" and "environment" as clearly distinct, despite their causal interactions. In the late 1990s, that view was challenged by some specialists in HCI (human–computer interaction: see Section v, below).

Combining Bruner's work on cognitive technologies (6.ii.c), Edwin Hutchins's on environmentally embodied knowledge (such as shipboard instrumentation: 8.iii), and Clark's on the extended self (16.vii.d), they defined "distributed cognition" as a new theoretical approach in HCI. "One major benefit", they said, "is the explication of the complex interdependencies between people, artifacts and technological systems *that can often be overlooked when using traditional theories* of cognition" (Y. A. Rogers forthcoming; italics added). Blurring the conceptual boundaries between self and environment, individual and culture, helped them to analyse collaborative workplaces in general and computer interfaces in particular.

In the 1980s, however, those developments lay in the future. Meanwhile, distributed cognition was understood in the "traditional", more individualistic, way. Indeed, the term was used interchangeably with "multi-agent systems"—which inevitably raised the question "Just what is an agent?"

The answer wasn't clear. The term "agent" had officially entered AI long before, in Oliver Selfridge's Pandemonium paper (having originated in discussions with McCulloch). His then futuristic term had had a double meaning: (1) a self-directed task-achieving (i.e. mindlike) system, or demon; and (2) a demon performing some relatively menial (but purposeful) task on behalf of human users, who would otherwise have to do it themselves. For the 1980s situationists there was a third meaning: (3) a very simple demon, which might be program code or a situated robot.

(For the record, Selfridge himself is still working on agents, within DARPA's agent-based computing programme: see Chapter 12.iv.b. He's trying to build adaptive purposive hierarchies made up of lower-level purposive mechanisms. These multi-loop-controlled agents are much more complex than those favoured by most "situationists" today.)

These three demon-types, all implemented in the 1980s (or earlier: see Doran 1968*a*), were very different. For instance, mindlike agents had goals of their own, and ways of deciding how to achieve them, while simple demons didn't. And whereas most agents were largely dormant, waiting to be triggered by their environment, some mindlike agents were continuously active. Minsky's *Society of Mind*, widely circulated in draft from the late 1970s, had referred repeatedly to "agents" of various kinds. By an agent, Minsky meant

> a machine that accomplishes something, without your needing to know how it works. You call it an agent when you want to treat it as a black box . . . When I call my travel agent Roy, I'm less concerned with how Roy works than with having him do what I need him to do. (Minsky and Riecken 1994: 24)

It followed that one shouldn't expect to define an agent in terms of how it worked, because the whole point (according to Minsky) was that one didn't need to know this.

Even now, AI workers debate the difference between an agent and *just any* AI program (S. Franklin and Graesser 1997). They also ask whether "multi-agent systems" are better described more anthropomorphically, in terms of the interactions within "societies of agents" (Huhns and Stephens 1999, sect. 2.4). And even now, they disagree about just what an agent is:

> [There] is no universally accepted definition of the term agent, and indeed there is a good deal of ongoing debate and controversy on this very subject. Essentially, while there is a general consensus that autonomy is central to the notion of agency, there is little agreement beyond this. (Wooldridge 1999: 28)

These differences of definition reflect differences in methodology. While some post-Kraken programmers were developing societies of mini-minds, each with internal state representing their own and even others' goals, others were modelling agents of a much more minimalist kind. So Agre, in his introduction to the special double volume of *Artificial Intelligence* devoted to agent research, pointed out that it was "inspired by a great diversity of formalisms and architectures" (Agre 1995: 1).

Let's consider the minimalists first. These were the situationists in general, including some people more closely associated today with A-Life than with AI (because distributed systems are often thought of as "self-organizing" systems: Chapter 15). As we've seen, they described intelligent behaviour—whether in individuals or in groups—as emerging from a collection of independent reflexes, not directed by top-down control or abstract plans.

The most minimal examples of all were cellular automata in which the rules were (*a*) very simple and (*b*) identical for each agent, or cell (15.v). Work on cellular automata—together with Douglas Hofstadter's discussions of ant colonies in *Gödel, Escher, Bach* (12.x.a)—inspired Mitchel Resnick's development of Seymour Papert's LOGO language into the massively parallel StarLogo (Resnick 1994, pp. xvii, 31, 59).

Each agent in a StarLogo system was typically very simple, and similar to all the others. (Multi-agent systems composed of mindlike agents, by contrast, would have fewer—and more diverse—components.) As we saw in Chapter 10.vi.b, StarLogo enabled programmers to simulate not only interactions between situated agents, and agents' actions on the environment, but also the actions/interactions of the many different parts of the environment itself. Its potential was huge. Resnick's agents could represent turtles, or termites (ants), or cars and lorries in traffic jams . . . indeed, indefinitely many classes/colonies of interacting individuals.

Besides being used for professional AI research, StarLogo made distributed computation/cognition accessible to the general public. In two trade books, one of them specifically aimed at teachers, Resnick explained how StarLogo could be used to build systems simulating hundreds, even thousands, of individual agents (Resnick 1994; Colella *et al.* 2001). And he made the software available on the Web (<http://www.media.mit.edu/~starlogo/>). Using StarLogo, or the broadly similar SimAnt software, "thousands of . . . households [began to] play with ants on their computer screens". Ants, indeed, were the flavour of the day: the leading ethology volume on these little insects became "a sort of cult classic, attracting attention far outside the [biological] community" (Resnick 1994: 59).

Resnick hoped both to teach StarLogo users and to learn from them (1994: 5). The former, in helping them to understand decentralized systems in general. The latter, in discovering the "*folk* systems science" (comparable to naive physics) used when people think intuitively about distributed systems. Much as naive physics contains many mistaken assumptions, so—he expected—naive-systems science does too. Indeed, the "centralized mindset" of modernism had specifically inhibited a correct understanding of distributed systems. The illusory notions it had encouraged included the Freudian ego, creationism in biology, conspiracy theories in politics, and authoritarian views of family and pedagogy (1994: 119–23; 129). Work in distributed AI, of which StarLogo was a special case, could help to change that:

> When something happens, [most people] assume that one individual agent must be responsible.
>
> But the centralized mindset is neither unchanging nor unchangeable. As decentralized ideas infiltrate the culture—through new technologies, new organizational structures, new scientific ideas—people will undoubtedly begin to think in new ways. People will become familiar with new models and new metaphors of decentralization. They will begin to see the world through new eyes. They will gradually recraft and expand their ways of thinking about causality. (Resnick 1994: 130)

Resnick didn't claim that *all* cognition is distributed:

> In economics, an unyielding commitment to decentralized, laissez-faire strategies can be just as debilitating as an unyielding commitment to centralized planning. So too with thinking: an unyielding decentralized mindset is no better than a centralized one. Many phenomena in the

world *do* have centralized explanations. As [we] construct theories about the world, [we] should be able to draw on both centralized and decentralized ideas. (Resnick 1994: 148)

In this, he was following Papert himself. For Papert, in close cooperation with Minsky, had described the mind as a *hybrid* society of agents (12.iii.d). That is, there were GOFAI-ish agents as well as situationist ones. Indeed, there were connectionist agents too, so the society of mind was hybrid in both senses of the term (see 12.ix.b).

During the 1980s–1990s, research into GOFAI agents went on alongside work on the minimalist variety. Indeed, "alongside" sometimes meant *within the same family*: Stanley Rosenschein's brother Jeffrey outlined how to design "rational agents" that would use game theory to make "deals"—language better suited to GOFAI than to Brooks (J. S. Rosenschein and Genesereth 1985; cf. Rosenschein and Zlotkin 1994). Broadly, a mindlike agent was a robot or chunk of software (sometimes called a softbot) with three characteristics. Its action was autonomously controlled by its own internal processes, and by cues in its real/virtual world; the action was relatively complex, or purposive; and its own goals and self-generated sub-goals might, if the agent so decided, take priority over the goals of other agents or even of the system as a whole.

Sometimes, agents were *defined* as having "mental state, consisting of components such as beliefs and commitments" (Shoham and Tennenholtz 1995: 231). And sometimes, their mental state was analogous to the Theory of Mind, or ToM (7.vi). In such cases, the agent could represent not only other agents' abilities, beliefs, and intentions but also the social relations between self and other: "In order to cooperate effectively with its peers, an agent must *represent* any social structures in which it plays a part, and *reason* with these representations" (d'Inverno *et al.* 1997: 600).

These agents used their knowledge of others' capacities and intentions to generate shared plans and/or to decide whether to assist in another's plan. That decision, in turn, could rest either on monitoring the other's performance or on anticipating future performance or intentions. As for how they acquired their model of their peers' intentions, they normally used inductive techniques of "plan recognition" based on their observation of one or more interactions with the fellow agent concerned. In many cases, their "peers" were human beings, monitored and/or anticipated in much the same way.

One must hope that their ToM is realistic, for the applications include NASA's "VISTA" ground-control support system for the space shuttle, which went live in 1992 (Horvitz and Barry 1995). This program monitors the status of the five major engine subsystems, deciding *how to present the data on the screen being watched by the human operators*.

As Eric Horvitz (personal communication) remarks, VISTA was "perhaps the first exploration of the use of two rich probabilistic models running side by side". One of these is of the shuttle itself, based on a stream of telemetry; the other concerns "the beliefs of a user of a particular level of expertise, based on the displayed information". The inferences of both models are combined to make decisions about the psychological costs and benefits of revealing or highlighting different information on the screen. Besides avoiding information overload, VISTA aims to capture/shift the user's attention when something significant happens. In short, it tries—in real time—to pre-empt the problems facing machine operators that were studied long ago by Kenneth Craik (4.vi.c) and Donald Broadbent (6.i.c), and (in respect of short-term memory) by George Miller too (6.i.b).

Selfridge's second meaning (above) became especially relevant in the late 1980s. For by that time, many autonomous software systems were being built into computer interfaces, designed to handle administrative tasks of information retrieval/filtering on behalf of the human user (see v.c–d, below). The variety of these unseen agents increased with the years. When Berners-Lee's vision of the Semantic Web finally comes about, it will be thanks not only to shared ontologies but also to reliable agents working behind the scenes on behalf of the human users.

As Kay put it, these 1980s agents were "computer processes that act as guide, as coach, and as amanuensis" (1989: 130). For instance, they might devise timetables (respecting the entries already in the user's diary), book hotel rooms, or arrange for flights and car hire—perhaps without bothering the user, or perhaps making suggestions for human ratification (D. A. Norman 1994). Some could even learn how to do better in future by inferring, or being told, why their suggestion was rejected. One such agent could learn up to twenty new timetabling rules each night, by applying a version of ID3 to the information gleaned from the user on the previous day (T. M. Mitchell *et al.* 1994: 87).

By the end of the century, intelligent/autonomous agents had caught the imagination of many AI scientists. The ever-growing body of research had led (for instance) to international conferences—and journals—on 'Multi-Agent Systems', 'Autonomous Agents', and 'Adaptive Systems', and to specialist sessions in the IJCAI meetings (e.g. M. E. Pollack 1997: 578–654). Distributed AI was being used in industry as well as in interfaces (Parunak 1999; Alonso 2002). Moreover, affordable robots were now available off the shelf, so that people could study not only single home-made robots (such as FREDDY, SHAKEY, Flakey, or Cog) but also groups of interacting robots (Arkin 1998, ch. 9).

The general public was pricking up its ears, too. "Agent" had even become a buzzword in the marketplace. The Apple Newton, for example, was proudly advertised as "agent software", and General Magic trumpeted their new "messaging agents" (Riecken 1994: 18).

Agents had first achieved media popularity through Minsky's 1985 trade book *The Society of Mind*. But one didn't need to read the book to hear of the idea. Quite apart from reports by journalists, Minsky himself had been spreading the gospel among the business community for some years. At fundraising meetings in MIT's new Media Lab, he (and Kay, too) had sung the praises of agents as a way not only of evolving minds but also of developing useful and user-friendly computer technology (Section v, below).

As for distributed cognition, this idea had entered the public consciousness in 1979 through Hofstadter's extraordinary best-seller *Gödel, Escher, Bach* (Chapter 12.x.a). It had been reinforced in the mid- to late 1980s by the PDP 'bible' (12.vi.a). And—as remarked above—self-organization in "ant colonies" had become so evocative that a specialist tome on ants had become a cult book.

The popularity of both these notions was partly due to the widespread counter-cultural disaffection with centralized control and affection for the concrete and specific (see 1.iii.c–d). Indeed, Resnick said as much in lauding the fall of the Soviet Union *and* the decentralization of IBM in December 1991: "Throughout the world", he remarked, "there is an unprecedented shift toward decentralization" (Resnick 1994: 7; cf. Toulmin

1999; Turkle and Papert 1990). Systems where many autonomous individuals 'did their own thing' were felt to be even more culturally/politically appealing than committees, which in turn had been seen as more attractive than autocratic hierarchy (10.iv.a).—So much, yet again, for the Legend.

e. Social interaction and agents

Modelling collections of mindlike agents raised analogues of problems in social management that people face every day. In the absence of a top-down executive control, questions arose about how the knowledge possessed by the various components of the distributed AI system is integrated within the system as a whole. How could robots, or softbots in a virtual world, cooperate so as to achieve a goal which none could achieve alone?

The "social" behaviour of agents thus became an issue: the challenge was "to guarantee the successful coexistence of multiple program and programmers . . . [where the] agents' actions, and perhaps also goals, may be either facilitated or hindered by those of others" (Shoham and Tennenholtz 1995: 231). Increasingly, then, designers of distributed AI focused on the *interactions* between agents.

Social behaviour, in this context, included various types of communication: not just informing and questioning, but negotiating, bargaining, and voting too. The simple demons in Pandemonium had 'voted' via the loudness of their shouts (12.ii.d). But some late-century agents had to 'think' much harder before deciding which way to vote: for instance, by comparing their existing goals with the probable results of what they were being asked to do by some other agent. Negotiation and bargaining were even more complex. (In 1997 a special issue of the *Artificial Intelligence* journal was devoted to these "economic" aspects of distributed AI: Boutilier *et al.* 1997; see also J. S. Rosenschein and Genesereth 1985; Sandholm 1999.)

Various types of negotiation and bargaining were modelled, Rosenschein's "deals" being among the first. In systems based on *contract nets* (R. G. Smith 1980), for instance, the agents made "bids" that described how their abilities (including their current beliefs) were relevant to solving the overall problem. A central "manager" then decided which "contracts" to assign to each agent. One recent example of this approach was developed for NASA, to aid astronauts on long-term missions (Naghshineh-pour *et al.* 1999). The NASA contract net schedules the production and transportation of food—planting, monitoring, harvesting, recycling, and processing—in outer space.

As that example indicates, some modern agents can negotiate about several different goals, with incomplete information, and differing time deadlines (Fatima *et al.* 2004). An individual agent may even have deadlines, known only to the agent itself; if so, these deadlines affect its bargaining about which tasks it will volunteer and which it will accept (Sandholm and Vulkan 1999).

Besides communication, there was cooperation: *when? how? with whom? why?* In other words, post-Kraken agent modellers had to think about the issues raised many years earlier by the psychologist Robert Abelson, regarding the identification of plan junctures at which one particular agent might be able to help another (Chapter 7.i.c).

That's not to say that they used Abelson's work, for they didn't. But a late-century abstract analysis of "cooperation structures" dealt with the questions he'd raised, and related them to specific difficulties in the programming of distributed AI—how to determine, for instance, whether cooperation is even in principle possible, given some group of agents and a particular agent's goal (d'Inverno *et al.* 1997).

Similarly, Abelson's questions arose when people discussed how to generate collaborative plans (Grosz and Kraus 1996, 1999). And although most AI researchers ignored the fact (which Abelson had highlighted) that an agent may want to aid *or obstruct* another agent's plans, a few didn't. One team, for instance, said: "[Unlike most of those working on cooperation, we do not] assume benevolent agents" (d'Inverno *et al.* 1977: 605)—and their abstract model of cooperative structures specifically allowed for indifference or malevolence in some agents. Again, a general ontology for describing social interactions between AI agents explicitly allowed for "selfish" as well as "collaborative" behaviour (Castelfranchi 1998).

A central feature of 'mindlike' distributed AI was the communication of one agent's internal state (its beliefs, goals, and willingness to help) to others, and/or the communication of non-shared knowledge about the environment (e.g. the constraints on or goals of the problem as a whole). The same had been true of previous efforts to model cooperation—for instance, the two robots discussing how to open a bolted door (see Chapter 9.xi.f). Indeed, people usually take it for granted that communication, and a fortiori cooperative communication, *must* have evolved so that information could be transmitted to creatures currently ignorant of it.

Researchers in distributed AI typically did so too—and so did most A-Lifers. Even minimalist examples like ant trails, formed by the pheromones dropped by individual ants (15.x.a), could be shoehorned into this assumption. And A-Life models of the adaptive evolution of language usually relied on it heavily (e.g. Steels 1998*a*,*b*,*c*; Steels and Kaplan 2001; Steels and Belpaeme forthcoming). Some writers made it absolutely explicit: "A first prerequisite for communication is that some organisms have access to information (knowledge) that others do not, for if they all have access to the same information, no communication is necessary" (MacLennan and Burghardt 1994: 165).

However, some turn-of-the-century work, broadly inspired by Francisco Varela's work on autopoiesis (15.viii.b and 16.x.c), challenged it. The Sussex A-Life researcher Ezequiel Di Paolo (1970–) showed that communication, and cooperation, can in principle evolve *without* information being transmitted from one agent who has it, to another, who doesn't (Di Paolo 1999, ch. 8; 1998).

He built an intellectually rich series of evolutionary computer models, which evolved various types of social coordination between (simulated) robots. Systematic experimental variations discovered which parameters, under which conditions, were most important, and suggested implications for biological theories of communication. In some versions of the model, information wasn't equally available to all, and (sometimes) cooperative action–response strategies evolved so that the robots could benefit from each others' knowledge. In other versions, all agents possessed the same information—but even then, cooperative communication sometimes developed.

Each robot needed to replenish its energy regularly by accessing food. If it failed, it would die. And only if its energy level was above a certain threshold would it be able to reproduce. The food was finite, so the more one robot consumed the less there was for the others. In that sense, there was a conflict of interest: prima facie, not a situation in which one would expect cooperation.

In the informationally asymmetric versions of the model, more energy was made available to those robots which happened to signal the presence and type of found food (as opposed to either 'cheating' or remaining silent) and/or which happened to approach suitable food when perceiving the signal. However, no robot 'knew' that a particular action was a signal. Indeed, an action's communicative function *as* a signal, denoting a certain type of food and prompting neighbour robots to approach it, evolved as an aspect of the increasing coordination between the robots.

In the 'universal information' version, fitness didn't depend on single actions by individuals (emitting, or responding to, a signal). Rather, it depended on *a sequence of alternating actions by both agents*. *For example*, *imagine* that you and I are both gazing hungrily at a tall cherry tree. You must say "bing", then I must say "bong", then you say "tring", and I say "trong"—for *only then* will any cherries drop from the tree. Or rather, *only if you happen to say* "bing" . . . etc. will the cherries fall: whether you're aware that you "must" say this to get that result is immaterial. An apple tree would require a different four-signal sequence (perhaps tring, trong, bing, bong—with me to start, this time) before either of us could sink our teeth into the fruit.

Similarly, energy in Di Paolo's model was released "partially depending on the actions being correct at the required steps of the sequence" (Di Paolo 1999: 163). At first, there was no "sequence", only random actions. But an accidental 'correct' action would provide some energy, and a correct four-action sequence would provide even more. Ideally, the robots would select their next action depending on the food type and the previous actions—all perceptible to both robots. And evolution, due to differential reproduction, often resulted in a final generation that coordinated their actions perfectly. (This was true *even though* one and the same robot might have to perform a given action in first place or in third place, depending on the food type: apples or cherries, as it were.)

Di Paolo's robots couldn't have 'learned' to produce the right actions as lone individuals. They could achieve high energy levels only by acting cooperatively in locating the food. This type of cooperation, however, was very different from that studied by Abelson, or by AI scientists designing cooperating mindlike agents. There were no plans, no intentions, no symbolic references to food . . . no internal state at all. Accordingly, Di Paolo argued that this work cast serious doubts on orthodox cognitive science (1999, ch. 10).

One implication, he said, was that an individual's successful performance needn't imply a corresponding competence at the individual level (Chapter 7.iii.a). An agent who can get the cherries by cooperating with another agent may have *no* individual competence for getting cherries. Similarly, a robot that produces rhythmic behaviour when interacting in a particular acoustic world may show no hint of rhythm when acting on its own (Di Paolo 1999, ch. 9; 2000*a*).

Another implication—highly subversive, from the point of view of orthodox cognitive science—was that AI work on "social" agents wasn't truly social. AI agents were conceptualized as *individuals*, who happened to be interacting. Certainly, things occurred when they interacted which wouldn't have occurred otherwise. (Remove the diary-checker from the multi-agent system responsible for personal timetabling, and the human user wouldn't be happy.) But there was little or no notion that social behaviour may depend largely on *the dynamical properties of the interaction as such*.

For purely technological purposes, this might not matter. But for explaining human social behaviour (7.i.c), Di Paolo felt that it did. Even situated AI, although superior (in his view) to classical GOFAI, hadn't gone far enough:

To take situatedness seriously sometimes implies renouncing the simplicity of explanatory monism and embracing the complexity of the multiple concurrent and interdependent factors that form a historical process. This may not always be an easy task. However, once a situated process is properly understood many issues that are initially mysterious can be explained in a natural manner. Thus, cooperative coordination in games with conflict of interest can be explained by a combination of Darwinian selection and ecological situatedness [as outlined above]. (1999: 190; italics added)

From this viewpoint, my claim (in Chapter 1.ii) that cognitive science has studied social matters as well as individual cognition is misleading. To be sure, an adequate cognitive science would have to encompass social interactions, and some cognitive scientists have *attempted* to do so. If Di Paolo is right, however, they've gone about it in a fundamentally wrong-headed way. Even the respected *social* psychologist Abelson is individualistic at heart.

This dispute is an old one, although Di Paolo has put it in a new, and scientifically intriguing, way. In a nutshell, the question is whether individual selves constitute society or whether they're constituted by it. (Note: constituted by it, not just influenced by it.) Opposing answers have been defended by philosophers and social scientists for at least a century (Hollis 1977).

f. Technology swamps psychology

Inductive learning is a prime example of psychological AI's spawning, and eventually being overtaken by, technological AI.

There's no doubt that the psychologists got there first, for they were already studying concept learning by mid-century (see 6.ii.b–c and 10.iii.d). Informational analyses were published by Carl Hovland in 1952, computational strategies by Bruner in 1956, and computer models by Hovland and Earl Hunt in 1960–2.

Through the 1960s, Hunt continued his research on concept learning. No longer working with Hovland, who'd died in 1961, he now cooperated with Janet Marin and Philip Stone. They defined a new method for inductive classification: the Concept Learning System, or CLS rule (Hunt *et al.* 1966). Having only five steps, it was much simpler than the strategies discussed in Hunt's 1962 book (see Figures 10.10 and 10.12). But like them, it was a recursive algorithm which, given a training set of examples, learnt a top-down decision tree for classifying them.

CLS started by asking whether either *all* or *none* of the input cases were labelled as being examples of one concept. If so, it would stop. If not, it would divide the set into subsets and ask the same question again. At each hierarchical level, it would choose a property with more than one value (including present/absent), and partition the set accordingly. So flowers might be sorted into subsets by scarlet, blue, yellow, or white (think poppies, bluebells, daffodils, and snowdrops).

One weak spot in this procedure, at least in the early version, was that the property used to partition the set was chosen at random. This could result in a very inefficient search. So the programmers made it possible for the human user to choose the property to be considered. In other versions, simple frequency counts (successors of step 3 in Figure 10.10) would select the most useful property for partitioning the set. A property possessed by only 1 per cent of the examples, for instance, isn't a good (i.e. computationally efficient) one with which to start—even if it's a *sufficient* condition of the relevant concept.

Another weak spot was that concepts were (still) assumed to have necessary and sufficient conditions: as though all bluebells were blue, and all daffodils yellow. In fact, some bluebells are white, or pink. But most are indeed blue, which is why blueness is a very useful indicator of bluebells. CLS, on being presented with a pink bluebell, wouldn't treat it as an exception to be tolerated. Rather, it would reject blueness as a criterion of bluebells—thus throwing out the baby with the bathwater.

CLS could learn from its mistakes. If a new example (a pink bluebell, perhaps) didn't fit its current model, it would reconstruct the entire decision tree so as to include it. Because the combinatorial explosion raised its ugly head as the number of past classifications grew, Hunt's team eventually amended CLS so that it considered only a random subset of its 'memories' while revising the decision tree.

Not everyone studying GOFAI induction in the 1960s was doing similar things. For instance, other methods were devised by several members of the Edinburgh AI group, its leader Donald Michie among them. They occasionally cited Bruner (e.g. Popplestone 1969: 203), and/or defined strategies which were generalizations of his (e.g. Richard M. Young *et al.* 1977). But their work was logical, not psychological. Much of it was an attempt to widen theorem proving to cover induction too (Popplestone 1969; G. D. Plotkin 1969, 1971).

In the 1970s, as remarked in Chapter 10.iii.d, learning was seen—in the USA, at least—as "a 'bad' area to do research in" (R. S. Michalski, personal communication). Nevertheless, this period saw several advances. In the mid-1970s, meta-DENDRAL induced previously unknown rules about the points at which certain chemical molecules are likely to split (Chapter 10.iv.c). Simon's psychology student Pat Langley (1953–) started work on BACONian induction at much the same time (iv.c, below). And Ryszard Michalski used a multi-valued version of predicate logic to define a new learning algorithm, AQVAL (Michalski 1973; Michalski and Larson 1978).

When it made a mistake, AQVAL reconsidered only those previous classifications which were most relevant—not a random subset, as with CLS. And it reconstructed only that part of the decision tree which had made the mistake, not the whole tree. In those ways, it was an improvement on CLS. But that hadn't been Michalski's aim in designing it. He'd first modelled learning (of handwritten characters from several alphabets, including Roman, Greek, and Cyrillic) in 1966, in Poland. When he started work on AQVAL, soon after his arrival in the USA in 1971, he knew nothing of Hunt's research (personal communication).

Suddenly, around 1980, induction turned from being a "bad" area to a popular one. The first international workshop on machine learning was held (at CMU) in 1980, and three issues of the *International Journal of Policy Analysis and Information Systems* were given over to it. A few months later, in spring 1981, a special number of the *SIGART Newsletter* followed suit. In 1983 an authoritative collection of papers appeared, which was very widely read (Michalski *et al.* 1983). The Japanese Fifth Generation project was spreading hopes of providing expert systems with learning capabilities. The mid-1980s saw Bundy's analytical unification and critique of superficially diverse learning programs (11.iii.b). And by 1986, the new journal *Machine Learning* had been established, with Langley as its founding editor.

The methods used included connectionism as well as GOFAI, and genetic algorithms as well as fixed programs (J. H. Holland *et al.* 1986; Goldberg 1987). By the late 1980s, some machine learning was taking account of underlying causal mechanisms (Bratko *et al.* 1988, 1989). And the field was influencing the philosophy of science (Thagard 1988, 1989, 1990; P. M. Churchland 1989).

Not all of this post-Kraken literature concerned Hunt's theme (and ours): what one might term "property-list" induction. But much of it did.

Why the sudden blossoming of interest? After all, people had been working on GOFAI induction for some years. They included Michie, Robin Popplestone, and Gordon Plotkin in Edinburgh; Ivan Bratko in Prague; Langley and Simon at CMU; Michalski at the University of Illinois; and Thomas Mitchell, also at Illinois—who'd cut his AI teeth (at Stanford) on the learning aspect of meta-DENDRAL, but who became well known for his method of "version spaces" (T. M. Mitchell 1979; Mitchell *et al.* 1983). One name, however, stood out above all: J. Ross Quinlan (1943–), ensconced at the RAND Corporation by 1983.

It was Quinlan's work, from the late 1970s on, which tipped the popularity scales in favour of machine learning. Specifically, he devised the ID3 algorithm (Quinlan 1979). This was instigated in the context of chess endgames, thanks to a "challenging" question posed by Michie: could one tell, from the state of the board alone, whether a certain endgame was likely to be lost within a fixed number of moves? (Quinlan 1986: 84; cf. Quinlan 1983: 481). But Quinlan soon moved beyond chess, generalizing his algorithm a few years later (1983, 1986).

ID3 revolutionized machine learning, setting the GOFAI agenda for many years to come. It was the first inductive program specifically designed to handle "large masses of low-grade data" (1983: 463). And it was mathematically guaranteed to find the most efficient classification method for a given domain, provided that it was given a representative sample of examples (i.e. all types of example included, and the rarities suitably rare). Potentially, the commercial applications were legion.

One might argue that the *theoretical* interest of Quinlan's work on machine learning hadn't increased—had perhaps even decreased—with ID3. For his initial research, presented at the very first IJCAI meeting in 1969, had been different. Like Arthur Samuel's game-player, Sussman's HACKER, and Michie's pole-balancer (Michie and Chambers 1968), his first learning program had used adaptive self-monitoring to improve its own problem-solving ability (Quinlan 1969). By contrast, ID3 discovered already structured concepts. That's a perfectly respectable type of learning—but it's not the only one, nor the deepest.

It was, however, the one with the greatest technological promise. But if ID3 pointed towards technology, it had started out in psychology. For Quinlan had been deeply influenced by Hunt. He'd even co-published with him, on problem solving (Hunt and Quinlan 1968). That had been during his time as the first Ph.D. student in computer science at the University of Washington, where Hunt had a home in the Psychology and the Computer Science departments (E. B. Hunt, personal communication). On turning to learning as his prime topic, he benefited from Hunt's ideas. Later, he described ID3 as "a relative" and "a descendant" of the CLS classification rule (1979: 171; 1983: 465). Indeed, he acknowledged CLS as "the patriarch" of the large family of post-Kraken inductive programs (1986: 84).

In outline, ID3 was given a training set of a large number of instances of various concepts: chess endgames, perhaps, or the nineteen common diseases that affect soya beans (see below). Each instance was described in terms of a set of properties (the position of king or rook, for example), and the program learnt rules for defining each concept. It did this by a hierarchical series of binary partitions of the training space. When the space could be divided no further, the program would be tested on sets of new examples (instances and non-instances).

So far, so CLS. But ID3 was hugely more efficient than its psychological precursor. Its advantages were:

* the much greater size of the training set,
* a larger number of attributes used in defining concepts,
* greater complexity of the concepts (measured by the number of nodes on the decision tree),
* much greater speed and computational efficiency,
* the ability to accept classifications accounting only for *most* of the data, and
* the possibility of discovering some classification-relevant attributes automatically (i.e. local *patterns* of attributes, as in chess positions—1983: 477–81).

ID3, said Quinlan, was "five times as fast as the best alternative method that I could devise" (p. 463). And it surpassed all the other inductive algorithms favoured at the time, such as Michalski's AQVAL or Mitchell's version spaces. Version spaces had been designed to deal with large data sets too, by setting boundaries of maximal and minimal generality to the concepts being developed. But as Quinlan pointed out, when the number of examples was *very* large, Mitchell's method (which required the system to remember all the still-possible maximal/minimal rules) would become unmanageable. Instead, he suggested a simple iterative strategy:

** select at random a subset of the given instances (called the *window*)
** *repeat*
  * form a rule to explain the current window
  * find the exceptions to this rule in the remaining instances
  * form a new window from the current window and the exceptions to the rule generated from it *until* there are no exceptions to the rule. (Quinlan 1983: 469)

(For generating *approximate* classification rules, that final "until" had to be relaxed—1983: 474–7.)

Experimenting with two different ways of forming the window, Quinlan used this approach to classify a data set of nearly 2,000 objects, involving fourteen attributes, and requiring a decision tree with no fewer than forty-eight nodes (Quinlan 1979). The results were staggering.

Irrespective of the window-forming method used, he found that *only four iterations* were normally needed to find a correct decision tree. The final window could contain only a small fraction of the 2,000 objects, and the size of the initial window didn't matter much. Moreover, the time required increased only linearly as the task difficulty rose (measured by the number of objects, attributes, and decision tree nodes). So whereas Hunt had been restricted to very simple concepts, like the experimental examples in Bruner's *A Study of Thinking*, Quinlan's method could be used to deal with much more interesting cases.

Consider soya bean diseases, for instance—no mere triviality in America's Midwest, since soya beans are Illinois's main commercial product. The nineteen common diseases are recognized by thirty-five descriptors, covering various types of leaf spots (with/without haloes, or water-soaked margins, or . . .) and holes, seed shrivelling, and information about season and rainfall.

There's no simple one-to-one mapping of symptom to disease. Rather, there's a complex pattern of symptoms for each disease, complicated by the fact that not all the symptoms need to be present in a particular case. (Pink bluebells, again.) So even experienced soya bean farmers can't always be sure what's ailing their plants. Illinois has long provided a number of Agricultural Extensions Offices which they can phone for advice. In especially difficult cases they can also arrange for microscopical tests to be done, at the farmer's expense, by the university.

Michalski and Richard Chilausky, both working—appropriately enough—at the University of Illinois, decided to use ID3 to write an expert system that might save time and money for all concerned (Michalski and Chilausky 1980). With the help of the textbooks, augmented by forty-five hours of consultation with a local plant pathologist, they identified the probably relevant descriptors and provided them to ID3. In addition, they designed a descriptive questionnaire to be filled in by the farmers whose crops were afflicted—and the farmers' answers were then used as input to the expert system (see Figure 13.2).

It turned out that, for fifteen diseases, their ID3-based program outperformed the world expert—who'd written the textbook on which the initial data classification had been based. Having been trained on 307 different cases, it was tested on 376 new ones. (The tree was automatically converted into a coherent set of production rules, implying that IF such-and-such symptoms were present, THEN such-and-such a disease was present.) It got only two diagnoses wrong, whereas humans following the textbook rules failed in 17 per cent of cases. Indeed, the computer-generated rules were accepted for daily use by soya bean farmers and pathologists alike (Michie and Johnston 1984: 111).

Even though all the data in ID3's training set (regarding soya beans or anything else) were presented simultaneously, the algorithm made sensible decisions about which attributes to consider first. The window was chosen at random, as we've seen. But the item "form a rule to explain the current window" involved frequency counts that improved on those used by CLS. In Quinlan's words:

> The whole skill in this style of induction lies in selecting a useful attribute to test for a given collection of objects so that the final tree is in some sense minimal. *Hunt's work* used a lookahead scheme driven by a system of measurement and misclassification costs in an attempt to get minimal-cost trees. ID3 uses *an information-theoretic approach* aimed at minimizing the expected number of tests to classify an object. (Quinlan 1983: 466–7; italics added)

That was the thin end of the technological wedge. Whereas Hunt had started from experimental data, common sense, and simple logic to define thinking strategies that

Environmental descriptors
- Time of occurence = July
- Plant stand = normal
- Precipitation = above normal
- Temperature = normal
- Occurence of hail = no
- Number of years crop repeated = 4
- Damaged area = whole fields

Plant global descriptors
- Severity = potentially severe
- Seed treatment = none
- Seed germination = less than 80%
- Plant height = normal

Plant local descriptors
- Condition of leaves = abnormal
  - Leafspots–halos = without yellow halos
  - Leafspots–margin = without watersoaked margin
  - Leafspot size = greater than ⅛"
  - Leaf shredding or shot holding = present
  - Leaf malformation = absent
  - Leaf mildew growth = absent
- Condition of stem = abnormal
  - Presence of lodging = no
  - Stem cankers = above the second node
  - Canker lesion color = brown
  - Fruiting bodies on stem = present
  - External decay = absent
  - Mycelium on stem = absent
  - Internal discoloration of stem = none
  - Sclerotia-internal or external = absent
- Conditions of fruit-pods = normal
  - Fruit sports = absent
- Condition of seed = normal
  - Mould growth = absent
  - Seed discoloration = absent
  - Seed size = normal
  - Seed shrivelling = absent
- Condition of roots = normal

Diagnosis:

Diaporthe stem canker( ) *Charcoal rot*( ) *Rhizoctonia root rot*( ) *Phytophthora root rot*( ) *Brown stem root rot*( ) *Powdery mildew*( ) *Downy mildew*( ) *Brown spot(x)* *Bacterial blight*( ) *Bacterial pustule*( ) *Purpose seed stain*( ) *Anthracnose*( ) *Phyllosticta leaf spot*( ) *Alternaria leaf spot*( ) *Frog eye leaf spot*( )

FIG. 13.2. A questionnaire completed by a soybean farmer, used as input to an ID3-based expert system, with the program's diagnosis (Brown spot) shown underneath. Reprinted with permission from Michalski and Chilauski (1980: 138)

might actually go on in human heads, Quinlan was using highly abstract formulae drawn from information theory. In effect, ID3 was doing entropy calculations in order to choose which attribute to consider next (1983: 467 ff., 475 ff.). These calculations were justified *not* by psychological theory (even though some psychologists were starting to use ideas about entropy in their models: 12.vi.b), but purely by mathematical efficiency.

As the post-Kraken period progressed, the psychological roots of automatic induction grew ever more obscure. For Quinlan (e.g. 1988, 1993) and others used increasingly arcane statistical measures to enable more efficient machine learning. Much as connectionist learning, during these years, grew closer to mathematical statistics *as such* (Chapter 12.vi.f), so most of the GOFAI versions did too.

This was pointed out in the early 1990s by Michie (Michie *et al.* 1994). Indeed, most of the papers given at his 1992 Machine Intelligence workshop, which was devoted to the topic of learning, exemplified the mathematical trend (Furukawa *et al.* 1994). Despite a handful of people-centred presentations, including one by a leading developmental psychologist based in Edinburgh (C. B. Trevarthen 1994), human beings weren't much considered. Like most other 1990s work on automatic induction, this was *machine* learning: technological, not psychological, AI. (The exceptions included Michalski's theories of plausible inference and human learning; based on a model of dynamic links between conceptual hierarchies, these were developed through the 1980s with the psychologist Allan Collins: Collins and Michalski 1989; Hieb and Michalski 1993.)

Some even began to claim that this research wasn't AI at all (see vii.b, below). Statisticians and computer scientists didn't want to be labelled as doing AI. This attitude was—is—due partly to intellectual territoriality ("Get off my patch!"), and partly to a wish to avoid diversionary philosophical challenges. Hard-headed technologists and mathematicians didn't want to be saddled with provocative psychological terms such as *intelligence*, *knowledge*, or even *learning*.

As a result, the most successful type of machine learning is near-invisible today, because it's blandly named "data mining". (Two of the field's pioneers now use both names in the same breath—and the same book title: Michalski *et al.* 1998.)

By "successful", I really do mean successful. As one commentator has put it, the AI equivalent of Columbus' looking for India and finding the resource-rich America is looking for machine learning and finding data mining (Whitby 2004). Huge sums of money are now spent on partitioning enormous (and noisy) data sets into useful categories. For example, advertising agencies are commonly asked to find detailed descriptions of the people likely to be interested in buying such-and-such a product, or voting for such-and-such a policy. It's a rare client, however, who thinks of this as machine learning—still less, as AI. Yet without AI, there'd be no data mining.

The *psychological* origins of automatic induction are less visible still. (Statistics rules!) For practical purposes, that doesn't matter. For historical purposes, however, machine learning's debt to experimental psychology should be recognized.

13.iv. Explaining the Ineffable

'Explaining the Ineffable' was the title—and 'Intuition, Insight, and Inspiration' the key words in the subtitle—of the paper which Simon, near the end of the century (Simon

1995*b*), said that AAAI Fellows should read if they doubted that AI is a *science* (see vii.a, below). Clearly, he felt that AI had gone some way towards fulfilling the promise of his 1958 RAND harbinger (10.i.f). But that promise hadn't been taken up explicitly until the 1980s.

a. Creativity ignored

Creativity was an obvious challenge for AI right from the start—or even before it. In the imaginary conversation that launched the Turing Test, Turing had depicted a computer as interpreting/defending a sonnet, tacitly implying that a computer might compose poetry too. However, once people started writing AI programs, this particular challenge was parked on the sidelines.

When GOFAI was still NewFAI, creativity was a no-go area. To be sure, Donald Mackay (1951) had described a probabilistic system which he said would show "originality" in a minimal sense—but that had been an aside, not the main object of the exercise. Similarly, problem-solving programs were occasionally described as creative. And Simon, in 1957, had mentioned the "theory of creativity" he and Newell were developing—and had praised the pioneering work of Lejaren Hiller and Leonard Isaacson, whose computer-generated Illiac suite (a string quartet, first performed in 1957) was, in his estimation, "not trivial and uninteresting" (McCorduck 1979: 188). Nevertheless, creativity in the layman's sense (i.e. art, music, and science—and maybe jokes) was all but ignored by AI professionals.

Outside the field, this was less true. Zuse's vision of automatic carpet design, with deliberate weaving-errors to add 'authenticity', wasn't yet in the public domain. But visual computer art was getting started in Europe and the USA by 1963, and interactive computer art had already begun in the 1950s (see Section vi.c, below).

In the 1950s, too, Hiller—a professional chemist, but also a Master of Music—had initiated the Illiac suite (the score is given at pp. 182–97 of Hiller and Isaacson 1959). The first three movements were generated from rules defining various musical styles (sixteenth-century counterpoint, twelve-tone music, and a range of dynamics and rhythms), sometimes combined with tone-pairs chosen by chance; the fourth movement was based not on familiar styles but on Markow chains (Hiller and Isaacson 1959). Systematic experiments in computer music, including instrumentation, were done at IRCAM in Paris from the early 1960s. And a competition for computer-composed music was organized at the 1968 International Federation for Information Processing meeting in Edinburgh (and led to the founding of the Computer Arts Society).

In general, however, these projects involved the artistic avant-garde, with a few art-oriented scientists—not the leaders of GOFAI. Their references to "creativity", "invention", and "discovery" in the Dartmouth proposal weren't being echoed in AI research (McCarthy *et al.* 1955: 45, 49 ff.). Even creative problem solving was rarely described as such, despite the LT team's use of the term in their late 1950s call to arms.

That's why my AI colleagues were bemused when, in the early 1970s, I told those who asked that I'd decided to include a whole chapter on the topic in my book on the field. Several protested: "But there isn't any work on creativity!"

In a sense, they were right. Admittedly, the 1960s had produced an intriguing model of analogy (see below), and meta-DENDRAL had touched on creativity in chemistry.

Moreover, much NewFAI work was an essential preliminary for tackling creativity as such. For instance, NLP researchers had asked how memory structures are used in understanding metaphysics (Wilks 1972: see 9.x.d), metaphor (Ortony 1979), or stories (Charniak 1972, 1973, 1974; Schank and the Yale AI Project 1975; Rieger 1975*a*,*b*). One brave soul was generating story-appropriate syntax (Davey 1978: see 9.xi.c), and another studying rhetorical style (Eisenstadt 1976).

But apart from a handful of painfully crude "poets", "story writers", or "novelists", there were no models of what's normally regarded as creativity (Masterman and McKinnon Wood 1968; Masterman 1971; Meehan 1975, 1981; Klein *et al.* 1973). The renowned geometry program was only an *apparent* exception (see 10.i.c and Boden 1990*a*: 104–10).

This explains why John Haugeland (1978, sect. 7), when he questioned the plausibility of cognitivism, raised general doubts about GOFAI analyses of "human insight" but didn't explicitly criticize any specific models of it. His first worry was that GOFAI systems "preclude any radically new way of understanding things; all new developments would have to be specializations of the antecedent general conditions". But Galileo, Kepler, and Newton, he said, invented "a totally new way of talking about what happens" in the physical world, and "a new way of rendering it intelligible". Even to learn to understand the new theory would be beyond a medieval-physics GOFAI system, "unless it had it latently 'built-in' all along". (He may have chosen this particular example because he'd heard of the about-to-be-published BACON suite on the grapevine: see below.) The underlying difficulty, he suggested (following Dreyfus: 11.ii.a), was that "understanding pertains not primarily to symbols or rules for manipulating them, but [to] the world and to living in it".

Before the 1980s, then, creativity was a dirty word—or anyway, such a huge challenge that most programmers shied away from it. The most important exception, and still one of the most impressive, was due to someone from *outside* the NewFAI community.

b. Help from outside

Harold Cohen (1928–) was a highly acclaimed abstract artist of early 1960s London. (Those words "highly acclaimed" weren't empty: a few years ago, a major exhibition on 'The 1960s' was held at London's Barbican. Even though this was focused on 1960s culture as a whole, not just on the visual arts, two of Cohen's early paintings were included.) He turned towards programmed art in 1968. He spent two years in Stanford as a visiting scholar with Feigenbaum, in 1973–5, where he not only found out about AI but also learnt to program.

In the four decades that followed, while at the University of California (San Diego), he continuously improved his drawing and colouring program, called AARON (H. Cohen 1979, 1981, 1995, 2002; McCorduck 1991; Boden 2004: 150–66, 314–15). Successive versions of AARON were demonstrated at exhibitions in major galleries (and at Science Centres) around the world, and received huge publicity in the media. (An early version is now available as shareware.)

Unlike his 1960s contemporaries Roy Ascott and Ernest Edmonds (vi.c, below), Cohen wasn't using computer technology to found a new artistic genre. Rather, he was investigating the nature of representation. To some extent, he was trying to achieve a better understanding of his own creative processes. But, over the years, he came to see his early attempts to model human thought as misdirected:

> The common, unquestioned bias—which I had shared—towards a human model of cognition proved to be an insurmountable obstacle. It was only after I began to see how fundamentally different an artificial intelligence is from a human intelligence that I was able to make headway. . . . The difference is becoming increasingly clear now, as I work to make AARON continuously aware of the state of a developing image, as a determinant to how to proceed. How does a machine evaluate pizazz? (personal communication, July 2005)

Cohen asked how he made introspectively "unequivocal" and "unarbitrary" decisions about line, shading, and colour. (From the mid-1990s his main focus was on colour.) And he studied how these things were perceived—by him and others—as representing neighbouring and overlapping surfaces and solid objects. Moving towards increasingly 'three-dimensional' representations, he explored how his/AARON's internal models of foliage and landscape, and especially of the human body, could be used to generate novel artworks. (He was adamant that they *were* artworks, although some philosophers argue that no computer-generated artefact could properly be classified as art: O'Hear 1995.)

Two examples, dating from either side of 1990, are shown in Figures 13.3 and 13.4. Notice that the second, drawn by the later version of the program, has more 3D depth than the first. (Which, in turn, has more than the drawing by AARON's early 1980s 'Acrobat and Balls' period—see Boden 1990*a*/2004, frontispiece.) In particular,

Fig. 13.3. An example of AARON's 'jungle' period, late 1980s; the drawing was done by the program, but the colouring was done by hand. Untitled, 1988, oil on canvas (painted by Harold Cohen), 54″ × 77″; Robert Hendel collection. Reproduced with permission of the artist

Fig. 13.4. An example of early 1990s AARON; the drawing was done by the program, but the colouring was done by hand. *San Francisco People* (1991), oil on canvas (painted by Harold Cohen), 60″ × 84″, collection of the artist. Reproduced by his permission

'Jungle-AARON' didn't have enough real 3D data about the body to be able to draw a human figure with its arms overlapping its own body. His early 1990s city-people could have waved, if he had wanted them to; but his late 1980s jungle-dwellers couldn't have crossed their arms instead of waving.

By 1995, Cohen had at last produced a painting-machine-based version of AARON which could not only draw acceptably but also colour to his satisfaction—using water-based dyes and five brushes of varying sizes. However, his satisfaction was still limited.

By the summer of 2002, he'd made another breakthrough: a digital AARON-as-colourer, whose images could be printed at any size. The program was regularly left to run by itself overnight, offering up about sixty new works for inspection in the morning. Even the also-rans were acceptable: one gallery curator who exhibited it told Cohen that "he hasn't seen AARON make a bad one since it started several weeks ago" (Cohen, personal communication). One might even say that AARON has now surpassed Cohen as a colour artist, much as Samuel's program surpassed Samuel as a checkers (draughts) player nearly half a century before. For Cohen regards this latest incarnation of AARON as "a world-class colorist" whereas he himself is merely "a first rate colorist" (personal communication).

A comparably spectacular, though later, AI artist was "Emmy"—originally EMI: Experiments in Musical Intelligence. This program was written in 1981 by the composer

David Cope (1941–), at the University of California, Santa Cruz. (It wasn't easy for other people to run, or experiment with, the early version. For the now familiar MIDI, or Musical Instrument Digital Interface, which defines musical notes in a way that all computers can use, wasn't yet available. Indeed, its inventor, Dave Smith, first had the idea in that very year, and didn't announce the first specification until August 1983: Moynihan 2003. Today, Emmy's successors are based on MIDI, so can be run on any PC equipped with run-of-the-mill musical technology.)

This wasn't the first attempt to formalize musical creativity (and nor was the Hiller and Isaacson effort). A system of rules for do-it-yourself hymn composition was penned in the early eleventh century by Guido d'Arezzo, who also invented the basis of tonic solfa and of today's musical notation (A. Gartland-Jones, personal). But Cope, almost 1,000 years later, managed to turn "formalize" into "implement". Moreover, his program could compose pieces much more complex than hymns, whether within a general musical style (e.g. baroque fugue) or emulating a specific composer (e.g. Antonio Vivaldi or J. S. Bach). It could even mix styles or musicians, such as Thai–jazz or Bach–Joplin, much as the Swingle Singers do.

Emmy gained a wide audience, though not as wide as AARON's. Part of its notoriety was spread by scandalized gossip: Cope has remarked that "There doesn't seem to be a single group of people that the program doesn't annoy in some way" (Cope 2001: 92). But as well as relying on word of mouth, people could read about it, and examine some Emmy scores, in Cope's four books (1991, 2000, 2001, 2006). Enthusiasts could even try it out for themselves, following his technical advice, by using one of the cut-down versions (ALICE: ALgorithmically Integrated Composing Environment, and SARA: Simple Analytic Recombinant Algorithm) provided on CDs packaged inside his books.

They could listen to Emmy's compositions, too. Several stand-alone CDs were released (by Centaur Records, Baton Rouge) in the late 1990s. In addition, several live concerts of Emmy's music were staged to public audiences. (These featured human instrumentalists playing Emmy's scores, because the program didn't represent expressive performance: it laid down what notes to play, not how to play them.)

However, the concerts were mostly arranged by Cope's friends: "Since 1980, I have made extraordinary attempts to have [Emmy's] works performed. Unfortunately, my successes have been few. Performers rarely consider these works seriously" (Cope 2006: 362). The problem, said Cope, was that they (like most people) regarded Emmy's music as computer "output", whereas he had always thought of it rather as *music*. Moreover, being "output" it was infinitely extensible, a fact—he found—that made people devalue it.

In 2004 he took the drastic decision to destroy Emmy's historical database: there will be no more "Bach" fugues from the program (2006: 364). Emmy's "farewell gift" to the historical-music world was a fifty-page score for a new symphonic movement in the style of Beethoven, which required "several months of data gathering and development as well as several generations of corrections and flawed output" (2006: 366, 399–451). From now on, Emmy—or rather, Emmy's much-improved successor—will be composing in Cope's style, as "Emily Howell" (p. 374).

Hofstadter, a fine amateur musician, found Emmy impressive despite—or rather, because of—his initial confidence that "little of interest could come of [its GOFAI] architecture". On reading Cope's 1991 book, he got a shock:

> I noticed in its pages an Emmy mazurka supposedly in the Chopin style, and this really drew my attention because, having revered Chopin my whole life long, I felt certain that no one could pull the wool over my eyes in this department. Moreover, I knew all fifty or sixty of the Chopin mazurkas very well, having played them dozens of times on the piano and heard them even more often on recordings. So I went straight to my own piano and sight-read through the Emmy mazurka—once, twice, three times, and more—each time with mounting confusion and surprise. Though I felt there were a few little glitches here and there, I was impressed, for the piece seemed to *express* something ... [It] did not seem in any way plagiarized. It was *new*, it was unmistakably *Chopin-like* in spirit, and it was *not emotionally empty*. I was truly shaken. How could emotional music be coming out of a program that had never heard a note, never lived a moment of life, never had any emotions whatsoever?
>
> [...Emmy was threatening] my oldest and most deeply cherished beliefs about ... music being the ultimate inner sanctum of the human spirit, the last thing that would tumble in AI's headlong rush toward thought, insight, and creativity. (Hofstadter 2001*a*: 38–9)

Hofstadter was allowing himself to be *over*-impressed, here. For Frederic Bartlett's "effort after meaning" (5.ii.b) imbues our perception of music as well as of visual patterns and words. The human performer projects emotion into the score-defined notes, much as human readers project meaning into computer-generated haikus (9.x.c). So, given that "Chopin-like" scores had been produced, it wasn't surprising that Hofstadter interpreted them expressively.

What was surprising was the Chopin-like musicality of the compositions. Simon's 1957 prediction that a computer would write aesthetically valuable music within ten years had failed, and had been mocked accordingly (H. L. Dreyfus 1965: 3). But Cope had now achieved this, though fourteen years late.

Emmy's basic method was described by Cope as "recombinatory", and summarized by Hofstadter as "(1) chop up; (2) reassemble" (p. 44). In fact, Emmy was exploring generative structures as well as recombining motifs. It showed both combinational and exploratory creativity—but not, as Hofstadter (2001*b*) was quick to point out, transformational creativity (Boden 1990*a*/2004). A 'new' style could appear only as a result of mixing two or more existing styles.

The program's database was a set of 'signatures' (note patterns of up to ten melodic notes) exemplifying melody, harmony, metre, and ornament, all selected by Cope as being characteristic of the composer concerned. Emmy applied statistical techniques to identify the core features of these snippets, and then—guided by general musicological principles—used them to generate new structures. (Some results worked less well than others—e.g. Cope 2001: 182–3, 385–90.)

Strictly, Emmy wasn't an exercise in "explaining" the ineffable. For Cope's motivation differed from Simon's (and Hofstadter's: see below). His aim wasn't to understand creative thought, but to generate musical structures like those produced by human composers. Initially, he'd intended EMI to produce new music in *his* style, but soon realized that he was "too close to [his] own music to define its style in meaningful ways", so switched to the well-studied classical composers instead (2001: 93). (A quarter-century later, having destroyed the historical database, he switched back to computer compositions in his own style—2005: 372 ff. and pt. III *passim*.)

In short, he was modelling music, not mind. (The task wouldn't necessarily have been easier had he known the psychological details; for instance, limits on short-term memory rule out the use of powerful generative grammars for jazz improvisation: Johnson-Laird 1989/1993.)

Nevertheless, the implication of Cope's writings was that all composers follow some stylistic rules, or algorithms. Sonata Form, for example, was supposed to be a formal structure rigidly adhered to by everyone composing in that style. This assumption has been questioned. It's known that Haydn, Mozart, and Beethoven (for instance) worked diligently through the exercises given in various musical textbooks. But whether they stuck rigidly to those rules in their more original, creative, music is quite another matter. Peter Copley and Drew Gartland-Jones (2005) argue that they did not.

On their view, the formal rules of style are extracted and agreed *post hoc*, and are then followed to the letter only by musical students and mediocrities. Once sonata form, or any other musical structure, has been explicitly stated, it tends to lose its creative potential. (This explains the "paradox" of sonatas in the romantic period being *far less* free than in the classical times: p. 229.) But the rules are flexible enough to evolve in use:

> It would be tempting to view this process as *generally agreed* forms, evolving. But . . . [it] would be more useful to see the general acceptances of common practice as *present to provide a frame for musical differences, changes, developments etc.* (Footnote: This is the basis for Boden's transformational creativity). If there is a constraint it comes organically from within a complex network of practitioners rather than [a] set of stated constraints that are accepted until someone decides they need breaking. This is a complex mechanism indeed, and even if we argue that algorithms might explain certain emergent patterns, it is not at all sure that such patterns stem from a simple, if enormously lengthy, set of rules. (Copley and Gartland-Jones 2005: 229; final italics added)

Cope's Emmy, they admit, can indeed compose many acceptable pieces. But even if it could generate fully "convincing" examples, "without a model of how [the abstracted rules] change we are capturing an incomplete snapshot of musical practice" (p. 229). In other words, Cope is modelling musical creations—not yet musical creativity.

c. In focus at last

It's no accident that those two highly successful programs, AARON and Emmy, were written by non-AI professionals. For they depended on a reliable sense of how to generate and appreciate structures within the conceptual space concerned (Boden 1990*a*/2004, chs. 3–4). In general, a plausible computer artist stands in need of an expert in art. Even if (like Hiller) the person isn't a professional artist, they need (again, like Hiller) to be a very highly knowledgeable amateur.

Often, artist and programmer are different people (e.g. William Latham and Stephen Todd, respectively: Todd and Latham 1992). But some artists are sufficiently computer-literate to design their own systems. Edmonds was a professional computer scientist in the 1960s, as well as being an influential artist. (Although he's always used his programs to help him understand creative thinking in general, he was less concerned than Cohen or Cope to talk about *the program as such*: if his viewers needed to realize that there was a program involved, they didn't need to think about just how it worked: see vi.c, below.) Today, forty years later, many young artists are computer-literate even if they're not computing professionals.

If artist programs need experts in art, much the same is true of programs focused on literature, maths, and science. A competent programmer is the sine qua non, but domain expertise is needed too. Since most AI researchers were reasonably proficient in those areas, one and the same person could be both programmer and expert. That's why AI work on creativity, once it got started, usually focused on them (rather than on music or, still less, visual art).

The everyday skill of analogy, which features in both literature and science, had been modelled as early as 1963 by Thomas Evans (1934–) at MIT. His program was a huge advance (T. G. Evans 1968). Implementing the ideas briefly intimated by Minsky in 'Steps' (see Figure 10.3), it not only discovered analogies of varying strength but also identified the best.

It could do this because it described the analogies on hierarchical levels of varying generality. Using geometrical diagrams like those featured in IQ tests (see Figure 13.5), Evans's program achieved a success rate comparable to that of a 15-year-old child. But it wasn't followed up. (This was an example of the lack of direction in AI research that so infuriated McDermott: 11.iii.a.)

By the early 1980s, analogy had returned as a research topic in AI. For instance, an international workshop held in 1983 included five papers explicitly devoted to it, plus several more that could be seen as relevant (Michalski 1983, esp. 2–40).

Evidently, the AI scientists concerned didn't read Fodor's *Modularity of Mind* (1983), or anyway didn't accept its pessimism about explaining the higher mental processes. Fodor despaired of any attempt to understand analogy in scientific terms. Despite its undeniable importance, he said,

> nobody knows anything about how it works; not even in the dim, in-a-glass-darkly sort of way in which there are some ideas about how [scientific] confirmation works. (Fodor 1983: 107)

And, according to him, they never would: "Fodor's First Law of the Nonexistence of Cognitive Science [states that] the more global . . . a cognitive process is, the less

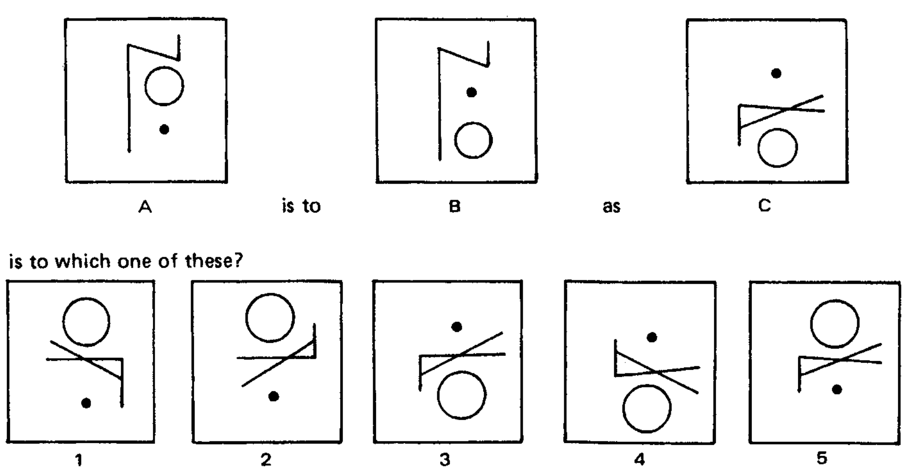


FIG. 13.5. Analogy problem tackled by Evans's program. Reprinted with permission from (Minsky 1968: 332)

anybody understands it." (As we saw in Chapter 7.vi.h, he was right in arguing that we'll never be able to predict/explain every case of analogy in detail, but wrong in concluding that nothing of scientific interest can therefore be said about it.)

Fodor's scepticism notwithstanding, several GOFAI scientists in the 1980s tried to model how conceptual analogies are generated, interpreted, and used (Gentner 1983; Holyoak and Thagard 1989; Thagard *et al.* 1988; Gentner *et al.* 1997). Like Newell and Simon before them, they sometimes went back to the Gestalt psychologists' 1930s work on problem solving (Chapter 5.ii.b). For instance, they asked (taking Karl Duncker's example) how the notion of a besieging army could help in discovering how to irradiate a tumour without killing the surrounding tissues.

In general, they wanted to know just how an analogous idea could be identified, and fruitfully mapped onto the problem at hand. The answer usually given was to show how distinct conceptual structures could be compared, and—if necessary—adapted so as to match each other more closely. Sometimes, the problem-solver's goal was allowed to influence the comparison. But the basic process was like that used long before by Patrick Winston's concept-learner (10.iii.d): comparing abstractly defined, and pre-assigned, structures.

A blistering critique of these "inflexible" and "semantically empty" approaches was mounted by Hofstadter (Hofstadter and FARG 1995: 55–193). He complained that they were less interesting than Evans's work of twenty years before, and radically unlike human thinking. (Although both these charges were fair, his stinging critique wasn't entirely justified; for 'structuralist' replies, see Forbus *et al.* 1998; Gentner *et al.* 1997.)

But Hofstadter didn't share Fodor's gloom about the impossibility of *any* scientific understanding of analogy. To the contrary, he'd already spent many years studying it, using a basically connectionist approach. He'd been thinking—and writing—along these lines since the early 1970s (Chapter 12.x.a).

By the mid-1980s, he and his student Melanie Mitchell had implemented the Copycat program (Hofstadter 1985*a*, chs. 13 and 24, 2002; M. Mitchell 1990/1993; Hofstadter and Mitchell 1993/1995; Hofstadter and FARG 1995, chs. 5–7). This was described in several seminars at MIT in 1984, although without attracting much attention at the time (personal communication).

Intended as a simulation of human thinking, it modelled the fluid perception of analogies between letter strings. So it would respond to questions like these: "If *abc* goes to *pqr*, what does *efg* go to?", or (much trickier) "If *abc* goes to *abd*, what does *xyz* go to?" (Lacking a 'circular' alphabet, it couldn't map *xyz* onto *xya*; instead, it suggested *xyd*, *xyzz*, *xyy* . . . and the especially elegant *wyz*.) Its descriptors were features such as *leftmost*, *rightmost*, *middle*, *successor*, *same*, *group*, and *alphabetic predecessor/successor*. Like Evans's program, Copycat could generate a range of analogies, and compare their strength. Unlike Evans's program, it was probabilistic rather than deterministic, and could be 'primed' to favour comparisons of one type rather than another.

Whereas Copycat worked on letter strings, Hofstadter's Letter Spirit project focused on complex visual analogies. Specifically, it concerned the letter-likenesses and letter-contrasts involved in distinct alphabetic fonts. An *a* must be recognizable as an *a*, no matter what the font; but seeing other letters in the same font may help one to realize that it is indeed an *a*. At the same time, all twenty-six letters within any one font must share certain broad similarities, all being members of *that* font (see Figures 13.6 and 13.7).

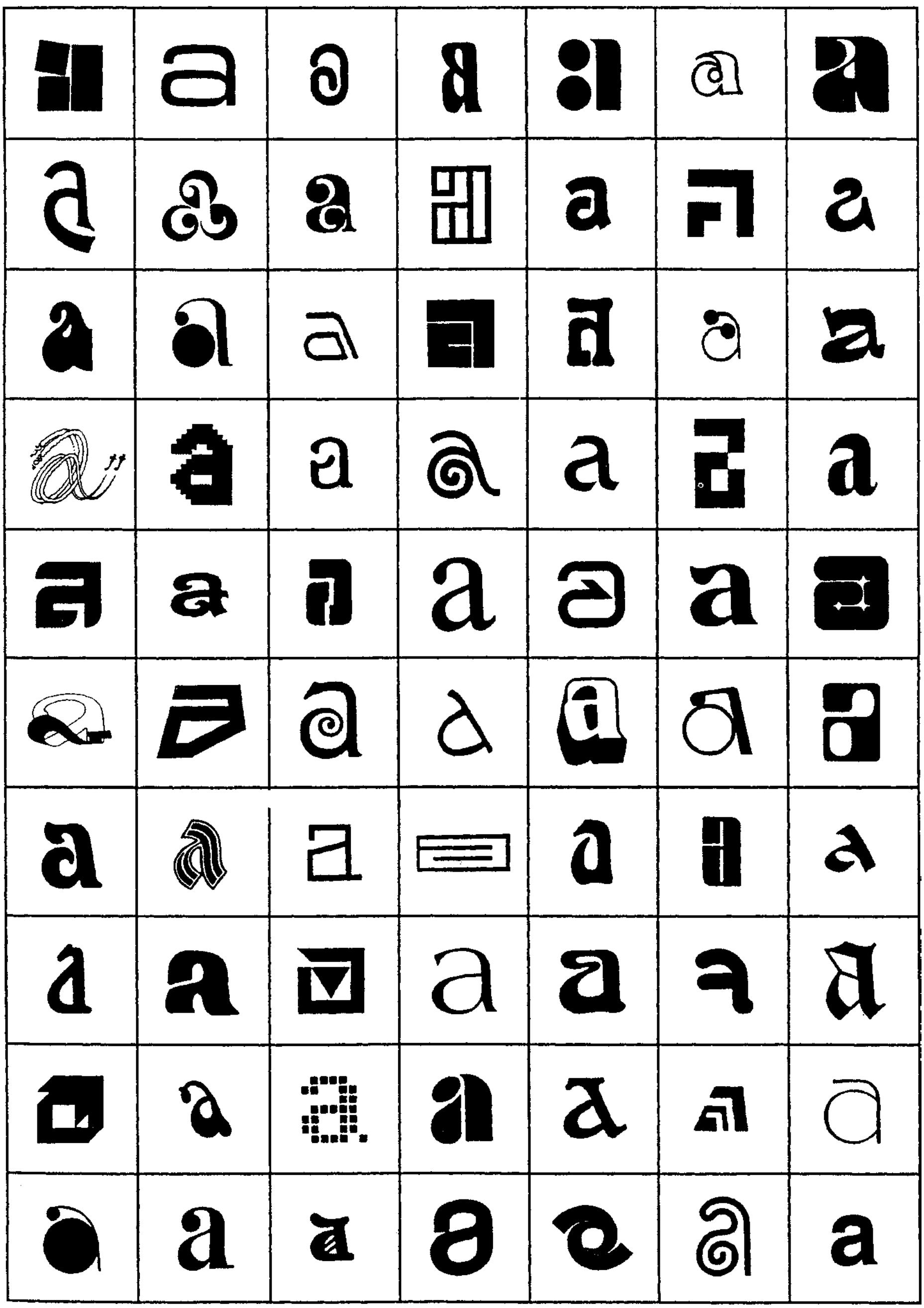


FIG. 13.6. The letter 'a' written in different fonts. Reprinted with permission from Hofstadter and FARG (1995: 413)

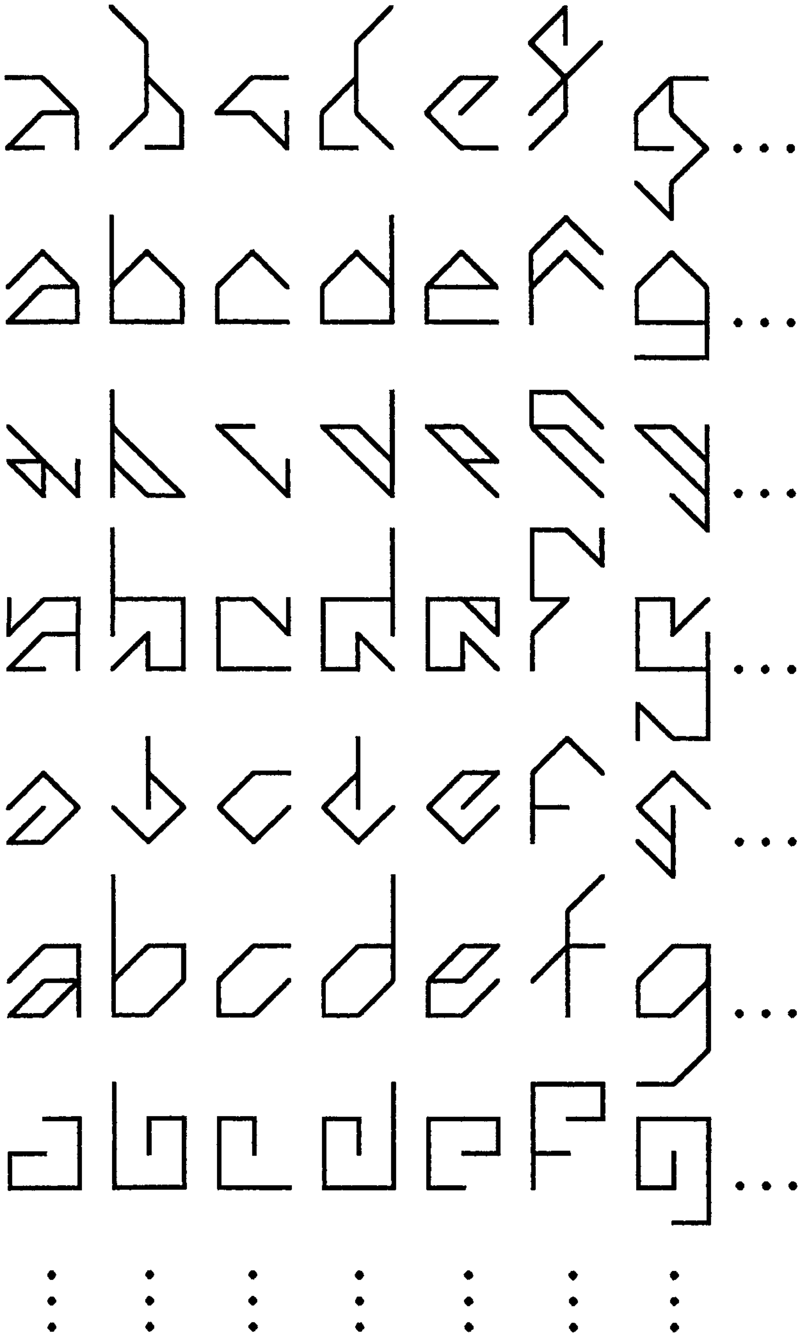


Fig. 13.7. Different fonts based on the Letter Spirit matrix. Reprinted with permission from Hofstadter and FARG (1995: 418)

Hofstadter's early writings on Letter Spirit had outlined a host of intriguing problems involved in interpreting and designing fonts (e.g. Hofstadter 1985*b*). And in the mid-1990s he described a program capable of recognizing letters in a variety of styles (Hofstadter and FARG 1995: 407–96; McGraw 1995; Hofstadter and McGraw 1995). By the new millennium, the design aspect of Letter Spirit had been part-implemented too (Rehling 2001, 2002). The new program could design an entire alphabet, given five 'seed' letters (*b, c, e, f, g*) as a guide. Today, the group's aim is to generate an alphabet from only a single seed.

Letter Spirit is the most ambitious AI analogy project, and in my view the most interesting. Whether it can readily be applied to other domains, however, is unclear. Even the much simpler Copycat would be difficult to generalize. However, if what one is interested in is how it's possible for human beings to engage in subtle and systematic analogical thinking, then it's a significant contribution. It's no simple matter to generate or appreciate an alphabetic font. So much so, indeed, that hardly anyone other than Hofstadter would have dreamt that anything cogent could be said about the computational processes that may be involved.

Turning from analogy to story writing, the best AI story-writer was authored not by a novelist or literary critic but by a computer scientist, now a professional games designer: Scott Turner (1994). (He's not to be confused with Mark Turner, a teacher of English at the University of Maryland who turned to cognitive science to illuminate the interpretation and creation of literature: M. Turner 1991; Fauconnier and Turner 2002.)

This program didn't produce high-quality literature. Its plots were simplistic tales about knights, princesses, and dragons, and its use of English left a great deal to be desired. But it had three interesting features.

First, it used case-based reasoning (ii.c, above) to create new story plots on the basis of old ones. For example, it generated a concept/episode of *suicide* (derived from *killing*) when the story's plot couldn't be furthered by third-party fighting. Second, it relied on the latest version of the Yale analysis of motivational and planning schemata to decide what might plausibly be done, and how (cf. 7.i.c and 9.xi.d). And third, Turner had realized—what previous AI programmers had not (e.g. Meehan 1975)—that a story needs not only goals and plans for each character involved in the plot, but also *rhetorical* goals and plans for the storyteller. Accordingly, a character's goals were sometimes rejected by the program, or their expression suppressed in the final narrative, for reasons of story interest or consistency. (For a sustained critique of Turner's approach, see Bringsjord and Ferrucci 2000.)

By the mid-1990s, AI had even made good on Babbage's suggestion that puns follow "principles" including double meanings and similar pronunciation of differently spelt words (Chapter 3.iv.a). Kim Binsted, at the University of Edinburgh, wrote a program—JAPE—that originated punning riddles fitting nine familiar templates, such as *What do you get when you cross an X with a Y?*, *What kind of X has Y?*, *What kind of X can Y?*, and *What's the difference between an X and a Y?* (Binsted 1996; Binsted and Ritchie 1997; Binsted *et al.* 1997; Ritchie 2003*a*). JAPE used a semantic network of over 30,000 items, marked for syllables, spelling, sound, and syntax as well as for semantics and synonymy. The program would consult the templates (no simple matter) to generate results including these: *What do you call a depressed train? A low-comotive; What do you call a strange market? A bizarre bazaar*; and *What kind of murderer has* *fibre? A cereal killer*. Babbage's "triple pun" (cane/Cain, a bell/a belle/Abel) couldn't have been created by JAPE, but its puns were no more "detestable" than most. (In fact, it's the most successful of today's AI jokers—Ritchie 2001; 2003*b*, ch. 10.)

As for creative mathematics, great excitement (within AI, if not outside it) was caused in the late 1970s by Lenat's program AM, or Automated Mathematician (Lenat 1977). This was his doctoral thesis at Stanford. Starting from a few simple concepts of set theory, it used 300 heuristics for modifying concepts, and criteria of mathematical "interestingness" (occasionally supplemented by specific nudges from the programmer) to generate many concepts of number theory. These included *integer*, *addition*, *multiplication*, *square root*, and *prime number*. It even generated a historically novel concept, which was later proved as a (minor) theorem, concerning *maximally divisible numbers*—a class which Lenat himself hadn't heard of. Later, he recalled how he found out about them:

> [I wondered whether any mathematician had thought of such a thing. Polya seemed to be the only one who knew.] He said, "This looks very much like something the student of a friend of mine once did." Polya was about 92 at the time. It turned out the friend was [Godfrey] Hardy and the student was Ramanujan. (D. B. Lenat, interviewed in Shasha and Lazere 1995: 231)

Coming up with something that had been discovered by such a giant as Srinivasa Ramanujan (1887–1920), people felt, was no mean feat.

Whether the excitement was fully justified was another matter. Critics pointed out that Lenat hadn't made clear just how the interesting concepts were generated, and suggested that a heuristic that was crucial for generating the notion of primes had been included, whether consciously or not, so as to make this discovery possible (Ritchie and Hanna 1984). What's more, they said, it may have been used only the once. (No detailed trace of the program was available.)

Lenat replied that AM's heuristics were fairly general ones, not special-purpose tricks, and that (on average) each heuristic contributed to two dozen different discoveries and each discovery involved two dozen heuristics (Lenat and Seely Brown 1984). He admitted, however, that writing AM in LISP had given him a tacit advantage. For minor changes to LISP syntax were relatively likely to result in expressions that were mathematically interpretable.

A few years later, yet more excitement was caused by Lenat's EURISKO, which—satisfying McDermott's plea for program development (11.iii.a)—incorporated heuristics for modifying *heuristics* (Lenat 1983). Besides being used to help plan experiments in genetic engineering, it came up with one idea (a battle-fleet design) that won a war-game competition against human players, and another (a VLSI chip design) that won a US patent—which are awarded only for ideas not "obvious to a person skilled in the art". Lenat himself then switched to research on CYC. But his AM program led others to focus on how mathematical "interestingness" could be used in automating creative mathematics (for a review, see Colton *et al*. 2000).

What of Simon himself? His late-century programming efforts were devoted to explaining creativity in science, although he gestured towards the humanities from time to time (Simon 1994*b*). With Langley and others at CMU, he wrote a suite of increasingly powerful programs intended to model the thought processes of creative scientists such as Francis Bacon, Joseph Black, Johann Glauber, Georg Stahl, and John

Dalton. These were initiated in the late 1970s, and continually improved thereafter (Langley 1978, 1979, 1981; Langley *et al.* 1981, 1987).

These inductive systems generated many crucial scientific principles—quantitative, qualitative, and componential. They came up with Archimedes' principle of volume measurement by liquid displacement, the very origin of *Eureka!* And they rediscovered Ohm's law of electrical resistance, Snell's law of refraction, Black's law of the conservation of heat, Boyle's law relating the pressure/volume of a gas, Galileo's law of uniform acceleration, and Kepler's third law of planetary motion. Occasionally (e.g. with Snell's law), they used a symmetry heuristic to choose between more and less elegant, though mathematically equivalent, expressions. Some could produce hypotheses to explain the observed data patterns, whether mathematical (e.g. Black's law) or qualitative (the chemical patterns observed by Glauber, and componentially explained by Stahl and Dalton). And the later versions could use the real (i.e. messy, imperfect) historical data, not just data doctored to make the sums come out exactly right.

Simon wanted to *explain* the ineffable, not just—as in meta-DENDRAL, or Emmy—to mimic it. So his group aimed to keep faith with human psychology.

Their programs, accordingly, respected the details recorded in the laboratory notebooks of the scientists concerned. For example, they used the same data that the long-dead authors had used. (Or rather, they used the same verbal/mathematical data: they couldn't accept visual/auditory/haptic input from the real world, nor recognize similarities between sensory patterns—cf. Chapter 12.v–vi.) And as well as generating the same scientific laws, they tried to match the heuristics that had been used by the human scientists, and the temporal order of their hunches and discoveries—and mistakes. Later work by the CMU group focused, for instance, on the general principles of how to suggest and plan experiments (Kulkarni and Simon 1988), and on the use of diagrams in scientific discovery (Cheng and Simon 1995).

At one level, the BACON program and its siblings (BLACK, GLAUBER, STAHL, and DALTON) were highly impressive. They offered successful models of induction, and illuminated certain aspects of how many human scientists go about their work. (Despite the common propaganda, some science is *not* concerned with numbers, nor with componential structure: Chapter 7.iii.d.) But the creativity involved was exploratory rather than transformational.

In other words, these programs were spoon-fed with the relevant questions, even though they found the answers for themselves. What their human namesakes had done was different, for they'd viewed the data in new ways. Indeed, they'd treated new features as data. To identify *mathematical* patterns in the visual input from earth or sky had been a hugely creative act when Galileo Galilei first did it. To look for numerical constants was another, and to seek simple linear relationships *before* ratios or products (search priorities that were built into the heuristics used by the BACON suite), yet another. The CMU programs were deliberately provided with the ways of reasoning which Bacon, Glauber, Stahl, and Dalton had pioneered for themselves. They were roundly criticized by Hofstadter as a result (Hofstadter and FARG 1995: 177–9; see also H. M. Collins 1989).

(This, of course, recalls Haugeland's principled objection to GOFAI-based models of "insight". It also relates to the worries about "open-ended" evolution mentioned below and discussed in Chapter 15.vi.d. We'll see there that *totally new types* of sensor have been evolved in artificial systems, but only by unexpectedly taking advantage of the contingencies of the physical environment and/or hardware. Compare these 'biological' examples with Haugeland's psychological claim that understanding pertains to "the world and to living in it".)

In short, Saul Amarel's (1968) hope that an automated system might come up with a radically new problem representation remained unfulfilled. And not only Amarel's, for Simon himself had expressed much the same hope in the 1950s. The LT team's harbinger memo had listed four characteristics of "creative" thought, of which the last was this: "The problem as initially posed was vague and ill-defined, so that part of the task [is] to formulate the problem itself."

That's *still* beyond the state of the art. Langley (1998) recently reviewed seven cases of AI-aided discovery that were sufficiently novel, and interesting, to be published in the relevant scientific journals. He pointed out that, in every case, the programmers had been crucial in formulating the problem and/or manipulating the data and/or interpreting the results. So Ada Lovelace's futuristic vision of science-by-machine has been realized: many new answers have been found automatically, and some new questions too (e.g. new experiments). But *fundamental reformulations of old problems*, still less *radically new* ones, have not. (As we saw in Chapter 3.iv.b, Lovelace wouldn't have been at all surprised by that. On the contrary, it's just what she expected.)

In one sense, it's what Simon himself expected, too. For machines, at present, lie outside the cooperative loop (Chapter 2.ii.b–c). Simon described scientific discovery as a matter of "social psychology, or even sociology" (1997*a*: 171). His main reason was that open scientific publication provides a "blackboard" that hugely extends the individual scientist's memory (p. 172). At present, machine discovery systems "are still relatively marginal participants in the social system of science". We might be able to hook them up to data-mining programs, to reduce their reliance on human beings for providing their data and problems. However, "Even this is a far cry from giving machines access to the papers, written in a combination of natural, formal and diagrammatic language, that constitute a large part of the blackboard contents" (p. 173).

As for negotiations about the value of supposed "discoveries" (Chapter 1.iii.f and Boden 1997), Simon's view was that "the machine (augmented now by the computer) has already, for perhaps a hundred years, been a member of the society of negotiators" (Simon 1997*b*: 226). (Here, he cited the prescient Henry Adams, who'd been so deeply troubled by his visit to the dynamo hall in an industrial exhibition: H. Adams 1900.) Future AI programs, said Simon, might persuade us to value their discoveries above our own previous judgements (1997*b*: 225). He added that this was already happening in the area of mathematical proof (but that's not straightforward: see MacKenzie 1995).

Disputes about what counts as a discovery are especially likely in cases of transformational creativity, in which previously valued criteria are challenged. This type of creativity *was* eventually modelled, up to a point, by evolutionary programs. Some were focused on art (Sims 1991; Todd and Latham 1992), some on music (Gartland-Jones and Copley 2003; P. W. Hodgson 2002, 2005), and some on science or engineering (Goldberg 1987; Sims 1994; Ijspeert *et al*. 1997).

To some extent, genetic algorithms (Chapter 15.vi) could transform the conceptual space being explored by the program. For example, Paul Hodgson, an accomplished jazz saxophonist, wrote several programs designed to improvise Charlie Parker-style jazz in real time. The first two, IMPROVISER and VIRTUAL BIRD, used brief melodic motifs as primitives (these weren't statistically culled, as in Cope's work, but were based on a systematic theoretical analysis of music: Narmour 1989, 1992). VIRTUAL BIRD played well enough, early in the new century, that the world-famous Courtney Pine was willing to perform alongside it. Another, an evolutionary version called EARLY BIRD, used only dyadic (two-note) primitives (P. W. Hodgson 2005). It explored 'Bird-space' even more adventurously than its predecessor, partly because of the transformations it generated and partly because its primitives were less highly structured. Even so, it wasn't 'transformational' in the sense of generating a recognizably different musical style. Hodgson felt that this would be possible, but only if he himself added a great deal more musical information to constrain the changes allowed (personal communication).

That's hardly surprising, for in all such evolutionary programs the fitness function was due to the human being, whether built into the program or provided interactively. Peter Cariani (1997) has criticized current GAs accordingly, arguing that they're incapable of the sort of "open-ended" creativity seen in biological evolution. We'll see in Chapter 15.vi.d, however, that wholly new sensory organs *have* been evolved in artificial systems, by a combination of GAs and environmental/hardware contingencies. Perhaps future research on psychological creativity will take a leaf out of this biological book?

Biological evolution is not only open-ended, but unpredictable. And most creative ideas are unpredictable, too. There are various reasons why that's so (Boden 1990*a*/2004, ch. 9; see also Chapter 17, below). But that doesn't mean that AI creativity researchers were wasting their time. One may be able to explain something—to show *how it is possible*—without also being able to predict it (7.iii.d). Much as a theoretical psychology could never predict every passing fancy or every suicidal thought within Jo Bloggs's mind, so it could never predict every creative idea. It might be able to say a great deal, however, about the general types of ideas that were likely or unlikely, and why. That, you'll remember, had been Cohen's aim when he embarked on AARON in the first place; and it was Simon's aim, too.

By the turn of the millennium, then, AI research on creativity had at last become respectable. Several books on the topic had appeared, written or edited by long-standing members of the community (Michie and Johnston 1984; Boden 1990*a*; Shrager and Langley 1990; Partridge and Rowe 1994; Hofstadter and FARG 1995). The interest was spreading way beyond a few enthusiasts. The *Stanford Humanities Review* published two book-length special numbers on AI and creativity, especially in relation to literature (Guzeldere and Franchi 1994; Franchi and Guzeldere 1995).

The respectability suddenly snowballed into a range of professional meetings. IJCAI-1997 commissioned a keynote presentation on the topic (Boden 1998*a*). And a flurry of creativity conferences and workshops were mounted by AI and A-Life researchers. These included a continuing series on 'Discovery Science', which complemented the 'Creativity and Cognition' meetings on computer art that had been organized in the UK for many years past by Edmonds (vi.c, below).

If the ineffable hadn't yet been fully explained, *that it was truly ineffable* was now highly doubtful.

13.v. Outreach to Everyman

You may remember Babbage's lady visitor, who asked: "Now, Mr. Babbage, there is only one thing that I want to know. If you put the question in wrong, will the answer come out right?" (Chapter 3.i.a). Harriet Martineau was outraged, seeing this as a waste of the great man's time.

In 1960 many might have shared her outrage, especially if they'd never read Vannevar Bush (10.i.h). After all, most people hadn't ever touched a computer, and it wasn't at all clear that they ever would. The easy assumption, at that time, was that anyone who used a computer would be a computer expert so the issue of "putting the question in wrong" simply wouldn't arise.

What a mistake that was! Even by then it was clear that programming languages were needed, both to prevent *the computer experts themselves* from putting the questions in wrong and to enable them to ask the right questions in the first place (10.v.a–c). And error-detecting compilers were soon devised to alert the computer buffs to some of their mistakes. (In the mid-1970s, IBM's PL/1 tried to help programmers to remove bugs: Des Watson, personal communication. But even today, 'silent' automatic error correction is used only for very simple/common cases; instead, diagnostic error messages help the human to correct what's been put in wrong.)

Above all, it was a mistake to assume that *ordinary people*—from doctors and lawyers to the man on the Clapham omnibus—wouldn't be interacting with computers. Indeed, a few privileged doctors were interacting fruitfully with MYCIN by about 1970. But the man on the Clapham omnibus would have to wait until the mid-1980s, when user-friendly home computers became available.

Today, computer scientists take it for granted that users will often be ignorant, careless, impatient . . . in a word, human. So anticipating their mistakes, and enabling human–computer interaction (HCI) to be so intuitively natural that mistakes are minimized, is a thriving research area (D. A. Norman and Draper 1986; Sharples 1996; Rheingold 2000). The softbots, or intelligent agents, mentioned in Section iii.d are an example: human users who can rely on a softbot's doing something not only save time and effort, but avoid the mistakes they might make if they tried to do it for themselves.

This section (and the next) describes how GOFAI helped to bring user-friendly computing about. As we'll see, GOFAI itself benefited from the exercise. In making Bush's "memex" a reality, AI scientists would devise ways of interacting with computers without which today's AI simply couldn't exist. Computer graphics and object-oriented programming languages, for instance, were initially developed to help fulfil Bush's prophetic vision.

a. Papert and the media lab

In a sense, "user-friendly home computers" had been available ever since the late 1960s. For Papert had provided a programming language, LOGO, that was intelligible to very young children (10.vi). He'd designed homely forms of implementation too: push-buttons instead of keyboards, for use by clumsy-fingered infants, and line-drawing "turtles" moving on the CDU screen or on the floor. Much of LOGO's success lay in the fact that it resulted in (real or virtual) drawings, rather than text. For visual imagery is more basic, more intuitive, than writing.

Someone might object that LOGO turtles weren't really computers, just programmable toys. But that would be too quick, and too dismissive. For Papert had always aimed to make computing in general more accessible to non-specialists. What's more, his vision and example played a large part in inspiring others to develop the home computers we know today.

Papert's role in encouraging user-friendly computing grew even more prominent in the 1980s than it had been before. Besides developing StarLogo, which made distributed computing possible even for children, he intensified his activities in public education. For instance, in 1985 he started an educational/experimental programme in Boston's James E. Hennigan School, offering the LOGO experience to children drawn from a deprived area of the city (Brand 1988, ch. 7).

Still more to the point, he moved sideways within MIT: having begun in the young AI Lab, in 1986 he joined Nicholas Negroponte's (1943–) newly founded Media Lab. In other words, he was a key player in the Media Lab's activities from the start. Indeed, he'd been a player even *before* the start. For the Media Lab's real beginnings were twenty years earlier: it was the successor of Negroponte's late 1960s Architecture Machine Group, or AMG (Brand 1988: 137–54). Papert had been closely involved with AMG. (So had his close collaborator Minsky, who is now the Toshiba Professor of Media Arts and Sciences, as well as being an MIT Professor of Electrical Engineering and Computer Science.)

The Media Lab would turn out to be crucial in developing various futuristic technologies, especially VR. Whereas the AI Lab itself had grown more technological and less psychological (and so less to Papert's taste), the Media Lab took the two dimensions of AI equally seriously. That's because their prime focus was on human–computer *interaction*. (A secondary focus was on what's now called A-Life—see Chapter 15, and Brand 1988, ch. 6. Selfridge, currently working on adaptive agents, is now yet another of the Media Lab's pioneer stars: Selfridge and Feurzeig 2002.)

The AMG had pioneered 'exploratory' computer graphics of the type foreseen by Douglas Engelbart (10.i.h). They made sure that even architectural designers—and even architects' clients—with no prime interest in computers would be able to use their software. Besides experimenting with the computer-aided design (CAD) of individual buildings, they also explored Negroponte's (1970, 1975) interests in cityscapes. In general, they aimed to achieve Joseph Licklider's (1960) "symbiosis" between human and computerized visual designers, whether in architecture or beyond.

For example, AMG produced the first detailed VR simulation of an actual town (Aspen, Colorado) in 1978–9. Its users could explore the simulated streets as they wished, see the buildings from varied perspectives, and even enter some of them—and all this in any one of the four seasons.

The "Aspen Movie-Map" had been funded by DARPA, because of the Pentagon's interest in the use by Israeli commandos of a (physical) mock-up of Entebbe airport for practising the freeing of the hostages held in 1973 (Brand 1988: 141). Not quite everyone was impressed: the Movie-Map was one of the visionary research projects ridiculed by Senator William Proxmire (see 6.iv.f). But as so often happened, the Senator's scorned targets eventually had the last laugh—and everyone playing video games today should be laughing with them. (Although the US Army's current VR training environment and children's game are no laughing matter: see <http://www.ict.usc.edu> and <http://www.americasarmy.com>.)

Also funded by DARPA, and also a seed of today's technology for VR 'telepresence', was the Talking Heads project (Brand 1988: 91–3). This was intended to enable the US's five top leaders to run the country together while sheltering from a nuclear attack in five different locations. Among other things, it initiated attempts to model realistic lip–sound synchronization for speech (9.xi.g).

The Media Lab, then, was highly interdisciplinary. Architects mingled with biologists, psychologists, linguists, acousticians, musicians, and educationists—and everyone mingled with AI scientists. Indeed, as befits genuine interdisciplinarity, much of the mingling went on *inside* individuals' heads.

One of the key interdisciplinary heads in this enterprise was Papert. On joining the Media Lab, he set up the Epistemology and Learning Research Group (Harel and Papert 1991). This was no ivory-tower containment. Besides many educational consultancies, he founded a successful commercial venture (LOGO Systems Inc.) in 1981, and the non-profit Logo Foundation ten years later (<http://el.media.mit.edu/logo-foundation>). These provided a wide range of software, books, and advice on educational methods inspired by Piagetian ideas in general and LOGO in particular.

Papert is well known today for various reasons. His fame within the AI community—and with most cognitive scientists—rests primarily in his cooperation with Minsky on *Perceptrons* and *The Society of Mind* (Chapters 12.iii.d and 7.i.e), and secondarily in his trenchant rebuttal of Dreyfus (11.ii.b). In addition, these people remember his pioneering work on LOGO.

The general public, and educational psychologists, know of him through LOGO and also through his wider interests in pedagogy. He's been a highly visible gadfly on the back of the educational establishment for nearly forty years. Like the counter-cultural Ivan Illich (1971), but with a much stronger theoretical base, he was a fierce critic of orthodox practices in the 1970s. Many years later, he's still pursuing radical reform of the school system (Papert 1993).

There's a third reason why Papert *should* be remembered. It's unknown to most people, even among his professional peers. But it's the one which is especially important here. Namely: his AI work has influenced every computerized desktop, and almost every multimedia application, of the twenty-first century.

For it was Papert's Piagetian approach, and his child-friendly LOGO language and display, which inspired Kay to design computer interfaces for use by Everyman (see subsection d, below). Without those, today's world of widespread personal computing would be impossible. And he encouraged Negroponte to try to simulate architectural spaces (in AMG), and to set up the Media Lab to explore the possibilities of realistic simulation in general (including A-Life). That too was an exercise in the design of novel, and relatively intuitive, interfaces. It was guided, according to Negroponte's initial proposal, by studies in "epistemology [and] experimental psychology" (Brand 1988: 11)—code words meaning Papert.

Minsky and Jerome Wiesner were also hugely important influences. Indeed, the well-connected Wiesner—just retired, in 1980, as President of MIT and previously science adviser to the Kennedy and Johnson administrations—was able to be even more supportive of the Media Lab than he had been of the infant AI Lab, a quarter-century before (10.ii.a). He and Negroponte "toured and lectured and demoed and bargained for seven years, and raised the requisite millions" (Brand 1988: 11; cf. 131–7).

In short, Papert helped to develop virtual *machines* into virtual *reality*. His help was largely indirect, but crucial nevertheless. The user-friendly LOGO was eventually surpassed by SMALLTALK, the push-button boxes by menus and icons. And the on-screen LOGO turtles made way for on-screen Aspen . . . and for the host of VR applications we know today.

b. The H in HCI

HCI is a special case of what Donald Norman (1986*a*) called "cognitive engineering": the area of "applied cognitive science" which deals with how we use tools—from bath taps to computers. Since *Home sapiens* is above all a tool-using species, cognitive engineering has ramifications in many areas. Indeed, if—with Bruner (and Engelbart: 10.i.h)—one regards even language and drawing as cognitive *technologies*, then cognitive engineering is potentially relevant to virtually every aspect of human life.

As Norman pointed out, any adequate study of tool use is going to need not only an analysis of task complexity, but also "a theory of action". It's no accident, then, that he's helped to develop a general psychological theory of action. Besides influencing HCI (and tool design in general: D. A. Norman 1988), this is now influential in clinical neuroscience (Chapters 12.ix.b and 14.x.b). It's also no accident that two early papers on the general principles of HCI were largely due to him (D. A. Norman 1986*a*; W. J. Hutchins *et al.* 1986).

HCI work falls broadly into two groups. On the one hand, it involves writing user-friendly programs *to be run on a computer*. These may concern education and professional training, for instance—a huge area today, with several dedicated conferences/journals. On the other hand, it involves designing user-friendly interfaces *to be part of the computer itself*. As this section will indicate, these concerns may overlap. A successful interactive VR program, for example, needs to be easily usable by people who aren't AI experts.

One aspect of a successful interactive program is that it makes the computer, and the program running it, invisible. That is, it gives users

> the qualitative feeling that we are *directly* engaged with control of the objects [of interest to us]—not with the program, not with the computer, but with the semantic objects of our goals and intentions. (E. L. Hutchins *et al.* 1986: 95; italics added)

The writer using a word processor, for example, has the illusion that they're dealing directly with words, lines, or paragraphs—none of which exist as such inside the physical machine. Rather, they are structures in the virtual machine. Likewise, someone playing a computer game feels that they're zapping the goblin itself, or negotiating a real ski jump. It should be no surprise, then, that early HCI involved the development of 'direct' image manipulation on the one hand, and of object-oriented programming on the other.

The invisibility of the tool, to the tool-user, is a very old idea. René Descartes pointed out that the blind man's stick is, in effect, an extension of his body. More recently, phenomenologists such as Merleau-Ponty have written at length on this theme; and David Sudnow (1978/2001) has described the simultaneous growth of power and invisibility in learning to play the piano. So HCI is a special case of the study of "in-dwelling" in one's tools.

HCI has both technological and psychological aims, for it seeks to expand *the mind* as well as the computer. (This, of course, is just what Engelbart predicted nearly half a century ago.) It's also an aspect of cognitive science. To write easily usable software, one has to know something about how the "H" in HCI functions—and how that mental functioning enables, or prevents, the I between the H and the C. Kay, perhaps the most famous of all interface designers, put it like this:

> [The] actual dawn of user interface design first happened when computer designers finally noticed, not just that end users had functioning minds, but that *a better understanding of how those minds worked* would completely shift the paradigm of interaction. (A. C. Kay 1990: 123; italics added)

Kay dated this realization as happening "to many computerists in the late sixties". And, as noted above, it was the guiding principle of MIT's Media Lab. However, both Bush and Gordon Pask had achieved it much earlier. Bush, indeed, was the first person to realize the need for HCI (Bush 1945, sects. 6–8). His memex was intended to achieve a man–machine fit by exploiting associative processing—and visual imagery—in both (10.i.h).

The memex was purely imaginary. Twenty years later, Pask looked more closely at *just how* people make what Bush had called associations. And he used what he found to pioneer human-friendly AI teaching machines. Pask's mid-1960s machines were designed for use by learners with three fundamentally different cognitive styles (Chapter 4.v.e). If he didn't allow for the questions being put in "wrong", he did allow for them being put in three contrasting ways. However, his ideas didn't catch on. In part, this was because they'd need to be reapplied in each subject domain. For Pask's HCI concern wasn't how to improve the computer itself, but how to write helpful educational programs for it.

Papert, too, was developing teaching machines in the mid-1960s. He wasn't allowing for human users' individual differences, as Pask was. But he was allowing for differences in age: his machines were designed to be accessible even to very young children, as we've seen.

Partly because of the child-friendly interface, Papert's ideas did catch on. During the 1970s, LOGO systems spread like wildfire (or like successful memes: Chapter 8.v.c). By the end of the decade, others were applying his Piagetian ideas about self-critical learning (as opposed to the LOGO language) in AI teaching machines. So they were used, for instance, to improve children's arithmetic, or university students' programming skills (Brown and Burton 1978; O'Shea and Young 1978; Brown and VanLehn 1980; Burton 1982; Sleeman and Brown 1982; O'Shea and Self 1984).

Babbage's lady visitor certainly wouldn't have outraged Papert, for his key point was that "putting the question in wrong" can be highly instructive. In other words, *bug* is a "powerful idea", and bug correction helps one think at a meta-level about one's own thinking (see 10.vi). But if that aspect of his psychological theory was widely taken up by others, his turtles weren't. They were restricted to simple drawing and/or locomotion.

What was required to go further was an interface that could be used for many different purposes. The 'universal' digital computer needed a 'universal' interface.

c. Good ideas in hibernation

The most important interface designers, besides Engelbart, were Sutherland and Kay. (For others, see: Laurel 1990; Rheingold 2000.) They wouldn't have been outraged by Babbage's visitor either. But besides hoping to minimize the non-specialist user's mistakes, they aimed to enable users—including AI scientists—to do things 'naturally' on computers which couldn't be done at all before.

To do that, they designed fundamentally new kinds of interface, for inclusion within the computer itself (i.e. the second type of HCI distinguished above). But their work took many years to be put into effect, because their ideas when first mooted were far ahead of their time.

Sutherland (1938–) was a wunderkind. While at high school in the early 1950s, he not only programmed simple computer calculators but also—with his elder brother Bert, and under the direction of Edmund Berkeley (1909–88)—built an electro-mechanical "squirrel" called Squee (D. G. Bobrow, personal communication).

Berkeley was an apt teacher for two machine-minded youngsters. He'd already designed a very early computer for personal use, called "Simon" (Berkeley 1949, 1956). It had 129 relays and a five-hole paper-tape feed. Given numbers of up to 255 binary digits, with coded instructions for up to nine different arithmetical operations, it displayed its answers in lights. The prototype was completed in 1950, and by the end of the decade he'd sold over 400 plans or kits for the machine (J. M. Norman 2004: 73).

In addition, he'd written the first popular book on electronic computers: *Giant Brains or Machines that Think* (Berkeley 1949). After describing various existing (and planned) machines, such as the Mark 1 and the ENIAC, he devoted the final chapters to speculations on how computers would affect society. In a sense, that was what it was all about. Indeed, the book's original title, cited in the contract he signed in 1946, had been *Machines to Help Us Think* (J. M. Norman 2004: 73). In other words, he was closer to Bush, Licklider, and Engelbart than to Minsky, McCarthy, and Newell-and-Simon. Possibly, the young Sutherland may have absorbed some of Berkeley's interest in the practical uses of computers.

For the time being, however, he and his brother were concentrating on their robot squirrel. Squee, which made the front cover of Berkeley's *Radio Electronics* magazine and was also featured in *Life* (19 March 1956), looked like a slightly more complex Grey Walter tortoise. It had two phototubes ("eyes") at the top of the steering post, two switches for sensing contact, and a scoop ("hands") at the front; guided by light, it searched for a "nut" (a torchlit tennis ball), which it picked up and took back to its "nest" (Berkeley 1956). Having exhibited this delight across the USA (New York, Pittsburgh, and Minneapolis) by 1956, Sutherland published a description soon afterwards (Sutherland *et al*. 1958).

Like the tortoises (4.viii.a–b), Squee was meant to draw a general moral—one which applied to human intelligence too. And many readers were persuaded. When he was still only 20 (in March 1959), Sutherland won a prize from the American Institute of Electrical Engineers for a paper on 'Parallels—Men and Machines'.

In later life, he deepened this early interest in mechanical creatures, writing about walking robots for the *Scientific American* (Raibert and Sutherland 1983) and patenting a robot arm in 1990. And his IT skills were recognized when, still only 26 years old (in 1964), he followed Licklider as head of ARPA's office for information processing technology.

But his fame arose from two achievements of his early to mid-twenties that had nothing to do with squirrels, artificial or otherwise, and which were decades before their time in terms of technology. For that reason, they had to go into hibernation until it was technically possible to put them into effect. These were his Ph.D. thesis on Sketchpad (1963), which initiated interactive computer graphics; and his 1965 vision of virtual reality, or VR.

Given that Sketchpad was described as early as 1962, why didn't I include Sutherland as a "harbinger" in Chapter 10.i? Well, that research—like Kay's, which was based on it (from 1966 on)—couldn't come to fruition until the late 1980s. Indeed, Kay said in 1989 that user-interface design was *still* hardly accepted as "a real subject" (A. C. Kay 1989: 123). Similarly, Sutherland's mid-1960s ideas on VR couldn't be fulfilled until the 1990s. Because both men had to wait twenty years or more for computer technology to catch up with them, their legacy to AI has a very modern, post-Kraken, feel.

Their CVs feel pretty modern too. Kay's includes appointments at Xerox, Apple, Disney, and Hewlett-Packard. As for Sutherland, his first job was as ARPA's Director of Information Processing in 1962–4 (supporting Project MAC, for instance). He's now Vice-President at Sun Microsystems. And, having co-founded a company doing VR simulations for aerospace and defence applications, he's now a billionaire.

Beginning life as a Ph.D. thesis, Sketchpad eventually became "one of the most influential computer programs ever written by an individual" (according to the citation for Sutherland's 1988 Turing Award). The thesis itself was published only as an MIT Technical Report, accessible to relatively few people until it was made available on the Web (Blackwell and Rodden 2003). But it was summarized in a conference publication (I. E. Sutherland 1963).

As it turned out, that was enough. For the details were far less important than the overall picture. Considered as a program, Sketchpad was highly limited. (Hardly surprising, given its date.) It couldn't even be shared, for it could be run only on the TX-2 at MIT's Lincoln Laboratory. But considered as a vision, it was dynamite.

For one thing, it was the first program which enabled the user to change the content of a computer's memory interactively, without reprogramming. (The SAGE operators in the 1950s had used lightpens to alter graphic displays by touching the screen; but they were changing *which stored memories were shown*, not *the memories that were stored*; see 11.i.) For another, it provided a suite of facilities for drawing and transforming pictures systematically. It wasn't the first graphics program: Spacewar, for example, had preceded it (10.i.i). But it was the first where the user could manipulate images *as such*.

The human could produce multiple instances of various image schemas ("master drawings"), and alter them at will in several ways. For example, the user could change the size of an existing drawing. Or if one had drawn a line on the screen that wasn't quite vertical, one could tell Sketchpad to make it vertical, maintaining the continuity of the whole object as it did so. Similarly, a carelessly drawn corner could be automatically transformed into a 'proper' corner. Physics could play a role too. So a truss in an engineering drawing, described as having a particular mass, could be made to bend as if it were governed by Newton's laws—that is, 'realistically'. In addition, Sketchpad allowed individual instances to be moved around the screen (with a lightpen) as whole objects, rather than remapping a collection of individual pixels.

Changing the master drawing would immediately change all its daughter images in the relevant way: so adding a third ear to a rabbit schema would magically add an extra ear to all the "copied" rabbits. What's more, images could be superimposed: "You could make a picture of a rabbit and a picture of a rocket, and then put little rabbits all over a large rocket. Or, little rockets all over a large rabbit" (Nelson 1977).

The details would disappear from view if the picture were made small enough, but would reappear when it was enlarged again. And the superpositions were recursive: you could have rabbits on rockets, on rabbits, on rockets. . . . Describing Sketchpad in a chapter with the title 'The Most Important Computer Program Ever Written', Ted Nelson (1977) admitted that "the rabbits and rockets are a frivolous example". But he pointed out many "obvious" applications: "blueprints, or electronic diagrams, or all the other areas where large and precise drafting is needed. . . . [A] new way of working and seeing was possible."

As for the still-untapped potential, Sutherland suggested (for instance) that facial characteristics in a drawing could in future be altered by the user. What's more,

> if the almost identical but slightly different frames that are required for making a motion picture cartoon could be produced semi-automatically, the entire Sketchpad system could justify itself economically in another way. (Blackwell and Rodden 2003: 4–5)

Yes indeed!

d. The human face of the interface

Sutherland had intended his program to be used by artists and engineering draughtsmen, as a tool for CAD. In other words, 'interface' HCI had begun. But it would be Kay who saw how the ideas in Sketchpad could be exploited in designing interfaces for an even wider range of users.

Kay first encountered Sketchpad in 1966, as a very new graduate student at the University of Utah. This was one of the first few locations to be put on the ARPAnet, and it was to be Sutherland's home for many years. As Kay remembered it:

> At Utah before you got a desk you got a stack of manuscripts and you had to read the stack. It described Sketchpad. Basically, you had to understand that before you were a real person at Utah.
>
> They also had a tradition there that the latest graduate student got the latest dirty task to do. Mine happened to be on my desk—a pile of tapes and a note which said "This is the Algol for the UNIVAC 108; if it doesn't work, make it work." It turned out to be the first Simula. (interview in Shasha and Lazere 1995: 42)

SIMULA, which had been developed in Norway just one year earlier, is now seen as a primitive object-oriented programming language. The first person to see it that way was Kay—whose "dirty task" would change not only his life, but (eventually) millions of other lives too. Like Sketchpad, SIMULA distinguished general classes from specific instances, which shared core properties with the master. Kay, who'd studied biology (and maths) as an undergraduate, felt that he was encountering something vaguely familiar in both cases: "The big flash was to see this as biological cells" (interview in Shasha and Lazere 1995: 43).

If the images/objects in Sketchpad and SIMULA were truly analogous to living cells, they'd have autonomy (as whole-object individuals), property inheritance, communication (passing and receiving messages), and the capacity to congregate in many-levelled hierarchies. Kay, then, sensed the possibility of a powerful object-oriented programming language (his term). Six years later, he'd designed one: SMALLTALK (Chapter 10.v.d). (And by 1991, an article about object-oriented programming, optimistically called 'Software Made Simple', was emblazoned on the cover of *Business Week* magazine—Resnick 1994: 42.)

But that was for the future. Meanwhile, on leaving Utah in the late 1960s, Kay—enthused by Licklider's concern for man–machine symbiosis (a near-synonym for HCI)—helped to design the ARPAnet. In 1970 he moved to Stanford's AI Lab, and in 1972 to Xerox PARC, where he headed the interdisciplinary Learning Research Group.

During the late 1960s, he worked on the FLEX machine, intended for use by doctors, lawyers, and the like (Shasha and Lazere 1995: 43–4). This experience—at a time when most people had hardly heard of computers, never mind seen one—led Kay to speculate about future interfaces which Everyman would find easy to use.

To address the H in HCI, Kay drew on Marshall McLuhan's *Understanding Media* (1964) and also on computationally informed theoretical psychology. As for the first, he recalled later:

> Though much of what McLuhan wrote was obscure and arguable, the sum total to me was a shock that reverberates even now. The computer is a medium! I had always thought of it as a tool, perhaps a vehicle—a much weaker conception. What McLuhan was saying [when he talked about the printing press and television having changed the thought patterns of those who learned to read, or view TV] is that if the personal computer is a truly new medium then *the very use of it would actually change the thought patterns* of an entire civilization. (A. C. Kay 1989: 124; italics added)

(Bush had said much the same about the memex: remember that medieval historian, researching the Crusades? 10.i.h.) Kay was struck, too, by McLuhan's idea that "anyone who wishes to receive a message embedded in a medium *must first have internalized the medium* so it can be 'subtracted' out to leave the message behind" (p. 124; italics added).

Clearly, the closer the medium was to the already existing mind, the more chance that it would be "internalized" effectively. Hence Kay's interest in the psychology of thought, creativity, and learning. This interest was deepened by his conversations with Papert from the late 1960s on, including his visit to "one of the earliest LOGO tests within a school" (A. C. Kay 1989: 125). Indeed, he was "greatly encouraged" by Papert's work (A. C. Kay and Goldberg 1977: 171), and "*possessed* by the analogy between print literacy and LOGO" (A. C. Kay 1989: 125; italics added).

On reading "many" books in theoretical psychology, he soon turned (at Papert's suggestion) to Jean Piaget. Piaget had pointed out that the child's ways of thinking, as compared with the adult's, aren't just 'less of the same' but *different*. Clearly, this had implications for how one should best design interfaces for use by children. But Piaget's writing was too like McLuhan's—intriguing, but obscure—to be really useful. What was useful was Bruner's new interpretation of Piaget's results, especially as described in his book *Towards a Theory of Instruction* (Bruner 1966*b*).

In particular, Kay was inspired by Bruner's notion of cognitive technologies (Chapter 6.ii.c). These were different systems of mental representation: enactive, iconic, and verbal–symbolic. Kay saw them as "multiple separate mentalities with very different characteristics" (1989: 126), and aimed to exploit all three. In his words:

> Now, if we agree with the evidence that the human cognitive facilities are made up of a *doing* mentality, an *image* mentality, and a *symbolic* mentality, then *any user interface we construct should at least cater to the mechanisms that seem to be there*. But how? One approach is to realize that no single mentality offers a complete answer to the entire range of thinking and problem solving. *User interface designs should integrate them* at least as well as Bruner did in his spiral curriculum ideas. (1989: 127; last two italics added)

Accordingly, he invented—initially, for FLEX—HCI facilities including a computer mouse (enactive); screen icons (visual); and overlapping windows of text (verbal–symbolic).

As is usual (see 1.iii.g), his highly original ideas didn't come out of thin air. Early versions of Kay's interface items had been available since the 1960s (10.i.h). But Kay's were more powerful, and easier to use—and, as we've seen, more directly connected with current psychological theory. They were also better grounded empirically:

> [It] took our group [at Xerox PARC] about five years and experiments with hundreds of users to come up with the first practical design that was in accord with Bruner's model and really worked. (A. C. Kay 1989: 128–9)

For example, his windows *overlapped* because there was psychological evidence—from a best-selling book on tennis coaching, believe it or not (Gallwey 1974, ch. 7)—that people think/learn better if they're concentrating on only one thing at a time. (Later, and probably thanks to Kay, the tennis author—Tim Gallwey—would become a consultant at the Media Lab—Brand 1988: 100.) So having one window almost completely obscuring another was a Good Thing, provided that the other/s could be made the centre of attention at the click of a mouse. (Gallwey's book had included advice on how to learn to focus on one aspect of performance at a time; where Kay generalized this to computer interfaces, Gallwey later published best-sellers on other sports, and on "work" . . . etc.)

These technical advances were made possible by means of object-oriented programming. Today, this is hugely important across GOFAI as a whole. But it was developed by Kay to build flexible interfaces.

The SMALLTALK programming language (completed in 1972) was also driven by psychological criteria. It matched people's natural tendency to think in terms of concepts, and to 'place' a concept in a new context without worrying about just how it will be able to fit in. This made it easier to design computer interfaces where the user simply has to press a button or click on an icon, instead of knowing—and telling the machine—just what computations have to be performed. It also made it easier to include graphics in the interface, which—following Bruner and Papert—Kay saw as especially helpful to children.

He didn't stop there. He predicted personal computers much more convenient than the 350 lb FLEX machine: "This note speculates about the emergence of personal, *portable* information manipulators . . . " (A. C. Kay 1972; italics added). He went on to design the Dynabook, a notebook-sized gadget "as small and portable as possible" combining pictures, animation, music, speech, and text (words and musical notation). The Dynabook was intended as "a dynamic medium for creative thought" of many different kinds, in which—for instance—maths could become "a living language in which children could cause exciting things to happen" (A. C. Kay and Goldberg 1977: 170, 177; see also Kay 1977, 1989; Ryan 1991).

Although this multimedia system was never fully built, its core ideas were implemented in the Xerox Alto computer by 1974 and it was the ancestor of today's laptops and palmtops. And as if all that weren't enough, he also instigated desktop publishing (thanks to multi-font word-processing), and helped develop the Ethernet, laser printing, and client–server architectures.

I say "he", but of course others at Xerox PARC were involved too. One of these was Kay's assistant Adele Goldberg. Another was David Canfield Smith, who coined the term "icons", and whose 1975 Stanford thesis—based on SMALLTALK—eventually fed into the Xerox Star machine (D. C. Smith 1977). This programming environment enabled the user to select, and hop about among, different visual presentations of the interface by choosing from a menu. In Smith's mind, his Pygmalion program was not only assisting creative thought, but—in a sense—modelling it too. The same had been true, of course, of Bush's "associative" memex.

Kay's success in HCI eventually helped foster the success of Apple Computers—and, indirectly, of Microsoft too. Xerox PARC had refused to manufacture machines or to market software based on his work, rejecting his visionary prediction of huge technological applications. They wanted to develop the paperless office, not home computers for Everyman. As for the multitude of visitors to PARC, most realized that Kay's team were making "spectacular advances", but "very few understood either in depth or in scope the applications of what they were seeing" (L. Tesler, interviewed in Brand 1988: 173). Steve Jobs, by contrast, saw the point:

> When I went to PARC [in November 1979], I thought it would be an interesting afternoon, but I had no concept of what I'd see. Larry Tesler was my guide for an hour, to show me around. My mind was just totally blown. The minute I saw an Alto (PARC's personal computer) and the mouse and the multiple fonts, I *knew* that we had to have it. I came back to Apple a raving maniac about this stuff and I grabbed a bunch of people . . . and dragged them over there. (Steve Jobs, interviewed in Brand 1988: 172–3)

After this "mind-blowing" visit to Kay's lab, Jobs tried to buy the SMALLTALK software, but Xerox refused to sell it—despite being a minor shareholder in Apple (Shasha and Lazere 1995: 49). Apple used ideas inspired by Kay in developing the AppleMac (and appointed Kay as an Apple Fellow in 1984). The rest, as they say, is history. (A chequered history, to be sure: because of IBM PC marketing patterns, Microsoft's "Windows" software is much more widespread than the AppleMac version, on which it's based.)

Over the years, Kay has become increasingly interested in children's education—partly as a result of a 1968 visit to Papert's lab (Shasha and Lazere 1995: 45, 47 ff.).

Following Papert's example, he regularly tested the accessibility of his HCI ideas by trying them out on children, both at a Palo Alto junior school and—via Papert—at Boston's Hennigan School. A second reason for working with children was that, being used to having coloured paints and musical instruments, they "really needed" computing facilities that were pushing the state of the art in the 1970s (A. C. Kay and Goldberg 1977: 171).

Having worked for Apple in the mid-1980s, and for Disney Imagineering in the 1990s, Kay is now a Hewlett-Packard Fellow. He was—and still is—an important presence/visitor for the Media Lab, where he's advised on projects ranging from Hollywood animation to A-Life (Brand 1988, ch. 6). And he's involved in various educational ventures, such as the non-profit Viewpoints Research Institute (founded in 2001). But despite his journey through the highest hills of high tech, he retains his interest in psychology. Indeed, he still calls Bruner on the phone about once a fortnight (Shore 2004: 87).

Bruner's ideas on cognitive technology are enthusing a new generation of HCI enthusiasts, too. For instance, Mike Scaife (who died suddenly in 2003) and Yvonne Rogers at Sussex have instituted a research programme called Interact. Explicitly inspired by Bruner (Y. A. Rogers *et al.* 2002*b*), this is centred on children's (often collaborative) use of external representations of various kinds: words, diagrams, pictures, toys . . . and so on.

These include 'tangibles', which are physical objects (bricks, balls, puppets, clothing . . .) electronically augmented so as to trigger events in other electronic artefacts. For instance, the children's play activities with a tangible interface can cause changes in a screen interface. The VDU may show a virtual animal, which engages the children's attention: as they do this or that with their electronically enhanced toys, the screen animal shows this or that aspect of its form or behaviour (Price *et al.* 2003). And those changes are recorded by the computer, to be used later by the children for discussion and self-reflection of many kinds.

The Interact programme is essentially interdisciplinary, for it has two closely related aims. One is to devise new technologies—in this case, for use by young children. The team's new designs go way beyond Papert's turtles, for they involve recent advances such as "pervasive" computing (i.e. the use of tangibles) and multimedia VR. (For examples, see Scaife and Rogers 2001; Rogers and Muller forthcoming; Marshall *et al.* 2004.) The second aim is to augment, as well as to exploit, our understanding of how children play, learn, collaborate, and represent (e.g. Scaife and Rogers 1996; Scaife 2005; Rogers *et al.* 2002*a*).

Of the many advances in these areas of psychology since Bruner's seminal 1960s work, some were initiated in the 1970s by Scaife himself (see 6.ii.c). Moreover, Rogers—whose expertise is strongly technological—was initially trained as a psychologist, as were several others in their group. In short, Interact is just one example illustrating the fact that in HCI research, psychology and GOFAI (and sometimes connectionism too) walk hand in hand.

Increasingly, they walk unnoticed—indeed, invisibly. In a futuristic article in the *Scientific American*, Xerox PARC's chief scientist Marc Weiser (1952–99) predicted the advent of what he called "ubiquitous" computing, or ubicom (Weiser 1991). This, he said, would be the "third wave in computing", following on time-shared mainframes and the personal computers foreseen long ago by Bush. What even Bush hadn't foreseen was the use of miniaturization and telecommunications (e.g. wireless technology, offering local links and/or entry onto the Internet) to embed tiny adaptive computers into almost every niche in our environment: offices, houses, cookers, cars, toys . . . even jewellery and clothes. The human–computer interface, traditionally understood, has largely disappeared: as Weiser (1994) put it, "the world is not a desktop".

There's now an international conference series on ubiquitous computing (Ubicomp), dedicated to putting his vision into effect. IBM preferred their own terminology: pervasive computing, or "percom". But whether driven by Xerox or by IBM, or by the many start-up companies at the turn of the millennium, the third wave of computing is gaining strength. The closer it gets to human lives, the more important the psychological issues become. And as we've seen, much of it was seeded by psychology in the first place.

13.vi. Virtual Reality

Virtual reality is the epitome of current high tech in IT. But it's not of interest only to 'techies'. It raises some deep philosophical issues (see 16.vii.d and viii.c), many intriguing psychological questions (e.g. 14.viii.b and 15.xi.a), and various social-psychological worries too (subsections d–e, below). Nor is it really new. The idea, and even the pioneering technology, dates back to the late 1950s.

One might argue that the general idea dates back even further than that, if one counts E. M. Forster's dark sci-fi story 'The Machine Stops' (1909). The characters are a mother, Vashti, and her son Kuno. Vashti, like most of the civilized human race, lives underground in a wholly machine-tended capsule. She communicates with her son (and "several thousand" others: "in certain directions human intercourse had advanced enormously") only by a videophone, never face to face. The videophone—"rightly", she thinks—doesn't "transmit nuances of expression": it conveys instead "a general idea of people . . . good enough for all practical purposes". Kuno prefers the despised real world, and eventually stops the machine—to his mother's desperation, who prays for it to restart some day.

Kuno's (Forster's) horror at the human isolation and machine dependence suffered/enjoyed by Vashti parallels some of today's worries about computer companions and avatars (see below). But even Forster's imagination didn't stretch to wholly machine-generated images, still less to the ambition and detail of VR as we know it a century later. (Nevertheless, a quotation from his story opens a recent paper on the philosophical implications of VR: H. L. Dreyfus 2000.)

The term "virtual reality" was coined in 1989 by Jaron Lanier, to denote "three-dimensional realities implemented with stereo viewing goggles and reality gloves" (M. W. Krueger 1991, p. xiii). Lanier, a computer scientist and musician, had already founded the first commercial VR company (in the early 1980s). So he wasn't announcing a new vision. On the contrary, his new label (like the term "artificial life": 15.x.a) was intended to pull together pre-existing work of diverse kinds.

VR has since been redefined in several ways (Steuer 1992). These vary on both technological and philosophical dimensions. We don't need to consider those variations here, but it's worth noting that "VR" means different things to different writers.

It also means different things to different writers in the sense that it's ethically approved by some and roundly condemned by others. By the mid-1980s, various commentators were raising worries about VR.

Some were sociopolitical. Donna Haraway (1944–) was especially influential, here (Haraway 1986/1991). (For recent examples, concerning multi-user VR and the BodyNet, see Rheingold 2002.) I'll ignore those, however. There will be plenty of psychological worries (and some philosophical implications) to discuss. That's only to be expected, given that VR is such an intimate form of cognitive engineering.

a. Intimations of VR

Among the pre-existing work gathered under Lanier's label was Sutherland's. Indeed, Sutherland's second claim to fame is as the prophet of VR.

Besides being its prophet, he was an early pioneer. His revolutionary VR headset, announced in the 1960s and completed by 1970, superimposed a transparent TV image over the real-world perception (I. E. Sutherland 1968). (Previous efforts had *substituted* a TV display for the real image: Comeau and Bryan 1961.) It provided a full-colour, three-dimensional, visual display that filled the user's entire field of view. (Moreover, Sketchpad-derived techniques enabled the wearer to manipulate the virtual images with various input devices.) The experience of 'total visual immersion' was new—and, even though it lacked stereovision, remarkably compelling.

Filmgoers, of course, had long got drawn into Bambi's hand-drawn virtual world. But while watching him they could see the walls, and the seats and people and popcorn too. The conventional movie screen took up a mere 5 per cent of the spectator's field of vision. Even Cinerama and Cinemascope, still novelties at the time, didn't get anywhere near 100 per cent visual immersion (Heilig 1955: 283–4).

One of the first cinematographers to be enthused by that "100 per cent" idea was Hollywood's Morton Heilig (1955). His description of 'The Cinema of the Future' saw the viewers near-inhabiting the film, being wholly immersed in a virtual world even more all-encompassing than Cinerama. Besides all-round visual experience and stereo sound, it would offer taste, touch, and smell—each sense to "dominate the scene in roughly the same proportion we found them to have in man: sight, 70%; sound, 20%; smell, 5%; touch, 4%; and taste, 1%" (Heilig 1955: 292). (Unappreciated in Hollywood, Heilig moved to Mexico, where he invented two early virtual reality devices that later inspired more powerful versions from others—Packer and Jordan 2001: 220.)

However it was Sutherland's prophecies, not his technical achievements, which caused the stir in the 1960s. For what he had in mind was a very high degree of reality attribution. Visual immersion was just part of it.

Even before announcing the VR goggles, he'd already urged his NewFAI colleagues to produce computer-generated pictures (and sounds, and forces) that would be experienced by the viewer as 'real'—and which might represent unreal "Wonderland" worlds too. So in his speech to a large IFIPS meeting in 1965, he'd said:

> We live in a physical world whose properties we have come to know well through long familiarity . . . A display connected to a digital computer gives us a chance to gain familiarity with concepts not realizable in the physical world [such as non-realistic geometries, or negative mass]. *It is a looking-glass into a mathematical wonderland.* (I. E. Sutherland 1965: 506; italics added)

To be convincingly realistic, he continued, the display "should serve as many senses as possible". Excellent sound generation was already achievable, although speech generation wasn't (nor would it be, until much later: see 9.xi.g). And computer-generated "kinesthetic displays" were well within reach, by extending the controls of flight simulators—which gave trainee pilots of the 1960s "the feel of a real airplane".

Besides providing joysticks with force-feedback capability (already available), future VR computers—he said—would "easily sense the positions of almost any of our body muscles". Because they'd be able to monitor not only hands and arms but eye movements too, we might use "a language of glances" to control a computer: "For instance, imagine a triangle [in the VR display] so built that whichever corner of it you look at becomes rounded."

The futuristic speech ended with a flourish:

> *The ultimate display* would, of course, be a room within which the computer could control the existence of matter. A chair displayed in such a room would be good enough to sit in. Handcuffs displayed in such a room would be confining, and a bullet displayed in such a room would be fatal. *With appropriate programming such a display could literally be the Wonderland into which Alice walked.* (p. 508; italics added)

One of the people in the audience for Sutherland's lecture was Frederick Brooks (1931–), who'd headed the team designing IBM's System/360 computers in the late 1950s. For him, it was an epiphany:

> [Sutherland] said, "Think of the screen as a window into a virtual world. The task of computer graphics research is to make the picture in the window look real, sound real, interact real, feel real." And I said, "That's where it's at." I've been working on that challenge ever since. (interview in Shasha and Lazere 1995: 170)

Brooks wasn't the only one. And once the technology had caught up with Sutherland's vision, VR found many industrial applications.

Vibrating seats, fan-generated winds, and chemical 'smell banks' arrived relatively early. By the late 1980s, thanks largely to work done at NASA-Ames and the Media Lab, and funded by Hollywood as well as by government grants, VR had added stereovision, headphones, microphones, gesture interaction, and fibre-optic data gloves that could measure the ever-changing bend of each finger joint and the distance between the fingers (Fisher 1990; Brand 1988). (An early pressure-sensitive glove, the "Teletact", was invented by Robert Stone and Jim Hennequin, but wasn't taken up by other VR workers: R. Stone, personal communication.)

Brooks ended his reminiscence by saying "Any year we'll get there." For already "by 1994", on his view, "one could honestly say that VR 'almost works' " (Brooks 1999: 16). This means that "any year" is roughly *now*. Presumably, he's disappointed. For despite all the hype, a near-fully realistic experience is only slightly nearer.

Complete sensory immersion still isn't possible. (Even if it were, it's not clear that it could ever be phenomenologically equivalent to having a bodily presence in a real situation—H. L. Dreyfus 2001: 59–72; Dreyfus 2003.) In research laboratories today, and even in artists' studios, VR goggles and gloves—and body suits—can be fed with signals from afar, or from simulated worlds instead of the real one. And entire virtual environments can be built, obviating the need for goggles or gloves. But the VR dreamt of by Sutherland, where the simulated world is wholly imaginary and yet utterly convincing, is still a very long way off.

Indeed, some multimedia experts are already claiming that VR, as originally understood, has not only "failed" to live up to its promises but has no chance of doing so in the future. More success, in Stone's (2005) view, has been achieved within what's called "serious gaming"—whether intended purely for entertainment or for training purposes. This is partly a matter of accessibility and affordability: unlike VR installations confined to specialist high-tech laboratories, these systems are already available to, and easily usable by, Everyman. But it's also a matter of verisimilitude in the characters and avatars depicted. As of January 2006, perhaps the best example of 'realistic' human movements and facial expressions in such a game is the "G-man" character in Half Life 2, which has "40 facial muscles and a skin texture that beggars belief" (R. J. Stone, personal communication). Indeed, this technology has inspired an eerily plausible simulation of the court room scene in the Jack Nicholson film *A Few Good Men*—called, as you might expect, *A Few Good G-Men* (see <http://www.machinima.com/films.php?id=1154>).

Partly because of the huge difficulties—and expense—involved (how many dollars per simulated muscle?), by the late 1990s much of the research emphasis had switched from the interface itself to the human's *interaction* with it. The 'reality' was largely projected onto the interface by that interaction (effort after meaning, again).

Accordingly, some current definitions of VR don't specify particular technologies (e.g. gloves, graphics, or headsets) but focus rather on the interactive experience. For instance, when the UK's Department of Trade and Industry launched their VR Awareness Initiative in 1996, they adopted Stone's definition: "Virtual Reality refers to a suite of technologies supporting intuitive, real-time interaction with multi-dimensional databases" (R. J. Stone, personal communication).

The omission of any reference to immersion, as envisaged by Sutherland and Brooks, was deliberate. For excessive hype had led to widespread scepticism. (The same old AI story!: see 11.ii–iii and 12.ii.f.)

b. VR as a practical aid

To illustrate the (limited) degree to which VR has already "got there", let's consider four practical applications: one for information access, one for doing surgery, one for surgical training, and one for training in hands-on mechanics. (Then, in subsection c, we'll look briefly at VR in contemporary art and entertainment.)

The first is an example of "wearable computing", intended for (literally) everyday use. This is a "Personal Information Architecture" gadget (for belt, briefcase, or handbag) called BodyNet (because the components communicate by low-powered radio waves extending no more than six feet from the body). The aim is to combine a person's radio, TV, video/CD-player, laptop, diary, and mobile phone. Like a waterproof wristwatch, this is "an intimate interface: always there, always on" (Shivers 1993: 7).

BodyNet is based on "magic goggles": clear-glass goggles which, besides enabling the wearer to see the real world, present small inset colour displays to each eye. These may carry information about the current state of the stock market, or they may present a TV newscast or chat show, or the wholly imaginary VR world of a computer game. Miniature earphones and microphone enable the wearer to hear and deliver speech within the virtual world. The focus of the (stereoscopic) goggle display was initially identical, irrespective of where the person was looking. But by the mid-1990s the system could monitor the wearer's eye movements to discover what they were looking at. If this is something in the virtual world, the next image sequence and/or audio input could be generated accordingly.

Wearing/carrying BodyNet, one could be walking through a simulated VR fantasy while also walking down the High Street. For the virtual images accompany the person's real perception rather than replacing it. Partial immersion, such as this, raises interesting HCI questions about how people manage to focus on physical and virtual worlds simultaneously, and how they can be induced by the VR engineer to attend to what's 'relevant' (Ohta and Tamura 1999). In other words, psychology and computing in a close embrace, again.

The second practical application concerns 'distance surgery', still in the experimental stage. The thinking and planning is done by the human surgeon. But he/she is immersed in a virtual world. This represents the real world at a distance, where the real patient is really located. In other words, this is a form of *telepresence*, as opposed to a wholly fictitious VR construction.

The surgeon receives camera input and force-feedback (haptic) data from the real-world site, thanks to VR goggles and gloves, and sends body-movement signals to the robot 'surgeon' at the other end of the line. If miniature robots are involved, the surgeon's movements are scaled down during transmission. In short, distance surgery combines sensory (visual, touch, and kinaesthetic) VR with sophisticated robotics.

One leading centre for this research is Oussama Khatib's laboratory at Stanford. Khatib, currently co-editor of the *Robotics Review*, directs a wide range of research in robotics. This includes "human-centred" robotics, in which the robot interacts and/or cooperates with a human being in real time (Khatib *et al.* 1999, 2002).

Human-centred robotics in general raises a number of problems which traditional robotics didn't have to face. Some concern the 'psychological' interlock between the person and the (largely) autonomous artefact: just how can their plans/perceptions be integrated? A mismatch between surgeon and robot could have horrendous consequences. Others concern their physical interlock: if they're sharing the same space, the robot must (usually) have no sharp edges, no sudden movements, and no excessive forces (Zinn *et al.* 2002).

Excessive force can be avoided, whether the robot is human-centred or not, by deliberately limiting its mechanical strength and/or by building in haptic sensors. These provide force feedback to control the power of its autonomously generated movements. In other words, the robot 'knows' by touch not only *that there's something there* but also *how resistant it is*, and therefore *how strongly the robot should push at it*. (Compare the feel of flesh with that of bone; and imagine pressing on a cushion, a balloon, or a bottle of wine.) In addition, 'kinaesthetic' feedback tells the robot how much force it needs to exert to lift the object, and/or carry it.

Yet other new problems concern the VR aspects—if any. In distance surgery, the surgeon doesn't share the robot's physical space but the patient does. There's an exception to the "no sharp edges" rule, since the robot manipulates a scalpel. It's especially important, then, to ensure that haptic information is conveyed to the surgeon effectively. If the robot responds autonomously to its own sensors, it must do so immediately and delicately. (In many distance-robotics applications the haptics don't need to be quite so delicate.)

One might argue that, in a very real—i.e. phenomenological—sense, the surgeon *does* share the robot's space. For as the surgeon becomes more familiar with the robotic tool, the tool (much like a blind man's cane) comes to be experienced as part of his/her own body. To put it another way, the boundary of his/her body seems to extend to the region of the robot. This is a special case of the sort of action-led adaptability in body image that was first studied by George Stratton over 100 years ago (see Chapter 14.viii.b). (Many other Strattonesque reorientations based on VR and/or telepresence are described in A. J. Clark 2003*a*.)

In principle, the same equipment can be used to convey 'sensory' information from a *virtual* world. The better the VR design, the greater the person's sense of immersion in a "genuine" reality will be. (I can vouch for that, having experienced Khatib's experimental visual/haptic system when visiting his lab in autumn 2003.) In one sense, this isn't new: as Sutherland pointed out in his 1965 talk, flight simulators have existed for years. But they've got steadily more convincing.

Distance surgery isn't the only use of VR by surgeons: our third example concerns the use of 3D simulations of bodily organs for surgical *training*. For instance, consider a VR brain being developed at the University of Nottingham under the direction of the neurosurgeon Michael Vloeberghs (Wang *et al.* forthcoming).

Not only does this VR brain have the appropriate 3D anatomy, but it's realistically squashed/deformed (in real time) when the trainee's virtualized surgical tool touches it. As well as *looking* different, it *feels* different too. Given that brains are very soft (compared with hearts, for example), the visual/haptic information at issue here is highly complex. For instance, the "touching" may be prodding, pinching, or cutting—each of which has its own distinctive material and perceptual effects.

The mathematical techniques being used by the Nottingham team have already been applied to other parts of the body too. And organs that aren't surrounded by a bony skull may be easier to simulate. For in listing their VR tasks in order of difficulty, these authors identify the deformations caused by "the contact of the tissue with itself and with its surroundings" as the most challenging of all (Wang *et al.* forthcoming, sect. 3).

The last illustration of practical VR is a recently developed system for training mechanical skills. A number of "embodied conversational agents" are already being developed for HCI-based training in a wide range of domains (Andre 1999; Cassell *et al.* 2000). That word "embodied" is misleading for these agents aren't robots. Rather, they're VR creatures existing in cyberspace—and interacting there with real people.

One of a variety of AI training systems developed by Jeff Rickel and Lewis Johnson, at the University of Southern California's School of Engineering, is called Steve (Rickel and Johnson 1998, 1999, 2000). The acronym stands for SOAR Training Expert for Virtual Environments. As that "S" indicates, this computer tutor is grounded in GOFAI work. It builds not only on SOAR (which itself is highly complex: Chapter 7.iv.b), but also on GOFAI agents, teaching machines, computer graphics, and NLP (e.g. B. G. Deutsch 1974; Grosz 1977; Grosz and Sidner 1979; Rickel *et al.* 2002). And, true to the interdisciplinary nature of HCI in general, it also uses psychological research—such as Paul Ekman's work on the facial expression of emotion (cf. Chapter 7.i.d).

Steve's designers describe him (*sic*) as "a new breed of computer tutor", since he's "a human-like agent that can interact with students in a virtual world to help them learn". (One of the first of the old breed of computer tutor was the programmed mechanic CBC, mentioned in Chapter 10.iii.c.)

The human pupil, in order to share the VR world with Steve, wears VR-display goggles and VR gloves, and carries a 3D mouse. And Steve—visually presented to the learner as a 3D humanoid (disturbingly, lacking hips and legs)—is able to watch and monitor the pupil's actions, and to give spoken advice, accompanied by gestures, when difficulties arise. (The "-oid" in "humanoid" is only too apt, for Steve also lacks realistic skin texture and facial expressions: he's hugely inferior in these respects to the Half Life 2 example mentioned above.)

Despite the lack of nether regions, Steve is a 'believable' agent. This is largely because he/it integrates perception, cognition, and motor control:

> The perceptual module monitors the state of the virtual world, maintains a coherent representation of it, and provides this information to the cognition and motor control modules. The cognition module interprets its perceptual input, chooses appropriate goals, constructs and executes plans to achieve those goals, and sends out motor commands. The motor control module implements these motor commands, controlling Steve's voice, locomotion, gaze, and gestures, and allowing Steve to manipulate objects in the virtual world. (Rickel and Johnson 1999: 343)

Specifically, what Steve teaches his students is how to perform *physical* tasks, where bodily demonstrations, as well as verbal instructions, may be helpful. Examples mentioned by his designers include the operation and maintenance of complex—and often dangerous—equipment in ships, factories, power stations, and the like. (In principle, other physical tasks, from cooking to surgery, might be demonstrated by Steve too. But this would require detailed simulation of the relevant physical objects in the VR world.)

Steve's tasks can involve more than spanners and knobs. They have a social dimension as well:

> In addition to training students on individual tasks, he can also help them learn to perform multi-person team tasks: he can serve as a tutor for a student learning a particular role in a team, and *he can play the role of a teammate when a human teammate is unavailable.* (Rickel and Johnson 1998: 30; italics added)

(In subsection d, we'll consider some worrying implications of the use of the words *teammate* and *he* in that quotation.)

c. VR in art and play

Sutherland's talk of Wonderland had carried associations of play, and of art. And indeed, artists were inspired by AI/VR to develop new artistic genres (Brand 1988; Stiles and Selz 1996; Packer and Jordan 2001).

Some of these could be seen as versions of performance art, in that the prime interest wasn't in the sensory qualities of the product (images, music . . .) but in the mental processes and physical skills that produced it. Arguably, *all* art, even including the *Mona Lisa*, is performance art in that sense (D. Davies 2004, esp. chs. 7–10). If so, then the new VR art wasn't as marginal as a conventional aesthetics would imply. In any event, the art world of the late twentieth century was greatly influenced by experiments in AI and VR (Stiles and Selz 1996: 384–498; Packer and Jordan 2001).

Sometimes, computers were used merely as "rather complicated tools, extending the range of painting and sculpture, performed music, or published literature". But in the more interesting cases, they made possible "*a whole new field of creative endeavor* that is as radically unlike each of those established genres as they are unlike each other" (Ascott 1990: 245; italics added). A key feature of *interactive* computer art was that the viewer wasn't just a viewer, but a playful participant—even a co-creator.

Computer art began in the mid-1960s (Klutsch 2005; Nake 2005). One of the first recognized computer scientists to support this new activity was Berkeley, who'd encouraged the young Sutherland brothers in their Squee project shortly before (and who'd written the first popular book on electronic computers: Berkeley 1949). The February 1963 issue of Berkeley's magazine, recently renamed *Computers and Automation*, announced a competition for "examples of visual creativity in which a computer plays a dominant role".

At much the same time, the first examples were already being produced. Some in New York, by Michael Noll (aided by the cognitive psychologist Bela Julesz), and some in Stuttgart, by Frieder Nake (1938–). Nake even used a new flatbed drawing machine constructed by Konrad Zuse himself (Nake 2005: 55; cf. Chapter 3.v.a).

As for interactive art, the first highly visible instance was *SAM* (Sound Activated Mobile), built by the cybernetic sculptor Edward Ihnatowicz (1926–88). Exhibited to public delight at the 1968 exhibition in London (and ending up in the Exploratorium in San Francisco), this looked like a large fibre-glass flower on a flexible stalk, with four petals and four stamens. But the stamens were radio sensors. They picked up noises made by the viewers, and caused the hydraulic pistons in the stalk to move the flower left or right, up or down, towards the source of the sound (Zivanovic 2005: 103).

That was the principle, too, of Ihnatowicz's even more famous interactive sculpture, *The Senster*. This eerie creature, built in the laboratories of University College London, had been commissioned by Philips, Eindhoven, and was exhibited by them from 1970. Looking like a 15 foot tall Meccano giraffe, it was equipped with sound and movement sensors attached to hydraulic motors that moved its head and neck towards the person making the sound/movement. It was even more compelling than Grey Walter's Festival of Britain tortoises (4.viii.a), because it seemed to engage with *you*, as an individual. In addition, it seemed to have feelings of its own: it would shy away from loud noises, and if the noise became overwhelming it would raise its head and "disdainfully ignore further sounds until the volume subsided" (Zivanovic 2005: 104–5).

(Philips eventually dismantled it, without informing Ihnatowicz, because they had problems in keeping it working properly. The visitors to their site, many of whom had come there only to view *The Senster*, had been complaining that it was malfunctioning—and Philips had had enough. Today, the reassembled and slowly rusting mechanical structure sits, *sans* electronics, on the premises of an engineering firm who'd done some work on it in its heyday.)

Other original applications were due to the Greek Australian performance artist Stelarc (1946–). His exceptionally imaginative performances included experiments linking/integrating his own muscles with robots, and with random or human-originated messages from the Internet (Stelarc 1986, 1994, 2002*a*,*b*; M. Smith 2005). They weren't mere physical exploits to be gawped at, but activities that raised questions about the limits of body and self (see Chapter 16.vii.d). Stelarc's artistic journey wasn't initiated by VR. He'd started out, in the late 1960s, with non-electronic man–machine linkages: meat hooks inserted into his skin, and attached to pulleys (Paffrath 1984). But as soon as it was possible to exploit robotics/telepresence/VR, he began to do so.

Most artists, however, were engaged in less intimate interactions, and with VR rather than robotics. Moreover, they weren't necessarily playing with different *realities* so much as a different *medium*: the computer-generated sensory image. That image might be shown on the machine's VDU, or projected onto a wall, or (occasionally) transferred onto canvas. And it might or might not be presented as an alternative world.

One of the first to produce "cybernetic" artworks, and explicitly to define a new aesthetic relating to them, was the British painter Roy Ascott (1934–), later described by the art historian Frank Popper as "the outstanding artist in the field of telematics" (Popper 1993: 124). While working at the Ealing School of Art in the 1960s, he helped initiate an artistic revolution. This was done partly through his own work, and partly by inviting like-minded pioneers such as Pask to lecture to his students.

His first interactive art didn't involve computers, but consisted of canvases with items/images on them that could be continually moved around by the viewer. So the "viewer" of the resulting collages was their *maker* too. This aspect was retained, and strengthened, in the computerized art that followed. The value of AI-based interactive art, for Ascott, was its ability to engage the viewer–participant *as creator* (Ascott 1964, 1966/1967; cf. 2003). By the same token, he held that its aesthetic crux was the nature of the interaction itself, not the (visual and/or musical) end result.

Inevitably, the authority of the single artistic signature was undermined, for crucial creative choices were now being made by the viewer. A quarter-century later, with the advent of the Internet and the Web, authorship of the "telematic" artwork might be very widely dispersed:

> [The] status of the art object changes. The culturally dominant objet d'art as the sole focus (the uncommon carrier of uncommon content) is replaced by the interface. Instead of the artwork as a window into a composed, resolved, and ordered reality, we have at the interface a doorway to undecidability, a dataspace of semantic and material potentiality. (Ascott 1990: 237)

> Telematic culture means, in short, that we do not think, see, or feel in isolation. Creativity is shared, authorship is distributed, but not in a way that denies the individual her authenticity or power of self-creation, as rather crude models of collectivity might have done in the past. On the contrary, telematic culture amplifies the individual's capacity for creative thought and action . . . *Networking supports endless redescription and recontextualization such that no language or visual code is final and no reality is ultimate*. (Ascott 1990: 238; italics added)

In brief: Ascott's telematic art is VR as a buttress of postmodernism. That particular philosophical interpretation is relatively recent. But right from the start Ascott was always more interested in the conceptual implications of cybernetic technology than in the technology itself. That's why, when the art critic Jasia Reichardt organized the now famous Cybernetic Serendipity exhibition in 1968, she didn't include him (J. Reichardt, personal communication).

Another early AI art experimenter was the artist–computer-scientist Edmonds (1942–), of the University of Loughborough—now, director of the Creativity and Cognition group at Sydney's University of Technology. In contrast with Ascott, he *was* primarily concerned with how the new technology could be used to create novel forms of art.

His inkjet print *Nineteen* (a static array of twenty coloured oblong patches on a near-white background) was first exhibited in 1968–9 and, having been accidentally destroyed, was reconstructed for a historical exhibition in the late 1990s. It was generated by a FORTRAN program designed to find the best of all possible placements of the twenty patches (each of which had already been designed by Edmonds), subject to a few constraints—e.g. that two particular pieces shouldn't be on the same row or column (because this would dominate the canvas).

Edmonds's aesthetic aim, here, wasn't merely to produce a visually interesting composition, but to focus the viewer's mind on the structural possibilities:

> It was intended that the assembly in the array be such that a feeling of finality be avoided. The variety of possible relationships between the elements is left for the viewer to sense without actually being able to move them about as he would in an assemblage. (Cornock and Edmonds 1970/1973)

In other words, Edmonds had used the computer "as a problem solver", to tackle an aesthetic problem which was "very hard to solve by hand or analysis" (Edmonds 2002). (In fact, in the three hours allocated, the computer didn't manage to find a solution satisfying all of Edmonds's constraints. But it got very near to doing so, and Edmonds himself was able to modify its solution slightly so as to get what he felt was the best arrangement. His Notes on how he did this were sometimes exhibited alongside the piece.)

His work in later years showed how to use computers as essential aspects of new creative styles, as well as aids in stylistic exploration. For example, he exhibited the world's first generative time-based artwork: *Fragments 1984/5* (a special case of what he named Video Constructs). Here, viewers could sense "the variety of possible relationships" by watching an ever-changing series of abstract compositions generated by a PROLOG program. *Time* was part of the artwork, as well as geometry and colour: variations in the order of images, and in the pace of change, provided different aesthetic effects. Even before Video Constructs, Edmonds had pioneered *interactive* art (e.g. Edmonds and Lee 1974). His novel HCI designs often involved threefold dependencies between action, graphics, and music. By the mid-1980s, he was developing complex VR technology too, for exploring 3D (or 4D, if one includes time) instead of 2D, as before (Wernald and Edmonds 1985).

In all these artistic endeavours, the underlying generative structure, not the individual images/sounds, was the focus of interest. So Edmonds opened a retrospective statement of his general approach by quoting Paul Cézanne's remark that "The technique of any art consists of a language and a logic", and Kasimir Malevich's that art-making involves "a law for the constructional inter-relationships of forms" (Edmonds 2003). (He also cited serialist composers, such as Pierre Boulez.) His very earliest art had been figurative. However, he was already tending towards abstractionism when computers came on the scene, and this technology led him (enabled him) to commit even more fully to abstract, formal, work.

The development of interactive art in the UK was strongly influenced, from the 1960s on, by Edmonds and Ascott. (Strictly, they weren't the ultimate pioneers, for Pask had placed his adaptive "Musicolour" in UK dance halls in 1953: see Preface, ii, and 4.v.e.) Across the Atlantic, the Bell Labs engineer Billy Kluver (1927–) cooperated in the 1960s with several New York artists—Jasper Johns, Andy Warhol, Robert Rauschenberg, and the composer John Cage—to produce electronic art of various kinds (Kluver 1966). By the end of the decade, the avant-garde interest was enough to prompt a seminal international meeting on "cybernetic" art at London's Institute of Contemporary Arts (J. Reichardt 1968). (Avant-garde, but not counter-culture: as remarked in Chapter 1.iii.d, this August 1968 exhibition couldn't have happened in Paris, given the political *événements* in July.)

In the 1970s the interest grew, and widened (Brown and Lambert in preparation). For example, the University of Wisconsin's Myron Krueger (1942–), trained as a computer scientist, was inspired by Cage's work on randomness to design interactive environments as artworks. In some of these, images of people located in different places were projected into a single virtual space—enabling the real individuals to experience the telepresence of their fellows, and to communicate accordingly. Like Ascott and Edmonds, he defined a new aesthetic, suggesting how "responsive environments" were best appreciated (M. W. Krueger 1977). (Krueger envisioned his interactive system spreading way beyond the art world, for use in education, psychology, and psychotherapy; the examples of surgery and training, above, wouldn't have surprised him.)

Today, computer-generated and/or interactive art is still cutting-edge, if no longer avant-garde (e.g. Krueger 1991; Candy and Edmonds 2002; Ascott 2003; Whitelaw 2004). It exploits VR, multimedia, the Internet, the Web, and A-Life evolutionary programming—all of which are dependent on AI research.

Both Ascott and Krueger had explicitly defined their new aesthetic in terms of the gallery visitor's *interaction* with the electronic object/environment. The beauty or interest of the product (the visual display, the music . . .) was much less important to them than the nature of the interaction. (This left open many questions about just what sort of interaction was to be valued, and why: Boden forthcoming.) At the turn of the century, a similar concern was taking over in VR research more generally—for education, certainly, but also for entertainment.

Now, in 2006, entertainment rules the VR roost. There are a number of massively multi-player online simulation games, such as Everquest. These may have hundreds of thousands, even millions, of human players, all continually building and extending online virtual worlds, and online relationships for play, trading, or debate. While most are fantasy worlds, in which players build imaginary cities and forge imaginary alliances, some are based on actual places and/or events—such as the Battle of Britain. The computer game based on this Second World War conflict provides detailed simulations of English airfields, towns, and cities (Bradbury 2003). The players create (or re-create) air crews, and engage in virtual bombing missions or Spitfire–Messerschmitt dogfights that may mimic specific events from August 1940. Clearly, AI/VR has come a long way since Spacewar (see 10.i.i).

Occasionally, the human–VR interaction spills intriguingly from simulation to reality in unprecedented ways:

> Some people are making their (real) living from playing virtual online games—by building up powerful characters or buildings, amassing virtual wealth, and then selling it for real money through eBay. (M. Sharples, personal communication)

And not only eBay: several dedicated web sites specialize in auctioning virtual characters and attributes for decidedly *non-virtual* money. Large sums of money may be spent on a single item, whether a carefully crafted VR person or a VR magic sword, and the overall spend is huge. Indeed, one US economist has estimated that the VR gaming world has a GDP per capita roughly the same as Namibia's (Mueller 2004: 13).

'Crazy', perhaps—but innocent enough. Indeed, less harmful than the quest for the black tulip in seventeenth-century Holland, in which livelihoods were lost as well as fortunes made. But whether all merging of the virtual and the real is as benign as this is questionable, as we'll now see.

d. Computerized companions

VR applications prompt the human subject to have 'reality experiences' to differing degrees. But they all foster experience as opposed to imagination. In other words, the willing suspension of disbelief with which one approaches films and theatre (and novels) differs radically from the *spontaneous experience* of reality that's brought about by successful VR.

For example, the world of a 1990s MUD (Multi-User Dungeon) chat room makes possible a set of shared experiences which simply weren't available before (Curtis 1992; Turkle 1995: 180–209, 248 ff.). It may generate a very strong sense of reality, leading MIT's Sherry Turkle (1948–) to say: "Our experiences [in cyberspace] are serious play. We belittle them at our risk" (Turkle 1995: 269). Some philosophers have even claimed that virtual worlds have *a new kind of reality*, rather than being simulations of something else (Graham 1999: 158–66; see also Haraway 1986/1991 and Turkle 1995).

These facts underlie some common misgivings about VR. For instance, the quotation above, referring to the virtual mechanic Steve as "he" and as a "teammate", raises some tricky questions. So besides asking whether VR (as Brooks predicted) has "got there", one might also ask *whether we'd really want it to*.

That question was addressed in the 1980s–1990s by many people. Some were professional VR technologists, such as Pavel Curtis (1960–) of Xerox PARC, creator of the popular MUD LambdaMOO (Lambda from LISP, MOO for MUD, Object-Oriented). LambdaMOO is an ever-growing multi-user virtual environment, with thousands of different rooms and outside spaces, described in words by its players (see Rex 2005). Besides creating LambdaMOO (in 1990), Curtis described his experience of maintaining it, which had shown him how people were using—and misusing—it (Curtis 1992). Some were social scientists: the sociologist Turkle (1995), for instance, or the social psychologist Neil Frude (1983). And some, such as Ascott (1990, 2003) and Haraway (1986/1991), were artists, littérateurs, or philosophers. The items represented by their various worry beads ranged widely over human experience, both actual and potential.

Concerns were raised, for instance, by VR applications that weren't just intellectually engaging, but emotionally engaging too. One example was Creatures, a computer game or (better) a computer world based on evolutionary programming (Grand *et al.* 1997; Grand and Cliff 1998; cf. Chapters 15.vi and 16.x.b).

Creatures caused considerable interest and experimentation on the Internet when it was commercially released in 1996–7, and swiftly led to several hundreds of dedicated web sites—and to improved versions in subsequent years (Cliff and Grand 1999). Indeed, it has aroused attention from cultural commentators, who see it as a paradigm of the technology of autonomy that underpins "the posthuman cyborg" (see Kember 2003: 83–109, and Chapter 1.iii.d). Here, however, what's relevant is that it turned out to be surprisingly addictive in a personal/emotional sense.

The user could hatch, teach, and evolve up to ten virtual animals ("norns") at a time. Each norn had an individual (genetically specified) outward appearance, a neural-network "brain", a particular temperament, and an underlying metabolism. And each learnt to behave in different ways, depending on its environmental history—including the situations devised, and the rewards and punishments administered, by the user. The properties that could be selected for breeding the next generation included temperament and bodily (i.e. computer-graphic) appearance.

The effect on users was compelling. Some youngsters became so emotionally attached to their carefully nurtured creatures that they would grieve if one was wiped from the disk—even though new norns could be hatched on command (M. Sharples, personal communication). Frustration and annoyance would have been appropriate, since the individual norns had been evolved and taught through many hours of highly concentrated interaction with the system. But grief, however fleeting, is another matter. It showed that the norns were being experienced as analogous to real people, or anyway pets (see 7.i.f).

Similarly emotion-ridden effects were often seen in the craze for Tamagotchis in the mid-1990s. (The Japanese word means 'egg-friends'.) These virtual beasties were even less real-looking than norns, having a near-minimal interface (only 10×10 pixels). The attraction, and the addiction, was in the need (if the beast was not to "die") for the user to nurture, feed, and even entertain it—all in real time. Norns and Tamagotchis were thus heralds of the end-of-century switch of VR emphasis from *interface* to *interaction*.

If even pixie-like norns and 100-pixel Tamagotchis can engage some people's emotions to this extent, what of more humanoid creatures? Pask's CASTE teaching machine had led some early 1960s users to experience "a sense of participating in a competition (some say a conversation) with a not dissimilar entity" (see 4.v.e). And Rodney Brooks's Cog robot could induce even a *technically knowledgeable and philosophically hostile* visitor to engage with it "as though in the presence of another being" (see 15.viii.a).

But those examples were merely tasters. Cog's long-eyelashed stablemate Kismet, built and nurtured (*sic*) by Brooks's students Cynthia Breazeal and Brian Scassellati, apparently expressed emotions as well as eliciting them (Breazeal and Scassellati 1999, 2000, 2002*a*,*b*). Like its predecessor *The Senster*, it seemed not only to pay attention but also to have its attention captured appropriately by the human's actions. Visitors to the MIT lab found it much more effective in eliciting emotional reactions than *The Senster*, and even more seductive than Cog (Fox Keller forthcoming). That's hardly surprising, given that its HCI was deliberately modelled on that of human infants/mothers (e.g. C. Trevarthen 1993).

Indeed, psychological research figures increasingly prominently in today's efforts to produce humanoid robots, and even "robot companions". For example, there have been systematic studies of how children's expectations of and interactions with robots compare with those of adults (e.g. Dautenhahn 1999, 2002; S. Woods *et al.* 2004, 2005).

Physical robots, such as Kismet, are limited by their creators' skills as mechanical engineers. With the potential of today's VR, the human's experience of "presence" can be made yet stronger.

Many years ago, the social psychologist Frude (1983) predicted personal GOFAI companions, in the form of talking sex dolls or screen displays. These would go far beyond Descartes's ill-fated (and probably apocryphal) automaton Francine (2.iii.f). They'd engage in intelligent and sympathetic conversations, speaking in friendly/sexy synthesized voices. And they'd learn what interested their human partner—so they wouldn't bore my son by talking about jewellery, or me by talking about cars. Moreover, they'd cater for a wider range of "interests" than one might think: cuddly/furry robots "would extend the range of potential activities and permutations to present a whole new realm of challenges to the sexually inventive" (Frude 1983: 174).

Frude's prediction hasn't come to pass. However, plenty of AI/VR work has been commenced which might help in moving towards it. (Some, indeed, is already being exploited for that purpose: see below.) For instance:

* The synthesized voices could now be given appropriate local accents (Chapter 9.xi.g).

* They could be given emotional intonations, too (Chan 1990; Cassell *et al.* 1994). In principle, they might one day include 'realistic' emotional features like those in John Clippinger's 'neurotic' NLP program (7.ii.c).

* The lip–sound synchronization for VR speech can be based on photographs of the lip movements of a specific person (9.xi.g).

* VR systems may also be able to 'lip-read' to aid their understanding (only sixteen lip positions are needed to represent English speech: Lucena *et al.* 2002).

* Animators are applying David Perrett's data on computer-simulated facial attractiveness (see 14.iv.d), and in principle could adjust their VR faces to exploit the female user's time of the month (Penton-Voak *et al.* 1999).

* Work's going on around the world on enabling AI systems to recognize people's facial expressions of emotion. (Steve is one example, mentioned above; others include robots developed by Fumio Hara's group at the Tokyo University of Science.)

* If the human user were to wear suitable VR gloves (as Steve's interlocutors already do), the AI companion could also use electrical signals from the skin to gauge the person's emotional state.

* Conceivably, the system might even be able to tell when the human is telling lies. For besides acting—through the gloves—as a conventional lie-detector, it might pick up facial cues to lying (7.i.d gives references to Ekman's work on lying).

* The companion's own 'face' might express emotions too, even if it were a doll (as opposed to a VDU screen). Roboticists in Japan and the USA have been modelling our detailed facial musculature, to enable their robot's rubberized 'face' to change its shape

according to one basic emotion or another—e.g. Hara's group, again. (Advances in materials science might provide more realistic 'skin' and 'flesh'.)

* Rod Brooks's Kismet robot—carrying floppy pink ears, huge blue eyes, and ultra-long eyelashes—engages people's interaction (and "caring" responses) despite its limited range of behaviour (15.viii.a);

* mutual gaze and joint attention are so convincingly modelled in Brooks's humanoid robots that even people who are in the know seem to sense an animate "presence" (15.viii.a);

* Negroponte's Media Lab have been aiming at highly personalized conversational computers since the 1970s, their ultimate test case being the intriguing all-anaphors interchange quoted in 9.xi.g.

* Finally, teledildonics (the mind boggles!) already offers a wide range of sex at a distance, sometimes involving wearable VR gear such as the "cybersex suit" or the "Virtual Sex Machine" (*Teledildonics* 2003). Future advances in haptics will probably be adapted for these purposes.

Frude saw convivial computerized companions as inevitable (1983: 58). The AI technology will be developed, and the businessmen (and the pornographers: p. 174) will then take over. The huge consumer demand, he said, will be driven by the universal human tendency to animism (which can now be understood as drawing on the concepts and inferential processes of Theory of Mind), in which intentional agency is ascribed to non-human beings (see 7.vi.f and 8.vi.d).

One must agree about the animism, and about the businessmen: just think of Tamagotchis, again. Although it's relevant to note, here, that robots are already being used for interacting with people who *lack* the normal tendency to animism. As we saw in Chapter 7.vi.f, autistic children lack Theory of Mind. Some people working in social robotics have tried to use robots to help mediate social interaction *between the autistic child and other humans* (Robins *et al.* 2005; cf. Robins *et al.* 2004). They've had some success, but they've also noticed that the robots can reinforce the autistic's tendency to fall into repetitive stereotyped behaviour. The use of non-human, and therefore non-threatening, robots in teaching autistic children isn't new: see the LOGO-based approach described in Weir and Emanuel (1976). What Weizenbaum would think about these projects probably doesn't bear repeating in polite company; but, as with Colby's automated psychiatric interviews, perhaps the key question is whether or not the child is actually helped (cf. 7.i.a).

But if one agrees with Frude about the animism, one may disagree about the full-blown technological prediction. Intellectually stimulating, and even emotionally engaging, robots and/or (Interneted) VR interfaces, yes. And pseudo-satisfying 'sexual' encounters, too. After all, both of these exist already. But the arguments elsewhere in this book suggest that the fully "intimate" machines envisaged by Frude are unattainable. NLP will never be up to it (Chapter 9.x–xi). Nor will our computational theories of motivation and emotion (7.i.e–f), nor of reasoning, relevance, and common sense (7.iv and iii.d, 10.i.f and iii.e, and i–ii, above).

Some AI professionals, however, might take Frude's side here. Attendees at the IEEE's latest international "Ro-man" symposium, whose theme was 'Getting to know Socially Intelligent Robots', probably would (see <http://ro-man2006.feis.herts.ac.uk>). And when I asked one of Steve's authors whether he shared my scepticism about this, he didn't say that I was obviously wrong—as some AI enthusiasts would have done. More cautiously, he said:

> Well, ever is a long time. I'm leery of speculating about what will ever happen in the future, not without some convincing argument that such a computerized companion is altogether impossible.
>
> I see rapid advances these days on computational models of emotion. I am not sure how far progress will go, but I don't see obvious barriers at the present time.
>
> And corpus-based NLP techniques are also making rapid advances. They make broad-based question–answer systems much more feasible than was the case in the past.
>
> Still, there are many barriers to creating a general conversational companion. Even if there is progress in some areas the overall goal still seems very far off. (W. L. Johnson, personal communication; quoted by permission)

As for whether such technical advances would be *ethically* welcome, Frude sat on the fence. He granted "the danger that contact with an artificial intimate [*sic*] might prove so fascinating and so satisfying that natural inter-human friendships would be overshadowed" (p. 174). But he pointed out that old people in Western cultures are often neglected, and suggested that—in the absence of real human interactions—this futuristic HCI would be better than nothing: "Such a machine promises freedom from loneliness and boredom and could end for ever the blight of social isolation with all its attendant evils."

You may find this suggestion chilling, a prime case of what Weizenbaum called "obscenity": substituting computers for humans in quintessentially personal contexts (7.i.a and 11.ii.d). If so, I agree with you. Nevertheless, at least one AI colleague—whom I shan't name—disagrees. Moreover, pushed by the demographics of an ageing population, Japan's Ministry of International Trade and Industry has recently launched a major research programme into humanoid robots, intended as helpers *and companions* for the elderly (Fox Keller forthcoming, n. 18). Presumably, these politicians have no ethical quibbles either.

Chilling or not, we don't need to worry too much about computerized companions—at least, not yet. VR avatars are another matter. People not at all worried by Frude's intimate machines, because they doubt that they'll ever exist, may already be very worried by these.

e. Psychology and avatars

In Hinduism, an avatar is an animal incarnation or representative of a god. In VR, it's a screen name/image that represents a particular human user (Damer 1998; Wilcox 1998). When someone uses the same avatar over a period of time, it grows a database for future use. (Steve isn't an avatar: he/it doesn't represent a specific person, merely a generalized human tutor.)

In general, an avatar prompts the psychological processes that enable us to interpret other people's behaviour. In other words, it taps into the user's Theory of Mind—as screen-borne or robotic companions would, too (7.vi.f).

Today's avatars include printed names/initials or abstract icons; 2D cartoons or photos of humans or animals; and 3D images of a person with face, body, and fingers—and bodily movements as well. The range of humanoid avatars is huge, not least because a company in Iceland sells software enabling you to design your own (Wilcox 1998, ch. 8). Already, several recent simulation games, such as The Simms, involve the DIY design of avatars.

Tomorrow's screen stand-ins will be even more realistic. Naturalistic gestures and facial expressions (remember Steve), and suitable voices, will be designable (Cassell *et al.* 1994). The more detailed the avatar's face, the more important that its lip movements match its voice plausibly. So special synchronizing algorithms will doubtless be employed too (Waters and Levergood 1995; Lucena *et al.* 2002).

An avatar converses either by writing text messages or by audible/visible speech. And it may walk to different locations in the virtual world displayed, taking up different points of view (affording different information) accordingly. It may soon be able to recognize emotions in the interlocutor's voice or written text, and *automatically* simulate them in its own facial expression (Olveres *et al.* 1998). (It's already possible for the avatar-holder to key certain facial expressions in by hand.) As ever in HCI, for the avatar to be realistic the psychologists must previously have found out just how *people* behave in comparable contexts (Cassell *et al.* 1994, again).

We don't have to look to the future to be worried by avatars, for they've already aroused social and psychological anxieties. The prime reason is that they enable a person to hide their identity, including their age and gender, and even to pretend that they're someone else. (Significantly, the chapter on avatar design that reports on the Icelandic company mentioned above is subtitled 'Who Would You Like To Be Today?'—Wilcox 1998: 187.)

Now, in the new millennium, several million people may be continually presenting (misrepresenting?) themselves as avatars within a single MUD. It's worth noting that these MUDs are often called MMORPGs: Massively Multi-Player Online *Role-Playing* Games.

The social psychologist Irving Goffman (1959) long ago pointed out the potential for deception and confabulation in role playing, and in the everyday "presentation of self". But VR is Goffman in spades. So one MUD enthusiast, for instance, said to Turkle:

> You can be whoever you want to be. You can completely redefine yourself if you want. You can be the opposite sex. You can be more talkative. You can be less talkative. Whatever. You can just be whoever you want, really, whoever you have the capacity to be. You don't have to worry about the slots other people put you in as much. It's easier to change the way people perceive you, because all they've got is what you show them. They don't look at your body and make assumptions. They don't hear your accent and make assumptions. All they see is your words. (quoted in Turkle 1995: 184–5)

This threatens not only to deceive other people, but also to confuse—and perhaps to destabilize—oneself. Just what might follow from a new realization of "whoever you have the capacity to be"?

Sometimes, the illusory avatars are harmless enough. For one thing, people only rarely adopt "a coherent character with features distinct from their real-life personalities" (Curtis 1992: 327). For another, their MUD avatars—according to Curtis—are driven by wish-fulfilment of a fairly innocent kind: "I cannot count the number of 'mysterious but unmistakably powerful' figures I have seen wandering about in LambdaMOO." The virtual world, he said, seemed to be enabling people to emulate various attractive characters from fiction. (How many Mr Darcys, I wonder? And how many James Bonds?)

But less attractive characters could be emulated too. This was illustrated by another of Turkle's interviewees, a male student whose avatars are often highly violent. He acknowledged that the violence is "something in me", and defended himself thus: "but quite frankly I'd rather rape on MUDs where no harm is done" (p. 185).

Well, perhaps. No woman is harmed (yet??). But the user himself may be harmed by repeatedly acting out such behaviour. "Perhaps", and "may be", because this is of course an empirical question. Given the ambiguous results of the massive research done on the psychological effects of watching violence on TV, it's a question that's not likely to be easily answered.

Again, a MUD user can sometimes take control of *someone else's avatar*, forcing it to do things which the genuine owner wouldn't want it to do. This allows for an especially 'realistic' version of virtual rape (Turkle 1995: 250–4). Here, the perpetrator doesn't merely act out the rape of a specially created (wholly imaginary) VR character, but forces cooperation (and 'signed' messages expressing satisfaction, and pleading that he should continue . . .) from a pre-existing avatar victim. Not surprisingly, the hidden humans violated in this way—who may or may not be female—don't like it. The common defence is: "This is a GAME, nothing more. [People need] to chill out and stop being so serious. MUDs are supposed to be fun, not uptight" (Turkle 1995: 253). I'm not persuaded.

In short, it's not at all clear that "no harm is done" by engaging in these unsavoury activities. But neither is it clear that simply *avoiding* unsavoury activities would neutralize all the potential problems.

Specifically, a number of VR critics have argued that merely adopting different personae at different times, whether unsavoury or not, may unsettle someone's psychological equilibrium. As Dreyfus put it, quoting Søren Kierkegaard, "the self requires not 'variableness and brilliancy' but 'firmness, balance, and steadiness' " (H. L. Dreyfus 2001: 82). You may feel that cognitive scientists shouldn't be troubled by this. For cognitive science sees the "self" as a narrative construction rather than an encapsulated essence (Chapters 7.i.e–f and 16.vii.d). But as a virtual machine (*sic*) that underpins the purposive coherence of the person's life, it had better have "firmness, balance, and steadiness". VR experiments in "multiple personality" are all very well, but in some cases they're playing with fire (Turkle 1995: 206 ff.; cf. 7.h).

Postmodernists, however, may not agree. For example, Kenneth Gergen, one of the first counter-culturalists to attack 'scientific' social psychology in the 1970s (see 6.i.d), has recently welcomed the relativizing effects of what McLuhan (1964: 3) called the global village:

> [We] exist in a state of continuous construction and reconstruction; it is a world where anything goes that can be negotiated. Each reality of self gives way to *reflexive questioning, irony, and ultimately the playful probing of yet another reality*. The center fails to hold. (Gergen 1991: 6; italics added)

He was talking primarily about the multiplication of real (i.e. geographical/cultural) environments in our experience, but he later included VR too (Gergen 1994).

The italicized phrases, above, express attitudes typical of VR users. So it's not surprising that other postmodernists, who already favoured the deconstruction of the self for independent philosophical reasons, welcomed the variation of self afforded by avatars in cyberspace. The leading proponent of that view was Haraway (1986/1991). But the influential artist Ascott, too, promulgated the notion of "Cyberself":

> We are each made up of many selves: de-centred, distributed, and constructively schizophrenic. We are the embodiment of technoetic relativity . . . [where a "technoetic" aesthetics focuses not on the surface image of the world but on] creative consciousness and artificial life. (Ascott 1998/2003: 375, 381)

And Turkle remarked approvingly that

> The rethinking of human . . . identity is not taking place just among philosophers [she meant postmodernists, but some philosophers of cognitive science are included too: Chapter 16.vii.d] but "on the ground," through a philosophy in everyday life that is in some measure both proved and carried by the computer presence. (Turkle 1995: 26)

Raising the question whether MUDs offer "psychotherapy" or "addiction", Turkle cited cases—including herself—where someone's experience of acting within a particular (real or VR) communicative world helped them achieve a more satisfactory personal balance (1995: 196–209, 262–3). And she endorsed postmodern relativism, "humor" and "irony" about these matters: "We do not feel compelled to rank or judge the elements of our multiplicity. We do not feel impelled to exclude what does not fit" (p. 262).

She also pointed out, however, that avatar experimentation has its limits. For instance, a man pretending to be a woman on the Internet *must* miss out on many aspects of the experience of being a woman, from the fear of pregnancy to worries about how much make-up to wear to a job interview (p. 238). And despite her postmodernist sympathies, she said: "Virtual environments are valuable as places where we can acknowledge our inner diversity. *But we still want an authentic experience of self*" (p. 254; italics added). She saw VR as offering us "a multiple but integrated identity whose flexibility, resilience, and capacity for joy comes from having access to our many selves" (p. 268). However, she added that "if we have lost reality in the process, we shall have struck a poor bargain".

In a nutshell, nothing here is straightforward. The evidence shows that avatars can be psychologically liberating as well as oppressive. They enable social experimentation and self-exploration that may be advantageous, up to a point. Possibly, they may even display 'the real person' for almost the first time. That was suggested by Turkle's interviewee Thomas, who told her: "MUDS make me more what I really am. Off the MUD, I am not as much me" (p. 240). (Of course, this isn't conclusive: he may have seen himself as much closer to Darcy or Bond than he really is.) It was suggested by a Texan muscular dystrophy sufferer featured in the Alter Ego photographic exhibition in London of MUD/MMORPG players and their online avatars (Mueller 2004: 13). And it was suggested, too, by William Horwood's (1987) haunting novel *Skallagrigg*.

That word "haunting" reminds us that novels, plays, films . . . provide forms of VR which may seem very real indeed. Kenneth Walton (1990) has argued that such arts depend on "make-believe"—in which we experience what it would *really* be like to be shipwrecked on a desert island, or to consider killing one's stepfather the king, but without experiencing the nastier aspects of those realities. (It's not clear that he's right:

if you know you can't drown, where's the "real" horror of shipwreck?—Graham 1999: 155–8; cf. Turkle 1995, ch. 9.) Walton's view implies that VR is just 'more of the same'.

If so, we should worry about it no more—though also no less—than we do about the violence in Quentin Tarantino's movies, or the pornography filmed on the other side of those Hollywood hills. There's a difference, however. VR worlds/avatars can offer an experience of 'immersion' stronger than that involved in reading a book or watching a film, and can even be individually crafted. As a result, they may be both more haunting and, in their practical consequences, more nasty.

Over and above the personal practicalities that have been the focus of this subsection, and the one before it, philosophical issues concerning self and embodiment are highlighted by VR, and especially by telepresence. These issues were raised long ago, for instance in a hugely intriguing thought experiment described by Daniel Dennett (1978*e*). The core question concerns the location, and/or the boundaries of the self. Does it stop at the skull? Or at the skin? Or does it encompass the cultural artefacts without which we couldn't function as we do—and wouldn't be the people that we are? If the latter, as argued for instance by Andy Clark (1997), then VR technology adds further opportunities, further complications . . . and further questions (A. J. Clark 2003*a*: 90–8). (The philosophy of self and embodiment is discussed in Chapter 16.vii.d; and the VR ideas that inform *The Matrix Trilogy* are related to philosophical arguments about realism in Chapter 16.viii.c.)

Much more could be said about this new cognitive technology, the realization of Bush's and Engelbart's dreams. There's already a huge literature on the virtues, and the dangers, of VR and the Internet. (Extensive bibliographies are given in Turkle 1995, Shields 1996, C. H. Gray 1995, and Haraway 1986/1991; for a strongly hostile approach, see the references cited in Kroker and Kroker 1997.) However, much of it is either technically ill-informed or philosophically naive—and some is both. My advice is to read Turkle's fascinating interviews and subtle psychological insights, and to look out for an informative and thought-provoking book being written by Yorick Wilks: a long-time aficionado of NLP (see 9.x.d), originally trained in philosophy, and currently a Fellow at the Oxford Internet Institute (Y. A. Wilks in preparation).

13.vii. Coda

Two hostile questions, one old-and-continuing, the other more recent, are often raised about AI. The first is whether it's a 'proper' discipline. In other words, is there a well-defined subject matter which AI is *about*? The second is whether AI—in particular, GOFAI—has failed.

Many critics answer "No" to the first and "Yes" to the second. I'll argue that the "No" is justified, up to a point, but that—again, up to a point—the "Yes" isn't.

a. Is AI a discipline?

In 1968 Michie and his co-editor Ella Dale opened *Machine Intelligence 2* with these questions:

> What is Machine Intelligence? Is it a theory, a discipline, an engineering objective? Or is it a pretentious name for the more peculiar parts of computer science? (Dale and Michie 1968, p. ix)

They weren't asking purely on their own account. Indeed, they answered immediately that

> In our view the matter is quite clear. Since the objective is to bring into existence an intelligent machine, those engaged in the attempt must look on themselves as engineers. (ibid.)

Rather, they were reflecting the puzzlement, not to say scepticism, being expressed by others.

There was a lot of that about. Even Lord Bowden, the leading computer scientist who agreed to write the Preface to one of Michie's *Machine Intelligence* volumes, asked, "is this enterprise of yours part of the main stream development of computers, or is it merely an interesting, but irrelevant, side line?" (Bowden 1972, p. v). Michie may have regretted inviting him to contribute the Preface, for he went on to say:

> How far can computers be used to solve the apparently insoluble intellectual problems of the day? I have probably misunderstood your work, but I have been disheartened by some of the research in which your members are engaged. It is pressing against the limits of knowledge and this is splendid, but are you right to be so worried about problems which appear to me to be semantically significant rather than technically important, and philosophically interesting rather than economically useful? (Bowden 1972, p. viii)

As for using AI to help us understand the workings of the human mind/brain, such as the recognition of gestalts, he despaired: "Heaven knows these problems defy solution—why therefore should one expect that they can be studied or solved or practised by computers?" (p. ix).

Bowden wasn't complaining that NewFAI was doing its job badly, rather that it had chosen the wrong job to do. Psychology (or the imitation thereof), he believed, was beyond the remit of AI as an intellectual discipline. But many other computer scientists at that time were asking not so much whether AI was a discipline as whether AI workers were disciplined. That is, the doubts about hype and lack of self-criticism which Drew McDermott would later discuss (see 11.iii.a), and which would soon land Michie himself in very hot water (11.iv.a), were already bubbling beneath the surface.

Those doubts were still bubbling in the late 1970s, when a historian of AI was told by orthodox computer scientists that it's "a 'freaky', rather dubious fringe activity" (Fleck 1982: 207). And they're still bubbling now.

In 1996, for instance, the AAAI Fellows had an email debate about whether AI is "engineering" or "science". Some voted for the former, as Michie had done long ago. Simon's contribution was brief, and to the point. He referred his fellow Fellows to two very recent papers, including a "case-study" on creativity (Simon 1995*b*,*c*), and to his classic *Sciences of the Artificial* (1969; see especially chs. 1, 3, and 4). He insisted that—alongside its engineering aspects—"artificial intelligence is science". And he added testily, "I do think that members of the community who wish to discuss these matters have some responsibility for familiarity with the serious literature on the topic" (AAAI Fellows' email list, 22 May 1996).

Two years later, the editors of the 100th volume of *Artificial Intelligence* exulted that it had been firmly established as "a discipline" (Bobrow and Brady 1998*a*). It was now respected even by engineers, they said—adding that Sir James Lighthill had got his come-uppance at last (cf. 11.iv).

But this upbeat editorial carried a whiff of Hamlet's "Methinks the lady doth protest too much". Other remarks in their essay bemoaned the specialist splintering that was preventing NLP or vision research, for example, from being submitted to the core journal. This splintering soon brought complaints also from two presidents of the core society, AAAI. Ronald Brachman's Presidential Address in 2004 complained about the centrifugal tendencies of AI, and his successor, Mackworth, declared that "The way to overcome these centrifugal tendencies is to develop better theories of cognitive architecture and to work in integrated applications [i.e. GOFAIR: see Section iii.c above, and 11.iii.b]"—adding that AAAI was "needed more than ever", being "the premier venue for bringing together the subdisciplines" (Hedberg 2005: 12). (However, he's optimistic: "There was a lot of evidence [at the AAAI and IAAI meetings in 2005] that AI is in a healthy state, [with many] exciting new ideas and applications. Our field is reaching out to many related disciplines in a new way. That to me is a sign of maturity: we are no longer insecure as a discipline"—Hedberg 2005: 15.)

Besides their complaints about subdisciplinary splintering (and the profusion of subdisciplinary journals), the *Artificial Intelligence* journal editors even complained that too many papers were now purely mathematical, rather than implementational. In short, they were implying that some of their own colleagues didn't have a good sense of AI *as a discipline*. (Soon afterwards, they underlined the wide scope of current AI by reprinting the fourteen invited lectures from IJCAI-1997—ranging from Bayesian analysis and non-monotonic logic, through NLP, planning, and visual identification, to social robotics and creativity: Bobrow and Brady 1998*b*.)

Further evidence of continued bubbling comes from the delightful Father Hacker, who's still a regular contributor to the *AISB Newsletter/Quarterly*. A key item of advice in his recent 'Guide for the Young AI-Researcher' is this:

> The basis for your reputation is a notable piece of science or engineering, with which you are associated worldwide. But in this environmentally aware world, recycling will avoid unnecessary effort on your part. The recent history of AI is replete with the revival of old techniques (neural nets, genetic algorithms), rejected as unfeasibly inefficient in the Mid-Twentieth Century [see Chapters 12.iii and 15.vi.b], but given new life by Moore's Law [i.e. the inexorable growth of affordable computing power] in the Twenty First. A bit of historical research, plus some judicious renaming to place the old ideas in a modern context, will pay dividends. (*AISB Quarterly* 2003: 12)

If today's AI researchers aren't deliberately stealing and renaming old ideas, as Father Hacker advises, they are sometimes reinventing the wheel—and lack of historical knowledge doesn't help. For instance, 1980s–1990s work in machine learning often replayed insights available in traditional statistics (see 12.vi.f, and iii.f above); and the NewFAI researchers Michie and Gregory both said that the best model of the real world is the real world itself, long before the situated roboticists did (iii.b, above).

A related failing is AI's vulnerability to fashion—and to the uncollegial scorn that comes with it. Dissension, if not active scorn, has characterized AI almost since the beginning.

(AI isn't alone. Professional psychology, too, has long been subject to fashion and unpleasantness: see 7.vii.b and e. Anthropology has suffered dreadfully from professional dissent: see 8.i.d and ii.a–c. As for philosophy, that's not all sweetness and light either. Just look at Bertrand Russell's comments on Ludwig Wittgenstein's mature work, or at some of the epithets hurled at Heidegger by Anglophone philosophers: see Chapter 16.vii.a.)

Much of the rivalry within AI was due to the explanatory/methodological schism that developed in the 1950s (4.ix). The late 1960s scandal over perceptrons was just one high-visibility example (Chapter 12.iii).

One might have expected the dissension to have settled down by the mid-1980s. But when AI was already some 30 years old, Newell identified no fewer than thirty-six issues on which AI researchers had been divided, and in many cases still were (Newell 1983: 191). Worse, the division was often unscientific, not to say ungentlemanly. Newell lamented the "sloganeering character" of these disagreements (p. 189). And he complained bitterly about the disputatious tone of an anti-GOFAI paper published in the same volume, "written to accomplish science entirely by means of commentary" (p. 292).

Today, things are scarcely much better. As remarked in Chapter 4.ix.a, a recent AISB editor complained: "The lack of tolerance [between different research programmes in AI] is rarely positive, often absurd, and sometimes fanatical" (Whitby 2002*b*). The charge of *being ill-disciplined*, then, still has some force.

But the scepticism that Michie was seeking to counter may have been caused also by AI's use of two very different methodologies, symbolic and connectionist. Lee Cronbach, had he seen Michie's questions, might have replied that AI wasn't a "discipline" for that reason (see Chapter 7, preamble). Were he alive today, Cronbach would have been even more puzzled, for AI now involves evolutionary computing too—not to mention the so-called nouvelle AI of situated robotics and enactive perception (15.vii–viii and 7.v.e–f). Even though all of these are in principle compatible, individual AI scientists tend to choose one rather than another.

There was another worry, too. That reference to "the more peculiar parts of computer science" could be interpreted as referring to peculiar *topics*, not just peculiar (even ill-disciplined) *methods*. If AI was truly a discipline, what was it studying which wasn't better studied by some other part of computer science?

Nearly twenty years after Dale and Michie, on the occasion of the *Artificial Intelligence* journal's fifteenth birthday, a different editorial duo addressed much the same doubts. Bobrow and Hayes (1985) invited about twenty people to answer ten questions about the achievements of AI, including both GOFAI and connectionism. One of their ten queries asked whether it was a single discipline or just a miscellaneous collection of ideas about non-numerical computing. The unspoken implication was that these ideas were perhaps not only disparate, but largely half-baked and/or "pretentious" too.

In answer to the editors' questions, a number of leading AI experts said it *wasn't* a single discipline. For example:

> AI, broadly conceived, is just too large to be a single discipline—mainly because intelligent perception and behavior touch so many aspects of computer science, control theory, and signal processing theory. (Nils Nilsson, quoted in Bobrow and Hayes 1985: 376)

> If AI is a single discipline, it is more like Biology than Neuro-biology. It is inherently multi-disciplinary, and the more AI is applied the more disciplines will be involved. (Aaron Sloman, quoted in Bobrow and Hayes 1985: 377)

At the same time, some of the very same people said that there *was* a core discipline there. But this was identified in very different ways, sometimes as "declarative" GOFAI, sometimes much more broadly—as the intellectual core of cognitive science in general:

> [That] part of AI based on reasoning with declaratively represented knowledge (e.g. logical formulas) with semantic attachment to specialized procedures and data structures is a coherent field. I think it will inherit the name AI and continue to develop as the core discipline underlying intelligent mechanisms. (Nilsson, quoted in Bobrow and Hayes 1985: 377)

> There is a central coherent core of AI/Cognitive Science [*sic*] which is the systematic study of actual and possible intelligent systems. This seeks general principles, not just successful designs. (Sloman, quoted in Bobrow and Hayes 1985: 377)

Very few people, Sloman added, were working on that—"partly because it is the hardest part of AI, and has least short-term pay-off". (His own work, however, had already been focused on it for some years: Chapters 7.i.f and 16.ix.c.)

My own answer said, in part:

> AI suffers from some of the same problems as philosophy: it tackles the unanswered, or even the unanswerable, questions. As soon as it manages to find a fruitful way to answer one of them, the question gets hived off as a specialist sub-field. The special sciences started emerging from philosophy in the Renaissance. Specialist areas of study have been emerging from AI for only thirty years. But already we have distinctive sub-fields—such as pattern-recognition, image-processing, and rule-based systems. Their executors often speak of AI (if they speak of it at all) as something not quite respectable, something nasty in the woodshed that was once glimpsed but is better forgotten. What should not be forgotten is that the respectable topics were excluded from what people are prepared to call "AI" as soon as they became "respectable". (M. A. Boden, quoted in Bobrow and Hayes 1985: 376)

Today, some twenty years later still, I stand firm by that reply—except that I should have made it clearer that the "sub-fields" (and the discoveries) are often attributed to *computer science*, or to *software engineering*, not to AI itself. Time sharing, for instance, was developed by McCarthy, and so were fast-prototyping and data mining. But they're not usually thought of as having hailed from AI.

A proliferation of specialist journals has appeared since *Artificial Intelligence* first came off the press. New ones are founded with relentless regularity, accompanied by specialist (and sub-specialist . . .) conferences whose papers, and even whose CFPs (Calls for Papers), are nigh-unintelligible to all but dedicated experts. Computational methods are there being perfected, details added to answers, new questions raised—and, often, new applications made possible. It ought to be acknowledged, however, that the pioneering methods, questions, and answers came largely from AI.

Those questions for which reliable methods and answers haven't yet been found still lie within AI itself. Since they aren't yet well defined, it follows that—in a sense—AI *isn't* a proper discipline. But it doesn't follow that it's not worth pursuing. The very existence of so many specialist offshoots, in areas which initially would have been regarded as "peculiar", "pretentious", or even "freaky", shows the contrary.

Some philosophers, of course, argue that the central goals and assumptions of AI, and especially of strong AI (16.v.c), are fundamentally incoherent. If so, then strong AI certainly isn't a proper discipline—much as fairyology isn't, and couldn't be. On their view, it's *philosophically* pretentious, since it pretends to possess an explanatory potential which, in principle, it lacks. Whether that's true is explored throughout Chapter 16. But even if it is, it doesn't apply to most AI work—for most AI researchers avoid making claims about explaining or constituting "real" intelligence.

b. Has GOFAI failed?

The second hostile question often heard today is sometimes expressed in terms of "AI", but is often intended primarily for GOFAI. The charge is that AI has *failed*.

This charge was made by Dreyfus (hardly a disinterested witness), in his 1998 Houston University lecture on 'Why Symbolic AI Failed'. And a few years earlier, he'd said:

> Like the dissolution of the Soviet Union, the speed of *collapse* of the GOFAI research program has taken everyone, even those of us who expected it to happen sooner or later, by surprise. (H. L. Dreyfus 1992, p. xiv; italics added)

> The rationalist tradition [in philosophy] had finally been put to an empirical test, and it had *failed*. (H. L. Dreyfus and Dreyfus 1988: 34; italics added)

The connectionist Robert Hecht-Nielsen might not agree with Dreyfus about the philosophical "why", but he does agree with him about the practical verdict:

> [As] it turned out, this approach [GOFAI] *never amounted to much*—a realization which it took two decades and billions of dollars to establish and which did yield *a handful of valuable accidental discoveries*. (in J. A. Anderson and Rosenfeld 1998: 303; italics added)

A negative verdict is strongly implied, too, by Brooks—another non-disinterested witness—in his 1991 paper 'Intelligence Without Representation', which argued that GOFAI was/is fundamentally wrong-headed (Section iii.b, above, and Chapter 15.viii.a). Indeed, given the huge media publicity which Brooks's work attracted, his habit of using the term Artificial Intelligence to cover *only* GOFAI was partly responsible for the general belief today that AI has failed (see 1.iii.g).

Two of those three men have a numerous following, among the public as well as in cognitive science itself. The feminist Sarah Kember, for instance, cites Dreyfus with approval (though adds that he's "exceeded by Donna Haraway's formulation of situated knowledge"—Kember 2003: 9). She has sympathy for Brooks's approach, even though she accuses him of working "without any real regard to human implications, social and scientific applications or philosophy" (p. 69). Indeed, she devotes many pages of cultural commentary to celebrating the successes and potential of A-Life. In her mind, however, "AI" (i.e. GOFAI) is different: her opening page proclaims "the failure of AI as a project" as an apparently self-evident proposition.

So an opinion poll on the matter of AI's failure would have depressing results for GOFAI pollsters. However, people—even *most* people—can be wrong. Are they wrong, in this case?

If the measure of failure is whether AI has lived up to its most extravagant promises, then they're right: it's failed. The "world chess-champion by 1967" didn't happen (although a world-beater had emerged by 1997). And much of the connectionist hype that irritated Bernard Widrow (12.vii.b) hasn't been fulfilled either.

To be sure, if something hasn't happened yet it may happen tomorrow, or the day after. Undated promises must be treated with care. However, my own view—some AI colleagues would disagree—is that some promises won't be fulfilled for hundreds of years, if ever. Perfect, or anyway human-level excellent, translation is one example (9.iv.b and x.e).

Further 'failures' are doubtless in store—especially if we listen to the futurologists. One such, British Telecom's Ian Pearson, recently hit the headlines (see BBC Online, 18 January 2002: Sci Tech News). He predicted that by 2010 the first robot will have passed its GCSE exams, which British children take at age 16; A Levels (age 18) will follow a few years later, and the robot will be ready for its degree a few years after that.

This fantasy makes RoboCup's predictions of a world soccer champion by 2050 seem cautious by comparison (11.iii.b). The AISB journal editor judged it extraordinary that a professional within a high-tech company like BT should believe such nonsense, and I agree with him:

> It could be that BT has something very special in a secret lab somewhere. Then again, it could be that Mr. Pearson has based this prediction on observation of tealeaves rather than of robots. (Whitby 2002*a*)

(In Britain, reading the tea leaves left in someone's cup is the equivalent of Roman augurers' reading chicken bones. Unfortunately, this charming practice is dying out, because of the widespread use of teabags.)

But fulfilment of hyped-up promises isn't a reasonable criterion of success-or-failure. That's especially true, given that many (most?) people in the field didn't subscribe to them in the first place.

By the same token, 'passing the Turing Test' has never been an appropriate criterion of success in AI (see Chapter 16.ii.c). So the fact that—with a handful of (arguable) exceptions—it hasn't been passed is neither here nor there. Nevertheless, many people still assume that it's highly relevant. Dreyfus's 'Failure' lecture, for example, opened with a reference to Turing's remarks about human–computer indistinguishability by 2000.

Rather, we should ask whether AI—symbolic, connectionist, evolutionary, or situationist—has made significant progress towards the goals expressed in less careless moments. In Naudé's terminology (see Preface, preamble), we should credit AI researchers with a concern not for Magick but for Mathematicks, and ask how effective their Mathematicks has been.

To answer that question, we must recall the distinction between technological and psychological AI. The goal of the former was/is to build useful computer systems—doing or, as Minsky forecast in 'Steps' (10.i.g), *assisting with* tasks which humans want done. (One can't say "tasks which humans would otherwise have done", for some tasks can be performed only by means of huge computer power.) The goal of the latter is to develop explanatory theories of mind, and perhaps also (according to strong AI) to build computer systems that are genuinely intelligent in themselves.

As regards technological AI, the response to "Has AI failed?" must be a resounding "No!" Even though this book virtually ignores applications, a few are briefly mentioned. Examples include:

* software for translation (Chapter 9.ix.f) and speech generation (9.xi.g);
* computer vision techniques (7.v.c) and 'retinal' VLSI chips (12.v.f);

* expert systems in many different areas (10.iv.c and ii.b–c, above);
* Hollywood animation (15.ii.a and x.a);
* interface design (v, above);
* virtual reality systems (vi, above);
* computer-assisted and/or interactive art (iv and vi.c, above);
* robotic surgery (vi.b, above);
* and, regrettably, battlefield management and missile guidance (3.v.a, 4.vii.a, and 11.i).

A volume even longer than this one could be written about technological AI as a whole. Besides the many high-profile examples, it would have to mention the host of *invisible* AI applications, of which the general public aren't even aware. Sometimes they're invisible because the businesses that commissioned/built them regard them as trade secrets. More often, they're invisible because miniaturization, and sometimes wireless communication too, have made them so. Weiser, in his *Scientific American* article of 1991, spoke of "calm technology, when technology recedes into the background of our lives".

Even before the arrival of literally "ubiquitous" computing, AI already pervades our lives to an unrecognized degree. Everyone in industrial societies is surrounded by it: in the street, the office, the supermarket, the factory, the bank, the car, the airport, the home . . . and so on, ad nauseam. Try to launder money for the Mob, for example, and you may be caught out not by a detective but by a machine (Senator *et al.* 1995). Some of these AI systems are neural networks, like those used (unseen by their customers) by financial institutions of various kinds. Others are GOFAI programs, like those used (again, unseen) by insurance salesmen in telephone call centres.

Even when the public are aware that such programs are being used, they may not think of them as *examples of AI*:

> Knowledge-based systems have permeated almost all areas of modern life. Indeed this area of AI has been so successful that many people no longer associate it with AI. They simply see advice-giving as yet another thing that computers can do. That is, I suppose, the ultimate success for any branch of science. (Whitby 2004: 36)

We've seen, for instance, that the longed-for Semantic Web—typically regarded as a futuristic project *for computer science*—will require advances in ontology research (Section i.c, above). But work on computerized ontologies was pioneered by NewFAI, both for AI planning and for NLP. Indeed, this was a key theme of the seminal paper on 'Some Philosophical Problems' (Chapter 10.iii.e).

Besides specific application programs (many of which outperform human beings in their particular specialized task), a host of widely used computer technologies, not normally thought of as AI, actually originated in AI research:

> Work in AI has pioneered many ideas that have made their way back to mainstream computer science, including time sharing, interactive interpreters, personal computers with windows and mice, rapid development environments, the linked list data type, automatic storage management, and key concepts of symbolic, functional, dynamic, and object-oriented programming. (S. Russell and Norvig 2003: 15)

AI technologies underlie many Internet tools, such as search engines, recommender systems, and Web site construction systems. (S. Russell and Norvig 2003: 27)

[There are many] examples of AI technology that are now so pervasive that few people even realize they involve AI concepts. These include spreadsheets, speech recognizers, digital maps, and even the World Wide Web, which is just a large semantic network. (Pat Langley, email to AAAI Fellows, 14 August 1998; quoted by permission)

Video games and animated movies are using techniques developed in robotics. For example, path planning algorithms are used to move soldiers around in video games. [Added May 2004: Bodily control and perception algorithms are also being used to produce plausible characters in visual reality.] Machine vision techniques are also in use to search and index images in large databases such as the world wide web. (Matt Mason, email to AAAI Fellows, 17 August 1998; quoted by permission)

[User-friendly AI-based systems include] programs which can detect the presence of known or new viruses in computer programs; checkout scanners which can identify fruit and vegetables through the use of scent sensors; car navigation systems which can guide a driver to a desired destination; password authentication systems employing biometric typing information; ATM eyeprint machines for identity verification; and molecular breath analyzers which are capable of diagnosing lung cancer, stomach ulcers and other diseases. (Lofti Zadeh, email to AAAI Fellows, 17 August 1998; quoted by permission)

As two of those quotations point out, many of these things are thought of as "mainstream" computer science, not as AI at all. (This is yet another illustration of AI's successes being relabelled as belonging to some *other* discipline.) Sometimes, such relabellings are truly bizarre: an AI colleague of mine was recently astonished, and outraged, to be told by a professional computer scientist that "NLP isn't AI, it's computer science" (Sharon Wood, personal communication).

Sometimes, to be sure, there are disasters. The newspapers abound with horror stories about hugely expensive government software failing to do what it was intended to do. Nevertheless, the notion that AI, or even GOFAI, has failed as a *technological* enterprise is absurd. So also—IMHO, as they say on the emails—is Hecht-Nielsen's reference to "a handful of valuable accidental [GOFAI] discoveries".

More to the point, for our purposes, is the question whether *psychological* AI has failed. This can be posed with respect to both weak and strong AI. The success/failure of strong AI will be discussed in Chapter 16, so let's concentrate on weak AI here.

People often say that whereas connectionist AI has succeeded in helping theoretical psychologists, GOFAI hasn't. At best, they'll allow that GOFAI was historically significant, in encouraging psychologists of the 1960s–1970s to think computationally and to build computer models (Chapters 6.iii–iv and 7). Considered as psychology, they say, GOFAI has failed.

The Dreyfus brothers, for instance, declared in 1978 that "classical, symbol-based, AI appears more and more to be a perfect example of what Imre Lakatos [1970] has called a degenerating research-programme". And fourteen years later, Hubert Dreyfus repeated that charge almost word for word (1992, p. ix).

It's true that some psychological topics haven't been, and perhaps never can be, adequately addressed by hands-on GOFAI modelling or even by detailed computational theorizing. The identification of relevance (7.iii.d and 8.vi.c) is an example. Such topics are intractable because of the frame problem (10.iii.e and i–ii, above); the fuzziness of concepts (8.i.b, 9.x.d, and 12.x); cultural variations in language and custom (8.i and 9.iv.b); the highly idiosyncratic experience and world-knowledge of human individuals; and, not least, human *embodiment* (16.vii, and iii.b–c above).

Nevertheless, GOFAI modelling is still being used—sometimes alone, sometimes in hybrid systems—to develop theories of hypnosis (1.i.g); of absent-mindedness and pathological disturbances of everyday action (12.ix.b); of artistic and scientific creativity (iv, above); and of anxiety (7.i.f). And GOFAI ideas are helping us to explore the mental architecture of the mind as a whole (7.i.e–f and 12.iii.d). The people engaged in this work include clinical neurologists and psychologists as well as AI scientists, so it can't be dismissed as merely a fantasy of the nerds, nor even as pure (impractical) theory.

Indeed, we saw in Section iii.b and 12.viii–ix (and shall see again in 14.ix.b) that symbolic AI's methodological rivals—connectionism, situated AI/A-Life, and dynamical systems—can't cope with some phenomena that are crucial features of human psychology. Means–end planning, deliberate reasoning, and flexible thought using objective concepts are the prime examples. Philosophers such as Fodor, Kirsh, and more recently Samuels, despite the disagreements between them (over modularity for instance: 7.vi.d–e), have spelt out some of the reasons. Plenty of puzzles remain: SOAR, or the GOFAI aspects of ACT* (7.iv.b–c), haven't solved all the problems. But the point is that, *in principle*, some sort of GOFAI approach will be needed (as well as connectionist and situationist ideas) to understand the human mind. Clearly, then, the charge that GOFAI is "a degenerative research programme" is mistaken.

Blay Whitby has put it in a nutshell:

> A myth has developed that AI has failed as a research programme. This myth is prevalent *both inside and outside* AI and related scientific enterprises. In fact AI is a remarkably successful research programme which has delivered not only scientific insight but a great deal of useful technology. (Whitby 2004: 1; italics added)

He points out that GOFAI and connectionism alike have been—and are—useful, in both academia and the marketplace. Indeed, one very widely used commercial application, the UK's prizewinning Clementine data-mining system, relies on an amalgam of both methodologies—with evolutionary computing (15.vi) thrown in for good measure (Khabaza and Shearer 1995).

In sum, GOFAI hasn't "failed", and it hasn't ceased either. Today's AI champions can still produce defensive rhetoric that makes one wince:

> Researchers in AI have traditionally met problems of formulation joyfully, courageously, and proudly, accepting the severe risks ensuing from such exploratory work ... (Doyle and Dean 1997: 88)

(Entire boxfuls of medals couldn't do justice to such heroes!) But the same writers aren't going over the top when they say:

> As a field, AI embarks on the next fifty years excited about the prospects for progress, eager to work with other disciplines, and confident of its contributions, relevance, and centrality to computing research. (p. 99)

As Mark Twain might have said, the rumours of its death are greatly exaggerated.

14

FROM NEUROPHYSIOLOGY TO COMPUTATIONAL NEUROSCIENCE

In his peppery *Dictionary of Psychology* (1995), N. Stuart Sutherland said that cognitive science and neuroscience are ''almost mutually exclusive'' (see Chapter 1.ii.c). This was more than a little naughty. As an expert in vision and a friend and admirer of David Marr, and as a colleague of Albert Uttley, Sutherland knew very well how necessary that ''almost'' was. They'd long pointed out that a theory of the brain needs a theory of what it is that the brain is doing.

Alan Turing had said this long ago (in 1951), in a letter to the anatomist J. Z. Young:

> I am afraid I am very far from the stage where I feel inclined to start asking any anatomical questions. According to my notions of how to set about it, that will not occur until quite a late stage when I have a fairly definite theory about *how* things are done. (quoted in S. S. Turing 1959: 146; italics added)

That ''how'', of course, was a computational how. And the principle hadn't changed since 1951. By the time Sutherland made his remark, many psychologists had said the same thing (e.g. Mehler *et al.* 1984), and so had Chomsky, in 1965 (Chapter 7.iii.a). What's more, many neuroscientists agreed. Michael Gazzaniga (1939–), for instance, had recently insisted that ''neuroscience needed cognitive science . . . to attack the central integrative questions of mind–brain research'' (1988: 241).

This is a very general point. Even in GOFAI, as we saw in Chapter 10.v, what's usually of interest is the *virtual* machine defined by the programming language and/or the program, not the manufactured tin can which—in *physical* terms—is doing the work.

Sutherland's claim (in the same dictionary entry) that cognitive science concerns the brain's *software* and neuroscience its *hardware* also needed qualification. He himself had long rejected Hubert Dreyfus's view that ''we must leave the physical world to the physicists and neurophysiologists'', to the exclusion of AI (see Chapter 16.vii.a). On the contrary, he'd said, we need high-level (software) concepts to understand brains as well as computers:

> Understanding the workings of the brain also involves us in developing the appropriate concepts to summarise blocks of similar operations conducted by it and an appropriate language in which

such concepts can be embedded and manipulated . . . e.g. negative feedback, receptive field, iconic store, and so on . . . (N. S. Sutherland 1974: 265)

In short, it's not true that computation is irrelevant if we have the neuroscience (a claim made also by Dreyfus's Berkeley colleague John Searle—1992, ch. 10). For, so Sutherland was saying, the one is integral to the other.

If neuroscience looks at the brain (and the rest of the nervous system) and asks "What does this bit do?", *computational* neuroscience asks "How does it manage to do it?" And that "How?" is computational. As John Mayhew, another vision expert (and a follower of Marr: see Chapter 7.v.e), has put it:

Finding a cell that recognizes one's grandmother does not tell you very much more than you started with; after all, you know you can recognize your grandmother. What is needed is an answer to how you, or a cell, *or anything at all*, does it. The discovery of the cell tells one what does it, but not how it can be done. (Mayhew 1983: 214; italics added)

Even if the detailed wiring diagram is available, as it was for the cerebellum by the early 1960s (see Section iv.c), *what the wires are doing* may be unknown. The key questions concern what information is received and/or passed on by the cell or cell group, and how it's computed by them. Put another way, they concern "how electrical and chemical signals are used in the brain to represent and process information" (C. Koch and Segev 1989: 1).

The differences between the systems discussed in Chapter 12 and those described here are outlined in Section ii. Then, we'll look at some early examples of neuroscientific work driven by computational questions, though rarely aimed at implementation. Section iv explains how computational ideas helped inspire the discovery of feature-detectors. Section v outlines early models of specific parts of the brain (such as the reticular formation and the cerebellum), while Section vi considers some especially 'realistic' system-level simulations. The whole-animal approach of computational neuro-ethology is considered in Section vii.

Various examples and definitions of "representation" are discussed in Section viii, followed (in Section ix) by some common sources of scepticism about AI neuroscience. Lastly, in Sections x and xi, we'll ask whether cognitive neuroscience has moved us—or ever could move us—closer to an understanding of consciousness.

Before embarking on the substantive issues, however, let's look very briefly at the history of two labels.

14.i. Notes on Nomenclature

In one sense, the story told in this chapter is anachronistic. For I often use vocabulary that's relatively recent to describe work done many decades ago. That's deliberate, for I'm seeking to show the links, indeed the continuity, between that older work and what went on later.

Specifically, if you go back to read the neuroscientific literature published before the early 1960s, you won't find the word "neuroscience". And you won't find the phrase "computational neuroscience" until a quarter-century after that. (As for "cognitive neuroscience", those words first left the presses after the one and before the other: see Section x.b, below.)

a. The naming of neuroscience

Neurophysiology became computational before it became (i.e. before it was called) an aspect of neuroscience. When I was a medical student at Cambridge in the 1950s, some of our supervisors (notably Horace Barlow) were already starting to talk about neurones in informational terms. But the word "neuroscience" was never used. We spoke rather of neurophysiology and neuro-anatomy (hardly, yet, of neurochemistry) . . . and of clinical neurology, psychopathology, and psychiatry. Moreover, we thought of these as largely *distinct*, in practice if not in principle.

The new term was coined at MIT in the early 1960s by the neurochemist Francis Schmitt. (He also coined the term "information substances" twenty years later, to cover "a variety of transmitters, hormones, factors, and protein ligands": S. Beer 1999. That is, even at the level of the neurochemicals themselves, *messages* were deemed to be as important as molecules.)

Schmitt needed the new name for a nationwide project specifically aimed at uniting several sociologically distinct camps. He was bringing together at least half a dozen experimental scientific disciplines (ranging from molecules to behaviour), and clinical expertise too. As he recalls:

> [Now] in the 1990s such a program seems entirely reasonable, *but in the 1950s and 1960s it was not so*. The boundaries between the various biomedical disciplines . . . were clearly defined, particularly for didactic purposes in medical school curricula. Professionals in individual fields looked somewhat askance at experts in other fields who presumed to be knowledgeable also in theirs, as would certainly be the case in the new field I then had in mind. (Schmitt 1992: 1; italics added)

He took care to involve the US funding agencies right from the start—a wise move, given their crucial role as René Descartes's wealthy philanthropists enabling research to be done (2.ii.b–c). Soon, the new label gained international respectability, when the editor of *Nature* invited him to write a piece explaining just what "neuroscience" was (Schmitt 1992: 10).

The date of this multidisciplinary foundation was perhaps no accident, being determined not only on intellectual grounds (e.g. the recent advances in biochemistry and molecular genetics) but also by sociological factors. We saw in Chapter 1.iii.c–d that the general cultural challenge to formalist modernism in the 1960s involved an increased respect for concrete, situated, studies, including what had previously been sidelined as mere "applied" sciences (Toulmin 1999: 165). So perhaps Schmitt had sensed that the "pure" scientists were now more willing to listen to what the medics had to say, and to think of *their own* problems in practical, clinically oriented, terms.

The Society for Neuroscience was founded a few years later, when (according to its web site) "neuroscience barely existed as a separate discipline". Today, the SFN still makes a point of describing itself as a group of "basic scientists *and physicians*". Now, SFN has over 36,000 members. But when it began in 1970 there were only 200. And it was almost ten years after that before cognitive psychologists, as opposed to brain scientists, started to take *clinical* data really seriously (see Section xi.b, below).

Here as elsewhere, McCulloch had been an ambiguous figure. On the one hand, he was (for cognitive science) *the* seminal formalist. On the other hand, he'd always valued practical/therapeutic relevance as much as rigour, and many-methods rather than just one (see Chapter 4.ii–v). Some of the more orthodox, more modernist, scientists had been wary of him accordingly.

Perhaps encouraged by *Nature*'s interest, the new word caught on. The journal *Neuroscience* was founded in the mid-1970s. Twenty years later Sutherland (1995), acerbic as ever, defined the term as "a fashionable catch-all phrase that includes all disciplines that directly study the nervous system . . . and all that attempt to relate behaviour to the nervous system (e.g. physiological psychology)".

He explicitly contrasted it with cognitive science: "an equally fashionable term, comprising a set of disciplines that barely overlap with neuroscience". But this was misleading. The overlap, though admittedly limited (and viewed with suspicion by some: see Section viii.d, below), was by then significant. Indeed, it had been growing steadily for fifty years.

b. The computational species

Computational neuroscience, focusing on information processing, was pioneered at mid-century, as we'll see. But the name dates only from the early to mid-1980s.

Its first high-visibility usage was in a paper in *Science*, written by three leading cognitive scientists—drawn from neuroscience, connectionism, and philosophy (Sejnowski *et al.* 1988). They used it to cover any research on the brain and nervous system which asked computational questions and/or used computational modelling.

For example, it would cover research that asked *just how* cells in a monkey's temporal cortex could detect—i.e. compute—another monkey's face looking in a particular direction (see Section iv.d, below). Merely finding those cells (by experiments with micro-electrodes), without ever considering such questions, would be interesting. But, as Mayhew would be quick to point out, it wouldn't count as *computational* neuroscience.

But there are stricter definitions. Three years earlier, in 1985, Eric Schwartz wrote to a number of authors inviting them "to contribute to a book whose purpose was to define the term *computational neuroscience*" (E. L. Schwartz 1990, p. ix). The result was a 1987 symposium in California, followed by the promised book in 1990.

In explaining why he'd bothered to do this, Schwartz reported "violent turbulence over the past four decades" in the fortunes, and even the name, of the area he'd been working in. All the names he listed (*cybernetics*, *neural networks*, *brain theory*, *artificial intelligence*, *neural modelling*), he said, had been "abused, and none felt untainted for the present purpose" (p. ix). His preference, *computational neuroscience*, was described as "that area of overlap between neuroscience and computer science which required sufficient specialized expertise to justify a new subdiscipline". More particularly:

> computational neuroscience, then, is the problem area in which *difficult algorithmic or implementational questions are intimately related to the data of the nervous system*. The interplay of neural data and of computation and applied mathematics define the scope of this term. (E. L. Schwartz 1990, p. x)

Since Schwartz's definition specifically mentions difficult applied mathematics, it doesn't cover all of the work discussed in this chapter. Of course, what's difficult for one person may not be difficult for another. Nevertheless, the seminal research described in Sections iii–v below, even including Marr's early models of the cerebellum and hippocampus, doesn't fall under Schwartz's rubric. Nor does the early work in computational neuro-ethology (Section vii).

To refuse to regard any of that research as computational neuroscience, however, would be to lose sight of two key points. First, it would jettison Mayhew's important distinction between what-questions and how-questions. Second, it would obscure the historical connections between the relatively straightforward discussions of the 1950s–1960s and the (often fearsome) mathematics of neuroscientific research in the 1980s–1990s.

Accordingly, I'll rely on the more catholic definition given above. What's more, I'll interpret it so as to include approaches inspired by cybernetics and/or information theory (cf. 1.ii.a).

This chapter, then, narrates how purely biological neurophysiology and neuro-anatomy were joined by computational neuroscience. The plot of the story is concerned less with the specific discoveries that were made, fascinating though these are, than with the changes in *the sorts of question being asked*. Over the past sixty years, the central nervous system came to be appreciated not only as a biological organ but also as an informational/computational machine. Besides enriching neuroscience as such, this change made possible its growing rapprochement with psychology. Without it, neurophysiology would have remained outside cognitive science.

14.ii. Very Non-Neural Nets

The computational properties of neurones and cell assemblies were studied in very general terms by the connectionist AI/psychology described in Chapter 12. From the neuroscientist's point of view, that research had four major deficiencies: too neat, too simple, too few—and too dry.

a. Too neat

Connectionist AI tried to be neat, whereas the brain is likely to be messy. Connectionist learning rules, as we saw in Chapter 12, were valued for their increasing mathematical power and elegance. But this is largely irrelevant, from the neuroscientist's perspective.

Francis Crick (1916–2004) put it like this:

> [I] suspect that within most [connectionist] modellers a frustrated mathematician is trying to unfold his wings. It is not enough to make something that works. How much better if it can be shown to embody some powerful general principle for handling information, expressible in a deep mathematical form, if only to give an air of intellectual responsibility to an otherwise rather low-brow enterprise? (Crick 1989*a*: 132)

All very well, he continued, but evolution is a tinkerer, employing a ragbag of "slick tricks" rather than purist mathematical principles. And those tricks are what biologists want to identify:

> Why not look inside the brain, both to get new ideas and to test existing ones? The usual answer given by psychologists is that the details of the brain are so horrendously complicated that no

good will come of cramming one's head with that sort of information. To which the obvious reply is, "If it's as complicated as that, how do you hope to unscramble its workings by a purely black-box approach, by merely looking at its inputs and outputs?" (Crick 1989*a*: 132)

Crick himself was oversimplifying. Many neuroscientists value the mathematical elegance of computer models, if only in providing 'existence' proofs. Sometimes, they show that something broadly similar to a real neural network, *but even simpler*, can do very surprising things. For instance, an utterly random network—which the embryo brain is not—can self-organize into structures very like those found in real brains (see Sections vi.b, ix.a, and ix.c).

One can't deny, however, that over-fierce mathematics may put neuroscientists off. The history of the field would have been rather different if that weren't so (see Sections v.d, vi.a, viii.b, and ix.b). Nor can one deny that the more 'lifelike' a neural model gets, the less formally intelligible it may be. Large 'evolutionary' networks, for example, may embody clear rules (even Type-I theories *à la* Marr: see Chapter 7.iii.b), but their final state may be nearer to a Type-II theory—which is almost to say, no *theory* at all (see Section ix.d).

But Crick was right about the slick tricks. Various ecologically relevant feature-detectors found in crabs, frogs, and mammals are mentioned in Section iv, and other special-purpose mechanisms (in frogs and insects) are described in Section vii and Chapter 15.vii. Significantly, Walter Pitts—a mathematician—was *disappointed*, even *a little appalled*, by his seminal co-discovery of bug-detectors, because they weren't formally tractable (see Section iv.a).

b. Too simple

The second deficiency of connectionism was its unrealistic 'neurophysiology'. A connectionist unit—of whatever type—is a mythical beast, as elusive in the biological world as the gryphon. The computational power of a real neurone is closer to that of a complex artificial network, or even a small computer, than to a single network-element.

Real neurones aren't (or aren't just) components of logic gates: one cell may have thousands of possible outputs. As Crick (1989a) has remarked, there may be many glutamate receptors on a single neurone, whose number and locations are surely relevant but which are simply ignored by connectionists. And it was discovered in the mid-1980s that dendrites, of which a neurone can have many thousands, aren't passive conduits but dynamical transmitters (with varying conductance). It's even been suggested, on the basis of a simulation (McKenna 1994: 83), that *an individual dendrite* may be able to compute exclusive-or—which Frank Rosenblatt's perceptron couldn't do (Chapter 12.iii.b).

What counts as a 'plausible' model, of course, changes as our knowledge grows. Implausibility can vanish overnight. For instance, multiplication rules were included in artificial networks (12.v.e) to provide mathematical neatness, certainly not biological credibility. But in the mid-1990s, "multiplication synapses" were discovered in the brain (Elman *et al.* 1996: 105).

In addition, *time* matters in real brains. For example, there's a wide range of 'windows' (measured in milliseconds) during which two inputs can have a joint effect; and spiking frequencies may be more relevant than single spikes. Yet connectionist networks are typically time-less. (Some exceptions are discussed in Section ix.g.)

Furthermore, the most widely used connectionist learning rule—back propagation—is unrealistic. Synapses don't transmit in two directions.

c. Too few

The third drawback of connectionist AI concerned its less than generous 'anatomy'. Judged by the numbers of units, artificial neural networks were tiny by biological standards.

True, some biological species have only 302 neurones (see v.b, below). And some have specific networks with only thirty neurones or fewer, whose connections and patterns of excitability can be individually modelled. For instance, there are alternative simulations of the network that enables a sea slug (*Aplysia*) to swim away from danger (Getting 1989; Kleinfeld and Sompolinsky 1989).

But even insect brains are enormously larger than that (M. O'Shea, personal communication). The bee brain is estimated to have about 1 million neurones (plus perhaps 100 million glial cells). And some molluscs, namely octopodes, have insect-like numbers of neurones: hundreds of thousands, if not millions. This is due to their very advanced visual systems: lower molluscs, such as sea slugs, have only about 20,000–30,000. As for the human cerebral cortex, this has about 20,000 million neurones (i.e. 20 US billions), and the number of dendrites and synapses is orders of magnitude greater. The largest of today's artificial networks is puny, indeed microscopic, by comparison.

Admittedly, network models soon started growing. In the late 1960s, for example, a Hungarian researcher simulating the cerebellum achieved a huge jump in scale: from a network of 1,296 units to one of over 64,000 (see Section viii.b). By the mid-1970s, he was coping with more than 1,700,000 components. But even that number, impressive though it is, was niggardly alongside the cerebellum itself.

Moreover, these numbers *matter*. It's not good enough to say there are "many" neurones, each one with "many" synapses. To understand the functioning of a particular part of the brain, one often needs to know *just how many* (see Section v.c).

Even more to the point, the brain isn't a formless mass of identical cells. So building a super-duper-computer that matched the huge *numbers* wouldn't help much. The networks described in Chapter 12 were very rarely assigned to specific parts of the brain. The assumption seemed to be that a neurone is a neurone, is a neurone . . . But there are many (perhaps twenty-five) distinct types of neurone, with different locations and physiological—and computational—properties.

(The even more numerous glial cells, which outnumber the neurones by about 100:1, seem to have mainly structural/guiding and nutritive functions, not informational ones. For instance, some of them form the myelin that coats mature axons. However, there's recent evidence that they can modulate the responses of the neurones, so in that sense they play a computational role: M. O'Shea, personal communication. For more on neuromodulation, see Section ix.f, below.)

This anatomical diversity of the neuronal cells was already being noted at the beginning of the century (2.viii.c). Santiago Ramón y Cajal identified five types of cerebellar neurone (the Purkinje, basket, stellate, Golgi, and granule cells), and distinguished some of their interconnections (the mossy, climbing, and parallel fibres)—see Figures 2.1–2.3. That was the beginning of the anatomical work used to ground the modelling of the cerebellum seventy years later (Section v.c, below). Ramón y Cajal also showed that the retina contains several types of unit: rods, cones, bipolars, horizontals, amacrines, and ganglion cells (whose axons form the optic nerve).

Later, John Eccles and others combined microscopy with single-unit recording, making the anatomical complexity even clearer (2.viii.e). Under the microscope, the cortex has six layers. And single-cell recordings showed functional distinctions, too. Visual cortex, for instance, contains over fifty distinct areas or neurone groups, simultaneously processing many different features, such as colour, lightness, texture, depth, and shape (Zeki 1993). Today's functional map of the striate cortex is more complex than that of the London Underground.

Most AI connectionists ignored such facts. Even Carver Mead's VLSI chips (12.v.f) only *approximated* the real retina, cochlea, and cortex. So when connectionists boasted that their models were more biologically plausible than GOFAI, they should rather have said they were *less implausible*.

To be fair, some did. Warren McCulloch and Pitts themselves, in 1943, had admitted that their formal units and "temporal" expressions, or TPEs, were idealizations (4.iii.e). This was confirmed in the 1950s by work on stochastic firing, grounded in continuous synaptic variation that's independent of the input from other neurones. By the 1980s, the idealization had become even more evident (Perkel 1988). Indeed, the PDP bible included a chapter on how PDP units differ from real neurones (Crick and Asanuma 1986).

But most of the connectionists featured in Chapter 12 paid little or no attention to the brain *as such*. Here, we'll consider research using computational theories and/or computer modelling *to cast light on the nervous system itself*.

d. Too dry

Computational neuroscience aims to match the details of the software to specific aspects of the biological hardware—or, as it's sometimes called, the wetware. And "wetware" is often interpreted as *wireware*. Certainly, the computational neuroscientists discussed in this chapter concentrated on the detailed anatomical connections within the nervous system. They weren't concerned with chemical/biophysical properties. The "wetness" of real neurones was ignored: linkage was all. In that sense, they're 'connectionists' too.

Strictly, however, wetware includes more than wireware. For neuroscientists study not only the networks of neurones but also what they are made of, how they actually work. The Hodgkin–Huxley equations for the nervous impulse were an early example (2.viii.e).

Accordingly, some computational neuroscience looks *below* the neuronal level. (For some examples, see C. Koch and Segev 1989, chs. 2–5; Feng 2004, chs. 2–7.) This is a relatively recent development: wireware models came first, as we'll see.

By the 1990s, however, a few researchers were simulating the computational functions of membranes and dendritic spines, and the action of the calcium ion and neurotransmitters (e.g. C. Koch 1990, 1999; Gazzaniga 1995, pt. i). A specific Hebbian learning rule was modelled in terms of biophysically realistic data on the strength, and temporal properties, of synaptic currents and membrane potentials (Rao and Sejnowski 2001). And some computer models included the effects of neuromodulators, which can temporarily change a neurone's properties (Philippides *et al.* 1998).

Two interesting wetware examples are described in Section ix.f, below. One concerns the computer simulation of neurochemical modulation; the other seeks to produce non-linear computation by harnessing real chemicals and/or neurones to electronic circuits. With those exceptions, I'll consider only models of the wireware: theories at the level of neurones or large neurone groups. This is partly because wetware models are state of the art rather than historical, and partly because they are less relevant for cognitive science in general than wireware models are.

I'll also ignore 'neuro-informatics'. This deals with the wireware too. It applies computerized statistical analysis to huge data-banks of results, so as to discover possible structure–function relations in the brain. It's increasingly necessary, given the flood of neurological data. Over 14,000 reports of connections in the rat's brain, in over 900 separate papers, appeared between 1980 and 2000, since when there will have been many more (M. P. Young 2000: 3, 56). And then there's cats, and monkeys . . . Neuro-informatics may help inspire computational theories, by leading people to ask what the anatomical data *mean* in functional terms. But it's still in its infancy. The theories I'll discuss here weren't based on statistical data analysis, but on individual experiments and observations.

An objection often heard from the more traditional neuroscientists is that computer models are (to put it politely) of dubious value, because they hugely oversimplify the neurophysiology *even if* they attempt to take it on board. Indeed, we'll see that it took many years before such models were seriously regarded by professional brain scientists (v.d and vi.c, below). However, two leading computationalists, namely Christoph Koch and Idan Segev, have given this scepticism a suitable reply:

> One frequently hears the criticism that simplifying brain models lack this or the other physiological feature and are thus unbiological and irrelevant for understanding the nervous system. . . . This argument, taken to its extreme, implies that we will not be able to understand the brain until we have simulated it at the detailed biophysical level! . . . [But] such a simulation with a vast number of parameters will be as poorly understood as the brain itself. Furthermore, if modeling is a strategy developed precisely because the brain's complexity is an obstacle to understanding the principles governing brain function, then an argument insisting on complete biological fidelity is self-defeating. (C. Koch and Segev 1989: 2)

It's only recently that books have started to appear which try to take both low-level neurophysiology and high-level psychology seriously. Among the first, in the 1980s, were two by Stephen Grossberg, whose pioneering work is described in Section v. And in the 1990s, Arnold Trehub of UMass Amherst published a monograph describing computational theories (and models) of *planning* and *narrative comprehension* (Trehub 1991).

Trehub's theories of these higher mental processes are based on Hebbian learning constrained by detailed neural structures, and even by neurochemicals. The biophysical facts involved concern the mechanisms of long-term/short-term memory (e.g. ATF and DTF: axon and dendrite transfer factors), and the key neural structures are the "synaptic matrix" and the "retinoid". Trehub's claim is that these wetware matters mustn't be ignored, because they enable us to rule out a wide range of alternative cognitive hypotheses.

As for textbooks integrating connectionist modelling with real neuroscience, the first example came off the press at the turn of the century. Randall O'Reilly and Yuko Munakata, both ex-students of James McClelland (and now based at the University of Colorado), hoped to follow in their tutor's footsteps in more ways than one. For in their Preface, they said:

> Our objective in writing this text was to replicate the scope (and excitement) of the original PDP volumes in a more modern, integrated, and unified manner that more tightly related biology and cognition . . . (O'Reilly and Munakata 2000, p. xxv)

They didn't entirely succeed, for the "excitement" that had been aroused by the original PDP volumes had been so great that probably no one could have matched it (see 12.vi.a). For one thing, the new textbook wasn't aimed at the still-uncommitted student, as the PDP bible was. (Still less would it enthuse the journalists and the general public, as its predecessor did.) For another, O'Reilly and Munakata were doing something which very obviously needed to be done, whereas the PDP volumes had struck most readers as a bolt from the blue.

Nevertheless, their book was important, for it did—as they hoped—relate biology and cognition much more tightly together. O'Reilly and Munakata, and many of the computational researchers discussed by them, showed a healthy respect for the wetware. Neuroscientific findings, which in any case had ballooned since the mid-1980s, were taken on board wherever possible.

Their hope of replicating the "scope" of the PDP books was also fulfilled. For their discussions (and models) ranged psychologically from language, memory, and attention to specific learning rules, and biologically from large-scale brain anatomy to axons, ion channels, and membrane potentials. In short, their volume was not merely a textbook, but an interdisciplinary tour de force.

One way in which their text surpassed the first edition of the PDP equivalent was that they made available (on the Web) a powerful software tool with a detailed manual, and gave tutorial appendices in the printed text. In addition, they discussed many examples of psychological simulations developed by using this system.

They called it "Leabra": Local, Error-driven and Associative, Biologically Realistic Algorithm. The phrase "Error-driven and Associative" was code for the fact that Leabra combined both reinforcement and Hebbian "ft/wt" learning.

Like Allen Newell's SOAR and John R. Anderson's ACT* (see 7.iv.b–c), this was intended as a "unified" model of cognition. They shared Newell's view that a coherent unified model is much more difficult to produce, and therefore much more informative, than specialized theories of specific phenomena (p. 11). All areas of experimental/clinical psychology were supposed to be covered, in an integrated fashion.

Leabra drew on core theoretical insights from connectionist AI. But relevant aspects of neuroscience were included too—and given greater prominence. For example, the activation function controlling the spiking of the simulated neurones was only "occasionally" drawn from mathematical connectionism (p. 42). Usually, it was based on facts about the biological machinery for producing a spike (the sodium pump: see 2.viii.e). These included detailed data on ion channels, membrane potentials, conductance, leakages, and other electrical properties of nerve cells (pp. 32–48).

One might think that *from the psychologist's point of view* this was over-egging the pudding: surely such details can't be relevant for understanding high-level phenomena such as reading, word meaning, or memory?

O'Reilly and Munakata readily admitted that "higher levels of analysis . . . provide a more useful language for describing cognition" (p. 40). They pointed out, however, that the basic equations governing the activity of Leabra when it was simulating reading, or memory, or . . . weren't (except occasionally) picked out of an abstract connectionist mathematics. Rather, they were painstakingly drawn from detailed biophysical data. This was true, for instance, of the equation used in Leabra for integrating all the inputs into a neurone (see their explanation of equation 2.8 on pp. 37 ff.).

They drew the line at applying this equation "at every point along the dendrites and cell body of the neuron, along with additional equations that specify how the membrane potential spreads along neighbouring points of the neuron" (p. 38). They had no wish "to implement hundreds or thousands of equations to implement a single neuron", so used an approximating equation instead. But, characteristically, they provided references to other books which did explain how to implement such detailed single-neurone simulations (including the delightfully named *Book of GENESIS*: J. M. Bower and Beeman 1994).

In general, the psychological models developed by O'Reilly and Munakata would have been different had the neuroscientific data been different. Their discussion of dyslexia, for instance, built not only on previous connectionist work (e.g. Plaut and Shallice 1993; Plaut *et al.* 1996: see Chapter 12.ix.b), but also on recent clinical and neurological information (2000: 331–41).

As our knowledge of the brain advances (thanks to brain scanning, for example), future psychological models—they believe—will, or anyway should, be different again. They see their book as "a 'first draft' of a coherent framework for computational cognitive neuroscience" (p. 11). In short, the functionalist philosophers' "multiple realizability" is now under strong attack (16.iii). (It's under attack not only in 'human' neuroscience, but in neuro-ethology, too; indeed, the electric fish studied long ago by Lord Cavendish have prompted some strong arguments against it: Keeley 2000*a*.)

Even so, the computational level of theorizing is still crucial. Future researchers who follow O'Reilly and Munakata's lead—or who use Michael Arbib's NSL language to model macroscopic brain structures (see Section vii.c, below)—will *not* be fulfilling Wittgensteinian philosophers' hopes for "the disappearance of psychology as a discipline distinct from neurology" (Rorty 1979: 121). Or rather, they'll be doing so only if one interprets that word "distinct" to mean something like "paying no attention whatsoever to . . .". Indeed, the main theme of this chapter is that neurology itself has become more computational, more psychological.

This fact, combined with the demise of multiple realizability in its strong form, makes the demarcation between "psychology" and "neurology" more fuzzy than it used to be. But that's not to report, nor even to predict, the "disappearance" of psychology.

14.iii. In the Beginning

Detailed computational questions first arose in neurophysiology at around mid-century. McCulloch and Pitts, and Kenneth Craik too, had considered such questions in general terms in the 1940s. What was different in the early 1950s was that some people started using ideas from information theory and computing to help them study the neurophysiological nitty-gritty.

a. Computational questions

Even in the late 1940s and 1950s, when 'neuroscience' was still unheard of, people had already started to ask computational questions about specific aspects of the nervous system.

Their answers were rarely put in terms of implemented models, because computers were still primitive. But the notion that the brain (with the sense organs) is an information-processing machine, whether digital or analogue, was being taken seriously by a diverse group of people:

* by neurophysiologists such as McCulloch, Barlow, and Jerome Lettvin;
* by cyberneticians such as William Ross Ashby;
* by physiological psychologists such as Uttley;
* by psychologists with a strong engineering bent, such as Craik and Richard Gregory;
* and by computer scientists such as Turing and Oliver Selfridge.

Moreover, these people were now talking to each other. This was new. Before the Second World War, and excepting the leaders of the cybernetics community discussed in Chapter 4 (including Nicholas Rashevsky's group for mathematical biophysics), there hadn't been much interchange between psychologists and physiologists.

Admittedly, Charles Sherrington himself had joined the British Psychological Society on its foundation, and contributed a paper (on binocular flicker) to the first issue of its journal in 1904. But this was more an expression of hope and encouragement than a mark of central research activity. Behaviourism treated the brain as a black box, and Gestalt psychologists had little constructive to say about it. Freudians and Piagetians, even less. Only William McDougall had made a concerted effort to bring the two disciplines together, and he didn't get very far.

What brought brain and behaviour—and computers—closer together was the challenge set by the war (see 4.vi–viii and 11.i). Craik and Frederic Bartlett, for example, were closely involved (even before war was officially declared) in applying psychology to various tasks undertaken by servicemen. And most members of the interdisciplinary Ratio Club of the 1950s (4.viii and 12.ii.c) had been involved in the war effort as very young men, cooperating across disciplines to study not only radar, sonar, and radio, but also a wide range of machines *and* their human operators.

Turing's war work had nothing to do with physiology, or psychology either (3.v.d). But we saw in Chapter 12.i.b that he was thinking about neural networks by the mid-1940s and gave a brief description of "cortex" in his NPL report of 1947. However, his account of the brain, and of the "pleasure–pain systems" that might "organize" the cortex, was brief—and very general:

Many parts of a man's brain are definite nerve circuits required for quite definite purposes. Examples of these are the "centres" which control respiration, sneezing, following moving objects with the eyes, etc.: all the reflexes proper (not "conditioned") are due to the activities of these definite structures in the brain. Likewise the apparatus for the more elementary analysis of shapes and sounds probably comes into this category. But the more intellectual activities of the brain [such as speaking English or French] are too varied to be managed on this basis . . . We believe then that there are large parts of the brain, chiefly in the cortex, whose function is largely indeterminate. (A. M. Turing 1947*b*: 16)

Turing's notional neuroscience had little or no influence. The 1947 report remained unpublished until 1969, and Turing himself soon turned from cortex to embryology (Chapter 15.iv). When invited to give a talk to the Ratio Club he spoke first on AI and later on morphogenesis, not on brains (H. B. Barlow, personal communication).

While Turing was writing his NPL report, Pitts and McCulloch (1947) were thinking about the brain in much more detail. Their earlier, 'logical', paper of 1943 is accepted by neuroscientists today as "one of the great boosters of modern brain science" (Braitenberg 1984: 108). It had been based on the recent discoveries about synaptic activity, which would soon be confirmed by work on cell membranes and neurotransmitters (Hodgkin and Huxley 1952; Eccles 1953, 1964).

But it had ignored both the 'noise' in the nervous system and its neuro-anatomy. The computational/statistical aspects of their 1947 theory dealt with the noise, and were outlined in Chapter 12.i.b. Here, our interest is in what they said about the anatomy.

The first point to note is that they said anything about it at all. Their opening sentences put the goal and justification of computational neuroscience in a nutshell:

To demonstrate existential consequences of known characters of neurons, any theoretically conceivable net embodying the possibility will serve. It is equally legitimate to have every net accompanied by anatomical directions as to where to record the action of its supposed components, for experiment will serve to eliminate those which do not fit the facts. (Pitts and McCulloch 1947: 46)

In other words, abstract networks such as those discussed in Chapter 12 are worth discussing, but one can also try to map them onto real brains—in which case one is generating empirical hypotheses that can help brain science to advance.

Ashby, to be sure, had said the same sort of thing (4.vii.c–d). But Pitts and McCulloch were more specific than Ashby in their suggestions about *just which* cells might be doing *just what*. In their words: "We endeavor particularly to find those [net designs] which fit the histology and physiology of the actual structure" (p. 47).

They dismissed Gestalt theories of brain–world isomorphism, seeing them as hand-waving. The Gestaltists relied on some mysterious pre-established harmony (which they *did not* explain in evolutionary terms), and didn't seriously consider how the brain can identify and represent percepts/concepts. That was what Pitts and McCulloch wanted to explain.

They rejected what was later termed the grandmother cell theory, because (for example) a square could conceivably be represented by spatio-temporal averaging over a "mosaic" of cells:

[For this reason, we disagree with] the neurologists of the school of Hughlings Jackson, who must have it fed to some specialized neuron whose business is, say, the reading of squares. That language in which information is communicated to the homunculus who sits always beyond any incomplete analysis of sensory mechanisms and before any analysis of motor ones neither needs to be nor is apt to be built on the plan of those languages men use toward one another. (p. 56)

In short, *one concept*, *one cell* is both unnecessary and implausible (see Section ix.e).

Their hypothetical representation of a square, they said, might be thought to arise in a brain area one level up from Area 17. But in fact, it didn't:

A square in the visual field, as it moved in and out in successive constrictions and dilatations in Area 17, would trace out four spokes radiating from a common center *upon the recipient mosaic*. This four-spoked form, not at all like a square, would then be the size-invariant figure of square. *In fact, Area 18 does not act like this*, for during stimulation of a single spot in the parastriate cortex, human patients report perceiving complete and well-defined objects, but without definite size or position, much as in ordinary visual mental imagery. *This is why we have situated the [computational] mechanism . . . in Area 17, instead of later in the visual system*. (pp. 55–6; italics added)

Other neural hypotheses attributed temporal "scanning"—supposedly involving the alpha waves—to a specific brain area (the stripe of Gennari), and even to particular types of neurone within that layer. Similarly, they described the gaze reflex as a "servo-mechanism" (*sic*) wherein the superior colliculus "computes by double integration the lateral and vertical coordinates of the 'center of gravity of the distribution of brightness' ", and sends impulses to the eye muscles so that the eyes turn towards the centre of gravity, gradually slow down, and stop when it's fixated. Claiming "considerable support for this conjecture in the profuse anatomical and physiological literature", they related the computations supposedly involved (expressed as mathematical equations) to particular laminae within the colliculus.

The issue, here, is not whether their suggestions were correct. Indeed, their more specific hypotheses were highly provisional: "It is evident that many details of [the] hypothetical nets of this paper might be chosen in several ways with equal reason; we have only taken the most likely in the light of present knowledge" (p. 54). But the general potential was clear:

We have focussed our attention on particular hypothetical mechanisms in order to reach explicit notions about them which guide *both histological studies and experiment*. If mistaken, they still present the possible kinds of hypothetical mechanisms and the general character of circuits which recognize universals, and give practical methods for their design. (p. 65; italics added)

And, quite apart from the contingent neuro-anatomy, they claimed to have established a mathematical principle covering the design of neural networks in general:

These procedures are a systematic development of the conception of reverberating neuronal chains, which themselves, in preserving the sequence of events while forgetting their time of happening, are abstracted universals of a kind. Our circuits extend the abstraction to a wide range of properties. By systematic use of the principle of the exchangeability of time and space [see Chapter 12.i.c], we have enlarged the realm enormously. (p. 65)

By implication, biologists would do well to remember that natural selection might have discovered this principle too, exploiting it in various ways within real nervous systems.

As for whether 'the facts' confirmed Pitts and McCulloch's theories, the jury is still out. Intracerebral EEG scanning is no longer accepted, but some of the processes they ascribed to visual cortex may actually go on there.

(In the Introduction to his *Cybernetics*, Norbert Wiener reported, with no little satisfaction, that the leading neurophysiologist Gerhardt von Bonin, on catching sight of the Pitts–McCulloch paper lying on his desk, had immediately asked him: "Is this a diagram of the fourth layer of the visual cortex of the brain?"—Wiener 1948: 22. In truth, it was a diagram of the abstract architecture of their computational theory, not of the brain as such. But the similarities were hardly surprising, and Wiener's satisfaction scantly justified, since their theory had been inspired by the neuro-anatomy in the first place.)

Current biologically informed models of "how we see universals" are hugely more subtle and complex (see Sections vi–vii, below). Nevertheless, Pitts and McCulloch had made the first serious attempt to locate specific computational functions in particular cell groups in the brain. As Christopher Longuet-Higgins said in a different context (12.v.c), "a demonstration of how something could possibly happen . . . may be almost as welcome as the discovery of how it actually does happen".

McCulloch, neuroscientist though he was, would have agreed. According to his close colleague Lettvin, he himself had little desire to test his new theories by the traditional methods. To be sure, he and Lettvin would co-author an experimental report ten years later that ruled out one of the mechanisms he'd posited in 1947 (Lettvin *et al.* 1959: 253). But McCulloch's general attitude to experimental neuroscience had turned to disappointment. He now preferred an unconfirmed but mathematically coherent theory to a ragbag of empirical facts:

> [By the mid-1950s] he was dedicated to knowing how the brain works in the way that the creator of any machine knows its workings. The key to such knowledge is not to analyze observation but to create a model and then compare it with observation by mapping. But the poiesis must come first, and McCulloch would rather have failed in trying to create a brain than to have succeeded in describing an existing one more fully. (J. Y. Lettvin, in Dupuy 2000: 137)

Another person newly critical of the traditional methods of neuroscience was Gregory, who'd worked on the development of radar in the war. At Uttley's NPL meeting on 'The Mechanization of Thought Processes', and in a discussion in Cambridge a few years later, he caused no little consternation by giving an engineer's perspective on brain ablation studies (Gregory 1959, 1961).

His critique was based in his understanding of complex systems—such as a radio set:

> Suppose that when [a] condenser breaks down, the set emits howls. Do we argue that the normal function of the condenser is to inhibit howling? Surely not. The condenser's abnormally low resistance has changed the system as a whole, and the system may exhibit new behaviour, in this case howling. (Gregory 1959: 678)

What's needed instead is a theoretical model of how the brain is working (and what it's doing), in terms of which the data can be interpreted. This applied also, he said, to experiments based on electrical stimulation; but ablation studies had led to especially shaky inferences.

Gregory was—and still is—sometimes accused of having "made clear [that] neuroanatomical investigations would have to be abandoned in favour of the electrical models pioneered by Craik and Ashby" (Hayward 2001*b*: 304). This is nonsense. He didn't rule out neuro-anatomical investigations, nor even ablation experiments. Rather, he used a cybernetic argument to warn against simplistic interpretations of them.

Even in the twenty-first century, such warnings are sometimes necessary. Several have been issued recently, applying to "double dissociations" in clinical cases, to animal experiments, and to studies of deficits in vision, face recognition, and dyslexia (M. P. Young *et al.* 2000; Shallice 1988*a*, chs. 10–11; McClelland 2000, pp. xxi–xxii). McClelland, for instance, puts it like this:

> [An] explicit computational perspective often leads to new ways of understanding observed [neuropsychological] phenomena that are apparently not always accessible to *those who seek to identify subsystems without giving detailed consideration to the mechanisms involved*. . . . [Several examples illustrate that] the inference from data to the modular architecture of the mind is not at all straightforward, and that explicit computational models can provide alternatives to what in some cases appears to be *a fairly simplistic reification of task or item differences into cognitive modules*, and in other cases manifests as *a reification of types of errors (semantic, visual) into lesion [sites]*. (McClelland 2000, pp. xxi, xxii; italics added)

So Gregory shouldn't be accused either of saying that brain ablation is a waste of time, or of issuing unnecessary warnings about reification. As a historical point, however, his strictures may have helped fuel the resentment of those brain scientists who later criticized the hype attending AI models in general, and simplistic 'brainlike' networks in particular (see Section vi.c).

b. Computations in the brain

By the early 1950s, and partly because of McCulloch and Pitts' two papers, more neurophysiologists were thinking about neural functions in informational/computational terms. One of these was Uttley (who *did* combine them with traditional experimental methods, as we'll see).

As explained in Chapter 12.ii.c, he was already using Claude Shannon's theory of information to describe neuronal function, and to design computer models. His "informon theory", developed through the 1960s and 1970s, represented synapses as calculating and communicating information—and also made various suggestions about the neurophysiology involved (Uttley 1975, 1979).

Uttley defined synaptic conductivity in terms of average impulse (spiking) frequencies, changes in which were the physical basis of learning. And he argued that synapses must contain three distinct counters, probably implemented as three neurochemicals of varying concentrations, in order to estimate the probability of co-occurrence of pre- and post-synaptic impulses. It is because these counters have to sample over time, and are imperfect ("leaky") anyway, that learning takes many minutes or even hours. (Long-term memory, he suggested, requires a second mechanism, involving some near-irreversible chemical process that fixes the short-term memory.)

He argued, too, that negative feedback from inhibitory neurones is needed to stabilize conductivities. Synapses are essentially variable, nevertheless, so learning is complemented by forgetting (extinction):

> The reinforcing signal to an informon corresponds to the unconditioned stimulus which reinforces a conditioned stimulus in animal learning experiments; without such a reinforcing signal synaptic conductivity decays; correspondingly an animal forgets what it has learned. (Uttley 1982: 30)

Remembering, on this theory, isn't threatened only by confusion, where one and the same neurone group is asked to store too many different representations (cf. Chapter 12.v.c–f). Even a single memory would decay in the absence of continual reinforcement.

Uttley actually designed a dynamic computer memory based on this principle. A photocell looked at a cathode-ray tube screen displaying the output, and its signal went through a mercury delay line to the CRT input. This feedback loop continually refreshed the CRT image, maintaining the dots on the screen (R. L. Gregory, personal communication).

He even tried to provide new data, collaborating on various experimental studies of synaptic conductivity. Some of these were done with Benedict Delisle Burns, who'd argued (in the 1950s) that some synapses must have more complex properties than current models of learning allowed (Delisle Burns 1958: 96 ff.). Part of Delisle Burns's reasoning was based on the recent discovery of noise in the nervous system, which moved neurophysiology from deterministic ('telephone exchange') to stochastic theories (see 2.viii.f).

Uttley's work reflected this new approach. He tried to explain why (as he put it) "the brain ticks by itself", and to clarify the functional implications. And he offered accounts—and simulations—of the action of specific cells, such as the basket and granule cells of the hippocampus and the orientation detectors in visual cortex (discovered in the late 1950s: see Section iv.b). For instance, he modelled the newborn kitten's (fairly poor) 'innate' orientation detectors, and the learning processes that improve these networks as the kitten encounters straight lines and boundaries. (As for whether these detectors really were *innate*, see Section x.a and c, below.)

In short, Uttley made a serious attempt to take account of the neurophysiological data, in considering the different *types* of synapse that might exist in the brain. His early work was described as "an invaluable contribution to the study of learning in animals" (Delisle Burns 1968: 151).

One young neurophysiologist strongly influenced in the 1950s by Uttley's use of information theory was his fellow Ratio member Barlow (1921–), a great-grandson of Charles Darwin. Indeed, Barlow now says, "I was very fortunate to have belonged to [the Ratio Club], for when I come to think about it I can see that much of what I've been interested in since has stemmed from those evenings" (personal communication). Others included Patrick Merton, also a Ratio member (see Chapter 12.ii.c), and Jack Cowan. Cowan went from Imperial College to spend a year with Uttley at NPL before moving to Chicago to direct Rashevsky's group (12.ii.b).

Barlow was then a researcher at the University of Cambridge (and a Royal Society Research Professor there, years later). He first outlined his hugely influential "coding" theory of perception at the same two meetings where Gregory had ridiculed howl-inhibitors (Barlow 1959, 1961).

He cited Craik (see Chapter 4.vi), saying that the brain must build a "model" of the external world, which can be used to guide appropriate behaviour (1959: 542). He wanted to show what such an internal model must be like, and how it might actually be built. So, borrowing Shannon's idea of optimal coding, he asked how a machine might be designed to reduce redundancy when recording sensory information. In other words, how can a machine—or a nervous system—find simplicity in complexity?

Low-level perceptual systems (such as the retina), said Barlow, appear to possess *built-in* mechanisms for doing this with respect to types of redundancy that are always present in the environment. But high-level perception—of one's grandmother, for instance (though Barlow didn't mention that example)—requires that the nervous system be able to *learn* to reduce the relevant redundancies. This, he argued, must involve changes in nerve signalling rather than neurone numbers:

> In the nervous system the number of nerve fibres available for a specific task must, to a large extent, be determined genetically. One may expect evolutionary adaptation to have performed part of the [communication] engineer's job in selecting codes for the sensory signals, but such inherited codes obviously cannot be adapted to the redundancy of sensory input which is peculiar to each individual. Now although the number of nerve cells available is probably determined genetically, the number of impulses in the nerve cells is not, and some of the advantages of optimal coding would apply if the incoming information were coded—not onto the smallest possible number of nerve fibres each working at its optimal mean frequency—but into the smallest possible number of impulses in a relatively fixed number of nerves. This type of coding can be epitomized as *economy of impulses*. (Barlow 1959: 550)

Barlow was more interested in how perceptual redundancies, or patterns, could be recognized than in how they might be learnt. So he offered no learning rules. Nor did he claim that optimal coding had been 'proved' to happen. But he did give neurophysiological evidence for it, including experiments on adaptation and lateral inhibition—in vision, hearing, and touch. These showed that spiking frequencies can depend on specific neuronal mechanisms—such as the ON–OFF/movement receptors he himself had recently found in the frog's retina (Barlow 1953).

(A few years later, he would discover *directional* detectors in the rabbit's retina—Barlow *et al.* 1964; Barlow and Levick 1965. Again, he asked just what information was being computed, and how. And he showed how inhibition of one cell by another functions in the discrimination of direction of motion.)

As for intelligence, a word that "was added to the title in an incautious moment", Barlow saw this as largely continuous with perception. Concepts are relatively high-level input redundancies, and reasoning is based in redundancies too:

> [When] one considers the two main operations required for optimal coding there is a striking parallel with the two types of reasoning which underlie intelligence.
>
> The outputs of a code can be thought of as logical statements about the input, and, if the code is reversible [in a sense explained in his paper], these logical statements, taken together, are sufficient to determine the exact input. Forming these statements and ensuring that they fulfil this condition are straightforward problems of deductive logic . . . [As for inductive reasoning, this depends] on counting frequencies of occurrence of events. Having been presented with 1000

white swans and no black ones, the relevant parts of a code would say "henceforth regard all swans as white unless told otherwise". Inductively one would say "All swans are white." *The tools of logical reasoning appear to be the same as those needed for optimal coding*, so perhaps they can be regarded as the verbal expression of rules for handling facts which our nervous system uses *constantly and automatically* to reduce the barrage of sensory impulses to useable proportions. (1959: 555; italics added)

The emphasis in Barlow's coding theory, as in Uttley's work, was on the brain's measurement and comparison of the *probabilities* of stimuli. This approach, Barlow has recently recalled, fell into the background for many years while people focused instead on how the brain generates *transformations* of physical stimuli (Barlow 2001*b*). But the original emphasis, he now thinks, was sound. Although earlier theories of perception were "wrong in over-emphasizing the role of compressive coding and economy in neuron numbers", they were "right in drawing attention to the importance of redundancy" (see Section ix.e, below).

In short, "Think probabilities":

In any [perceptual] representation probabilities are key elements, because they are the fuel for accurate decision making. *So the take-home message for the neuroscientist should be: "Think probabilities*: What probabilities are needed? How are they represented? How are they estimated? How are they modified? How are they transmitted to other places in the brain? And how are they combined for making the moderately rational decisions that we observe brains making[?]" (Barlow 2001*b*: 251; italics added)

(As we saw in Chapter 12.vi.f and ix.d, "thinking probabilities" is just what the mathematical connectionists had been doing.) Barlow still recognizes his fifty-year debt to Uttley, whose name is one of nine mentioned in the Acknowledgments of his 2001*b* paper.

c. Formal synapses

One of Barlow's colleagues at Cambridge—and at the Ratio Club—was the physiologist Giles Brindley, who worked there until moving to London in 1968. In the 1960s, Brindley developed a formal theory of "modifiable" synapses. And Mayhew would have been happy, for Brindley was asking *just how* learning can be achieved by the brain.

Specifically, he was asking—as Uttley (also a Ratio member) had done before him—(1) what different types of synapse might exist, and (2) how their computational properties compared.

Brindley started from Donald Hebb's (1949) hypothesis about learning (5.iv.b). But Hebb's ideas had been expressed verbally, not mathematically, and Brindley (1967) pointed out that they could cover many distinct types of synaptic change. For instance, the neurones involved might be excitatory or inhibitory, and the modification might affect the pre-synaptic and/or post-synaptic cells. So several "Hebbian" learning rules were conceivable. Indeed, four had been described already (though only one had been defined formally).

Brindley suggested that different types of observable behaviour—such as classical and operant conditioning—might involve different learning rules. And if forgetting was to be explained, then extinction rules were needed too. Rashevsky's journal had reported a model of Pavlovian conditioning as early as 1950, but it hadn't allowed for extinction.

By exploring the possible combinations, Brindley defined ten types of synaptic modification. He showed that their logical input–output relations fell into three classes: A, B, and C. (The crucial question was whether the set-theoretic intersection of the two inputs was equal to one of them, or zero, or neither.) Six of the ten synapses were class A, three B, and one C. And these classes were systematically related:

> [Any] two members of the same class can, with the aid of non-remembering elements that perform simple logical operations [i.e. McCulloch–Pitts neurones combined as *and*, *or*, and *and-not* logic gates], replace one another in any net; but a member of class A cannot replace one of class B or C. A member of class B can replace one of class A or C only if non-logical elements, for example noise generators, are included in the net. (Brindley 1967: 361)

In effect, Brindley was giving a computational justification for the newly discovered noise in the nervous system (Delisle Burns 1968).

A mathematical Appendix described a wider range of modifiable synapses. These involved multiple inputs, closed (re-excitatory) loops, and temporal summation. They, too, could be formally classified in terms of their input–output relations.

Using this classification, Brindley designed (pencil-and-paper) models supporting different types of learning—such as classical and operant conditioning, both with and without extinction. His network for *classical conditioning with extinction*, for instance, used two modifiable synapses, the first of class B and the second of class A (p. 367). This was no accident, for he proved that if extinction was to be modelled at all then "classical conditioning *requires* modifiable synapses of classes A and B or of class C, and operant conditioning of class B or C" (p. 361; italics added). It followed that if Hebbian synapses are indeed involved in storing learnt information, then some of the nervous system's modifiable synapses *must* be of class B or C.

One methodological moral was that computational arguments based on the behaviour of the whole nervous system can't tell us just which synapses are present. For there are "a number of quite different but roughly equally plausible kinds of modifiable synapses that will do the same things (i.e. perform the same computations) in networks of nearly the same complexity" (p. 365).

But such arguments can suggest hypotheses about which *classes* of synapse are present, in what proportions. They prove, for example, that it is "uneconomical to use class B synapses to produce a class-A input–output relation, and infinitely uneconomical to do the reverse". And since the nervous system as a whole can exhibit class B input–output relations, it must (as noted above) involve synapses of class B or C.

In a follow-up paper, Brindley (1969) discussed the construction of much larger networks, in which only a few simple details need to be accurately specified and which can learn indefinitely many different things. (All previous models had been devoted to a single learning task.) And the *numbers* of neurones now entered the picture:

> The number of cells required for performing tasks of the kind considered [e.g. verbal rote learning] as well as the human brain can perform them, is only a small fraction of the number of cells in the brain.
>
> [The] models proposed [here] are likely to be the most economical possible for their tasks, components and constructional constraints, and . . . any others that approach them in economy

must share with them certain observable features, in particular an abundance of cells with many independent inputs and low thresholds. (Brindley 1969: 173)

Various experimentally testable predictions about synapses and cell numbers could be generated accordingly. In other words, this computational approach not only 'made sense' of the brain, but promised to help illuminate its detailed structure.

Among those whom Brindley acknowledged at the end of his 1969 paper were Barlow (already widely respected) and two unknowns: his students Stephen Blomfield and Marr. As we'll see in Section v.c, Marr was soon to publish the first formal theory of the brain *as such*. Eventually, because of his later research (on vision), he would become a household name for cognitive scientists in general. Brindley, by contrast, remained largely unknown.

But his work had been crucial. It confirmed what McCulloch, Uttley, Barlow, and Gregory had already argued: that *traditional neuro-anatomy must be combined with computational analysis*, if the nervous system is to be understood.

14.iv. A Fistful of Feature-Detectors

If Barlow's theory of perceptual coding was important for the rise of computational neuroscience, so was his practical work. Indeed, some of his early experiments led indirectly to perhaps the most famous paper (and certainly the best title) in the field.

Neurophysiologists had known since the late 1930s, thanks to Keffer Hartline at Rockefeller University, that some retinal cells in frogs aren't mere passive receptors of points of light. Rather, they respond to simple patterns of illumination: ON, OFF, or ON–OFF (Hartline 1938). In the early 1950s, Barlow reported in the *Journal of Physiology* that such cells are more sensitive to edges (light contours) than to unstructured light, and that the ON–OFF variety are especially sensitive to movement (Barlow 1953). Half-jokingly, he even called these cells "bug detectors".

In 1959 that term would start to ring still-resounding bells.

a. Bug-detectors

Most people today associate bug-detectors not with Barlow, but with the four authors of 'What the Frog's Eye Tells the Frog's Brain': Lettvin, Humberto Maturana, McCulloch, and Pitts (1959).

McCulloch had been a founder of the cybernetics movement (4.v–vi), and the 'Frog's Eye' work was done at the hub of cybernetics: MIT. So it's prima facie not surprising that although three of the four authors were neuroscientists, the paper appeared—like many 'cognitive' pieces at that time—in a journal officially aimed not at biologists but at radio engineers. However, this didn't happen by choice. Lettvin remarked in a lecture, years later, that they'd tried to get the paper published in several biological journals but were "laughed off the stage". The reason was that they saw coding not as all-or-none but as implemented by variable *bursts* of nerve signals (John Collier, personal communication).

The paper's message was as memorable as its title. For it showed that the frog's eye has a great deal to tell its brain, over and above the presence of light—or even of Barlow's edges and movement.

As Lettvin's group put it: "the eye speaks to the brain in a language already highly organized and interpreted, instead of transmitting some more or less accurate copy of the distribution of light on the receptors" (Lettvin *et al.* 1959: 251). Specifically, they had found single cells in the frog's retina that were sensitive to one of four different light patterns:

> (1) local sharp edges and contrast; (2) the curvature [within specific limits] of edge of a dark object; (3) the movement of edges; and (4) the local dimmings produced by movement or rapid general darkening. (p. 253)

These results were observed even when the stimulus, instead of being artificially isolated (as in Barlow's experiments), was surrounded by a *naturalistic* image of the countryside as presented to a frog.

The 'Frog's Eye' authors also found that the four cell-types mapped onto four distinct cell sheets in the frog's brain (the optic tectum, or colliculus). And these were precisely co-registered: neighbouring points in the retina were connected to neighbouring points in sheet 1 . . . and sheet 4. What's more, the nerve fibres would grow back to the right place if they were cut. The idea of *some sort of* point-to-point mapping had been suggested by Julia Apter, and by Pitts and McCulloch in their 1947 paper (see 12.i.c). Now, there was detailed experimental evidence. (The explanation for *how* this mapping is established was then unknown: see ix.a, below.) And there were various subtleties. For instance, some class 3 cells fired only if the movement was in a certain, very broadly defined, direction.

In fact, there were some subtleties so surprising that they weren't even mentioned in the published paper. Lettvin recalls:

> What we did not report, and what to me is still [in 1994] the most astonishing thing about the bug detectors, is the following property . . . You bring one spot in, move it into the [receptive] field, and as long as you move it around, wonderful response.
>
> You bring in two spots; if they're rigidly coupled in their motion by a fixed distance between them and are moved around, you get a good response almost as if they are only one spot.
>
> You bring three rigidly coupled spots in, and it doesn't matter their size or their disposition or their distances from each other: move them around as a rigidly coupled triad and there's no response at all.
>
> . . . If you have a white background and two black spots, OK; three spots, forget it. You connect any two spots of the three by a barely visible black line, and all of a sudden it's a two-spot system. It becomes visible. Or else you move any one spot with respect to the other two, now there's a response. (J. A. Anderson and Rosenfeld 1998: 18)

How, Lettvin asked himself, could a mere retina possibly compute such distinctions? He still has no answer—so still hasn't officially published the finding. In his judgement, then, this discovery wasn't really a discovery (see Chapter 1.iii.f). Putting it in Mayhew's terms, he'd discovered the cells, and what they were computing—but not *how* they were computing it.

What's more, it seemed that the eye might be telling the brain very interesting things—interesting, that is, *to the frog*. These concerned the approach of predators (casting a shadow over the scene: see class 4 above), and the whereabouts of food:

> The operations thus have much more the flavor of perception than of sensation if that distinction has any meaning now. That is to say that the language in which they are best described is the language of complex abstractions from the visual image. We have been tempted, for example, to call the convexity detectors "bug perceivers". Such a fiber responds best when a dark object, smaller than a receptive field, enters that field, stops, and moves about intermittently thereafter. The response is not affected if the lighting changes or if the background (say a picture of grass and flowers) is moving, and is not there if only the background, moving or still, is in the field. Could one better describe a system for detecting an accessible bug? (Lettvin *et al.* 1959: 253–4)

Indeed, the retinal cells seemed to guarantee the frog a healthy diet of fresh food. They responded only to stimuli that were moving, so—in its normal environment—probably *alive*. This tallied with previous observations of the whole animal's behaviour:

> The frog does not seem to see or, at any rate, is not concerned with the detail of stationary parts of the world around him. He will starve to death surrounded by food if it is not moving. His choice of food is determined only by size and movement. He will leap to capture any object the size of an insect or worm providing it moves like one. He can be fooled easily not only by a bit of dangled meat but by any moving small object. (p. 231)

Remarkably, the radius of curvature that strongly triggered the convexity detectors corresponded to bug size. In short, the frog's eye was seeing only what the frog needed to know.

Later, Lettvin would complain about the use of such "anthropomorphic" language even in talk about human brains, adding that "people have screwed up the frog because they're taking bachtriomorphic views of the frog. If you really want to understand the frog, you must learn to be objective and scientific" (J. A. Anderson and Rosenfeld 1998: 344). The 'Frog's Eye' paper itself, however, had initiated this perspective.

The 'Newtonian' assumption inherited from the associationists, and dating back to John Locke's distinction between "sensation" and "reflection" (Chapter 2.x.a), had been that the sense organs—and by extension, the peripheral parts of the brain—are both passive and atomistic. Sensitivity to points of light, *yes*; to complex patterns of light, *no*. To perceive (infer, compute) a light–dark gradient with a specific radius of curvature, it had been tacitly assumed, was a matter for the cerebral cortex—present only in mammals and birds—and must involve a large number of cells. Now, that assumption was shattered.

But the surprise, though great, wasn't unqualified. Recent work in both neurophysiology and cybernetics/AI had implied that some such results might be found. Barlow's experiments a few years before had modified the assumption already. Indeed, others besides the MIT team were looking for feature-detectors of some sort (see below). And Pitts and McCulloch (1947) themselves had posited several layers of visual computation in the brain, going from simple features to increasingly complex concepts.

(Admittedly, they'd assumed that the features would be relatively simple, and formally tractable. Pitts was therefore, so Lettvin recalls, "a little appalled by the results, which were very different from what he expected", and "while he accepted the work enthusiastically, at the same time it disappointed him"—J. A. Anderson and Rosenfeld 1998: 10.)

Moreover, many cyberneticians—though not the orthodox neurophysiologists—had encouraged attention to what *the animal* needs to know, given what it wants to do. In particular, Lettvin's mentor McCulloch had taught him to think of sensory neurophysiology as "experimental epistemology". (I remember being startled to find these words written on the door, on my first visit to Lettvin's lab.) He meant that one should ask not merely how perception tells animals about the world, but also how it makes appropriate action possible (Chapter 4.iii.b).

A visual neurophysiologist who became close to the MIT team a few years later was told by Lettvin that "Pitts and McCulloch actually were on the eye/brain paper as a friendly courtesy. They had no role in the planning or execution or anything of the eye/brain thing" (C. G. Gross, personal communication). However, McCulloch's lack of hands-on contributions doesn't rule out his having had a background influence on Lettvin's thinking.

Similarly, Selfridge had argued—on computational grounds, and partly influenced by Pitts and McCulloch—that complex perceptions *must* be based on many distinct feature-detectors (or "data demons"), where the "features" have survival value for the animal. And he'd illustrated part of what he meant by the Pandemonium program. (Only "part", because the program didn't need to survive, so didn't *want* anything: see Chapter 12.ii.d and iv.b.)

Lettvin was a very close friend of Selfridge, and of McCulloch and Pitts. Their mutual friendship had led to the 'Frog's Eye' work in the first place. The paper described itself as "an outgrowth" of the 1947 study of universals, which—in effect—had postulated layers of feature-detectors. In addition, it carried this acknowledgement: "We are particularly grateful to O. G. Selfridge, whose experiments with mechanical recognizers of pattern helped drive us to this work and whose criticism in part shaped its course" (Lettvin *et al.* 1959: 253, 254). In short, although the authors were doing "wet" neuroscience, not describing a computer model, they were plainly inspired by computational ideas.

What actually happened, according to Maturana (personal communication), began with Maturana's accidental discovery (in November 1958) of movement-sensitive cells in the frog's retina. He found these interesting because they confirmed his hunch that such cells might exist, a hunch based on purely anatomical grounds (microscopic evidence of asymmetries in the connection patterns of some of the retinal cells). But Lettvin's familiarity with Selfridge's ideas enabled him to see the wider importance of Maturana's finding. He immediately switched the laboratory's main research programme to a hunt for other specialized, and ecologically significant, cells—and they found them.

A further surprise was in store. Within a few years, Maturana rejected the computational interpretation of these experiments. He developed their whole-animal emphasis into a neo-Kantian philosophy of biology, in which the concepts of computation and information had no place (see 15.vii.b and 16.x.c). Orthodox neuroscientists, however, knew little or nothing of this—and cared less. Even with Maturana's blessing, neo-Kantianism was out of fashion.

b. And more, and more . . .

The whole-animal emphasis was unusual in the 1950s: most people looking for feature-detectors at the time weren't guided by it. David Hubel and Torstein Wiesel, for instance, were not.

They'd arrived at Stephen Kuffler's lab at Johns Hopkins (where Barlow was also working) in 1958, and were searching not in the frog's eye but in the cat's brain. Their discovery of feature-detectors in the striate cortex in effect confirmed Turing's hunch, that "the apparatus for the more elementary analysis of shapes and sounds probably comes into this category [i.e. definite structures in the brain]".

Their results, which revolutionized the study of visual cortex, were even more surprising than the MIT group's were. In the late 1950s they identified brain cells responsive to very highly specific patterns on the retina: lines/edges of many different orientations (Hubel and Wiesel 1959). More accurately, they found cells that responded to small line *segments*. Anything we'd normally regard as a 'line' would be represented by a *set* of 'line-detectors'. (Twenty years on, Hubel confessed that how the fragmentary information from such cells is assembled to build a percept of a line "is still a complete mystery" Hubel 1982: 519.)

Later (at Harvard), they would find even more types of feature-detector, at various levels in visual cortex. (Right from the start, they produced putative circuit diagrams for their various cell-types.) The levels seemed to be functional as well as anatomical, for some neurones, the "hypercomplex" cells, were fired by particular *combinations* of lower-level features. Moreover, the orientation detectors were arranged in regular columns, perpendicular to the brain's surface: same orientation, same column; and cells responding to similar orientations inhabited neighbouring columns (Hubel and Wiesel 1962, 1968, 1974).

They also found that many cells in visual cortex were binocular, whereas those in the first way-stage (the LGN, or lateral geniculate nucleus) were 'wired' to only one eye. The binocular cells were evidently wired with remarkable precision, each having two receptive fields closely similar in size, complexity, orientation, and position. However, most of them responded more strongly to one eye than to the other: Hubel and Wiesel called this effect "ocular dominance".

Their initial discovery had amazed them as much as anyone else, for they hadn't been expecting "features" quite like that. A year after receiving the Nobel Prize for their work, Hubel recalled:

> We had been doing experiments for about a month . . . and were not getting very far; the cells [in the cat's cortex] simply would not respond to our [two stimulus-classes, namely] spots and annuli. One day we made an especially stable recording . . . The cell in question lasted 9 hours, and by the end we had a very different feeling about what the cortex might be doing. For 3 or 4 hours we got absolutely nowhere. Then gradually we began to elicit some vague and inconsistent responses by stimulating somewhere in the midperiphery of the retina. We were inserting the glass slide with its black spot into the slot of the ophthalmoscope when suddenly over the audiomonitor the cell went off like a machine gun. After some fussing and fiddling we found out what was happening. The response had nothing to do with the black dot. As the glass slide was inserted its edge was casting onto the retina a faint but sharp shadow, a straight dark line on a light background. That was what the cell wanted, and it wanted it, moreover, in just one narrow range of orientations.

This was unheard of. *It is hard, now, to think back and realize just how free we were from any idea of what cortical cells might be doing in an animal's daily life . . .*

It took us months to convince ourselves that we were not at the mercy of some optical artefact . . . We did not want to make fools of ourselves so early in our careers. (Hubel 1982: 516; italics added)

Orientation detectors, bizarre though they seemed, were just the beginning. Hubel and Wiesel themselves soon discovered detectors for directional movement, for instance. And then, *mirabile dictu*, other researchers found cells higher up in the visual system (in inferotemporal cortex) that were responsive to hands, or to faces. These discoveries were amazing—indeed, apparently incredible (see below). But they weren't totally unexpected, and nor were they accidental.

The Warsaw neurophysiologist Jerzy Konorski (1903–73) had been the first to suggest, in 1960, that highly complex features such as these (which he called "unitary perceptions") might be coded by single neurones, or "gnostic units". In his book a few years later, he'd argued that these units might be grouped in separate places in the inferior temporal cortex. What's more, he speculated that there were different units coding for faces, limbs, animals, handwritten letters, or emotional expressions, and so on (Konorski 1967).

Konorski's ideas were still maverick in the early 1960s, when "most visual psychologists had never heard of this area [inferotemporal cortex]" and thought of the striate cortex alone as the ground of vision (C. G. Gross 2002: 86). But they hadn't come out of nowhere. He'd been inspired not only by Hubel and Wiesel's findings but also by Karl Pribram's work on the visual effects of lesioning monkey temporal cortex, and by the descriptions of highly specific clinical agnosias then coming from Luria in the Soviet Union (Pribram and Mishkin 1955; Luria 1968).

Maverick or not, Konorski was largely right: we can now see that he "anticipated subsequent discoveries to an amazing degree" (C. G. Gross 2002: 85). Even so, and despite a highly complimentary review in *Science* (C. G. Gross 1968), Konorski's book was largely ignored by visual psychologists and neurophysiologists:

For at least the next decade virtually all the many citations to the book were to the parts concerned with learning rather than with perception; learning theory [aka behaviourism] still dominated American psychology. (C. G. Gross 2002: 87)

One American who didn't ignore the parts concerned with perception (and who wrote the glowing *Science* review) was Charles Gross (1936–). While still based at Cambridge University as a graduate student (see Preface, ii), he'd visited Konorski's Warsaw lab in 1961 to learn about his strange new ideas.

Gross was as excited as anyone else by Hubel and Wiesel's recent discoveries. And he knew (from lesion studies like Pribram's, being carried out at Cambridge by his supervisor Larry Weiskrantz) that inferotemporal (as well as striate) cortex was crucial for vision (personal communication). So on moving to MIT a few years later, he decided to try to find further feature-detectors and/or gnostic units in that part of the brain—which he did.

But he could hardly believe his own eyes. He found a cell fitting one of Konorski's speculations: well, not for a limb—but for a hand. When Gross first drafted his research paper, he "did not have the nerve" to mention the cell that coded for the monkey's hand (C. G. Gross 1998: 199). He was as wary as any seventeenth-century gentleman of witnessing to the astonishing (see Chapter 2.ii.b). Like Hubel and Wiesel before him, he didn't want his career to be over before it started because of being seen as either sensationalist or gullible. A senior colleague—Hans-Lukas Teuber, a member of the cybernetics group—encouraged him to be brave. As a result, the hand-detector was duly announced (C. G. Gross *et al.* 1969).

However, it evidently beggared belief. People didn't even *try* to replicate it. No one else reported any follow-up experiments for over a decade. And Gross himself, "perhaps because of the general skepticism", waited just as long before fully describing the face-detectors (Bruce *et al.* 1981). Yet they, too, had shown up in his initial experiments.

By then, the scepticism had lessened. A flood of papers soon appeared on visual (and auditory) single-cell detectors in various parts of the monkey's cortex. They included many from David Perrett's lab at the University of St Andrews, Scotland (Perrett *et al.* 1982, 1985, 1990, 1992).

By the mid-1990s, cells had been found which discriminated (for instance) hands; faces; facial expressions; humans walking towards or away from the monkey subject; and anyone—human, monkey, or robot—picking up a raisin or performing some other action salient to monkeys. In addition, 'bimodal' detectors had been discovered, which responded to *visual and tactile* stimuli from the same body area, such as the arm and the space near to it (Graziano and Gross 1993).

Even the humble (humble?) retina had offered up further riches. Today, neuroscientists can distinguish "over 70 retinal neurons, well-differentiated in terms of morphology, connections, and transmitters and therefore presumably function too" (C. G. Gross, personal communication).

As that word "presumably" implies, not all of these retinal cells can be classified—yet—as detectors, for we don't know what they detect. But the fact remains that neural detectors of many different kinds, including the recently discovered mirror neurones (Section vii.c, below), are still being found in the nervous system.

c. But how?

As Mayhew's remark reminds us, however, the discovery of a single-cell detector doesn't tell one *how* that detection is achieved.

Of course, systematic experiments were done right from the start to identify the relevant stimulus classes. Lettvin's team, for example, studied many different stimuli, including diverse radii of curvature and various types of (smooth/jerky, fast/slow) movement. Similarly, Gross used a range of stimulus classes in testing the monkey's hand-detector (see Figure 14.1). Such experiments indicated which aspects of the stimulus are important. But they didn't show how they are computed. (Even now, the precise circuitry isn't known.)

By the mid-1980s, the biological experiments were being compared with specific computational theories. So, for example, when the St Andrews group discovered a wide variety of face-detector cells in the brain, they discussed them in the light of Marr's theory of vision (Chapter 7.v.b–d), Marvin Minsky's ideas about visual frames (10.iii.a), and Robert Baron's (1981) computational model of face recognition.

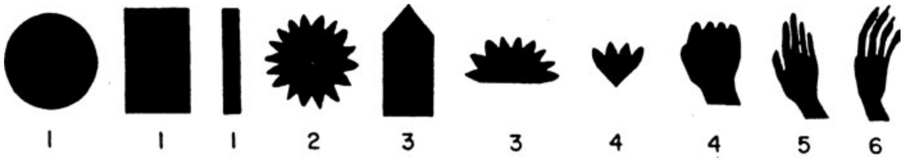


Fig. 14.1. Examples of shapes used to stimulate an inferior temporal cortex neuron "apparently having very complex trigger features. The stimuli are arranged from left to right in order of increasing ability to drive the neuron from none (1) or little (2 and 3) to maximum (6) . . . The use of [these] stimuli was begun one day when, having failed to drive a unit with any light stimulus, we waved a hand at the stimulus screen and elicited a very vigorous response from the previously unresponsive neuron . . . We then spent the next 12 hrs testing various paper cutouts in an attempt to find the trigger feature for this unit. When the entire set of stimuli used were ranked according to the strength of the response that they produced, we could not find a simple physical dimension that correlated with this rank order. However, the rank order of adequate stimuli did correlate with similarity (for us) to the shadow of a monkey hand" (C. G. Gross *et al.* 1972). Reprinted with permission from C. G. Gross (1998: 199)

Baron, a neurophysiologist at the University of Iowa, had worked on formal models of the nervous system since the early 1970s. In his account of face recognition, he'd posited various viewer-centred templates, such as distinct views of the head as it would appear at different stages of rotation. A face can be recognized (by brain or computer), he said, only if such templates are present, and "In every case, recognition is determined by correlating the processed input patterns against all stored templates" (Baron 1981: 145).

Before template comparison could get started, the system had to fixate on the relevant face features (if possible, the eyes first), and to standardize the image size. Since the relevant features were fixed (eyes, nose, chin, etc.), the program's task wasn't to distinguish between faces and other objects, but between this face and that one: Janet or Jill. Each face was represented in memory by up to five features, with up to four distinctly different templates for that feature—hence up to twenty templates in all. And, in accordance with experimental data on human eye movements, recognition was sometimes "Gestalt" (immediate), sometimes sequential (feature by feature).

Baron suggested that the various types of abnormal face recognition observed in clinical patients, such as prosopagnosia (where even one's spouse may not be recognized), are caused by breakdowns at different points within the system (pp. 171–5). He assigned specific computational roles to cells in the retina, lateral geniculate bodies, primary visual cortex, and cortex.

When discussing the types of feature-detector involved in locating a face in the first place, he said:

> It is perhaps no accident that the eye responds so readily to patterns having a dark center and light surround. The iris of the eye, eye-ball, eye socket, eyebrow, face, and hairline form a series of light and dark concentric circular regions that can easily be detected in the visual field. (p. 168 n.)

As this remark implies, he generalized his theory to visual recognition in general. And he stressed that in outlining the brain's "logical architecture", one must "carefully distinguish between the information and control processes involved" (p. 138). In short, Baron was addressing Mayhew's question about *how* a given cell computes what it does.

d. Monkey business

Perrett's group in Scotland, part-inspired by Gross and Baron, made some remarkable discoveries (Perrett *et al.* 1985). Although some cells were responsive to faces as such, others responded only to faces in particular orientations: full-face, profile, looking upwards, or looking downwards. Moreover, many of these were sensitive also to the direction of gaze, coding full-face-with-eye-contact, full-face-with-averted-gaze, or profile-with-averted-gaze.

The team suggested that these are monkeys' business. In other words, they are important social signals for monkeys, as they are for humans.

(With respect to humans, these researchers would later do hugely influential work on universal/cultural facial recognition, including a study of the attractiveness of facial symmetry—a topic discussed in Chapter 8.iv.b: Perrett *et al.* 1999; Penton-Voak *et al.* 1999; Keysers and Perrett 2004. The results of their experiments in presenting subjects with systematically varied computer simulations have been widely applied in Hollywood animation, and for VR faces too (see 13.vi): Burt and Perrett 1995; Calder *et al.* 2000.)

They observed, in separate experiments, that young monkeys "give appeasement gestures at an increased frequency for faces making eye contact compared with faces where the eyes are averted" (Perrett *et al.* 1985: 309). But, for social purposes, it presumably doesn't matter (to monkeys) whether the eyes are averted by 20, 50, 60, or 70 degrees. And indeed, the experimenters *didn't* discover a continuous range of eye-aversion detectors, analogous to Hubel and Wiesel's line-orientation cells:

> It is important to note that in the present study no cells were found that were maximally active to the face positioned at some intermediate angle between face and profile. *Only a small number of distinct views of the head seem to be receiving independent analysis in [this part of the brain]* but for each view there is considerable generalization over size and isomorphic rotation. (p. 315; italics added)

In general, they said, one might expect to find cells sensitive to features relevant to the animal's lifestyle:

> To avoid jumping into the lion's mouth one has to know which direction to jump. The importance of such information might have encouraged the evolution of neural mechanisms capable of direct and independent computation of the orientation of the head and body with respect to the observer and hence a viewer-centred analysis of these objects. (p. 315)

One must remember, here, that Perrett was studying monkeys. Most animals wouldn't be able to see *the lion*, even if they were able to avoid its mouth. In other words, very few animals—only birds and mammals—can do pattern recognition, or see whole objects such as lions or faces. All the others use "slick tricks" instead, including many species-specific feature-detectors.

For a crab, for example, if something is above the horizon it's dangerous and if it's below the horizon it's a crab. Or rather, the action that the visual stimulus evokes in the crab—either freezing/escape or claw-waving—is appropriate as a response to seagulls or crabs, respectively. The size and shape of the object involved don't matter, because they can't be perceived.

Moreover, the crab doesn't need any "neural 'software' image" to code the horizon. Instead, it's coded in such a way that "an object will be treated as a threat *if it appears* *on a particular part of the retina*, and thus the behaviour can be 'hard-wired' " (Layne *et al*. 1997: 52; italics added). That is, the crab has an appropriate slick trick. (Compare the crickets discussed in Chapter 15.vii.c.)

In relation to Marr's theory of object recognition, Perrett's group pointed out that whereas some cells were responding to faces as objects independent of the viewer, others were coding viewer-centred information. Because the oriented-face-detectors weren't representing ("making explicit") all the information available in the $2\frac{1}{2}$D sketch, their functioning was described as "a stage beyond two-and-a-half-dimensional sketches" (Perrett *et al*. 1985: 315).

Independent analysis of distinct views of the same object was necessary, they said. (*Computationally* necessary, that is.) But only relatively few distinct views may be needed (here they cited Minsky 1975). And, as remarked above, they'd found only very few views of eye gaze to be independently coded. Accordingly, they criticized Baron's model for requiring a distinct template for every 20 degrees of head rotation.

Their general conclusion was that "the recognition of one type of object may proceed via the independent high level analysis of several restricted views of the object (viewer-centred descriptions)" (p. 293). This, they said, challenged Marr's theory, which did *not* predict cells sensitive to certain views of faces. Partly, this was because viewer-centred representations were supposed to exist only at the level of the $2\frac{1}{2}$D sketch. Also, Marr had favoured distributed representations, considered as emergent properties of large groups of neurones (Chapter 12.v–vi). But Perrett's group had seemingly discovered paradigm cases of grandmother cells (individual neurones coding crucial information).

For our purposes, the important point is not whether Perrett's specific claims were right or wrong. What's important is their general nature. In a word, they were *computational* claims.

Such analysis simply couldn't have appeared in the early 1960s, when feature-detectors were both new and few. At that time, Marr and Minsky hadn't published on vision. There were no computer models of face recognition. And Barlow's theory of grandmother cells hadn't been formulated (see Section ix.e).

To be sure, by 1961 there was already a computer model of feature detection that was seen, by some, as biologically relevant. I'm thinking of Leonard Uhr and Charles Vossler's version of Pandemonium, which learnt its own basic operators (Chapter 12.ii.d). Citing Hubel and Wiesel, Uhr and Vossler described their work as embodying "relatively weak, [biologically] plausible, and 'natural-looking' assumptions" (1961*a*/1963: 265). The young Marr apparently agreed, for he would soon describe the model's operators as near-equivalents to what he termed "codons" in the brain (D. C. Marr 1969: 469)—see Section v.c, below.

Most neuroscientists at that time, however, would have disagreed. The general view—based on studies of newborn kittens—was that feature-detectors, and their anatomical organization within the brain, aren't learnt but innate. They might be fairly crude in the newborn animal, to be perfected in the critical period; and they might be lost through lack of use. (There was evidence for both of these claims.) But in essence, they seemed to be built in.

MIT's experimental epistemologists, following McCulloch, had expressed this in explicitly Kantian terms (Chapter 12.ii.c). Describing the organization of eye–brain connections, they said:

> The way that the retina projects to the tectum [to the four co-registered sheets: see above] suggests a nineteenth-century view of visual space . . . By transforming the image from a space of simple discrete points to a congruent space where each equivalent point is described by the intersection of particular qualities in its neighborhood . . . every point is seen in definite contexts. *The character of these contexts, generally built in, is the physiological synthetic 'a priori.'* [. . . If] there is any randomness in the connection of this [retina–tectum] system, it must be at a very fine level indeed. (Lettvin *et al.* 1959: 252–3; italics added)

Such views fitted well with Barlow's seminal arguments about low-level coding mechanisms (see Section iii.b), if not with behaviourist psychology. And they would endure for some years. For they cohered with the newly fashionable nativism of the late 1960s (Chapter 7.vi.d–e), including the growing evidence for a host of anatomical specializations related to language (see Chapter 9.vii and Lenneberg 1967).

In sum, feature-detectors were unexpected examples of what Turing had called "definite structures" in the cortex. They were initially assumed—by Hubel and Wiesel (1962) as well as the MIT Kantians—to be genetically determined.

By the mid-1970s, this assumption would be overturned. For computer models would show that the structure of visual cortex might arise spontaneously (see Sections vi.b and ix.a).

14.v. Modelling the Brain

The Pitts–McCulloch paper of 1947 had presented a computational theory of (part of) the nervous system, but not a computer model. Twenty years later, McCulloch would help to design such a model. It was still impossible in the 1960s, however, to simulate the brain in any detail.

Nevertheless, this decade saw the beginnings of four ambitious—and interestingly different—research programmes in computational neuroscience. Each eventually involved computer simulation, and each grew closer to the biological data over the years. They focused on brain modelling (discussed here and in Sections vii and viii.b); neuro-ethology (Section vii); and psychological realism (Section vi).

They were pioneered by Marr, Arbib, Andras Pellionisz, and Grossberg. All four men were highly interdisciplinary. The first three turned to neuroscience (and in Arbib's and Marr's case, to psychology too) after being trained in mathematics and/or physics and engineering. Grossberg's initial training was in psychology and, despite his voracious reading in neurophysiology and mathematics, he always regarded himself (and still does) as a theoretical psychologist (personal communication).

In addition, the equally interdisciplinary McCulloch initiated a brain model in the 1960s that was perhaps the first computer simulation to be constrained by specific details of neuro-anatomy and neurophysiology. After lying largely forgotten for many years, it has very recently been resuscitated. Let's start with that.

a. The Mars robot

One of the earliest—and most perceptive—working simulations of a specific brain region was produced by an engineer, a neuroscientist, and a programmer: William Kilmer, McCulloch (the senior partner), and Jay Blum (Kilmer *et al.* 1969).

This program—named RETIC—was a model of the reticular formation (RTF), a structure in the vertebrate brainstem that was attracting much interest at the time. The received wisdom was that the RTF switched the animal between sleep and waking. But some had suggested that it might also switch between different modes of activity, or instinctive behaviours.

Under the microscope, it appeared to be a stack of tissue slices (likened to poker chips: cf. Ito 1984). There were visible interconnections within each slice and also between slices—and sensori-motor connections, too. But what the microscope couldn't show was *just what* pattern of connections would be capable of activity switching of the type proposed. That was the biological question driving the project.

There was a technological aspect too, for the program was intended as a controller for a Mars robot. Indeed, it was an early example of situated robotics (see Chapter 15.vii.a), although it was a simulation rather than an actual robot. The situatedness was a technological necessity, because of physics. Because radio signals between Earth and Mars travel slowly, neither new sensory information (picked up by the robot) nor new goals (suggested by the humans back home) can be swiftly communicated between the two planets. So a Mars robot must be largely autonomous, switching automatically from one activity to another as the local circumstances require.

GOFAI planning (then in its infancy: see Chapter 10.iii.b), used for NewFAI robots such as SHAKEY, was eschewed partly because it was too slow. In addition, however, the anatomical evidence didn't favour a central controller.

Many vertebrate species have an RTF but no cortex, which suggests that their behavioural 'decisions' aren't made top-down. For the same reason, a hierarchical program like Pandemonium, with its autocratic master demon, seemed biologically implausible. Rather, the lower-level units should somehow decide the outcome themselves, "shouting" not to some higher-level demon but to each other.

What was needed, then, was distributed, cooperative, computation. McCulloch had already described this in his 1947 paper (12.i.c). But he called it "redundancy of potential command". This terminology—and the idea itself—was a legacy from McCulloch's Navy days (4.iii.a). He'd learnt then that, in battle conditions, control can shift continually from one ship to another, according to the information they're receiving. Even the Admiral's flagship isn't all-powerful—nor, unfortunately, ever-present (Arbib 2000: 194).

The Mars robot was designed accordingly. Each RTF slice was modelled by a small network, or module, which estimated how likely (how appropriate) a particular type of activity was, given the current sensory input. These within-slice estimations were tentative, in that they could be influenced by between-slice connections. The final selection was distributed over the whole system. That is, the overall network would identify the maximum inter-module consensus, so choosing the most appropriate activity as its motor output.

In principle, one might have expected occasional confusion and/or paralysis, with no *one* activity being a clear winner. (Remember Buridan's ass: placed exactly halfway between two identical bundles of hay, and lacking 'freewill', the creature would supposedly starve to death.) In fact, this very rarely happened. Irrespective of the input, RETIC almost always converged to a majority-based choice of a single mode of action.

McCulloch's team (let's call them KMB) were making a crucial assumption, here. Namely, that distinct bodily activities have distinct neural mechanisms driving them, plus a 'switch' to activate one rather than another.

Peter Greene at Chicago, building on the ideas of Beurle and Belmont Farley (Chapter 12.ii.b), had already shown that in principle this needn't be so (P. H. Greene 1962). He'd sketched a hypothetical horse whose gaits—walking, trotting, cantering, galloping—were all generated by a single system of four interconnected 'neurones'. Gait selection involved tipping *the whole system* into one of four modes, each with its "natural" or "resonant" frequency. If the brain was anything like this mathematical model, he said, single-unit analysis and/or theories of separate neural circuitry could never provide the true explanation.

In modern parlance, Greene was talking about attractors in dynamical systems (see Section ix.b, below). But for most neuroscientists at that time, McCulloch included, specific neural circuitry was more attractive than attractors.

Attractive or not, RETIC was only very superficially related to the brain. This was partly due to the fact that it was a functioning program, designed when computing power was still very limited.

The early model has recently been updated by Kilmer (1997), one of the original authors. Incorporating a heterarchical 'committee' of up to thirty interconnected modules, the new network converges to a single output (action pattern) in less than thirty cycles of computation. It's still described by Kilmer as being inspired by the brain (the RTF). But it's more strongly oriented to practical robotics than to neuroscience.

Some other models of action selection are grounded in hypotheses about different areas of the brain. For instance, in a simulation built at the University of Sheffield, specific behavioural gating is done by mechanisms based on the anatomy of the basal ganglia (e.g. Prescott *et al.* 1999; Gurney *et al.* 1998). The RTF wasn't represented in the model. At that stage, Tony Prescott (personal communication) allowed that the RTF affects the general arousal state (sleep/wakefulness), but believed that its sensory innervation is too primitive to change the organism appropriately from one behaviour to another.

Besides their basal-ganglia simulation, Prescott's team have built a robot embodying the same principles. It switches cleanly between actions of wall following, search, food pickup, corner finding, and food depositing (Gonzalez *et al.* 2000). The overall behaviour appears coherent nevertheless—indeed, this integration was a prime constraint guiding the research (Prescott 2001). It's achieved partly because competing (potential) actions don't distort the chosen action, and also because the winning action persists even when the strength of the salient input falls somewhat below the level that was required to initiate it.

At least, that's true when the simulated dopamine level is "normal". But varying the dopamine level results in behavioural changes analogous to those seen in humans. Low dopamine leads to difficulty in approaching and picking up "food" objects, and/or in slower movements (compare Parkinson's disease). High dopamine may lead to inappropriate behaviour, such as repeated lifting/lowering of the robot's arm, caused by two action systems being selected simultaneously (compare Tourette's syndrome).

Yet more recently, Prescott's team have returned to the 1960s RTF Mars robot (Humphries *et al.* forthcoming). They now acknowledge that the basal ganglia can't be the only action selection system operating in vertebrates. For both helpless neonates and decerebrate cats "have a limited behavioral repertoire that can be expressed in the absence of basal ganglia (in [helpless] neonates [the basal ganglia are] not connected; in decerebrates [they] have been lesioned)" (sect. 2.2). For instance, these animals can switch between grooming, feeding, locomoting, escaping, and self-defence. It follows that "some neural structure *within the intact brainstem* must also be capable of functioning as a limited action selection system" (italics added). And the prime candidate for that structure, as McCulloch suggested long ago, is the RTF.

The Sheffield group tested the KMB model (and Kilmer's updated version of it) in three ways:

* by simulating it,
* by implementing it as a control architecture for a robot,
* and by comparing its performance with various other controllers.

They found that the original KMB version often worked, but not always. For some input sets, it didn't converge in the simulation (although it did when embodied as a robot). And in the simple task they set to it, it did no better *for the robot* than a random-choice controller, and less well than a winner-takes-all controller. In other words, the sequence of action selections didn't reliably lead to behaviour enabling the robot to "survive". However, when they "fixed" the KMB model by preventing random reassignments of connections, performance improved dramatically.

In addition, they used GAs (genetic algorithms; see 15.vi) to test various controllers of the same general type but which *weren't* constrained by the facts of neuro-anatomy. Among the huge number of configurations available in KMB space, they found many that could function as robot controllers. Some of these outperformed the winner-takes-all strategy that had beaten KMB's original design. Moreover, some could function even with very noisy input sets—alias relatively primitive sensors.

Their paper ended with some speculations about how the RTF and basal ganglia may interact in vertebrate action selection (sect. 6.2), and how KMB models might be useful—even advantageous—in robotics (sect. 6.3).

In a nutshell, then, the Sheffield team had been able "to fulfill McCulloch's original hope for the model for the first time" (sect. 2.3).

b. The musician in the spare room

While the early RTF program was being designed in one Cambridge, a much more ambitious formal theory of the brain was being developed in the other—but without a computer program.

The location was a spare room provided by Crick and Sydney Brenner at the Molecular Biology Unit, sponsored by the Medical Research Council in 1963 (the successor of Max Perutz's 1950s Cavendish Laboratory group). Crick, with James Watson and Brenner, had recently discovered the nature of the genetic code—and before that, in 1953, the structure of DNA. As for Brenner, he was initiating a long-term study on the development of every single cell (under the electron microscope) of the nervous system of the 1 mm-long nematode worm *C. elegans* (Brenner 2001: 125–36). With only a few hundred cells (it turned out to be 302), always interconnected in the same way, *C. elegans* was destined to become hugely important for neurobiology and genetics. (Both Crick and Brenner received Nobel Prizes: in 1962 and 2002, respectively.)

The focus of the new theory that's of interest here, however, wasn't biochemistry or genetics. Rather, it was computation.

The occupant of the spare room was Marr, who'd been given a job in the lab "simply because he was working on something interesting" (Brenner 2001: 136). (Those were the days!) He turned out to be a wizard in dealing with the Unit's first computer—acquired only after much wrangling with the MRC funders, who didn't see the point of biologists having their own machine (Brenner 2001: 140–1). But that was more of a hobby than a serious research interest. Although Marr—persuaded by Brenner, persuaded in turn by his childhood friend Seymour Papert—saw the scientific potential in computing, the Unit's hard-won machine wasn't suitable for the ambitious work he wanted to do. Specifically, his ambition was to understand the brain.

After completing a degree in mathematics at Cambridge, Marr had done a year's intensive study of neuroscience and biochemistry—when he wasn't playing the clarinet, at which he was a master. He'd been selected for the National Youth Orchestra, had played various types of music at Rugby public school, and had been a member of a flourishing jazz group, the J. L. Dixie 7. His musical interests were an added bond with his neuroscience supervisor, Brindley—who is a respected amateur composer, some of whose works have been performed by the London Sinfonietta (P. Husbands, personal communication).

But music wasn't Marr's only passion. Like McCulloch and the 12-year-old Pitts before him (4.iii), Marr had been seduced in his youth by Bertrand Russell and Alfred North Whitehead's *Principia Mathematica*. One of his schoolfellows, also a Dixie player, remembers him—as a teenager in the early 1960s—reading this with "fascination and vast admiration", alongside the work of William Grey Walter on robots and John Dunne on time (P. M. Williams, personal communication). By the late 1960s, Marr's intellectual gurus also included Hebb—and therefore McCulloch too (see Chapter 5.iv.b). It was their concern with network-logic and learning which inspired his postgraduate studies.

His dissertation for his Trinity College Fellowship of 1968 had considered associative memory in the brain. It asked not only (qualitatively) how this is possible, but (quantitatively) how large the memory storage could be, given a certain network size. Others, notably David Willshaw and Longuet-Higgins, were addressing similar topics at that time (see Chapter 12.v.c). But Marr combined his abstract mathematical argument with a profusion of neuroscientific detail.

Soon, he converted his Trinity dissertation into three closely related theoretical papers (Marr 1969, 1970, 1971), plus a fourth co-authored with Blomfield (Blomfield and Marr 1970). All four appeared in leading scientific journals. One of these—the *Journal of Physiology*—had never published a non-experimental text before. In short, Marr was engaged in a fundamentally new *type* of neuroscientific research.

His four papers discussed the cerebellum, cerebrum (neocortex), and hippocampus (archicortex). They aimed to explain how the cerebellum learns and perfects movement in bodily skills, and to show that the neocortex and hippocampus are the sites of long-term and temporary memory respectively, with many memory-relevant interconnections. And they did this in terms of specific hypotheses about *what*, *where*, and—Mayhew's question (directly inherited from Marr)—*how*. These hypotheses covered many different things:

* input–output relations;
* neural connectivities, numbers, and activity levels;
* and the ways in which different classes of synapse,
* assigned to specific types of neurone,
* can be modified during learning
* and later used for retrieval.

In other words, Marr was following in Brindley's footsteps. Indeed, Brindley was acknowledged in each of Marr's papers. And "Brindley synapses" were repeatedly mentioned in the text—for instance, in the discussion of (Marr's equivalent of) feature-detectors (D. C. Marr 1970: 205–6, 215; 1971: 32–3, 78–9). But besides his classification of learning rules, Brindley had also inspired Marr to think about the cerebellum.

c. Secrets of the cerebellum

The cerebellum doesn't initiate skilled (for Marr: "voluntary") movements: that's done by motor cortex. Rather, it coordinates and controls them. More accurately, it coordinates skilled, learnt, or voluntary movements. (Reflex movements, including integrated patterns involving all four limbs, are controlled by the spinal cord and/or brain stem; they can occur even in decerebrate animals: Chapter 2.viii.d.)

In 1964 Brindley had published a brief paper on how the cerebellum uses sensory information. He'd suggested that the task of the cerebellum is to learn motor skills in such a way that when a simple or incomplete message (compare: an intention to pick up a glass, without knowing exactly how) is sent to it from the cerebral cortex, the required movement is performed automatically. That is, the cerebellum functions as an associative memory.

Marr agreed (1969: 438). But his discussion was very much fuller than Brindley's, and included a host of predictions about synaptic functions, connections, and numbers.

Even that fact, however, was partly due to his supervisor. Many years later, Brindley modestly said he'd given Marr no more than "a few" ideas—plus "one or two pep talks". The purpose of those pep talks was "to persuade him that unless his work led to experimentally testable predictions whose prior probability was neither almost zero nor almost unity, no experimenter would read his work" (Vaina 1991: 308).

Marr's view of what the cerebellum was doing reflected the several different 'languages' found even in the early digital computers. For instance, he said:

> Where it is possible to *translate* the combined activity of many cerebral fibres rather simply in *directives*, doing so in the cerebellum would free the cerebrum from an essentially tedious task. In these circumstances, the cerebellum becomes rather more than a slave which copies things originally organized by the cerebrum: it becomes an organ in which the cerebrum can set up *a sophisticated and interpretive buffer language* between itself and muscle. . . . The automatic cerebellar *translation* into movements or gestures will reflect in a concrete way what may in the cerebrum be diffuse and *specifically unformulated*, while the analysis leading to that diffuse and unformulated state can proceed *in its appropriate language*. (Marr 1969: 468; italics added)

So, where Craik had talked of "translation" from sensory to cerebral to motor languages (Chapter 4.vi.b), Marr implied several (many?) cerebral languages suited to modelling different things. One brain level may pass messages to another, which translates them into some other internal language . . . and ultimately into muscle movements.

Besides this general notion of computer 'languages', Marr would employ many specific computational arguments. His thinking started, however, from the neuroscientific data.

This is clear from the rhetorical structure of his first paper. Its opening sentence was "The cortex of the vertebrate cerebellum has a simple and extremely regular fine structure."

He cited the recent (1967) book by Eccles and two younger colleagues (one of whom was a Hungarian expert on control-engineering), with the telling title *The Cerebellum as a Neuronal Machine* (Eccles *et al.* 1967). (These authors had suggested that learning takes place in the cerebellum, perhaps by the growth of dendritic spines on the Purkinje cells.) And he immediately went on to summarize what was known of the (cat's) neuro-anatomy:

> The axons of the Purkinje cells form the only output from the cortex of the cerebellum; and these cells are driven by two essentially different kinds of input, one direct, the other indirect. The first is the climbing fibre input, and the second the mossy fibres, *whose influence on the Purkinje cells may be complicated.*
>
> The inferior olive is the only known source of climbing fibres . . . Each [olivary cell] sends out an axon which terminates in one climbing fibre on just one Purkinje cell; there are very few exceptions. The climbing fibre completely dominates the dendritic tree of the Purkinje cell, and its action has been shown to be powerfully excitatory . . . (Marr 1969: 438–9; italics added)

(It's now known that there are more than a "very few" exceptions: although each Purkinje cell has only one climbing fibre, each climbing fibre connects with ten to fifteen Purkinje cells—Rolls and Treves 1998: 191.)

Bearing these biological constraints in mind, he then outlined his solution. What the cerebellum was doing, and *how*, could be understood in terms of associative memory. And, unlike the writers discussed in Chapter 12, Marr ascribed this memory to distinct structures in the brain:

> [I suggest] that each olivary cell corresponds to a "piece of output" [an *elemental movement*] which it is necessary to have under control during movements.
>
> . . . Every action therefore has a defining representation as a sequence of firing patterns in the olive.
>
> The final assumption . . . is that the nervous system has a way of converting the (inhibitory) output of a Purkinje cell into an instruction which provokes the precise movement to which its uniquely related olivary cell responds.
>
> [I shall argue that] the Purkinje cell can learn all the "situations" in which the olive cell movement is required, and later, when such a situation occurs again, can implement the movement itself . . . [So] the cerebellum could learn to carry out any previously rehearsed action which the cerebrum chose to initiate, for as that action progressed, the context for the next part of it would form, would be recognized by the appropriate Purkinje cells, and these would turn on the next set of muscles, allowing further development of the action. In this way, each muscle would be turned on and off at the correct moment, and the action would be automatically performed. (Marr 1969: 439)

As for the mossy fibres ("whose influence on the Purkinje cells may be complicated"), they provide the information that defines the "context" for each Purkinje cell. And how could that be done? This was his suggestion:

[It] is necessary to demonstrate that the mossy fibre–granule cell–Purkinje cell arrangement could operate as a pattern recognition device. The notion fundamental to this is that the mossy fibre–granule cell articulation is essentially a pattern separator. That is, it amplifies discrepancies between patterns that are rather similar . . .

[A] mossy fibre input has been learnt by a given Purkinje cell if, and only if, the input is transformed into impulses in a bundle of parallel fibres all of whose synapses with that Purkinje cell have been facilitated. (p. 440)

But there were two crucial complications:

First, the number of parallel fibres into which a mossy fibre input is translated increases very sharply with the number of active mossy fibres unless the threshold of the granule cells also increases . . . [Economy] arguments suggest that the granule cell threshold should be controlled in a suitable way. An inhibitory interneurone could achieve this, and the Golgi cells are interpreted as fulfilling this role.

The second point is that [variation in parallel fibre activity] will still exist. Whether or not a Purkinje cell should respond to a given mossy fibre input cannot therefore be decided by a fixed threshold mechanism. . . . The natural way to implement [the necessary threshold-setting mechanism] is to allow the parallel fibres to drive an interneurone which inhibits the Purkinje cell: and it will be shown that the various stellate inhibitory cells can be associated with this function, *although their dendritic and axonal distributions are at first sight unsuitable*. (p. 440; italics added)

The theory offered many surprises. These included functional interpretations for previously mysterious structures, and various counter-intuitive claims too (cf. "although their dendritic and axonal distributions are at first sight unsuitable").

One such claim was that a single pair of olivary and Purkinje cells might perform in two different ways at different times. In other words, its input–output relations depend on the type of learning involved—for maintaining posture or for active movement (pp. 466–9).

Another was that the *only* modifiable synapses in the cerebellum are those between the parallel fibres and the Purkinje cells. This was pure theory: these synapses weren't experimentally shown to be modifiable until very much later (Ito 1982, 1984: 115–30). Similarly, Marr's claims that the synapses between the Golgi cells and the mossy and parallel fibres are *not* modifiable were argued on purely computational grounds (p. 454).

At the core of the theory was a new Hebbian learning rule, not included in Brindley's classification:

[If] a parallel fibre is active at about the same time as the climbing fibre to a Purkinje cell with which that parallel fibre makes synaptic contact, then the efficacy of that synapse is increased towards some fixed maximum value. ("At about the same time" is an intentionally inexact phrase: the period of sensitivity needs to be something like 50–100 msec.) (pp. 455–6)

This rule was "Hebbian" in the broad sense. Whereas Hebb had posited learning when a pre-synaptic and a post-synaptic cell are co-active, Marr demanded two co-active pre-synaptic cells, irrespective of activity in the post-synaptic cell. Moreover, only one of the two synapses involved was modifiable.

The function of the climbing fibres, on this theory, was to carry feedback (about the intended movement) from the olivary nucleus to the Purkinje cells. In other words, the cerebellum was being described as a system for 'supervised' learning (see Chapter 12.ii.d).

Perhaps even more surprising, Marr's interpretations often involved precise numbers: of cells, synapses, and excitation levels. Right at the beginning of the paper, he pointed out that "Behind this general structure lie some relatively fixed numerical relations." For example:

> Each Purkinje cell has about 200,000 (spine) synapses with the parallel fibres crossing its dendritic tree, and almost every such parallel fibre makes a synaptic contact. The length of each parallel fibre is 2–3 mm . . . Each basket cell axon runs for about 1 mm transversely, which is about the distance of 10 Purkinje cells . . . There is one Golgi cell per 9 or 10 Purkinje cells, and its axon synapses (in glomeruli) with all the granule cells in that region, i.e. around 4500. (pp. 442–3)

The list continued, on and on . . . and on. But why? Who cares?

Marr cared, because he assumed that these numbers, being "relatively fixed", must have some functional (computational) significance. Eccles and his fellow neuro-anatomists, who had helped discover these numbers in the first place, had doubtless made the same assumption. But Marr tried to show, in detail, just what the functional significance is.

He even *predicted* many as yet unknown numbers. Some of these quantitative predictions suggested minimum–maximum limits between which the thresholds of the modifiable synapses must vary, or time periods during which 'simultaneous' events must happen (see above). Others concerned activity levels within cell populations; or the number of synapses on a single cell; or overall cell numbers; or numbers of simultaneously active cells. For instance:

* "The number of granule cells active at any one time (say in any 50 msec period) is a small fraction (less than $\frac{1}{20}$) of all granule cells" (p. 469).
* "[Given that there are 200,000 parallel fibres for each Purkinje cell], the maximum desirable number of facilitated synapses on any one Purkinje cell [is] 140,000, and
* the minimum number of parallel fibres active in any learned event [is] 500" (p. 457).
* "Calculations based on slightly tenuous assumptions suggest that each Purkinje cell receives connections from about 7000 mossy fibres" (p. 443).

Similarly, in defining his crucial concept of a *codon* (a subset of a collection of active mossy fibres, feeding into a granular *codon cell*), Marr committed himself to specific quantities. He gave mathematical equations for the exact number of codons of a given size that are associated with a given number of active mossy fibres, and for the number of mossy fibres that may influence a single Purkinje cell (pp. 444–5). These numbers were needed, he said, in order to understand "the codon sampling statistics" that enabled the cerebellum to distinguish similar patterns.

These hypotheses were presented as being based solely on neurophysiology and mathematics. But AI had apparently been an inspiration too. For towards the end of the paper, he pointed out that a codon is "closely related to the feature analysis ideas current in the machine intelligence literature" (p. 469)—and he cited Uhr and Vossler (see Section iv.d).

In making his numerical predictions, Marr relied on the general principle of computational "economy", or "efficiency". For instance:

> It is [evident] that the maximum codon size used depends critically on the number of claws to each [granule] cell. Given this, the factor that will determine the number of claws to each cell will be *economy of structure*; and the relevant question is what is *the least number* of claws such that [the system has the properties required]. (p. 447; italics added)

> The number of patterns a Purkinje cell can learn decreases sharply as the number of active parallel fibres involved in each increases. It is therefore essential to the *efficient* functioning of the system that the codon size should depend on the amount of mossy fibre activity . . . The codon size must be maximal, subject to conditions [previously specified]. This ensures that the number of modifiable synapses used for each learned event is minimal, *and hence that the capacity is maximal*. (pp. 449–50; italics added)

Throughout the paper, Marr assumed that evolution must have found the mathematically optimal way of doing things.

Bearing in mind Crick's contrast (Section ii.a) between biology's "slick tricks" and the mathematician's "powerful general principles", it wasn't obvious that Marr's optimality assumption was warranted (cf. Boden 1988: 60–3). Indeed, it's still not obvious, despite an intriguing attempt by a computer scientist in the late 1980s to defend an information-maximizing role for the brain (see Section ix.a, below). But that's just to say that Marr's quantitative predictions might have been wrong. What's more important is that he made them at all.

The paper bristled with testable hypotheses, as well as novel explanations of known facts. Some were so central that "If this is not true, the theory collapses", while the disproof of others would be "embarrassing but not catastrophic" (p. 468). In the two companion papers, Marr elevated such distinctions to a fine art, marking each prediction by up to three asterisks. Unasterisked predictions were those which lay "strictly outside the range of the theory, but about which the theory provides a strong hint" (D. C. Marr 1970: 231; cf. 1971: 77).

Evidently, Brindley's "pep talks" had worked. Marr offered many hostages to experimental fortune. But did the experimenters respond, as Brindley had hoped?

d. Audience reaction

A few did. Rashevsky's intellectual successors—computationally inclined and mathematically competent neuroscientists—found Marr's ideas hugely exciting. Cowan remembers that "the 1969 cerebellum paper created a sensation", and that all three papers generated "a tremendous amount of interest among neurobiologists" (J. A. Anderson and Rosenfeld 1998: 114).

These people, of course, were already familiar with mathematical theories of associative memory. Indeed, Cowan saw Marr's core cerebellar learning rule as a development of Wilfred Taylor's pioneering research in the 1950s (Chapter 12.ii.b), and essentially similar to his colleague Greene's (1965) theory of retrieval, published in Rashevsky's journal four years earlier (see Chapter 12.v.c). According to Cowan: "It requires only inhibitory neurons to control thresholds, and intrinsic climbing fibers to train synapses (both suggested but not implemented by Greene), to complete the picture" (Vaina 1991: 204).

It's not clear whether Marr knew of Greene's paper. In general, he cited others less often than he should have done. I've been persuaded of this by several private conversations over the years, and it's now been remarked in print more than once—by Cowan and Arbib, for instance (J. A. Anderson and Rosenfeld 1998: 110, 231–2). But Cowan also allows that "David had a very original mind, so it's easily credible that David just invented the whole thing himself" (J. A. Anderson and Rosenfeld 1998: 111). However that may be, Marr had added ample neuroscientific flesh onto Greene's mathematical core. (Greene hadn't even mentioned the cerebellum.)

One might have expected neurobiologists *in general* to appreciate that fact. The Rashevsky school did. But many others didn't. As remarked above, no purely theoretical paper had appeared in the *Journal of Physiology* before—and not all of the readers welcomed the novelty. Cowan, again: "a lot of the theoretical biologists felt that [Marr's first paper] was so speculative that it couldn't possibly be right and weren't impressed with it" (J. A. Anderson and Rosenfeld 1998: 115).

Marr's theory of the cerebellum was indeed speculative, for it couldn't (yet) be directly confirmed by single-unit recording or neurochemistry, nor tested as a functioning computer model. And, as Brindley had shown (see Section iii.c), *behavioural* tests were inadequate in principle.

Even some people already working on the abstract mathematics of memory saw it as premature. For instance, Longuet-Higgins described Marr's work as "excellent"—not a term he ever used lightly—but said (in his reply to the Lighthill Report) that "for some time to come" it would be less valuable for the cognitive sciences than GOFAI theories of cognition (Longuet-Higgins 1973: 36–7). (By the end of Marr's short life, Longuet-Higgins believed that that time *had* now come: see subsection f, below.)

Furthermore, Marr's work was difficult to read. Even highly sympathetic critics have remarked on the numbing profusion of neuro-anatomical detail, the "pedantry" of the style, and the "over-elaborate" mathematics (Willshaw and Buckingham 1990).

The mathematics was indeed taxing. In the neocortex paper, Marr defensively said, "The results [of these technical preliminaries] are mainly of an abstract or statistical nature, and despite the length of the formulae, are essentially simple," and "The reader who is not familiar with this notation should not be put off by it. All the important arguments of the paper have been written out in full" (Marr 1970: 193, 167). Many readers were put off, nonetheless. And those who weren't, didn't always master what they were reading. So Willshaw has recently complained that more people have cited Marr than have understood him (Willshaw and Buckingham 1990).

Eventually, however, Marr's early work set a theoretical and experimental agenda that's still driving some neuroscientific research today. Masao Ito, in his now classic book on the cerebellum, recalls that Marr's theory assumed a special type of synaptic plasticity in cerebellar cortex, and says: "[Being] impressed by the close conformity of this assumption to the neuronal circuitry [worked out in our laboratory in the early 1970s], I was prompted to verify [it] experimentally" (Ito 1984, p. ix). (Ito also praises Marr for his three-level account of explanation, outlined in Chapter 7.iii.b—Ito 1984: 2.)

Bruce McNaughten (of the University of Arizona) sees Marr's papers as "guiding lights" for himself and various other neuroscientists. In his judgement, they showed "astounding prescience", given the sparsity of experimental evidence available at the time. Their insights, he says, have been largely substantiated, and "have brought order to a number of otherwise disconnected data on the anatomy, biophysics and information transmission of [the brain]" (Vaina 1991: 121, 126).

Marr's theory of supervised learning in the cerebellum was especially influential, and—although some doubts remain (Vaina 1991: 47–8)—is still widely regarded as essentially sound. That's not to say that every current model of the cerebellum is like his. We'll see in Section ix.b–c, for instance, that Pellionisz's account relies on a very different formal analysis. And Grossberg's model is different again (see Section vi). But Marr's central insights are now accepted—with two qualifications.

One of these concerns the precise mathematical form of his learning rule. It was later proved that the algorithm as he defined it would eventually lead to a state in which all the weights are at a maximum value (Sejnowski 1977). (Nevertheless, it was an ancestor of some now current connectionist learning rules.) The other concerns his neurological interpretation of it.

Marr proposed that the parallel-to-Purkinje synapse is strengthened by co-activity in the parallel and climbing fibres (see above). James Albus had been developing a similar idea—which he likened to a perceptron (Albus 1971). Soon afterwards, he implemented it as the "CMAC" mechanism (Cerebellar Model Arithmetic Computer); this was initially developed to control a robot arm, but is today used by others for various purposes (Albus 1975; van der Smagt 1998). But if Albus shared Marr's commitment to computational explanations, he differed on the neurophysiology.

In Albus's account of the cerebellum, joint activation of the climbing and parallel fibres would *decrease* the efficiency of the parallel-to-Purkinje synapse. Moreover, he saw the learning as driven (via the feedback in the climbing fibres) by *error*, where Marr had seen it as driven by *intention*—that is, by the "context" alerted by the cerebrum. At that time, it seemed that Marr was right; now it's known that they both were. So people speak today of the "Marr–Albus" model.

As with back propagation (Chapter 12.vi.d), there are several priority disputes here. Quite apart from Brindley's and Eccles's vague suggestions about cerebellar learning, formal equivalents of Marr's learning rule had already been defined.

One example was Greene's (1965) work, mentioned above, which Cowan describes as "virtually identical with David Marr's cerebellum model, except it's not applied to the cerebellum". What's more, Grossberg had formulated the same rule in 1964—and he *had* applied it to the cerebellum (Grossberg 1969). He'd even made the same prediction, that learning would occur at the synapses of the parallel fibres and Purkinje cells. However, because of the difficulties in getting his work accepted by journals of psychology or neurophysiology, his cerebellum paper was published in *Studies in Applied Mathematics*—not something which neuroscientists were likely to see.

e. Beyond the cerebellum

Because of the cerebellum's relatively simple histological structure (one climbing fibre to each Purkinje cell, for example), Marr had been able to represent all the neuronal pathways involved. But that wasn't possible for the cerebrum or hippocampus. His theories about these were even more "speculative" as a result.

The paper on neocortex, Marr later told Terrence Sejnowski, was the one he was "most proud of" (Vaina 1991: 297). It focused on the unsupervised learning of high-level concepts: sophisticated examples of Pitts and McCulloch's "universals" and Konorski's "gnostic units" (see Sections iii–iv).

Marr described neocortex as "classifying cortex", in contrast with the "memorizing cortex" of the hippocampus. He claimed that whereas the hippocampus stores patterns in the "language" in which they were input to it, the cerebrum can also generate "a new language" in which to describe (classify) them (1971: 73–4).

The vocabulary of both these languages consisted of codons. Whereas his cerebellar theory had assumed the existence of preformed codons, Marr suggested that the neocortex can learn codons as a result of environmental input (compare Taylor's machine, described in Chapter 12.ii.b). He defined a method whereby certain synapses could calculate conditional probabilities. Uttley had discussed such synaptic calculations too (see Section iii.b), but whereas he'd focused on the *frequency* of events, Marr focused on their *similarities*. He described this learning method as a form of "numerical taxonomy", in which the differences between objects are computed, and then clusters are formed which minimize these differences (1970: 175).

The neocortex paper was full of intriguing ideas—not least, that sleep is necessary for the formation of new codons (Marr 1970: 215). But most of its intellectual cheques still can't be cashed—nor even presented to the cashier. As Sejnowski has pointed out, increased computer power will be needed to evaluate Marr's theory of neocortex properly (Vaina 1991: 298). (Cowan believes that the central learning method will work if the outside world is reasonably coherent, and likens it to classification techniques recently developed by Grossberg and by Teuvo Kohonen—Vaina 1991: 208.)

As for the hippocampus, a huge amount of computational work has been done in the last quarter-century, including much based on 1970s research on cognitive maps (Gluck 1996; Burgess *et al.* 1998). Nevertheless, a very recent review states that "Models of the hippocampal system and episodic memory still have many elements in common with Marr's basic view of hippocampal function, [even though] recent efforts have added considerable elaboration and refinement" (Gluck *et al.* 2003: 274). And Willshaw's judgement (around 1990) was that "Even 20 years after publication, Marr's theory remains the most complete computational model of the hippocampus" (Vaina 1991: 120).

"Complete" doesn't mean "correct", however, and Willshaw himself rejects a number of Marr's claims (Willshaw and Buckingham 1990). The fundamental problem, he remarks, is that the hippocampus is very difficult to analyse, because—unlike the cerebellum—it's not completely connected. In general, Marr's computational arguments didn't sufficiently constrain his theory of hippocampus: other models are possible too.

Moreover, neurones may have computational properties that Marr didn't fully consider, which undermine some of his key arguments. For example, he suggested (giving no algorithm) that some neurones may have a *dual* threshold, in which activation requires inputs from both a minimal number and a minimal proportion of the cell's many synapses. He even discussed what types of local circuitry could do the relevant arithmetic. But whereas he said his model would be "impaired" if only one threshold-type were available, Willshaw sees dual threshold-setting strategies as *essential* for incompletely connected nets. Such units will behave differently from 'classical' neurones, and a particular cell's activity will depend on the specific numbers involved.

With this in mind, Willshaw argues—and has confirmed, by extensive simulation—that Marr's reasons for positing a third neurone layer in the hippocampus were mistaken. These reasons concerned storage capacity in associative memories (a long-standing interest of Willshaw's: Chapter 12.v.c).

Willshaw showed that, in principle, if the dual thresholds and connectivities are set appropriately then two layers will suffice. Not only that: in most conditions, his own two-layer simulation (based on Grossberg's "competitive" nets: see Section vi) performed as well—stored as many memories, with similar recall—as the three-layer version, even though it contained many fewer synapses. Willshaw has also compared the behaviour of various types of dual-threshold mechanism. But he has the advantage of considerable computer power. Marr, when he wrote and 'published' his Fellowship dissertation in the late 1960s, didn't.

Both Marr and Willshaw, of course, were committed to computational theorizing about the brain. But (as remarked already) by no means all neuroscientists are. In the 1960s, that was only to be expected. Brenner, for instance, recalls:

> [In] those days biologists were in general very antagonistic to computers. They thought that anybody interested in computing was choosing an easy way out of a responsible job. In other words, if you didn't work at the bench you weren't worth anything! (Brenner 2001: 139–40)

However, many people who specialize in hands-on brain research still, in the twenty-first century, have scant regard for the computational approach. Gross, for example, who is more knowledgeable than most about the history of his field, says this:

> I'm a bit dubious about the role of theory *or at least of theoreticians* in biology. So far, I'd suggest, no pure theoretician ever made any contribution to biology at all, and indeed they disappeared so quickly that we have all but lost their names.... [The] great theoreticians in biology were above all empiricists who steeped their waking days and nights in observing, collecting, experimenting—such as Aristotle, Darwin, Bernard, Mendel, Freud, Sherrington and in our day Sperry and Hubel and Wiesel. So far, theory without empirical slogging in the same head has yielded nothing. Maybe biology is changing. Maybe one can be a pure computational biologist and never touch a plant and animals but only a computer... we'll see. Crick and Watson are the only exceptions I can think of. Maybe they herald a new way in biology. But Crick in neuroscience makes the rule. He is very smart, very well read in neuroscience, gets all the latest data fed to him and for decades has produced zilch but an absurd idea about dreams and a silly localization of consciousness. (C. G. Gross, personal communication; italics added; quoted by permission)

On this view, Marr's work on cerebellum was influential largely because it was so dependent on Hebb and Brindley—both of whom were empiricists first and theoreticians second. (Their books cite many empirical papers with themselves as sole/first author, plus others written with others.) Marr's later work on the brain is much less widely known by neuroscientists (he's not mentioned, for example, in the widely used textbook by Kandel *et al.* 1995). In Gross's opinion, that's as it should be.

This is a special case of a general type of cross-disciplinary complaint. Similar criticisms have been made by biologists of the "pure computational biologist" Stuart Kauffman, as we'll see in Chapter 15.ix.c. And professional psychologists commonly rebuke—and even more commonly ignore—AI modellers for their lack of attention to the experimental data. AI researchers, in turn, often lament others' blithe ignorance of computational concepts and constraints. As for philosophers, they're more than ready to point out philosophical naivety on the part of scientists of all stripes. (Maybe this is because the scientists so often express contempt for philosophy: see ix.d below, and 16, preamble.)

In general, one needs to decide to what extent such attacks are mere defensive territoriality, and to what extent they have real substance. Such decisions are especially relevant if one claims that interdisciplinarity is crucial—which is a large part of the message of this book.

f. A change of tack

The lack of computer power was partly responsible for Marr's unexpected change of direction in the early 1970s. Instead of following up his seminal papers on the brain, he turned to consider the retina and primary visual cortex instead. He studied these until his premature death (aged 35), from leukaemia, in 1980.

His computational theory of vision was outlined in Chapter 7.v.b–d. Here, what's of interest is how he related it to neuroscience.

His first paper on vision, in 1974, aimed to explain how the primate retina computes subjective lightness—which is almost independent of the objective illumination. His Abstract stated that "The operation of the midget bipolar–midget ganglion channel is analysed in detail, and a functional interpretation of various retinal structures is given" (D. C. Marr 1974*a*: 1377). And his paper ascribed distinct computational functions to (for instance) the small stratified amacrine cells (unistratified and bistratified); the diffuse amacrine cells (narrow-field and wide-field); the amacrine/bipolar synapses . . . and so on.

So far, so familiar: this is just what one would expect from the author of the papers discussed above. And here too, Brindley had part-inspired him (D. C. Marr 1974*a*: 1379). But Marr's intellectual strategy was already changing.

Instead of starting with a description of the retina *as such* (compare: cerebellum), he started with a lengthy mathematical discussion of the abstract problem of computing lightness. Next, he asked, "Is this method relevant to retinal function?" Only then did he offer a section on 'The Anatomy of the Retina', before making various predictions about the functions of the retinal circuitry. Possibly, this 'mathematical' strategy put the experimenters off: not one contemporary reference to Marr's lightness paper is recorded in the Science Citation Index (Vaina 1991: 226).

From then on, the abstract definition of visual computation increasingly took precedence in Marr's work. The neuro-anatomy, by contrast, almost faded into the background.

Marr explicitly defended his new approach. He did this first (at a vision meeting in December 1973) in terms of two 'Levels of Understanding', and later (in a 1976 grant proposal and in his book of 1982) in terms of his three-level theory of psychological explanation *in general*: see Chapter 7.iii.b. (Perhaps it wasn't a totally new approach: Willshaw points out that the hippocampus paper had havered between *Any such theory must be like this* and *It must be like this, because the hippocampus is like this*—Willshaw and Buckingham 1990.)

Marr's final verdict on the lightness paper was that the neuroscience was mistaken (it turns out that lightness is computed in visual cortex: Zeki 1993), but that the methodology was sound. As he put it: "[It] showed the possible style of a correct analysis. Present is *a clear understanding of what is to be computed, how it is to be done*" (D. C. Marr 1982; italics added)—where the "how" was computational, not neurological.

The turning point—away from neuro-anatomy, towards abstract computational analysis of the task—dated from May 1972. That was when Marr first met Minsky and Papert, an engineer and mathematician then doing pioneering work in GOFAI in MIT's AI Laboratory (see Chapter 10.i.g, ii.a, and v.f).

The occasion was a small interdisciplinary workshop on Marr's theory of cortex, organized at Boston University's School of Medicine by the physiologist Benjamin Kaminer. The participants included many biological luminaries, such as Barlow, Hubel, Wiesel, Kuffler, Crick, and Brenner (and Blomfield, too). But Kaminer remembers that Marr spent much of his time with Minsky and Papert (Vaina 1991: 312).

Their research was very different from his, and they had scant respect for connectionism in general (see Chapters 10.iv.b and 12.iii). The latter point was perhaps a 'plus' for Marr (see below). But, by his standards, they knew next to nothing about the brain. Nor could they outdo him in mathematics, despite their mathematical expertise. So what could they possibly teach him?

The pivotal moment, according to Tomaso Poggio (personal communication), occurred when Marr took a few hours off to give a seminar on the cerebellum at MIT. Minsky remarked from the floor that we have to think about "the [abstract] problem of motor control" before we can ask the right questions about the cerebellar hardware. Marr later described this to Poggio as a crucial insight. For it would underlie his definition of the "computational" level of explanation as psychologically basic (Chapter 7.iii.b).

In addition, Minsky and Papert together persuaded him that computer simulation could help develop and test computational theories. This persuasion was a classic case of business mixed with pleasure. Crick recalls that "The boat trip after [Kaminer's] meeting was David's Road to Damascus . . . He became converted to AI and before long moved to MIT" (Vaina 1991: 314).

Just as Saul already knew of Jesus before his Damascene conversion, so Marr already knew of AI—indeed, Uhr and Vossler's work had been one of only ten citations in his cerebellum paper (see above). Moreover, machine translation and other aspects of NLP had long been established in Cambridge, England (Chapter 9.x.a–d). But AI simulation in general was much more advanced in Cambridge, Massachusetts.

So, in 1973, Marr left the one Cambridge for the other. Initially, he'd intended no more than a three-month visit. In the event, he never left: in 1976 he was officially appointed to the Department of Brain and Cognitive Sciences. (While at MIT, he soon added a passion for flying to his long-standing passion for the clarinet.)

Minsky's remark about "the problem of motor control" had prompted Marr to reassess his own earlier work. In December 1973, he wrote to Brindley:

> I do not expect to write any more papers in theoretical neurophysiology—at least not for a long time; but I do not regard the achievements of your 1969, or my papers as negligible. At the very least, they contain techniques that anyone concerned with biological computer architecture should be aware of, and I shall be very surprised if my 1969 or 1971 papers turn out to be very wrong. (quoted in Vaina 1991: 2)

Why had he given up on "papers in theoretical neurophysiology"? Writing to a potential translator at much the same time, he said:

> It would be fun to have some of [my early papers] translated into Russian. My present opinion of my earlier work is, however, that even if it is correct it does not take one much further in the study of how the brain works than, for example, the study of more obviously physical phenomena like . . . the conduction of nervous impulses. The reason why I believe this is that this part of my work has to do more with computer architecture than with biological computer *programs*! I have studied how some basic "machine-code" instructions can be implemented in nervous tissue; but these studies tell you rather little about how the brain uses these facilities—e.g., what is the overall structure of a particular motor program for picking an object up, or for throwing a ball. *It is the second kind of question that I am now interested in.* (quoted in Vaina 1991: 3)

Having moved to MIT's AI Laboratory, Marr soon abandoned the problem of motor control for the problem of *vision* instead. Another letter to Brindley (in October 1973) declared:

> I turned to vision when I arrived here, hoping that insight into the functions you had to perform to recognize something, together with the detailed neurophysiological knowledge and an unexcitable disposition, would be capable of illuminating many questions that are surely not yet vulnerable to the microelectrode. (quoted in Vaina 1991: 2)

The switch to vision was influenced by his MIT colleague Berthold Horn, who was then working on the (mathematical) computation of lightness. Marr's transitional paper (1974*a*) opened with a discussion of Horn's algorithm, and the claim that if it were implemented in the retina that would "make sense" of many of the known biological facts.

That paper was "transitional" not only in (still) saying a good deal about the neuroanatomy, but also in preceding Marr's second switch: from lightness to stereopsis. This interest was grounded in recent neurophysiological work by Barlow, and in Bela Julesz's studies of random-dot stereograms (Barlow *et al.* 1967; Julesz 1971). It owes something also to Parvati Dev, although her model of stereopsis was—dismissively—cited only in a footnote at the end of the paper (J. A. Anderson and Rosenfeld 1998: 231).

Even at MIT, however, Marr was initially frustrated by lack of computer power. In an early lab report on stereopsis, he said:

> [complex parallel algorithms] are very expensive to simulate, and it is extremely difficult to derive analytically, from a system with complex non-linear components, quantities that could be measured experimentally. (D. C. Marr 1974*b*: 232)

He discussed various ways in which different 'families' of theories based on stereo disparity might be implemented. But he couldn't actually implement them. (Computer models of scene analysis, by contrast, had been up and running in the AI Lab for some time: Chapter 10.iv.b.)

A few years later, that changed. Now, he was able—with Poggio, then based in Tübingen—to implement two powerful theories of stereopsis (D. C. Marr and Poggio 1976, 1979). And these papers, unlike the one on lightness, received enormous attention from experimentalists. They formed the core of an influential research programme on vision that continued up to—and after—his death, and which spread around the world (see Chapter 7.v.f).

Although Marr did relate his final theory of stereopsis to visual neuroscience, it hadn't been *driven by* the biology, as his cerebellar theory had been. On the contrary:

> We feel that an important feature of this theory is that *it grew from an analysis of the computational problems* that underlie stereopsis, and is devoted to a characterization of the processes capable of solving it *without* specific reference to the [neural] machinery in which they run. (D. C. Marr and Poggio 1979: 324; italics added)

Previous theories of depth vision, said Marr, for all their ingenuity in dreaming up algorithms, were fundamentally wrong-headed: "not one of them computed *the right thing*" (D. C. Marr 1982: 122; italics added). As for connectionism in general, much of it was useless for psychological purposes:

> Again, the primary unresolved issue is *what* functions you want to implement, and *why*. In the absence of this knowledge, a neural net theory, unless it is closely tied to the known anatomy and physiology of some part of the brain and makes some unexpected predictions [like his own early work], is of no value. (D. C. Marr 1975*c*: 876)

Evidently, Minsky's lessons—in the boat and the seminar room—had been well learnt. Neuroscience needed cognitive science, not just the other way around.

And people listened. It was Marr above all who made this point clear—both in his work on vision and in his models of the brain. In a glowing obituary of his younger friend, Longuet-Higgins put it like this:

> If neurophysiology was *a theoretical vacuum* when he entered it, it is now seething with lively controversy about the validity of his ideas on the visual system . . . Even if no single one of Marr's detailed hypotheses ultimately survives, which is unlikely, the questions he raises can no longer be ignored and the methodology he proposes seems to be *the only one that has any hope of illuminating the bewildering circuitry of the central nervous system*. (Longuet-Higgins 1982: 992)

14.vi. Realism Rampant

While Marr was formulating his theory of associative memory in the 1960s, an even more data-driven approach was already being developed by Grossberg. He combined neuroscience with an extraordinarily wide range of findings from experimental psychology. Indeed, he started from the behavioural data and brought in the neuroscience to explain it.

(Grossberg's technical work isn't easy to read. His neologisms don't help: for instance, he speaks of "syncytia" and "subsyncytia" where others would speak of networks, neurone pools, or cell assemblies. For an especially accessible summary, see his *American Scientist* article: 1995.)

a. A voice in the wilderness

Grossberg had first written about his project in 1957, as a freshman at Dartmouth—only one year after the famous Dartmouth summer gathering (see Chapter 6.iv.b). As a graduate student at Stanford in the late 1950s, he'd circulated manuscripts containing many detailed hypotheses about integrated brain processes. These even contained an equivalent of the Marr–Albus rule—and also, in what Grossberg termed his Generalized Additive Model, a near-equivalent of what were later named "Hopfield" nets (12.v.f).

Unlike Marr, however, Grossberg had to wait a long time for recognition. By the late 1980s, he was being described as "one of the most visible scientists working in neural networks for nearly twenty years"—that is, from about 1970 (J. A. Anderson and Rosenfeld 1988: 243). But he'd been doing important work since the late 1950s. Between then and the early 1970s he was treated as an eccentric, though brilliant, outsider. Hence Paul Werbos's remark (quoted in Chapter 12.v.g): "We were not people they trusted, neither Steve nor I."

He had great difficulty in getting his early papers published. Although a 500-page monograph appeared from the Rockefeller Institute for Medical Research, where he was a newly arrived graduate student (Grossberg 1964), journal editors proved less amenable. For instance, it wasn't until 1980 that a paper appeared in *Psychological Review*. Before then, they'd been sent back to him without review, as not being the sort of thing the journal published.

One problem was the relentless interdisciplinarity. The bristling mix of psychology, mathematics, neurophysiology, and neuro-anatomy—though not, yet, computing: see below—would have daunted all but the most eclectic cybernetician (see 1.iii.h). Another was the very richness of Grossberg's fare. He provided so many novel ideas in a single paper that any one risked being obscured by the others.

Another hurdle, Grossberg recalls, was the low value that psychologists in the 1950s–1960s gave to abstract mathematical argument:

> One had to function primarily as an experimentalist. Even Bill Estes, I was told, had a lot of trouble getting his modelling papers published at first, even though he was already a distinguished experimentalist. (J. A. Anderson and Rosenfeld 1998: 174)

Much as Marr's early brain models seemed merely "speculative" to empirically minded biologists, so Grossberg's early theories of mental processing—despite being grounded in behaviour—failed to impress the psychologists.

It didn't help that the mathematics was taxing. ("The problem is that, although I would often have an idea first . . . I would develop it too mathematically for most readers"—J. A. Anderson and Rosenfeld 1998: 179.) In any one paper, Grossberg would offer many interacting non-linear differential equations—with no way, at that time, of leaving a computer to do the sums.

He provided proofs, to be sure. But some of these were even more difficult than Rosenblatt's proof of the convergence theorem (see Chapter 12.ii.f). Many readers simply didn't believe them. And his qualitative predictions of the system dynamics and observable behaviour which, he said, would result from his equations were far from obvious to most people. Often, they were counter-intuitive.

Even Stanislaw Ulam and his fellow physicists at Los Alamos, who implemented the equations on their huge computer in 1966, had been sceptical initially. Grossberg, again: "At first they didn't believe the theorems either, but then they ran the networks on the computer, and the simulations did exactly what the theorems said they should" (J. A. Anderson and Rosenfeld 1998: 179).

(Two decades earlier, machine demonstration wouldn't have convinced Los Alamos. In 1944 the newfangled IBM punched-card machines weren't trusted by the scientists designing the implosion atom bomb, and their wives were asked to do the relevant sums using mechanical hand calculators: MacKenzie and Spinardi 1995. Even the EDSAC, a few years later, made numerous errors because of overheated valves, so calculations had to be repeated several times.)

It wasn't only the mathematically inept who wilted. As we saw in Chapter 12.v.g, Grossberg's undergraduate dissertation—which he'd sent, unsolicited, to numerous renowned researchers in 1965–6—"perplexed" the leading mathematical psychologists at Stanford. They realized that there was a good mind there, and invited him to join them as a graduate student accordingly. But they recognized the work's true quality only much later.

The key difficulty was that *the general nature* of Grossberg's mathematics was unfamiliar, quite unlike what was typically used by psychologists or neurophysiologists at the time. Specifically, it was "nonlinear, nonlocal, and nonstationary" (1980: 351).

Put another way, he was discussing complex dynamical systems, adapting from moment to moment—not chains of atomistically conceptualized events (Markov processes, for instance). Dynamical approaches in cognitive science came to prominence from the late 1980s on (see viii–ix below; 15.viii.c, ix, and xi.b; and 16.vii.c). In the late 1950s, when Grossberg circulated his first work, dynamical systems theory was distinctly outré in psychological circles. So the researchers who'd invited him to Stanford didn't actually take up his ideas about additive networks. Instead, they continued working on stimulus sampling and Markov chains, the preferred intellectual approach at that time (Chapter 6.i.a).

Even as an undergraduate, Grossberg addressed virtually every problem area in psychology. The breadth of his ambition would be evident in the subtitle of his 1982 book: *Neural Principles of Learning, Perception, Development, Cognition, and Motor Control*. In his eyes, these are not utterly different phenomena, grounded in independent modules somehow bolted together in the brain. Rather, they are emergent characteristics of a shared body of fundamental processing principles, which generate and stabilize a hierarchically structured connectionist network. This didn't make things easy for academics trained in narrowly defined specialist areas.

In effect, then, he was trying to keep Rosenblatt's promises (see Chapter 12.ii.e–f). But this was controversial, as well as ambitious. For Rosenblatt had made many of those who *did* value mathematical analysis distinctly wary of any sort of connectionism (12.iii).

Even broadly sympathetic brain scientists such as Cowan were deeply doubtful about Rosenblatt's ambitious claims. And in the computational psychology of the 1960s–1970s GOFAI was the preferred approach. Grossberg attracted scepticism accordingly. Today, he's lauded as one of "a few hardy researchers" who persevered with neural-network models (O'Reilly and Munakata 2000: 9). Then, he was seen as a maverick.

Even so, MIT appointed him Assistant Professor of Applied Mathematics in 1967 (when he still hadn't completed his Ph.D.), and Associate Professor in 1969. He was on the conventional tenure track . . . until the unexpected recession of the early 1970s put MIT into financial difficulty. Because of OPEC's sudden hike in the oil prices in 1973, plus political influence from the counter-culture, there was less R & D money available in the USA than before (see 11.i.b).

Since MIT was hugely dependent on R & D funding, it was hit harder than most universities were. Berthold Horn recalls it as a time when "There was almost no funding for a while" (quoted in Crevier 1993: 117). One result was that almost all of the untenured junior faculty were unexpectedly let go, as the euphemism has it.

In this context, a knight on a white charger—or anyway, a knight—championed Grossberg in 1973. Sir James Lighthill, whose recent critique of GOFAI had set back AI research in the UK and cast a shadow also in the USA (11.iv.a–b), was asked by MIT for his opinion on Grossberg's work. As the Lucasian Professor of Applied Mathematics at Cambridge, he would understand the material if anyone could—and it was clear that many couldn't.

MIT had approached Lighthill because they were bemused by the extraordinarily mixed response to their first call for references. As Grossberg remembers it:

> [When asked whom to approach for recommendations] I naively gave them a list of about fifty names of distinguished people across the fields of psychology, neuroscience, and mathematics. I got a very wide range of letters. A number of letters said I deserved a Nobel prize and I am a genius. I also had other letters that said, in effect: "Who the hell does he think he is trying to model the mind?" (J. A. Anderson and Rosenfeld 1998: 182)

Lighthill's answer to the query, says Grossberg, was "a glowing three- or four-page letter which basically said that I was doing exactly what AI should have done".

But to champion isn't necessarily to rescue. Despite this encomium from MIT's chosen court of appeal, they still didn't offer him tenure. His work was felt to be too controversial, given the unhappy financial climate, and he was warned that he'd soon have to leave. However, he was rescued—not by another knight but by Boston University, who offered him a full professorship. He left MIT for Boston in 1975.

Soon after Grossberg's arrival there, he founded the interdisciplinary Center for Adaptive Systems (CAS)—where he remains today (2006). The Boston group covered a wide spectrum, for he insisted that every CAS member be trained in at least three of four disciplines: computer science, mathematics, psychology, and neurobiology.

Much of his later work was done in collaboration with his wife, Gail Carpenter, a leading researcher in her own right. Her first work had involved mathematical analyses of single cells, using the Hodgkin–Huxley equations (J. A. Anderson and Rosenfeld 1998: 200 ff.), but her interests soon broadened. Like him, she was invited as one of only fourteen opening speakers at DARPA's crucial workshop on connectionism in 1987 (12.vii.b).

By that time, clearly, he was accepted as a leader in the field. Indeed, he'd just become the first President, and co-founder, of the International Neural Network Society (12.vii.a). The wilderness had been left far behind.

b. Adaptation—and feature-detectors

What Grossberg was doing from the start, and what Lighthill thought AI should have been doing, was explaining mind and behaviour by bringing in the brain. Starting from behavioural data, he looked for neurophysiological explanations. And he tried to do this as *realistically* as possible. Toy problems were avoided: the focus was on "problems requiring *real-time adaptive* responses of individuals to *unexpected* changes in *complex* environments" (Grossberg 1988, p. viii; italics added). Blocks world, this was not.

He also tried to do it as *parsimoniously* as possible. He spoke of "minimal anatomies", meaning the simplest neurological model that could account for the observed behavioural constraints. A few general principles—i.e. mathematical theorems, some of which he saw as "particularly simple and lucid" (1976*a*: 253)—were used to explain a profusion of empirical facts. Many superficially diverse (and seemingly unrelated) aspects of behaviour were unified, as unexpected emergent properties of an underlying dynamical system.

The data were drawn from psychology, Grossberg's first love as an undergraduate at Dartmouth, and from various levels of neuroscience. Besides explaining various *psychological functions* in terms of common processing principles, he aimed to show how a variety of *neural structures* can emerge from the same underlying neurological source. That is, he wasn't merely looking to current neuroscience for ideas that would explain behaviour. He was also looking to behaviour—in particular, to the way in which the organism adapts moment by moment to a changing world—to suggest hypotheses about as yet unknown neural mechanisms.

With hindsight, one can list many experimental confirmations of biological predictions made by his ART computer model (see below). For example, since 1988 there have been at least a dozen confirmations of his claim that attention and concept matching involve a top-down modulatory on-centre/off-surround network. Some of these, though not all, were experiments done by his Boston team. So he can surely escape Gross's charge (above) about "theoreticians" having contributed nothing to biology.

Consider, for instance, what he said about feature-detectors. By the mid-1970s, neurophysiologists—including Barlow (Barlow and Pettigrew 1971)—had discovered that even if cortical orientation detectors are in some sense innate, they aren't genetically *determined*. (People didn't yet realize that the fact that newborn kittens already possess vertical/horizontal detectors *does not* prove that these must be coded in the genes: see Section ix.a.)

Recent experiments had shown not only that normal experience fine-tunes the detectors already present at birth, but also that unusual visual input after birth can produce corresponding types of detector or, through disuse, destroy them. The relevant differences in *behaviour* had been noted in the 1960s, but now people were studying the neurology. If a newborn kitten wears goggles that present it with only vertical lines in the right eye and only horizontal lines in the left, the orientation cells in its brain will develop/atrophy accordingly. In a word, even low-level feature-detectors can be *learnt*—and unlearnt.

Grossberg asked how this is possible, and why the visual cortex has the columnar structure that it does. Tellingly, his answers were published, soon after his arrival at Boston, in a journal of cybernetics, not neuroscience (Grossberg 1976*a*,*b*). He described a form of Hebbian learning, or synaptic modification, justified by proofs of mathematical theorems. This two-part paper was built on the account of brain processing he'd been developing for almost twenty years—and it graduated from "additive" to "adaptive resonance" theory (see below).

The first part of the paper concentrated on feature-detectors, and offered a functional classification of the different types. Grossberg defined equations that would enable single cells in the cortex to learn different patterns, having compensated for various types of real-world noise. They might do this by enhancing contrast in the input, for instance, or by computing relative rather than absolute light intensities.

He added that the equations implied that cortical cells learning similar patterns would become grouped together. He indicated how the range, or degree of resolution, of coded similarities would depend on the numbers of neurones involved (Chapter 12.v.c). And he argued that pattern-discriminators can be hierarchically organized—as in Hubel and Wiesel's hypercomplex cells (which he mentioned specifically—1976*a*: 250), Gross's monkey's-hand-detectors, and perceptual concepts in general. In addition, he offered some testable hypotheses about the role of the retinal amacrine cells.

Characteristically, the paper used powerful mathematics inspired by specific findings in neuroscience. For example, it relied on S-shaped (sigmoid) learning rules, which modelled the cells' responses at high/low extremes of stimulation. In thus avoiding over(under)shoots, this mimicked some properties of biological neurones—and other cells, too.

Grossberg used sigmoids for a reason. He'd published a theorem in 1968 which proved that sigmoid learning enables cells both to suppress noise and to enhance contrast sensitivity, thus avoiding what he called the "noise-saturation dilemma". The idea was later used by the PDP connectionists in two famous papers (McClelland and Rumelhart 1981; Rumelhart *et al.* 1986*a*,*b*).

The proof concerning sigmoid cells wasn't a stand-alone result, but part of an analysis of *all* the ways in which cells could signal one another. That analysis also showed that *on-centre/off-surround interactions* (between cells that obey the membrane equations describing neurones) are *essential* to avoid the noise-saturation dilemma. In other words, it's no accident that the nervous system contains so many on-centre/off-surround units, and so much lateral inhibition (so Hartline had discovered cells whose existence was largely explained by the mathematics of system dynamics: see iii.c, above). Accordingly, Grossberg's model of pattern learning simulated centre–surround cells and lateral inhibition to increase the contrast in the input.

Third, the information that modified the simulated synapses wasn't carried by single responses, as in the networks of Chapter 12. Instead, it was coded by changes in a cell's rate of firing, averaged over small amounts of time. This reflected the neuroscientists' long-held conviction that the *frequency* of neuronal firing is crucial for coding (see Sections iii.b and ix.g). Again, Grossberg's learning rules—which defined recurrent networks (12.viii.b)—distinguished between short-term and long-term memory.

Fifth, some of his equations presupposed facts about local connection structures in the retina and the brain. And last, but by no means least, the model showed how a pattern can be *distributed* over a large number of individual neurones.

All very intriguing—at least, if one could understand it. But as we've seen, at first many couldn't. So why should they believe him? ("Who the hell does he think he is . . . ?") How could they be persuaded that his theory really did imply what he said it did? Perhaps it was just hot air?

Well, no. For another computer model, besides the Los Alamos experiment, had already suggested there was gold in the Grossberg hills. This model, which Grossberg commented on in his 1976 paper, was focused on feature-detectors. It was inspired by additive theory, and in particular by some equations that Grossberg had published, and criticized, early in the decade (Grossberg 1972). Its designer was Christoph von der Malsburg (1942–), who'd implemented it a few years earlier (von der Malsburg 1973).

Von der Malsburg adapted Grossberg's equations in building a two-layer connectionist network that modelled the development of striate cortex from a partly random starting point. Only "partly" random, because the system started off with more local structure than the networks discussed in Chapter 12: as remarked above, Grossberg's theory presupposed certain facts about brain connectivities. The results were startling.

When the 162-unit "retinal" layer was repeatedly presented with lines in various orientations, the 338-unit "cortical" layer gradually organized itself into a two-dimensional structure that matched Hubel and Wiesel's descriptions of visual cortex to a remarkable degree. Units that responded to the same orientation were grouped together in clusters. In addition, neighbouring units responded to similar orientations, so that the clusters were systematically arranged across the "cortex": see the seventh line down in Figure 14.2.

These results ensued even though the input had contained significant amounts, and interacting types, of noise. (This model *illustrated* the emergence of ocular columns rather than *explaining* it: a few years later, von der Malsburg (1979) gave an explicit analysis.)

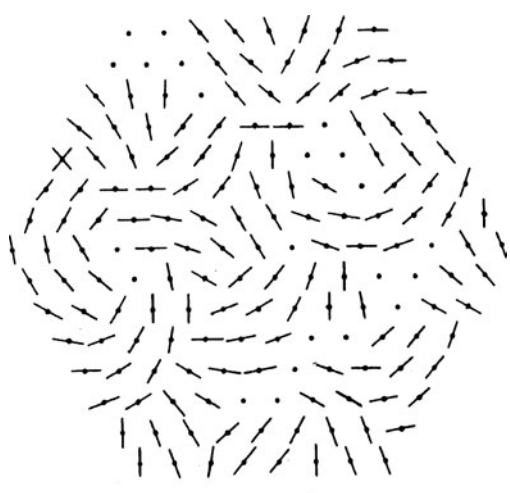


Fig. 14.2. View onto the [simulated] cortex after 100 steps of learning. [Each small line shows the orientation to which the relevant unit responds most strongly.] Reprinted with permission from von der Malsburg (1973, fig. 13)

Moreover, von der Malsburg found that non-standard stimulation had 'neuro-anatomical' effects comparable to those which occur in real animals. It's evident from Figure 14.1 that the input for the standard learning history depicted there had ranged across many different orientations. When the network's input was restricted to only vertical or only horizontal stimuli, the resulting orientation detectors were skewed and/or absent accordingly. And this skewing became irreversible after a certain "critical" period. Both these results had recently been observed in newborn kittens (C. Blakemore and Cooper 1970).

(As we'll see in Section viii.a, Von der Malsburg was also working on a model of neural self-organization *in the embryo*, prior to any perceptual experience: Willshaw and von der Malsburg 1976.)

This simulation proved that Grossberg's account, at least in its modified form, wasn't merely hot air. However, it didn't model every aspect of additive theory—never mind its later substitute (Grossberg 1976*b*). For instance, von der Malsburg used only very simple stimuli, namely motionless straight lines, whereas Grossberg had also discussed more complex cases.

Indeed, Grossberg was arguing—as always—in highly general (i.e. theoretically parsimonious) terms. His equations, he said, fitted many different parts of the brain, including olfactory, auditory, and cerebellar cortex, and also the hippocampus. They were part of a general psychophysiological theory he applied also to attention, arousal (including consciousness), analgesia, motor action, communication, reasoning, and psychopathology.

Even an outline simulation of that would require a much more ambitious computer model.

c. ARTful simulations

Together with his wife, Grossberg eventually produced one. They called it ART-2 (Carpenter and Grossberg 1987).

This system learnt to recognize input patterns, both binary and analogue, in real time and despite noise. The categories involved could be familiar, near-familiar, or novel, and they could self-organize on several hierarchical levels. Where GOFAI systems at that time were testing pre-set perceptual hypotheses (see Chapter 10.iv.b), ART-2 formed, learned, and stabilized its own. Moreover, it did this in real time, even after having learnt many different categories, because learnt categories could be accessed *directly*.

ART-2 was based on adaptive resonance theory, introduced in Grossberg's 1976*b* paper. The core idea was this:

> The functional unit of cognitive coding [is] an adaptive resonance, or amplification and prolongation of neural activity, that occurs when afferent data and efferent expectancies reach consensus through a matching process. The resonant state embodies the perceptual event, or attentional focus . . . (Grossberg 1980: 1)

The "expectancies" concerned included not only what we'd normally call expectations, but also predictions, questions, goals (intentions), and even learnt or 'instinctive' motor patterns such as those modelled by Kilmer (see Section iv.a).

On this theory, activity patterns are passed between two networks until they match. At that point, they stabilize. Inputs are held in STM, and compared with previously learnt categories (“codes”) in LTM. Near-matches lead to refinement of the pre-existing codes. If there’s no match, previously uncommitted LTM (if any) is used to encode the pattern currently held in STM.

This way of putting it may suggest that the patterns are passed *to and fro* between the networks, which could take a long time. But ART-2’s activity patterns oscillated at most once, and self-stabilization was mathematically guaranteed. Unlike the networks described in Chapter 12, it didn’t require repeated presentations of the input pattern, but “quickly learned to group fifty inputs into thirty-four stable recognition categories after a single presentation of each input” (Carpenter and Grossberg 1987: 151). It did this by using fast parallel search, the nature of which changed adaptively during learning.

ART-2’s predecessor, ART-1 (now there’s a surprise!), could handle only binary input patterns. Ideas from ART-1 had been borrowed by a number of fellow connectionists for use in their own work. ART-2 was less borrowable, because its many-levelled analogue-friendly representations were much more complex.

Nevertheless, patents were taken out on both systems, and on their successors ART-3 and ARTMAP. By the late 1990s “a lot of people” were using them for industrial R & D (J. A. Anderson and Rosenfeld 1998: 190). Whereas ART-1 and ART-2 dealt with perceptual categories, the later systems went beyond these to include aspects of attention, goal seeking, and motor action.

One might describe ART theory as an updated, and enormously complexified, version of TOTE units (Chapter 6.iv.c), in the sense that the core idea was *testing*:

> [Within] an ART system, adaptive pattern recognition is a special case of the more general cognitive process of discovering, testing, searching, learning, and recognizing hypotheses. Applications of ART systems to problems concerning the adaptive processing of large abstract knowledge bases are thus a key goal for future research. (Carpenter and Grossberg 1987: 152)

In other words, Barlow’s “incautious” suggestion about intelligence was being deliberately taken up (see Section ii.b).

Strictly speaking, ART-2 wasn’t “one” model, but a general class of models. So were ART-1, ART-3, etc. For the neuropsychological theory was expressed as a set of differential equations, whose variables could be instantiated in different ways and where one or more could be experimentally dropped, or “ablated” (pp. 154–5).

Grossberg’s theory had initially been developed without any reference to computers, as we’ve seen. When he arrived at Stanford in 1961, the leading simulator there tried to model his theory, but failed (personal communication). Later, when suitable technology became available, Grossberg turned to simulation himself. Indeed, he not only used that methodology, but proselytized about it.

Besides helping to clarify one’s theory, he said, it can help advance it. For it prompts thinking about how the data could arise as “emergent properties of a real-time process engaged moment-by-moment by the external environment” (1988, p. viii). One-off simulations, however, are irrelevant. (Compare Drew McDermott’s critique of one-off programs in GOFAI: 11.iii.a.) Copious, and systematic, modelling is required:

Only through the sustained analysis of many hundreds or even thousands of such experiments can one accumulate enough data constraints to discard superficial modelling ideas and to discern a small number of fundamental design principles and circuits. (Grossberg 1988, p. viii)

Through this approach, "a series of design paradoxes, or trade-offs, come into view which balance many data and computational requirements against one another".

Neuroscience in particular, he insisted, needs computer simulation:

The dynamics of large ensembles of neurons are as yet difficult to observe directly. Even if direct observation were possible, it would not explain *how* the interactions among neurons generate the emergent properties that subserve intelligent behavior. Additional methodologies are needed to investigate *how* the collective properties of a neural network are related to its components. Computer simulations of neural networks are crucial tools in the current explosion of work in brain science.

... Once a mathematical model of neural functioning is formulated, [it may be possible to prove] theorems concerning [its] stability or convergence behavior ... [However, neural processes] involving large and hierarchically organized systems of nonlinear ordinary differential equations are characteristically difficult to analyse through purely formal procedures. For these systems there may be no way to determine the output of the model when given a certain input short of "running" the model in a numerical computer simulation. Thus "experiments" can be run on a model, in ways that are similar in some respects to experiments run on human or animal subjects. (Grossberg and Mingolla 1986: 195; italics added)

This method enables neuroscientists to do things they simply couldn't do before:

Simulations can be cheaper and faster [than experiments on organisms], but more importantly they permit a much more precise level of control of many variables than could ever be realized in physiological or behavioral experimental paradigms. [They can even lead to] general design insights which can at times be formalized into mathematical proofs that would otherwise have been difficult to discover. (Grossberg and Mingolla 1986: 196)

In short, computer modelling can promise significant results that wetware experiments can't achieve.

Why did he spell this out at such length, as late as 1986? After all, cognitive scientists had been saying this sort of thing for years. The essential point had even been made, with respect to *mechanical* simulation, by some early 1930s behaviourists (see Chapter 5.iii.c).

The reason was that, even in the late 1980s, many *neuroscientists* were still suspicious of computer-based methods.—Computational (information-processing) questions, yes; computer models of the brain, *No!*—James Anderson recalls that at Rockefeller University in the early 1970s, "psychologists were far more open and responsive to new ideas [i.e. connectionism] than were neuroscientists" (J. A. Anderson and Rosenfeld 1998: 254). Similarly, Arbib found in the mid-1980s—when Grossberg was writing the words just quoted—that "the experimentalists in neuroscience, instead of being excited at having a distinguished program of brain modelling on campus [UMass, Amherst: see Section vi.b–c], saw it as either irrelevant or threatening" (J. A. Anderson and Rosenfeld 1998: 229).

Most brain scientists today are less suspicious (although Gross's comment on Marr smacks of the old school: v.e, above). They're increasingly using computer models in their work. Indeed, virtually all neuroscience—excepting nitty-gritty neurochemistry and theoretically unmotivated brain scanning (see Section x.b)—now involves computational questions about brain function, whether or not simulation is involved. This applies to work on feature detection, cognitive maps, motor control, action selection, language, schizophrenia . . . and consciousness.

As Arbib has remarked, his computationally oriented *Brain Theory Newsletter*, which attracted only a few hundred subscribers in 1980, would be selling in the tens of thousands were it still running today (J. A. Anderson and Rosenfeld 1988: 231). (In fact, it was incorporated into the journal *Cognition and Brain Theory* in 1982: see Chapter 6.v.c.)

d. Avoiding the black box

Inevitably, neuroscientists' computer models will become increasingly realistic. But there's a danger here. Lighthill commended the young Grossberg to MIT for doing what he thought AI should have done: taking account of the brain. The message of his notorious Report (Lighthill 1973) was that biological realism is a Good Thing—and, by implication, the more of it the better. But that implication was questionable.

Even in the 1940s, Ashby (1948: 383) foresaw that advanced versions of the Homeostat might behave in ways "too complex and subtle for the designer's understanding" (see Chapter 4.vii.d). Similarly, von Neumann predicted at the Hixon Symposium that complex automata would be almost as mysterious to their builders as animals are to biologists (Jeffress 1951: 109–10). And a widespread worry in the late 1980s, as we saw in Chapter 12.vi.f, was that large connectionist systems, once they became available, would be unintelligible.

Given enough computer power, one can stuff extra data parameters into one's simulations indefinitely. But if the result is in effect a black box, the exercise has scant scientific value. In the mid-1980s, one British computer scientist put the worry like this:

> Connectionism and its second coming is the next unavoidable issue [for AI] . . . [If] subsymbolic networks are necessary to support intelligent systems, then the conceptual transparency of the resulting AI systems is likely to be *on a par with that of the brain—i.e. somewhere close to zero*. Homogeneous networks of subsymbolic elements will severely test our abilities to understand our models. So if subsymbolic network architectures are in some sense necessary then that might be bad news for AI. (Partridge 1986–7: 16; italics added)

Similarly, Willshaw, in defending early Marr against charges of oversimplification, has pointed out "the danger that any completely specified model will become just as difficult to analyse as the brain itself" (Willshaw and Buckingham 1990: 116).

How does Grossberg's research stand up to the charge of black-boxery? It must be said at once that his work appears forbiddingly complex to non-specialists. Consider this statement of his commitment to realistic modelling, for instance:

> We [at CAS] typically begin by analysing a huge interdisciplinary data-base within a prescribed problem area. In our work on preattentive vision, for example, we have studied data from many parts of the vision literature—data about boundary competition, texture segmentation, surface perception, depth perception, motion perception, illusory figures, stabilized images, hyperacuity, brightness and color paradoxes, multiple scale filtering, and neurophysiology and anatomy from retina to prestriate cortex. (Grossberg 1988, p. viii)

All these data from visual psychology feature in a single paper on the perception of colour and 3D form (Grossberg 1987*b*). The same paper also draws on hypotheses about the cortical and retinal cells involved—and the neurotransmitters, too.

But the paper—like other CAS publications—isn't a mere ragbag of empirical facts. On the one hand, it draws many theoretical morals. For example, it argues that "the popular hypothesis of independent modules in visual perception is both wrong and misleading. Specialization exists, to be sure, but its functional significance is not captured by the concept of independent modules" (Grossberg 1987*b*: 4). It allows for learned (largely top-down) as well as automatic (bottom-up) processing in object recognition. It offers a new theory of stereopsis. And it makes many testable predictions.

On the other hand—and even more to the point, with respect to black-boxery—the paper rests on a set of mathematically proven theoretical principles. These unify the huge variety of detailed behavioural constraints simulated in the model.

A more recent publication, which explains depth vision and various visual illusions in terms of highly specific parts of the brain, makes the simplicity beneath the complexity even more evident (Grossberg and Howe 2003). To appreciate that simplicity is to understand the model. And the general principles underlying Grossberg's work are clearly intelligible to some people outside his own group at Boston. For—as we've seen—ideas from ART-1 have been used in other connectionists' models, and ART systems are even being used commercially.

One way of putting all this is to point out that it would be misleading to say that the behavioural constraints (about visual illusions, for instance) are "built into" the ART models—still less, that "extra data parameters" are repeatedly "stuffed into" them. Rather, the behavioural details *emerge* from, are *generated* by, the underlying system dynamics. This didn't happen by magic, of course. The design of the system dynamics—the "minimal anatomies", and combinations thereof—was guided by Grossberg's intuitions about what sort of system could generate those (seemingly unrelated) behavioural data. (Intuitions which, as we've seen, even the Los Alamos physicists didn't share.)

The complexity is due to his commitment to biological realism. That is, to his continual addition of further behavioural and neurological constraints (as in the colour/form and stereopsis examples mentioned above) and, when necessary, further theoretical principles. The intention is to *test and expand* the existing theory, not to *complicate and obscure* it by adding innumerable ad hoc hypotheses. Thus far, Grossberg's team, and a fair number of outsiders too, are still able to see the theoretical wood as well as the observable-data trees.

Even so, this approach is not for the faint-hearted. And it remains to be seen whether, in the new century, the CAS group will ever reach a point where even they can't really understand what's going on. The same applies, of course, to 'realistic' brain modelling in general. Anyone who aimed for a *completely* specified model would risk sacrificing understanding to observation. In Marr's terminology, Type-I explanation would threaten to give way to Type-II (see Chapter 7.iii.b).

In short, biological realism could conceivably go too far—especially if the modeller doesn't build the system up from a theoretically parsimonious base. This is one way of expressing the familiar claim that the brain may be too complex for the human mind to understand it. Whether that's true is still an open question, though we shouldn't stop trying yet.

Future "unintelligible" simulations whose performance successfully matched the neuropsychological data would be existence proofs that the brain isn't essentially *mysterious*. But lack of mystery isn't enough for scientific understanding. (The "mystery" of consciousness involves additional issues: see Sections x–xi.)

14.vii. Whole Animals

Kenneth Craik, in the early 1940s, pointed out the importance of "selection pressures" in evolving internal "models" of the environment (see Chapter 4.vi.b). The interests of the animal as a whole were crucial, he said, in shaping such models. But he could give few specific examples.

A fortiori, he couldn't give neuroscientific chapter and verse in describing the *whole* animal. No one at that time could—and it's not even clear that anyone tried. Thinking about the whole animal, as opposed to specific sensory or motor mechanisms, wasn't a practical aim. Moreover, the environmentalist influence of behaviourism kept ethology out of the mainstream (5.ii.c).

Eventually, that changed. The last four decades of the twentieth century saw the gradual rise of what's now called computational neuro-ethology (CNE), a term coined in the late 1980s. CNE's strongest disciplinary links are with neuroscience, ecological psychology, and A-Life. But it has implications for cognitive science in general (Keeley 2000*b*).

One pioneering example, in which the *general* implications were given especial prominence, is discussed in this section. Further examples—focused on crickets, hoverflies, cockroaches, and lampreys—will be described in Chapter 15.vii. All these CNE programmes have generated novel hypotheses about the nervous system, whether in humans or in other animals.

a. CNE—what is it?

CNE is Computational. It's Neural. And it's Ethology. Ethology considers the whole animal's behaviour in its natural habitat, focusing on species-specific adaptations to a particular environmental niche. Neuro-ethology (named in the early 1980s) studies the underlying neural mechanisms. And CNE interprets these in computational terms.

The earliest CNE work was done in the 1960s. The scientist concerned—namely, Arbib—continued his research and expanded his theory over the next forty years, as we'll see (subsections b–c, below). But there was *no* steady growth in interest within the cognitive science community as a whole.

On the contrary, we'll see in Chapter 15.vii that most of the influential CNE research—Arbib excepted—wasn't done until the late 1980s. Randall Beer's "manifesto" (his word) for the field was published in 1990, soon after Christopher Langton's definition of "Artificial Life" (15.xi.b). Partly because Beer allied himself with the newly emerging A-Life community, CNE was now seen in the context of A-Life as much as of neuroscience.

One might wonder why CNE didn't blossom long before then. After all, ethology as such had been growing since the First World War (Chapter 5.ii.c).

One reason was that ethology was overshadowed by behaviourism for many years. Indeed, hardly any of the early work was translated into English before the late 1950s (C. H. Schiller 1957). The behaviourists focused on general mechanisms, studied in laboratory conditions (5.i.a). They saw Konrad Lorenz's fieldwork, for instance, as birdwatching—not science. This wasn't fair: Lorenz had provided systematic observation and even some experiments, not mere anecdotes. But, the power of fashion being what it is, the ethologists were sidelined.

Another reason was that AI hadn't progressed sufficiently. Robotics, in particular, was hugely difficult. Beer's six-legged cockroach robots, for instance, were a triumph of mechanical engineering no less than CNE. By the mid-1990s, however, the *programmable* Khepera robot—designed by Francesco Mondada in 1992–3—could be bought off the shelf. In addition, VLSI techniques could be used to provide (for instance) speedy sensory processing based on neuroscientific data.

A third reason was that neurophysiology itself wasn't far enough advanced until after mid-century (Chapter 2.viii.e). Moreover, it was typically conducted in a reductionist spirit that lost sight of the animal as a whole.

The 'Frog's Eye' work of the late 1950s is a good illustration. It could be regarded as the first exercise in CNE—but if so, this was more by accident than design. Lettvin and his colleagues were driven to consider the frog *as a frog* only by the surprising feature-detectors they found (see Section iii.a). They hadn't set out with the intention of analysing the visual system in terms of predator and prey.

Similarly, in recalling his own "unheard of" results, Hubel has said: "It is hard, now, to think back [to the late 1950s] and realize just how free we were from *any idea of what cortical cells might be doing in an animal's daily life*" (1982: 516; italics added).

If they'd started from an ethological viewpoint, their findings would have been less of a surprise. Indeed, Lettvin's friend Selfridge had already argued, on computational grounds, that ecologically relevant feature-detectors must exist. But he didn't know which, or where, they were: it was Lettvin's team who put the N into the CNE. And it was a research student in their laboratory who would make that "N" *whole*.

b. A wizard from Oz

One of the many people enthused by the 'Frog's Eye' research was Arbib (1940–), an English-born Australian mathematician who arrived at MIT as a graduate student in 1961. He started his Ph.D. with Wiener (later continuing with the probability theorist Henry McKean). Meanwhile, he worked as a research assistant in McCulloch's laboratory until he left in 1963 to go to Stanford (via Europe and Australia).

This seemingly innocuous association with McCulloch had to be kept from Wiener, who became "apoplectic with rage" when, on Arbib's last day, he discovered it (Arbib 2000: 203–4). His vitriolic hatred of McCulloch has been 'explained' in various ways. McCulloch himself told Arbib a story about having involved Wiener's daughter with his Mafia contacts, for her own protection. This was fascinating, says Arbib, but—as with many of McCulloch's stories—probably largely fiction.

The most likely explanation, Arbib now believes, is that Wiener (in 1952) had asked McCulloch to tell him about the brain, and McCulloch's mini-lecture had (characteristically) contained as much conjecture as fact. When, after three years' work, Wiener triumphantly presented a meeting of neurophysiologists with an elegant mathematical theory accounting for everything McCulloch had told him, he was laughed out of court. He hadn't realized that McCulloch was one of those people whose talk is peppered with speculation dressed up as fact—albeit hugely stimulating, insightful, intelligent speculation. (Turing had realized this, but concluded that McCulloch was "a charlatan": Chapter 4.iii.a.)

Nor did Wiener realize it now. Instead, he assumed that McCulloch had maliciously set him up for this fall. With better personal antennae, he would have avoided the humiliation—and the dreadful decline of the young Pitts, whose life was shattered by the feuding (see 4.iii.d).

Lettvin (1999) mentions "a viciously phrased letter" sent by Wiener breaking all connections with McCulloch's group. And he says:

> It had nothing to do with any substantive cause but was the result of a deliberate and cynical manipulation designed to sever Wiener's connection with McCulloch and his group. The details are not edifying. Wiener was victimized as much as the group. (Lettvin 1999)

With no further information on the "details", or on who was doing the "manipulation", it's not clear whether this angry letter was part-prompted by the events described by Arbib. But that there was a rift, and that the effect on Pitts was (as Lettvin puts it) "devastating", isn't open to doubt.

The newly arrived Arbib was already more than familiar with computational ideas. He'd found superior proofs for McCulloch and Pitts' 1943 paper while still an undergraduate (Chapter 4.iii.f).

He very soon co-authored a paper on neural modelling with McCulloch and Cowan, then a 'mature' graduate student alongside Arbib, for the seminal conference on self-organizing systems in Chicago: see Chapter 12.iii.e (McCulloch *et al*. 1962). And shortly after that, at one of Oliver Waddington's maverick meetings on theoretical biology, he would define a self-reproducing automaton based on von Neumann's ideas (see Chapter 15.v.b). So his maths was in excellent shape: he went to McCulloch to learn neurophysiology.

The prime lesson Arbib took from the lab's work on feature-detectors wasn't a conversion to single-cell recording, but the realization that perception serves the interests of the organism concerned. Presumably, then, it's integrated with the creature's motor mechanisms in ecologically appropriate ways. He was also inspired by the Mars robot RETIC (see Section iv.a), because of its concern with the action of *the whole animal*. Kilmer, in fact, was the junior colleague at MIT who had the most influence on him (and later joined Arbib's brain research group at the University of Massachusetts, Amherst).

In short, Arbib's aim was to progress from 'What the Frog's Eye Tells the Frog's Brain' to "What the Frog's Eye Tells the Frog" (Arbib 1981*a*: 23). But whereas the 'Frog's Eye' author Maturana would soon interpret the whole-animal insight in an anti-computational—and highly unorthodox—way, Arbib stayed within the computational camp.

Indeed, he soon became one of its most prominent champions. In 1964 he published an introductory text on the mathematics of automata theory, including McCulloch–Pitts networks (Arbib 1964). To his amazement—he was only 24, and still a Ph.D. student when he wrote it—it got the lead review in *Scientific American*, and a mention in an article on Wiener in the *New York Review of Books*. He now says this was "not so much because of the merit of the book, but the fact that Wiener had just died, and so cybernetics was a hot topic at that time" (J. A. Anderson and Rosenfeld 1998: 223). However that may be, it sold very well.

Eight years later, he added neuroscientific flesh to the computational bones in a widely read book about the brain, tellingly subtitled *An Introduction to Cybernetics as Artificial Intelligence and Brain Theory* (1972). In effect, this was a declaration of intent for his interdisciplinary brain research group at Amherst, where he went after six years at Stanford.

The project was set back by the drying-up of funds for connectionism in the mid-1970s—and Kilmer even deserted the field as a result. After DARPA's crisis meeting in 1987, however, the funding pendulum swung back again (Chapter 12.vii.b). Arbib soon went to the University of Southern California to direct their Brain Project.

The 1980s were hugely busy years for Arbib, but not hugely influential. As remarked above (Section vi.c), most 'pure' neuroscientists didn't take connectionist modelling seriously—especially before PDP became widely visible at the end of the decade. Arbib's pioneering *Brain Theory Newsletter* was discontinued before the wider research community caught up with him.

Eventually, however, his influence grew. His interdisciplinary research was suddenly 'discovered', as other neuroscientists began to express their sensori-motor theories as computer models—and as whole-animal approaches in general gained ground.

In the 1990s, he was Editor of the *Handbook of Brain Theory and Neural Networks* (with a second edition in 2003). He wrote a paper for the AI community recommending biological design principles in robotics (Arbib and Liaw 1995). And he co-authored a book uniting neuroscience and (animal and human) psychology with the computational theory he'd been developing since the early 1970s (Arbib *et al.* 1997). By the turn of the millennium, he was using the recent discovery of 'mirror neurones' to part-explain the evolution of language in the context of his long-established approach (for his latest statement, see Arbib 2005).

In sum, he's been a key player across the whole of cognitive science for over forty years.

c. *Rana computatrix* and its scheming cousins

To find out just what the frog's eye tells the frog, Arbib and his students (starting with Richard Didday) did some behavioural and neurophysiological experiments on the real animals. Their main activity, however, was developing a computer model—more accurately, a still-continuing *series* of models—of sensori-motor integration in the frog (Didday 1976; Arbib 1972, 1982, 1987, 2002*b*; Arbib and Cobas 1991; Cobas and Arbib 1992; Arbib and Lee 1993; Liaw and Arbib 1993; Arbib *et al.* 1997).

Arbib named it *Rana computatrix*. One might naturally assume (many people did, myself included) that this was in memory of Grey Walter's *machina speculatrix*

(Chapter 4.viii.a–b). And indeed, Arbib had already read *The Living Brain* when he first started work on *Rana*. But he wasn't aware of the connection:

> I had read [Grey Walter's book]. But what was interesting was that I had not consciously realized the inspiration for the name *Rana computatrix* until some years later, when Dan Dennett asked if *machina speculatrix* were the inspiration—and I suddenly realized it surely was! (Arbib, personal communication)

So, like Paul McCartney's unrecognized memory of 'Yesterday' (Chapter 1.iii.g), Arbib was recalling someone else's work—in a quite specific way—without knowing that he was doing so. If he hadn't been asked about it, he might still be convinced of his originality today.

Putting the name of his computerized frogs on one side, what about the ideas implemented in them? To some extent, Arbib's early inspiration came from Valentino Braitenberg (see 15.vii.a). He says now (personal communication) that he was "excited" by Braitenberg's ideas on the cerebellum, and "enjoyed" a paper foreshadowing the book on *Vehicles* (Braitenberg and Onesto 1960; Braitenberg 1965). But the main intellectual prompting, believe it or not, came from SHAKEY (see 10.ii.a and iii.c): "My real interest in robots was developed by the work of Nils Nilsson and colleagues at SRI on SHAKEY, from around the time I was at Stanford" (personal communication).

One of the lessons learnt from SHAKEY, and from his own later experience too, was the enormous difficulty involved in not merely ("merely"?) designing robots, but actually building them. After all, SHAKEY itself had got its name not from any of the influential ideas implemented in it, but from the fact that—considered as a mechanical object moving on the floor—it wobbled (see 10.ii.a).

Accordingly, Arbib's frog model was only ever implemented as computer programs. He did consider building hardware robot frogs, and published a paper on robot design (Arbib and Liaw 1995). But his robots were all simulated, not real. (One of his students, Tony Lewis, did eventually build a robot salamander, with a flexible spine and a GA-generated neural controller: M. A. Lewis *et al.* 1994.)

Amphibia weren't Arbib's only interest. From the early 1970s, he also worked on mammals. For instance, his team modelled stereo vision, and motor control by the cerebellum (Arbib *et al.* 1974). Much later, as we'll see, he widened his scope still further, applying the ideas initially developed with respect to frogs to the highest reaches of human thought.

Arbib's artificial frog carried out highly distributed information processing, so developing McCulloch's ideas about "redundancy of command". Initially, Arbib used competing, winner-takes-all, networks (see Chapter 12.ix.a). These modelled the dynamic perception–action cycle, wherein perception leads to action which leads to a new perception . . . and so on. Perceptual recognition depended on a "slide-box" defining a fixed set of categories. And (like RETIC) the system specified a finite set of possible actions, one of which would be activated after competition between the relevant networks.

By 1980, however, fixed networks and slide-boxes had given way to multi-layered perceptual and motor "schemas" (Arbib 1981*a*,*b*). Arbib later said that schema theory "is rooted in the *Critique of Pure Reason* of Immanuel Kant" (Arbib 1995: 11 and sect. 3). But it was mediated by McCulloch's use of Kantian ideas, and the 'Frog's Eye' paper's identification of "genetically built in" neural contexts as "the physiological synthetic *a priori*".

These Kantian ideas, in those behaviourist days, were unorthodox, not to say shocking. However, neuroscientists had Henry Head's example to encourage them (4.vi.a). Accordingly, Arbib—like Bartlett before him (5.ii.b)—borrowed Head's word: what Craik had termed models, he termed schemas. He hadn't used this term initially, but started doing so after friends pointed out the similarity to Bartlett and Jean Piaget—both of whom had spoken of "schemas" (Arbib 1985: 3; S. Gallagher 2004: 53). Indeed, Piaget's constructivism, with its emphasis on continual change in accommodating to the environment, became another important influence.

Schemas were dynamical structures, continually adapted as a result of experience. Each perceptual schema tested for, or (as Grossberg would say) expected, a certain type of object. The simplest ones were feature-detectors under another name. The more complex ones, schema "assemblages", defined general classes. Subtle variations between class members could be identified by "fine-tuning" of the schema's parameters. Similarly, each motor schema specified a general type of action. The simplest of all caused some utterly invariable movement. Usually, however—and unlike the Mars robot—they allowed for fine-tuning to generate movements adapted to circumstances: same action, different movements.

Schemas could be closely associated and/or hierarchically nested—and could be thought of in both neurological and AI terms:

> For [brain theory], the analysis of interacting computing agents called *schema instances* is intermediate between the overall specification of some behavior and the neural networks that subserve it. For [distributed AI], schemas provide a form of knowledge representation which differs from frames and scripts by being of a finer granularity. A schema is more like a molecule than an atom in that schemas may well be linked to others to provide yet more comprehensive schemas. (Arbib and Cobas 1991: 143)

Arbib saw schema theory as a way of coping with the problem posed by Pitts and McCulloch in 1947: how to move between a universal and its instances. It was also a way of answering Marr's early question, how the cerebellum can make the 'right' detailed movements when a general type of action is intended. Indeed, Arbib's student Curtis Boylls had started to model the cerebellum and brainstem motor nuclei in the early 1970s. His main aim was to discover how they could convert a general motor schema (such as grasping something) into specific behaviour (such as grasping *this* thing).

Even as a tadpole, *Rana computatrix* wasn't a toy frog living in a toy world. Arbib's early model of vision was relatively realistic when compared with GOFAI's blocks world, or even Marr (1982), for it computed depth using the optic flow caused by bodily movement. That is, it modelled 'animate' vision.

Moreover, the possible actions were based on ethologists' studies of just what real frogs/toads do in subtly different circumstances. A frog will snap at prey that's perceived as being nearby, for instance, but jump towards prey that's further away; and, in certain circumstances, it will make a detour around a barrier placed between itself and the prey (Arbib 1982; Cobas and Arbib 1992; Arbib and Lee 1993; Collett 1979, 1982).

Neuroscientific realism was attempted too. As early as the late 1960s, Arbib's first student, Didday—like Kilmer, originally an engineer—had started to program a model of the frog's neuro-anatomy (Didday 1976). By the 1990s, *Rana computatrix* included many more neurological details (Arbib *et al.* 1997).

Arbib soon generalized his research beyond frogs. For instance, he provided a theory of the "cognitive maps" first ascribed by Edward Tolman to rats (Chapter 5.iii.b) and later located in the hippocampus by John O'Keefe and Lynn Nadel (1978). This model of approach and avoidance in *Rattus computator* (Lieblich and Arbib 1982) eventually grew to include (visual) Gibsonian affordances—such as *go straight ahead*, *hide*, *eat*, *drink*, and *turn (left/right)*—which often led the rat into currently invisible parts of space, sometimes by a complex route (Guazzelli *et al.* 1998).

After rats, monkeys: *Macaca computatrix* simulated the neural schemas controlling visual saccades in macaque monkeys. It included attentional saccades to new light stimuli, and memory saccades to points of previous interest (Dominey and Arbib 1992).

As for humans, they weren't ignored either. Arbib (1981*b*) published an influential diagram of the functioning of the human hand that would have delighted the heart of Charles Bell (2.viii.f). This integrated motor schemas for moving the arm, rotating the wrist, and pre-shaping the hand to 'fit' the object to be grasped:

> [When] we reach for an object, the brain has to simultaneously figure out the control of the arm and the shaping of the hand. It's not that you get there and then figure out what to do with the hand. The brain is preshaping the hand at the same time that it's coming up with the trajectory . . . [In 1981 I gave] a fairly simple diagram of the interaction between perceptual schemas—figuring out where the object is, what size it is, and what orientation it is in—and the [motor] schemas for control of the arm, control of the hand, and so on, and how they all interacted. (J. A. Anderson and Rosenfeld 1998: 233)

This *Handbook of Physiology* diagram, he now says, is "probably the most successful thing I've done, in terms of the number of reproductions that have occurred in textbooks and papers" (J. A. Anderson and Rosenfeld 1998: 233).

A few years later, he moved from diagram to program. That is, he developed a schema-based program for controlling a (virtual) three-fingered hand (Arbib *et al.* 1985). The hand was designed to perform the central task of the intellectual life: picking up a coffee cup. And, characteristically, the paper discussed the possible neural implementation of the interacting schemas involved.

When Arbib later applied schema theory to high-level cognition and language, however, the neuroscience was largely notional (Arbib *et al.* 1987). The same was true, in spades, when he trod in the footsteps of Hans Driesch (Chapter 2.vii.b), and of John Z. Young and Longuet-Higgins too, and gave the Gifford Lectures on natural theology (Arbib and Hesse 1986: see Chapter 7.i.g).

He co-authored these with Mary Hesse, a distinguished English philosopher of science—and a regular visitor to the Epiphany Philosophers group in the Cambridge apple orchard (Preface, ii). The vagueness was hardly surprising. A schema (assemblage) for a belief system such as Christianity or Islam, or even for component ideas such as salvation or justice, is difficult enough to define in conceptual terms—as philosophers know to their cost. Relating those concepts to detailed neuroscience simply wasn't possible.

Indeed, it may never be possible, *in principle*. Schema theory is expressed at the functional level, between observed behaviour and neural implementation—and functionalists have good reasons for denying any systematic equivalence between conceptual thought and its neural implementation (see Chapter 16.iii–v). One doesn't have to accept the thesis of multiple realizability undiluted—and Arbib certainly wouldn't do so—to grant that many concepts (schemas) can't be mapped onto, or identified with, specific neural mechanisms.

In other words, Arbib used functionally defined 'architectural' theories to interpret *and to guide* his neurology. (In this, he was comparable to Aaron Sloman, who'd been developing a wide-ranging architectural theory, though without the neurology, for many years: see Chapters 5.i.f and 16.ix.c.)

As the century drew to its close Arbib, with Peter Erdi and Eccles's co-author Janos Szentagothai, published a major book on neural organization. (For diverse peer commentaries, see *Behavioral and Brain Sciences* 2000.) Among many, many, other things, this described their computational models of the olfactory system and hippocampus, including schema-based cognition (Arbib *et al.* 1997, ch. 6). They used a dynamical systems approach to model spatio-temporal processes at various different levels, from the subcellular level up (see Section ix.b). And they drew on:

* anatomy (brain structures defined at many levels of analysis);
* physiology (of the cell, the synapse, and EEG);
* psychology (addressing a wide variety of behaviour and cognition, in animals and humans);
* mathematics (abstract analysis of the functions and dynamics involved);
* and various areas of AI.

This was interdisciplinarity in action.

The millennial arrow in Arbib's quiver was a development of Giacomo Rizzolatti's idea that mirror neurones, which he'd discovered in 1995, may have enabled the evolution of language (Rizzolatti and Arbib 1998; Arbib 2002*a*,*b*). A mirror neurone fires not only when a monkey *performs* a certain hand movement, but also when it *observes* that same hand movement performed by another monkey, or by a human.

Arbib listed three possible uses of mirror neurones, including enabling the animal to anticipate, and to imitate, what another animal is doing. Monkeys aren't good at imitation, but people are. In 1994 PET imaging (see Section ix.b) implied that a part of Broca's area 'lights up' when the person makes hand movements; and in 1995, mirror neurones for grasping were indicated there too. Moreover, comparative anatomy suggests that this sub-area may share its evolutionary origin with the 'grasping' area in the monkey's motor cortex. In short, the language area in our brains seems to involve cells capable of aiding gestural communication. Possibly, it may also contain, or be linked to, mirror neurones for facial expressions and speech movements.

(As Arbib pointed out, this hypothesis *does not* address the origins of syntax, negation, or reference to past events. So Chomsky's scepticism about evolutionary explanations isn't allayed: see Chapter 9.iv.e.)

In sum, when Arbib could relate schemas to actual neurology, he did. When he couldn't, he tried to provide computer simulations and/or formal theories that were at least compatible with neuroscience. When he couldn't do *that*, he tolerated vagueness.

But even his Gifford Lectures were delivered with countless glances towards the brain. He was trying to show that a general type of neural processing could underlie a huge range of behaviour—from feeding in frogs to manual dexterity, and even prayer, in *Homo sapiens*.

Arbib's perceptual and motor schemas were analogous to the "T" and "O" of the TOTE units posited in the visionary *Plans and the Structure of Behaviour* (see Chapter 6.iv.c). The creators of both sides of this analogy indulged in optimistic hand-waving. But *Rana computatrix* and its computational cousins incorporated vastly more neuroscientific detail, and much subtler computational insights, than the *Plans* authors could have dreamt of.

Besides being an entire research industry in himself, Arbib has enabled others to engage in a fast-growing neuroscientific/neurocomputational industry. In part, he's done that by his editing of comprehensive interdisciplinary tomes. Very recently, however, he and two colleagues have made available their own object-oriented programming language, NSL (Neural Simulation Language), together with an interesting variety of examples of its use (Weitzenfeld *et al*. 2002).

As we saw in Chapter 10.v.d, an object-oriented language enables the programmer to think at a 'natural' conceptual level. Even more importantly, it can embody a good deal of unseen knowledge within the types of "object" provided. That being so, the objects will—up to a point—behave (and interact) in the naturally expected, appropriate, ways without the programmer having to tell them precisely how to do so. In the case of NSL, the hidden knowledge is drawn from neuroscience. For the language was specially designed to enable people to model *macroscopic* brain structures, as well as lower-level neural networks. Indeed, anatomical structures and schemas can be represented on a number of hierarchical levels. Thanks to NSL, *Rana*'s cousins will surely multiply.

14.viii. Representations Galore

By the end of the 1950s, the notion of mental/cerebral models was a near-universal presupposition in the nascent cognitive science. (Though not in Gibsonian psychology: see Chapter 7.v.e.) In neuroscientific circles, analogue examples were hale and hearty—though still few and far between. In psychology, linguistics, and AI, formal–symbolic representations had the higher profile.

Today, the idea is still prominent in neuroscience. A recent review of research on hand movements, for instance, declared that "*internal models are fundamental* for understanding a range of processes [involved in motor planning, control, and learning]", and cited many others working on that assumption (D. M. Wolpert and Ghahramani 2000: 1217; italics added). In short, neuroscientists differ over what the internal models are like, but *that there are such models* has long been widely agreed. (Widely, but not universally: see subsection d, below.)

I'm taking for granted, here, that "representation"—considered as a theoretical term (what the positivists called an intervening variable)—is interpreted realistically by cognitive scientists, even by those who deny that they exist. That is, it's not merely a helpful shorthand, or *façon de parler*. Philosophers of science who take an instrumentalist view of scientific concepts and theories *in general* would disagree. For them, to say that representations (or electrons, or molecules, or genes . . .) exist is simply to say that it's explanatorily/predictively helpful to talk about them, *not* to say that there are real, independently existing, entities out there which these terms are denoting. But such radical instrumentalism is unusual among working scientists, and I shall ignore it here.

For the neuroscientist (or other cognitive scientist), then, there are two questions: Do cerebral representations really exist, yes or no? And if so, what form/s do they take?

These questions aren't straightforward. For *just what could count* as a "cerebral model" or "representation" wasn't clear when Craik introduced the term—and it still isn't. Craik and Minsky both tried to clarify the concept (Chapter 4.vi.b), and—later—so did John R. Anderson and Sloman (7.v.a). But it's extremely slippery. For instance, the late 1980s AI scientists famed for *attacking* representations relied on them nonetheless—albeit of a (situation-bound) type different from those used in GOFAI (see 13.iii.b).

This section indicates how, in the fifty years after Craik, cognitive scientists tried to pin the term down. In addition, it looks at the evidence for (various kinds of) representation in the brain.

a. What's the problem?

Disputes about the nature of representation, or internal models, were—and still are—both empirical and philosophical. Craik himself realized this, and it became increasingly evident in the fifty years after his death.

On the one hand, Craik's (and Mayhew's) computational question of *how* this or that can be represented in the brain remained. Cognitive scientists offered various answers, including some very different from those described in Sections iii–vii above. The representational mechanisms they suggested included the following, each of which has been mentioned in one or more of the preceding chapters:

* GOFAI symbols, seen as stable, manipulable, and copyable entities;
* 'iconic' representations of various kinds;
* deictic (situationist) representations;
* activity patterns in PDP networks;
* "fluid" concepts;
* feature-detectors; and
* anticipatory schemas.

They also included examples introduced in this chapter (three of them in subsections b–c below):

* neurophysiological models reflecting real-world dynamics;
* systematic mappings of one functional/behavioural space onto another;
* emulator systems that 'march ahead' of the processes being modelled; and
* evanescent representations used 'online' and then discarded.

On the other hand, philosophers argued about what *counts* as a representation, in principle. Those sympathetic to cognitive science didn't always accept every item on the list above as a genuine representation.

And many who weren't sympathetic rejected all of them. They weren't denying that such neural-computational mechanisms might actually exist. Rather, they were claiming that *no* scientific theory could ever explain representation (or intentionality, or meaning) as such. Those disputes are outlined in subsection d.

b. From probabilities to geometries

A theory of neural representation fundamentally different from those described so far was initiated in the late 1960s by Pellionisz.

The notion of internal models was central: "[the theory] hinges on the assumption that the relation between the brain and the external world is determined by *the ability of the CNS to construct an internal model of the external world* using an interactive relationship between sensory and motor *expressions*" (Pellionisz and Llinas 1985: 246; italics added). But there were several hierarchical levels of "expressions", with specified ways of translating between them.

As a young man in Budapest, Pellionisz developed Farley's pioneering work on two-dimensional neural nets (see Chapter 12.ii.b). Still in his early twenties, he defined a network of over 64,000 neurones, consisting mostly of two neuronal fields of 31,510 units each (Pellionisz 1968, 1970). Besides this huge jump in scale (Farley had managed only 1,296 units), his system differed from Farley's in being modelled on specific aspects of the brain. It was, in fact, the first computer simulation of the cerebellum. (Marr's work had been formal analysis, not simulation.)

Even as a student (an engineer at Budapest's University of Technology), Pellionisz saw *geometry* as the key to the brain. But as we'll see, he didn't mean Euclid. In an early publication (his first in English), he declared: "[I] analyse the transfer function of the neuronal information preprocessing system of the cerebellar granular layer [in terms of a] geometrical model of the regular neuronal arrangement . . . illustrated by a pattern transformation method" (Pellionisz 1968). In other words, the key idea was that one pattern could be transformed into another, by way of a systematic geometrical method.

While still a teenager, he'd been inspired by reading the Hungarian translation of von Neumann's posthumous book (1958; see 12.i.c) in about 1960. But whereas von Neumann had tentatively suggested that thermodynamics is the language of the brain, for Pellionisz it was geometry:

> On literally the last page of his book, the man who knew the mathematical language of computers so well . . . confessed in public on his death bed that we have no idea of the mathematical language of the brain (but he was sure it is different from Boolean algebra, the mathematics of man-made computers).
>
> I always excelled in creating new stuff—thus I was flabbergasted in the era of "Cybernetics", ready to create "thinking machines"—that the "top expert" has no idea of its basic mathematics. And he dies, though he feels that he is coming close.
>
> My life was cut out, right there. We have to discover the mathematical language of brain structure and function (I never thought of the two as separate entities). Looking at the geometry of the brain, I was damn sure the mathematical language was geometrical. (personal communication)

Pellionisz's mathematical "teachers", he now says, were von Neumann, Einstein, and Roger Penrose (1931–). The last two described forms of geometry more general than Euclid's, which Pellionisz eventually used to describe the brain. (So Penrose has contributed constructively, if indirectly, to cognitive science after all: see Chapter 16.v.a, and Section x.d below.)

But he needed neurological teachers, too. Like Marr at much the same time, he relied on the recent surge in histological knowledge of the cerebellum. Indeed, he took this from the horse's mouth. His adviser for his Master's thesis on 'Neuronal Modeling' was Szentagothai, of the Semmelweiss Medical University—co-author of the definitive book on cerebellar anatomy (Eccles *et al.* 1967).

Pellionisz's first model of associative memory was described in some detail in 1970 (the 1968 publication was merely an abstract, based on a fuller talk he'd given in Hungary). It simulated the connections between four distinct cell-types: mossy fibres, granule cells, Purkinje cells, and basket cells. And it even included the relative distances, and the precise numbers of connections.

The different cell-types were represented as four two-dimensional matrices, with formulae expressing the excitatory and inhibitory connections between them. Having computed the activity resulting from mossy-fibre inputs, Pellionisz noted "the emergence of concentrated excitatory spots as a result of the local averaging effected by the granule cells" (1970: 78). And he pointed out that certain unexpected features of the simulation depended directly on the histological data incorporated in it. In other words, Farley's (neurally inspired) connectionism had endured a *rite de passage* to become computational neuroscience.

A further *rite de passage* ensued when Pellionisz, after working for a few years with Szentagothai in Budapest, began his collaboration (at NYU Medical School's Department of Physiology and Biophysics) with the experimentalist Rodolfo Llinas, an expert on the cerebellum. With Llinas's help, he developed his early geometrical approach and applied it to a wide range of neurological data. Following some exploratory simulations (Pellionisz and Szentagothai 1973; Pellionisz *et al.* 1977), his final theory of representation was defined in abstract terms by the late 1970s (Pellionisz 1979).

When they first applied it to the cerebellum, to explain the control of bodily skills, Pellionisz and Llinas took the brain's representational system as given (1979, 1980, 1982; Pellionisz 1983*a*,*b*). Later, they asked how it could be learnt (Pellionisz and Llinas 1985). They also applied the theory to vision, and to the balance-sensitive semicircular canals (Pellionisz and Llinas 1981, 1985; Pellionisz 1985). In short, they were talking about representation *in general*.

Their theory differed radically from other models of associative memory. Where those relied on Hebbian rules defined over isolated—and identical—neurone pairs, they described representation and learning in terms of systematic mappings between large *sets* of neurones. And "large" meant large: already by the mid-1970s, their computer network had jumped from 64,000 components to over 1.7 million. These included over 8,000 Purkinje units, and others modelling cerebellar and brainstem nuclei. Moreover, each Purkinje unit had its own distinctive role, determined by its precise position in the neural "space" concerned (see below).

The reason was that they were addressing the *coordination* of movements. They accepted Brindley's idea that the cerebral cortex generates broadly defined motor "intentions", which the cerebellum converts into more detailed instructions (the output of the Purkinje cells). But they weren't interested in single body movements, still less in individual muscles. Rather, they considered movements generated within integrated behavioural suites, nicely adapted to the sensory information available at each moment.

They wanted to explain, for example, George Stratton's (1896, 1897*a*,*b*) and Ivo Kohler's (1962) intriguing findings that people can gradually compensate for distorting spectacles of various kinds. (See also the 'homeostatic' model discussed in Chapter 15.viii.d.)

Even if the visual image is turned upside down (i.e. if the retinal image is turned right side up), the person's movements and perception, hugely dislocated at first, become quasi-normal within a week or so. Only *quasi*-normal: both Stratton and Kohler had noted bizarre visual aberrations. And only a week "or so": Stratton (1897*b*) actually wore the inverting mechanism for only eighty-one hours out of the 200, although for the rest of the time he was blindfolded. (His detailed accounts of his changing experiences—including their relation to various movements, to bizarre dislocations in his body image, and at first to near-overwhelming feelings of depression—are fascinating reports of the phenomenology involved.)

Cats can compensate for distorted input, too (Melvill-Jones and Davies 1976). But some species never do. Snap little goggles, shifting the light 7 degrees to the right, over a chick's eyes as it hatches from the eggshell, and it will starve to death if it's given only sparsely scattered grains of corn to peck at (Hess 1956).

Why is it that people, unlike chicks, can adapt to such sensory distortions? Stratton, at the end of the nineteenth century, had sketched the answer as follows:

> Vision as a whole and by itself is indeed neither inverted nor upright. . . . [Upright] vision must mean a vision which gives us objects upright with reference to some non-visual experiences which are taken, for the time being, as the standard of direction. Upright vision, in the final analysis, is *vision in harmony with touch and motor experience*; and the only problem of upright vision is one concerning *the necessary conditions for a reciprocal harmony in our visual and tactual or motor perceptions*. (1897*a*: 184–5; italics added)

The different sense perceptions, Stratton said, "are organized into one harmonious spatial system", where the harmony consists in having our experiences meet our expectations. All that's needed is "a reliable cross-reference" between the senses, not any absolute marker of locations in reality.

Pellionisz and Llinas, too, were concerned with a complex representational *system*, not just a clutch of isolated representations. And they, too, considered the match/mismatch between actual and expected perceptions in guiding bodily action.

To integrate perception and action, they developed an arcane corner of mathematics: a form of non-Euclidean geometry known as tensor analysis. They defined systematic coordinate transformations, or "tensors". These provided a way of passing automatically between highly complex sensory and motor representations, looping through the environment to check on the sensory predictions in play.

The transformations had to be automatic because, as Karl Lashley (1951*a*) had pointed out, nervous conduction is too slow to explain bodily skills as chains of sensory-motor reflexes (Chapter 5.iv.a). They had to be complex, because any 'one' action involves a large number of cooperative and antagonistic muscles, and any 'one' perception involves many sensory receptors—in the retina, inner ear, tendons, and muscles (including the muscle spindles mentioned in Chapter 2.viii.d). There are countless ways of using one's muscles to pick up a coffee cup, and the skilled coffee-drinker finds the most efficient: *these* specific muscle movements are executed, given *those* sensory data about one's current bodily attitude and the position of the cup.

That was already well known. But where Arbib had used schema theory to compute how this could be done (see Section vii.c), Pellionisz and Llinas used tensor geometry.

Tensor network theory wasn't for the faint-hearted. The pure mathematics involved had been developing since Einstein's use of it early in the century, and had "a legendary reputation for difficulty and complexity" (J. A. Anderson *et al.* 1990*a*: 352). (When I mentioned this assessment to my young astrophysicist son-in-law, he agreed: "It bloody is!") Accordingly, their papers bristled with references to "non-Riemannian overcomplete CNS hyperspaces" and "spectral representation of the covariant metric tensor and its proper inverse (or Moore–Penrose generalized inverse) as expressed by their eigendyads".—Light reading, this was not.

Fortunately (for me, at least), the mathematical details needn't concern us. What's important is that tensor theory maps one geometry onto another. The projective geometry used in blocks world (Chapter 10.iv.b) maps Euclidean 2D space onto 3D space, in a very simple (non-perspectival) way. But tensor theory can coordinate multidimensional hyperspaces.

For an intuitive example of a hyperspace, consider the set of people eligible to apply for an imaginary scholarship. Applicants must be male, and must also satisfy at least two of these criteria: between 18 and 32 years old, but the younger the better; born in Derbyshire or Yorkshire; father (preferably) or grandfather a clergyman or a soldier; educated at one of twelve named schools; intending to study science at university; orphaned before the age of 15, and the earlier the better.

(This hyperspace isn't quite so "imaginary" as you may think. In 1961 I spent some hours in a large reference library searching for scholarships to study in the USA: of the forty or so that I found, many listed criteria just as bizarre as these—and only six of the forty were open to women.)

The 'geometry' of this scholarship space has many dimensions, namely the criteria just listed. Some are binary, some continuous; some are mandatory, some optional; and some are disjunctions, with two or twelve disjuncts (e.g. the two counties and twelve schools). An applicant at the 'centre' of the space would satisfy every criterion. Someone on the 'periphery' (besides being male) would satisfy only two, and those only weakly. Try thinking about just how you would locate fifty different applicants within this multidimensional space, deciding on their 'positions', 'neighbours', and 'distances'—and your mind may well begin to boggle. Imagine mapping them onto fifty points in some even more complex space, such as the criteria defining an ideal husband (GSOH etc.), and you might as well give up.

In tensor network theory, such decisions—for highly complex, and diverse, neuronal spaces—were computed precisely, coherently, and testably. And algorithms (sets of neural connections) were defined for transforming one set of hyperspatial coordinates into another, so that the *appropriate* movements were made at each moment, given the sensory information available. The GOFAI robot SHAKEY (Chapter 10.iii.c) had computed only simple 2D-to-3D mappings, and chose from a highly restricted set of movements. But a tensor network robot could generate a much wider range of behaviour, informed by a more complex array of sensory inputs (Pellionisz 1983*a*).

On this view, a cerebral 'intention' is sculpted into a more precise representation of action by the cerebellum, starting from the current bodily attitude. This attitude is known (represented) by mapping the proprioceptive input from the many tendons and muscle spindles onto 'muscle space'. The series of many-muscled movements that ensues is computed by finding a path through muscle space so as to arrive at the end point (e.g. holding the coffee cup).

Each movement 'predicts'—is associatively mapped onto—a particular set of proprioceptive (and visual and/or tactile) inputs. If the *actual* sensory input is different, perhaps because the arm is blocked by some obstacle, a different muscular 'starting point' is found by automatic mapping, and a new muscular 'pathway' computed accordingly. When such failures of prediction occur systematically (as in wearing distorting spectacles), an adaptive process of *meta-organization* gradually refigures and recoordinates the sensori-motor spaces concerned. This process was mathematically defined, and was described by the authors as the first precise definition of the "largely intuitive" concept of self-organization (Pellionisz and Llinas 1985, sect. 4.1).

An important general point, here, is that this theory posited *hierarchies* of dual geometries in the nervous system, with different tensors linking each pair of contiguous levels. This idea was used (for example) to explain mammalian gaze control—which isn't a simple matter, since both eye movements and neck movements are involved. (Hoverflies lack both, so don't need a complex mechanism of gaze control: Chapter 15.vii.b.) Pitts and McCulloch (1947) had discussed some relevant visual computations. But Rafael Lorente de No (1933*b*) had already suggested that vision isn't enough, that gaze control also involves feedback from the inner ear (see Chapter 4.v.c). Pellionisz (1985) took this added complexity on board—later adding the neck muscles (Pellionisz and Peterson 1988). He even sketched, though couldn't simulate, the frog's *entire* nervous system in (multilevel) tensorial terms (1983*b*), depicting the animal as what he might have called—but didn't—*Rana geometrica*.

The theory was applied to self-organization in the embryo, too. People had long observed spontaneous oscillations, or coordinated twitches, in the legs of chick embryos. Now, Pellionisz and Llinas (1985) suggested that these apparently useless movements enable the chick to develop its (initially vague) proprioceptive space in coordination with its (fixed) muscular space. That is, tensor coordinations enable established neural representations to be used, on successive levels, to organize less well-developed ones. (A related idea, of "representational trajectories", was being developed informally in connectionist psychology: Chapter 12.viii.d.)

Tensor theory was tested by biological experiments in Llinas's lab, by computer simulation, and (later) by robotics. By the late 1980s, it was beginning to receive independent experimental support (e.g. Gielen and van Zuylen 1986). James Anderson included the meta-organization paper in his influential *Neurocomputing 2*, and invited Pellionisz to co-edit the volume (J. A. Anderson *et al.* 1990*a*). And Pellionisz received the first international award for neurocomputing (Germany's Alexander von Humboldt Prize) in 1990.

One might think this would have 'licensed' Pellionisz as a professional researcher, but it didn't. (Llinas didn't need a licence, being already established when their collaboration began.) Like Grossberg and 'cerebellar Marr' (see Sections v–vi), Pellionisz suffered from neuroscientists' suspicion of fancy mathematics. Even mathematically competent researchers such as Arbib and Shun-Ichi Amari were sceptical, although Amari later adopted somewhat similar ideas (Arbib and Amari 1985; Amari and Wu 1999; cf. Pellionisz and Llinas 1985, endnote).

Pellionisz wasn't offered a tenured academic job, and left NYU in 1989—soon to be employed by NASA, to design software controls for F-15 jet fighters (Pellionisz *et al.* 1992). Later, he worked in the IT industry, and founded a company using tensor network theory to do personalized pattern matching—applying his four-level PureMatch software in finance and medicine, for example (personal communication). Whether he'd have had more to offer to neuroscience, who can tell?

c. Emulation and subjectivity

Tensor network theory—once it was applied to the body's geometry, in the late 1970s—was an early example of what's now called an emulator system (Grush 2004). It's partly because of neural emulators that our walking can be continually adjusted (unlike the path of a ballistic missile, or the hoverfly's flight towards its mate: see Chapter 15.vii.b).

An emulator is an internal model of a feedback system, which works in a similar way but faster. Using feed-forward processes, it continually anticipates the system's responses *and the resultant sensory information*. Given a mismatch between expected and actual input, the emulator can adjust the output controls immediately, instead of just a little late. Without the emulator, and especially if the stimulus is continually changing, the output would be jerky rather than smooth.

Emulators have been used in control engineering from about 1960. Craik couldn't cite them, because they hadn't yet been built. However, they're a paradigm case of the type of representation he had in mind. Since their appearance on the engineering scene, they've been found in the nervous system.

Tensor network theory, as remarked above, defines a hierarchy of emulator systems. Another example was described by Mayhew and colleagues, in a study of the cerebellar control of visual saccades—in which the eyes move to focus on, and track, a particular object (Dean *et al.* 1994). Yet another concerned the control of hand movements made in the dark (Wolpert *et al.* 1995). In view of the controversy over the very existence of cerebral representations, it's worth remarking that those authors saw their results as "direct support for the existence of an internal model" (Wolpert *et al.* 1995: 1880).

The same authors soon argued that emulators and other representations are involved in sensori-motor integration *in general*, where the organism combines multiple sources of information to estimate its own state and that of the environment (Ghahramani *et al.* 1997). And after further research in this area by themselves and many others, they concluded:

> The computational study of motor control is fundamentally concerned with the relationship between sensory signals and motor commands. . . .
>
> *Computational approaches have started to provide unifying principles for motor control.* Several common themes have already emerged. First, internal models are fundamental for understanding

a range of processes such as state estimation, prediction, context estimation, control and learning. Second, optimality [i.e. optimal control theory] underlies many theories of movement planning, control and estimation and can account for a wide range of experimental findings. Third, the motor system has to cope with uncertainty about the world and noise in its sensory inputs and motor commands, and the Bayesian approach provides a powerful framework for optimal estimation in the face of such uncertainty. We believe [thanks to a wide range of experimental evidence] that these and other unifying principles will be found to underlie the control of *motor systems as diverse as the eye, arm, speech, posture, balance and locomotion*. (Wolpert and Ghahramani 2000: 1212, 1217; italics added)

Such neurological theories support Stratton's *psychological* insight that it was the difference between the actual and expected sensory inputs which enabled him to adapt to an inverted visual world. A hundred years after Stratton's experiments, Jennifer Freyd (with Geoffrey Miller) offered further psychological evidence for neural processes emulating the world.

She posited dynamic mental models having "representational momentum", to explain her finding that people who perceive a moving object tend to misremember it as having moved further than it actually did (Freyd 1987; G. F. Miller and Freyd 1993). Such models have evolved, she said, to help us track real-world motion by anticipating it rather than merely following it.

Like Roger Shepard and Stephen Kosslyn before her (see Chapter 7.v.a), she reported a linear relation between representation and reality. The size of the mistake in the memory of final position depended directly on (1) the speed of movement and (2) the time elapsed between perception and memory probe. That's just what one would expect, if there were some physically realistic internal model that "keeps moving in the mind"—or, as Craik would have put it, keeps running in the brain—after the actual movement has ceased.

Freyd's evidence may be persuasive to neuroscientists working in the Craikian tradition, and to psychologists—such as Shepard himself—who argue that "second-order isomorphism" has evolved in our perceptual systems (see 7.v.a). But it wouldn't convince committed formalists such as Jerry Fodor and Zenon Pylyshyn. They'd interpret it in GOFAI-friendly terms, as they did Shepard's and Kosslyn's data on mental imagery (Pylyshyn 1973). On that view, her results show that the perception and/or memory of movement is "cognitively penetrable", owing to top-down symbolic inference (see Chapter 7.v.a). Her representational hypothesis would be vindicated only if a Craikian emulator of the type she implied were actually identified by neuroscience.

Lashley (1951*a*) had argued that there must be some central anticipatory mechanism controlling skilled movement, but could say very little about just how it might work. Today, neural emulation is the key to our understanding of bodily skills. Ito, who in the 1960s co-authored (with Eccles and Szentagothai) *The Cerebellum as a Neuronal Machine*, now says:

With respect to cerebral control functions, cerebellar chips appear to [form] *an internal model of a controller or a control object*. If, while a chip and the system to be modeled are supplied with common input signals, differences in their output signals are returned to the chip as error signals, the chip will gradually assume *dynamic characteristics equivalent to those of the system to be modeled*.

> Cerebellar chips [in one area] are connected to the cerebral motor cortex in such a way that these chips constitute *a model that mimics the dynamics of the skeleto-muscular system* (Ito 1984). The motor cortex thus becomes capable of performing a learned movement with precision *by referring to the model* in the cerebellum and not to the skeleto-muscular system. [The] failure to perform a precise reaching movement without visual feedback could be due to the loss of such internal models. (Ito 1999: 111; italics added)

Indeed, the emulator control of skilled (automatic) movement is seemingly only one function of the cerebellum. Brain-imaging techniques have recently suggested that it might also be crucial in skilled thinking (Schmahmann 1997). So Ito adds this:

> During thought repetition, a cerebellar chip may form a model of the parietolateral cortex or the prefrontal cortex. A repeatedly learned thought may thus be performed quickly yet accurately even without reference to the consequences of the thought or without conscious attention. (p. 111)

Craik, had he lived, might have seen this suggestion as confirming his claim that thinking involves analogue symbols, too.

Neural emulators—like their control-engineered cousins—are online representations, used to improve the efficiency of behaviour while it's happening. In human beings they can also be run 'offline', so that people can practise physical activities by imagining them. This explains why (as a Norwegian diving instructor told me many years ago, to my amazement at the time) top-level divers living beyond easy reach of an Olympic swimming pool can improve their performance by diving 'in their heads'.

The emulators themselves are enduring memories. The connections that implement them are retained, once they've been learnt. But their adaptive function is to represent momentary 'present-tense' events: where the muscles are *now*, and what the input data are *now*. In other words, they are subject-dependent representations, not objective ones.

Some representations posited by cognitive science are even less objective than this. For they are evanescent structures, coding intermediate results in some multi-stage computational process. Neuroscientific evidence is difficult to come by, for obvious reasons—although high-resolution fMRI might help, especially if combined with electrophysiology (Logothetis 2002). But both computational arguments and psychological evidence are relevant:

* For instance, Aaron Sloman argued in the late 1970s that high-level visual interpretation requires temporary data structures, and implemented them in his POPEYE project (see Chapter 10.iv.b).

* Marr's theory of vision distinguished the viewer-dependent $2\frac{1}{2}$D sketch from representations at the level of object models (Chapter 7.v.c).

* And Perrett's group explained the monkey's face recognition in terms of a limited set of "viewer-centred descriptions" (see Section iv.d, above).

* As for psychological evidence, Ann Treisman (1988) explained her experimental data on perception and problem solving by positing temporary "object files", generated 'on the fly' (i.e. during processing) in working memory.

The crucial point here is that there are relatively 'subjective' and relatively 'objective' representations. In the last decades of the twentieth century, neuroscientists—and some philosophers—increasingly accepted this. They tried to specify the differences, and to explain how objectivity could develop from subjectivity, whether in evolution or in ontogeny (see Chapter 12.x.e–f and Cussins 1990).

In short, by the new millennium cognitive scientists had posited representations galore. Neuroscience now used the term liberally, offering a wide range of hypothetical examples (Parker *et al.* 2002).

d. The philosophers worry

That's not to say, however, that the *philosophers* regarded all of these 'representations' as the genuine article. Their quarrels weren't with the biological evidence, but with its intentional interpretation—and (predictably) with each other.

Craik's philosophy of mind had defined "symbols" and "models" in analogue terms. GOFAI philosophers, by contrast, tried to restrict such concepts to formalist examples (hence their top-down interpretation of imagery, mentioned above). For them, connectionism was concerned with "implementation details", not representation. PDP philosophers favoured a third position (see Chapters 12.x.a–c and 16.iv.e). They argued that activation patterns distributed over many computational units, none of which can be assigned a constant meaning or regarded as a symbol for a nameable concept, are genuine representations too. And some allowed talk of *subjective* representations, as precursors to full-blooded objective concepts (see 12.x.f).

Some philosophers preferred a very wide definition: *any* internal mechanism that was reliably correlated with an external event and causally implicated in ecologically appropriate behaviour (Dretske 1984, 1995). This would include single-cell feature-detectors (which aren't "models" in the Craikian sense, although the circuitry activating them may be), and also the relatively subjective, evanescent, examples noted above. Both of those would be excluded, however, by the familiar definition—familiar to philosophers—of a representation as a (non-semantically identifiable) internal state that causes behaviour to be adaptively coordinated with some environmental feature *even when that feature is absent.*

Moreover, the reference to "an identifiable internal state" excludes cases (highlighted by the dynamical approach) where there's a complex interaction of body, brain, and environment. Of the philosophers who recognized this interactive causation, some allowed that certain aspects of the causal nexus play a special role in adaptive behaviour and should be counted as representations accordingly (Clark and Grush 1999; Clark and Wheeler 1999). Others—including 'Frog's Eye' Maturana (wearing his autopoietic philosopher's hat)—denied the existence of *any* representations in the brain. Far from there being representations galore, there are none at all. (The reasoning behind this unfashionable, even counter-intuitive, view is sketched in Chapters 15.vii and 16.vii and x.c.)

Those philosophers of mind who had no special interest in cognitive science focused on the centuries-old concept of representation, or intentionality, *as such*. They ignored the specifics of neuroscience and AI, although some did ground intentionality in evolutionary biology (see Chapter 16.x.d).

Finally, neo-Kantians didn't simply ignore science but explicitly dismissed it. Or rather, they wished to keep it strictly in its place—which was to answer *empirical* questions. According to them, there can be no naturalistic explanation of intentionality—

not even in neuroscience, never mind AI. They weren't merely saying that there's a distinction between empirical and philosophical questions. Rather, they were saying that science is irrelevant, so much so that it can't even give us helpful clues on philosophical matters. It would follow, for instance, that *even if* scientists discovered innate 'syntactic' mechanisms, Chomsky's *philosophical* claims wouldn't be supported, still less proved.

Clearly, that wasn't the view of the many philosophers of cognitive science I've cited throughout this book, who *did* think that cognitive science could help us to a more adequate philosophy of mind. But even they had to admit that they had no knock-down arguments against neo-Kantianism (see 2.vi and 16.vi–viii).

On one thing, however, all the philosophers agreed. Namely, that empirical cognitive scientists used the words "representation" and "model" in largely intuitive senses—obscuring important distinctions and begging controversial questions.

Craik and Minsky each tried to define these concepts in *philosophical* terms, as we've seen (Chapter 4.vi.b). So did Newell and Simon (16.ix.b). But most of their scientific colleagues didn't. They simply picked them up from the Zeitgeist and ran with them. If they offered 'definitions' at all, these concerned the mechanism rather than the concept.

From the late 1970s on, gallons of philosophical ink were spilled on trying to define representation more strictly. Indeed, it's still spilling, unabated. I've no intention of tracking each drop. Several rivulets are traced in other chapters, however. And two major streams of disagreement—concerning *reportability* and *intentionality*—will be followed in Chapter 16.

(*How to find the rivulets*: David Kirsh's critique of non-representational robotics is outlined in Chapter 13.iii.c. Andy Clark's defence of PDP, and Adrian Cussins's of increasing objectivity, appear in Chapter 12.x. Fodor's 'classical' representationalism is outlined in Chapter 16.iv.c–d; Timothy van Gelder's dynamical critique in Chapter 16.vii.c; and Maturana's autopoietic theory in Chapters 15.vii.b and 16.x.c. Finally, Michael Morris's argument that *no* brain mechanism is a representation is given in Chapter 16.viii.a.)

Here, let's just note that neo-Kantian philosophers insisted on reportability as a criterion of representation. For them, language is essential for mind, intelligence, and thought (16.vi–viii). They would never write books called *Animal Thinking* or *Animal Minds* (Griffin 1984, 1992), and regarded "cognitive ethology" (Ristau 1991) as a contradiction in terms. However, experimental psychology, ethology, AI, and neuroscience developed within the opposing philosophical tradition (see Chapter 2). They drew no sharp line between 'mindful' *Homo sapiens* and 'mindless' dumb animals.

The slipperiness of the concept of representation affects how people think about cognitive science itself. Since the concept was often used in defining the field, disagreements about *what representation is* were reflected in judgements about its scope, and its success.

If one accepts Fodor's sense of representation, for instance, connectionism is either a *refutation* of cognitive science (as Dreyfus claimed) or a mere implementational *adjunct* to it (as Fodor himself believed). Accepting a more catholic definition of representation, connectionism is an interesting *example* of cognitive science (and further putative examples have been given in this section). Similarly, dynamical theories and situated robotics (for instance) must be excluded if, by definition, cognitive science posits representations in explaining behaviour.

This is why, as I said right at the start, the boredom barometer would shoot through the roof if one compared every definition of cognitive science. And it's why I deliberately avoided the elusive term "representation" in my own definition of the field (Chapter 1.ii.a).

It's not even clear that trying to legislate on "the best" definition of representation is sensible. The same applies to life (see 16.x), and to theoretical terms in general (Quine 1951, 1960; Putnam 1962*a*). Comparing putative definitions carefully is one thing, and can be highly illuminating. But announcing *just one* to be the only defensible option is quite another. That's especially so in scientific contexts. As Craik put it, when discussing perception:

> [Scientists and philosophers alike] fail to see that their remedy of exact definition may be *impossible and unattainable* by the very nature of the physical world and of human perception, and that their definition should be corrected in the way of *greater extensiveness and denotative power, rather than greater analytical, intensive, or connotative exactitude.* (p. 4; italics added)

The catch-all sense of 'representation' (which I deliberately haven't tried to *define*) is therefore useful. It acknowledges the intriguing variety of intentional and quasi-intentional mechanisms that generate adaptive behaviour. Philosophers can help clarify the differences, for they're already familiar with a wide range of relevant distinctions and implications. But that variety becomes apparent through empirical research, not philosophical diktat.

14.ix. Computation Challenged

In the final decades of the century, seven research themes in neuroscience challenged certain aspects of the computational approach. Those challenges were largely met—sometimes, by modelling in radically novel ways.

All seven were older ideas revivified by new data. The first four—self-organization, dynamical systems, epigenesis, and neural selection—were closely interrelated, as we'll see. The fifth concerned grandmother cells; the sixth, neurochemicals; and the last, time.

a. Structure without description

At the mid-century Hixon symposium, McCulloch had argued that the information stored in our genes could determine the connections between at most 10,000 neurones, "[even] if that was all it had to do" (W. S. McCulloch 1951: 85). "As we have 10^{10} neurons", he continued, "we can inherit only the general scheme of the structure of our brains. The rest must be left to chance." And by "chance", he said, he really meant learning from experience.

Only a few years afterwards, he and his 'Frog's Eye' colleagues discovered a detailed 'mapping' between the frog's retina and tectum (see Section iii.a). Soon, others found organized columns of orientation detectors in the visual cortex of cats—and in newborn kittens too (Section iv.d). This was a puzzle, for the kittens couldn't have "learnt from experience" in the womb. Their detailed brain structure, it was therefore assumed, *must* have been cleverly programmed by the genes—perhaps by means of genetically specified chemical 'labels' to guide the developing neurones. (McCulloch's worry about the amount of information required for such a program was seemingly forgotten.) After all, brains can't get organized by magic.

In the final quarter-century, however, a number of people showed that (as Ashby had suggested in the 1940s: Chapter 4.vii.c) there may be *general* mechanisms in the developing brain which structure it spontaneously, without either 'external' learning or specific genetic instructions. Specifically, the claim was that the various feature-detector cells, *and* their systematic arrangement in neighbouring clumps and columns in the cortex, were self-generated. Highly influential versions of this claim were due to the biologists Willshaw and von der Malsburg, the engineer Kohonen, and the computer scientist Ralph Linsker.

Willshaw and von der Malsburg (1976) asked how patterned structures might arise within an unorganized mass of cells in the *embryo's* brain. Von der Malsburg had already shown that specific visual inputs could lead to columns of feature-detectors (Section vi.b, above). But the womb doesn't provide specific visual inputs. So now, he and Willshaw generalized the point.

They showed that for any two interconnected sensory cell layers, simple Hebbian rules will make the second develop a 'map' of the first. The one constraint was the presence of short-range excitation and long-range inhibition (as in the 'Mexican hat' detectors described in Chapter 7.v.d). Both of these, of course, had long been observed in the brain.

They proved—and demonstrated by computer modelling—that those mappings will arise in an orderly way, owing to properties of the system as a whole:

> [We] have shown that the mappings are set up in a system-to-system rather than a cell-to-cell fashion. The pattern of connections develops in a step-by-step and orderly fashion, the orientation of the mappings being laid down in the earliest stages of development. (Willshaw and von der Malsburg 1976: 431)

They described two independent factors, operating at different scales. One was an optimizing process ensuring a topographical mapping, since "neighbouring presynaptic cells come to connect to neighbouring postsynaptic cells" (p. 433). The other used boundary and initial conditions to affect the position, size, and orientation of the final mapping. They varied the boundary conditions in their computer models, showing how this affected the network's self-structuring.

As if in response to McCulloch's worry, they pointed out that theories of self-organization "have the advantage of requiring only an extremely small amount of information to be specified genetically". Indeed, they noted that the amount of information that would be required for pre-wiring is even greater than inspection of adult brains suggests.

A frog's retina and tectum, for instance, grow at different rates and in different ways, so "the only way the systems could remain matched throughout development [is] for the synaptic relations between retina and tectum to be constantly changing" (p. 432). A rigid pre-wiring plan wouldn't provide the flexibility required. To 'settle' this plasticity into one form or another, the *intra-cerebral* environment was crucial. Their models showed how "the same genetical program for the topographic part can in different situations lead to quite different retinal points being connected to a given tectal location, without the need for [chemical] relabelling".

In saying this, Willshaw and von der Malsburg weren't denying that the brain may use chemical labelling to guide *this* neurone to connect with *that* one. Their point, rather, was that Hebbian self-organization is functioning too (perhaps to 'tune' the results of initial chemical guidance), and that it's potentially powerful enough to work alone. Moreover, a 'chemical' method could in principle specify *any* mapping, however bizarre. Their theory generated only 'natural' (topographical) mappings. (Compare Allen Newell and Herbert Simon's decision to constrain their production systems, converting a universal programming language into a 'weaker' but more biologically realistic framework: see Chapter 7.iv.b.)

A few years later, Kohonen (1982)—who'd been modelling associative memories since the 1960s (see Chapter 12.v.e)—generalized these ideas still further. His analysis applied in principle not only to mappings of spatial features but also to "completely abstract or conceptual items"—*provided that* "their signal representations or feature values are expressible in a metric or topological space that allows their ordering".

The good news was that Barlow's hunch (Section ii.b), that logical reasoning may be grounded in the mechanisms that make efficient perception possible, was reinvigorated. (It had survived also in Grossberg's and Arbib's work, of course.) The bad news was that establishing a suitable "topological space" for concepts (*plough*, *betrayal*, *cousin*, *justice* . . .) is easier said than done. Margaret Masterman's computerized thesaurus (Chapter 9.x.a) and Robert Abelson's matrix of "themes" (Chapter 7.i.c) might be seen as early attempts, and theories of semantic primitives too (Chapter 9.viii.c). But much greater subtlety and richness would be required to define the "topology" of a plausible self-organizing concept-mapper.

Kohonen (1988) soon used his analysis to build a device for speech recognition. It took speech (single words, or strings of words separated by pauses) as input, analysed it into similar but varying phonemes, and output the words on a typewriter. (In Finnish, unlike English, sound and spelling match closely.) The phoneme classes weren't built in; and they weren't identical with the linguist's phonemes (Kohonen called them "quasi-phonemes", accordingly). Nevertheless, the system learnt to recognize the 'same' phoneme in differing phonetic contexts, and spoken by individuals with different voices and accents. It developed a large vocabulary, and was the best speech-recognizer at the time.

This 'science-fictional' automatic secretary was described in the widely read pages of *Computer Magazine*. The same issue also featured an elegant pebble thrown into the theoretical waters by Linsker. And this made even more of a splash.

Based at IBM's T. J. Watson Research Center, Linsker (1949–) had defined—and implemented—highly abstract models of multi-layer feed-forward networks (1986, 1988, 1990). These showed that simple Hebbian rules, given *random* activity, could lead to structured orientation detectors. Again, the newborn kitten's visual skills were less surprising than they'd seemed.

So far, so familiar. What was original here was Linsker's formulation, using Shannon's information theory, of the "infomax" principle. He applied this both to individual cells and to the network as a whole:

> The organizing principle I propose is that the network connections develop in such a way as to maximize the amount of information that is preserved when signals are transformed at

each processing stage, subject to certain constraints . . . The constraints or costs may reflect, for example, biochemical and anatomical limitations on the formation of connections, or on the character of the allowed transformations. (Linsker 1988: 529, 536)

In other words, the nervous system has found *the best possible* way of analysing complex sensory input, given that flesh and blood is doing this at all. The broad idea wasn't new (Marr had relied on it in his theory of edge detection: see Chapter 7.v.b–d). But Linsker had apparently found a powerful way to generalize it.

Admittedly, he said, much work would be required to apply the infomax principle to real biology. For example, it wasn't clear whether it could be generalized to cover 'circular' networks involving feedback—as in top-down action by schemas, or recurrent networks. Moreover, only empirical neuroscience could show which "constraints" had been favoured by evolution, or perhaps "by other principles not yet identified". (His reference to "other principles" *besides* evolution was a sign of the times: A-Life had recently revived interest in such principles—see Chapter 15.iii–iv and viii–ix.)

Two additional bonuses were remarked at the close of his paper. "Infomax" made it much more likely that an isolated mutation, in the evolution of a complex neural system, would be adaptive. The apparent 'necessity' for several *simultaneous* mutations evaporates if each neural level can spontaneously adapt to a small alteration in another. The same may apply to other bodily organs too. For the paper's final squib was the suggestion that infomax might apply to dynamical systems of many different types.

In other words: neural development, goal seeking, immune response, and biological evolution might all share similar self-organizing properties. This exciting idea had recently hit the news-stands (see Chapter 15.ix.a), and Linsker's mathematics was taken as strong support.

The "splash" caused by Linsker's ideas is evident in Cowan's rueful comment a few years later:

In the late 1970s, Christoph von der Malsburg and I worked together on the formation of orientation detectors. I had the idea that maybe this idea of a natural tendency to form stripes and blobs [he'd already cited Turing: see Chapter 15.iv.b] was the key to understanding it, and all you needed was a two-layer network stimulated by noise, and it would automatically make the correct feature detectors.

Christoph didn't believe me; he said, "That's magic."

I said, "No, it's just spontaneous symmetry breaking."

It turns out I was right. We never did it, and I have been kicking myself ever since because Ralph Linsker did it. That's exactly what Linsker discovered: that stripes and blobs will spontaneously form in a map. That's the origin of center–surround orientation detectors in the visual cortex. (J. A. Anderson and Rosenfeld 1998: 120–1)

Neuroscientists, besides further analysing Linsker's general results (D. J. C. Mackay and Miller 1990), built more 'realistic' models. For example, a different Hebbian rule was used; and ocular dominance columns were generated partly by neural competition between cells connected to the two eyes (Linsker had considered only one eye): K. D. Miller *et al.* (1989). This work also showed *why* certain sorts of abnormal visual experience result in distinct neural pathologies.

Harry Barrow—a pioneer of AI vision (see Chapter 10.iv.b)—also built on von der Malsburg's 1973 paper, and later on Linsker's work. He and Alistair Bray modelled the origin of the simple and complex cells of visual cortex, to find out which types of pattern were 'preferred' by the brain. They used natural images, whereas von der Malsburg had used artificial line patterns.

Their networks developed systematically structured groups of orientation-sensitive edge- and bar-detectors, or of detectors for colour or position-invariant orientation (Barrow 1987; Barrow and Bray 1992*a*,*b*, 1993, 1996; Bray and Barrow 1996). (They suspected that a 'composite' version of their model would develop *all* these feature-detectors, but didn't have enough computational power to test this.) They asked why the networks developed as they did, and showed how the results would differ if various parameters were altered.

In short, they explored the mathematical core of several *superficially distinct* self-organizing systems. Barrow and Bray, Linsker, and Willshaw and von der Malsburg were all asking important questions about the inevitability of crucial types of brain mechanism. These mechanisms didn't have to be sketched by some all-powerful Designer in the sky. They didn't even have to be nicely prefigured in the genes. They arose naturally, as a result of self-organizing principles dependent on relatively low-level properties of neural tissue. (If D'Arcy Thompson had still been alive, he'd have been fascinated: see Chapter 15.iii.a.)

b. Dynamics in the brain

The studies described above weren't the only examples of late twentieth-century neuroscience taking self-organization seriously. A number of people (including Arbib, as we've seen) were now drawing not only on Hebb but also on dynamical systems theory. (In general, a dynamical system cycles between a finite number of holistic 'attractors'—Ashby's "equilibrium" states—in a more or less regular fashion.) They weren't alone: people in other areas of science were also exploring these ideas (Chapter 15.viii.c, ix, and xi; and 16.vii.c).

Some philosophers were doing so, too. They described the mind/brain in terms of spontaneous self-organization, instead of the input–output systems favoured by classical cognitivism. Besides those to be mentioned in Chapter 16.vii and x, these writers included Susan Oyama (1985). Her unorthodox approach to the developmental sciences (including evolutionary biology) resembled Romantic views that had 'died the death' in scientific circles with the publication of *The Origin of Species* more than a century before (Chapter 2.vi). Nevertheless, it now influenced a number of hard-headed developmental scientists and A-Life researchers.

Oyama criticized GOFAI cognitivism—and most neuroscience—for treating the mind/brain as a given, static, structure. Instead, she argued, it's an ever-changing system, developed by a lengthy process of self-organization. Unless we take this seriously, we can't understand *adult* cognition. It followed that developmental neuroscience/psychology isn't an optional extra, to be studied by those who like that sort of thing and safely ignored by everyone else. On the contrary, it's central to neuroscience/psychology *in general*.

She wasn't making an empirical claim, although she did cite empirical evidence in support. Rather, she was making a philosophical point. To ask, like McCulloch for example, whether the structuring information is in the genes or in the stimulus, or even in some epigenetic interaction between the two (see subsection c), is to imply that the system—genome, embryo, brain, cortex, feature-detector . . . —accepts its structure from something else. This, she argued, is unsatisfying *in principle*, because it leaves the ultimate origin of structure unexplained. We must show how the system can structure itself, owing to its inherent nature. If not, then the mystery remains.

The psychologist Esther Thelen (1941–2004) agreed (1989: 556–7). Putting empirical flesh onto Oyama's philosophical bones, she studied integrated motor actions, including those described by Sherrington (Chapter 2.viii.d). She focused on the nature and development of rhythmic patterns such as walking, kicking, rocking, and communicative waving, and the sudden shift between (for example) crawling/toddling, suckling/ingesting, and sleep/waking (1981, 1985, 1989; Thelen *et al*. 1987; A. Fogel and Thelen 1987; Thelen and Smith 1994).

Where Marr had ascribed bodily skills to specific neurone circuits, and Pellionisz and Llinas to tensor transformations, Thelen ascribed them to stable attractors in dynamical systems (see Chapter 7.vi.g).

Everyone allows, Thelen said, that behaviour arises from a 'system'. But they usually do this only in the Discussion section, after admitting failures in their main-effect theories (1989: 557). She had scant respect for these empty admissions, which "can dilute systems concepts to the point of vacuousness" (1989: 557). What we need, she said, is a mathematically precise account showing how complex systems can produce emergent order without a prior description of it, and how one type of order is 'chosen' over another.

For Thelen, what are normally regarded as mentally guided, pre-prepared, actions are best thought of as forms of self-organizing bodily skill. She ascribed such skills to stable attractors in dynamical systems (see Chapter 15.viii.c–d). Even a small change may tip the system into the basin of a different attractor, so we should expect sudden behavioural shifts. But not *any* change will do: we must discover the 'switches' involved.

On this view, sensori-motor development involves the construction and stabilization of new attractors, and the 'setting' of control parameters that shift the system from one to another. The system complexity is hierarchical, since new attractors are generated by "a cascade of successive bifurcations or phase-shifts" (1989: 570). (Hence the impossibility of going *directly* from a trot to a gallop.)

For example, consider Piaget's "A-not-B error", in which infants search for a toy in its original place even though they've seen it being moved (Chapter 5.ii.c). Piaget himself had explained this in cognitive terms, speaking of the developing "object concept". But Thelen didn't. Instead, she analysed perseverative reaching by differential equations representing the interacting constraints involved (Thelen *et al*. 2001).

These constraints included alternative movement-suites such as reaching and looking (*seeing*, for Thelen, is a form of motor activity); the distance between object and child; the perceptual salience of the hiding place; the time passed before the child is allowed to reach for it; the attractiveness (for the child) of the object; and the past history of the child's actions in respect of it. Each of these had previously been shown to affect whether the child succeeded (which is a puzzle, if some object concept 'inside the head' is supposed to be responsible). But Thelen united them in a coherent way.

Her approach to action was very different from GOFAI planning. This had already been criticized as over-intellectualized by people within the AI stable. Now, Thelen provided psychological meat, and measurement. (How far dynamical theorizing can be applied to paradigm cases of 'rational' planning remained controversial: see 12.vii.c.)

Although her theory was couched at the psychological level, Thelen related her findings to neural mechanisms whenever she could. Among the neuroscientists she cited with approval was Berkeley's Walter Freeman. He'd been the first to apply chaos theory (a variant of dynamical systems theory) to the brain.

'Chaos' in the mathematical sense is very different from 'chaos' in the everyday sense: it sometimes looks random, but it isn't. Chaotic phenomena (which include the weather and the heartbeat) are deterministic, but in practice unpredictable. The reason is that they're hugely influenced by tiny variations in initial conditions. Nevertheless, their overall behaviour shows a number of holistic patterns, or dynamical attractors.

Freeman had been drawn to dynamical theorizing by Turing's 'Morphogenesis' paper and by Ilya Prigogine's work on dissipative structures in physics (J. A. Anderson and Rosenfeld 1998: 31–2). From the 1960s on, he'd experimented—unfashionably—with EEG recordings. Most of his peers paid little attention: as he put it later, there was "a virtual obsession with unit recording" (J. A. Anderson and Rosenfeld 1998: 27).

By the late 1980s, however, some were ready to listen. (Both biological-Turing and Prigogine had by then become names to conjure with: Chapter 15.iv and viii.d.) A widely read paper on the neurobiology of smell, written with Christine Skarda, appeared as a target article for interdisciplinary peer review in *Behavioral and Brain Sciences* (Skarda and Freeman 1987). It claimed that a particular scent is represented (after Hebbian learning) not by single-cell feature-detectors but by a spatial pattern of EEG-recorded activity distributed over the brain's olfactory bulb, and involving "every neuron of the bulb".

And smell was just the beginning: "We hypothesize that chaotic behavior serves as the essential ground state for the neural perceptual apparatus [in general]," and "[We see] implications of our neural model for behavioral theories [too]" (Skarda and Freeman 1987: 161). This claim received an even wider audience four years later, when it appeared in *Scientific American* (Freeman 1991).

As a young man, Freeman had avoided computer models:

> [This] was, of course, when the perceptron was in vogue and a dozen other network type of devices . . . which I looked upon as, well, interesting gadgets, but they had nothing to do with how nervous systems work. (J. A. Anderson and Rosenfeld 1998: 27–8)

Later, he did do computer modelling alongside his extensive animal experiments (Freeman 1979, 1987). But further simulation (and experiments) would be needed: "[Our claims can't be advanced] until engineers have built hardware models based on our equations and determined whether they behave the way parts of brains do" (Skarda and Freeman 1987: 170).

Many neuroscientists weren't persuaded—some, because they were still in the grip of the "obsession" with unit recording. In 1993 Freeman said:

> We've found the same dynamical patterns as in the olfactory system in the visual and auditory and somatosensory cortices. But they [other neuroscientists, such as Koch and Francis Crick] are still interpreting the process in terms of coupled single cells. They characterize it as "the binding problem"—how to get together a bunch of feature detectors. They're not thinking of the processes in terms of a dynamical systems approach. (J. A. Anderson and Rosenfeld 1998: 38)

Others were sceptical for different reasons. Chaos theory, Donald Perkel (1987) suggested, was a "fad", one in a long line of technological–mathematical metaphors for the brain (he cited under-sea cables, telegraphs and telephones, holograms, tensors, maps, non-linear networks, and computer programs) that were superficial or even misleading. Chaos theory had recently become popular. Indeed, a widely read account by a science journalist had fed the "news-stands" excitement mentioned above with respect to Linsker (Gleick 1987).

Despite his use of the word "fad", Perkel couldn't accuse Freeman of jumping on the bandwagon. For Freeman had been using chaos theory for years. But he did accuse him of being seduced by the mathematics:

> [The] beauty, versatility, and power of the mathematical approach may have led its aficionado to find areas of application in the spirit of the proverbial small boy with a hammer, who discovers an entire world in need of pounding. (Perkel 1987)

He recalled Sherrington's (1940) description of the brain as an "enchanted loom", wherein "millions of flashing shuttles weave a dissolving pattern though never an abiding one; a shifting harmony of subpatterns". This newly fashionable approach, said Perkel, *might* be the right way to describe the loom's activity. But only "more fine-grained experimental evidence and correspondingly realistic simulation studies" could show this to be so.

The target authors had already made the last point themselves (1987: 170–1). Now, in their *BBS* Reply, they countered that their theory was "no more or less metaphorical than any other use of descriptive equations properly selected" (p. 187). If it seemed promising, they said, it would continue to be used; if not, some other theoretical tool would be found. That's how science works.

For our purposes, what's interesting is that Skarda and Freeman had considered self-organization at all. But they soon had company: eventually, Freeman co-edited two special numbers of the *Journal of Consciousness Studies*, in which several authors defended comparable views (Núñez and Freeman 1999). Thanks to both dynamical systems theory and connectionism, the last quarter-century saw this topic being taken seriously in neuroscience, much as it was in theoretical biology and A-Life. Even Ashby was now respectable.

'Brain-as-program' had suffered a stunning blow (though not a knockout: see 12.ix.b). But computational neuroscience *in general* hadn't. In fact, this change couldn't have happened without it.

c. Epigenesis

The computational work described in subsections a–b rode a coach and horses through the familiar notion of 'innateness'. For it destroyed the cosy assumption that behaviour already present at birth *must* be genetically specified. (Sometimes, such behaviour is partly due to 'external-world' learning in the womb. It was known by the 1980s that newborn babies can recognize their mother's voice—and also the intonation patterns of her language: DeCasper and Fifer 1980; Mehler *et al.* 1988; Kisilevsky *et al.* 1989.)

Most people had previously thought that behaviour must be due either to genetic specification or to post-natal learning. The long-standing debate on 'innate ideas', for example, reflected this assumption (see Chapters 2.vi.a and 9.ii.c and vii.c–d). But there'd been an important exception: namely, Jean Piaget (Chapter 5.ii.c and 7.vi.g).

Piaget had argued since the 1920s that behaviour is neither purely innate nor purely environmental, but a subtle "epigenetic" interaction of the two. (He borrowed this term from the biologist Conrad Waddington: see 15.iii.b.) He'd offered psychological evidence, based on observations of babies and children. But convincing neurobiological evidence wasn't then available. Cognitive neuroscience didn't get off the ground until Piaget's old age (see Section x.b, below), and the *developmental* version blossomed only after his death.

By end-of-century, however, developmental cognitive neuroscience was an established discipline, with its own textbooks (e.g. M. H. Johnson 1993*a*, 1996). By that time, many examples of epigenesis, in brain as well as behaviour, had been discovered.

As we saw in Chapter 7.vi.g, six co-authors—including neuroscientist Mark Johnson, connectionists Jeffrey Elman and Kim Plunkett (12.viii.b–e), and Piaget's ex-colleague Annette Karmiloff-Smith—wrote a book called *Rethinking Innateness*, accordingly (Elman *et al.* 1996). This book challenged the widely held belief that cognition rests on inherited, unchangeable, 'modules'.

That view had been championed by two towering figures in the early years of cognitive science, Jerry Fodor and Chomsky (see 7.vi, 9.vii.c–d, and 16.iv.d). Meanwhile, evolutionary psychologists such as Steven Pinker and Leda Cosmides had applied Chomsky's and Fodor's ideas to many different types of behaviour, which they supposed to be innate in a strong sense (Chapters 7.vi.d–e and 8.ii.d–e). The modular, "Swiss army knife", picture of the mind/brain was rife.

Nevertheless, the evidence against 'simple' modularity was now strong—not modules, but modularization, was the key (Karmiloff-Smith 1992). Granted, something like Fodorian modules exist in the adult brain. But they weren't there at the beginning, and their specific content depends crucially on what happens during development.

Let's consider the example of face recognition, a skill possessed not only by humans but also by birds and mammals. Building on research on imprinting in chicks by Gabriel Horn (one of my anatomy supervisors for human dissection at Cambridge!), Johnson constructed a biologically plausible model of the forebrain region involved (O'Reilly and Johnson 1994). This generated a range of 'behaviours' like those observed in imprinting experiments, and provided various predictions for further experimental study.

He and Horn also did experiments on chicks (Johnson and Horn 1986, 1988). These showed that imprinting is grounded in two independent neural mechanisms, one subcortical and one cortical, with *no* neural connections growing between them. The first, present at birth, makes the chick turn towards stimuli—natural or artificial—patterned like certain elements of a hen's head and neck. (This mechanism is *not* strictly species-specific: it also works if the stimulus is a duck's face, or even a polecat's.) The second develops later, enabling the chick to distinguish its mother from other hens.

Soon afterwards, with the psychologist John Morton (Director of the MRC's Cognitive Development Unit in London), Johnson turned to study face recognition in young babies (M. H. Johnson and Morton 1991; Morton and Johnson 1991; Johnson 1993*b*). And "young" means young: some of the baby subjects were only half an hour old (and other researchers had even got down to 10 minutes). The method used was to record the baby's head and eye movements in tracking—or ignoring—various stimuli: faces, jumbled 'faces', and non-faces. Here also, his experiments suggested two independent neural mechanisms, with different behavioural effects and time courses.

The first is subcortical, as it is in the chick. It develops in the womb, and its function is to make the newborn baby orient to, and fixate on, facelike stimuli: two dark blobs above another blob, in the general spatial relations of eyes and mouth. 'Scrambled' or malpositioned stimuli are ignored (as they are by the chick). Moreover, any face will do: mother, puppy, teddy bear . . . Given three blobs in the relevant positions, the newborn will orient towards them.

The other mechanism is cortical. It develops two or three months after birth, 'bootstrapping' on the first. But this is a behavioural bootstrapping, not a matter of neural connectivities. The blob system does its job simply by predisposing the neonate to look towards faces. What happens after that doesn't affect it.

The newborn baby shows no preference for pictures of faces over crude facelike blob designs, and doesn't seem to notice any difference. Gradually, however, a preference for faces—and for individual faces—emerges (*sic*). Since the baby's visual attention is drawn to faces, which in ecologically normal conditions will belong to other humans (especially the mother), he/she will receive many stimuli that can be provided only by these particular experiences. As Johnson and Morton put it, the baby is born into a "species-typical environment" which leads to the construction of more complex mechanisms from the seed of the "primal components"—e.g. the blob–attention circuit.

The environmental face stimuli configure the developing cortical circuitry before it gains control over behaviour in the third month of life—after which *that* circuit disposes the infant to look at faces. (The subcortical circuit appears to 'fade' around the fifth week of life, when preferential face tracking declines sharply—M. H. Johnson and Gilmore 1996: 343.) Gradually, the baby learns to prefer human faces to teddy-bear faces, and eventually to individuate them—normally starting with recognition of the mother.

Presumably, detailed facial-feature-detectors, such as those identified in monkeys by Perrett (Section iv.d), develop as a result of this brain–environment interaction. These may even include cells specialized to recognize expressions of disgust (Richard M. Young *et al*. 1997). But if so, these work only on human, or human-like, faces.

Facial-feature-detectors are typically species-relevant, because the infant is normally surrounded by members of its own species. (This adds a new dimension to Ludwig Wittgenstein's famous remark, "If a lion could speak, we would not understand him"—see Chapter 16.ii.b.) But the developing brain can be fooled by abnormal experience. For instance, if a horned sheep is reared with sheep without horns, it doesn't develop horn-detector cells—as it does if reared naturally, with other horned sheep (Kendrick and Baldwin 1987).

If one must use the term "innate", then what's innate here is the *predisposition* to orient towards crudely facelike stimuli. "Knowledge" of faces-as-such, or of their individual features, certainly isn't innate. By contrast, there's no innate predisposition to look at telephones. To be sure, some of the feature-detectors that will later be involved in learning to recognize telephones are present at birth. They aren't "learnt by experience" (although they are later tuned by experience). But as we saw in subsection a, they aren't specified by "heredity" either.

Granted, certain aspects of brain circuitry do appear to be genetically controlled. These include the shapes of the neurones' dendritic trees; how much these trees branch; where, and to which cell classes, a neurone's axon projects to; how strong the initial connections are; and what neurotransmitters are effective (J. A. Anderson *et al.* 1990*a*: 296). So there's no question of the brain's starting out as a randomly wired system, as in Linsker's work, or von der Malsburg's. But that's a far cry from saying that observable behaviours are innate.

In short (and as we've already seen in Chapter 7.vi.g), nativism and environmentalism are equally misleading. The adult mind/brain is formed by an epigenetic interplay, on many successive (bootstrapping) levels, of brain, behaviour, and environment.

The "environment", of course, includes the *cultural* environment. This provides cognitive technologies (6.ii.c) ranging from language and writing to shipboard instruments and navy rituals (8.iii). These don't merely *aid* the mind but, to a large extent, come to *constitute* it (see A. J. Clark 2003*a*). They become internalized within the brain, so that the brains of two adults from different cultures will be more different from each other than are the brains of any two newborn babies.

Various examples of behavioural "scaffolding" have been reviewed by Horst Hendriks-Jansen (1996), within a wide-ranging critique of GOFAI cognitivism. Although neural epigenesis can't be captured by GOFAI, or even by most connectionism, it has been simulated in more 'realistic' computer models. Some of these focus on neuroscientific details such as orientation detectors, but others deal with socially significant human behaviour (A. W. Young and Burton 1999). (For a very different, non-epigenetic, model of face recognition, see Duvdevani-Bar *et al.* 1998.)

One caveat remains. Oyama's critique of the developmental sciences applied to most epigenetic theories too (1985, ch. 3). The "heart" of the theories she was rejecting is the assumption that

> form, or its modern agent, information, exists before the interactions in which it appears and must be transmitted to the organism either through the genes or by the environment. (1985: 25)

"Compromises", she said, "don't help because they don't alter this basic assumption." We should abandon the nature/nurture duality and concentrate on *how the system creates form (order, structure, information) by its own spontaneous activities*. To be sure, it's sometimes convenient to regard genes, or feature-detectors, or interneurones . . . as simply *there*. But they weren't put there by some pre-existing force, nor even by two such forces interacting. They emerged within the living system, as a result of general principles and specific biological/ecological constraints.

d. Neural selection

In the 1950s, the UCL-based anatomist J. Z. Young questioned the widespread assumption—shared from Hartley to Hebb—that learning happens by facilitating the neural pathway that's just been excited. Instead, it might happen "by reducing the effectiveness of the other" (J. Z. Young 1964: 282).

If so, then "learning occurs by elimination of the unused pathways", involving "a reduction of the initial redundancy of connexions". Like Darwinian evolution, learning produces "adaptation by random small changes and shuffling demand selection among large numbers" (p. 288). But it's less random, being guided by "rapid and precise feedbacks [providing] precise information about the results of each action".

The "mechanism" (he continued) was an enigma, calling for "inquiry into the fundamental nature of the information-gathering circuits and the types of models [in Craik's sense] that they produce" (p. 298). The functional aspects, he said, had been illuminated by the learning machine built by his UCL colleague Wilfred Taylor (Chapter 12.i.d). But the molecules were a mystery.

Over the next thirty years, this idea was taken up in a number of fields. By the late 1980s, neurochemists had clarified the mechanism, in terms of various neurotransmitters and other molecules. Anatomists had shown that synapses continue to proliferate immediately after birth, but soon become fewer—sometimes, they're reduced to under 50 per cent.

Psychological experiments in the 1960s had implied that this pruning results from non-use (e.g. C. Blakemore and Cooper 1970). It was later confirmed that if brain circuitry is deprived of its normal input it will degenerate and/or be taken over for some other function. So, for instance, auditory cortex may develop systematically structured *visual*-feature-detectors (Sur *et al.* 1988, 1990). And (as remarked in Chapter 7.vi.i) the auditory cortex of congenitally deaf children becomes organized so as to compute visual input for linguistic purposes. (Much the same thing happens in A-Life evolutionary robots, where unused neural 'whisker drivers' can get taken over for visual purposes: Chapter 16.vi.c.)

Several neuroscientists integrated these results by explaining the brain's plasticity in terms of "neural selection". Foremost among them were Jean-Pierre Changeux (1936–), of the Institut Pasteur and Collège de France, and Gerald Edelman (1929–), initially at Rockefeller University, then UC San Diego.

Changeux did pioneering work on the molecular mechanism of neural selection (Changeux *et al.* 1973; Changeux and Danchin 1976), and later discussed the development of various neuropsychological "levels", including language (Changeux 1980, 1985; Changeux and Dehaene 1989). Edelman was interested in the synaptic chemistry too, but his main goal was to explain cognition—and consciousness (G. M. Edelman 1978, 1987, 1989; Edelman and Tononi 2000). Already a Nobel prizewinner (in 1972) for his work in immunology, his neuroscience attracted great interest—and withering criticism too, as we'll see.

Neither man shared McCulloch's 'informational' worry about genetic coding: Changeux, for instance, said it was only an "*apparent* paradox" (Changeux and Dehaene 1989). His reason was the huge number of *combinations* of sets of genes: "On strictly theoretical grounds, no opposition thus exists to a full genetic coding of brain organization" (Changeux *et al.* 1984: 125). Nevertheless, neither believed in a pre-set genetic coding of specific connectivities. *Selection*, not *instruction*, was the name of the game. A profusion of connectivities is initially available, only some of which will be stabilized during development or learning.

Young hadn't been able to outline a plausible mechanism. Now, advances in immunology offered a powerful analogy. Edelman himself had shown that all possible antibodies are potentially present at birth, but that only those which match—as he put it, "recognize"—the antigens happening to invade the body are "selected" for clonal reproduction (G. M. Edelman 1992: 74–8).

Applying these ideas to the brain: the hugely diverse "pre-representations" (Changeux's term), or items in the "primary repertoire" (Edelman's), are selectively stabilized or destroyed/damped down by the specific input. And as in Grossberg's ART, this happens by some sort of resonance, or reverberation, between percept and pre-representation. Feedback (for Edelman: "re-entrant connections", influenced by "values" which modulate the Hebbian rule being used) leads to the loss of unused connections and the strengthening of the others. Moreover, Hebb would be happy: besides single connections, *groups* of co-acting neurones (particularly stressed by Edelman) are formed. These are organized in systematic ways—often, as topographical maps.

What's of special interest here is what these neuroscientists said about computational views of the mind/brain. Both rejected functionalism, classical cognitive science, and mainstream connectionism. Yet Changeux looked forward to more realistic connectionist models, and Edelman produced some of the most complex neurocognitive models so far.

Changeux located his work in a long philosophical tradition (epigenetic theory: Chapter 7.vi.g–i), and related it to recent philosophy of mind and cognitive science (Changeux *et al.* 1973; Changeux 1985, ch. 7). Like Piaget, he believed that one can't understand adult cognition unless one understands how it developed. So he had no time for GOFAI, nor for most connectionism, because—in both cases—the instructions/structure are provided 'externally' by the modeller.

Moreover, he rejected the dogma of orthodox cognitive science: multiple realizability (Chapter 16.iii and iv.c–d). For Changeux, the molecular mechanisms at the synapse put functional constraints on development at higher levels, so even cognitive psychologists shouldn't ignore them (Changeux and Dehaene 1989; cf. M. H. Johnson and Karmiloff-Smith 1992).

He explicitly dismissed Fodor's vision of psychology as a theoretically autonomous "special science" (Chapter 16.iv.c), and rebutted Philip Johnson-Laird's assertion that "the physical nature [of the brain] places no constraints on the pattern of thought . . . any future themes of the mind [being] completely expressible within computational terms" (Changeux and Dehaene 1989: 63). His aim, by contrast, was "to *reconstruct* (rather than reduce) a function from (rather than to) its neural components"—and to do this by recognizing the brain's inherent powers of organization.

If most of the then current cognitive science was on the wrong track, Changeux said, suitably realistic computer systems wouldn't be. Connectionist models of highly evolved functions, such as NetTalk or the past-tense learner (Chapter 12.vi.e–f), were "far too simple and even naive compared to what the human brain actually uses for . . . multimodal performances with deep cultural impregnation" (Changeux and Dehaene 1989: 99). Instead, we needed "brain-style computers based on the actual architectural principles of the human brain and possessing some of its authentic [self-organizing] competences rather than simply mimicking some of its surface performances". Hopfield's cooperative networks (12.v.f) were interesting, he allowed, but didn't include selection; von der Malsburg's work was more biologically oriented, so more promising.

Changeux himself went some way towards producing "brain-style" simulations. Besides modelling his wetware ideas (Gouze *et al.* 1983), he implemented a selective network for learning temporal patterns, such as birdsong (Dehaene *et al.* 1987). But

Edelman tried to go much further, modelling "multimodal performances" if not "deep cultural impregnation".

From 1980 on, Edelman's team implemented a still-ongoing series of computer models called Darwin-I, Darwin-II, and so on, plus various 'spin-off' systems (Reeke *et al.* 1990; Krichmar and Edelman 2002). These models focused on perceptual categorization, and on the integration of perception and action. Some included neuronal group selection and/or re-entrant circuitry, but others didn't. Similarly, some had "values" modulating the Hebbian rules, while others didn't. And whereas some were 'pure' simulations, others were robots (the NOMADs: Neurally Organized Multiply/Mobile Adaptive Devices).

Darwin-I, in 1981, was a pure simulation, and focused on pattern categorization. Darwin-III, nine years later, simulated the development of sensori-motor coordination in reaching behaviour (Edelman and Reeke 1990: 47–61). The first incarnation of NOMAD—alias Darwin-IV—appeared in 1992, and added visual tracking, conditioning, and pattern categorization (G. M. Edelman 1992: 91 ff., 192–3). Darwin-VII was completed by 2002, and by mid-2004 Darwin-IX was well under way, adding artificial whiskers (simulating rat neurology) which can both learn and discriminate different textures. (As this book goes to press, the most recent published account describes Darwin-X; this uses a simulated hippocampus to aid navigation and spatial exploration: Krichmar *et al.* 2005.)

Even the relatively early models drew heavily on neuroscientific data. For instance, one simulated the innervation of the back and palm of a monkey's hand, and the way in which their different tactile 'experiences' lead to the formation of differently organized groups of receptive cells (cortical maps) in the monkey's brain (Pearson *et al.* 1987).

The most recent systems are comparable to Grossberg's in size and complexity—and run similar risks of opacity. As some fellow neuroscientists have said:

> One reason that many feel uncomfortable with learning by selection is that it makes a proclaimed virtue of complexity and imprecision. There is no sense at all of elegance, proportion, and abstract beauties of simple systems, well understood. Therefore the whole modeling approach sometimes seems slightly mystical. It is difficult to see what is actually going on in the simulations, that is, to see what the mechanisms assumed are doing to the behavior of the network. However, there is no requirement that nature is necessarily easy to understand. (J. A. Anderson *et al.* 1990*a*: 299)

To put the point in another way, it is as though we can get only so far by using what Marr called Type-I theories (Chapter 7.iii.b). The post-selection model itself is, to a large extent, a Type-II theory.

The current NOMAD (as I write), alias Darwin-VII, which isn't 'evolutionary', has about 20,000 units and 450,000 synapses, and models eighteen cortical and subcortical brain regions (Krichmar and Edelman 2002: 819). It attempts the "multimodal processing" requested by Changeux, for it integrates visual and auditory signals in categorizing the objects in its environment. The team is planning to add tactile whiskers, and probably a 'hippocampus' too (A. Seth, personal communication, June 2004; see Krichmar *et al.* 2005). Other plans are afoot to add re-entrant connections *between* neural regions, as well as within them; to explore the spiking dynamics of *individual* neurones, instead of merely averaging over small groups; and—for practical, not theoretical, purposes—to construct *hybrid* systems in which a NOMAD is linked to a conventional digital computer (Krichmar and Edelman 2002: 827).

The NOMAD of 2002 has a gripper that picks up visually distinguishable cubes and "tastes" their electrical conductivity. It "values" high conductivity, so can be conditioned to pick up striped cubes and avoid spotted ones (only the striped cubes have high conductivity); and it shows higher-order learning, by chaining conditioned stimuli. This NOMAD-ic creature learns to integrate auditory and visual signals in locating the cubes, since the striped cubes emit high tones and the spotted cubes low ones. It has horizontal- and vertical-feature-detectors, which influence categorization differently according to the temporal order of its 'experience'.

I just said that the NOMADs were robots. But Edelman didn't say that. For him, they were "noetic" (perceptual) devices, quite "unlike" the more familiar cybernetic and robotic artefacts (1992: 192). His caption of a diagram showing what is clearly a wheeled robot was this:

> While NOMAD looks like a robot, it does not operate like a robot under the strict control of a program. It operates like a noetic device, one that is neurally organized and works according to selectionist principles. (1992: 193)

The implication was that anything properly termed a robot is a GOFAI system of the sort described in Chapter 10.iii.c. He simply ignored the fact that some robots were already being based on very different principles, from Arbib's *Rana computatrix* to evolved robot cockroaches and fish (see Chapter 15.vi.c, vii, and viii.a).

Similarly, he repeatedly denied that his computer models were "computational", or even "connectionist". Darwin-VII, for instance, was said to promise "the development of intelligent machines that follow neurobiological rather than computational principles in their construction" (Krichmar and Edelman 2002: 818).

His models were presented as being based on new principles, originated by him. For instance, when he reported the model of cortical maps of the hand (Pearson *et al.* 1987), his contemporaries—his potential rivals—were ignored. Von der Malsburg wasn't mentioned, and nor were Grossberg, Arbib, Longuet-Higgins, Kohonen, or any other brain-modeller (although one survey was cited: Meinhardt 1982). The great Turing was acknowledged, for his work on morphology (Chapter 15.iv); and so was the luminary Hebb. All the other references were to experimental data from neuroscience (plus one to a computer model of crystallography by one of Edelman's colleagues).

Yet, in the judgement of three neuroscientific experts, "It is likely that the assumptions made in [Edelman's] complex models contain the essential core analyzed by Amari and by Willshaw and von der Malsburg" (J. A. Anderson *et al.* 1990*a*: 125). In short, whereas Changeux made a point of locating his own ideas in their intellectual context (both scientific and philosophical), Edelman didn't. When he mentioned that context at all it was usually to contrast, or even to scorn, not to give credit. As Daniel Dennett put it, "he seems to have a low opinion of everyone else in cognitive science" (1993*a*: 285).

This insistence on his own heroic creativity (Chapter 1.iii.e) is part of the reason why Edelman was criticized with such acerbity. For instance, Barlow's review of *Neural Darwinism* opened by noting that Darwinian theories were "much in the air these days", and had been used to good effect in neuroscience (he cited Changeux's work on birdsong). After this coded snub, the review went into high gear. Edelman's version of these by now familiar ideas was described as obscure, incomplete, one-sided, and mistaken on crucial facts—and his suggested selection process "would probably wreck the function of the whole brain". The piece ended with a gentlemanly caveat carrying a vicious sting in its tail:

> But epoch-making books are often panned unfairly by bigoted reviewers, so readers are strongly urged to study "this magisterial work" (as the publisher's blurb calls it) and decide for themselves whether it ushers in a new era in neuroscience, or whether it's just a hopeless muddle. (Barlow 1988)

Other stings were in store. Crick (1989*b*) soon complained that the theory should be called "Neural Edelmanism", since it bore no significant resemblance to Darwin's theory—and nor could he see any real benefit in applying evolutionary theory to self-organization in the brain. These were clashes of Titans: two great names infuriated by another. (Edelman cited both reviews in a popular book, crying "Vive le sport!": 1992: 94–7, 257.) But other people, too, were offended by Edelman's self-serving style. For instance, Dennett—having already complained of Edelman's "low opinion of everyone else" (see above)—tartly remarked that his work "would be a nicer try if he didn't present it as if it were such a saltation in the wilderness" (Dennett 1995*a*: 397 n.).

Edelman's self-congratulatory rhetoric reached its zenith in his trade books (1989, 1992), whose core messages were that he'd pretty well solved the problem of consciousness, and that "The brain is not a computer." Only members of his own trade unions, biochemists and neuroscientists, were allowed any authority or credited with any common sense. He recycled familiar criticisms of classical cognitive science and early connectionism—so ungenerously, that several readers of my acquaintance were led to defend positions they'd rejected long before.

In particular, he belittled the philosophers at length and en masse (1992, ch. 15). And he quoted an anonymous university president thus:

> Why is it that you physicists always require so much expensive equipment? Now, the Department of Mathematics requires nothing but money for paper, pencils, and waste paper baskets, and the Department of Philosophy is better still. It doesn't even ask for waste paper baskets. (G. M. Edelman 1992: 159)

Just good knockabout stuff? We all like a good fight. (And the story about the university president is genuinely funny.) Remember "Vive le sport!", and forget that Wittgenstein, for instance, consigned his entire previous œuvre to the waste-paper basket. If Chomsky could famously get away with mocking his opponents (9.vii.b), why reproach Edelman?

The short answer is that whereas Chomsky's mockery was an amusing frill tacked onto a careful argument, Edelman's was riddled with unexamined assumptions, misattributions, and crude misjudgements of the views he was ridiculing. For instance, when triumphantly noting (1992: 263; see also Edelman and Reeke 1990: 68) that Hilary Putnam had abandoned his own baby, functionalism, he failed to mention—even failed to realize?—that *in the very same book* Putnam had also given up on realism (see Chapter 16.vi).

The long answer is given in the discussion of consciousness below (Sections x–xi) and in Chapter 16 throughout. We'll see that Edelman's brisk adoption of three basic assumptions—realism, evolution of mind, and naturalism—was too easy (1992, chs. 12, 15). Certainly, these are accepted by virtually all neuroscientists (though not Maturana), and by most cognitive scientists too—myself included. But they're problematic nonetheless. Adopting them without argument, or dismissing opposing positions as mere "cotton candy" (as Edward Feigenbaum does: Chapter 11.ii.f), isn't good enough.

e. Grandmother cells

"It is with a sense of proud shame that I accept fathership of the 'grandmother' cell"—so wrote Lettvin (1991), recounting a bizarre story he'd made up in 1969 for an undergraduate lecture. Too complicated to summarize here, it featured an imaginary Russian neurosurgeon and the mother-obsessed Mr Portnoy of Philip Roth's just-published novel *Portnoy's Complaint*. It also featured imaginary cells (not just one, but "18,000") in Portnoy's brain which fired whenever he thought of his mother, and others which fired in respect only of his grandmother.

His terminology had caught on so quickly that Barlow used it a few years later without even bothering to define it (Barlow 1972). Later, however, he did define it: talk of grandmother cells describes

> the notion that there are cells in the brain that become active when and only when a grandmother, or some other arbitrary but specific feature, is present to the senses. (Barlow 1995: 421)

The idea was already familiar when Lettvin gave his lecture, if the memorable terminology wasn't. (Konorski, as we've seen, had called them "gnostic" units—although Lettvin hadn't heard of Konorski's work at the time—C. G. Gross 2002: 84.) And its popularity has waxed and waned over the years.

In the 1920s, Lashley argued that "It is very doubtful that the same neurons or synapses are involved even in two similar reactions for the same stimulus", and that his experimental results (on maze learning in rats) were "incompatible with . . . any theories which assume that particular neural integrations are dependent upon definite anatomical paths specialized for them" (Lashley 1929*a*: 3). Twenty years later, Hebb (1949) muddied the water by positing specific "cell assemblies" *as well as* broadly ranging "phase sequences" (Chapter 5.iv.c). But Lashley reiterated his earlier opinion:

> All of the cells of the brain are constantly active and are participating, by a sort of algebraic summation, in every activity. There are no special cells reserved for special memories. (Lashley 1950: 477)

Pitts and McCulloch (1947) didn't go quite that far. Indeed, even Lashley had admitted some specialization in the visual system: "[Equipotentiality] probably holds only for the association areas and for functions more complex than simple sensory or motor coordination" (1929*a*: 25). But they did contrast their mosaic theory with the "specialized neuron" earlier posited by "the school of Hughlings Jackson" (see Section iii.a).

Yet only ten years later, they helped describe the frog's bug-detectors. And a plethora of increasingly exotic feature-detectors soon followed, as we've seen (iv.b). By 1970, the grandmothers were flourishing: it was widely believed that there may be cells which respond to faces, perhaps even cells which respond to one's grandmother. (Not just one, but many: remember Lettvin's "18,000".)

They were flourishing in the computer labs, too. Many modellers in the 1950s–1970s thought in terms of computerized grandmother cells, and Uhr and Vossler compared their version of Pandemonium to the cells newly described by Hubel and Wiesel (Section iv). Those GOFAI programs were connectionist in spirit, as we've seen (12.ii.d). But they were localist in nature (one unit, one concept), as most early neural network models were.

However, the numbers didn't add up. Lettvin's original story had envisaged 18,000 grandmother cells per grandmother. Yet Mayhew's remark at the opening of this chapter implies there may be only one. Of course, he was speaking informally—but the point stands. The 'Frog's Eye' findings (about the numbers of retinal bug-detectors per bug, and the four correlated sheets of collicular cells) clearly didn't imply *just one* brain cell per bug. However, most neuroscientists who believed in grandmother cells didn't think there were thousands of them, even if there were more than one. But it was soon pointed out—in 1968 (see Page 2000: 445)—that there wouldn't be room in the brain for even *one* neurone devoted to *every* arbitrary combination of features, such as a yellow Volkswagen (C. S. Harris 1980) or a purple Rolls-Royce.

This point was trumpeted to neuroscientists in 1972 (see below), but didn't spread to AI modellers until later. James Anderson remembers "one of the high points" of the first connectionist conference (in 1979) as

> Jerry Feldman [an AI pioneer of localist networks] standing up in front of the group and computing how many cells you'd need to have enough grandmother cells to do vision. The numbers became astronomical extremely quickly. He realized he might have to rethink the problem. (J. A. Anderson and Rosenfeld 1998: 371)

Feldman wasn't the only one. The impossible numbers helped buttress the notion of distributed representation, in which each unit contributes to *many* representations—and, in the ideal case, *every* unit contributes to each representation. That too was an old idea, which went to ground in the late 1960s but later hit the headlines (see Chapter 12). Its sudden popularity seemed to ring the grandmother cells' death-knell: drowned by the 'D' in PDP.

A less extreme version (describing 'sparse' distributed representation) had appeared in 1972, in a paper Barlow wrote for the first volume of *Perception*, newly founded by his long-time colleague Gregory. Although hugely influential, its effect was less to promote interest in distributed representation than to feed the "virtual obsession with unit recording" about which Freeman would complain later. It drew heavily on recent experimental data (feature-detectors, for example), but was primarily devoted to abstract arguments about computational efficiency—Barlow's main concern ever since his Ratio Club days.

His revolutionary paper laid down five "dogmas" for perceptual neuroscience. The first was that theories should be couched at the level of individual cells, not "globalist" EEG potentials or holographic memories (Barlow 1972, sect. 6.1). Despite Lashley's (1950) evidence for mass action (as he'd put it: "every instance of recall requires the activity of literally millions of neurons"), experimenters should concentrate on single cells, rather than averaging over many.

The "unreliability" inferred from recent work on stochastic activity in the nervous system (Chapter 2.viii.f) had been overestimated. Although there is some intrinsic noise, its level is "extraordinarily low":

> Individual nerve cells were formerly thought to be unreliable, idiosyncratic, and incapable of performing complex tasks without acting in concert and thus overcoming their individual errors. This was quite wrong, and we now realise their apparently erratic behaviour was caused by our ignorance, not the neuron's incompetence. (Barlow 1972, sect. 3.5)

A single neurone, he argued, could code a complex reality (compute a complex decision). But there was a hierarchy here: "at the higher levels, fewer and fewer cells are active, but each represents a more and more specific happening in the sensory environment" (sect. 8). The entire visual scene may be represented by as few as 1,000 neurones. Each of these codes, or corresponds to, "a pattern of external events of the order of complexity of the events symbolized by a word" (he recalled the adage that "A picture is worth a thousand words"—p. 371 and sect. 8.3).

Nevertheless, Barlow spurned grandmother cells. He called them "pontifical cells", a term borrowed from Sherrington (1940), who in turn had taken it from William James. James had spoken of "one central or pontifical [cell] to which our consciousness is attached," in which the physical activities of all other brain cells are "combined" (1890: 179). In his Gifford Lectures,

> [Sherrington] introduced the notion of "one ultimate pontifical nerve-cell . . . the climax of the whole system of integration" and immediately rejected the idea in favour of the concept of mind as a "million-fold democracy whose each unit is a cell". (Barlow 1972, sect. 12.4)

(The imagery is telling: it's no accident that a book published in England in 1940 should show an unargued preference for "democracy".) Barlow's rejection of pontifical cells was more carefully reasoned.

He allowed that people who see perception as "a cooperative or emergent activity of many cells" would question his first dogma, saying that "carried to its logical conclusion" it implies there must be a single brain cell corresponding to "each and every recognizable object or scene". And he agreed that this is implausible. That was partly because (as noted above) there aren't enough cells, and partly because a single cell's activity couldn't convey the "richness" of any given perception:

> The "grandmother cell" might respond to all views of grandmother's face, but how would that indicate that it shares features in common with other human faces, and that, on a particular occasion, it occurs in a specific position surrounded by other recognizable objects? (Barlow 1972, sect. 12.4)

Instead, we must posit many "cardinal cells", of whom only a few speak at once: "each makes a complicated statement, but not, of course, as complicated as that of the pontiff if he were to express the whole of perception in one utterance". (Remember the intermediate-level demons in Pandemonium.)

But the neuronal hierarchy is very different from the Catholic Church, since the cardinals outnumber the worshippers in the pews. In other words, there are more cortical neurones capable of being influenced by vision than there are receptors in the retina. Far from there being a *single* cell that recognizes one's grandmother, "the college of these cardinals . . . must include a substantial fraction of the 10^{10} cells of the human brain".

In short, Barlow was recommending 'sparse' distributed representations, in which *only some* of the system's units are active. This was consistent with his second dogma: "The sensory system is organized to achieve as complete a representation of the sensory stimulus as possible with the *minimum number* of active neurons" (italics added).

The paper closed with a frank admission, a telling comparison, and a tacit obeisance to Craik:

> But clever neurons are not enough. The simplest computer program with its recursive routines and branch points has more subtlety than the simple hierarchy of clever neurons that I have here proposed as the substrate of perception.
>
> I think one can actually point to the main element that is lacking. [What is needed is] a corresponding model . . . for our own motor actions and their consequences. Such motor and sensory models could then interact and play exploratory games with each other, providing an internal model for the attempts of our ever-inquisitive perceptions to grasp the world around us. A higher-level language than that of neuronal firing might be required to describe and conceptualize such games, but its elements would have to be reducible to, or constructible from, the interactions of neurons.

Even tensor geometry and dynamical approaches, then, wouldn't undermine Barlow's first dogma—for they study systems composed, at base, of individual units.

Although experimental neuroscientists read Barlow's paper avidly, computer modellers—especially those from an engineering background—didn't. As we saw in Chapter 12.v–vii, a variety of people in the 1970s worked on 'whole-network' distributed representations—including PDP, which grabbed the spotlight in the 1980s. Barlow's sparse theory was sidelined, and grandmother cells apparently forgotten.

But could that really be right? After all, those feature-detectors *had* been found in visual cortex. Perhaps the most basic sensory features had cells all to themselves, while more complex and/or ecologically arbitrary patterns didn't? (Lettvin's story had actually started by imagining the discovery of *mother* cells, which are biologically plausible in a way that grandmother cells—and "yellow Volkswagen cells"—are not.)

The PDP group themselves, when talking about *the brain*, had steered a mid-course between Lashley and Lettvin:

> Clearly, there is specificity. Cells do not respond to all conceivable stimuli or even a small subset of them but are quite specific in their responses. At the same time they are not, in our opinion, so specific as to be what Barlow's dogmas would lead one to expect . . .
>
> We suggest that truth lies somewhere in the middle of the two extreme views and that there is a moderate amount of distribution in the cortex, so that any single cell responds to many things but nowhere near all things. [Both experiments and informational considerations point to] a considerable range of specificities from quite specific to quite broad. (Hinton and Anderson 1981: 43–4)

Geoffrey Hinton (1981) had even proved that features are most efficiently coded by a few very coarsely tuned units, not a multitude of finely tuned ones (12.iv.h). In short, the brain isn't a fully distributed system (as their computer models were)—but it doesn't contain 'pure' grandmother cells either. (Anderson still holds that "single-unit responses report only the dimmest shadows of the real mechanisms of neural computation"—J. A. Anderson and Rosenfeld 1998: 262.)

At the millennium, grandmother made her comeback. *Behavioral and Brain Sciences* published a spirited "localist manifesto", plus thirty-eight pages of peer commentary and response (Page 2000). Mike Page (at Cambridge's Cognition and Brain Sciences Unit) complained that localist representations "have acquired a bad reputation in some quarters" largely because "PDP/distributed" and "localist" had come to be seen as mutually exclusive. As he used the terms, however, "Localist models are characterized by the presence of localist representations rather than the absence of distributed representations" (2000: 443). (Compare Minsky's K-line theory, which combined single-unit significance and distributed processing: 12.iii.d.)

Localism in that sense, Page argued, is more biologically plausible than pure localism—and pure PDP, too. Various tasks—including symbolic modelling and memory for serial order—are tractable only for a localist system, so that "in the domain of psychological modelling, localist modelling is to be preferred". And he offered mathematical arguments showing why, given our psychological abilities, neuroscientists should *expect* to find both grandmother cells and distributed cell assemblies.

One of the many peer reviewers was Barlow himself, who declared:

> Almost all representations have both distributed and localist aspects, depending upon what properties of the data are being considered. With noisy data, features represented in a localist way can be detected very efficiently, and in binary representations they can be counted more efficiently than those represented in a distributed way. Brains operate in noisy environments, so the localist representation of behaviourally important events is advantageous, and fits what has been found experimentally. Distributed representations require more neurons to perform as efficiently, but they do have greater versatility. (Barlow and Gardner-Medwin 2000: 467)

Barlow was now having second thoughts about just how sparse a sparse representation should be (Barlow 2001*a*,*b*; Gardner-Medwin and Barlow 2001). He'd originally emphasized *economy* in neural coding, having "eagerly stepped on the boat" of Shannon's and Fred Attneave's (1954, 1959) studies of redundancy. This approach was popular for a while, but then "information theory dropped out of the limelight in neuroscience" until a reawakening in the 1980s. His new, or newly revived, "take-home message for the neuroscientist" was "Think probabilities" (Barlow 2001*b*)—see Section iii.b, above.

He now argued (against Shannon and Attneave) that coding should convert the hidden redundancy into an explicit, immediately recognizable, form rather than reducing or eliminating it. Fast behavioural response needs cortical representations with minimum overlap (i.e. very few elements activated by both inputs) when two inputs need to be distinguished, but not when they don't—in learning to respond similarly to both, for instance. So the second dogma bit the dust:

> Economy in the number of neurons used for the representation of sensory information is a bad idea, and the reverse is what actually seems to happen. On the other hand economy in the number of *active* neurons would make redundancy explicit rather than hidden, and would make each impulse represent an important, informative, event. (2001*b*, sect. 4)

Thinking probabilities, he said, can help us to understand *why* representations in the cortex "appear to use extravagant numbers of cells" (Gardner-Medwin and Barlow 2001: 477). In other words, we should drop the assumption that the more patterns a memory can hold, the better (see 12.v.c).

The truth about grandmother cells, whatever it turns out to be, will help decide which 'brainlike' models we favour. But it can't undermine computationalism as such. Much of the argument has concerned the computational power of different information-processing systems, and both 'unit-based' and distributed models have been implemented. Computational neuroscience can love its grandmothers or leave them.

f. Modelling modulation

For the first half-century of brain modelling, the general assumption was that a neurone can affect only those others which are connected to it, either directly or indirectly. To be sure, chemicals were involved: neurotransmitters at the synapse enabled one neurone to excite or inhibit the next. But these were thought of as mechanistic nuts and bolts, theoretically invisible at the functional (psychological) level. At that level, it was thought, linkage is all.

Or almost all: neural excitability *in general* might be affected—in changes of mood, for instance—by hormones diffusing throughout the brain. (As Norbert Wiener had put it, hormonal influences were messages "to whom it may concern"—Dupuy 2000: 116; Wiener 1948: 129–30.) Indeed, this fact led to complaints that AI can't model moods or emotions, because it can't model diffuse hormonal influences (Haugeland 1978: see Chapter 7.i.d). But *specific* neuronal functions—feature-detectors, for instance—were thought to depend on connections, not chemicals.

The first nail in this assumptive coffin was driven by the discovery of neuromodulators in the 1970s–1980s. These substances can temporarily alter a neurone's properties: in effect, they substitute one broadly Hebbian rule for another. They inspired various computer models (Fellous and Linster 1998). But, like neurotransmitters, they were thought to function only *locally*. All the then known neuromodulators were large organic molecules. Because they can diffuse only slowly inside the cell, and can't pass through cell membranes, they're stored and released near the synapse and can act only at the synapse itself. And "synapse", after all, is just Sherrington's word for "connection".

Around 1990, however, it was found that at least one neuromodulator is a very small inorganic molecule: nitric oxide, or NO (Garthwaite *et al.* 1988; Holscher 1997; O'Shea *et al.* 1998). This is generated, given the right enzymatic trigger, by (the whole cell body of) specialized neurones. But because it's so readily diffusible, even across cell membranes, NO can't be localized (or stored: it has to be synthesized 'on the spot', when needed).

It diffuses in all directions, and its effects—which depend on the concentration at the point concerned—endure until it decays. (The rate of decay can be varied by enzymes.) Consequently, NO works on all the cells within a given volume of cortex, *whether they're synaptically connected or not*. The resulting functional dynamics of the system (network) are highly complex, and very different from those of 'pure' connectionist systems, for volume signalling replaces point-to-point signalling.

This finding (later repeated for carbon monoxide and hydrogen sulphide) inspired researchers at Sussex's interdisciplinary Centre for Computational Neuroscience and Robotics to design artificial networks of a radically new type, where linkage is *not* all. For instance, the size of the diffusion volume matters, and so does the shape of the source (simulated as a hollow sphere, not a point source). Some of their models were kept as close to the biological data as possible, thanks to Michael O'Shea, a leading researcher on NO in invertebrate nervous systems (Philippides *et al.* 1998, 2005*a*). Others were more abstract, defining a general class of connectionist system called a GasNet.

In GasNets, some nodes scattered across the network can release diffusible 'gases', which modulate the intrinsic properties of other nodes and connections in various ways, depending on concentration. So one and the same node behaves differently at different times. Given certain gaseous conditions, a particular node will affect another despite there being no direct synaptic link. In other words, it's the *interaction* between the gas and the electrical connectivities in the system which is crucial. And, since the gas is emitted only on certain occasions, and diffuses and decays at varying rates, this interaction is dynamically complex.

General properties of neuromodulation could be explored using these systems. Indeed, the 'Gas' could be switched off, leaving the 'Net' unmodulated. When that was done, the resulting ("NoGas") dynamics were less subtle, the sensory morphology less efficient, and evolution (using the techniques described in Chapter 15.vi.c) much slower.

A specific behaviour might involve two *unconnected* sub-nets, which 'worked together' because of the modulatory effects. For instance, a variety of oscillator circuits, with various spiking frequencies and behavioural functions, were generated when GasNets were evolved to function as controllers for autonomous robots (Husbands 1998; Husbands *et al.* 1998; T. Smith *et al.* 2002; for the GasNet group's most recent paper, see Philippides *et al.* 2005*b*).

Feature-detectors (implemented as partially *unconnected* 'networks') arose which could recognize a triangle and use it as a navigation aid. As we'll see in Chapter 15.vi.c, the Sussex A-Life group had already evolved a wholly connective network to do this. Now, they were using neuromodulation to evolve such mechanisms faster, and to make them function more efficiently. *Post hoc* dynamic analysis showed that the modulated feature-detector involved a motor-node with a 2-cycle oscillation, which stimulated gas emission from two other nodes (T. Smith *et al.* 2002, sect. 7). Without the gas, the connectivities in that particular network couldn't do the job.

Space and time, as well as connectivities, affected the behaviour of GasNets. In that sense, even the highly abstract versions were more 'realistic' than conventional connectionist systems—for the brain, too, isn't just circuitry. But they were also highly unrealistic. In current GasNets, the modelled diffusion is instantaneous, not gradual (still less, carefully timed). And there's no interaction between different 'gases', as considered by Turing in his work on embryology (Chapter 15.iv.a). In principle, however, the physics and pharmacology could be modelled much more faithfully (cf. Wood and Garthwaite 1994).

By the time you read this, there will surely be many more simulations of neuromodulation. For, having long avoided neurochemistry, 'psychological' modellers had finally woken up to it.

Arbib was an example. After years of considering only connections (see Section vii, above), he confessed in 1993:

> [Until] recently, there's been a burgeoning world of neurochemistry which I've tried to stay away from, quite distinct from the world of relatively large-scale neural networks. Now, suddenly, we're seeing that we need to tailor the learning rules. Now, if you tailor the learning rules, you have to get into the biochemistry and understand how the playing off of the different mechanisms can change the time parameters . . . I now for the first time really have an integrated view in my work which shows the need to unify the neurochemistry with the systems modelling. (J. A. Anderson and Rosenfeld 1998: 235)

Similarly, David Rumelhart abandoned the 'pure' PDP that he'd helped to popularize in the 1980s (see Chapter 12.vi). Interviewed in the same year as Arbib, he said:

> [One] of the things I've been working on lately is trying to factor neurochemistry into my models, neuromodulators and things like that, which I take to be much more important than we've realized. I have a paper on the role of neuromodulators. It's a generalization of our conventional [PDP] networks, but which have neuromodulators included. (J. A. Anderson and Rosenfeld 1998: 289)

In short, there'd been a sea change in computational neuroscience. Connections had been married to chemicals, understood not as invisible nuts and bolts but as dynamic functions within neural networks.

At the turn of the millennium, an even more exciting change was in sight, as these ideas moved from pure simulation into the 'wet' world. For instance, in January 2005 a UK team started trying to evolve non-linear computers combining both wetware (diffusing chemicals and/or cultured neurones) and silicon (initially, up to sixty-four miniature electrodes arranged in grids/circuits). If successful, these systems would be capable of reliably achieving a user-defined computation.

This highly interdisciplinary project drew on several sources. One was the Sussex work on the evolution of GasNets, another Sussex's experience in culturing neurones *in vitro*. A third source was research by the physical chemist Annette Taylor, of the University of Leeds, on the dynamical properties of diffusing chemical waves (Chapter 15.iv). And fourth and fifth, previous work by AI scientists and toxicologists at the University of the West of England in Bristol—focused respectively on non-linear computing and the biochemistry of cell culture.

The general aim was to use GA techniques to evolve networks of chemicals and/or neurones which would compute a particular logical function. For instance, when electrodes X and Y were both stimulated artificially, the resulting activity in the final-generation 'wet' network should reliably result in the firing of some other electrode, Z—thus implementing an and-gate.

During the GA evolutionary process, several factors are randomly varied. They include the nature and concentration of various chemicals (e.g. NO, dopamine, and serotonin); the location, at different nodes in the neural network, of the release of neuromodulators; and the amount/timing of growth factor provided while the cells were being cultured. (Both vertebrate and invertebrate neurones are being studied.)

It's too early (as of June 2005) to report any firm results. But you can check up on what's happened by accessing the team's web site (<http://www.informatics.sussex.ac.uk/ccnr/research.html>). Meanwhile, the hope is that this research will throw light on non-linear computation in both engineered and organic machines, as well as teaching us more about neurones, neuromodulators, reinforcement learning, brain development, and medical prosthetics. ("Watch this space!", as they say.)

g. Time blindness—and glimmers of light

Rumelhart continued the conversation just quoted by mentioning the importance of *time*:

> If you are interested in dynamics, it turns out that there's a whole host of timescales involved. Different neuromodulators have different temporal properties; the neurons have different temporal properties, and learning has still others. In the nervous system, these things are all overlapping timescales, not independent ones like in physics. (J. A. Anderson and Rosenfeld 1998: 289)

The GasNet modellers, too, stressed the importance of different timescales in their systems. Indeed, they were among those 'A-Lifers' who had claimed for some years that conventional connectionism was hugely unrealistic—perhaps even biologically irrelevant—because it ignored temporal constraints on neural coding (I. Harvey 1992*b*). (As we've seen, this time blindness was found only in *conventional* connectionism; it didn't apply, for instance, to Grossberg's work.)

Timing matters not merely at the level of neural spikes, but at grosser levels too. For instance, if someone tries to learn two different movements in quick succession, they interfere; if separated by a few hours, they don't (Wolpert and Ghahramani 2000). From about 1970 on, experiments on conditioning showed repeatedly that timing (the onset and offset of the conditioned and unconditioned stimulus) was crucial.

Accordingly, some neuropsychologists even claimed that *associationism itself* was unrealistic, largely because of its 'timelessness'. Charles (Randy) Gallistel, a psychophysicist at UCLA, went so far as to call the associative bond "the phlogiston of psychology" (1997: 85). He not only claimed that *some* learning (e.g. gauging one's position by dead-reckoning, which many insects can do, or learning the centre of rotation of the night sky) isn't associative. He even insisted that "the process of association formation—as traditionally understood—is not involved in *any* form of learning that has been experimentally investigated, including classical and instrumental conditioning" (1997: 81; italics added).

Gallistel linked this claim with a commitment to Chomskyan modules (see Chapters 7.vi.d–e and 16.c–d). In his words: "A bird's gotta learn what a bird's gotta learn, and ditto [as regards language] for humans" (1997: 88). So, bearing in mind the findings of CNE (Chapter 15.vii), he said:

> [There] is no unitary learning process at the computational level of analysis. There are many different learning mechanisms or modules . . . different problem-specific learning mechanisms [which we may call] "instincts to learn". Each such mechanism has a structure that enables it to compute certain facts about the world. This specialization of computational structure renders the mechanism hopelessly ill-suited for computing other facts about the world. For that, one needs other mechanisms, with a different computational structure. (1997: 82)

So, much as GOFAI had moved from general-purpose mechanisms to expert knowledge (Chapter 10.iv), neuroscience should do so too:

> Despite long-standing and deeply entrenched views to the contrary, the brain can no longer be viewed as an amorphous plastic tissue that acquires its distinctive competencies from the environment acting on general-purpose cellular-level learning mechanisms. Cognitive neuroscientists, as they trace out the functional circuitry of the brain, should be prepared to identify adaptive specializations as the most likely functional units they will find. *At the circuit level*, special-purpose circuitry is to be expected everywhere in the brain, just as it is now routinely expected in the analysis of sensory and motor function. (Gallistel 1995: 1266; italics added)

He did allow that generality at the *cellular* level is "possible". All specialist learning mechanisms may employ the same set of elementary computational operations. But they may not: "At a time when we cannot specify the instruction set underlying any computation of any substantial complexity, we are in no position to say whether different complex computations use the same elementary computational operations" (1997: 83).

Even classical conditioning, which links *arbitrary* stimulus–response pairs, can't be explained by associationism. For Hebbian rules can't handle the "fascinating" results found in the last quarter-century, the "golden age" of experimental conditioning:

> [It appears that *all*] learning involves computing and storing the values of variables. In the case of conditioning, the crucial remembered variables are *the temporal intervals between events* [CS and US onsets and offsets]. But this conception of conditioning is as different from the traditional associative conception as the oxygen theory is different from the phlogiston theory. If this conception prevails, then the associative bond will indeed prove to have been the phlogiston of psychology. (Gallistel 1997: 85; italics added)

In a word, associative theories should be replaced by cognitive theories (Gallistel and Gibbon 2002). These assume that "the crucial remembered variables" are represented in the animal's brain, and that representations and decision rules together determine whether, and when, a response will occur.

No golden age had been needed to persuade neuroscientists that timing was often crucial. They'd known this since before mid-century (and one of Barlow's five dogmas concerned "high impulse frequency"). However, McCulloch in the early 1940s had deliberately ignored temporal properties he knew to be important: his "temporal propositional expressions" concerned temporal sequence, not time as such. His example was followed by virtually all connectionist modellers thereafter. (Early exceptions included Joseph Licklider 1951, 1959, and Braitenberg 1961.)

A few people had remarked that time could be incorporated in principle. Marr, for example, said in the 1960s:

> There is no reason why a context dependent system should not be run at different speeds, and if the extra postulate were made of some general intensity control acting uniformly over the effector circuits, a movement learnt at one speed could be performed at another. (D. C. Marr 1969: 466)

Moreover, Marr had inferred specific hypotheses about time intervals from his formal model of the cerebellum (see Section v.c). But most computer modellers ignored time entirely. Even 'temporal' connectionism, aka recurrent networks, considered temporal order rather than measurable time.

This wasn't inevitable: there's no principled reason why time can't be taken seriously in computational work. Although von Neumann computers operate on a 'clock' of uniform time-steps, it's not difficult to simulate real time in most practical circumstances. The block that prevented this was more psychological than computational.

In the early 1990s, Inman Harvey (1992*b*) urged network modellers to stop ignoring the real-value time lags on signals passing from one node to another. And in the late 1990s, he pointed out that models of *asynchronous* networks behave very differently from their synchronous cousins (Harvey and Bossomaier 1997). Some connectionists combined recurrent networks with simple models of synchronous neural oscillations (e.g. Lane and Henderson 1998). Others took time lags into account by experimenting with 'pulsed' networks. Here, the significance of a signal is coded by its timing; for instance, two pulses separated by a particular time interval may excite a certain unit, whereas pulses separated by any other time interval may inhibit it (Maass and Bishop 1999). But these weren't intended as serious models of real brains.

There were some exceptions (all in the last quarter-century), mostly coming from the neuroscientific stable. These included Grossberg, von der Malsburg, Arbib, Freeman, Changeux, and Edelman.

For instance, Grossberg modelled real-time perceptual processes, and used codes based on the frequency of unit firing. Arbib did so too, and simulated a number of interacting rhythms and arhythmias defined at the single-cell, correlated-cell, network, and (various) brain-regional levels (Arbib *et al.* 1997, ch. 6). His model hippocampus was one of the most time-informed, as well as one of the most complex, 'brainlike' systems of the period (see Section vii.c). By the new century, enough general interest had been raised to justify a special (double) number of *Adaptive Behavior* devoted to timing issues (Di Paolo 2002*a*).

There are technical obstacles if one wants to *emulate* time. Whereas one can if necessary take months to *simulate* a millisecond, emulation—where the computer generates real-time events at real moments in time—requires "a millisecond" to mean a millisecond. Some computer operating systems don't allow the real-time precision that would be needed to emulate highly time-sensitive processes. But others can already implement robot controllers driven by spike-timing measured to a few tens of milliseconds (Di Paolo 2002*b*: 143). So even emulation isn't always impossible.

As a critique of past work, then, this arrow meets its mark. Most modellers have ignored time, even including the biologists among them. This wasn't because they knew no better. Connections are clearly important, and people were focusing on those: one can't do everything at once.

By the turn of the century, however, it was clear to everyone—not just to the neuroscientists—that if one wants to model the brain, this couldn't continue. As Sejnowski had put it in 1993, "the real problem in the next five, ten years is coming to grips with real time signals [and imperfections too]" (J. A. Anderson and Rosenberg 1998: 331). Seven years later, one of the striking features of the models described by O'Reilly and Munakata (2000) was that many displayed close attention to time. As one would expect, given the authors' commitment to biological realism, the temporal features involved were based on the spiking times of real neurones (see ii.d, above). It was time, at last, for time.

14.x. Cartesian Correlations

This section outlines some theories of consciousness developed within cognitive science. Only *some*: as we'll see, there was an explosion of work on consciousness in the last two decades of the century. This happened quite suddenly. As late as 1979, the person who first gave cognitive psychology its name (Chapter 6.v.b) declared that psychology wasn't "ready" for consciousness (Neisser 1979). And Ernest Hilgard, who'd pioneered work on attention and hypnosis (7.i.h), said in the mid-1980s that most experimental psychologists were still "hostile" to the topic (Hilgard 1977/1986: 294).

At this point, the philosophical health warning given in Chapter 2.iii.a bears repeating. References to conscious experiences, or sensations, trip easily off the tongue—even the tongue of the man on the Clapham omnibus. "And why not? What could be more familiar? We've all had more experiences than we've had hot dinners."

Well, perhaps . . . In Section xi, we'll ask whether this common-sense notion is for scientific purposes better avoided. Why? Not because questions about the brain's role in consciousness can't be definitively answered (subjectivity, and all that). But because, given the usual interpretation of *experience*, certain 'obvious' questions about mind and brain *can't even be sensibly asked*.

Meanwhile, I'll continue to use our everyday vocabulary as though it were unproblematic. This is appropriate, for nearly all neuroscientists (like the passengers on the omnibus) do so too.

a. Consciousness comes in from the cold

Ever since the seventeenth century (specifically, ever since Descartes), philosophers discussed conscious experience and its relation to events in the brain: causal? parallel? epiphenomenal? identical . . .? And in the years surrounding the rise of cognitive science, they devoted countless pages to this topic (see Chapter 16.i–vi). But neuroscientists didn't.

Admittedly, neuroscientists asked just what types of consciousness occur when the brain is affected in various ways. In the late nineteenth and early twentieth centuries, they had only clinical evidence to go on. Later (starting with the invention of the EEG: Chapter 4.vi–vii), they could do systematic experiments of various kinds. With the rise of computational ideas, they looked at consciousness in terms of the information processing going on—and the ways in which this might break down. And when brain scanning became possible, various abnormalities of consciousness—including those seen in schizophrenia, multiple personality, and hypnosis—were studied by them accordingly (see references in subsection c, below).

Moreover, they realized that there's no such thing as *the* problem of consciousness. On the contrary—and as the philosophers were pointing out at length (16.iv.a–b and ix.c)—there are many such problems. For the words 'conscious' and 'consciousness' are used to refer to various phenomena, ranging from minimal alertness, through selective attention (6.i.a–c), to self-awareness via higher-order thoughts, or HOTs (7.i.g–i) and sensory experience.

So neuroscientists in the first three-quarters of the century experimented extensively on sleep and waking, various levels of anaesthesia, attention and orientation, conscious memory-recall, and perception—normal, illusory, and subliminal. They studied the brain's role in pain, anxiety, hallucination, and depression. And they described clinical syndromes involving various types of breakdown in conscious unity, such as 'multiple personality' (Chapters 5.ii.a and 7.i.h) and hemi-neglect—in which brain-damaged patients ignore one half of their body, even attributing it to someone else.

The Montreal neurosurgeon Wilder Penfield (1891–1976), an ex-student of Sherrington, studied brain–consciousness correlations in a relatively direct way. In around 1950 he discovered, unexpectedly, that he could elicit memory-like experiences by stimulating his patients' brains during surgery for temporal lobe epilepsy. (Only memory-*like*: the normal associative penumbra, they said, was missing.) One person, for instance, reported: "I heard my mother talking on the phone, telling my aunt to come over that night," and another remembered seeing her nephew and niece in her dining-room: "they were getting ready to go home, putting on their coats and hats . . . [and] my mother was talking to them" (Penfield and Perot 1963; see also Penfield 1952; Penfield and Rasmussen 1957; Penfield 1958). But he couldn't do this systematically: the stimulations served surgery, not curiosity.

With the advent of single-cell recording, researchers occasionally came up with results that tempted interpretation in terms of conscious experience. As early as the mid-1950s, for example, it was found that cells in the cat's auditory system (the cochlear nucleus) that are responding to 'meaningless' clicks will immediately stop responding if the cat is shown a mouse, allowed to smell a fish, or shocked on its paw (Galambos *et al.* 1956). The explicit inference was that the cat's *attention* had been captured by the new stimulus. But the tacit implication was that the cat stopped *consciously hearing* the clicks when it had more important fish to fry—or anyway, to *smell*. (That's why this experiment was sometimes cited by New Look psychologists as a support for "perceptual defense": e.g. Bruner and Klein 1960—see Chapter 6.ii.a.)

However, this experiment had been designed to study conditioning, not consciousness. It was found that the auditory cells would eventually stop responding (habituate) if the clicks continued—*unless* the clicks were 'meaningful', having been used as conditioned stimuli warning the animal of an impending electric shock.

The emphasis, in the click experiment, on matters other than consciousness was typical of its time. Conditioning, and even attention, could be studied, for both could be defined in objective terms. What neuroscientists didn't do, until relatively recently, was to consider the origin of conscious experience, or sensations, as such: what philosophers called 'raw feels' or 'sense data' around mid-century, and '*qualia*' today. (Remember that health warning!) They might ask *when* conscious experience occurs, relative to events happening in the brain. After mid-century, they started to ask *what computations* were involved in its construction. But they fought shy of asking *how it's generated by the stuff inside our skulls*.

Occasionally, an acclaimed neuroscientific guru—such as Sherrington (1940) or Eccles (1953, ch. 8)—would dare to speculate on this issue. Their discussion would be self-confessedly bemused and/or embarrassingly naive from the philosophical point of view (see Chapter 16.i.a). But their colossal reputations couldn't be harmed by sniggers behind the reader's hand. Neuroscientists in a more vulnerable professional position—and psychologists, too—tended to avoid the topic entirely.

As late as 1981, John Haugeland remarked that "the term itself is almost a dirty word in the technical literature" of cognitive science (Haugeland 1981*a*: 32). Even the maverick McCulloch, when he'd declared contra Sherrington that "Mind no longer goes 'more ghostly than a ghost'" (see Chapter 4.iii.e), hadn't grasped this particular nettle firmly.

A few courageous souls tried to fling the nettle onto the bonfire. So Barlow, in his 'five dogmas' paper, declared robustly that

> Thinking is brought about by neurons, and we should not use phrases like "unit activity reflects, reveals, or monitors thought processes", because the activities of neurons, quite simply, *are* thought processes. (1972, sect. 5)

But the nettle was slippery—maybe even fireproof. A few pages later, Barlow referred to "the activity of nerve cells accompanying [the personal, subjective, aspect of my] experience", which last is "something that one must be content to leave on one side for the moment" (1972, sect. 10.1). And in the next paragraph, having granted that not all aspects of visual perception are conscious, he insisted—in defence of his fourth "dogma"—that "an element of [conscious] perception can possess a simple neural cause without it necessarily being the case that all simple neural events cause perception". He didn't stop to ask how, if X is *identical* with Y, it can also be said to *accompany* Y, or to *cause* (or *bring about*) Y. We'll pick up this fire-resistant nettle in Chapter 16.i.d.

Much as Barlow was happy to talk about "cause" in this context, so neuroscientists in general were happy to talk about "correlations". However, there were a few intriguing experiments suggesting that mind–brain correlations are much less straightforward than Descartes—and our own contemporaries—imagined.

Most of these were done by Benjamin Libet, who started work on the neuronal basis of specific experiences in humans in the early 1960s (Libet *et al.* 1964, 1967; Libet 1965). For instance, he placed electrodes on the surface of people's sensory cortex during brain surgery, and asked them what—if anything—they felt when he delivered a stimulus; and he measured the brain waves elicited when they experienced (or thought of) something. He discovered that as much as half a second of neuronal activity is required before an experience can occur, although perceptual information is registered subliminally in half that time. Apparently, then, *qualia* are *constructed* by the brain.

This was already known by experimental psychologists. For instance, Paul Kolers—at Harvard's Center for Cognitive Studies—had been working since the late 1950s on "metacontrast", in which presentation of one visual stimulus very soon after another can block or even transform conscious awareness of the first (Kolers and Rosner 1960; Kolers 1972; Kolers and Grnau 1976; Kahneman 1968). But psychologists' experiments on visual masking had nothing to say about the neural mechanisms involved (see subsection b, below).

Libet's results (and Kolers's) didn't arouse widespread interest at the time—still less, heated discussion of their implications for the philosophy of mind (but see P. S. Churchland 1981 and Libet 1981). By the end of the 1980s, however, that had changed.

Suddenly, the neural basis of experience was a hot topic for scientists, as well as for philosophers. Indeed, the two communities started interacting much more than they'd done before, and contributed jointly to many collections of papers on the various aspects of consciousness (e.g. Pope and Singer 1978; Marcel and Bisiach 1988; B. Milner and Rugg 1992; Bock and Marsh 1993; Posner and Raichle 1994).

Discussion was further boosted, for the general reader as well as the professionals, by the publication in 1991 of two best-selling books: Tor Nørretranders's *The User Illusion: Cutting Consciousness Down to Size* and—especially—Dennett's provocatively titled *Consciousness Explained*. As we'll see in Section xi.b, this solved, or rather dissolved, the problem of mind–body correlations by giving a computational/behavioural analysis of consciousness, *including qualia*. But, like Nørretranders's book too, it also leant heavily on neuroscience—including recent work by Libet and by Marcel Kinsbourne (a co-author with Dennett in 1992) that cast further doubt on the *timing* of consciousness.

A biennial interdisciplinary conference entitled 'Toward a Science of Consciousness' was instituted by the newly formed Center for Consciousness Studies in Tucson (Arizona) in 1994. It attracted huge attention, and soon prompted the formation of the Association for the Scientific Study of Consciousness. Journals such as *Cognition and Consciousness* and the *Journal of Consciousness Studies* (tellingly subtitled *Controversies in Science and the Humanities*), and various 'consciousness' email discussion groups, were founded—and flourished. Quantity didn't guarantee quality: many of the papers were weak, to put it politely. By century's end, however, there was a wide variety of more 'respectable' work, both philosophical and neuroscientific (for a taster, see Dehaene 2001).

In the new century, robotics and AI got into the act in a big way (Haikonen 2003; Torrance 2003; Chrisley *et al.* 2005*a*,*b*). Of course, people had been asking philosophical questions about robot consciousness for years—not least, as a result of Turing's 1950 paper. But now things seemingly became more serious.

Between 2001 and 2005, there was a flurry of international meetings on "machine consciousness": in Cold Spring Harbor, Skövde (Sweden), Memphis, Birmingham, Turin, Antwerp, and London. An increasing number of papers on this topic graced the pages of the *Journal of Consciousness Studies*, which then decided to devote a double-number special issue to it (O. Holland 2003). Conferences named for other topics, such as 'Adaptation in Artificial and Biological Systems', ran symposia on machine consciousness. And a dedicated web site was established (<http://www.machineconsciousness.org>).

Some of this interest was purely philosophical. But much of it was linked to practically oriented work on "robot companions" (see Chapter 13.vi.d) and/or the evolution of linguistic communication in robots (Steels 2003). AI/A-Life researchers were now receiving research grants to develop "conscious robots" (Owen Holland, personal communication). And a commercial company, Nokia, was providing funds for this goal too.

Or at least, Nokia were happy to have the project described in that way. Just what they—or anyone else—really understood by the term "machine consciousness" was obscure, and philosophically up for grabs. Often, people tried to avoid the philosophical problems by defining "machine consciousness" in a non-committal way. For instance, the Call for Papers for AISB's symposium at Bristol in 2006 defined "MC" as "the study and creation of artefacts which have mental characteristics *typically associated with* consciousness such as (self-)awareness, emotion, affect, phenomenal states, imagination, etc." (italics added).

Some researchers, such as Igor Aleksander, were even describing their laptops as conscious. However, when challenged he'd retreat into objectivity, saying that he was studying the mechanisms underlying attention, choice, discrimination, and reflexive monitoring—i.e. a large part of what we mean by consciousness, but *not* "consciousness as it is in us" (personal communication). Aleksander's (2000) five criteria of consciousness were: having a sense of place; imagination; focusing of attention; planning future actions; and being guided by emotions. Given the discussion of Chapter 7.d–f, it's clear that the last criterion didn't necessarily include "experiencing feelings of emotion". In short, whether or not people were hoping/intending to attribute *qualia* to their future robots and/or laptops was ambiguous. (As for whether this would have been a nonsensical hope, see Section xi below, and Chapter 16.v.b.)

But that sudden surge of work on machine consciousness was a very late development. Meanwhile, from the late 1980s, the public interest in *human and animal* consciousness grew. By the early 1990s it had become so widespread that the Royal Society in London organized a brief press conference—*not* an academic meeting—on 'Consciousness—Its Place in Contemporary Science' in February 1995. Various recent findings and theories were described to the visiting journalists, with some pride: these were fascinating matters. But the Royal Society's scientists rang loud warning bells:

> [The] remarkable consensus of the speakers [was] that, despite all the recent books, TV programmes and other hype, science really did not understand *anything* about consciousness—what it is, how it evolved, how it is generated by the brain, or even what it is for! (K. Sutherland 1994: 285)

We'll return to this point in subsection c. First, let's look briefly at some of the neuroscientific findings that had contributed to this intellectual feeding frenzy.

b. Cognitive neuroscience

The key contributions had come from the discipline dubbed "cognitive neuroscience" by George Miller in the late 1970s (Gazzaniga 1988: 230). He'd realized the need for this after seeing the bewildering display of cognitive deficits and dissociations suffered by Gazzaniga's clinical patients at Cornell Medical College.

To be sure, he was already aware that there were relevant questions to be asked. For instance, he—and many other cognitive psychologists in the West—knew of the pioneering work on cerebral control and aphasias done by the Russian neuropsychologist Alexander Luria. The first English translation had appeared in the 1930s, and—after a long gap—by 1970 there were several more, including Luria's first best-selling trade book *The Mind of a Mnemonist* (Luria 1932, 1960, 1966*a*,*b*, 1968, 1970; Luria and Yudovich 1959). Miller had actually cited Luria's work on language development in *Plans and the Structure of Behavior* (p. 140 n.), and his MGP co-author Pribram had written a preface to *Higher Cortical Functions in Man* (Luria 1966*a*) a few years later. But he'd never actually seen for himself the phenomena that Luria was describing.

On visiting Gazzaniga's clinic, Miller was surprised, as well as bewildered. One reason was that his previous knowledge of clinical deficits was very general. Even Luria's professionally oriented books, like the papers in the clinical journals, were descriptive rather than analytic. And in those days, clinicians and theoreticians rarely mixed (i.a above).

Psychological studies of memory, for instance, typically ignored clinical amnesias. As late as 1980, the second edition of Roberta Klatzky's (1980) widely used textbook on *human* memory said nothing about the topic (although it did give brief discussions of "mnemonists" and of the effects of alcohol and drugs: pp. 307–10, 321–5). The experimental psychologist Brenda Milner (1959) was an exception: she'd described one such case (but in psychiatric/medical journals), and her research was followed up in the 1960s by a handful of her peers. In general, however, clinical work—as William Hirst recalls—was "treated lightly" by them:

Cognitive psychologists clearly felt more comfortable writing about laboratory experiments than about experiments with brain-damaged patients and did not feel compelled to account for neuropsychological results if they contradicted their theoretical position. (Hirst 1988*b*: 250)

Miller now tried to make amends. With Gazzaniga's help, he soon founded the Cognitive Neuroscience Institute (at Rockefeller University) to focus on the theoretical issues raised by clinical syndromes, and by neuroscience in general.

He made no apologies for asking psychologists to consider the brain—which Fodor and Putnam, and most early cognitive psychologists, had declared to be unnecessary (16.iii–iv). Nor did he worry about being called a reductionist. As he wrote in a letter to Gazzaniga in 1978, psychology needed neuroscience:

Since I have always thought of scientific psychology as a branch of biology, this objection [reductionism] carries little weight with me. It would carry greater weight, however, with such distinguished scientists as [the behaviourist] B. F. Skinner or [the functionalist] H. A. Simon. (Gazzaniga 1988: 239)

Gazzaniga was equally unapologetic, but in the opposite direction:

[The] idea that *neuroscience needed cognitive science* has prevailed. I put it that way because that, after all, is what is at stake. The molecular approach in the absence of the cognitive context limited the fashionable neuroscientist to pursuing answers to biologic questions in a manner not unlike that of the kidney physiologist. Although such approaches represent an admirable enterprise, they do, when put in that light, make it impossible for the neuroscientist to attack the central integrative questions of mind–brain research. (Gazzaniga 1988: 241; italics added)

Miller was still a cognitivist, of course. But, twenty years after his manifesto and mission station (6.iv.c–d), his enthusiasm for computers and AI had waned. Writing to Fritz Machlup in January 1983, he said:

I find it difficult to communicate clearly my enormous debt to people like Simon, Newell, and Minsky for helping cognitive psychology take a great leap forward, yet at the same time to communicate with equal clarity that the human brain/mind system is enormously more complicated than, and different from, any contemporary computational system. (G. A. Miller 1983*a*: 57)

Contemporary computers manipulate symbols, and symbol manipulation is certainly one kind of intelligence. But whether that kind of intelligence is the only kind remains an open question. (p. 57)

[As] the difficulties become more and more apparent, the tendency for the computer exploration to expunge psychologism (just as logic did a century earlier [see Chapter 2.ix.b]) and move off on its own seems stronger and more attractive. That might be a good development, but it would spell the doom of cognitive science as a new discipline encompassing both artificial intelligence and psychology. (p. 58)

The newly named discipline grew quickly. Initially based in clinical neurology (e.g. Shallice 1982, 1988*a*,*b*; Weiskrantz 1986, 1988; Vallar and Shallice 1990), it soon embraced non-patients too. At first, the focus was on vision, motor control, and language. By century's end, motivation and emotion—and their intimate links with cognition—were included also (Rolls 1990; Damasio 1994, 1999, 2001; LeDoux 1996, 2002; Gazzaniga 1995, pt. IX). (So "cognitive" neuroscience, like "cognitive" science itself, is *not* limited to cognition: 1.ii.a.)

The field soon spawned its own communication channels, the *Journal of Cognitive Neuroscience* and *Neuropsychologia*. Besides, hefty review volumes started appearing in the early 1990s (Marcel 1993; Bisiach 1993; Gazzaniga 1995, 1999, 2000; M. H. Johnson 1993*a*; Squire and Kosslyn 1998; O'Reilly and Munakata 2000; Parker *et al.* 2002). In the late 1990s, the interdisciplinary study of development in normal and brain-damaged children was flying (see Chapter 7.vi.i and Section ix.c above). By the turn of the century, then, cognitive neuroscience had made its mark.

According to its name-bestower, the central goal was to understand consciousness:

> I consider consciousness to be the constitutive problem for psychology, just as life is the constitutive problem for biology . . . Psychologists want to discover the molecular logic of the conscious state. [That is to say]: the set of principles that, in addition to the principles of physics, chemistry, and biology, operate to govern the behavior of inanimate matter in conscious systems. (G. A. Miller 1978: 233)

Whether this "set of principles" includes anything over and above abstract (functional, computational) principles of brain and behaviour will be discussed in Section xi. But it's worth noting that Miller's examples here did *not* include experience (*qualia*), and that—in the same letter—he swiftly substituted first "knowledge" and then "epistemic system" for the troublesome "consciousness" (1978: 235–6).

Cognitive neuroscience didn't start with Miller, for the name was newer than the activity. Arbib's schema theory (see Section vii.c, above), Eric Lenneberg's (1967) survey of neural-anatomical specializations for language, and Libet's 1960s research had all been early exercises in the genre. Indeed, so had the late 1960s neurophysiological studies of visual masking in cats (P. H. Schiller 1968). In this and later single-cell work on visual masking (Felsten and Wasserman 1980; Keysers and Perrett 2002), the assumption was that the underlying mechanisms in cats, monkeys, and people might be broadly similar—and that the consciousness was similar too.

Miller, citing Minsky and Marr, stressed the need to pass from purely correlational (brain–behaviour) studies to theories of cerebral information processing and representation (Gazzaniga 1988: 240). Accordingly, many brain researchers now adopted the computational approach he'd pioneered twenty years earlier (Chapter 6.iv.c), analysing behaviour and thinking into distinct functional components.

Clinicians had long noticed that brain damage often leads to some very specific loss: trouble with producing (though not with understanding) abstract nouns, for instance, or 'blindness' to and/or neglect of one side of one's own body. Charles Darwin had remarked the puzzling case of a man who, after suffering a stroke, could tell the time by looking at a clock-face but not by reading the words. Over 100 years later, in the

1980s, some curious cases of mental dissociation—and of abnormal memory—were familiar even to the general reader, thanks to fascinating trade books written by clinical neurologists or psychologists (Luria 1968, 1972; Sacks 1985, 1989).

As for the 1980s professionals, they no longer spoke merely of aphasia, amnesia, ataxia, or apraxia (i.e. disturbances of speech, memory, movement, or action). Instead, they distinguished many different *kinds* of disability previously lumped together under these terms. For instance, they looked out for the various types of action error distinguished by Timothy Shallice and Donald Norman (Chapter 12.ix.b), or for the wide variety of memory disorders (e.g. Cermak 1982). And besides trying to match these with particular brain lesions, they tried to explain and systematize them in terms of (formal or informal) theories of the information processing going on.

Some people took these functional dissociations as proof of the existence of 'modules', inborn mechanisms specialized for the relevant types of information processing. They were wrong: modules-in-the-adult don't necessarily imply modules-in-the-developing-brain (Section ix.c–d above, and Chapter 7.vi.i). However, this wasn't widely recognized at the time—except by *developmental* neuroscientists.

By the 1980s, too, Libet's research programme had delivered some even more intriguing results. In 1979 he reported (1) that the subjective time-of-occurrence of a sensory experience was shifted backwards, so that someone might experience something *as* happening at time-of-stimulation, even though the experience itself occurred half a second later (Libet *et al.* 1979). Yet more startling, his group did work on voluntary action that showed (2) not only that 'spontaneous', 'instantaneous', conscious decisions (like sensory *qualia*) take time to be constructed, but that the relevant motor areas of the brain are *already* being activated before the decision is experienced (Libet *et al.* 1983).

The first finding cast yet more doubt on the common-sense notion of temporal 'correlations'. Prompted by a critique from the British philosopher Ted Honderich (1984), Libet (1985*a*) wrote a paper for the *Journal of Theoretical Biology* specifically relating it to "mind–brain theories" (for Honderich's response, see his 1986, 2005). Eccles—with Karl Popper—argued that the result was inexplicable by any neurophysiological process, so provided further evidence for his long-held dualism (K. R. Popper and Eccles 1977: 364).

The second finding threatened popular notions of free will. Libet was immediately invited to write a paper for *Brain and Behavior Sciences*. This attracted huge attention—and an unusually large number of peer reviews accompanying it in the journal (Libet 1985*b*). By the end of the century, the *Journal of Consciousness Studies* had published a special issue on "the neuroscience of free will" (Libet 1999). Because of the general interest in the topic, this was made available also as a stand-alone book.

Most late twentieth-century neuroscientists still concerned themselves with only one of the many problems of consciousness indicated above. But a few developed comprehensive theories interrelating them all (e.g. Baars 1988; Cotterill 1998; Velmans 1996, 2000; Weiskrantz 1997; G. M. Edelman 1989; Edelman and Tononi 2000; Damasio 1999).

These writers usually took pains to distance "the brain" from "the computer". Nevertheless, most of them used computational ideas—even if they protested (like Edelman) that they weren't doing so.

They all agreed that a huge amount of cognitive processing occurs without consciousness. This had long been known, of course. But new technologies, including brain scanning, now provided further evidence. For instance, manual responses resulting from highly complex operations (integrating perceptual, semantic, and motor processes, plus the interpretation of verbal instructions) show up in motor cortex before the person is aware of them (Dehaene *et al.* 1998*b*). Similarly, they all agreed that attention is necessary for consciousness (although 'blindsight' showed that it's not sufficient: see subsection c), and that certain—ill-specified—types of task apparently can't be achieved unconsciously.

Their theories relating the different levels or aspects of consciousness, and explaining the 'need' for consciousness in certain cases, varied. However, certain key ideas cropped up repeatedly. One of these was the concept of a general workspace, explored at length by Bernard Baars (1988, esp. ch. 2; 2001) and adopted by others too (e.g. Dennett 1991, ch. 9; Dehaene and Naccache 2001).

The core notion here was that 'higher' (including deliberative) consciousness requires a *global* mechanism—broadly comparable to a GOFAI blackboard: Chapter 10.v.e—making information from many different areas simultaneously available. Fodor (1983) had implied as much in respect of the "higher mental processes", but had despaired of explaining such non-modular effects (see Chapters 7.iii.d and 16.iv.d). However, he was interested in why *this* person arrives at *this* belief on *this* occasion. The neuroscientists, by contrast, wanted to know how conscious thought in general is possible. Stanislas Dehaene and Lionel Naccache put it like this:

> [Various recent theories suggest that] besides specialized processors, the architecture of the human brain also comprises *a distributed neural system or "workspace" with long-distance connectivity that can potentially interconnect multiple specialized brain areas in a coordinated, though variable manner* . . . Through the workspace, modular systems that do not directly exchange information in an automatic mode can nevertheless gain access to each other's content . . .
>
> If the workspace hypothesis is correct, it becomes an empirical issue to determine which modular systems make their contents globally available to others through the workspace. Computations performed by modules that are not interconnected through the workspace would never be able to participate in a conscious content, regardless of the amount of introspective effort. (Dehaene and Naccache 2001: 13)

Significantly, their term "architecture" was ambiguous: it was primarily computational, and only secondarily neural.

In short, computational ideas were guiding neuroscientific experiments on the many aspects of consciousness, and informing the interpretation of the results.

c. The $64,000 question

The $64,000 question concerned the brain's role in generating conscious experience. Among the topics most discussed by *fin de siècle* thinkers interested in this problem were experiments on alternating perceptions, perceptual binding, and blindsight.

Alternating perceptual experiences seemed to be correlated with specific processes in the brain. It was already known that if a pattern of vertical bars is continuously presented to one eye, and a pattern of horizontal bars to the other, what a human subject actually sees isn't a vertical/horizontal grid, but a regular alternation between the two (simultaneously presented) patterns. When Nikos Logothetis and Jeff Schall (1989) put a monkey in the same experimental situation, its discriminatory behaviour—raising its left or right paw—suggested that it, too, saw first the one pattern and then the other.

Moreover, a group of cells in the monkey's visual cortex was active only when its behaviour implied that it was *aware* of the vertical pattern. A different group was continuously active while the vertical pattern *was being presented as a stimulus* to the eye. The same applied in respect of the horizontal patterns. This implied that a distinct group of cortical neurones is active *only when a particular type of visual phenomenology is being experienced*.

'Binding' is an aspect of conscious unity—remarked on, for instance, by Descartes—whereby a multifaceted object is experienced as 'one' thing. How do auditory, tactile, and visual features get to be attributed to one cat, for instance? And how are the many different visual features integrated, given that each is computed in a different part of visual cortex (Zeki 1993)?

Crick (with Christof Koch) suggested that distinct perceptual features will be attributed to one and the same thing (one and the same Gestalt) if the relevant neurone groups are not only simultaneously activated but are also resonating at the same frequency—perhaps 40 Hertz (Crick and Koch 1990). Von der Malsburg had already suggested something similar as the basis of the cocktail-party effect: hearing one's own name stand out within a confusing buzz of conversation (von der Malsburg and Schneider 1986). But now there seemed to be experimental confirmation. A recent issue of *Nature* had reported that the neurones in different visual-feature-detector columns in cats oscillate synchronously when responding to the same object (Gray *et al.* 1989).

These results had led others to ask whether the researchers had "uncovered the cellular basis of consciousness" (Baringa 1990: 856)—a question which Crick and Koch answered with a firm "Yes!" (The firmness soon became a wobble, as they weren't able to replicate these findings in monkeys: Nørretranders 1991: 432 n. 46.)

As for blindsight, this was first discovered in monkeys (Weiskrantz 1972). Animals with certain lesions in the visual cortex lost much of their discriminatory capacity, but not all of it. Soon, the condition was seen in humans too (Poppel *et al.* 1973; Weiskrantz *et al.* 1974; Weiskrantz 1986, 1997; Moore *et al.* 1998). But here, there was a further surprise. Patients with similarly located brain lesions claimed to be blind—*to have no conscious experience*—with respect to a particular part of their visual field. Asked to point to an object, or to move their eyes in its direction, they'd protest that this was ridiculous: they couldn't see it, so how could they point to it? Nevertheless, if they could be persuaded to 'guess', they usually indicated the right place—sometimes, even positioning their fingers to match the object's shape.

Apparently, much of the sensori-motor processing that Arbib tried to describe (Section vii.c) was going on. But the sensory information wasn't getting through to the 'higher' brain centres so as to become introspectively accessible. (It became clear later that blindsight isn't normal vision near the threshold of consciousness, but involves non-normal processing: Azzopardi and Cowey 1997.)

In the 1990s, an extraordinary clinical case of blindsight was reported (Goodale and Milner 1992; A. D. Milner and Goodale 1993, 1995), which reached the general public early in the new century (Goodale and Milner 2003). The unfortunate woman concerned, who'd suffered carbon monoxide poisoning, appeared to be blind. She couldn't recognize familiar faces, nor identify simple geometrical shapes. Nevertheless, she could put her fingers into the correct attitude and position when reaching out to pick something up. The key demonstration of her visual agnosia involved a plastic card and a narrow rotating slit in a box. She couldn't match the position of the slit perceptually; that is, she couldn't hold the card in the same orientation as the slit. However, if asked to "post" the card through the slit (in many different orientations), she had no problems at all.

In short, she was apparently able to see (see-for-action) things unconsciously which she couldn't see (see-for-perception) consciously. (Her clinicians used this as just one piece of evidence in arguing that there are two anatomically and functionally distinct visual pathways in the brain: one for perception, one for action—see 10.iv.b.)

When the new millennium dawned, then, 'consciousness' was no longer a dirty word in neuroscience. On the contrary, it was the acme of trendiness. In 2000 the British neuroscientist Jeffrey Gray (1934–2004) was asked to review no fewer than three recent books on consciousness at the same time—with several others being reviewed elsewhere in the same publication.

But Gray knew the difference between trendiness and intellectual progress. Despite having been a guest editor for the *Journal of Consciousness Studies* a few years earlier (J. A. Gray 1995), he didn't welcome these three new volumes unreservedly:

> There is a great deal of fuss these days about consciousness. Yet, 10 years ago or so, the fussometer reading would have hovered around the zero mark, where it had been stuck for most of the past century. Just a few philosophers worried about a problem that was totally ignored by the scientists. (J. A. Gray 2000: 36)

Why the vaulting fussometer reading? In Gray's opinion, it wasn't due to any scientific or philosophical advance. (Given that he'd introduced the cautionary Royal Society press conference mentioned above, this judgement was only to be expected—cf. J. A. Gray 1995.) Rather, he said, it was due to the recent technologies of brain imaging.

If the technologies were recent, the idea wasn't. James (1890: i. 97) mentioned the Italian physiologist Angelo Mosso, who'd noticed regional pulsations of the cortex, in patients with bits of their skulls missing after operations, which seemed to depend on active thinking. He'd suggested, and James agreed, that this was caused by changes in blood circulation. But studying such changes in subjects with intact skulls wasn't possible before the 1980s.

From 1982, it became possible to measure activity in specific parts of the brain—as opposed to EEGs taken across the whole brain (Frith 1992; Frackowiak *et al.* 1997; Raichle 1998; Frith *et al.* 1999). PET scanning (positron emission tomography) and fMRI (functional magnetic resonance imaging) identified the areas—perhaps only a few millimetres across—with the highest level of glucose metabolism or, what comes to much the same thing, the greatest increase in blood flow. These methods depicted the brain *in action*, for they showed (in some cases, several times a second) how the brain's energy consumption varies while someone thinks about or perceives something, or performs a motor act. What action the energy was being consumed *for*, however, wasn't as clear as it was usually assumed to be (see below).

(For the record, it was taken for granted at that time that most of the brain's energy consumption is for information processing at the synapses. We now know that this takes only 40 per cent of the energy, while another 40 per cent is consumed simply in passing messages along the axons. The rest is used for housekeeping/maintenance: Attwell and Laughlin 2001; Laughlin 2004.)

Cognitive psychologists and clinicians—and the USA's science administrators who named the 1990s "The Decade of the Brain"—were enthused. Questions previously way beyond the reach of neuroscience now seemed to be answerable. For instance:

* Brain scans done when people were asked to think about beliefs and desires supported the early 1980s claim that autism and Asperger's syndrome involve failings in "theory of mind" (Chapter 7.vi.f).

* Studies of schizophrenics implied that their hallucinations of 'inner voices' are due to a failure or delay of communication between different parts of the brain (Cahill *et al.* 1996; Silbersweig *et al.* 1996).

* Schizophrenic, and experimentally induced, delusions about alien control of one's own body were explained in a similar way (Frith *et al.* 2000; S.-J. Blakemore *et al.* 2003; cf. Chapter 7.i.i).

* Hypnotic states and multiple personality were correlated with changes in brain activation (Aikens and Ray 2001; Reinders *et al.* 2003; cf. 7.i.h–i).

* 'Oceanic feelings' and intimations of the numinous, described long before by James (1902), were associated with a certain area in cerebral cortex (Alper 1996; Saver and Rabin 1997; Cook and Persinger 1997; cf. 7.i.i).

* Brain-imagers even managed to explain why one can't tickle oneself (S.-J. Blakemore *et al.* 1998).

A host of other examples had been described, all linking conscious experiences (and voluntary movements) of various kinds with cerebral activity. Similar effects were observed in non-human mammals—but using implanted electrodes rather than brain-scanners. In one widely reported study on rats, signals from motor cortex which normally cause the rat to press a lever attached to a water-dispensing robot were transmitted directly to the robot after the lever had been disconnected (Chapin *et al.* 1999). Because the robot arm responded more quickly than the paw did, the animal soon learned that it needn't actually press the lever any more. In short, the rats had learnt to move robot arms 'by thought alone'.

This caused huge excitement, because of the potential for helping paralysed people. A few months later, the same experimenters (at Duke University, North Carolina) sent brain signals over the Internet to enable an owl monkey to control a 3D robot arm 600 miles away, at MIT (Nicolelis 2001). They planned to include visual and tactile feedback, to see whether closed-loop 'circular causation' could integrate the monkey's real and virtual worlds. And very soon after that, another group used similar techniques to enable a rhesus monkey to learn to play a video game controlled by a joystick—without actually touching the joystick (Serruya *et al.* 2002). (These experiments raise interesting questions regarding 'cognitive technology' and the concept of self: see 16.vii.d.)

Linking rats or monkeys to robots is a major exercise, not undertaken lightly. But brain-scanning studies on humans are less tricky, and are multiplying merrily. Emails reporting some new example reach me several times a week. One that arrived when I was first drafting this chapter announced an fMRI study that used John R. Anderson's computational model of goal-directed problem solving (Chapter 7.iv.iii) to predict blood-oxygen levels "in brain areas involved in the manipulation and planning of goals" (Fincham *et al.* 2002). Since then, many others have followed it (although very few are related to specific computational models, as that one was). The journalists have learnt to keep an eye out for them: brain-scan studies of 'religious' experience (see 8.vi.b, list-item 2), for instance, were widely featured in the media. As for the professional journals, by the time you read this book they will have carried thousands of PET/fMRI reports.

Their theoretical significance, however, is debatable. Brain imaging has even been dubbed "a neo-phrenological fad", because of the difficulty of interpreting it in terms of psychological functions (Uttal 2001). There are four main problems.

One is that such studies are only correlational: *causal* hypotheses have to be inferred from them, and this is a risky business. For instance, a neurone may be using energy to *excite* another cell, or to *inhibit* it. How can we decide? Recently, Logothetis's team at Tübingen's Max Planck Institute for Biological Cybernetics have combined fMRI with single-neurone recording and/or local injections of neurotransmitters (Logothetis 2002). Such studies can tell us whether what's going on is excitatory or inhibitory. But they're technically very difficult, and can't be done on human beings. They're therefore irrelevant to the 'sexy' discoveries reported in the journals—and in the newspapers.

Another difficulty recalls Gregory's complaint about "howling centres" (see Section iii.a, above). If a part of the brain increases its activity when the person thinks about X, that doesn't mean it's a "centre"—still less, *the* "centre"—for thinking about X: many other things will be going on elsewhere. In short, Gregory's critique applies to PET and MRI no less than to classic ablation experiments.

Third, brain-imaging results (unlike the results of brain lesions) aren't stable. They're highly sensitive to context, such as the order in which stimuli are presented and perhaps what the person was thinking about beforehand.

And fourth, one can't sensibly suggest *just what's being done* by the high activity (even if one knew it was excitatory activity) without a theory at the cognitive/psychological level, specifying just what computations might be involved when the thought in question occurs. Usually, no such theory is available. (The research programme described in Chapter 7.vi.i is an exception: there, a significant effort is made to relate neurological details to a specific theory of cognitive processing.) In short, most brain imaging is an a-theoretical fishing expedition: more natural history than science. As Gazzaniga had put it, before this new 'industry' burgeoned, neuroscience *needs* cognitive science.

Responsible researchers know all this, of course, and are careful. But the irresponsible ones—and a fortiori the journalists—seemingly don't, and aren't.

Gray, however, was less worried by such theoretical difficulties than by the naive optimism that brain scanning had banished the mind–body problem. These new gadgets, he declared, had gone to the neuroscientists' heads:

> The power of this method has convinced scientists that now they can watch the brain in action, they can forget the hoary old issues raised by philosophers and get on with the *real* job by the simple expedient of describing what the brain does. (J. A. Gray 2000)

But the hoary old issues hadn't gone away. What he'd described some years earlier as "the most basic issue—the theoretical link between the occurrence of conscious experience and the neural substrate of the brain" (J. A. Gray 1993: 263) was still unresolved.

As Gray remarked, the philosophers had been way ahead of the scientists on this one. Or anyway, they'd spent incalculably more hours discussing it. Whether they'd got much nearer to solving it was another matter.

They'd long ago considered what it would mean for our understanding of consciousness if we knew in detail every correlation between events in the brain and conscious events. And they'd (mostly) concluded that this alone would tell us nothing fundamental about the nature of consciousness, or about *how it's possible* for the brain to generate it (see 16.i). In a word, or three: correlation isn't causation.

Moreover, functionalism—or so it was widely believed (Chapter 16.v.b)—hadn't helped. Most people felt that an experiential quality, such as redness or sourness, can't be the same as an abstract computational or causal function (important exceptions are discussed in Section xi, below). Even the arch-functionalist Fodor had washed his hands of consciousness:

> Nobody has the slightest idea how anything material could be conscious. Nobody even knows what it would be like to have the slightest idea how anything material could be conscious. So much for the philosophy of consciousness. (Fodor 1992)

Gray agreed. He concluded that modern neuroscience has no idea of how neuronal firing *can* cause conscious thoughts or percepts. Nor can it explain why these evolved at all: given the closure of physics, it's hard to see how conscious states could possibly affect behaviour. In short, this was Princess Elizabeth all over again (see 2.iii.b).

If Gray was so sceptical, why were other neuroscientists so hopeful? What experimental evidence, *over and above* the discovery of many previously unknown mind–brain correlations, had persuaded them that they were about to solve the problem?

The most persuasive examples came not from human brain imaging but from single-unit recording in animals. For instance, experiments on rhesus monkeys (Newsome and Salzman 1993) showed a remarkably systematic correlation between activity in certain orientation-selective cells in the animal's visual cortex and its perceptual discriminations of movement—*and*, common sense would have us say, its experiences of seeing movement.

Before the experiment proper, the monkey was trained to move its eyes in the direction of the perceived movement. Then, stimulating a certain cell circuit caused the monkey to behave as though it saw motion in a certain direction. Moreover, these results were predictable. In other words, the relevant cells are spatially organized in a way that corresponds to the geometry of the monkey's discriminatory behaviour. Assuming that the monkey has conscious states, this evidence suggested not only a causal relationship between individual cells and experiences, but also a systematic mapping from spatial structures in the brain to perceived phenomenal structure. It even enabled the neuroscientists to predict that if such-and-such a cell group were to be stimulated, then such-and-such an experience *must* ensue.

This hypothetico-deductive reasoning is typical of scientific explanation. So, surely, this constituted a scientific explanation of (certain aspects of) the monkey's visual experiences?

Well, yes and no. This experiment couldn't have assuaged Gray's worry. What it showed was that, *given* that certain neurone groups give rise to (cause, generate, are correlated with . . .) visual experiences, *these* neurones lead to *those* experiences, whereas others lead to others. This, of course, was a lot better than nothing. But it didn't answer Gray's question about how it's possible for *any* neuronal activity to lead to *any* conscious experience. It confirmed the previous belief (the common-sense hunch, inherited from Descartes) that there are systematic mind–brain correlations there to be found. But it didn't explain how this can be so.

Moreover, it was (and is) still impossible—or anyway, deeply problematic—to understand causal relations going in the opposite direction. To be sure, some cognitive scientists claimed to explain how conscious experiences can have neuromuscular effects (Velmans 2002). These accounts were intelligible if interpreted functionally in informational, computational, terms. But if some extra (correlated) realm of conscious mental events was posited, Princess Elizabeth's problem remained.

In brief, work on consciousness was riddled with "paradox and cross purposes" (N. Block 2001).

d. Philosophical contortions

Gray wasn't the only one to be worried by the mind–brain problem. Several neuroscientists, and philosophers sympathetic to neuroscience, suggested more or less radical 'solutions' to it.

Crick (1994), for instance, proffered the "astonishing" hypothesis that conscious states are processes in the brain—or, as he put it, that " 'You', your joys and your sorrows . . . , your sense of personal identity and free will, are in fact no more than the behaviour of a vast assembly of nerve cells and their associated molecules." He claimed, for example, that "freewill is located in or near the anterior cingulate sulcus" (p. 268).

However, whether we take "freewill" to connote a conscious act of choice or a control mechanism for scheduling potentially conflicting motives (see Chapter 7.i.g), it's a category mistake to describe it as being "located" anywhere. What was truly astonishing, at least to the philosophers, was that Crick thought this hypothesis was either new or scientifically straightforward. In fact, it was deeply problematic—like the 'identity theory' of the 1950s (see Chapter 16.i.d), or the more recent 'eliminative materialism' (16.iv.e).

Eliminative materialism had been presaged by Willard Quine in the 1950s (Quine 1953*b*; 1960: 264), but was developed further by Paul Churchland (1979). In the 1970s, he'd formulated his theory without any knowledge of neuroscience or connectionism (see Chapter 12.x.a). Ten years later, he was a colleague of Crick's in San Diego, a devotee of PDP modelling, and—even more to the point—an admirer of Pellionisz and Llinas.

He adapted their theory of coordinate transformations in the cerebellum to define his concept of the "state-space sandwich", which he speculatively applied to the discrimination of tastes—and, he suggested, also to smells, sounds, and colours

(P. M. Churchland 1986*a*,*b*). He admitted that the last three seemed to be much more complex than tastes, but pointed out that they'd each been described in terms broadly comparable to those of Pellionisz and Llinas. That is, they'd been theorized by appeal to coordinate transformations, "variously called 'multivariate analysis', 'multidimensional scaling', or 'across-fibre pattern coding', and so forth" (1986*b*: 303).

Churchland wasn't assuming a mere collection of unrelated correlations between individual neural events and experiences. For his abstract state-space (like tensor geometry) depicted systematic metrical and similarity relations between the individual points within it. He posited a four-dimensional "taste-space", based on the four types of taste-receptor found in the tongue. The points in the space represented specific distributions of four neuronal spiking frequencies, in a system of four distinct sets of nerve fibres. Crucially, they also represented specific tastes *as experienced*: they could be interpreted *either* as specific neural events *or* as specific tastes.

The *empirical* hypothesis was that people's introspective reports on the similarities and dissimilarities between different tastes would map onto the similarity metric defined by the neural taste-space. (Or anyway, onto something broadly like it: for simplicity's sake, Churchland was ignoring the multidimensional contribution of smell to what we normally regard as the subjective experience of "taste".) In that case, we'd be able to predict that a newly discovered (never-tasted) substance, having such-and-such observable effects on the relevant neural mechanisms, will taste very like *x*, something like *y*, and not at all like *z*—indeed, we could say that it *must* taste like this. Other discriminatory spaces, perhaps with many more than four dimensions, could (he suggested) be similarly defined, and the relevant phenomenology mapped accordingly.

The *philosophical* claim—a highly provocative one—was that to have a sensory experience *simply is* to have one's brain visit a particular point in the relevant sensory hyperspace. We should stop thinking of 'experiences' as separately existing events, in some subjective mental space—what Dennett (1991) later called "the Cartesian theatre". Experiences (as commonly understood) were *eliminated*, the only truly existing things/events being brain states. If neuroscience ever progressed as far as he'd imagined, said Churchland, we'd interpret our taste-language as referring not to mysterious 'inner' events but to the material, neuroscientific, reality (this claim is spelt out further in Chapter 16.iv.e).

Some philosophers were highly sympathetic, and developed their own naturalistic theories broadly on Churchland's model (e.g. Flanagan 1992). But others regarded it as—literally—absurd. Churchland had anticipated their arguments in his first book (see 16.iv.e). Now, he offered the taste-space sandwich as a neurologically plausible example illustrating how his 'absurdities' could be understood.

In other words, Churchland—unlike Crick, who also identified conscious experiences with brain states—realized that eliminative materialism wasn't a straightforward scientific theory. It required careful philosophical argument and scene setting *even to make sense*.

Crick wasn't the only neuroscientist to treat eliminative materialism as a straightforward scientific hypothesis. Edelman did so too. His account of *qualia* was similar in spirit to Churchland's (G. M. Edelman and Tononi 2000, ch. 13). But, true to rhetorical form (see ix.d above), he scorned discussions of *qualia* as "ridiculous", generating "esoteric exertions" from philosophers arguing "unnecessarily" about zombies. He failed to see that his own position was comparable with that of Dennett or Sloman, for whom zombies are—necessarily—*non-sense* (Chapter 16.iv.b). His subtle neurophysiology may have answered the question "Why *this quale* rather than *that* one?" But, despite his assertion to the contrary, it didn't explain why *any* experience should be either "associated with", "represented by", "produced/generated by", or "defined as" (expressions he used interchangeably) the neural mechanisms concerned.

Another quasi-scientific account of experience was sketched by the mathematical physicist Penrose. He suggested that the brain's generation of consciousness could be explained by CQG: a new theory of "complete quantum gravity". This theory, he said, was needed to explain certain fundamental puzzles in quantum physics. In addition, however, it would show how *non-random* quantum events could happen, and how these could implement information processing that's *not* describable as Turing-computation (see Chapter 16.v.a and ix). This, in turn, he believed to be involved in human creativity (Penrose 1989) and in consciousness (1994*a*,*b*).

Penrose admitted that he had no idea what this imaginary theory might be. Understandably, then, he was widely accused of trying to solve one utter mystery by twinning it with another. (For an excellent rebuttal based on other grounds, see Sloman 1992.) Moreover, CQG—if we ever achieved it—would apply to all matter indiscriminately: to mud, just as to neuroprotein. So what's special about the brain? His answer, in a word, was *microtubules*. These are very tiny tubes in the neurone's protoplasm, described by the neurologist Stuart Hameroff (1994).

At first, Penrose said merely that neurones have microtubules, and that CQG quantum effects isolated within them might underlie consciousness. (MacKay (1957) had argued long before that quantum effects would be swamped by a combination of neurone size and redundancy. So perhaps the microtubules would prevent the swamping?) When it was pointed out that virtually all nucleated cells have microtubules—so are oak trees conscious, too?—Penrose replied that those within neurones are different (Grush and Churchland 1995; Penrose 1994*a*,*b*; Penrose and Hameroff 1995).

Specifically, they're arranged in parallel, not radially from centrioles. They're relatively stable. There are more of them, arranged in more complex networks, than in other cells. They show greater genetic variability. They transport chemical vesicles along the axon and dendritic processes. And there are neuron-specific proteins associated with them. There's even some evidence, he said, that they transmit "complicated signals" and "act rather like a cellular automaton", and that they're responsible for learning in paramaecium (Penrose 1994*b*: 248).

Even supposing all that to be true, however, the question remained how these facts relate to consciousness as such. Certainly, the "stability" of neuronal microtubules might make one think of enduring memories. But memory storage isn't consciousness. Neither is learning, whether in paramaecium or people.

Another argument put forward by Penrose was that the "curious relationship between conscious events and time" (in meta-contrast, for instance: Chapter 6.ii.c) might be explained if the relevant quantum events took about half a second—in which case "about 1,000 to 10,000" neurones might be involved in one conscious event (which "more or less agrees with other estimates"). But this begged the question. Taking for granted that conscious experiences happen *at some time*, it focused on specifying *which* time. But why they should happen *at all* remained unclear, not to say unintelligible.

Penrose had said that mysterious quantum effects might be relevant—which they might: who knows? And he'd said that microtubules might isolate and guide these effects—which, again, they might (though he gave no good reasons to believe this). But, as two Alice fans put it, his theory was "no better supported than any one of a gazillion caterpillar-with-hookah hypotheses" (Grush and Churchland 1995).

In short, Gray's "hoary old issues" were left untouched. Penrose was trying to blind people with (dodgy) science much as Eccles had done fifty years earlier, when he spoke of the will acting through "critically poised neurones" (see Chapters 2.viii.f and 16.i.a), and as Descartes—in his talk about the pineal gland—had done earlier still (2.iii.b).

The same could be said, perhaps even more strongly, with respect to Evan Walker's (2000) account of "the physics of consciousness"—another of the many quantum-based 'explanations' published around that time. Starting from the unpredictability of single neurone-firings, Walker suggested that quantum tunnelling at the synapses could integrate many such firings into "a single quantum mechanical conscious existence" (p. 228). This happens, he said, by means of the electrons travelling from one synapse to another via RNA molecules—and guided by free will. I can't comment on his theory as *physics*—although the Cavendish Laboratory's Matthew Donald (2001) was far from impressed. But Walker was begging crucial philosophical questions, just as Eccles and Penrose had done before him.

A very different scientific (and philosophical) revolution was envisaged by David Chalmers (1966–). Chalmers allowed that near-millennium neuroscience was well on the way to solving all the problems of consciousness—except one (Chalmers 1995, 1996*a*). "The hard problem" (how conscious experience can arise from the brain), he said, hadn't been touched.

The crux of Chalmers's "prototheory" of consciousness was Shannon's concept of information. As we saw in Chapter 4.v.d, this was defined not semantically, as a meaning or intentional content, but as one of a set of possibilities. For the telephone engineer Shannon, information was a transmittable state. For Chalmers, too, "physically realized information is only information in so far as it can be *processed*" (1996*a*: 181). But for him, information had a dual aspect:

> Physical realization is the most common way to think about information embedded in the world, but it is not the only way information can be found. We can also find information realized in our *phenomenology*. States of experience fall directly into information spaces in a natural way. There are natural patterns of similarity and difference between phenomenal states, and these patterns yield the difference structure of an information space. Thus we can see phenomenal states as realizing information states within those spaces. (1996*a*: 283–4)
>
> [So we can state "the double-aspect principle":] Whenever we find an information space realized phenomenally, we find the same information space realized physically. And when an experience realizes an information state, the same information state is realized in the experience's physical substrate. (p. 284)

This was reminiscent of Churchland's emphasis on structured phenomenal spaces, related to neural mechanisms by state-space sandwiches. But whereas Churchland thought that an adequate scientific theory would eliminate experiences from the furniture of the world, Chalmers didn't. He insisted that phenomenal qualities are "irreducible".

As for whether information is a metaphysically basic concept, underlying both physics and phenomenology, or simply "a useful construct in characterizing the psychophysical laws", this question was left open (Chalmers 1996*a*: 286). So too was the question whether it's "a *primitive* feature of the physical world in the way that mass and charge are primitive". Possibly, those physicists who'd already suggested that physics ultimately deals in pure information were correct: "It could be that in some way the physical is derivative on the informational, and the ontology of this view could be worked out very neatly" (p. 287). It could even be that experience is "ubiquitous": that the very simplest systems—not just slugs or thermostats, but atoms too—have a phenomenal aspect (p. 293).

This speculative panpsychism was trumped by a gesture towards a thoroughgoing idealism: perhaps the phenomenal isn't just one of two equal-status "aspects", but the ultimate *basis* of reality. If so, then

> Every time a feature such as mass or charge is realized, there is an intrinsic property behind it: a phenomenal or protophenomenal property, or a *microphenomenal* property for short. We will have a set of basic microphenomenal spaces, one for each fundamental physical property, and it is these spaces that will ground the information spaces that physics requires. The *ultimate* differences are these microphenomenal differences. (p. 305; second italics added)

Chalmers discussed all these seemingly "outrageous, or even crazy" ideas, arguing that it's not *obvious* that they're misguided (pp. 293–310). But he didn't commit himself to any one of them.

Chalmers's book drew much attention, not least because he'd refused to sweep the hard problem under the carpet. (Health warning, again: perhaps there's no hard problem to be swept? See Section xi.) But his theory was hardly better received than Penrose's. His speculations about panpsychism and the metaphysics of information didn't help. They were even weirder than Brian Smith's views on computation and intentionality (Chapter 16.ix.e). In most people's opinion, Chalmers's quasi-psychic account of information was mere mystificatory hand-waving, analogous to the preceding *fin de siècle* myth: *élan vital* (see Chapter 2.vii.b).

Searle—unlike Penrose, Walker, or Chalmers—didn't try to sketch a new scientific theory of consciousness. However, he repeatedly stated, "to the point of tedium" (Searle 2001: 513), that there must be some scientific explanation to be found.

He'd already said this about intentionality (Searle 1980). But his comments about the brain's "causal role" in generating meaning had been scientifically empty (Chapter 16.v.d), and so were his comments about how brains cause consciousness—"the central mental notion", without which concepts like meaning, subjectivity, and intelligence can't be understood (Searle 1992, ch. 4).

One might agree with him that "Consciousness . . . is caused by neurobiological processes and is as much a part of the natural biological order as any other biological features such as photosynthesis, digestion, or mitosis" (1992: 90). But where does this get us? We're still "very far" from having an adequate biological theory of consciousness (p. 91). Probably, he said, its neurobiology is at least as restricted as that of digestion—but we can't be sure. Perhaps, on other planets, non-carbon-based molecules (though not silicon) underlie conscious experience.

Ten years later, he was still bemused: perhaps even more so, since he was now stressing the paradoxes involved in "the neurobiology of free will" (Searle 2001). But he argued that even if consciousness is based in quantum indeterminacy, conscious thought and decisions needn't be random too—and they clearly aren't. With respect to all three puzzles (free will, consciousness, and intentionality), he said:

> [We] *know* that the causal features of the system level phenomena are entirely explainable by the behaviour of the micro phenomena [in the brain] . . . [The] causal relations have the same *formal* structure as the causal relations between molecular movements and solidity. (Searle 2001: 513; first italics added)

This "knowledge", however, amounted to no more than the fact that we now have even more evidence than Descartes did to believe that the brain causes consciousness. What we want to know is *How?* Searle offered no new scientific ideas, dodgy or not: no microtubules, no dual-aspect information, no special types of computation . . . nothing. He simply said that the problem of consciousness is a scientific problem, and encouraged the scientists to get on with it.

Some pessimists agreed with the diagnosis but withheld the encouragement. The philosopher Colin McGinn (1950–) had said in the early 1980s that "We should persist in the hope that some day philosophy (or perhaps science) will find the answer [to the mind–body problem]" (1981: 36). Now, in the early 1990s, he saw any such hope as doomed (McGinn 1989, 1991).

He argued that a scientific theory of mind–brain would be *so* radically different from all known science as to be inconceivable by human beings. Neither our (perceptual) concepts of physical matter nor our (introspective) concepts of consciousness can describe any intelligible *union* of them. Moreover, these are the only conceptual realms open to us. Any third-type concepts that did explain the mind–body relation would *necessarily* be so different that we couldn't entertain them. We simply don't have the cognitive capacity to understand these matters—much as dogs can't understand arithmetic or physics:

> We know that brains are the *de facto* causal basis of consciousness . . . [But] we are cut off by our very cognitive constitution from achieving a conception of that natural property of the brain (or of consciousness) that accounts for the psychophysical link. (McGinn 1989: 350)

In a God's eye view, said McGinn, there's no ultimate scientific or metaphysical mystery here. For us, however, there is. We can glimpse the link between brain and consciousness only superficially, as temporal correlations. Interpreting those correlations causally is forever beyond us.

Most readers admitted that McGinn might, conceivably, be right. To deny that would be hubris. But cognitive scientists felt that we've no convincing reason to think he was. Our knowledge of brain mechanisms is so scanty, and the history of neuroscience so short, that it would be defeatist to give up now. McGinn's intellectual pessimism was scornfully termed "the new mysterianism"—as opposed to the "old" (neo-Kantian) mysterianism, which *denied* that consciousness is naturalistic (Flanagan 1992: 8–9).

These valiant—or hopeless—attempts to explain mind–body correlations all came up against a long-familiar problem, memorably posed by Thomas Nagel (1937–) in his paper 'What Is It Like To Be a Bat?' (Nagel 1974). In a nutshell, science is objective whereas consciousness is subjective.

We all know, said Nagel, "what it is like" to be conscious, and this—subjective, inner—experience is inaccessible to science *in principle*. Neurones, neurotransmitters, microtubules, quantum events, computation, information . . . you can forget them all. These objectively describable matters may be necessary conditions for consciousness, but they simply *cannot* provide adequate explanations of it. At best, he said, science might offer an "objective phenomenology" describing the structural aspects of subjective experiences, including those (of bats, for instance) incomprehensible to human beings. (Nagel offered no examples: possibly, one might include AI work showing that identity can be computed without computing shape, which implies that a creature might be able to experience object-identity without being able to recognize shape: Ullman 1979.)

In light of these difficulties, some philosophers—and the occasional neuroscientist, such as Maturana—tried to renegotiate the central concept at issue here. We'll see that some of these negotiators still hoped for a scientific theory of consciousness, whereas others declared this to be impossible in principle—even (*pace* McGinn) for God.

14.xi. Descartes to the Tumbrils?

Now's the time, at last, to consider the anti-Cartesian heresy that *the very idea of mind–body correlations* is so deeply confused that neuroscientists can't be required to explain it. They're off the hook because, as this idea is commonly understood, *there's nothing there to be explained*.

Dennett, having discussed conscious experience ('*qualia*') with various neuroscientists for years, recently exclaimed: "To put it bluntly, nobody outside of philosophy should take a stand on the reality of *qualia* under the assumption that they know what they're saying" (in Gazzaniga 1997: 178). I agree, for reasons outlined below.

But I'd add (and he'd concur) that people *inside* philosophy usually don't know what they're saying either. The difference is that they're well aware that the very notion of conscious experiences—without which, there can be no talk of Cartesian correlations—is radically problematic. They may use the notion, of course. But they cross their fingers behind their backs while doing so.

Cognitive scientists also cross their fingers while speaking of introspection—the way in which (so it's normally said) we gain access to our conscious experiences. For "introspection" is a weasel word. That's been evident since the late nineteenth century, when experimental psychologists following Wilhelm Wundt, James Titchener, or the Würzburg school inspected their own consciousness in very different ways (2.x). Indeed, disagreements about *what counts* as introspection largely account for the phenomenally rapid rise of behaviourism after 1913 (Chapter 5, preamble). Now, it's even clearer that there are various kinds of introspection, involving different underlying mechanisms (Prinz 2004*c*).

In short, the common assumptions that we all understand what's meant by "experience", and how to gain access to it, are much too quick.

a. Describing the mind, or inventing it?

You'll have noticed that the neuroscientists we've been considering spoke of conscious experiences in animals as well as humans. Even their paper titles sometimes made this explicit (Logothetis 1998). They may have crossed their fingers sometimes, when ascribing *specific* experiences to cats or monkeys. But they saw no problem of principle in saying that some animals have conscious sensations. (You probably agree—although we'll see in Chapter 16.viii.b that some philosophers writing today do not.) A fortiori, they saw no problem of principle in speaking about experiences occurring in adult human minds.

Admittedly, cognitive psychologists had long ago discovered that sincere introspective reports—of visual imagery, for example—sometimes have to be taken with a pinch of salt (see Chapter 7.v.a). And Paul Churchland (1979) had argued that introspection is just as "theory-laden", just as prone to top-down conceptual influence, as ordinary perception is. This was troubling to people who took seriously Descartes's view that we have infallible direct access to our own consciousness. However, *that we do have* introspectible conscious experiences was taken for granted.

We open our eyes and have visual experiences, run our hands over our bodies and have sensations of touch . . . and so on. The experiences may sometimes be surprising, as they are in cases of illusion or meta-contrast. But they occur. Who could deny *that*? It's just common sense.

"Common sense", however, may consist largely of old wives' tales—or old philosophers' tales. If so, we need to be clear about just which tales the old philosophers were telling, and whether they were fact or fiction.

Descartes's *originality* in discussing mind–brain correlations (Chapter 2.iii.a) can be glossed in two very different ways:

* On the one hand, he might have been the first to say that conscious experiences (understood as inner mental states accessible only to the subject concerned), *already* a familiar topic of conversation, are correlated with specific states of the brain—as opposed to the heart, or the liver, or even some supernatural realm.

* On the other hand, he might have been the first to say that *there are conscious experiences* (*in the sense just defined*)—adding, for good measure, that they're closely related to the brain. In that case, the newly minted notion of experiences wouldn't have been part of common sense at the time, even though it became part of common sense very soon afterwards.

The question arises, then, as to which of these two readings is correct. In other words, did Descartes *describe* the conscious mind? Or did he rather *invent* it—and if the latter, would neuroscience profit by abandoning his invention?

b. A computational analysis

Virtually all twentieth-century neuroscientists—though not Maturana (see Chapter 15.vii.b)—tacitly opted for the first interpretation of Descartes's position. To be sure, they didn't posit mental *substance*. Indeed, most of them probably hadn't even read Descartes. But they didn't need to: their common sense (deeply imbued with Cartesian ideas) told them that there are conscious experiences. Moreover, the recent experimental advances sketched in Section x had persuaded them that these experiences could at last be studied by neuroscience. That is, they could be correlated with specific brain events, considered either as their causal basis or as identical with them.

One philosopher who agreed with them wholeheartedly was Chalmers:

> [We must] *take consciousness seriously* . . . [I assume] that consciousness exists, and that *to redefine the problem as that of explaining how certain cognitive or behavioural functions are performed* is unacceptable. This is what I mean by taking consciousness seriously.
>
> Some say that consciousness is an "illusion", but I have little idea what this could even mean. It seems to me that we are surer of the existence of conscious experience than we are of anything else in the world . . . I find myself absorbed in an orange sensation, and *something is going on*. There is something that needs explaining, even after we have explained the processes of discrimination and action: there is the *experience*. (Chalmers 1996*a*, p. xii; second italics added)

Among the views Chalmers was rejecting here were those of the computationalist philosophers Dennett (1988; 1991, ch. 12; 1993*b*) and, by implication, the less well-known Sloman (1999; Sloman and Chrisley 2003). Although Dennett wished to outlaw talk of *qualia* and Sloman didn't, their analyses were similar in spirit.

Each of them had argued that the notion of 'experience' can be cashed out *completely* in terms of discriminatory computations and behaviour, both actual and potential. (Having been influenced in their youth by Gilbert Ryle, they were well aware of the relevance of *dispositional* concepts: Chapter 16.i.c.) They both allowed that these matters are grounded in neurophysiology. But they analysed the concept of experience at the level of the computational architecture involved. Sloman, for instance, saw *qualia* as states in the virtual machine that constitutes the mind.

'Zombies'—creatures looking and behaving exactly like us, but having no conscious experiences—were therefore said by them to be logically impossible (see Chapter 16.v.b). More to the point, the traditional question of mind–brain correlations *simply doesn't arise*, for there are no ontologically distinct experiences over and above bodily/cerebral events (Chapter 16.iv.b).

Both Dennett and Sloman glossed *qualia* as self-reports generated by an information-processing system with a complex, and reflexive, computational architecture. 'Subjectivity' was interpreted as the fact that these reports are directly accessible to the highest level of the system itself. They may sometimes be betrayed by involuntary movements, such as facial expressions; and they may be deliberately communicated, by words or body language, to observers. In essence, however, they are reflexive reports: the system talking *to* itself, *about* itself. The directness, or lack of 'evidence', of first-person experiential statements is due to the particular kind of (reflexive) computation involved. It's *not* due to any 'privileged access' to a mysterious inner realm of mental being (or Cartesian theatre).

The "Cartesian theatre", with its lonely audience of one, was repeatedly criticized by Dennett, whose 1991 book was hugely influential. That's not to say that it was hugely endorsed: as we'll see below, many—probably most—of his readers couldn't swallow his denial of *qualia*. That is, they couldn't accept his *interpretation* of *qualia*-talk.

They might be willing to accept his theory of "multiple drafts", in which conscious experiences are continuously (re)constructed from many computational sources (hence their paradoxical timing properties: Dennett and Kinsbourne 1992). They might even be willing to accept his account of the self as a "center of narrative gravity", an intentionalist-stance projection *of* the system *to* the system—which, once constructed, can be used to further voluntary action and moral choice (cf. Chapters 7.i.g and 16.iv.b). But that was a far cry from accepting his account of *qualia*.

Like Nagel (1974), they thought it obvious that there's *something that it's like to be* a bat, and that this experience—grounded in the bat's sensory and behavioural skills and informed by its (language-free) concepts—can't possibly be understood by us. Or like Frank Jackson (1982, 1986), they felt that a scientist (Mary) encaged from birth in a windowless black-and-white room might know everything there is to know about the physical aspects of vision, but she'd *learn something quite new* if she opened the door and stepped out into the garden.

Dennett's response (1991: 398–401) was to pour scorn on the easy assumption that we know what we mean when we say that Mary could know *everything* about the physical aspects of vision. (If she really did, she wouldn't be at all surprised that she was now able to make discriminations, and utter sentences, that she'd never made/uttered before.) He compared this easy assumption to the careless notion that imagining that someone is "filthy rich" enables one to work out the implications of asserting that *she owns everything*. Like the infamous Chinese Room (Chapter 16.v.c–d)—and, for that matter, Nagel's bat (Dennett 1991: 441–8; Akins 1993)—Jackson's Mary is "a classic provoker of Philosophers' Syndrome: mistaking a failure of imagination for an insight into necessity".

Dennett expected no easy victories: "I know that this will not satisfy many of Mary's philosophical fans, and that there is a lot more to be said" (p. 401). Indeed, he foresaw that many (most?) of his readers would feel that consciousness hadn't so much been explained, as explained away. (Several reviewers would remark that his book title invited prosecution under the Trade Descriptions Act.)

So he constructed a fictional fall guy called Otto, who made all the obvious, common-sense objections—only to be answered, not to say mocked, in computational–behavioural terms. For example:

[OTTO:] It seems to me that you've denied the existence of the most indubitably real phenomena there are: the real seemings that even Descartes in his *Meditations* couldn't doubt.

[DENNETT:] In a sense, you're right: that's what I'm denying exist. Let's [consider] the neon color-spreading phenomenon. There seems to be a pinkish glowing ring on the dust jacket. [This refers to a visual illusion, the diagram printed on the shiny white dust jacket of Dennett's book. There's a grid of vertical/horizontal black lines. Red part-lines (in place of black) form a circle around the middle. The viewer 'sees' a glowing pink ring, half an inch wide, bounded by the inner and outer ends of the red part-lines.]

[OTTO:] There sure does.

[D.C.D.:] But there isn't any pinkish glowing ring. Not really.

[OTTO:] Right. But there sure seems to be!

[D.C.D.:] Right.

[OTTO:] So where is it, then?

[D.C.D.:] Where's what?

[OTTO:] The pinkish glowing ring.

[D.C.D.:] There isn't any; I thought you'd just acknowledged that.

[OTTO:] Well yes, there isn't any pinkish ring out there on the page, but there sure seems to be.

[D.C.D.:] Right. There seems to be a pinkish glowing ring.

[OTTO:] So let's talk about *that* ring.
[D.C.D.:] Which one?
[OTTO:] The one that *seems to be*.
[D.C.D.:] There is no such thing as a pink ring that merely seems to be.
[OTTO:] Look, I don't just *say* that there seems to be a pinkish glowing ring; *there really does seem* to be a pinkish glowing ring!
[D.C.D.:] I hasten to agree. I never would accuse you of speaking disingenuously! You really mean it when you say there seems to be a pinkish glowing ring.
[OTTO:] Look. I don't just mean it. I don't just *think* there seems to be a pinkish glowing ring; *there really* seems to be a pinkish glowing ring!
[D.C.D.:] Now you've done it. You've fallen in a trap, along with a lot of others. You seem to think there's a difference between thinking (judging, deciding, being of the firm opinion that) something seems pink to you and something *really seeming* pink to you. But there is no difference. There is no such phenomenon as really seeming—over and above the phenomenon of judging in one way or another that something is the case. (Dennett 1991: 363–4)

As this book goes to press, Otto still lives. But so does Dennett, and he's unabashed. His briefer account of consciousness (Dennett 2005) takes note of his earlier critics, but gives essentially the same answer: *qualia* don't need to be explained, since there are no *qualia*. (For Sloman, there are: they exist as states in virtual machines of a complex kind—see 16.ix.c.)

c. The other side of the river

Computationalists such as Dennett (and Churchland, and Sloman) weren't the only people to criticize the very notion of '*qualia*'. The critics also included thinkers from a very different philosophical tradition—so different, indeed, that many neuroscientists were unaware of them.

These radical voices were mostly neo-Kantian, or 'Continental', philosophers (see Chapters 2.vi and 16.vi–viii). However, they also included 'ordinary language' philosophers such as John Austin (1962*b*) and the later Wittgenstein (1953). And by the end of the century they even included Putnam, who'd provided the philosophical base for functionalist cognitive science in the 1960s but who'd changed his mind fundamentally thereafter (see Chapter 16.iii and vi).

Like Dennett, but on very different (non-computationalist) grounds, such people argued that the concept of conscious experiences, *considered as private, inner, events utterly distinct from physical events*, is unintelligible. They saw it as so fundamentally confused as to be nonsense: that is, non-sense.

They blamed Descartes, whom (as remarked in Chapter 2.iii.a) they saw as having *invented* the conscious mind. Aristotle and the medieval philosophers had never mentioned it—and neither had the Renaissance humanist Michel de Montaigne. To be sure, Montaigne used French words (*l'âme*, *l'esprit*) which Descartes would use too: but he didn't use them in the same—post-*cogito*—sense. In some languages, moreover, no equivalents exist. There's no vocabulary in Ancient Greek—or in Chinese or Croatian either—that maps onto Cartesian concepts of mind and consciousness (K. V. Wilkes 1988). The familiar notion (familiar, that is, to you and to me) that anyone with any common sense, anywhere, must *know* that conscious minds exist, and that conscious experiences happen, is therefore suspect.

In short, it's no accident that Descartes is famed for tussling with the mind–body problem, for it was he who was responsible for it—*not* "he who noticed it"—in the first place. Our common-sense notions of the mind and conscious experience date back to Descartes. They weren't part of common sense before him, and (on this view) they shouldn't be part of common sense today—nor of science, either. Moreover, racking one's brains over mind–body correlations is a waste of time: *there are no mind–body correlations*, as this notion is normally understood.

Nor, for the neo-Kantian, is there any 'identity' between brain states and experiences. On this view, experience (and mind) isn't a product of brain processes but a feature of certain interactions between an agent and its surroundings. These interactions—such as speaking to one's daughter, or sorting apples and oranges into two heaps—are largely due to the internal physical structure of the agent's whole body, and especially of the brain. (So it's no surprise to the non-Cartesian that 'correlations' are found, if people are so philosophically misguided as to look for them.) But brain processes *enable* these interactions, they aren't *constitutive* of them.

Let's take Putnam as our example here, because (unlike some others in this philosophical camp) he's relatively tough-minded. Quite apart from his pivotal role in orthodox functionalism (Chapter 16.iii), he can't be accused of ignorance of cognitive science, nor of lack of sympathy with science in general. The fact that he, of all people, eventually reached this unorthodox position shows (at least) that it deserves to be considered seriously *even by the Ottos of this world*—which, I'd wager, includes most readers of this book.

By the turn of the century, Putnam was saying that "neither the standard problems in the philosophy of mind, nor the 'philosophical positions' they give rise to are really intelligible" (Putnam 1999: 112). This applied also to concepts like "internal phenomenal state" and "sense datum" (and, from his own earlier writings, "functional state").

He wasn't asking, he said,

> whether the phrase "internal psychological condition" or "internal psychological state" *ever* has an intelligible use—of course it has!—*but whether we understand what is being claimed when it is said that, e.g.* believing that there are churches in Vienna *is an internal psychological state with the same causal-explanatory role [with respect to behaviour] as the noninternal state* of knowing that there are churches in Vienna. (Putnam 1999: 113)

Similarly, he wasn't denying that we have (as we say) experiences of *yellow*. (Much as Dennett wasn't denying that we have, as we say, experiences of a pinkish glowing ring.) But, whether we're looking at a yellow door or merely hallucinating one, there's no yellow *experience*, *sense datum*, or *quale*:

> Yellow isn't a property something we experience *has* (or a property the experience *is*) as talk of qualia suggests; it is a property the experience *ascribes* to the door. The experience *portrays the environment as pertaining yellow* (it [intentionally] "refers to" yellow, as it were); it isn't a yellow (or "subjectively yellow") particular or universal. As William James put it, the quality is "in" the experience "intentionally", but the experience doesn't have it "as an attribute". Confusing having redness or hotness "intentionally" with being red or hot "adjectively" (i.e. as attributes) is [a common philosophical fallacy]. (Putnam 1999: 154)

Of course, sometimes we seem to see a yellow door when there isn't one there. In general, we're subject to perceptual illusions, and even to hallucinations. Positing (illusory) "experiences" appears, prima facie, to explain this. But—according to Putnam—this assumption, too, is mistaken. We don't see an experience, we see real things. That is, our perception is *direct*, not mediated by inner experiences. When we see mistakenly, the explanation lies in the causal transactions between the real thing and the brain. Illusory aspects of the world are, for Putnam, in a sense "veridical". But that's just to say that they are the natural effects on our perceptual system of real things, having certain physical properties and perceived in certain physical/biological conditions.

In the Müller-Lyer illusion, for example (see Figure 6.3), the real-lines-drawn-on-real-paper cause processes in visual cortex that resemble those which occur when we're presented with really unequal lines. And in schizophrenic hallucinations, the misperceptions are caused by chemical imbalances and/or learnt patterns in the brain. Those patterns, in turn, can be traced back to earlier causal transactions with real things—including pictures and printed text in real books, and real words spoken by real storytellers (remember Arbib's hierarchy of schemas).

Certainly, then, we may say that the brain constructs the illusory interpretation. What—on this view—we must *not* say is that it constructs, or causes, or correlates with . . . some mysterious mental something-or-other, some inner 'experience'. In short, none of the experimental or neurophysiological facts is denied by Putnam. What's denied is that they are to be explained by positing *qualia*, somehow interposed between the brain and the thing being (mis)perceived.

This may strike you (as it would Otto) as crazy. You may want to echo Chalmers's remark, quoted above: "It seems to me that we are surer of the existence of conscious experience than we are of anything else in the world." Is Putnam mad? And is Dennett mad, too? *What on earth* are they up to?

d. Lions and lines

As an analogy, imagine that a great scientist–philosopher on his deathbed was heard to say, just before his last gasp, that navigators would benefit from considering the lions running around the earth.

His mourners found, to their delight, that he'd already drawn their pathways on the globe in his study. Evidently, the lions were quite convivial, for they met regularly (the best-attended gatherings took place at the poles). Sure enough, over the next few centuries the navigators—and eventually also the tourists—found it very helpful to consider the lions. They were even recognized as individuals: four were given special names, and the rest were named by numbers.

Field expeditions aimed at capturing one of these animals, however, always failed. Perhaps the lions were very shy, or very small, or running unseen in microtunnels . . . or perhaps (because of some unsuspected glitch of quantum physics?) forever invisible. The arguments raged on and on. But no one doubted that the lions were there. After all, their pathways had been measured precisely, and they were mentioned every day—in conversations that undeniably helped people to reach their intended destinations. *Of course* the lions existed!

But, of course, they didn't. The dying man had been misheard: he'd said "lines", not "lions". Moreover, he hadn't meant *real* lines, such as might be made by paw marks. No, these were imaginary lines, of latitude and longitude (what Dennett would call *abstracta*, or "exquisitely useful fictions"—1991: 367; see Chapter 16.iv.b).

There was no harm in his followers' speaking about "lions", if this language was used purely *geographically*. But if it was interpreted as referring to actual lions (perhaps very small ones!), with paws, whiskers, and purrs, it was highly misleading—not to say false. In Ryle's (1949) terms, belief in such lions was a category mistake (Chapter 16.i.c).

The mistake was by now so entrenched, however, that the few people who questioned the lions' existence (as *lions*) were accused of denying the facts underlying map-making and navigation. In truth, they were doing no such thing. But they were undercutting every single one of the ingenious theories (shy, small, quantum-invisible . . .) constructed to answer questions about the elusive lions' real nature. More: they were showing why *no such questions need arise*, and why *no such theory could ever be successful*.—And all without denying *any* of the facts that made lion talk so useful, and which had led the long-dead philosopher to posit the lines/lions in the first place.

One of the points at which this analogy (like all analogies) breaks down is that Descartes himself had *not* been misheard. He did believe in 'lions'—that is, in mental states considered as aspects of a special type of existence, just as real (indeed, just as substantial) as physical matter.

By the twentieth century, his followers had abandoned his belief in mental substance. (The lions could leave no paw marks.) But they still assumed the metaphysically distinct existence of conscious mental states, as events within a distinct level of reality. (The lions' purrs could be heard.) They called them experiences, sensations, raw feels, sense data, *qualia* . . . and woe betide anyone who questioned their existence *as so conceptualized*. The brave people who did question them were wrongly supposed to be denying the facts that had led to such talk in the first place—such as the fact that we sometimes think we're seeing a yellow door, or a pinkish glowing ring, when there isn't one.

Putnam, and Dennett too, is on the side of the *lines*. For both of them, although for somewhat different reasons, the claims of Crick and Churchland that so-called conscious experiences are in truth *the very same thing as brain states* are literally nonsensical. (Churchland, then, had been quite right to say that this wasn't a straightforward scientific theory, inviting tests rather than philosophical analysis.)

What's more, for Putnam (and Dennett) the question of mind–brain correlations, *as it's normally understood*, simply doesn't arise. There are no such correlations. There couldn't be any such correlations, because there are no *qualia* or inner conscious 'experiences' (again, *as these are normally understood*) that might be correlated, or not, with specific brain events. There's no need to puzzle over how to explain mind–brain correlations, or how to 'strengthen' them—pressed, perhaps, by Princess Elizabeth?—into physics-defying *causes*. For there's no correlation there to be explained.

Certainly, Putnam admitted, we need to explain the huge variety of behavioural discriminations displayed by people and animals. And he allowed that neuroscience—indeed, *computational* neuroscience—will be needed to do so (Putnam

1999: 48). Unlike Dennett, however, he didn't try to say what the neurocomputational architecture must be like. His interest was simply to show that the neo-Cartesian assumption shared by virtually all neuroscientists, *that there are mental (conscious) events and physical (brain) states, and that we should try to explain the correlations between them*, must be rejected.

If Putnam (or Dennett) is right, it's no wonder that neuroscientists face huge difficulties in explaining how the brain causes conscious experiences—and that increasingly bizarre 'scientific' hypotheses are dreamt up accordingly. This is just what one would expect, if their basic Cartesian assumption (italicized above) is philosophically confused.

If we decide to reject this assumption, none of the intriguing discoveries of neuroscience (nor of cognitive psychology) need be denied. But they must be interpreted in a very different way.

We can still say that rhesus monkeys—and, perhaps, humans—discriminate motion perception by cortical cells arranged around the compass, and that these discriminations are nicely predictable in neuroscientific terms. What we can't say is that the monkeys, or the humans, *have inner visual experiences* of motion-in-a-certain-direction.

Again, we can still say that the blindsight patient can make certain visual discriminations without being able to report them, and that this is because their information processing doesn't include reflexive computations of the kind necessary for self-report. We can even say that their common-sense belief system leads them to protest at the 'absurdity' of being asked to *guess*. What we can't say is that they lack what the normal person possesses: *inner experiences* (*qualia*) of objects seen in 3D space.

Last (to take an example discussed in Chapter 7.v.a), we can still say that when people imagine seeing landscapes or elephants, they're able to answer certain questions about them. We can also say that this capacity may involve such-and-such parts of, and/or such-and-such computational processes in, the brain. But we can't say that this behaviour involves *inner mental pictures*, *or conscious experiences (images)*.

The fundamental problem, then, is whether the neuroscience of consciousness should concern itself with *lions*, or with *lines*.

Despite neuroscience's huge intellectual debt to Descartes, who first defined it as a respectable activity (see Chapter 2.iii and viii), perhaps it should dismiss him as the storyteller of consciousness? Admittedly, lifting Descartes onto the philosophical tumbrils would be just as revolutionary as what his compatriots did in 1789—perhaps even more so. For our common sense (and Otto) cries out: "Rubbish! *Of course* neuroscience studies lions!" But the stubborn persistence of Gray's hoary old problems, not to mention the respect due to serious thinkers (albeit some of a very different philosophical persuasion), should at least give us pause.

e. Hung jury

My own view is that *if* we are ever to understand mind–brain correlations, some radical change in both neuroscience and the philosophy of mind will be required. So it's no accident that the end-of-century presses hummed with strange theories such as those of Penrose, Walker, and Chalmers—and others I've not had space to discuss, including various forms of panpsychism.

Some years ago, I argued that the conceptual change concerned would need to be comparable in depth to Maxwell's field theory (Boden 1998*b*). This theory reinterpreted physicists' seemingly contradictory talk of "waves" and "particles". These utterly different, even mutually exclusive, phenomena had been needed to account for different sets of experiments. Maxwell explained both in terms of an underlying theory mentioning neither, and also showed why they'd been used in distinct types of experimental situation. An analogous advance, I said, would be required to explain how the brain can generate *any* conscious experience. (Perhaps mind-as-virtual-machine is the core of the solution?)

However, I also said that this is a big "if". And I asked whether a fundamentally non-Cartesian neo-Kantian (as opposed to a Dennettian) approach might be better.

Then, I answered that it wouldn't. Not only would it mean that the 'mind–body correlations' we all talk about so confidently would have to be systematically redescribed (lines, not lions), but the nature of science in general would be redefined—not to say undermined. Instead of being a *realistic* enterprise, it would be a construction of human minds (and societies), no more 'truthful' than any other. As I said in the brief remarks on postmodernism in Chapter 1.iii.b, I regard this neo-Kantian view as fundamentally irrational. Even in the hands of a skilled analytic philosopher like John McDowell, or the middle Putnam, it's unconvincing (see Chapter 16.viii.b and vi, respectively).

But the late Putnam is different, for he now insists on the "natural realism" of science. He even defends the "direct realism" of perception—where, as we saw above, direct doesn't mean infallible. So—for him—*that* troubling philosophical bird is scotched.

Significant problems remain, however, about whether intentionality (meaning)—which is a crucial aspect of consciousness—can ever be naturalized. One can't responsibly offer an answer to our question about *qualia* without considering this question also.

For my part, if I were confident that intentionality can be scientifically explained—perhaps by Ruth Millikan's (1984) evolutionary approach: see Chapter 16.x.d—I'd gladly abandon the lions, or locate them in virtual reality. We've seen, after all, what trouble they cause. But intentionality is a huge, and far-reaching, philosophical problem—to be considered at length in Chapter 16.

Here, let me just say that my own preference—I have no knock-down argument—is for an evolutionary–computational account of these issues, not a neo-Kantian one (see Boden 1972). Any such account takes science to be a realist enterprise, an assumption I cannot bring myself to drop. One well-developed example, here, is Dennett's. He sketched an early version of Millikan's approach in his first book (1969), and holds that intentionality is something we *ascribe* to certain sorts of complex computational system. We do so because it helps us to predict, manage, and understand their behaviour (16.iv.a–b).

For Putnam, both middle and late, and for other neo-Kantians too, intentionality is a real property. Moreover, for them it is *prior* to science—not something that can be explained by it (Chapter 16.vi–viii). It follows, on that view, that neuroscience can never explain meaning. At most, it can describe what goes on in our brains when we have meaningful thoughts.

In sum, the existence of *qualia*—and therefore of mind–body correlations, *as normally understood*—isn't as "obvious" as it's typically assumed to be. And if their existence is in doubt, their explanation is necessarily problematic. The jury's still out. The discussion is often bedevilled by disagreements rooted in a clash of intuitions wholly impervious to argument (a fact noted by Dennett: see Chapter 16.iv.b). But one thing's sure: mind–brain correlations aren't a purely *scientific* problem. The philosophy has to be sorted out too.

15

A-LIFE IN EMBRYO

Artificial life is widely assumed to be excitingly new. Exciting, it is. But new, it isn't.

It was born in the 1940s, fathered by 'the two Williams': Grey Walter and Ross Ashby (Chapter 4.viii). It developed healthily in the 1950s, nurtured (as we'll see) by Alan Turing and John von Neumann. By 1960, the core ideas—situated behaviour, self-organization, adaptation, and evolution—had been sketched, and all but the last had even been simulated.

Then, A-Life (like connectionism) lapsed into a Sleeping Beauty phase. Nothing much happened for a while. Or rather, it did—but few people noticed. Even Herbert Simon's ant walked largely unseen (Chapter 7.iv.a). In the early 1980s, the sleeper stirred. The highly public awakening happened in 1987, at an interdisciplinary naming party that introduced "A-Life" not only to science but to the outside world. Today, the outside world accepts A-Life in a spirit both more mystified (S. R. L. Clark 1995) and more welcoming (Kember 2003; Whitelaw 2004) than the public perception of classical AI (see 13.vii.b). Even the postmodernists had learnt to love A-Life (see 1.iii.d).

The cyberneticists in general had assimilated life to mind. They believed that the life–mind gap could be bridged by a single type of explanation. Ashby, for example, held that his dynamical theories—and his artefacts—could advance both biology and psychology. Self-organization and adaptation, discrimination and teleology, were seen as equally central concerns.

But their explanatory monism went underground with the breakaway of GOFAI (see 4.ix.b). Most early computational psychologists focused on logical-symbolic modelling. Propositional contents ruled the day, while metabolism, adaptation, and self-organization were forgotten—or, in the latter case, assimilated to learning. Psychology, it seemed, had nothing to do with biology. Indeed, functionalist philosophy made this explicit as "multiple realizability" (16.iii.b). Cybernetics itself, meanwhile, became more closely associated with control engineering than with psychology or biology.

Later, things changed. By the end of the century, computer modelling was focused increasingly on life, and (in some cases) on the unification of life with mind. Ideas from many different sources were used in explaining the structural changes in biological (embryonic and evolutionary) and psychological development.

The biochemical origins of life were discussed too, sometimes in highly abstract, functional, terms (e.g. Drexler 1989). And people tried to model, and/or to build, chemical analogues of the origins of life at the molecular level (e.g. Luisi and Varela

1989; Szostak *et al*. 2001; Luisi *et al*. 2004). Apart from a few brief remarks at the close of Section x.b, I'll ignore these chemical discussions.

The theoretical focus of A-Life is the central feature of living things, namely self-organization. So to define self-organization is to identify the basic themes of this chapter:

* Self-organization is the spontaneous emergence (and maintenance) of order, out of an origin that's ordered to a lesser degree.
* It concerns not mere superficial change, but fundamental structural development.
* The development is spontaneous, or autonomous, in that it results from the intrinsic character of the system (often in interaction with the environment), rather than being imposed on it by some external force or designer.
* In self-organizing systems, higher-level properties result from interactions between simpler ones—and there may be many different levels of organization involved.

The phrase "spontaneous emergence" in that definition may sound like magic. It may even *feel* like magic:

> When I wrote the program [i.e. Biomorph: see vi.b, below], I never thought that it would evolve anything more than a variety of tree-like shapes . . . Nothing in my biologist's intuition, nothing in my 20 years' experience of programming computers, and nothing in my wildest dreams, prepared me for what actually emerged on the screen. . . . I distinctly heard the triumphal chords of *Also Sprach Zarathustra* (the "2001 theme") in my mind. I couldn't eat, and that night "my" insects swarmed behind my eyelids as I tried to sleep. (Dawkins 1986: 60)

If even the hard-headed Richard Dawkins reacted in this way to something unexpected on his utterly unmagical computer, how much more have people wondered at the examples of self-organization in the biological realm.

Emergence isn't imaginary: surprising phenomena, even radically *new* phenomena, do happen. In particular, unexpected high-level patterns can result from interactions between very simple lower-level processes (A. J. Clark 2001: 112–17). But it isn't magic, either—even though "emergent" is sometimes used as a buzzword with near-mystical overtones. (For a wide range of definitions, see Stephan 1998, 2003.) The aim of A-Life, with respect to emergence, is to provide the explanation without losing the wonder.

This chapter outlines the diverse origins of A-Life, and recounts how they were eventually woven together. It starts with a brief sketch of the historical links between the concepts of life, mind, and self-organization. Then, Section ii distinguishes A-Life from biomimetics. Section iii relates it to early mathematical biology, and to neo-Kantian biology too.

The story of A-Life as such begins with Section iv. (More accurately, it began in Chapter 4.viii, as remarked above.) Sections iv and v outline the relevance of reaction–diffusion equations and self-replicating automata, respectively. Evolutionary networks are discussed in Section vi.

The sensori-motor integration of the whole animal is the topic of Section vii, which outlines research in computational neuro-ethology (CNE), much of which appears in A-Life journals. Sections viii and ix describe a wide range of work on complex systems, including some done by physicists and computer scientists, and some done in a neo-Kantian spirit. The way in which A-Life was eventually recognized as a single field is described in Section x. Finally, Section xi discusses two state-of-the-art examples that are potentially relevant to cognitive science in general.

15.i. Life, Mind, Self-Organization

The historical roots of cognitive science are highly diverse, as we've seen. In particular, questions about mental processes and living things have long been intertwined. Recently, that intertwining has become even more closely tangled. For it's now clear that self-organization—the core feature of life—is an important concept in neuroscience and psychology too (see Chapters 12 and 14—especially 12.ii and v, and 14.vi and viii.c–d).

a. Life and mind versus life-and-mind

It's widely assumed—though rarely explicitly argued—that life is *necessary* for mind. This assumption, and the closely related dispute about the possibility of "strong A-Life", will be explored at length in Chapter 16.x. Meanwhile, let's merely acknowledge that all the minds we know of are found in living things. That's why "life" and "mind" are so commonly associated.

René Descartes didn't make the 'necessity' assumption (Chapter 2.ii–iii). He didn't believe in life-and-mind. Rather, he thought that the association between minds and living (human) bodies is fundamentally unintelligible, arranged by the whim of God. On his view, life and mind are radically different: living organisms, including human bodies, can be described in scientific (mechanistic) terms whereas minds can't. But as we saw in Chapter 2.v–vii, his successors didn't all agree with him.

Some disagreed by doubting the possibility of a physicalist biology. The chemist Justus von Liebig, for instance, held that chemical processes are "altered" by a vital force so as to produce compounds "altogether different" from those in inanimate matter. Others disagreed by claiming that life and mind can be explained in essentially similar ways—perhaps mechanistic (e.g. Julien de La Mettrie), perhaps not (e.g. Johann von Goethe and the Naturphilosophen).

The late nineteenth-century advances in general physiology and neurophysiology didn't settle the dispute. Non-physicalist accounts of both life and mind were still proposed after the turn of the century. It was said, for instance, that embryonic development is directed by vitalist entelechies, that adaptive evolution results from a creative life force, and that purposive behaviour involves a specifically psychic energy (see Chapter 2.vii.b and 2.x.b).

In short, in the early twentieth century life and mind were still thought by many people to lie beyond the bounds of empirical science. And life-and-mind was still a highly controversial idea.

b. Self-organization, in and out of focus

The four concepts just mentioned—embryogenesis, adaptation, evolution, and purpose—are fundamentally akin. Indeed, the word "evolution" used to mean embryogenesis—which is why Charles Darwin wrote of "descent with modification" in *The*

Origin (Bowler 1975). All four are examples of self-organization. So another way of putting the point made above is to say that, well into the twentieth century, self-organization—whether biological or psychological—was felt to lie outside science. A fortiori, it was out of reach of machine-based theories.

By the mid-twentieth century, the cybernetic movement had made more people willing to believe that these phenomena could be scientifically explained. Cybernetic explanations weren't mechanistic in the Cartesian sense outlined in Chapter 2.ii.a. They focused not on matter or energy, but on abstractions such as information, computation, adaptation, or equifinality (see Chapter 4.v–viii). These were assumed to be instantiated in physical systems, notably in organisms.

The cyberneticians, then, saw no essential mystery in self-organization. Moreover, they discussed its *origin*, as well as its *maintenance*. Ross Ashby's "ultrastable" Homeostat, for instance, showed how randomness could give way to order. And Norbert Wiener, in discussing quantum physics at the fifth Macy seminar, spoke of order arising from chaos. So whereas Claude Bernard and Walter Cannon had focused on the body's self-maintenance (see Chapter 2.vii.a), their followers in the 1940s–1950s were interested also in the generation of new order, whether in adaptation or (more rarely) in development.

Nevertheless, they weren't all happy with the term "self-organization". Ashby sometimes used it, and "self-coordinating" too (Ashby 1960: 10). But he also complained that such phrases were "fundamentally confused and inconsistent" and "probably better allowed to die out" (Ashby 1962: 269). He saw them as potentially mystifying because they imply that there's an organizer when in fact there isn't. Some modern researchers agree, carefully avoiding 'self-organization' and referring instead to *organization*: the ability to act as a unified whole (see Chapter 14.viii.c).

Yet the concept of self-organization hasn't died out. It's often mentioned in relation to neural networks—which Marvin Minsky and Seymour Papert (1969) saw as potentially related to "how the genetic program computes organisms" (see 12.iii–v). Neuroscientists use it in explaining the development of topographical maps in the brain (14.vi.b and ix.a). And it's employed by many scientists in A-Life—not to imply some mysterious 'inner organizer', but to focus on *the spontaneous origin and development* of organization at least as much as on its maintenance. As I use it in this chapter, the term carries that bias.

Self-organization is the central feature of life (see Section ii.b, below). It's no accident, then, that it's especially prominent in A-Life—as opposed to AI, psychology, or neuroscience. Those three disciplines focus on particular examples of behaviour/cognition, which they occasionally explain in terms of self-organization. By contrast, A-Life tends to focus on self-organization *as such*, using particular behaviours (such as flocking, ant trails, walking, or bodily control via perceptuo-motor mechanisms) as illustrations of it. (Similarly, the cyberneticists treated reflexes as illustrations of circular causation, rather than behavioural problems to be studied in their own right.)

The initial excitement about cybernetics in biology was soon swamped. It lost out to another way of mapping abstractions onto physical reality: the genetic code. This was hypothesized by Erwin Schrödinger in the 1940s, and attributed to DNA's double helix by Francis Crick and James Watson in 1953. It was finally decrypted, as amino-acid triplets coding for proteins, in the 1960s. As a result, the holism of cybernetics was swiftly overshadowed by a strongly reductionist molecular biology (Judson 1979).

This approach soon detailed many specific mechanisms involved in self-organization. These included gene regulation, immune reactions, and the 'programming' of neurones to make connections of specific types. But self-organization *as such* wasn't a concept of molecular biology.

It was to be born again, in a new guise, towards the end of the century. (Ironically, some of these revivalist ideas drew on the molecular biology of the immune system: Chapter 14.ix.d.) At that time, too, the cyberneticists' faith that similar types of explanation apply to both life and mind was renewed. Whether that faith was *justified* will be discussed in Chapter 16.x.

15.ii. Biomimetics and Artificial Life

Not every physical artefact that mimics living things is an example of A-Life. To show why, let's consider two examples of artificial fish—one ancient, one modern.

a. Artificial fish

In 1776 Henry Cavendish (1731–1810) nominated Captain James Cook for election to the Royal Society. Having just completed his second great voyage of discovery, Cook had exciting tales to tell of exotic fish and alien seas. But so did Cavendish. For, in the very same year, he had built an artificial electric fish and lain it in an artificial sea (Wu 1984; Hackman 1989).

Its body was made of wood and sheepskin, and its electric organ was two pewter discs, connected by a brass chain to a large Leyden battery; its habitat was a trough of salt water. Cavendish's aim was to prove that "animal electricity" is the same as the physicist's electricity, not an essentially different (vital) phenomenon. His immobile 'fish' wouldn't have fooled anyone into thinking it was a real fish, despite its fish-shaped leather 'body'. But, and this was the point, it did deliver a real electric shock, indistinguishable from that sent out by a real torpedo fish.

A hundred years later, a latter-day Cavendish might have interpreted his fish's electric organs (sensors and effectors) as Darwinian adaptations, with specific functions in the organism's biology. Today's neo-Darwinians are happy to do the same.

(However, some of them see no possibility of a *determinate* answer to the question "What is it for?" Daniel Dennett argues that attributions of biological function can *never* be wholly closed, wholly cut and dried: "It is not just that I can't tell, and they can't tell; *there is nothing to tell*"—1987: 312; italics added. His view on this matter is a biological version of his non-realist stance on psychological intentionality, outlined in Chapter 16.iv.b. It certainly doesn't match the practice of working biologists—neither in general (Kitcher and Kitcher 1988; Amundson 1988), nor in the particular case of electric fish (Keeley 1999). I'll take it for granted in this chapter, then, that biological/adaptive function can often be assigned with confidence, even if there are some difficult cases.)

Just over 200 years later, the fourth international conference on A-Life opened with a paper—and an uncannily lifelike demonstration—on the computer simulation of fish (Terzopoulos *et al*. 1994).

Whereas Cavendish's 'fish' had been a solitary object lying inert in a dish of water, these denizens of the VDU were constantly in motion, sometimes forming hunter–hunted pairs or co-moving schools. Each one, swimming around constantly, was an autonomous (independently acting) creature, with simple perceptual abilities that enabled it to respond to the world and to its fellows. Unlike the crickets and cockroaches discussed in Section vii below, or Jacques de Vaucanson's anatomically detailed duck (2.iv.a), they weren't robots: they were software creatures existing in a computer-generated virtual world.

To achieve these lifelike simulations, the modern authors started with digitized still photographs of real fish representing shape, colour, and texture. They converted these into movies not by hand-crafted image-by-image animation (as in Walt Disney's *Snow White*) but by defining rules enabling a computer to change the images automatically. These rules were based on the anatomy, mechanics, and ethology of various species of real fish, and also on real-world hydrodynamics.

The major bodily movements of the animations, with their associated changes in body shape, resulted from twelve internal muscles (conceptualized as springs). The computerized fish learned to control these in order to ride the (simulated) hydrodynamics of the surrounding sea water. A host of minor movements arose from the definitions of seventy-nine other springs and twenty-three nodal point masses, whose (virtual) physics resulted in subtly lifelike locomotion.

But realistic bodily movement doesn't suffice for realistic behaviour: where should the (animated) fish move to, how fast, how urgently, and when? These questions were answered by ethologically credible rules defining the movement trajectories and environmental conditions appropriate for various behaviours. These included feeding, hunting, fleeing, nuzzling, and reproducing. Interactive aspects, such as predator–prey behaviour, resulted from rules defining the mutual responses of two or more autonomous creatures.

Cavendish had intended his artificial fish to deliver an intellectual shock, as well as a real one. His aim was to demystify a vital phenomenon, to show the continuity between the physical and the organic—and, of course, to display the physical principle underlying the living behaviour.

He thought this shocking hypothesis to be so important that he invited some colleagues into his laboratory to observe the experiment—so far as we know, the only occasion on which he did so (Wu 1984: 602). Certainly, such an invitation from the taciturn Cavendish was a remarkable event: an acquaintance said that he "probably uttered fewer words in the course of his life than any man who ever lived to fourscore years, not at all excepting the monks of la Trappe" (Lord Brougham, quoted in *Encyclopaedia Britannica* 1990: 975). (It's been suggested that Cavendish's unsociability was due to Asperger's syndrome; the same posthumous 'diagnosis' has been made of Albert Einstein and Isaac Newton: Sacks 2001; Baron-Cohen and James 2003.)

Our contemporary fish-makers had a similar intellectual aim. Their digitized fish weren't intended as mere toys, or tools for Hollywood animators—although similar techniques are now being used for both home entertainment and commercial cinema.

Rather, they were intended to clarify "the interplay of physics, locomotion, perception, behavior, and learning in higher animals" (Terzopoulos *et al.* 1994: 17). That is, they were examples of research in A-Life.

Should we say, then, that A-Life originated with Cavendish, two centuries ago?—No. Cavendish's work was an early foray into biomimetics, not A-Life.

b. What is A-Life?

Biomimetics studies physical artefacts that mimic the material stuff of life, putting these studies to scientific or engineering use. (The term "biomimesis" also has another meaning: as defined by Warren McCulloch in 1961, it is "the imitation of one form of life by another"—for instance, a stick insect that looks just like a leaf: Bensaude-Vincent forthcoming. That meaning isn't relevant here.)

For instance, biomimetics explores the irritability and excitability of (non-biological) membranes. Or it builds iron-wire-in-nitric-acid models of nervous transmission (Lillie 1936). Or it analyses living structures for purposes of architectural design: water lily leaves for Joseph Paxton's magnificent Crystal Palace, perhaps. Or it models artificial sensors on sensors in plants and, especially, animals (Barth *et al.* 2003). Specific material properties are taken seriously, so that if some chemical element were missing, or some physical measure different, the biomimetic model would ideally be correspondingly altered.

A-Life is more abstract than this, even though the virtual-fish world does include data about actual fish and does attempt precise simulation of hydrodynamics. It draws its distinctive ideas from computer science and complex dynamics (see Sections v–viii). It uses computer modelling—including robotics—as its methodology. And it aims to explain not only particular vital phenomena but also the properties of life in general. As Christopher Langton (1949–) said when he first defined it in 1986, it studies "life as it could be" rather than merely "life as we know it" (Langton 1989*b*: 2).

The core of A-Life is self-organization, which (as remarked in the preamble) can involve interactions occurring on several levels. In living organisms, the relevant interactions include chemical diffusion, perception and communication, adaptation, and evolution.

The emergent properties are also of many different kinds. They include universal characteristics of life, such as autonomy, reproduction, and metabolism. They also cover distinct lifestyles, such as parasitism, and particular behaviours, such as flocking, hunting, or evasion. Bodily morphology is included: for instance, the anatomy of sense organs, motor organs (such as fins), and control mechanisms. So, too, are widespread developmental processes, such as cell differentiation.

The formation of single cells, the simplest living things, is a fundamental example of biotic self-organization. Many would say it's *the* fundamental example (see Section vii.b). But they'd allow that the spontaneous formation of new chemical compounds in prebiotic 'soups' is also relevant to biology.

Self-organization *tout court* doesn't suffice for life. It occurs in some purely chemical systems—such as the Belousov–Zhabotinsky diffusion reaction, wherein organized ring patterns of different colours arise within an initially homogeneous fluid. Life requires some or all of the types of self-organization just mentioned. ("Some or all", because there's no universally agreed definition of the concept of life (Boden 1996, pt. IV): see also 16.x.b–c.) Accordingly, work in A-Life has ranged across all these aspects of living organisms.

The aim of A-Life is to understand life, not to construct new life forms. Admittedly, a tiny handful of A-Life researchers hope to synthesize new life, even *virtual* life (see Section ix.b). In general, however, A-Life doesn't try to go one better than biomimetics. Its late twentieth-century artefacts, like Cavendish's leather fish, are meant to exemplify *theories about life*—not life itself.

15.iii. Mathematical Biology Begins

Someone might seek to distance A-Life still further from Cavendish's research, by suggesting another sense in which it is "more abstract".

Cavendish's experiment couldn't have been done without the artificial fish in its bath of conducting fluid, because his aim was to reproduce the same physical phenomenon (electrical conductivity) that occurs in some living things. Biomimetics requires physical mimesis. But—so someone might argue—A-Life's artefacts, namely computers, are dispensable. If artefacts are needed at all, then just three are enough: pencil, paper, and armchair. For A-Life is a recent variety of mathematical biology, and mathematics is an abstract science.

Our imaginary interlocutor is right in saying that A-Life is a form of mathematical biology. That is, it uses (broadly) mathematical concepts and techniques to analyse and explain properties of living things. But it doesn't follow that A-Life's artefacts are dispensable, as we'll see.

a. Of growth and form

Isolated examples of mathematically expressed biological research were scattered in the pre-twentieth-century literature. But mathematical biology as an all-encompassing and systematic approach was attempted only after the turn of the century. Its instigator was the zoologist Sir D'Arcy Thompson (1860–1948) of the University of St Andrews, whose visionary work *On Growth and Form* was first published in 1917. Richly packed with fascinating examples, it ran to nearly 800 pages. A much enlarged (1,116 pages) second edition appeared in 1942, six years before the author's death.

D'Arcy Thompson—he's hardly ever referred to merely as "Thompson"—was born only a year after the publication of *On the Origin of Species*, and was already middle-aged when Queen Victoria died in 1901. He survived through both world wars, dying at almost 90 years old in 1948. That was the year in which the Manchester MADM computer, for which Turing was the first programmer, became operational.

If he had an exceptional span in life years, he also had an extraordinary span in intellectual skills. He was a highly honoured classical scholar, who translated *Historia Animalium* for the authoritative edition of Aristotle (D. W. Thompson 1910). In addition, he was a biologist and mathematician, and was offered chairs in Classics and Mathematics as well as in Zoology.

While still a teenager (if 'teenagers' existed in Victorian Scotland), he edited a brief book of essays from the Museum of Zoology in Dundee (D. W. Thompson 1880). But he soon graduated to larger tomes. In his early twenties, he edited and translated a German biologist's scattered writings on how flowers of different types are pollinated by insects—in a 670-page volume for which Darwin himself wrote the Preface (D. W. Thompson 1883).

Forty years later, he was commenting on ancient Egyptian mathematics in *Nature* (D. W. Thompson 1925), and analysing the catches made by fishermen trawling off Aberdeen (D. W. Thompson 1931). His essays ran from classical biology and astronomy, through poetry and medicine, to 'Games and Playthings' from Greece and Rome, and included popular pieces written for *Country Life*, *Strand Magazine*, and *Blackwood's Magazine* (D. W. Thompson 1940). His last book, out a few months before he died, was *A Glossary of Greek Fishes*: a "sequel" to his volume on all the birds mentioned in ancient Greek texts (D. W. Thompson 1895, 1947). Clearly, then, D'Arcy Thompson was a man of parts.

He was no mere list-maker, as some of the titles above might suggest. On the contrary, he was a great intellect and a superb wordsmith. His major book has been described by the biologist Peter Medawar as "beyond comparison the finest work of literature in all the annals of science that have been recorded in the English tongue" (Medawar 1958: 232). And his intoxicating literary prose was matched by his imaginative scientific vision.

Although Darwin had written the Preface for his first 'real' book, D'Arcy Thompson became increasingly critical of Darwinian theory. An early intimation of this was in his paper 'Some Difficulties of Darwinism', given in 1894 to an Oxford meeting of the British Association for the Advancement of Science (one of Babbage's many brainchildren: Chapter 3.i.a).

His book, over twenty years later, explained at length why he felt Darwinism to be inadequate as an explanation of the living creatures we see around us. Like some maverick modern biologists (see Section viii), he regarded natural selection as strictly secondary to the origin of biological form—which must be explained in a different way.

He integrated a host of individual biological facts within a systematic vision of the order implicit in living organisms. That is, he used various ideas from mathematics not only to describe, but also to explain, fundamental features of biological form. He wasn't content, for example, to note that patterns of leaf sprouting on plants may often be described by a Fibonacci number series (such as 0, 1, 1, 2, 3, 5, 8, 13, 21, . . .). He converted this finding from a mathematical curiosity into a biologically intelligible fact, by pointing out that this is the most efficient way of using the space available.

Significantly, he often combined 'pure' mathematical analysis with the equations of theoretical physics. In this way, he tried to explain not only specific anatomical facts (such as the width and branching patterns of arteries, relative to the amount of blood to be transported), but also why certain forms appear repeatedly in the living world.

D'Arcy Thompson referred to countless examples of actual organisms, but he had in mind also *all possible* life forms. As he put it:

> [I] have tried in comparatively simple cases to use mathematical methods and mathematical terminology to describe and define the forms of organisms . . . [My] study of organic form, which [I] call by Goethe's name of Morphology, is but a portion of that wider Science of Form which deals with the forms assumed by matter under all aspects and conditions, and, in a still wider sense, with forms which are theoretically imaginable. (1917/1942: 1026)

For D'Arcy Thompson, then, the shapes of animals and plants aren't purely random: we can't say "Anything goes". To the contrary, developmental and evolutionary changes in morphology are constrained by underlying general principles of physical and mathematical order.

As his own acknowledgement made clear, D'Arcy Thompson's work was closely related to Goethe's rational morphology (see Chapter 2.vi.e). Like Goethe, whom he quoted with approval several times in his book, he sought an abstract description of the anatomical structures and transformations found in living things—indeed, in all possible things. So he discussed the reasons for the spherical shape of soap bubbles, for instance.

His reference to "forms which are theoretically imaginable" recalls Goethe's remark (quoted in 2.vi.e) that

> With such a model [of the archetypal plant and its transformations] . . . one will be able to contrive an infinite variety of plants. They will be strictly logical plants—in other words, even though they may not actually exist, they could exist. [Compare: "life as it could be".] They will not be mere picturesque and imaginative projects. They will be imbued with inner truth and necessity. And the same will be applicable to all that lives. (quoted in Nisbet 1972: 45)

And like Goethe, he believed that certain forms were more natural, more likely, than others. In some sense, he thought, there are "primal phenomena".

Also like Goethe—though here, the analogy becomes more strained—he asked questions about the physical mechanisms involved in bodily growth. But his philosophical motivation for those questions was importantly different. Although D'Arcy Thompson was sympathetic to some of the claims of the Naturphilosophen, he wasn't a fully paid-up member of their club. Indeed, he opened his book by criticizing Immanuel Kant and Goethe, complaining that they had ruled mathematics out of natural history (Thompson 1917/1942: 2).

In part, he was here expressing his conviction that "the harmony of the world is made manifest in Form and Number, and the heart and soul and all the poetry of Natural Philosophy are embodied in the concept of mathematical beauty" (pp. 1096–7). This conviction wasn't shared by his professional colleagues: "Even now, the zoologist has scarce begun to dream of defining in mathematical language even the simplest organic forms" (p. 2). But in part, he was saying that physics—real physics—is crucially relevant for understanding "Form".

The idealist Goethe had seen different kinds of sap as effecting the growth of sepal or petal, but for him those abstract possibilities had been generated by the divine intelligence self-creatively immanent in Nature. D'Arcy Thompson, by contrast, argued that it is real physical processes, instantiating strictly physical laws, which generate the range of morphological possibilities. Certainly, those laws conform to abstract mathematical relationships—to projective geometry, for example. But biological forms are made possible by underlying material–energetic relations.

Accordingly, D'Arcy Thompson tried to relate morphology to physics, and to the dynamical processes involved in bodily growth. He suggested that very general physical (as opposed to specific chemical or genetic) constraints could interact to make some biological forms possible, or even necessary, while others are impossible.

Had he lived today, D'Arcy Thompson would doubtless have relished the work on the self-organization of feature-detectors described in Chapter 14.vi.b and ix.a. For this explains why we should expect to find systematic neuro-anatomical structure in the brain, as opposed to a random ragbag of individually effective detector cells. Moreover, the "why" isn't a matter of selection pressures, but of spontaneous self-organization. But that recent research required computational concepts and computing power (not to mention anatomical data) that he simply didn't have. He could use only the mathematics and physics available in the early years of the century.

Although D'Arcy Thompson wasn't the first biologist to study bodies, he might be described as the first biologist who took *embodiment* seriously. The physical phenomena he discussed included diffusion, surface forces, elasticity, hydrodynamics, gravity, and many others. And he related these to specific aspects of bodily form.

His chapter 'On Magnitude', for example, argued both that size can be limited by physical forces and that the size of the organism determines which forces will be the most important. Gravity is crucial for mice, men, and mammoths, but the form and behaviour of a water beetle may be conditioned more by surface tension than by gravity. A bacillus can in effect ignore both, being subject rather to Brownian motion and fluid viscosity. Again, his discussion of 'The Forms of Cells' suggested, among many other things, that the shape and function of cilia follow naturally from the physics of their molecular constitution.

(A very recent discovery about axon sizes would have delighted his heart: Faisal *et al.* 2005. The thinner the axons are, the more neurones can be packed into a given volume. But no axons thinner than about 0.1 of a micron have ever been found, even in species with tiny bodies and tinier brains. Computer simulations show that the molecular 'noise' involved in the opening and shutting of sodium/potassium ion channels becomes overwhelming below that point, causing random spontaneous action potentials. In brief, the *biophysics* constrains the minimal diameter of an axon. A copper wire measuring 0.001 of a micron could transmit currents efficiently. But equally tiny axons, if they are to transmit information reliably, are *impossible*.)

Perhaps the best-known chapter of *On Growth and Form*, and the one which had the clearest direct influence, was entitled 'On the Theory of Transformations, or the Comparison of Related Forms'. This employed a set of two-dimensional Cartesian grids to show how differently shaped skulls, limb bones, leaves, and body forms are mathematically related. One form could generate many others, by enlargement, skewing, and rotation.

So, instead of a host of detailed comparisons of individual body parts bearing no theoretical relation with each other, anatomists were now being offered descriptions having some analytical unity.

To be sure, these purely topological transformations couldn't answer questions about more radical alterations in form. The gastrulation (self-invagination) of an embryo, for example, couldn't be explained in this way (see Section iv.a, below). And only very few zoologists—of whom Medawar was one—tried to use D'Arcy Thompson's specific method of analysis. But his discussion inspired modern-day allometrics: the study of the ratios of growth rates of different structures, in embryology and taxonomy.

b. More admiration than influence

One didn't need to be doing allometrics to admire D'Arcy Thompson. By mid-century, he was widely revered as a scientist of exceptional vision (Hutchinson 1948; Le Gros Clark and Medawar 1945). The second edition of *On Growth and Form* was received with excitement in 1942, the first edition (of only 500 copies) having sold out twenty years before. Reprints had been forbidden by D'Arcy Thompson himself, while he worked on the revisions, and second-hand copies had been fetching ten times their original price.

However, only a decade after the long-awaited second edition, the advent of molecular biology turned him overnight into a minority taste. Ironically, much the same had happened to his muse Goethe, whose still-unanswered biological questions simply stopped being asked when Charles Darwin's theory of evolution came off the press in 1859 (see Chapter 2.vi.f). By the end of the 1960s, only a few biologists regarded D'Arcy Thompson as more than a historical curiosity.

One of these was Conrad Waddington (1905–75), a developmental biologist at the University of Edinburgh (whose theory of "epigenesis" influenced Jean Piaget: see Chapter 7.vi.g). Waddington continually questioned the reductionist assumption that molecular biology can—or rather, will—explain the many-levelled self-organization of living creatures. It's hardly surprising, then, that D'Arcy Thompson was often mentioned in his 'invitation only' seminars on theoretical biology, held in the late 1960s at the Rockefeller Foundation's Villa Serbelloni on Lake Como (Waddington 1966–72). (The seminars gathered together a fascinating group of people, including several mentioned in this book: Michael Arbib, Ted Bastin, David Bohm, Brian Goodwin, Christopher Longuet-Higgins, John Maynard Smith, and Howard Pattee.)

But Waddington, too, was a maverick, more admired than believed. His theory of epigenesis couldn't be backed up by convincing empirical evidence, whether in the developing brain or in the embryo as a whole. Only after his death did his ideas gain ground. Significantly, the proceedings of the first A-Life conference were dedicated to him (Langton 1989*a*, p. xiii).

D'Arcy Thompson's most devoted admirers, however, had to concede that it was difficult to turn his vision into robust theoretical reality. Despite his seeding of allometrics, his direct influence on biology was less strong than one might expect, given the excitement (still) experienced on reading his book.

Even subsequent attempts to outline a mathematical biology eschewed his methods. Joseph Woodger's (1929, 1937) axiomatic biology, for instance, owed more to mathematical logic and the positivists' goal of unifying science (see Chapter 9.v.a) than to D'Arcy Thompson. And Turing's mathematical morphology employed numerically precise differential equations, not geometrical transformations—even though *Of Growth and Form* was cited at the end of Turing's paper (see Section iv.a). In short, D'Arcy Thompson figured more as inspirational muse than as purveyor of specific biological theory or fact.

The reason why his influence on other biologists, although "very great", was only "intangible and indirect" (Medawar 1958: 232) is implied by his own summary comment.

At the close of his final chapter, he recalled the intriguing work of a naval engineer who, in 1888, had described the contours and proportions of fish "from the shipbuilder's point of view". He suggested that hydrodynamics must limit the form and structure of swimming creatures. But he admitted that he could give no more than a hint of what this means, in practice. In general, he said:

> Our simple, or simplified, illustrations carry us but a little way, and only half prepare us for much harder things . . . *If the difficulties of description and representation could be overcome*, it is by means of such co-ordinates in space that we should at last obtain an adequate and satisfying picture of the processes of deformation and the directions of growth. (1917/1942: 1090; italics added)

c. Difficulties of description

The "difficulties of description and representation" remained insuperable for more than half a century after the publication of those first 500 copies of D'Arcy Thompson's book.

Glimpses of how they might be overcome arose (in the early 1950s) a few years after his death. Actually overcoming them took even longer. Or perhaps one should rather say it *is taking* even longer. We're now in a position to appreciate the potential of his ideas better than his contemporaries could, and to put more computational flesh onto the bones he sketched so many years ago (Chaplain *et al.* 1999). For this early exercise in mathematical biology resembled current work in A-Life in various ways.

Were D'Arcy Thompson alive today, he'd be very interested in A-Life:

* He'd be fascinated by the exhibition of the virtual swimming fish, with its detailed interplay of hydrodynamics and bodily form.
* He'd be intrigued by the A-Life evolution of decidedly unlifelike behaviour, as a result of a specific mistake in the simulated physics (Sims 1994).
* He'd recognize the potential relevance for morphology of computational work on diffusion gradients (see Section iv),
* and he'd sympathize with those who see evolution as grounded in general principles of order (Section viii).
* He'd appreciate the artificial evolution of a range of networks controlling lamprey swimming (Section vii.d).
* As remarked above, he'd appreciate the recent work on self-organization in the brain (14.vi.b and ix.a).
* He'd agree with A-Lifers who stress the dynamical dialectic between environmental forces and bodily form and behaviour.
* He might have embarked on a *virtual biomimetics*: a systematic exploration of the effects of (simulated) physical principles on (simulated) anatomies.
* And he'd certainly share A-Life's concern with life as it could be—his "theoretically imaginable forms"—rather than life as we know it.

Nevertheless, there are three important, and closely related, differences between D'Arcy Thompson's work and research in A-Life. Each of these reflects his historical situation—specifically, the fact that his work was done before the invention of computers. One difference concerns the practical usefulness of computer technology, and shows why (contrary to the suggestion noted above) A-Life's artefacts are not, in fact, dispensable. The other two concern limitations on the mathematical concepts available when D'Arcy Thompson was writing: in his words, the difficulties of description and representation that needed to be overcome.

First, D'Arcy Thompson was able to consider only broad outlines, largely because he had to calculate the implications of his theories using hand and brain alone. Today, theories with richly detailed implications can be stated and tested with the help of superhuman computational power. The relevant theories concern (for instance) the hydrodynamics of fish; the interactions between various combinations of diffusion gradients; and processes of evolution and co-evolution, occurring over many thousands of generations.

In addition, we can now study chaotic phenomena (which include many aspects of living organisms), where tiny alterations to the initial conditions of a fully deterministic system may have results utterly different from those in the non-altered case. These results can't be predicted by approximation, or by mathematical analysis. The only way to find out what they are is to watch the system—or some computer specification of it—run, and see what happens. In all these cases, the "help" A-Life gets from computers isn't an optional extra, but a practical necessity.

Second, D'Arcy Thompson's theory, though relatively wide in scope, didn't encompass the most general feature of life: self-organization as such. Instead, it considered many specific examples of self-organization. This isn't surprising. Prior to computer science and information theory, no precise language was available in which this could be discussed.

And third, although he did consider deformations produced by physical forces, D'Arcy Thompson focused more on structure than on process. This is characteristic of pre-computational theories in general. Prior to computer science, with its emphasis on the exact results of precisely specified procedures, scientists lacked ways of expressing—still less, of accurately modelling (and tracking)—the details of change.

Uniform physical changes could be described by linear differential equations, to be sure. And Charles Babbage could lay down rules, or programs, determining indefinitely many "miraculous" discontinuities (see Chapter 3.i.b). But much as Babbage (as he admitted) couldn't program the transformation of caterpillar into butterfly, so D'Arcy Thompson's mathematics couldn't describe the morphological changes and dynamical bifurcations that occur in biological development.

One might have expected that cybernetics would provide some of the necessary advances in descriptive ability. Not only was it a form of mathematical biology, but it used computer modelling as a research technique (see Chapter 4.v–vii). As the study of "circular causal systems", it drew on mainstream ideas about metabolism and reflexology, not on the morphological questions that interested D'Arcy Thompson. But it considered some central biological concerns now at the core of A-Life: adaptive self-organization, the close coupling of action and perception, and the autonomy of embodied agents.

It even made some progress. For instance, Ashby's (1952) "design for a brain", and his Homeostat, depicted brain and body as dynamical physical systems (see section xi, below). And Grey Walter's (1950*b*) tortoises, explicitly intended as "an imitation of life", showed that lifelike behavioural control can be generated by a very simple system (see Chapter 4.vii).

However, the cybernetics of the 1950s was hampered by lack of computational power, and by the diversionary rise of symbolic AI. Only much later—and partly because of lessons learned by symbolic AI—could cybernetic ideas be implemented more convincingly. Even so, some recent (dynamical) approaches suffer the limitation remarked in Chapter 4.vi.a with respect to cybernetics: they can't easily represent hierarchical structure, or detailed structural change.

As it turned out, it was physics and computer science—not cybernetics—which, very soon after D'Arcy Thompson's death in 1948, produced mathematical concepts describing the generation of biological form. Indeed, two of the founding fathers of computer science and AI, Turing and von Neumann, were also the two founding fathers of A-Life. (Von Neumann's intellectual range was even greater than Turing's, including chemical engineering for example: Ulam 1958.)

Around mid-century, they each developed accounts of self-organization, showing how simple processes could generate complex systems involving emergent order. They might have done this during D'Arcy Thompson's lifetime, had they not been preoccupied with defence research. While Turing was code breaking at Bletchley Park, von Neumann was in Los Alamos, cooperating in the Manhattan Project to design the atom bomb.

The end of the war freed some of their time for more speculative activities. Both turned to abstract studies of self-organization. Their new theoretical ideas eventually led to a wide-ranging mathematical biology, which could benefit from the increasingly powerful technology that their earlier work had made possible.

15.iv. Turing's Biological Turn

Turing's towering presence in the history of cognitive science is without parallel. He pioneered the theory of computer science and the design of digital computers, and outlined the research programme of AI—including connectionism (Chapters 4.i–ii, 10.i.f, and 12.i.b). In addition, his tongue-in-cheek challenge that came to be known as the Turing Test provoked a huge philosophical response, and still prompts attention today (Chapter 16.ii.c).

What's been less widely recognized, until recently, is his important contribution to mathematical biology—and, indirectly, to neuroscience.

a. A mathematical theory of embryology

The origin of biological form was still hugely mysterious at mid-century. Babbage's suggestion (Chapter 3.i.b) that metamorphosis might be due to some predetermined change in the laws of physics wasn't satisfactory. Scientists wanted to be able to explain the emergence of new forms *without* positing any change in the underlying physics. Better still, they wanted to do this by reference to specific aspects of the known laws of physics. But how was that to be done? Despite D'Arcy Thompson's provocative writings, no one had any clear idea. It was Turing who sketched a preliminary answer.

For the last few years of his life, Turing's energy went primarily into what he called "my mathematical theory of embryology". Indeed, after writing the first Manchester programming manual in 1950, he neglected his duties in the computing laboratory there as a result of his new interest.

His attitude perplexed and irritated his university colleagues, and the engineers and government officials charged with improving and applying the novel technology. For them, this was an exciting time. The world's first commercially available electronic computer (the Ferranti Mark I), which had been largely designed by Turing himself, was delivered to the Manchester laboratory early in 1951 (see Chapter 3.v.b). But he took little interest in it. His concentration was directed instead on his biological ideas. In 1952, when his Manchester colleagues were enjoying their new toy, he published a mathematical paper on morphogenesis (A. M. Turing 1952).

That was only two years before his untimely death: he committed suicide in 1954 (see 4.i.a). He was still working on these ideas when he died, and Max Newman's brief summary written for the Royal Society's memoir appeared in his mother's book (S. S. Turing 1959: 139–44). Drafts of, and extensive notes for, three further mathematical papers on 'The Morphogen Theory of Phyllotaxis' were found in his rooms. (They were partly corrected/prepared by N. E. Hoskin and B. Richards, with notes by Robin Gandy; details are available online in the Turing archive, <http://www.turingarchive.org>, AMT/C/8–10 and 24–7.) Generally, however, when people today refer to "Turing on morphogenesis" they mean his 1952 paper.

In his published account of morphogenesis, Turing followed the path pioneered by D'Arcy Thompson. Indeed, Thompson's book, which he had read before the war, was one of only six references he cited. This paper, in which he discussed the role of fundamental physical processes in the generation of biological form, is now recognized as an early essay in A-Life.

It is also, in principle, an early essay in neural dynamics—and is seen as such by Jack Cowan and Walter Freeman, for instance (see 12.ii.b and 14.x.a–b). The reason is that the crux of the 1952 paper was the problem of how self-organization (pattern development) *in general* is possible. Turing (1947*b*) had already asked how organization could arise in unorganized neural networks, but there he'd assumed some outside interference, or training procedure (12.i.b). Now, he considered the origin of organization without outside interference.

In particular, he asked how homogeneous cells can develop into differentiated tissues, and how these tissues can arrange themselves in regular patterns on a large scale, such as stripes or segments.

Embryologists of the 1950s knew well enough that ordered complexity arises from the undifferentiated egg. But how this is possible was a mystery. They appealed to vaguely conceptualized "morphogenetic fields", controlled by hypothetical, presumably biochemical, forces called "organizers". But what an organizer was, and how it could produce novel structure, they couldn't say.

Turing didn't claim to know the chemical details either. His achievement was to prove, in mathematical terms, that relatively simple chemical processes could, in principle, generate order from homogeneous tissue. The processes concerned involve chemicals whose mutual interactions as they diffuse throughout the system can (sometimes) destroy or build each other.

Since no one knew just which chemicals these might be, Turing referred to them simply as morphogens (from the Greek: form originators). He showed that the interactions between two or more morphogens, each initially distributed uniformly across the system, could eventually produce waves of differing concentrations. This can happen in non-living systems (chemicals diffusing in a bowl, for instance) as well as living creatures. But Turing suggested that in an embryo or developing organism, a succession of these processes might prompt the appearance of ordered structures such as spots, stripes, tentacles, or segments.

This may seem like magic: how can difference arise from homogeneity? Turing allowed that a perfectly homogeneous system in stable equilibrium would never differentiate. But if the equilibrium is unstable, even very slight disturbances could trigger differentiation.

Some disturbances, he pointed out, are inevitable, given that—as D'Arcy Thompson had insisted—living matter is subject to the laws of physics. For example, random Brownian motion within the cell fluids must vary the pairwise interactions between the molecules. And molecules will be slightly deformed as they pass through the cell wall. Even minute disturbances like these could upset the initial (unstable) equilibrium.

Turing gave simple differential equations defining possible interactions between two morphogens. Various terms specified the initial concentrations of the two substances, their rates of diffusion, and the speed at which one could destroy (or build up) the other. He showed that certain numerical values of these terms would result in ordered structures with biological plausibility.

For instance, irregular dappling or spot patterns could result from two morphogens diffusing on a plane surface (see Figure 15.1). Diffusion waves within a twenty-cell ring could give rise to regularly spaced structure, reminiscent of the embryonic beginnings of circular patterns of cilia, tentacles, leaf buds, or petals—or segments, if the ring were broken. (Such a ring is, in effect, a cellular automaton: see Section v.a.)

Turing suggested that order could be generated in three dimensions also. For example, diffusion waves could cause embryonic gastrulation, in which a sphere of homogeneous cells develops a hollow (which eventually becomes a tube). And interactions between

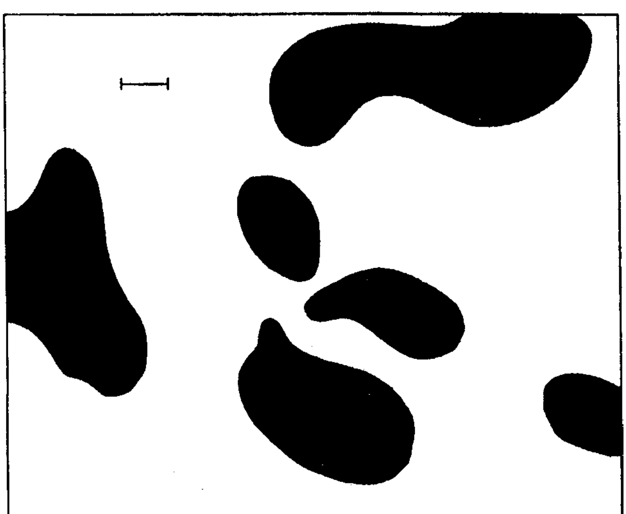


FIG. 15.1. Dappling produced by diffusion equations. Reprinted with permission from A. M. Turing (1952: 60)

more than two morphogens could produce travelling waves, such as might underlie the movements of a spermatozoon's tail.

b. History's verdict

Turing's imaginative paper was read with excitement by embryologists. It proved beyond doubt what D'Arcy Thompson had suggested: that self-organization of a biologically plausible kind could, in principle, result from relatively simple chemical processes. But it didn't, and couldn't, prove that real biological structures actually do emerge in this way. Only experimental developmental biology can show that. (Compare the point made in Chapter 7.iii.c, that only psychological experiments can confirm the psychological reality of a computer model of mind.)

This question couldn't be experimentally addressed at mid-century, because the necessary biochemical techniques weren't yet available. Today, it's still being debated.

For instance, the biologist Maynard Smith (1920–2004), who'd hoped to find that Turing waves underlie the segmentation of the fruit fly *Drosophila*, later admitted that this seems not to be the case: apparently, a different gene controls each segment (personal communication). However, he allowed that Turing waves may be responsible for segmentation in those animals, such as earthworms, in which most of the segments are identical, and/or that they were crucial at an earlier stage of evolution, before specialized segment genes appeared. Moreover, he said, the *numbers* of petals in flowers, for example, may be specified by wavelength ratios of Turing waves (though as yet there's no evidence for this).

Again, the geneticist Gabriel Dover (personal communication) argues that even in earthworms the generation of iterated segments is driven by strictly *local* processes. On his view, it's like adding yet another square at the end of a toilet roll, as opposed to taking a sheet of flimsy paper and spreading waves across it to generate stripes of perforation. And finally, the embryologist Lewis Wolpert (personal communication) denies that Turing waves can explain the development of fingers (an example of repeated structure).

In developmental neuroscience, however, recent work (described in Chapter 14.x.a) suggests that self-organization in the brain—of feature-detectors, for instance—does occur by means of mechanisms like those which Turing described. Cowan, for example, sees "a very close relationship" between Turing's ideas and the brain (J. A. Anderson and Rosenfeld 1998: 118). And this, he says, is no accident:

> The same epigenetic mechanism for pattern formation, the tendency to make stripes and blobs, is ubiquitous in nature. Cloud patterns, animal coat markings, hallucination patterns [in migraine, for example], maps—all that is sitting there. The brain is no different in many respects from any other physical organization. There's a tendency for pattern formation to occur because it's got all the same kinds of machinery in it. (J. A. Anderson and Rosenfeld 1998: 121)

D'Arcy Thompson would surely have agreed.

If Turing's paper didn't immediately affect embryology, nor did it spawn A-Life as a computational discipline. For he faced a methodological obstacle comparable to one facing D'Arcy Thompson before him.

Turing had access to the Manchester University computer, and used it (or, occasionally, a desk calculator) to calculate the successive steps of interaction between the morphogens. But this was a time-consuming and tedious matter. Moreover, in the absence of computer graphics, the computer's numerical results had to be laboriously converted to a (more easily intelligible) visual representation by hand. Turing himself remarked that much better machines would be needed to follow up his ideas.

Now, half a century later, such machines exist. Powerful computers have recently been used to vary the numerical parameters in Turing's own equations, to calculate the results, (often) to apply two or more equations successively, and to convert all these numerical results into graphical form (Turk 1991). In this way, Turing's equations have generated spot patterns, stripes, and reticulations resembling those seen in various living creatures.

Some of the structures produced in these computational experiments are shown in Figures 15.2–15.4. The large and small spots in the upper half of Figure 15.2 result from changing the size parameter in Turing's reaction–diffusion equation. If the large-spot pattern is frozen, and the small-spot equations then run over it, we get the 'cheetah spots' at bottom left. The 'leopard spots' at bottom right are generated by a similar two-step process, except that the numbers representing the concentrations of chemicals in the large spots are altered before the small-spot equation is run.

These cascades of reaction–diffusion systems kicking in at different times (reminiscent of the switching on and off of genes: see Section viii.b) result in more naturalistic patterns than does a simultaneous superposition of the two equations. Similar cascades of a 5-morphogen system generate the lifelike patterns of Figure 15.3. And the addition of rules linking stripe equations to 3D contours produces the moulded zebra stripes of Figure 15.4. This work was done for the purposes of computer graphics, not theoretical biology. But it shows that simple rules can produce lifelike complexity.

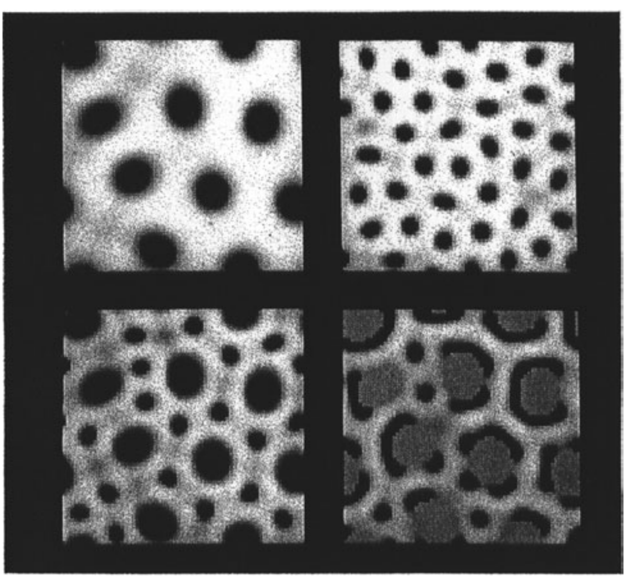


FIG. 15.2. Naturalistic spot patterns. Reprinted with permission from Turk (1991: 292)

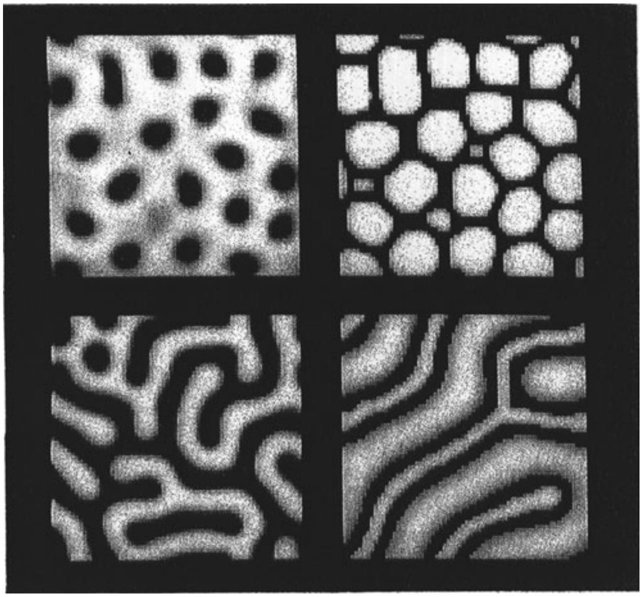


Fig. 15.3. Complex naturalistic patterns. Reprinted with permission from Turk (1991: 292)

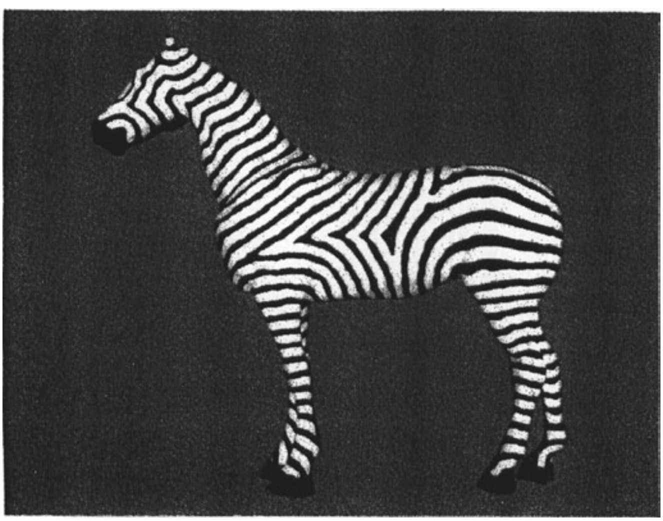


Fig. 15.4. Zebra with 3D-algorithmic stripes. Reprinted with permission from Turk (1991: 293)

Other A-Life work on the physical principles underlying morphogenesis combines computer simulation with biomimetics. For example, chemical experiments (first done by Stéphane Leduc, soon after 1900) show that osmosis can produce lifelike forms, even in inorganic solutions. If pieces of calcium chloride are dropped into a saturated solution of sodium phosphate, the surface film of calcium chloride molecules functions as a 'membrane' separating a growing form (made of calcium chloride plus water) from the surrounding phosphate solution. This happens because osmotic pressure (more water molecules outside than inside) causes water to pass through the surface film, which increases in area as a result.

Computer simulations of osmosis show that, if the rates of production and disintegration of molecules are in balance, it will in general produce persisting (dynamically self-maintaining) topological unities (Zeleny *et al.* 1989). Because these are regarded as possible precursors of autonomous cells bounded by cell walls, this type of research is sometimes called "synthetic biology" (see Section x.b, below).

Some A-Life research has explored the morphological implications of differential cell growth, but without specifying actual physical mechanisms such as diffusion. For instance, the Dutch biologist Aristid Lindenmayer (1925–89) and others have followed up D'Arcy Thompson's ideas by defining various algorithms (known as L-systems) for describing cell growth in plants (Lindenmayer 1968, 1977; Jean 1984).

A Fibonacci number series, for example, may be employed without asking how it's instantiated in chemistry. When L-systems are expressed as computer graphics, a wide range of lifelike plant forms results. These can aid biologists by showing which forms can or can't be generated by certain types of rule, applied in one order or another. (L-systems are also widely used in computer animation.)

The focus on morphogenesis as a result of bottom-up, localized, physical processes is very much in the spirit of Turing, and D'Arcy Thompson too. Now, it's respectable. Early in the century, it wasn't. Indeed, in 1907 the French Académie des sciences refused to publish Leduc's account of osmotic precipitation, because it "touched too closely on the then-discredited notion of spontaneous generation" (Zeleny *et al.* 1989: 126). Self-organization, apparently, was seen by the Académie as either myth or magic.

Finally, exciting work (outlined in Chapter 14.ix.f) has recently begun on using A-Life techniques to evolve computers partly composed of diffusing waves of chemicals. The hope is that logical functions, such as *and-gates*, can be implemented in this way—and that these non-linear machines may be able to do other things which today's digital computers cannot.

So history's verdict on Turing's biological paper is one of approval. The editor of a collection of Turing's work has even stated that this paper has been cited more often "than the rest of his works taken together" (P. T. Saunders 1992).

I suspect that that isn't true, since the Turing Test paper has been cited countless times by philosophers and computer scientists—not to mention members of the general public. But there's no way of knowing for sure, because citation indices don't normally search the bibliographies given in books, often omit non-scientific journals, and never search the 'public' media.

What is true, however, is that by the time that statement was made, many more people were aware of the biological paper than was the case even ten years earlier. I'd noticed this myself. Having greatly admired it when I came across it as a medical student in the mid-1950s, I was surprised to find over the next twenty-five years that knowledge of it was pretty limited even among my scientific colleagues. (As for philosophers, they'd very rarely heard of it, never mind read it.) Now, references to this mid-century paper are ten a penny.

15.v. Self-Replicating Automata

Self-organization, and its biological relevance, interested von Neumann (1903–57) no less than Turing.

His work on this topic was even wider in scope than Turing's. For it included the problems of reproduction and evolution—including the puzzles of how complex a system must be in order to be able to replicate itself, and how a self-replicator could generate descendants more complex than itself. (Turing (1950) had mentioned the possibility of a child machine inheriting features from its parents, but hadn't detailed how this might be possible.)

a. Self-organization as computation

Von Neumann's approach differed from Turing's in treating self-organization in terms of information, not physics. His aim was to define the necessary conditions for self-replication in general: reproduction as it could be, not merely reproduction as we know it.

That "merely", however, may be misleading. The biological mechanism of heredity was still a mystery when von Neumann first described it in informational terms. When the double helix was discovered five years later (in 1953), and the genetic code decrypted some years after that, it turned out that biological reproduction did indeed fit von Neumann's abstract description.

He thought of replication as a computational process, requiring a minimal level of computational complexity. Specifically, he realized that part of the self-replicating system must function both as instructions and as data—namely, as a self-description. Turing had noted in 1936 that one and the same tape state could be interpreted sometimes as data and sometimes as instructions, but he didn't apply this insight to self-replication (see Chapter 4.i.d).

Von Neumann even defined a universal replicator, a computational system capable of reproducing any system, itself included. (Biological organisms aren't universal replicators: cats give birth only to kittens.) He also remarked that errors in copying the self-description could lead to evolution, which might thus be studied computationally.

In other words, "evolution" was being seen as an abstract principle. It could be applied not only to genes and/or species but to computational structures too—and, as eventually became clear, to the immune system and neural development (Chapter 14.ix.d). Formulated by Donald Campbell as BVSR, or Blind Variation and Selective Retention, it was even applied to concepts and cultural practices too (8.v.b). However, all that was in the future. In von Neumann's day, evolution was mostly thought of as strictly biological.

In the 1930s, von Neumann was already sharing his fascination with these topics with the mathematician Stanislaw Ulam (1909–84), who had raised them in coffee-house conversations as early as 1929. And they continued to discuss them after the war, when they were both working at Los Alamos.

Ulam has been described by a colleague as "probably the only close friend von Neumann ever had", and also as "the more original mathematician of the two—but notorious for not doing the detailed work" (Rota 1989). He published almost nothing throughout his life, but had many seminal ideas for which others (who went off and did the detailed work) got the credit.

One of these was a pioneering chess program (for a 6×6 board: no bishops) that took an average of twelve minutes per move, but which spent that time on a comprehensive two-move lookahead (McCorduck 1979: 157). Another earned him the dubious distinction of being joint patent-holder with Edward Teller for a crucial process in the production of the H-bomb—and the same colleague, Gian-Carlo Rota (1989), judges that Ulam's ideas were the more important contribution.

It's entirely possible, then, that Ulam seeded von Neumann's mind with the problem of self-reproduction and/or its outline solution. Certainly, they discussed these matters repeatedly:

> Our usual conversations [in about 1954] were either about mathematics or about his new interest in a theory of automata. These conversations had started in a sporadic and superficial way before the war at a time when such subjects hardly existed. After the war and before his illness [in 1955] we held many discussions on these problems. I proposed to him some of my own ideas about automata consisting of cells in a crystal-like arrangement. (Ulam 1989: 19; cf. Ulam 1962)

However, Ulam went on to say that von Neumann's "more concrete ideas developed [only] after his involvement with electronic machines".

In the 1930s, von Neumann couldn't formulate the problems clearly—still less, solve them. In 1943, however, he read McCulloch and Walter Pitts' "neural logic" paper and was introduced to computing on a visit to England (see Chapters 4.iv.a and 3.v.e, respectively). Thus inspired by the likely biological relevance of Turing machines, he claimed in 1946 to have formulated the problem of self-replication rigorously.

Two years later, he had the outlines of a solution—and of a general theory of automata, which he took to include both natural and artificial systems. That theory underlies much of what is now called A-Life.

He presented his outline in a talk at the Hixon symposium in 1948. This was the same interdisciplinary meeting at which Karl Lashley described his ideas on serial order in behaviour (Chapter 5.iv.a), and it was largely thanks to Lashley's urging that von Neumann later wrote up his talk for publication (von Neumann 1951). The Hixon paper sketched the general form of a self-reproductive system, and pointed out the possibility of heritable copy errors, or "mutations". It also described an (imaginary) mechanical system capable of self-assembly from physical parts.

This "kinematic" model caught some people's imagination, inspiring a simple mechanical model of self-replication that used interlocking tiles (L. S. Penrose 1959) and speculations by NASA scientists about robotic self-assembly in space (Levy 1992: 32–42). Von Neumann himself, however, was dissatisfied with it. It was limited to a specific type of physical system, and it left crucial processes unexplained (for example, recognizing, picking up, and welding the parts): "By axiomatising automata in this manner, one has thrown half of the problem out of the window, and it may be the more important half" (quoted in Burks 1970, p. xv).

He therefore turned (at Ulam's suggestion) to study systems defined not by physics, but by logic. Because Ulam had compared them to crystal growth in two dimensions, von Neumann called them crystalline regularities, or tessellations (tilings). Now, they are known as cellular automata, or CAs.

A CA is a computational "space" made up of many discrete cells, each of which can be in one of several states, and each of which changes (or retains) its state according to specific rules. More strictly: a CA is a space plus a specification of an initial space state. The same space may give very different results, given different initial states. CAs are artificial constructs, products of the logical imagination. But (as von Neumann foresaw) they can be used to simulate natural systems, if these involve equivalent principles of change.

Before the initial state can be specified, randomly or otherwise, the CA space itself must be defined. This requires decisions on many different options. The person defining the space must specify the number of dimensions; the size (sometimes infinite) of the space; the set of possible states per cell; the 'shape' (boundaries) and 'neighbours' of each cell; the timing (synchronous or asynchronous) of the cells' state changes; and the rules governing those state changes.

These rules are expressed in logical terms. For instance, a cell in state 6 (out of 12 possible states) must change to state 8 if and only if at least two of its neighbours are in state 10, and/or at least one is in state 3. The rules may apply universally across the space, or they may (in effect) vary according to region. And they can be of various general types. For instance, they may be fixed or mutable (changing systematically with time and/or subject to random mutation). And they may be deterministic, indeterministic, or probabilistic. On top of all this, a CA may be 'pure', or it may be subject to a certain amount of noise, or error.

Clearly, the potential variety of CAs is vast. Among the myriad possibilities are some systems with immense computational power. Indeed, as von Neumann suggested (but couldn't prove), his universal constructor is equivalent to a universal Turing machine, which can in principle perform any possible computation (Chapter 4.i.c).

Correlatively, CAs can in principle be used to simulate any natural system governed by local laws (as opposed to action at a distance), and having no essential discontinuities that can't be approximated discretely (as in the differential calculus). In short, CAs can be used to model dynamical systems describable by the laws of physics.

As usual, practicalities fall short of possibilities. For one thing, a natural system that could in principle be simulated by a CA may be so complex that such a simulation can never be found. Even if the CA rules can be specified, the initial state cannot. Putting this point in David Marr's terminology (Chapter 7.iii.b), many natural phenomena may be explicable only by Type 2 theories, which are extremely difficult to find. Von Neumann himself, for example, suggested that:

> [It] is futile to look for a precise logical concept, that is, for a precise verbal description, of "visual analogy". It is possible that the connection pattern of the visual brain itself is the simplest logical expression of this principle. (von Neumann 1951)

For another thing, most CAs that have actually been studied are relatively simple—although "simple", here, is a relative term. In practice, CAs normally have only a few dimensions: a 1D line, a 2D grid (as in von Neumann's case), or a 3D volume. Even CAs modelling hyper-dimensional fitness landscapes (see Section viii.b), or the networks called CTRNs (Section xi.b), don't approach the number of possible dimensions. (Whether they're large enough, nevertheless, to represent all the relevant evolutionary factors is an empirical question.)

Many CAs have an initial state defined by only a few cells. Von Neumann's universal replicator was an exception. It had a "body" (the central construction unit) of 32,000 cells, plus a "tail" (the instruction tape) 150,000 cells long.

Similarly, many spaces assume a strictly limited number of neighbours per cell, each located immediately adjacent to the cell. For von Neumann, each cell had four neighbours: those sharing square boundaries with it. For John Conway (1937–), whose three-rule game of Life was the first A-Life entertainment to hit the popular scientific press (M. Gardner 1970), cells have eight neighbours: the four boundary-sharers, plus the cells touching the four diagonals. (Conway's Life is not to be sneezed at: in principle, it can generate a Universal Turing Machine.) However, some CAs allow *any* cell in the space to count as a neighbour, thus allowing for large-scale parallel processing (see Section vi.a).

In practice, too, the state-change rules also are relatively simple. Most CA rules are both deterministic and unvarying. The rules are usually applied across all regions of the space, each cell deciding its next state according to the same rule set. (Again, some recent work discussed below is an exception.) As for the number of possible states per cell, von Neumann specified twenty-nine when defining his universal replicator (these were based on the logical functions used in his design for the EDVAC computer). But many CAs, Life included, use only two.

Finally, most CAs are defined with respect to distinct time steps. That is, all the cells change (or not) simultaneously. Asynchronous CAs, in which the rules may be identical but the cells have independent "clocks", can produce very different behaviour (Ingerson and Buvel 1984; Bersini and Detours 1994).

Not all CAs can self-replicate, for many lack the necessary complexity. But self-replication isn't the only lifelike property that makes CAs interesting. Even highly restricted systems can generate unpredictable order, which sometimes emerges only after hundreds of iterations. In general, the bottom-up (cell-by-cell) parallel processing characteristic of CAs can give rise to higher-level order which the GOFAI approach outlined in Chapter 10 would have tried to impose top-down.

For example, the fascination of Life is its ability to generate unexpected, and sometimes systematically repeated, 2D forms such as "gliders". A simple 1D system (a single line of cells, generating another line below it . . . and so on) may produce lifelike patterns reminiscent of the markings on cone shells (Wolfram 1984*a*). A set of three simple rules can enable computer-animated creatures (bats, dinosaurs, or milk bottles) to "flock" realistically (Reynolds 1987). And some recent A-Life models show complexities of co-evolutionary behaviour that are still more intriguing (see Section vi.c).

b. Why the delay?

Given the intellectual power of von Neumann's pioneering research, why wasn't it followed up immediately? D'Arcy Thompson's vision of a systematic mathematical biology had been reinforced. But where was the rush to turn vision into reality?

A few unorthodox biologists saw the point—McCulloch, of course, included. Automata theory was discussed at Waddington's ground-breaking meetings on theoretical biology (where D'Arcy Thompson was explicitly remembered). At one of these, the young Arbib presented a pencil-and-paper definition of a self-replicating program

(Arbib 1966, 1969). At another, Longuet-Higgins presented his ideas about the mathematics of associative memory (see Chapter 12.iv.c). And Gordon Pask, ahead of the pack as usual, experimented with simple evolutionary machines—not programmed as abstract cellular automata, but implemented as solutions of chemical salts (Pask 1961: 105–8).

But such experiments couldn't then be taken much further, and most computer scientists remained unimpressed. Almost all mid-century computer modellers focused their attention on other forms of cybernetics, or on GOFAI, or on simple neural networks. A-Life wasn't identified as a scientific endeavour until many years later (see Section x.a). It was later still that "chemical" computing, broadly anticipated by Pask, became a hot topic (Adamatzky *et al.* 2003; Sienko *et al.* 2003).

There are three reasons for this. One is the lack of computer power at the time. Von Neumann's A-Life research, like Turing's (and D'Arcy Thompson's), was a pencil-and-paper exercise, an abstract study in computational logic. He couldn't explore its implications by computer modelling, because the necessary technological advances were still far in the future. Accordingly, the first people to take up his ideas were logicians or mathematicians.

Even they were—for the second reason—very few in number. Von Neumann's CA research didn't immediately excite widespread attention because most of it remained unpublished until well after his death (he died of cancer early in 1957). A few brief hints were included in his (unfinished) lectures on *The Computer and the Brain*, published soon after he died (von Neumann 1958). And an account of the main ideas (written by the originator of BASIC) appeared in *Scientific American* (Kemeny 1955). However, this paper on 'Man Viewed as a Machine' contained very little detail. And Ulam, who had suggested CAs to von Neumann in the first place, published a relevant paper in the early 1960s (Ulam 1962)—but this, too, was very sketchy as compared with von Neumann's still-unpublished writings.

Von Neumann's major manuscript on automata, begun in 1952, was never finished. In 1949, a year after his Hixon talk, he lectured on 'The Theory and Organization of Complicated Automata' to an audience of mathematicians and computer scientists at the University of Illinois. There, he discussed various general types of automata. These included not only CAs, but also continuous systems—such as might be used to model the physical diffusion processes discussed by Turing. Others were quasi-neurological automata inspired by McCulloch and Pitts, involving thresholds, excitation/inhibition, and refractory periods, or fatigue. (He also revised his mid-1940s estimate of the number of individual units needed for self-replication, from the high 10,000s to about a million.) But these lectures, too, remained unpublished at the time.

The person most responsible for disseminating—and, initially, for developing—von Neumann's work on self-organization after he died was the philosopher and logician Arthur Burks (1915–). He did many years of editorial work to bring von Neumann's papers to public view. He circulated mimeographed notes of the 1949 Illinois lectures to those who asked for them, and in 1961 tried to interest behavioural scientists in general (Burks 1961). But von Neumann's own manuscripts weren't made widely available until a decade after his death (Burks 1966, 1970).

Burks was an ex-colleague of von Neumann's, having worked with him since the mid-1940s. They had cooperated on the design of the EDVAC, the ENIAC, and the Princeton computer (and jointly originated flow diagrams as a tool for program design: Aspray 1990). And, although he never discussed CAs personally with von Neumann (Levy 1992: 60), Burks had been thinking along similar lines for a long time. Indeed, it was he who coined the term "cellular automata".

His 'Logic of Computers' group at the University of Michigan was the first place where CAs were actually run on computers. Some of these runs were simulations of physical or biological processes (such as heart fibrillation), while others aimed to advance the theory of CAs in general. He developed, and even corrected, some of von Neumann's ideas while editing the manuscripts. For example, he proved his suggestion that a universal constructor, equivalent to a universal Turing machine, is possible; and he improved on his ("double-path") design for constructing instruction loops of arbitrary length within the CA space. In short, his intellectual contribution to the field was considerably more than the term "editor" normally implies.

Burks tried to interest the AI community in von Neumann's work, but he came up against the third reason why it was neglected. The fundamental difference in approach—bottom-up rather than top-down, and largely abstract to boot—was a formidable barrier.

Add the fact that MIT and Stanford (and later Yale) were recognized, not least by their own inhabitants, as the main centres of AI, and Michigan stood very little chance. It probably didn't help, in meetings dominated by ex-physicists and AI hackers, that Burks described himself as a philosopher. I remember attending several AI meetings in the early 1970s where he made an effort to talk about CAs and von Neumann to others, including people he hadn't met before. But it was obvious, and quite distressing in human terms, that he wasn't being taken seriously.

Inside Michigan, it was a different matter. Burks's students were introduced to von Neumann's ideas some years before they were published. They, too, helped to advance the field (Burks 1970).

In the early 1960s, one (James Thatcher) completed an alternative design for a universal constructor. Another, Edgar Codd (who would pioneer relational databases in the late 1970s), showed that a universal replicator could be axiomatized with only eight cell states instead of von Neumann's twenty-nine (Codd 1968, ch. 4). He did this by modelling candidate CAs on computers, iteratively redefining them when the computer tests failed—a methodology foreseen by von Neumann, but not available to him.

Later, another Michigan student, Richard Laing (1977), showed that a self-replicating machine needn't be 'magically' provided with a self-description, but could build one by inspecting itself. (It was Laing who'd been so critical of Mortimer Taube's anti-AI *Computers and Common Sense* in the early 1960s, and who'd been simulating large neural networks at that time too: see Chapter 11.ii.b and Richard Laing 1961*a*.) And later still, Langton (1984)—an ex-student of Burks's—would produce the first functioning implementation of a self-replicating automaton (see Section ix.a).

15.vi. Evolution Enters the Field

The most important work done in Burks's department was John Holland's (1929–). In his early twenties, Holland (1929–) had co-authored a model of Donald Hebb's theory of self-organizing cell assemblies (Rochester *et al.* 1956), which discovered unexpected difficulties in the theory (see Chapters 5.iv.b and 12.ii.d). But his prime inspiration came from von Neumann, not Hebb.

He turned towards automata theory as a very young man. Partly because von Neumann himself wasn't publishing on the topic, Holland's 'Survey' (1959) and two other general papers were cited in Minsky's bibliography of AI (Feigenbaum and Feldman 1963: 494). But Holland was much more than a competent surveyor, for he pushed the field forward in a very important way. Only a few years after von Neumann's death, he defined a new type of CA that answered the master's question of how an automaton could give rise to something more complex than itself (J. H. Holland 1962).

He did this by following up von Neumann's hint that a CA could support evolution, if it had an imperfect copying mechanism and some principle of selection. In other words, Holland was using biological ideas to extend CAs, rather than CAs to study biology.

His subsequent research elaborated these ideas. Eventually, they became highly influential. But this outcome was long delayed. The importance of Holland's approach wasn't widely recognized for over twenty years.

a. Holland, and mini-trips elsewhere

Holland's new systems, which he called iterative circuit computers, were more general than von Neumann's CAs. Not only did they allow neighbourhood relations and transition functions to change over time, but they weren't strictly localistic. Holland allowed for direct communication between *any* two cells (all cells could count as "neighbours"), so that his systems performed essentially parallel processing within a huge, and very noisy, space.

This meant that

* programs (of any length) stored in contiguous cells could be moved (or copied) to another set of cells in one step.
* In addition, they could be combined, by being located in adjacent cell sets.
* Or they could be split into two parts—
* which in turn could be combined with other programs or program parts.
* Specific types of noise, or copy error, were allowed to produce novel combinations of program parts.
* Moreover, given a measure of program efficiency, the best of the new combinations could be selected so as to generate programs better adapted to their task.

The analogy to biological genetics and evolution was intentional. Holland had first experienced self-improving computer programs in the late 1940s, when he worked with Arthur Samuel and John McCarthy on a neural-net simulation of IBM's first commercial calculator. Samuel, who "devoted his spare time to the subject of machine learning" (Samuel 1959), would soon (in 1952) complete a GOFAI checkers-player that learnt to play better than he did himself (see Chapter 10.i.e). The neural network he developed with Holland was much less impressive, but it did show a crude form of learning.

Holland's subsequent research benefited also from his Michigan-based familiarity with von Neumann's ideas. In particular, he was inspired by von Neumann's hints about evolution in automata.

By the early 1960s, Holland was already thinking in terms of concepts borrowed from biological genetics: crossover, linkage, and dominance (J. H. Holland 1962). By the late 1960s, he had developed a comprehensive logical theory of adaptive systems, and had formally defined the first genetic algorithm (GA) (J. H. Holland 1975). GAs enable a program to improve not by top-down hand coding, but by bottom-up evolution. (The program may or may not be capable also of individual learning.) They involve parallel search over a large number of candidates at each generation.

Even before Holland showed how these effects could be produced, several researchers (independently) had already raised the possibility of computerized—as opposed to mechanical (see L. S. Penrose 1959)—evolution (D. B. Fogel 1998). Most of them were inspired by Ashby and Grey Walter rather than von Neumann, whose work on CAs wasn't yet widely known (see Section v.b).

Perhaps the first was George Friedman at UCLA, who in the mid-1950s outlined how a "selective feedback computer" might evolve electrical circuits to maintain the internal temperature of a mobile robot (Friedman 1956). This goal was more taxing than it sounds. The robot's environment included not only hotter and colder areas, but areas where the temperature changed suddenly or slowly, and areas of very high temperature signalled by a flash of light (one of which changed position unpredictably). The parameters of the robot's circuits could be altered by a switchbox, and the most successful circuits at a given stage were selected and then randomly varied by instructions from the "Control".

At much the same time, Hans Bremermann (then at the University of Washington, Seattle, but later at Berkeley) began a series of theoretical studies of the evolution of "goal-seeking, self-organizing" systems (Bremermann 1962; Bremermann *et al.* 1966). He tried to analyse a range of evolutionary systems, showing how changing the variables would lead to different types of result. And in the early 1960s, Ingo Rechenberg in Germany actually evolved engineering designs based on wind-tunnel experiments (Rechenberg 1964).

These efforts had come out of the cybernetic stable, being focused on Shannon-type predictive systems. But others were influenced also by the nascent research on AI planning (Chapter 10.i.d).

For instance, the engineer Lawrence Fogel proposed the development of "goal-seeking" AI, in which artificial evolution looked after the tactics while artificial intelligence set the purposive strategy (L. J. Fogel *et al.* 1966). In the late 1960s, he joined with some central figures of the cybernetics–AI community—including Minsky, Allen Newell, McCarthy, and the ubiquitous McCulloch—in exploring the potential for evolving program-controlled sensory/motor prostheses for human patients and air pilots (Fogel and McCulloch 1970: 271–95). These instruments, he suggested, would autonomously adapt to their "experience" yet also be controllable by the human user. The user could thus concentrate on the higher-level goals, leaving the detailed execution—and eventually the setting of low-level sub-goals—to the machine.

This work, whether implemented or merely speculative, was less sophisticated—though more visible—than Holland's. Large-scale parallelism wasn't available; indeed, the current generation was sometimes restricted to only two programs or genotypes. And it relied on mutation rather than crossover (but see below). In other words, although it was termed "evolutionary programming", it didn't follow the biological analogy so closely as Holland's "genetic" method did. (The term "GAs" is sometimes used to cover all types of evolutionary computing, but is sometimes restricted to methods similar in spirit to Holland's.)

The early evolutionary programming didn't sweep the field. Besides its lack of power, it was sometimes associated with extravagant claims that invited hostility from other AI modellers. The proposal for evolutionary prosthetics, for instance, aroused scepticism from some discussants.

Thus Minsky commented: "The question is not, 'Is tree search better?,' because this is a tree search. The question is, 'Is the language of mutation and offspring as good as the language of subgoal, and so forth?' " (L. J. Fogel and McCulloch 1970: 284). His own answer (pp. 287, 288) appeared to be "No", since he doubted whether Fogel's evolutionary technique was an advance on Albert Uttley's conditional probability machine (Chapter 12.ii.c). Doubts were expressed also by McCarthy and Papert, both of whom saw random variation as a much less promising way of improving programs than the more 'rational' AI approach (pp. 292–4).

Nevertheless, AI work on evolutionary computing continued in a few places outside Michigan, from the early 1960s onwards. And some of this exemplified what would now be termed A-Life, since it was directed to biological concerns.

The Stanford biophysicist–philosopher Pattee (1926–), another regular Waddington invitee, had already applied automata theory to the origin of life (Pattee 1966). Now, he simulated evolution in an entire ecosystem (Conrad and Pattee 1970). But his model produced no significant results. Years later, he commented:

> [The] populational behavior of the biota appeared to be chaotic, although we did not know the significance of chaos at that time. However, in spite of biotic behavior that was unendingly novel in the chaotic sense, it was also clear that the environment was too simple to produce interesting emergent behavior. (Pattee 1989: 71)

Holland's (then unrecognized) achievement had been to show how "interesting emergent behaviour" could indeed be generated.

A GA program makes random changes (copy errors) in a specific batch of code (equivalent to the genotype), evaluates the results, and selects suitable candidates for breeding the next generation. As Holland pointed out, the "suitable" programs aren't always the "best": occasionally, his GA would select a low-performance program for breeding. This probabilistic approach enabled potentially useful program parts hidden within even highly inefficient programs to be (sometimes) preserved in the gene pool.

Von Neumann hadn't specified just which sorts of random copy error would be most evolutionarily fruitful. He appeared to assume that point mutations, wherein a genetic unit is substituted by another, suffice in an asexual population. By contrast, Holland stressed crossover or recombination between a pair of parents in a sexual population, wherein a set of adjacent units on one string—compare: adjacent genes on a chromosome—is swapped for a set of adjacent units on another string. Indeed, he allowed for multiple crossovers, wherein two or three gene sets are exchanged between the two strings.

Holland pointed out that crossover (as defined above) enables useful building blocks to be preserved during self-replication. A point mutation can affect any gene, anywhere. But this type of crossover is unlikely to make changes within small sets of adjacent units, because there are fewer potential breakage points within a short string than within a long one. Moreover, it can build up high-level blocks consisting of two or more lower-level blocks, and these hierarchical structures also can be preserved throughout evolution.

(It turned out later that Holland's restrictive definition of crossover was unnecessary. 'Uniform crossover'—wherein a coin is tossed at each locus in the paired gene strings, and either mother or father is chosen at random—makes widespread changes, yet can drive successful evolution: I. Harvey, personal communication.)

As Holland realized, however, strict adjacency is both improbable and unnecessary. It's unlikely that potentially cooperative lower-level blocks will always lie together. Even if they start out that way, some future crossover may separate them. For that matter, even a single block may contain functionally irrelevant units lying between the relevant ones. And the statistics are borne out by the biology: adaptively co-functional (sets of) genes needn't lie next to one another on the chromosome.

Accordingly, he specified building blocks (sub-strings within a program) as flexible "schemata" rather than fully determinate strings. And he defined a new type of GA to deal with them.

He represented schemata as fixed-length strings of "0s", "1s", and asterisks—where an asterisk means "it doesn't matter whether this unit is a 0 or a 1". For example, the schema "0010****" can be instantiated by any 8-unit string starting with 0010 (00101111, 00100000, 00101010, and so on); the schema "**0010**" is instantiated by 00001000, 11001010, 10001001, and so on. Holland's program could identify any string in which the relevant building block appears, and could manipulate sequences or hierarchies of building blocks while ignoring insertions of 'irrelevant' units. That was just as well, since such insertions would often be produced by the GA's own crossover activities. In short, his GA was efficient and highly robust.

Moreover, Holland proved the "Schema Theorem": that schemata, which enabled cooperation between different building blocks, would increase their representation within the population exponentially with time. Much later, it became clear that he should have stressed that they would do this *ceteris paribus*. This get-out clause is important, because as soon as schemata do start increasing, their relative fitness will change.

In other words, the conditions under which the Schema Theorem holds break down almost immediately. Partly for this reason, many now consider that the Schema Theorem didn't give a good picture of what was going on in Holland's models. Nevertheless, it was influential for a time, since it appeared to offer a mathematical proof of their evolutionary efficiency.

By the mid-1980s, Holland had found a way of making GAs even more powerful—and he'd applied them to classification, inductive reasoning, and philosophy of science (J. H. Holland *et al.* 1986). In artificial evolution, unlike the biological case, it's in principle possible to protect the most useful building blocks from being dropped, or damaged, during replication. But how is this to be done?

Given that many different strings (rules) are being searched in parallel, and that several contributory rules are acting together in performing the task, it's no trivial matter to discover which are especially useful. Holland devised an ingenious method for automatically advantaging the most adaptive rules within a complex evolved program. This was the bucket-brigade algorithm, whereby appropriate fractions of the overall "credit" are passed back to individual rules (J. H. Holland *et al.* 1986: 70–3).

b. Awaiting the computers

One might ask the same question about Holland's research as about von Neumann's. Why didn't it receive immediate recognition? Why have GAs been widely used in AI and A-Life only since the 1980s, given that they were first defined in the 1960s?—The answers are very similar.

Holland's interests in cellular automata, like von Neumann's, were more theoretical than technological. In his early work he did discuss the advantages and disadvantages of actually building machines as iterative circuit computers (Burks 1970). But he didn't implement his GA after he'd defined it. That was done by his student David Goldberg in 1983, to solve problems about controlling the flow of gas in pipelines (Goldberg 1987). (Other Michigan students had already used the ideas in theoretical studies—of adaptive game playing, for instance: Bagley 1967.) In any case, the computational power available before the 1980s hardly sufficed for interesting GA research.

Again, Holland—unlike some of his intellectual descendants—didn't initially seek to publicize his work, still less to turn it into the latest AI 'cult'. Although he published papers in the 1960s, his book on the formal theory of adaptive systems didn't appear until 1975.

Even that wasn't widely read. I recall being hugely impressed by the paper he gave at a twenty-person weekend meeting held on the edge of Dartmoor in 1981 (cf. Selfridge *et al.* 1984). I'd never heard of him, and when I got home from the Devon countryside I asked my AI colleagues in Sussex why they weren't shouting his name to the rooftops. Some replied that his work wasn't usable—and some had never heard of him either.

Finally, Holland's approach, like von Neumann's, was fundamentally at odds with the then dominant culture in AI. Burks had failed to persuade GOFAI researchers of the potential of CAs and bottom-up processing, but at least he'd focused on deterministic automata. Holland's programs, by contrast, relied crucially on rampant (and richly parallel) random change. It's perhaps not surprising that highly skilled GOFAI programmers, well aware that to omit a single LISP bracket was to crash their system, failed to see the intellectual promise of his work. (It probably didn't help that he was based in Michigan: theoretical linguistics wasn't the only discipline to exclude those outside the magic circle—see Chapter 9.viii.a.)

But that's not to say that mainstream AI was ignoring the possibility of evolving programs. As remarked above, such efforts had started in the early 1960s. Twenty years later, a number of people were using some sort of evolutionary method—variation plus selection—to evolve GOFAI programs.

John Koza (1943–) was a leading example (Koza 1989, 1992*a*). His technique enabled programs of increasing length and complexity to be generated, because larger program parts could be substituted for (and nested within) smaller ones. Besides applying this approach for optimizing AI problem solving, Koza used it to produce programs acting as sensori-motor controllers for simulated robots, which evolved the capacity to follow irregularly shaped "walls" (Koza 1992*b*).

However, Koza's method was less richly parallel than Holland's, and also less 'free'. Instead of allowing changes at any point in the code, it was constrained to swapping syntactically sensible LISP expressions. Koza therefore avoided the difficulty, which Holland had faced, of deciding how to identify and preserve useful mini-sequences—including 'interrupted' sequences containing blocks of non-relevant material.

Similar remarks apply to Karl Sims's (1962–) *interactively* evolved image-generating programs (developed on the Connection Machine at MIT's Media Lab: see 13.v.a), in which the selection is done by a human (Sims 1991). Although some variations can affect the very heart of the LISP code, the set of genetic operators ensures that only 'sensible' programs result.

Gradually, interest in evolutionary computation increased. By the mid-1980s, it had grown enough to support the first international conference on genetic algorithms (Grefenstette 1985). And this interest grew apace. A few years later, simple evolutionary programs such as Dawkins's Biomorph had even entered the home, influencing children's views on the concept of life (Dawkins 1986: 51–74; Turkle 1995).

By the late 1980s, AI work on evolutionary programming had burgeoned. Holland's ideas were better known, and huge computational power was becoming available. A number of interesting implementations of GAs—some directly inspired by Holland's work, some not—were appearing.

One was produced by Daniel Hillis (1956–) at MIT (Hillis 1992). To do so, he took advantage of the enormously powerful Connection Machine that he'd designed in the early 1980s, having been greatly influenced by Minsky's ideas on parallelism (12.iii.d). The first, quarter-sized, version had been bought by MIT's Media Lab in 1986, but the full-sized version was available soon afterwards. This had no fewer than 65,536 processors, with a maximum of sixteen 'steps' for any one to connect with any other (Hillis 1985).

Using that machine, Hillis was able to study populations of up to a million individuals, with up to 256 "chromosomes" per individual. For illustrative purposes, Hillis used his GAs to optimize sorting networks. Sorting algorithms put some set of elements into some rational order. For instance, they arrange a random set of numbers into an increasing series. In general, they are easy to test: their "fitness" can be readily assessed. Accordingly, Hillis was able to show a clear improvement as a result of applying GAs.

Other AI workers had used GAs to optimize computer programs. Hillis deliberately made the task—one might think—more difficult, by introducing "parasites", or "predators". These were sets of up to twenty test cases, which evolved to become more and more difficult as the sorting algorithms improved. Hillis found, as he expected (and as Darwinian theory predicted), that the parasites would make the improvement more rapid than it had been without them.

The reason is that competitors can dislodge a population from a local peak in its fitness landscape (see Section viii.b), so that it has the chance of finding a higher peak elsewhere. As Hillis remarked, this is similar in principle to the technique of simulated annealing used in connectionism to move a system away from a local minimum (Chapter 12.vi.b).

Whereas Hillis wasn't primarily interested in the biological relevance of co-evolution, others were. In the mid-1980s, people began to use GAs as experimental test beds for theoretical biology. The phenomena they studied would include parasitism, symbiosis, predator–prey behaviour, the evolution of sensori-motor anatomy, evolutionary change in fitness criteria, and the speeding-up of evolution by the introduction of competitors.

In some cases, mutation rates and population size were systematically varied, to investigate their effect on the pattern of evolution. Simulated species were tracked for thousands of generations, to see what types of order emerged, and why. In general, such GA work showed that very simple rules can give rise to richly ordered, and often unexpected, behaviour.

It was found, for instance, that the "punctuated" evolution posited by some biologists (Eldredge and Gould 1972; Gould and Eldredge 1993) can emerge in computer models—and can be explained in precise, and purely Darwinian, terms. Background mutations (ones not causing a change in the phenotype) would sometimes accumulate over many hundreds, even thousands, of generations—only to cause a sudden change in the phenotype when a few 'last-minute' mutations occurred and interacted with them. In short, hidden gradualism was producing observable saltations.

Early research of this type was done in the mid-1980s by Thomas Ray (1954–) at the University of Delaware. Ray was a botanist, specializing in the ecology of tropical rainforests (Ray 1992, 1994). Accordingly, his Tierra system was a virtual world of co-evolving "species", which competed and cooperated in ways broadly similar to biological species.

Ray's individual "creatures" were strings of self-replicating computer code, competing for space in computer memory. Some strings evolved the ability to borrow the self-replication facilities of other strings; and some host strings became resistant to these parasites, by evolving parts that prevented such attachments. After many generations, a variety of species, including parasites, hyper-parasites (outwitting the host's evolved resistance), and symbionts, usually emerged. Ray's work would eventually spawn a still-continuing worldwide experiment in computational ecology, in which virtual creatures are free to evolve in a "Digital Reserve" formed by pooling the idle time of computers linked by the Internet (Ray 1996; Ray and Hart 1998). (Ray himself is convinced that virtual creatures such as these can be genuinely alive: see Section ix.b, and Chapter 16.x.)

c. The saga of SAGA

GAs were developed in a new direction in the early 1990s, by Inman Harvey (1946–) at the University of Sussex (I. Harvey 1992*a*). Harvey extended GAs to cover open-ended, potentially indefinite evolution of increasingly complex artefacts, with genotypes of potentially unlimited length. ("Artefacts", because he didn't claim that his GA models were detailed theories of real life.)

Harvey's main aim (personal communication) was to discover how to set optimal mutation rates, given genotype spaces of different kinds. For example, some might involve "ridges" or "neutral" (*sic*) networks. In addition, he wanted to work with genomes that could vary in length.

From his point of view, Holland's approach had two drawbacks. First, Holland had considered only fixed-length genotypes, and his Schema Theorem guaranteed increasing fitness only for these. Second, he'd implicitly assumed that there was typically a large amount of genetic diversity within the population being evolved. Harvey argued that both these assumptions are inaccurate for natural evolution, and needn't apply to artificial evolution.

The use of fixed-length genotypes wasn't compatible with open-ended evolution (see subsection d, below). The search space of a fixed-length genome is finite—perhaps astronomical, but finite. Radically novel forms simply can't arise. Moreover, biological organisms that are more complex tend to have more genes. The reason is that a longer genome allows for more complex gene–gene interactions. It follows, said Harvey, that open-ended artificial evolution, too, requires variable-length genomes.

In relying on fixed-length genomes, Holland had supposed that the finite search space included some (relatively) optimal solution, which would eventually be found. For specific problems, this assumption is justified—in principle, if not always in practice.

But, as Harvey pointed out, biological evolution isn't like that. Species don't evolve in order to solve pre-given problems—although we sometimes speak as if they do, saying for example that blinking evolved "to protect the eye". Indeed, new fitness criteria arise as a result of evolution: no eyes, no 'need' for blinking. In other words, new 'problems' and new types of 'relevance' are spontaneously created by evolution.

This point had been implicit in Pask's early models of the emergence of new types of sensor: crystalline threads sensitive to auditory pitch or magnetic fields, for instance (see Chapter 4.v.e). But Pask hadn't specifically related it to evolutionary models, whereas Harvey did.

So, too, did Peter Cariani (1956–), a philosopher of A-Life in Pattee's group at SUNY Binghamton (and also an auditory neurophysiologist, now at the Massachusetts Eye and Ear Infirmary), and Peter Kugler and M. T. Turvey of the University of Connecticut's Ecological Psychology group (Cariani 1992; Kugler and Turvey 1987; Kugler *et al.* 1990). All of them insisted that biological evolution is open-ended—and, in general, leads to increasingly complex structures.

The second problematic assumption noted by Harvey concerned genetic diversity. Holland's Schema Theorem had implicitly presupposed wide genetic variation in the artificial population, but this doesn't apply to natural populations. The individuals within a biological species have very similar genotypes. In other words, the population is relatively *converged* within genotype space.

Further, Harvey argued, even if one were to begin with a highly diverse population, it would rapidly converge so that individual genotypes became more similar. The population genetics would then have settled down into a dynamic equilibrium between the forces of selection and variation. Thereafter, evolution must take place in the context of low genetic diversity. (Harvey's original argument focused on populations with variable-length genomes, but he later showed that it applies also to most fixed-length GA problems.)

The dynamic balance between selection and variation—and therefore the potential for further evolution—will be affected by altering the mutation rate. This in turn will depend on various factors, including the form of selection used, the shape of the fitness landscape, and—he realized later—whether crossover as well as mutation is allowed.

With these points in mind, Harvey developed the SAGA algorithm (Species Adaptation GA), to enable "hill-crawling" rather than hill climbing (I. Harvey 1992*a*, 1994). The technical arguments driving SAGA were tested out on Stuart Kauffman's models of fitness landscapes (see Section viii.b).

On rugged landscapes, sudden large-scale genetic change is likely to decrease fitness drastically. Fruitful interbreeding over many generations therefore requires not a ragbag of hugely different individuals but a genetically converged population: in other words, a species. SAGA's technical parameters were defined, and set, accordingly. Taking account of the three factors mentioned above, Harvey tried to determine the optimal mutation rates and set his models so as to approximate to them. For instance, it followed from Kauffman's point remarked above that, given a highly converged population, only small variations in length would tend to be viable. Similarly, crossover should result only in small variations in the genome.

In terms analogous to those used in Chapter 1.ii.b to categorize types of AI, Harvey's work counts as "technological"—not "biological"—A-Life. His work was done for engineering purposes, to make artificial evolution more powerful. Nevertheless, his ideas were potentially relevant to biology, for example to the question whether biological evolution has (sometimes? usually?) settled on near-optimal mutation rates.

In either case, the importance of SAGA was that it wasn't restricted to optimization on pre-defined problems. It could simulate the long-term incremental evolution of entire populations, and the generation of increasingly complex forms.

SAGA was used in the early 1990s by Harvey and his colleagues, and a few years later by others also, to evolve both virtual and physical 'species'. These were used to explore (for example) the rate of evolutionary improvement given varying mutation rates, the emergence of sensori-motor coupling between system and environment, and the effects of different levels of environmental noise. (Later, the Sussex group would evolve robot brains from GasNets, as well as from purely connectionist systems: Chapter 14.ix.f.)

For instance, a simulation of co-evolving predator and prey was used to measure evolutionary progress in strategies for hunting and escape (Cliff and Miller 1995). Evolutionary 'progress' is a problematic concept, not least because species evolve in complementary ways. As the Red Queen said to Alice, they have to run as fast as they can to stay in the same place. This causes difficulty: although an animal's speed can be directly measured, its efficiency cannot, because predator and prey are always in equilibrium. In a simulation, however, improvement can be measured by 'reviving' ancestor generations and pitting them against later ones.

As for physical artefacts, populations of robots embedded in specific environments evolved control mechanisms coupling them to those particular environments (see below).

Computer *hardware*, too, was developed by incremental (fixed-length) evolution using SAGA. Adrian Thompson (1970–) achieved the first evolved hardware robot controller in the mid-1990s. He worked with a reconfigurable home-made circuit on board a real robot, where the sonar echoes were simulated (during evolution) according to how the robot moved in a simulation of the corridor. After evolution the simulation was switched off, and the real sonar echoes in the real environment were used, to confirm that the robot did indeed work (A. Thompson 1995). Later, when suitable reconfigurable chips became available, he evolved circuits in simulation and then downloaded them into actual silicon chips (A. Thompson *et al.* 1996; Thompson 1998).

We noted in Section iii.c, when discussing D'Arcy Thompson, that a mistake in the simulated physics of a virtual environment may be picked up by the software creatures evolving in it (cf. Sims 1994). Similarly, contingent features of a laboratory task environment can become locked into evolving material artefacts—sometimes, bearing a startling resemblance to actual biological mechanisms. The Sussex work on evolutionary robotics showed that this can happen in negative or positive ways, decreasing or increasing complexity, respectively.

For instance, one of a robot's two 'eyes', and all of its 'whiskers', sometimes lost their connections to the controlling neural network (the 'brain'), if—in the environment provided—neither stereopsis nor touch was essential to perform the task (Cliff *et al.* 1993). (Compare the fact that auditory cortex in the congenitally deaf, or in laboratory animals deprived of auditory input, comes to be used for visual computation: Chapter 14.ix.d.)

More positively, the 'brains' of robots encountering a triangle of white cardboard sometimes evolved a 'feature-detector' analogous to those discovered in monkey brains in the 1960s by David Hubel and Torstein Wiesel (14.iv.a). This was a mini-network sensitive to a light–dark gradient at a particular orientation. It evolved as part of a visuo-motor mechanism, its connections to motor units enabling the robots to use the white triangle as a navigation aid (I. Harvey *et al.* 1994; P. Husbands *et al.* 1995).

In another experiment, the group evolved a neural circuit capable of controlling sixteen robot motors—two for each of the artificial octopod's legs—in coordination. As a result, the robot could move coherently and 'appropriately'. What's more, the evolved neural network took account of the signals from the robot's four infra-red sensors, ten light sensors, and various sensory bumpers and whiskers (Jacobi 1998).

Likewise, in the evolution of computer hardware at Sussex, circuits evolved to compensate for—and sometimes even to exploit—physical properties of the chip that were irrelevant to the chip fabricator's model of what was going on (A. Thompson 1997). Being irrelevant (in that sense), they were unknown, and not part of the chip specification.

Up to a point, such opportunistic noise tolerance is an advantage. But the exploitation of accidental details can be dangerous. In biological terms, a species that exploits a highly specialized niche is especially vulnerable to environmental change. Likewise, if the evolved logic circuit were to be copied into a different silicon chip, it might not work. And the robots' feature-detector would be useless if the triangle were black, or had sides sloping at a different angle.

Environmental 'noise' was therefore deliberately provided by the Sussex group, to prevent such overspecialization—and to follow up their intuition that evolution is easier in noisy environments. Biological evolution has noise provided anyway, in the sense that organisms live in variable environments. Even animals that eat only one type of food don't insist on its absolute purity. And many vertebrates have evolved comprehensive sets of feature-detectors, sensitive to both light–dark and dark–light gradients, in any orientation (Chapter 14.iv.b).

Evolution was modelled also by exploiting genetic drift across neutral (*sic*) networks. Some units in a genome may be irrelevant, in the sense that mutations have no effect on fitness. In the current genetic context, they have no causal influence on the phenotype. But, given mutations elsewhere in the genome, they might be useful in the future (see the discussion of "punctuated" evolution in Tierra, above).

Many organisms contain large amounts of 'junk DNA'. But it's hard to believe that evolution favours useless burdens—so perhaps junk DNA does convey some advantage? The molecular biologist Manfred Eigen suggested in the late 1980s that junk DNA could enable wider exploration of the genetic space, by providing fitness-neutral pathways that sometimes lead to high-fitness structures that would otherwise be inaccessible. In other words, neutral networks could prevent the species from getting stuck on a local optimum in the fitness landscape. Instead, it could move 'sideways' through the space, altering the genome without altering the fitness.

Harvey and Thompson (1996) combined Eigen's idea with (a fixed-length version of) SAGA, and with the recognition that the way of encoding the genome did indeed allow potentially useful "junk". Using GA parameters optimized to take advantage of this phenomenon, they evolved hardware circuits and also studied various abstract properties of evolution in general (see also Barnett 1998).

At much the same time as Harvey was working on SAGA, Steve Grand was independently developing a new type of computer world called Creatures, inhabited by evolving virtual organisms of various types (Grand *et al.* 1997; Grand and Cliff 1998; Cliff and Grand 1999). Although this was initially intended for home entertainment—and swept the world on its release in 1996—it could also be used as a test bed for theories of mental architecture, and for various commercially useful applications. For instance. it was used to simulate customers' behaviour in a bank, to enable the management to plan where to position the counters, advice tables, and automatic tellers (R. Saunders 2000).

The story of Creatures is told in Chapters 13.vi.d and 16.x.b. Here, its interest is that it was initiated outside academia, by someone who wasn't a trained computer scientist—and that it enthused a number of artists and cultural commentators. Artificial evolution had entered the real world.

d. Open-ended evolution

By the late 1990s, evolutionary computing was an established methodology in AI and A-Life:

* It merited a handbook of over 600 pages to describe the various techniques (Baeck *et al.* 2000).
* It could be used for systematic experiments concerning both life and mind.
* At the level of evolutionary biology, it could investigate the effects of different types and rates of mutation, on different types of fitness landscapes, given different amounts of environmental noise, and allowing for fitness-neutral genetic drift. (These were the questions driving SAGA, as we've seen.)
* And at the psychological level, simple relations between environment, neural mechanisms, and movement and behavioural strategies could be explored in evolutionary terms.

All very well . . . However, a significant problem remained, one which had been highlighted by Harvey and by the Pattee group mentioned above—namely, the open-endedness of biological evolution.

They'd pointed out, for example, that phylogenetic evolution has seen the appearance of novel types of sensory organ. As well as producing *improved* eyes, ears, or noses, it has produced *first-time* eyes, ears, or noses. How can that be?

The new sensory organs don't spring out of nothing, of course. They are adaptations of pre-existing structures, which may or may not have had some other function (e.g. the bones of the inner ear evolved from jaw parts, which in turn evolved from gill slits). Because novel forms of perception afford novel problems/challenges and novel solutions, they can't be generated by a GA with a fixed set of parameters—not even by a length-variable SAGA system. If the parameters foreseen by the programmer as potentially relevant don't happen to include (e.g.) light, then no eye can possibly emerge in the simulation.

In the new century, two of Harvey's younger colleagues managed to circumvent this problem—not by overturning the logic of the argument, but by bypassing it. They did this by working with physical hardware as well as computer programs. In brief, Jon Bird (1968–) and Paul Layzell (1964–) used Thompson's SAGA-based technique to evolve oscillator circuits—and unexpectedly ended up with a novel sensor: a primitive radio receiver (Bird and Layzell 2002).

We encountered this research in Chapter 4.v.e, in the context of Pask's self-organizing artificial "ear". As explained there, the evolution of the radio wave sensor depended on unforeseen parameters, such as proximity to a PC monitor and the aerial-like properties of printed circuit boards. Bird and Layzell concluded:

> We have argued that there are three key properties that devices must embody in order for selection pressure to form them into novel sensors:
>
> * they are situated in the physical world;
> * they consist of primitives with no fixed functional roles;
> * and the primitives are sensitive to a wide range of environmental stimuli. (Bird and Layzell 2002: 1841)

The good news was that if those conditions are satisfied, it needn't matter if the A-Life programmer doesn't—and couldn't—foresee every physical factor required for a novel sensor to arise. (Which isn't to say, of course, that a novel sensor actually will arise.) The bad news was that simulations, i.e. programs running on computers, aren't enough: truly open-ended evolution can happen only in physical devices, situated in the physical world. (D'Arcy Thompson would have approved!)

As I write this (in early 2005), comparable experiments are virtually non-existent. The small proportion of A-Life workers who do deal with "hardware" issues are almost all concerned with biochemistry (see Section x.b, below). Although they plan/interpret their experiments in the context of biological evolution, they don't use evolution in the experiments themselves. One exciting exception is a just-initiated project on evolving organic–silicon computers (described in 14.ix.f). Besides relying on GAs, this work is exploiting—and investigating—the physical dynamics of diffusion reactions. In such (still rare) cases, Turing and von Neumann and Holland *and D'Arcy Thompson too* are all marching in step. Most current evolutionary A-Life, however, recalls only the first three.

Not surprisingly, then, open-ended evolution was recently identified by the A-Life community as *the* major challenge facing them in the new century. That judgement appeared in spring 2003, when the journal *Artificial Life* published a paper bravely titled 'Collective Intelligence of the Artificial Life Community on its Own Successes, Failures, and Future' (Rasmussen *et al.* 2003*b*).

In a sense, they'd done this already. For the paper was a report drawing on the 'A-Life VII' conference in Portland, Oregon, and an earlier report on that had appeared in the same journal three years before (Bedau *et al.* 2000). Indeed, that one had nine authors, whereas the new version listed only four (two appeared on both lists). So why bother? And why call the report of the *smaller* group a "Collective" opinion of the A-Life "Community"? The reason was that this 2003 paper was based on an analysis of an extensive Internet consultation of A-Life researchers. This was intended for conversion into a continuing presence on the Web. In Francis Bacon's terminology (Chapter 2.ii.b), it was an attempt to establish an electronic Solomon's House, to serve the A-Life community in a more disciplined way than an email list or bulletin board.

The 250 delegates to 'A-Life VII' had been asked to consider the "grand challenges" for the field. After discussion through questionnaires, and through meetings held in Portland, the final verdict listed fourteen "open problems" (Bedau *et al.* 2000). The more detailed email survey added an order of priority (i.e. number of votes). The most important issues were said to be: open-ended evolution; better theory, to underpin/unify ad hoc experimentation; the definition of life; creating life (see Section x.b); and understanding dynamical hierarchies. You'll have noticed which one was top of the list.

15.vii. From Vehicles to Lampreys

Evolutionary robotics, as we've just seen, might throw light on how neural mechanisms can evolve to generate ecologically appropriate behaviour. But how neural is "neural"?

It's one thing for a robot's control mechanism to be broadly analogous to real biology, as the artificial triangle-detector was analogous to the orientation cells in visual cortex. It's quite another for a robot (or a computer simulation), whether evolutionary or not, to model actual neurology in any detail. If that could be done, however, it should provide *both* more lifelike robots/simulations *and* a way of testing, and generating, specific biological hypotheses. In other words, robots or simulations produced with real animals firmly in mind could give us computational neuro-ethology, or CNE (Chapter 14.vii)—a term coined by one of the scientists responsible for the triangle-detecting robot (see subsection b, below).

One of the first people to realize this was Arbib, who took the whole animal—and its detailed neurology—into account when modelling integrated sensori-motor mechanisms. In the 1960s, he was doing this virtually alone: his *Rana computatrix* had no implemented CNE cousins. But as the years passed, the four obstacles remarked in Chapter 14.vii.a lessened. From the early 1980s, practical work in CNE flowered.

Frogs were joined, as subjects for computer modelling, by many other animals. And 'computer modelling' increasingly came to mean robots as well as simulations—with predictable disagreements about which methodology was superior. Some of these artificial animals, whether implemented as robots or as programs, weren't designed but evolved—with interesting implications for neuroscience.

Claims to be modelling the "whole" animal, of course, had to be taken with a pinch of salt. Even apparently 'simple' animals often turn out to be much more complex than they seem.

For example, every one of the hundreds of hairs found on a spider's leg (eight legs, remember) bears a number of sophisticated mechanoreceptors and chemoreceptors (Barth 2002; Humphrey *et al.* 2003). (I say "a spider", but that's shorthand: there are eight times as many species of spider as of mammals.) And whereas the nematode worm has 302 neurones (whose action and development have been individually mapped by Sydney Brenner's team at Cambridge: 14.v.b), ants have approximately half a million.

a. Valentino's vehicles

While Arbib was concentrating on increasingly realistic frogs and largely speculative humans, the MIT neuro-anatomist Valentino Braitenberg (1926–) was defining a whole family of imaginary animals (Braitenberg 1984). These were toys, in the sense that they were descriptions of possible mechanisms *stripped down to the bare essentials*. (They also part-inspired the design of actual toys, such as the robot dogs manufactured in end-of-century Japan: R. A. Brooks, personal communication.) However, the "essentials" were all drawn from neuroscience—including the work on feature-detectors discussed in Chapter 14.iv.

Braitenberg wasn't the first to imagine models of whole animals. Friedman, for example, had speculated on a homeostatic robot (see Section vi.a). Edward Tolman and Clark Hull had sketched the "schematic sowbug" and other notional robots (Chapter 5.iii.c). Grey Walter had not only imagined his tortoises, but built them (Chapter 4.viii.a–b). And McCulloch had helped program a controller for a Mars robot (Chapter 14.v.a). But Braitenberg's discussion was much more systematic, concerning not a single model animal but an increasingly complex series of them. He showed persuasively how a wide range of behaviours could be generated by a relatively simple set of basic (and neurologically plausible) mechanisms.

He'd been interested in physical simulations since the late 1950s, when he described systems of coupled oscillators as functional models of cell assemblies and correlation-based models of the cerebellum (1961). In the mid-1960s, he wrote a lively paper giving designs for simple creatures that would move around in response to smell and vision. The three key concepts were *taxis* (automatic movement whose direction and speed is determined by the position and intensity of the stimulus), *kinesis* (unoriented movement, whose speed depends on stimulus intensity), and *decussation* (wherein one side of the brain is connected to the opposite side of the body). It was this paper, together with Braitenberg's earlier theory of movement timing by the cerebellum, which had inspired Arbib to work on *Rana computatrix* in the first place (Arbib 2002*b*: 13).

By the early 1980s, neuroscientific work on learning, and on the integration of perception and action, had blossomed. In an intriguing and highly accessible book—which introduced many people to cognitive science—Braitenberg (1984) now defined a series of increasingly complex creatures, or "Vehicles".

Vehicles were described by Braitenberg himself as a "fantasy" (1984: 95). They weren't implemented: not as robots, and not as programs either. They count as a form of CNE nevertheless, because his book was an abstract exercise in comparative neuropsychology.

In effect, his Vehicles were paper-and-pencil designs for what are now called situated robots (see Section viii.a, below). They provided a phylogenetic scale, going from simple to complex and ending with robots supposedly capable of "running after a dream" (p. 83). Each chapter started from, and added to, the design achieved in the chapter before. So, like biological evolution, the sequence had no viability gaps.

The first Vehicle had (i.e. was imagined as having) one motor, and one sensor—for temperature, light, chemicals, or sound waves. Braitenberg pointed out that even this creature, which simply moved in the direction in which it happened to be pointing, with a speed proportional to the stimulus intensity, would in practice do things that weren't built into the rules. For it would encounter friction, which would affect both its speed and its direction. In general, the behaviour of an *implemented* (embodied) Vehicle would depend not only on its design but also on its dynamic interaction with the environment.

Vehicle 2, which had *two* motors and *two* sensors, was a combination of two instances of Vehicle 1. Although its motors, too, moved faster with increased stimulation of the sensors, its behaviour was more complex. Moreover, the behaviour differed according to just how the two component Vehicles were combined. Each sensor might be connected to the motor on its own side, or to the one on the opposite side (the simplest case of decussation). In the first case (Vehicle 2a), the creature would tend to avoid the source of stimulation. In the second (Vehicle 2b), it would turn towards the source and move forward until hitting it.

The other twelve designs, each with distinct varieties as in Vehicles 2a and 2b, were synthesized by adding a relatively simple computational feature to the one before. These features included inhibition, thresholds, lateral inhibition, association, and one-way facilitation. As Braitenberg remarked, the last of these—which an engineer would regard as "the main technical problem to be solved"—had been modelled by Uttley in the 1950s, and was now being used in many models of conditioning (p. 114).

Even Vehicle 1, Braitenberg had claimed, would be described by an observer as *alive*. If someone were to watch Vehicle 2 for a while, their descriptions would be psychological as well as biological:

> Let Vehicles 2a and 2b move around in their world for a while and watch them. Their characters are quite opposite. Both DISLIKE sources. But 2a [the same-sided version] becomes restless in their vicinity and tends to avoid them, escaping until it safely reaches a place where the influence of the source is scarcely felt. Vehicle 2a is a COWARD, you would say. Not so Vehicle 2b [the decussated version]. It, too, is excited by the presence of sources, but resolutely turns towards them and hits them with high velocity, as if it wanted to destroy them. Vehicle 2b is AGGRESSIVE, obviously. (Braitenberg 1984: 9)

As this passage shows, the Vehicles were mischievously described in psychological terms—such as LOVE (as early as Vehicle 3!), VALUES, LOGIC, CONCEPTS, IDEAS, FORESIGHT, EGOTISM, and OPTIMISM. These anthropomorphic properties were said to be just as "obvious" as AGGRESSION.

Clearly, Braitenberg's tongue was wedged firmly in his cheek. But there was method in his mischief. He was aiming not only to show how highly complex behaviour could be generated by fundamentally simple computational processes, but also to ground this fact in known neurological mechanisms.

Like Pitts and McCulloch (1947), but with the benefit of nearly forty more years of brain research, he related his abstract designs to the specifics of neuro-anatomy. He referred, for instance, to the pyramidal and stellate cells of the cerebellum, and to the orientation columns in visual cortex. He also referred to non-vertebrate examples, such as the compound eyes of insects (pp. 100 ff., 105 ff.).

He even offered computational explanations of some puzzling biological facts. For example, he argued that decussation hasn't evolved by chance as a selective advantage. Rather, it's a natural consequence of a mathematical fact: if each computational unit is connected to all the others then, given bilateral symmetry, there will be more connections crossing the median plane than staying on one side of it (pp. 95–9). Similarly, he suggested that the feature-detectors for orientation and direction that had so amazed their discoverers, Hubel and Wiesel, "may be just a special case of a general principle of the cortex" (p. 140).

Not surprisingly, Braitenberg's work later became highly influential in CNE, and in other areas of A-Life (see Section viii.a). But whereas he'd discussed different types of animal in highly general terms, most of this late-century research modelled the neurology and behaviour of particular species.

The animals chosen were many and various. Besides frogs, which Arbib had been simulating since the late 1960s, several types of fish were considered. Multi-finned fish were modelled by computer graphics programs that simulated how they swam, taking into account both muscle movements and detailed hydrodynamics (see Section ii.a, above). The "robotuna" was a swimming robot fish, whose tail propulsion depended on the production of (and interactions with) specific vortices in the water (Triantafyllou and Triantafyllou 1995). Lampreys were modelled also (see subsection d, below). So were rats: for example, how their navigation abilities depended on the spatial maps in the hippocampus (Mataric 1991).

b. Of hoverflies

Lest you think that only vertebrates were so honoured, insects were studied too. Indeed, the relative simplicity of insects' behaviour and neurology made them a prime focus for CNE. The many insect species involved included hoverflies, crickets, stick insects, and cockroaches—and the CNE methods used to study them differed significantly, as we'll now see.

A male hoverfly will track a female as she flies through the air. Once she settles on a flower, he shoots forward at a very high rate of acceleration, lands on the female, and mates with her. His change of direction, on seeing her settle, depends on the particular approach angle subtended by the target female at the time. If the flower suddenly moves in the breeze, too bad: the male's movement is ballistic. Like a bullet, its trajectory can't be adjusted in mid-flight. The reason is that, unlike the guided missiles developed by the early cyberneticians (see Chapter 4.vii), the fly has no internal mechanism enabling its mating path to be influenced by feedback from the target.

The male's tracking of the female while she's in flight does vary as her position changes. But it doesn't show the flexible selection and variation of pathways often seen in humans and other mammals. Instead, the male fly's tracking flight path is determined by a small set of simple and inflexible rules, hardwired into the insect's nervous system. These rules transform a specific visual signal into a specific muscular response.

One doesn't normally think of anatomical sex differences as including the eyes. But in hoverflies (more specifically, in *Syritta pipiens*), they do. The male has a specialized frontally directed high-resolution set of ommatidia (eyelets) in its compound eye that is functionally a fovea, like that in human eyes. The female doesn't. As she flies around, her eyes aren't good enough to tell her that, just beyond her visual range, lurks the male who is tracking her. In other words, whereas he's capable of a primitive form of visual *attention*, she isn't.

For the male to do the tracking, he needs to keep her foveated—i.e. in the centre of his visual field. Just like humans, the male *Syritta* uses a ballistic eye movement (a saccade) to foveate a target that appears in its peripheral vision. Once the target is in the fovea, it's kept there by means of smooth tracking movements (more precisely, by a succession of tiny ballistic movements, which are damped by the resistance of the air). The male regulates the distance to the female by keeping the vertical height of her image on his eye within a set range. He has feature-detectors in his retina that respond in an appropriate fashion: the closer the target image is to the right height, the more actively they fire.

The "right" height, of course, refers implicitly to the size of a female hoverfly. Just as a frog will snap at a moving object only if its radius of curvature lies within certain limits (14.iv.a), so a male hoverfly will accept as a target only something that happens to be much the same size as a female. But this is all that's needed to induce tracking: if an appropriately sized speck of ash happens to be floating in the air, it may be tracked instead.

These facts were discovered in the mid-1970s by biologists Michael Land and Thomas Collett at the University of Sussex (Land 1975; Collett and Land 1975; Collett 1980). They were later turned from NE into CNE by David Cliff (1966–), one of the Sussex 'A-Lifers' responsible for the triangle-detecting robot (Cliff 1991*a*).

Given the question raised in Chapter 1.i.a, whether the study of insects can teach us anything about human minds, it's worth noting that both Land (1975) and Cliff saw the hoverfly as a simple analogue of animate vision in humans. In "animate" vision, important perceptual information is provided by the creature's own movements (Ballard 1989, 1991). It's worth noting, too (with respect to the attacks on GOFAI described in Chapter 13.iii.b), that it was Cliff who suggested reading "AI" as "artificial insects".

Much as a human's gaze, or visual attention, is captured by certain visual stimuli (a phenomenon discussed in Pitts and McCulloch's 'Universals' paper: see Chapter 12.i.c), so is the hoverfly's. In both cases, behaviour—including eye movements—is influenced accordingly. But once the hoverfly's eyes have centred, its action selection is more rigidly constrained.

Because its eyes are fixed in its head and it has no neck movements, there's no distinction between direction of gaze and body orientation. Moreover, although it can fly in any direction—including sideways, backwards, and stationary hovering—its flight direction is generated by only three components. On top of all this its eyes are less complex, so more easily simulated, than human eyes. Each has about 30,000 photoreceptors, as against 127 million in the human case. In short, 'realistic' simulation of the hoverfly's visuo-motor system was a reasonable goal.

Cliff designed a neural network named *Syritta computatrix*, or SYCO, in homage to Grey Walter and Arbib—and Alfred Hitchcock. This modelled how the hoverfly uses image sampling to lock the most important stimulus onto the area of greatest visual acuity in its eyes; how it tracks that stimulus in motion; and how the visual information is used to determine its flight.

Things weren't made unnaturally easy for SYCO: varying air turbulence and cross-winds were simulated too. As Cliff pointed out, this form of computer vision was very different from that of GOFAI, which typically used toy worlds, static receptors with uniform resolution, and static images (see 10.iv.b).

To simulate the rigid flight control described above, Cliff relied not on exhaustive hand design but on training. SYCO used backprop (see 12.vi.c) to learn to match specific retinal images with specific flight directions. The intention was to compare the 'neurology' of the successful network (once it emerged) with the neurology of the real fly, and to choose between alternative explanations—or generate new ones—accordingly. Cliff constantly evaluated and improved his CNE model by comparing it with the NE data, and managed to answer some previously posed theoretical questions.

He pointed out that "Experiments impossible in living tissue or impracticable in robotics can routinely be performed on SYCO" (1991*a*: 95). But SYCO was just one system. So Cliff broadened his argument in a "provisional manifesto" for CNE in general (Cliff 1991*b*). He explicitly endorsed Humberto Maturana's whole-animal approach (see Section viii.b), including his argument that "the nervous system should not be treated as an input–output device" (1991*b*: 32). Unlike Maturana, however, Cliff was happy to describe his own project as a computational one:

> Computational neuroethology replaces the *in vacuo* approach of connectionism with a (simulated) *in vivo* approach; and in doing so, the semantics of the model are automatically grounded.
>
> So, computational neuroethology can be provisionally defined as the study of neuroethology using the techniques of computational neuroscience. (1991*b*: 34)

(He was happy to use the term *computational* because he thought of it then as fairly "neutral"—as it is in this book. Today, because of the later developments in A-Life, he'd prefer *synthetic*: personal communication.)

Although Cliff here described this definition as "provisional", he'd been using the term CNE in unpublished work since 1988 (1991*b*: 34). It first left the press in 1990, when Randall Beer used it in the subtitle of his book *Intelligence as Adaptive Behavior: An Experiment in Computational Neuroethology*. By the mid-1990s, the new coinage had been widely adopted.

Besides vaunting CNE as a way of using A-Life to do neuroscience, Cliff drew a moral for other areas of cognitive science. He criticized the biologically unrealistic assumptions of both GOFAI and connectionism. And he ended with this anti-anthropocentric flourish:

> All current connectionist models of language are wildly premature. Language will be best understood as a very high layer in a subsumption architecture: how it interacts with lower layers could be of vital importance, and we should study these lower layers first.
>
> If we are still yet to discover the subsymbolic or neural basis underlying the dance-language of bees, how then are we supposed to study such aspects of human language at anything but the

most gross level of neuroanatomy (i.e. studies of lesioned patients)? We simply do not know enough. (1991*b*: 37–8)

It didn't follow that the studies of language described in Chapter 7.ii and 9 were a waste of time, for they hadn't addressed the "neural basis" of language. But connectionist work on past-tense learning, for instance, was seen by Cliff as fundamentally wrong-headed (Chapter 12.vi.e).

As for Arbib's talk of linguistic schemas (Chapter 14.vii.c), this (by implication) was possibly on the right track—but highly untimely nevertheless. The message was that to understand language, we must first understand hoverflies.

c. Playing cricket

Cliff had defended *Syritta computatrix* partly by reference to the "impracticalities" of robotics (see above). Nevertheless, some influential CNE research relied on this methodology.

One example was Barbara Webb's (1965–) work on crickets, begun in Edinburgh's AI Department and continued under Psychology's flag at Stirling (and now back in Edinburgh again, in the School of Informatics). Like Cliff, she was offering not only a contribution to neuroscience but also a critique of symbolic and connectionist AI. Her theoretical focus was on "behaviour and physics" rather than "representation" or "general abilities" (1994: 52–3; 1996; 2001*a*,*b*).

Francis Crick pointed out that evolution employs "slick tricks" when it can (see Chapter 14.ii.a). Turning from Crick to crickets, it turns out that these creatures use an especially slick trick to manage their courting behaviour. The female finds the male by locating the source of his song (produced by drawing one wing across the other). *Good! So she can hear!* Well, yes and no.

A human Juliet hearing her Romeo can first recognize his song, distinguishing it from all the other sound patterns she can identify, and then decide in which direction she should go to meet him—or to avoid him, if they've had a tiff. She doesn't have to go straight there: if her ardour isn't overwhelming, she can run a few errands on the way. (The female cricket may not respond if she's already mated, or if there are predators around—but if she goes, she goes.) Moreover, if Romeo sings half an octave lower than he did yesterday, or half as fast, Juliet can still find him. She can even learn to appreciate a new song, should he decide to sing one.

Not so, the cricket. As remarked in Chapter 14.iv.d, insects can't learn new patterns. Even more to the point, the female cricket can recognize only one song, sung at only one speed and frequency—give or take . . . (cf. Webb and Scutt 2000: 247 ff.). What she takes as the "speed" of the song is the rate of repetition of the syllabic patterns within it—which varies for different species of cricket. When she hears her conspecific's song, she can do only one thing: move towards it. Unlike the hoverfly, however, she can walk *or* fly to meet her mate—and if she walks, she can do so more or less directly.

Her ears respond to many different sounds, but she doesn't rely on that fact to recognize the male's song. She doesn't need, and doesn't possess, a brain stuffed full of auditory-feature-detectors for coding a wide range of sounds. Instead, she has a custom-built mechanism—a single 'feature-detector'—sensitive to only one frequency. (She also has a simple mechanism for hearing and fleeing bat ultrasound: Webb, personal communication.)

Her automatic mate-finder isn't a *neural* feature-detector, like those described in Section iv. Instead, it's implemented as a fixed-length tube in her thorax (the trachea), which is connected to the ears on her front legs and to her spiracles.

When the male cricket sings, the sound waves travelling through the air in the tube interact with those in the air outside, resulting in phase shifts that produce vibrations of different intensity at each eardrum. But the length of the tubes in the female's body is an exact proportion of the physical wavelength of the sound emitted by the male. So the *physics* ensures that phase cancellation happens only for sounds having the frequency of the male cricket's chirp. Similarly, the *physics* ensures that the intensity difference depends wholly on the direction of the sound source. The female doesn't have to work that out, nor decide what to do about it: she's neurally hardwired to move in the direction concerned. (Notice that there's no need to assume she's 'conscious' of any sound.)

So much was suspected by the early 1980s. The cricket's sound-directed movement (phonotaxis) and its neural underpinnings were already "one of the insect systems most thoroughly studied in neuroethology" (Webb 1994: 45). Researchers knew, for instance, that there are about fifty auditory receptors (though they weren't yet individually identified), and several pairs of auditory interneurones in the pro-thoracic ganglion (Webb and Scutt 2000: 248). In 1985 the behaviour and neuro-anatomy were described for the public, in the *Scientific American* (Huber and Thorson 1985).

All very intriguing . . . But despite the congratulatory tone of the *Scientific American* article, the underlying mechanism still wasn't understood. Various sound cues had been suggested, but how they might be discriminated by the cricket was unclear. Equally, whether—and how—they interacted in the nervous system wasn't known. Another mystery was how the cricket's hearing could be integrated with any other modality, such as vision (this isn't too surprising, since most of the phonotaxis experiments were done in the dark).

It was generally assumed that the *recognition* of the song happened in the brain, and that the neural system representing the song was distinct from the *localization* system. Nevertheless, this wasn't certain. So a neurological paper published in the *Journal of Comparative Physiology* of 1988 admitted:

> It is still entirely unclear whether or how [the direction and the characteristic sound] of the calling song are processed independently of one another in the brain, or how the brain triggers and controls phonotactic walking. (quoted in Webb 1994: 45)

In the mid-1990s, the same journal published a paper (modelling the role of the pro-thoracic ganglion) which simply took recognition for granted, and which used spiking rates to predict the direction of movement without specifying how this correlation might be implemented (Webb and Scutt 2000: 248; Webb 2001*c*). In short, a great deal of neuro-ethological hand-waving was going on.

In the early 1990s, Webb decided to address these questions by devising "the simplest possible robotic mechanism that could support the observed behaviour of the cricket" (Webb 1994: 46). That is, she turned the NE into CNE—and she expressed the "C" as physical robots, not (like Cliff) as computer simulations.

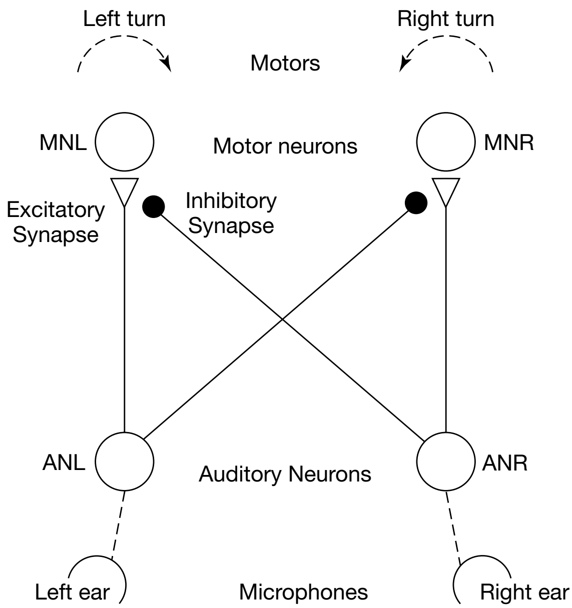


FIG. 15.5. The four-neurone system for phonotaxis. Auditory neurones (AN), modelling the AN1 pair in the cricket pro-thoracic ganglion, receive auditory input. They have a high recovery level, i.e. continue producing spikes over a raised membrane potential while the stimulus continues, and a relatively slow time constant. Each connects to a motor neurone (MN), which will be excited at the onset of firing, and to the opposite AN–MN synapse, which will be inhibited at the onset of firing. Thus, repeated onsets of one AN before the other are required to drive the respective MN above threshold, which causes a turn. Redrawn with permission from Webb and Scutt (2000: 251)

As for seeking the simplest possible mechanism, she was closely guided by the neuro-anatomy but didn't try to model every neurone that had been observed. Eventually, she got down to as few as four (see Figure 15.5).

Unlike the artificial cockroaches described below, Webb's early 1990s robots looked nothing like insects. They had wheels, not legs, and were built on a Lego base or a commercial Khepera robot. After all, the theoretical questions didn't concern the movements of the cricket's legs, but the sensori-motor circuitry making her move towards the song.

Like Arbib's *Rana computatrix* (see 14.vii), Webb's machines grew closer to the biological data as the series progressed. By the turn of the millennium, her robots modelled the neural processes of summation, firing latency, thresholding, recovery, decay, and comparison. Changes in membrane potential were simulated by a small set of state rules, defined in terms of above/below resting, above/below threshold, and above/below recovery (Webb and Scutt 2000: 250).

The neurones were connected to the motors on either side in such a way that recognition of the ideal signal would cause the robot to turn towards it (compare Braitenberg's Vehicles). And Webb tracked the activity levels of the individual neurones, as well as observing the whole robot's behaviour.

Most of these robots didn't move in a straight line—and nor do female crickets. The machines, like the insects, zigzagged within 60 degrees of the optimal direction, and weren't put off by obstacles unless the sound source was very close to the starting point. Later, Webb's group showed (with the help of specially designed VLSI chips from Caltech) that combining hearing with vision enabled the creature to move to the target more directly (Webb and Harrison 2000).

The internal parameters were systematically varied, to test the neural hypotheses involved. Significantly, it turned out that the robot's success was generated by a decision mechanism much simpler than had previously been thought necessary. That is, CNE had provided—and tested—a more elegant theory than straight NE. A-Life had advanced our knowledge of real life.

Webb's work showed conclusively (for example):

* that the various neurological theories which had assumed that there *must be* distinct systems for recognition and localization were mistaken;
* that this was so *even though* the female is able to choose the more attractive of two simultaneous songs (a fact that had previously been thought to support the distinct-systems hypothesis);
* that the neural firing rate was *not* (as had often been assumed) the crucial feature of the signal;
* and that one and the same mechanism could both compare latencies and identify the temporal patterning of the song.

In short, she had clarified many neuroscientific questions, answered some, and raised some more. Answering the unanswered questions, she said, would require work both in robotics and in 'wet' neuro-ethology—and, ideally, close cooperation between them.

To critics who sneered at the use of wheeled robots to model graceful crickets, Webb had an effective reply—a modern version of Vaucanson's remarks about what he'd learnt by building his flute-player (Chapter 2.iv.b). In general, she said, the practical weaknesses of robotic CNE could be seen as theoretical strengths:

> The precision of sensing and motor control is generally much worse in the robot than would be expected for the cricket, so the success of the mechanism is unlikely to be due to abilities that the cricket could not really have. The robot constitutes a *subset* of the capabilities of the cricket rather than an *abstraction* of them. (Webb 1994: 51)

By contrast, purely programmed simulations could be too powerful to be biologically convincing:

> Computer modelling of *realistic* physics of phonotaxis in a moving animal or robot is quite difficult. "Ideal" sound propagation can be described by fairly simple equations, but in any real situation, with a directed speaker, a floor surface, reflecting walls and so on, it becomes extremely difficult to calculate with any accuracy . . .
>
> Using a computer model rather than a robot would, in this case, substantially weaken the justification for extending the results of testing the hypothesis to the cricket. For example, it would be hard to claim that, insofar as sensors and motors differ, those in the model are substantially *worse* in accuracy than in the cricket: it would be more likely that the mechanism only works because of the idealised conditions. (p. 53)

In some studies, Webb's group worked backwards from robot to insect, instead of from insect to robot (Lund *et al.* 1997). In other words, they asked whether, and how, sensori-motor robots could be made to respond to the songs of *real* crickets.

In the earlier experiments, the "ideal" sound signal had been partly determined by parameters such as robot size, processing time in the circuitry and software, movement speed, and turning inertia. As a result, the robot had sought a signal very different from that preferred by biological crickets. For instance, the songs it responded to were ten to twenty times slower than those of the real animals, and the area that could be experimentally explored was only a fifth of the size (relative to the size of the creature) typically used in NE experiments on crickets.

By the mid-1990s, technological progress in robotics offered Webb speedier processing, and a more fast-moving creature. As well as confirming that her earlier neural hypotheses could indeed explain response to real cricket songs, these new experiments enabled her to test ideas about *why* crickets, given their sensory anatomy, prefer certain frequencies rather than others.

In effect, then, Webb was now in a position to say not only "The cricket behaves as it does because its neurology is what it is", but also "If the cricket's neurology were like *this*, she'd behave *thus and so*." Indeed, she could even have said "No cricket could have a neurology like *that*, because if she did she'd never find a mate." It's no surprise, then, that Webb's work was sometimes published in A-Life journals. For A-Life studies not only "life as we know it" but also "life as it could be".

Early in the new century, Webb's team reported a model of phonotaxis that included a much larger number of the known neural properties, and a wider behavioural repertoire (Reeve and Webb 2002). Drawing on biophysical theories of computation developed by Christof Koch (1999), they included (for instance) a variable exponential decay of membrane potential; a biologically plausible 'weight' determining the standard change of conductance in particular ion channels; and short-term mechanisms for facilitating or depressing this change. Whereas the earlier models had focused on one pair of auditory neurones (marked AN in Figure 15.5), they now included several other types. The speed of the cricket's movement was varied, as well as its direction, and these were integrated with both visual and auditory inputs. For example, in brighter light the robot was less likely to stop when it was moving towards the sound.

Webb's new robots included not only wheeled systems, but also six-legged creatures similar to those discussed below. Significantly, these insect-like robots can move across natural terrain, such as summertime and frost-covered grass, not just the unnaturally flat laboratory floor (Horchler *et al.* 2004; Reeve *et al.* 2005). As well as responding to real cricket songs, they rely on biologically based neural simulators (and specially crafted VLSI chips) which are even closer to the actual neurophysiology than in Webb's earlier experiments. (Her team hope to make them closer still, by feeding the spiking patterns from real cricket neurones into the model.) This example illustrates how far "neuromorphic engineering" had come since Carver Mead's pioneering VLSI retina (see 12.v.f).

We shouldn't look down our noses too hard at the cricket. For humans, too, sometimes rely on inbuilt anatomical tricks, and on 'quick-and-dirty' responses to limited aspects of the sensory information.

Indeed, one set of human responses probably involves another fixed-length physical mechanism for sound location. In cats, the time difference between the moments at which the signal arrives in the two ears is coded by the connection lengths of specific neural circuits in the brain, and this tiny (microsecond) delay is used as a cue in locating the source (Smith *et al.* 1993). The same is very likely true of people. However, the mammalian system—because it depends on a range of time differences rather than a specific phase interaction—isn't limited to a particular sound frequency, as the cricket is; and mammals have a series of delay lines of different lengths rather than just one fixed delay.

A closer analogy, perhaps, is the strong tendency people show to turn to face a sound source (Webb, personal communication). They don't need to recognize the sound, or to locate it accurately. They simply keep turning until the two ears have the same input. Whether or not this is done by dedicated neural circuitry, the point is that the information in the sensory signals doesn't have to be 'explicitly' decoded to enable the response.

So, much as William Harvey's studies of slugs, newts, and eels helped him understand the human blood system (Chapter 2.ii.a), CNE studies of crickets can help us think about our own psychology. But whereas blood circulation in newts is very similar to ours, although slower, perception and action selection in insects is profoundly different. Humans (Juliet, for instance) can do much, much more.

As for stick insects and cockroaches, their neuro-anatomy was simulated in six-legged robots modelled on NE findings about real beasts. The theoretical focus, here, was on walking *as such*.

These hexapod robots, using their multi-segmented legs, could clamber over obstacles rather than having to avoid them, and could even climb stairs. Some could recoil from an obstacle. They'd walk backwards steadily for a while before turning, instead of repeatedly colliding with the obstacle while edging along its surface as Grey Walter's *Machina speculatrix*, and the simplest Vehicles too, had done. (Beer sometimes referred to his cockroaches as *Periplaneta computatrix*, in homage to Grey Walter.)

Such results weren't trivial. The choice of the next movement—*which* leg should be moved, and *how*?—had to be just right. What was the correct placement, force, timing, and sequence? And how should the legs interact? They had to be largely independent, because there might well be a pebble near the 'foot' of the second leg and none near the sixth. But there had to be some degree of integration. If the second leg were lifted higher than usual, the balance of the insect's body would be affected—so the other legs would have to compensate. Moreover, each leg was jointed: how were the several segments to be coordinated?

Some of the most influential work of this type was done in the 1980s by Beer (1961–) and colleagues, including mechanical engineers (directed by Roger Quinn), at Case Western Reserve University, Cleveland. Initially, they were inspired by Braitenberg's Vehicles, by Rodney Brooks's situated robotics (viii.a, below), and by Phil Agre and David Chapman's Pengi (13.iii.b). (Later, they'd be enthused also by ideas about dynamical systems: see xi.b below.) Beer's group designed movement controllers for artificial cockroaches (R. D. Beer 1990; Beer and Chiel 1991). In the late 1980s, they began to experiment with GAs to *evolve* the controllers, rather than designing them by hand (Beer, personal communication).

Most of their robots were physical machines. Others, especially when they got into evolutionary robotics, were abstract simulations, functioning within a virtual environment. In both cases, their "neuro-anatomy" initially reflected NE data from several insect species, not just cockroaches. But they later studied the behaviour of the death-head cockroach in particular detail, and used their robot results to suggest further NE hypotheses (Quinn and Ritzmann 1998).

Beer's robots modelled the control of walking, on uneven terrain and with occasional obstacles. They had six pacemaker neurones, all of which automatically produced rhythmic bursts of activity, and each of which controlled the swing and stance of a single leg. Kinaesthetic feedback from leg-position sensors and local leg reflexes was included, and some models also had two sensory "antennae". In addition, the leg circuits were coupled together. The result was a family of robots with a range of physically stable gaits very like those seen in real cockroaches.

In the simulated creatures, walking was done with a purpose (Beer and Chiel 1991). That is, it was triggered and guided by several internal drives, or motives: eating, seeking food, edge following, and wandering. Some of these were mutually incompatible. After all, if a human nursemaid has only two hands (see Chapter 7.i.f), a cockroach has only six legs. Since it can't be everywhere at once, it must choose what to try to do next (as well as how to do it). In short, Beer's creatures faced the same fundamental problem as the Mars robot described in Chapter 14.v.a, but had a more advanced neural architecture with which to do so.

d. Evolving lampreys

The nervous systems of Beer's early cockroaches were hand-designed—as was the artificial stick insect, some of whose features Beer incorporated in his own work (Cruse *et al*. 1995). But some of his later creations were evolved, using GAs (R. D. Beer and Gallagher 1992). Starting with largely random networks—and therefore with paralysed cockroaches—he ended with happily perambulating creatures.

Cliff and his colleagues, as we've seen (Section vi.c), also did pioneering work in evolutionary robotics. Myriad 'neural' circuits were brought into existence at each generation. But only those which—like the artificial orientation detector—contributed to the robot's "fitness" (in this case, its navigational ability) had a high probability of surviving into the next.

Intriguing results such as these inspired some professional neuroscientists to start using GAs to study real animals. By the late 1990s, David Willshaw (some of whose earlier work was described in Chapters 12.v.c and 14.ix.a) was doing evolutionary CNE—though not evolutionary robotics.

With two AI researchers from Edinburgh, he compared the efficiency of artificially evolved neural networks with real ones, namely those used for controlling swimming in lampreys (Ijspeert *et al*. 1997). (These eel-like fish have a much earlier claim to fame: King Henry I of England is said to have died from "a surfeit of lampreys" in 1135.)

A lamprey doesn't have paired fins, but swims by rhythmic undulations of its long body. By 1990, biological NE had discovered a great deal about the neural circuitry, transmitters, and membrane properties involved. What it couldn't discover was what alternatives, if any, are biologically possible. What non-existent, but viable, lampreys might biological evolution have generated, had the genetic mutations been different? Even straightforward CNE simulation of the lamprey's neuromuscular system, which had been achieved in the early 1990s, couldn't answer that question.

Willshaw and his AI colleagues now used GAs to show that there are many network architectures capable of controlling this type of swimming. Moreover, they found that some of their artificial networks, *although* composed of 'neurones' closely based on the biological data, were much more efficient than those found in real lampreys. Some had a frequency range five times larger. Even when the connections (and their type: excitatory or inhibitory) were fixed to be identical with those in real lampreys, some networks evolved—within only 100 generations—with frequency ranges three times larger.

This was interesting, in view of Marr's earlier claim that biological evolution can be trusted to find the *optimal* solution for a given computational task (see Chapter 7.iv.g). It was also interesting as yet another description of "life as it could be". Evidently, CNE could provide both science and biologically plausible science fiction.

15.viii. Parallel Developments

Computer-based research on self-organization accumulated slowly. Most of it, including Holland's and the CNE examples described above, could be properly developed only when vast computational power became available in the 1980s.

The germinal ideas were growing, nevertheless. Some focused on the *results* of self-organization, some on the self-organizing *process*. In general, however, this research led to new ways of thinking about life and mind. Or rather, it used new theories and data to go back to pre-GOFAI days, building on Turing's and von Neumann's 'biological' writings and even revivifying ideas from the late eighteenth century.

These 'new' ideas were arising both within and outside cognitive science. Various people were engaged in fundamentally similar enterprises, often without realizing it. The potentially unifying themes were self-organization (emergent order), complex dynamics, distributed control, the close coupling of perception and action (and 'whole-animal' approaches in general), and autonomous systems embedded in their environment. But the unification still hadn't happened.

By 1986–7, when A-Life was first identified as a unitary field (see Section ix.a), relevant research lay scattered across mathematics, physics, and biology—and almost every discipline of cognitive science.

* In AI, for example, evolutionary modelling was studying the emergence of novel systems of increasing order (see Section vi, above), and work on agents was exploring the nature of distributed cognition (see Chapter 13.d).
* Researchers in AI and computational psychology, partly inspired by PDP connectionism (12.vi–viii), were focusing on self-organization in distributed systems (Rumelhart, McClelland, *et al*. 1986; Forrest 1991).
* Much of this work was drawing on the growing body of research in dynamical systems (see below, and Section xi).
* Neuroscientists, too, were looking at the origin and the functioning of integrated self-organized systems (Chapter 14.vi–ix, and Section vii above).

* And a few philosophers and/or biologists-turned-philosophers were exploring theories of life and mind very different in spirit from orthodox empiricism (Chapters 14.ix.b and 16.x.c, and subsection b below).
* Only theoretical linguistics, preoccupied by the "Linguistic Wars" (Chapter 9.ix.a), was immune.

This section, and the next, illustrates both the diversity and the commonality of this widespread work.

a. Artificial ants

Emergent order was being studied by a new approach in robotics, in which Herbert Simon's ant (Chapter 7.iv.i) was at last coming into its own.

As described by Simon twenty years earlier, the ant's apparently complex behaviour resulted from local interactions with its environment. Similarly, the newly built "situated" robots possessed a small number of simple reflexes, movements triggered by specific environmental cues (R. A. Brooks 1986; Beer and Gallagher 1992). These weren't movements of the entire creature, but of a single part, such as a leg. Distinct body parts would each have their own inbuilt reflex mechanism, triggered independently of the others. But these separate reflexes functioned in parallel, so that the creature as a whole 'walked' successfully.

The integrated behaviour of these biologically inspired robots, like Beer's artificial cockroaches (see Section vii.c, above), was organized bottom-up. At the lowest level of reflexes, there were no interconnections. (In the language of NK networks, N was small and K was zero: see Section ix.b, below.) But several levels of reflex mechanism might be provided, as in Brooks's "subsumption" architecture. The extra levels came from addition rather than reconstruction: the robot remained effective ('viable') at every stage.

Subsumption architectures were very different from GOFAI, and Brooks's insect-like robots not at all like SHAKEY (Chapters 10.iii.c and 13.iii.b). Earl Sacerdoti's (1974, 1975*a*,*b*) ingenious contingency planning and last-minute adjustments were given no place here. There was some flexibility, to be sure. Higher-level reflexes could inhibit lower-level ones in certain circumstances, given certain environmental cues. Apparently purposive behaviour could even emerge as a result (Mataric 1991; Maes 1991*a*,*b*). But there was no top-down behavioural control, still less any detailed modification of the execution of lower-level modules.

In short, these systems were very different from GOFAI robots. Arguably, they supported explanations of adaptive/purposive behaviour, even in animals (including humans), very different from what orthodox functionalism had recommended (Hendriks-Jansen 1996: 135–53).

Situated robots, and similarly conceived software "agents" (Riecken 1994), were proudly described by their builders as "autonomous" (e.g. Maes 1991*c*). What this actually meant was that their behaviour resulted from their own inner structure, not from any externally imposed program or plan. But there was a risk (dare one say "a hope"?) that people would read much more into it. This sort of autonomy is a far cry from human autonomy, or freedom—which, counter-intuitive though this may seem, is better modelled by GOFAI than by A-Life (Boden 1995*a*; see also Chapter 7.i.g).

These A-Life efforts, and their overly anthropomorphic interpretation, had been anticipated by Braitenberg. As we saw in Section vii.a, he'd discussed designs for "Vehicles" that were, in effect, unimplemented designs for situated robots. They'd also been anticipated by Grey Walter, though in a less neurologically explicit, and less systematic, fashion (see Chapter 4.viii.a–b).

As befitted the director of an institute of biological cybernetics, Braitenberg had acknowledged the influence of Wiener (Braitenberg 1984: 2). But his brief book didn't mention Grey Walter, whose tortoises were the first situated robots, and which generated approach–avoidance behaviour comparable to Braitenberg's LOVE and COWARDICE. Even Brooks, who did salute Grey Walter's work, criticized him for "the lack of mechanisms for abstractly describing behaviour at a level below the complete behaviour, so that an implementation could reflect those simpler components" (Brooks 1991*b*, sect. 3.3).

This criticism wasn't entirely merited: in a manuscript of 1961, Grey Walter had analysed his tortoises' behaviour in precisely this way (see Chapter 4.viii.b). But the piece had remained unpublished. It wasn't until the end of the century that Grey Walter was recognized in the A-Life conference Proceedings as "the pioneer of real artificial life" (O. Holland 1997). And it wasn't until the millennium had passed that an international A-Life meeting, on biologically based robotics, was explicitly dedicated to his memory (Damper and Cliff 2002).

Situated robots were compared by Brooks himself to Jacob von Uexküll's animal species, each with its own life-world (Chapter 5.ii.c). And—despite Brooks's disclaimer (1991*a*, sect. 7.3)—they might have been likened to Simon's Production Systems of the early 1970s (see 7.iv.b). For these, too, had relied on automatic responses to predefined cues. But Simon had focused on human reasoning, and had posited a rich internal environment of symbolic representations.

Simon had also stressed the role of the external environment, such as pencil marks on paper. But this aspect of his work was ignored by the situated roboticists of the 1980s. It was taken up (and much extended) later by philosophers and psychologists interested in situated cognition and embodiment, who recognized the importance of cultural artefacts for prompting human behaviour (see Chapters 8.iii.a and 16.vii.d).

The A-Life roboticists were inspired not by human reasoning or language, but by the world-driven behaviour of relatively simple animals. Many of their robots looked something like insects, having long bodies and six or eight jointed legs. A few, including those developed by Webb and Beer, even modelled insect neurology in some detail. Accordingly, some wags repeated Cliff's remark that "AI" now stood for "artificial insects".

The point of this remark, besides its power to raise a laugh, was tacitly to classify A-Life as a form of AI. In my view, that's correct: just as connectionism is a form of AI, so is A-Life. However, the jibe wasn't welcome in the A-Life community, most of whom went to great lengths to distance their work from "AI".

In part, this new AI methodology was driven by failure. It was trying to avoid the notorious failure of classical AI to solve the frame problem (see Chapters 10.iii.e and 13.i). But in part, it—and evolutionary robotics, too—was grounded in a different conception of intelligence, one which took seriously the fact that human and animal minds evolved in real-world environments.

This approach (outlined in 13.iii.b) insisted that advanced behavioural abilities emerge from simple ones, and that the path from simple to complex has no viability gaps (though it needn't appear continuous: see the comment on punctuated evolution in Section vi.b). The simple abilities are closely coupled with environmental conditions, so the frame problem can't arise.

Moreover, the view was that even advanced mental abilities function in similar sorts of ways, so that the concepts of computation and representation are irrelevant for explaining cognition. The leading evolutionary roboticists described their work in terms of dynamical systems (see subsections c–d, below, and Section xi), explicitly rejecting the types of explanation familiar in classical AI and (most) connectionism (Beer and Gallagher 1992; Husbands *et al.* 1995). Similarly, Brooks saw situated robotics as offering "intelligence without representation" (Brooks 1987/1991*a*) and also "without reason" (1991*b*).

Brooks wasn't speaking, here, only of insect intelligence. Despite some (ambiguous) backtracking with respect to linguistic and logical thinking, he was soon to initiate a long-term robotics project modelling the early development of human cognition—and, with Daniel Dennett's cooperation, even consciousness (Brooks and Stein 1994; Dennett 1994*a*). Dennett (1978*c*) had suggested many years before that AI researchers should try to build a *whole* cognitive system: now, here was his chance.

The Cog robot, which has already given rise to a commercially marketed baby doll, combined inbuilt reflexes with connectionist learning. It was an early exercise in what's now called "epigenetic robotics", in which a robot's control system and behaviour develop differently according to the particularities of its environment.

So Cog incorporated some of the findings in developmental psychology, linguistics, and cognitive neuroscience described in previous chapters. For example, psychologists had shown in the mid-1970s that joint attention develops by mutual gaze, gaze following, and pointing (6.ii.c). Brooks's team used these features to model the development of joint attention—not mother–baby this time, but human–robot—in Cog (Scassellati 1996, 2001).

In this still ongoing project, now directed by Brooks's students Cynthia Breazeal (Ferrell) and Brian Scassellati, the artificial denizens of "The Cog Shop" became ever more "human" (for the most recent reports, see the web site at <http://www.ai.mit.edu/projects/humanoid-robotics-group/cog/cog.html>). This was true not only of their appearance, but also of their behaviour—especially, their "social" interactions. The robot was not only given "needs" (drives, motivations) to induce caretaking responses on the part of the human interacting with it, but also babylike responses that signalled to the carer whether or not those needs had yet been met.

(More recently, the MIT group have worked on a puppy-like furry robot called Leonardo, which—up to a point—can imitate the facial expressions of the human being. Babies can do this too, probably by using the "mirror neurones" discovered in the 1990s: 14.vii.c. And Scassellati, now at Yale, is building Nico, a creature that will recognize and respond to gestures as well as speech. This is no mere trivial toy. Like his medical funders, Scassellati hopes to use Nico to identify, and perhaps even to cure, early childhood autism: see 7.vi.f. Indeed, he hopes eventually to be able to use humanoid robots to *test* theories in developmental psychology, much as computer simulations are so used today.)

Given that description, this work may sound reminiscent of the "social" AI described in Chapter 7.i.c. But its methodology was very different. For Brooks's team avoided top-down control, symbolic computation, planning, and objective world modelling. In short, the GOFAI approach was being turned on its head: *situated action*, including social interaction, was the buzz phrase (see 13.iii.b).

Predictably, there was resistance. That is, there was theoretical resistance (13.iii.c). Behavioural resistance was less predictable. On encountering Cog, and despite its highly *non-human* appearance, people tended to engage with it as though it were human—or anyway, animate. The key feature seemed to be the robot's ability to catch the human's eye and maintain mutual gaze and/or attention.

This animistic response was even stronger with respect to Kismet, a Cog-like creature equipped with floppy pink ears, large blue eyes, and an amazing set of eyelashes (Breazeal and Scassellati 2002). The robot's ears, eyes, and eyelashes could be put into different physical positions, spontaneously interpreted by people as sadness, anger, tiredness, interest, excitement, happiness, or even disgust. With the addition of "needs" and associated feedback signals to its carers, Kismet was able to trigger still more of the emotional responses normally elicited by (and evolved for) vulnerable fellow humans. The carers even lapsed into 'motherese', the syntactically and intonationally distinct dialect that mothers use when speaking to babies and infants (see 9.vii.c).

Even Sherry Turkle—not only a trained psychoanalyst, but a long-time colleague of the MIT AI community (and ex-wife of Papert)—reacted in that way. She didn't need Kismet or Leonardo to make her do so, for the more primitive Cog sufficed:

> Cog's mobile torso, neck, and head stand on a pedestal. Trained to track the largest moving object in its field (because this will usually be a human being), Cog "noticed" me soon after I entered its room. Its head turned to follow me and I was embarrassed to note that this made me happy. I found myself competing with another visitor for its attention. At one point, I felt sure that Cog's eyes had "caught" my own. My visit left me shaken—not by anything that Cog was able to accomplish but by my own reaction to "him". For years, whenever I had heard Rodney Brooks speak about his robotic "creatures", I had always been careful to mentally put quotation marks around the word. But now, with Cog, I had found that the quotation marks disappeared. Despite myself, and despite my continuing skepticism about this research project, I had behaved as though in the presence of another being. (Turkle 1995: 266)

Turkle's scepticism was based largely on postmodernist philosophical grounds. The same was true of her MIT colleague, the feminist philosopher of science Evelyn Fox Keller, who queried the "human" responses to Kismet (Fox Keller forthcoming). Others looked askance at Cog for different reasons.

Quite apart from scepticism over the "consciousness" of the humanoid robot envisaged by Brooks, many felt that he'd failed to learn the lesson that both Lashley (1951*a*) and Noam Chomsky (1959*b*) had tried to teach Burrhus Skinner: that S–R reflexology isn't enough. In particular, Brooks's claim that representations are unnecessary for intelligence—and especially for human intelligence—was soon challenged (13.iii.c). The disagreement eventually led to a better understanding of the diversity of mechanisms that can be called "representations" (14.viii).

The proof of the pudding . . . ? Some ten years after its start, Cog is now virtually on ice (for a recent account, see Brooks *et al.* 2000). It was always a 'spare-time' project, and most of the original team—then, graduate students—have left MIT to take jobs elsewhere. (Brooks himself has been swamped by administration since becoming director of the AI Lab.) A comparable project, well funded by Sony, has begun under the direction of Luc Steels (1952–) at the Free University of Brussels. Their pudding may turn out to have a more satisfying taste.

So, too, may the concoction due to Alan Mackworth's group, in their study of robot soccer (Sahota and Mackworth 1994). Unlike some workers in the area (Chapter 11.iii.b), they're guided by a generally important theoretical aim. Namely, they want to explore the need for, and the possibility of, an *integration* of classical and situated robotics. Describing the first as the Hegelian "thesis", and the second as its "antithesis", they offer an architecture for *reactive deliberation*—seemingly, an oxymoron. Their robots integrate sensory perception, real-time decision making, planning, plan recognition, learning, and coordination (11.iii.b).

Less 'sexy', but interesting nonetheless, is the recent robotics work based on insect navigation. To mention just one example (similar research is being done at Sussex), a team that includes Brooks's ex-student Maja Mataric (1965–) is modelling the way-finding strategies of desert ants, so that their robots will be able to return to 'base camp' after going out to explore unfamiliar territory (Roumeliotis *et al.* 2000). This project was inspired by Collett's discovery that desert ants, unlike their more familiar cousins, don't use pheromones to find their way home (Collett *et al.* 1998). Rather, they rely on path integration, backed up by visual landmarks. The landmarks are needed because if integration were used alone, tiny errors would accumulate and eventually lead the creatures astray.

In sum, the Brooksian dogma—though fruitful at its inception—is looking less and less persuasive. Nevertheless, his advice that roboticists should study insects is still being followed, if often interpreted in a broader sense.

b. New philosophies of biology

The 'biological' view of intelligence wasn't restricted to AI, for it had already developed within other areas of cognitive science.

In psychology, James Gibson's (1966) ecological theory was the outstanding example (see Chapters 5.ii.c and 7.v.e). Although it had been regarded, not least by Gibson himself, as radically opposed to AI, it was now beginning to influence AI research. Recent computational work on animate vision, for instance, had assumed that the creature's own movements through its environment provided crucial perceptual information (Ballard 1989, 1991).

Neuroscientists had discovered sensori-motor circuits illustrating the close coupling between animal and environment. And computer models of such systems had been developed since the late 1960s by CNE researchers, as we've seen. Moreover, sophisticated models of self-organization in mammalian brains had been developed by Stephen Grossberg, Christoph von der Malsburg, and Andras Pellionisz and Rodolfo Llinas (see Chapter 14.vi and viii.b–c).

Developmental psychology and cognitive neuroscience had indicated the close epigenetic interplay between inbuilt dispositions and experience (Chapters 12.viii.c–e and 14.viii.c). And some linguists had attributed fundamental aspects of both syntax and semantics to their grounding in our material embodiment (Boden 1981*a*).

Philosophy, too, was involved. Non-Cartesian accounts of mind, stressing embodiment and rejecting the representational theory of perception, had long been available within the neo-Kantian tradition (see Chapter 2.vi). And they'd featured in the philosophy of cognitive science since the 1960s, when Hubert Dreyfus first put forward his critique of GOFAI (11.ii.a and 16.vii.a). He'd drawn heavily on the ideas of the continental phenomenologists Maurice Merleau-Ponty and Martin Heidegger, and of the later Wittgenstein.

Dreyfus's critique had initially been dismissed by most cognitive scientists, whose work lay within the alternative, empiricist, tradition (see 11.ii.a–b). When Terry Winograd was converted by Dreyfus (and Fernando Flores) in the early 1980s, this was widely seen as a betrayal by one of the masters of GOFAI (11.ii.g). In the 1990s, these ideas were stirring again. Heidegger, for instance, was becoming prominent in certain philosophers' thoughts about cognitive science—including some highly sympathetic to situated and evolutionary robotics (van Gelder 1992/1995; M. W. Wheeler 1996, 2005).

For many years, Heidegger had been regarded by analytical philosophers with utter contempt. Hardly any of them bothered to say anything about him—although Anthony Quinton complained about his "ponderous and rubbishy woolgathering" (a judgement he still stands by: personal communication). At the millennium, however, the heat was turned up. Heidegger was called "a dismal windbag, whose influence has been completely disastrous" (Blackburn 2000), and "the greatest catastrophe in the history of philosophy" (Edwards 2004: 9).

Both those remarks were made by highly eminent analytic philosophers, disturbed (especially in Edwards's case) by the fact that by the end of the century, Heideggerian ideas had spread apace. Just why he was now thought by some respectable Anglo-Saxon philosophers, and by some cognitive scientists, to be relevant is discussed in Chapter 16.vii.

Besides downplaying the mind's embeddedness in the physical world, Cartesianism had encouraged analytic methods in biology (see Chapter 2). Not until after the mid-twentieth century was there a wide range of scientific studies providing ideas about self-organization. By the late 1980s, a number of people had developed holistic philosophies of life informed by these scientific advances.

Pattee, a participant at the first A-Life meeting (see Section x.a), was an influential case in point. He'd always drawn on both cybernetics and biology. In the 1960s he'd edited a collection of early papers on computer and mathematical models of life (Pattee *et al.* 1966), and soon simulated an ecosystem himself (see Section vi.b). But his main project was to articulate a philosophy of life.

He described organisms as self-organizing dynamical systems realized in a physical medium (Pattee 1985, 1989). And he was especially concerned to show how stable "symbols"—in particular, genes—could arise within dynamically changing physical systems (2001). He saw this symbolism as the necessary ground for the genotype–phenotype distinction, and indeed for the phenomenon of life itself.

Others with similar views included two of Pattee's students: Robert Rosen (1985, 1991) and Cariani (1992). (Rosen was initially a student of Nicholas Rashevsky, whose Chicago biophysics group gave him a lasting interest in metabolism as the basis of life.) All three rejected the Cartesian view of organisms as "mechanisms"—that is, as analytically decomposable systems. They allowed that computer simulations of the self-organizing dynamics involved can be useful. But they insisted that these must be very different from symbolic AI models. In particular, the emergence of new dynamical wholes, and of new types of function (such as new powers of perception), are problematic for traditional AI.

The emergence of new dynamical wholes was the key concept in an unorthodox philosophy of biology (and cognition) that had been maturing for thirty years. It was developed by the Chilean neuro-anatomist Maturana (1928–) and his student, a Chilean psychologist, Francisco Varela (1946–2001). Although originated by Maturana, the theory—and the A-Life work inspired by it—was eventually associated just as strongly with Varela (Di Paolo 2004; McMullin 2004).

Maturana had been a member of the cybernetics group in the 1950s, and co-authored the hugely influential 1959 paper 'What the Frog's Eye Tells the Frog's Brain'. In the 1960s, he defined both life and cognition in terms of "autopoiesis": the continuous self-production of an autonomous entity (Maturana 1969; see 16.x.c).

The word was coined from the Greek: *autos*, or self, and *poiein*, or creation. But the idea, as Varela acknowledged later (Weber and Varela 2002), had been largely inspired by Kant. Specifically, it was grounded in Kant's vision of organisms as systems in which the parts are formed for and from the others, giving a dynamic whole without which the existence of the parts would be meaningless (see Chapter 2.vi.b).

This 1960s biological approach advanced steadily, though always in the shadow of the mainstream. The 1970s saw an account of autopoiesis appear in the *International Journal of Man–Machine Studies*—hardly a neo-Kantian enterprise (Maturana 1975). And the same decade produced the first computer simulation of *the* fundamental autopoietic event: the spontaneous formation of the cell membrane (Zeleny 1977, 1981; McMullin and Varela 1997). Now, in the mid-1980s, Maturana's ideas had been included in a highly respected series on the philosophy of science (Maturana and Varela 1972/1980). (Soon, they would be applied to cognitive science as such: Varela *et al.* 1991.)

The theory of autopoiesis was fundamentally opposed to non-holistic, logical–computational accounts of life and mind. This may seem strange, for the 'Frog's Eye' paper had been prompted by computational ideas, and had led to their wider use in neurophysiology (Chapter 14.iv). But in his mature philosophy, Maturana regarded artefacts of any sort, and especially of the logical–computational variety, as misleading analogies of living things.

Even admittedly convenient terms such as *input, output*, and (ironically) *feature-detector* were discouraged. Talk of *representations*, too, was to be avoided. Instead, one should conceptualize the brain as a dynamical living system, wherein stimuli aren't "inputs" but "perturbations". As his colleague Varela put it:

> The LGN [lateral geniculate nucleus] is usually described as a "relay" station to the cortex. However . . . most of what the neurons in the LGN receive comes not from the retina (less than 20%), but from other centers inside the brain. . . . What reaches the brain from the retina is only a gentle perturbation on an ongoing internal activity, which can be modulated . . . but not instructed. This is the key. To understand the neural processes from a nonrepresentationist point of view, it is enough just to notice that whatever perturbation reaches from the medium will be informed according to the internal coherences of the system. (Varela 1987: 59–60)

Maturana wasn't the only neuroscientist to object to informational/computational terminology. Jean-Pierre Changeux, for instance, saw it as a mistake to describe brain processes as being determined by the sensory input:

> [Variations] in a physical parameter in the environment are translated into variations in nerve impulses . . . A chain of successive reactions, explicable in strictly physico-chemical terms, *regulates the spontaneous activity of the oscillator, which pre-exists all interaction with the outside world*. The impulses produced are therefore *independent from the physical stimulus* to which the organ is sensitive. The sense organs behave like *regulators* of molecular clocks. The stimulation that they receive from the outside world sets them forward or backward and corrects their timing. *There is no direct analogy, however, between the physical stimulus received from the environment and the nervous signal produced*. (1985: 82–3; italics added)

In other words, Changeux (like Maturana) favoured a dynamical, even 'non-mechanistic', account of the brain, while allowing that brain processes could be described at the physico-chemical level if one wished.

But Maturana went beyond neuroscience, to develop a philosophy of biology in general. He objected to descriptions of the nervous system as "mediating between perception and action". Instead, the living autopoietic unity—cell or organism—was conceptualized as a self-organizing dynamical system, closely coupled with its environment.

So closely coupled, indeed, that they could even be regarded as a single system. For Maturana, the "environment" isn't objectively present independently of the organism. Rather, it's constituted in opposition to and cooperation with the activity of the living thing. What the observer chooses to count as 'one' system is a matter of scientific convenience. In the 1990s, some philosophers of cognitive science would draw on such ideas to smear the boundaries of the 'mind'/'self' in a similar way (Chapter 16.vii.d).

As for the computer modelling of autopoiesis, this too became more prominent in the 1990s, though still a minority taste (McMullin 2004: 283–92). Increasingly, it was pursued in the context of biochemical theories.

For example, Naoki Ono and Takashi Ikegami in Japan developed an approach called LAC, or Lattice Artificial Chemistry. They employed abstract chemical schemas to generate "proto-cells" showing boundedness, self-maintenance, reproduction, and evolution. Initially, they could deal with only one or two dimensions (Ono and Ikegami 1999, 2000, 2001); later, they modelled a 3D lattice (Madina *et al.* 2003).

Their experiments would start from an assortment of randomly moving (abstract) molecules: some hydrophilic, some hydrophobic, and some autocatalytic. Given parameter values within a certain range, structures such as vesicles would emerge, and spontaneously split into two. The authors see this as a study of the origins of life, which must have involved molecules in the primeval soup coming together into "compartments" somehow separated from what was now the first "environment". This sort of self-organization is the crux of autopoietic theory.

c. Dynamical systems

It's not surprising that Maturana's work received a more sympathetic hearing in the late 1980s than it had done in the preceding years. For by that time, more people were avoiding talk of representations and/or studying self-organizing dynamical systems.

For instance, PDP-based connectionism ascribed meaning to holistic patterns of activation, not to individual units (Chapter 12.vi). A few neuroscientists and psychologists were starting to explain brain and behaviour in terms of dynamical attractors (14.ix.b). Some 'neurological' work in CNE (R. D. Beer 1995*a*,*b*) was interpreted using dynamical systems theory. The evolution of the robot orientation detector (Section vi.c, above) was described by the research team in terms of state space attractors, not input–output functions (P. Husbands *et al.* 1995). And some philosophers were arguing that dynamical, not computational, concepts are needed to explain cognition (van Gelder 1992/1995; Port and van Gelder 1995).

A dynamical system is one whose states change according to definite rules. The rules may be deterministic or probabilistic; linear or non-linear; fixed or alterable; uniform or region-/unit-specific; and time-sensitive or synchronously "clocked". A self-organizing dynamical system is one in which new forms of order spontaneously emerge.

A von Neumann computer is a dynamical system, although it's rarely described as such (Giunti 1997). So is a pendulum, whose system dynamics are relatively simple. In addition, the term is commonly applied to massively parallel systems, where it's easier to identify overall patterns of activity than to keep track of individual units. Often, these patterns are stable or recurrent, in which case they are described in terms of attractors. An attractor is a point, region, or state cycle within the phase space (the set of all possible states) of a dynamical system, such that the system tends to approach it from any randomly chosen starting point and to return to it when perturbed.

Dynamical systems go under various names. They're called circular causal systems, reaction–diffusion systems, neural networks, cellular automata, NK networks, and CTRNs (Continuous-Time Recurrent Networks). And they even include gases.

At the most fundamental level, all these are similar (although the defining rules are different):

* That's why von Neumann's CAs could be used to model the laws of physics (see Section v.a),
* why CTRNs can model any dynamical system (see xi.b, below),
* and why thermodynamics can be used to specify connectionist networks (see 12.v.f and vi.b).
* It's why Ashby could include plants growing, plants ageing, and machines moving—as well as brains living—as examples of dynamical systems (Chapter 4.viii.c).
* And it's why D'Arcy Thompson's and Turing's claims about the formative powers of physics and chemistry count as early studies of the dynamics of development.

By the late 1980s, dynamical systems were being studied by many different disciplines. Some of the authors were biological scientists trying to explain specific phenomena. Such phenomena included the brain's control of (and switching between) different bodily gaits (Thelen 1985; Thelen and Smith 1994), the coupling of organism and environment by way of movement (Kugler and Turvey 1987, 1988), and the evolution of new systems of perception, or "measurement" (Kugler *et al.* 1990; Turvey and Shaw 1995). Others were physicists, mathematicians, or computer scientists seeking abstract descriptions of dynamical systems in general. (Some interesting recent examples are discussed in the following subsection, and in Sections ix.a–b and xi.)

Whether the growing sympathy for dynamical systems was entirely merited is controversial. It's not clear, for example, that theories couched in terms of dynamical systems can explain all the specifics of human thinking. It's one thing to say that the mind/brain is a dynamical system (compare: made of atoms). It's quite another to say that dynamical concepts (compare: atomic theory) suffice to explain everything that it does.

This isn't a question of accepting magic, but of rejecting reductionism. Even chemistry requires concepts not needed in physics; and genetics, neurophysiology, and psychology require yet more (see Chapter 16.iv.c). Nor is it a question of the *philosophical* problem of meaning, or intentionality. Even if one takes for granted that a dynamical system could express meanings and/or behave meaningfully, it doesn't follow that the mathematical concepts of dynamical systems theory suffice to represent structured linguistic propositions or inferences.

In other words, this is a current version of the problem that mid-century cybernetics had with propositional content (see Chapters 4.v.d and ix.b, and 16.vii.c).

15.ix. Order and Complexity

The emergence of order in cellular automata had been investigated for many years before the naming of A-Life, both at Michigan (see Sections v–vi) and elsewhere. A group at MIT's computing laboratory, directed by Ed Fredkin, had been working on CAs since the 1970s, and an ex-Michigan member had recently outlined a dedicated CA machine (Toffoli 1984). But especially interesting work of this type had been done by Stephen Wolfram (1959–) and, in particular, Stuart Kauffman (1939–).

Both Wolfram and Kauffman used CAs to explore complexity—including life—*as such*. If it had any interest at all, then, their work was relevant to a huge range of topics, both within biology and outside it.

a. The four classes of CA

By the late 1980s, the computer scientist Wolfram was extending the study of cellular automata in various ways. He'd noted that even very simple CAs could generate lifelike visual patterns, such as shell markings (see Section v.a), so might be relevant in explaining specific biological phenomena. But he also used CAs as grounds for claiming that a certain level of complexity, involving both order and novelty, is needed for life in general—and for computation, too (Wolfram 1983, 1984*a*,*b*, 1986).

Taking advantage of the newly available computational power, Wolfram modelled many different CAs, and observed them in action. On the basis of these experiments—experiments, not theorems—he suggested a classification into four classes (a classification anticipated in outline by Turing in 1950: see 16.ii.a).

Wolfram's four classes of CA were: I—those which eventually reach stasis; II—those which settle into rigidly periodic behaviour; III—those which remain forever chaotic; and IV—those which achieve order that's stable without being rigid, so that the structural relations between consecutive states are varied yet intelligible. The final category, he said, included life.

Later, in the early 1990s, Langton would add numbers to this qualitative classification. He did extensive, and hugely tedious, CA modelling to explore the conditions in which each of Wolfram's four classes would in fact emerge. And he came up with an intriguing result.

His experiments led him to claim that life is possible only within a very narrowly restricted range of numerical values on a simple statistical measure of complexity, which he called the "lambda parameter". This type of complexity was located "at the edge of chaos": near the phase transition between Wolfram's Class IV and his Class III (Langton 1990, 1992). As Langton pointed out, this means that there's not only a lower limit to the complexity of living things—as von Neumann had argued in the 1940s—but an upper limit also (Langton 1990: 36–7).

Another person inspired by Wolfram's classification of CAs was Andrew Wuensche (1943–). Independently of Langton, Wuensche defined a different measure of complexity, the Z-parameter (Wuensche and Lesser 1992; Wuensche 1999). This was a natural extension of his remarkable "reverse algorithm", developed in the late 1980s for computing the *predecessors* of CA states—which in effect provided CA systems with "memory" (Wuensche 1997). (You may remember that McCulloch and Pitts had argued that it's in principle impossible to describe neural nets retrospectively: see 4.iii.f.) When the two complexity measures were compared, it turned out that there was a strong relationship between them. But the Z-parameter was the more general, being able to avoid certain exceptions to lambda.

Largely because Wolfram's qualitative classification of CAs had whetted people's appetite for quantitative measures of complexity, both Langton's work and Wuensche's attracted significant attention.

In fact, however, the cyberneticist Ashby had beaten them to it. He and a colleague, Crayton Walker, had defined a broadly comparable measure over twenty years earlier (Walker and Ashby 1966). Their concept of "internal homogeneity" was equivalent to Langton's lambda, for CAs having only two possible states. In the absence of a flourishing CA modelling community, however, this earlier theoretical approach hadn't been much noticed.

b. K for Kauffman

Even before Wolfram and Langton, Kauffman had investigated the nature and limits of complexity with various biological questions in mind. His first papers had appeared in the late 1960s, when he was a young member of the mathematical biology group in Chicago.

Twenty years later, his work was a provocative mix of mathematics, biochemistry, and biology—integrated by a highly unfashionable, though ancient, philosophical framework. For some readers, it was intoxicating. For others, infuriating. Mainstream biologists still rejected the general approach, especially the downplaying of natural selection (see below). They also questioned the fit between Kauffman's mathematics and biological fact.

Kauffman was originally trained in philosophy, physiology, and psychology (his teachers included Stuart Sutherland and McCulloch). He then entered medical school.

His interests at that time (the early 1960s) were in brain function and cell differentiation in the embryo.

The then current research on neural functioning was largely inspired by the 1940s models of McCulloch and Pitts, which represented neurones as Boolean on–off units (see Chapters 4.iii.e and 12.i–ii). As for cell differentiation, this still wasn't understood. Turing's ideas on diffusion reactions (see Section iv.a) remained speculative and non-specific. However, the recent discovery of regulatory genes (Jacob and Monod 1961) suggested the possibility that embryonic development might be directed by specific genes becoming active at successive stages. Since the role of regulatory genes is to 'switch' other genes on or off, Kauffman decided to use Boolean networks to model both kinds of activity.

He contacted McCulloch to discuss these ideas, and was introduced by him to the developmental biologist Goodwin (1931–), with whom he would later co-author several papers. Goodwin was a student of Waddington at the time, and through him Kauffman was invited to speak at Waddington's seminar (Kauffman 1969). There could have been no more suitable a context for using mathematical analysis to illuminate epigenesis.

In these studies of the metabolic base of cell differentiation, Kauffman was investigating the action of what Turing had called morphogens. But his questions, like Turing's, were very general ones. He didn't ask which protein causes a liver cell to develop, and which produces a neurone. Rather, he wanted to know (for example) why the morphogens, whatever they happen to be, generate 264 distinct human cell types—as opposed to 100, 1,000, or even millions. (Just how one can count "cell types" is a thorny question, to which we'll return.)

His (primitive) computer models explored the behaviour of randomly connected Boolean NK networks. Such a network has N units, each with K inputs. Each unit follows some (randomly assigned) rule, which determines its on–off activity according to the (binary) values of the K inputs.

Kauffman found that NK networks often settle into, or cycle between, a restricted number of stable patterns. That is, of the many—perhaps astronomically many—possible states of the system, only a small number can be reliably maintained. Moreover, when $K = 2$ there's a simple ratio between the value of N and the number of attractors: the latter is the square root of the former.

It was then thought that the number of genes in the human genome is about 100,000 (now, it's believed there are only 30,000–60,000). Kauffman pointed out that with $N = 100,000$, there would be 317 types of stability—which, he said, approximates the number of human cell-types. (With $N = 60,000$, the number of attractors would be about 245; with $N = 30,000$, it would be 173.) In other words, it was perhaps no accident (on this view) that Santiago Ramón y Cajal had been able to find more than one type of brain cell, but fewer than 100 (see 2.viii.c).

There were three intriguing implications. First, that there may be something special about networks where $K = 2$—or (more generally) that the numerical values of N and K may be significant in determining network behaviour. Second, that these abstract properties may be instantiated in a wide variety of contexts, wherever order emerges in systems of interconnected units. And third, that the power of natural selection may therefore be limited: the number of cell-types (for instance) may depend as much on the self-organizing properties of the embryo as on the *post hoc* effects of selection. Over the 1970s and 1980s, Kauffman explored all three implications.

As for the significance of $K = 2$, Kauffman soon proved that very richly interconnected networks, where K approaches $N - 1$, produce chaos (Wolfram's Class III), not order. More surprisingly, he showed that, in an NK network of this simple type, chaos will result if K is larger than 5. In general, the precise numerical values of N and K are crucial determinants of the overall dynamics of the system.

In one sense, he admitted, both N and K are irrelevant. *All* NK networks will achieve order eventually, because they have a finite number of states. So if any state reoccurs, the preceding trajectory through the phase space must be traversed again . . . and again. But "finite" can mean astronomical, and "eventually" can mean billions of years. So there's a real distinction between NK networks that will achieve order within some reasonable time and those which won't.

What counts as a "reasonable" time depends on the context, and in living organisms will involve many different constraints. But whatever the context, the numerical values of N and K affect the system's properties as a whole. Moreover, numerically tiny differences in N or K may result in huge system differences: either a phase transition into a different type of order (analogous to a new embryonic stage, for example), or a collapse into chaos.

In his later work, Kauffman supported Langton's suggestion that life exists "at the edge of chaos". And he proved, what had long been regarded as intuitively obvious, that with increasing complexity an organized system becomes more resistant to change. As he put it, selection becomes less able to alter the system's fundamental properties (see below).

As for generalizing this approach, Kauffman was nothing if not ambitious, seeing a wide range of phenomena as essentially comparable. He first applied his ideas to autocatalytic networks (in which various chemicals cooperate in generating each other), which he thought of as primitive metabolisms (Kauffman 1969, 1971). Later, he extended them to the functioning, and the evolution, of systems defined at many different levels.

These included enzymes and antibodies; genomes; embryos; neural networks; species; ant colonies; ecosystems—and even market economies and human cultures (Kauffman 1983, 1986*a*,*b*, 1989, 1993). For instance, he used NK networks to model fitness landscapes and co-evolution, and to define the circumstances in which genetic variation and natural selection would be most likely to lead to an increase in fitness.

Kauffman's ideas influenced various groups working in A-Life, including some who used NK networks for evolving artificial species (see Section vi.c). But his seminal claims weren't all well founded.

For instance, near the end of the century Harvey showed that Kauffmann's analysis of NK networks is false in the general case. It applies only to synchronously updated random Boolean networks, not to asynchronously updated ones. For these, the picture is so different that "there is no sense in which the behaviour of synchronous [NK networks] gives any clue towards [their] behaviour" (Harvey and Bossomaier 1997: 74).

In other words, Kauffmann's work provides yet another example of a mathematician ignoring real time. Von Neumann's CAs, too, were defined synchronously (see Section v.a). The Turing machine assumed a sequence of atemporal steps, defined in practice by the digital computer's—essentially arbitrary—clock (see Chapter 4.iv). And neural network theorists have—until very recently—typically ignored real-time properties, despite their importance in actual brains. Temporal *order* might be taken into account (by Uttley or Jeffrey Elman, for instance: see Chapter 12.ii.c and viii.b). But precise temporal intervals usually weren't (14.ix.g). Only at the turn of the millennium, for instance, did a connectionist textbook pay attention to them (O'Reilly and Munakata 2000, e.g. sects. 2.5 and 6.7).

One might argue that all these mathematical idealizations are necessary in the initial stages of research, and that their results will at least approximate those of the relevant real systems. Harvey allowed the first of these defences, but not—or not always—the second. He also showed that, contrary to what was widely assumed, asynchronous networks can be both analysed and simulated (pp. 74–5). In short, while his critique cast serious doubt on Kauffman's models of genetic regulation, for instance, it also showed how his ideas could be built on in unexpected ways.

c. Morphology revived

Kauffman's work on evolution had been initially prompted by Maynard Smith, a leading evolutionary biologist. In the early 1980s, Maynard Smith persuaded Kauffman that he should no longer ignore evolution, and suggested that he combine the mathematics of fitness landscapes with his own NK approach (Maynard Smith, personal communication). But the models of evolution that ensued merely confirmed Kauffman in his commitment to a highly unorthodox conclusion—one already held by Waddington and Goodwin.

Maynard Smith and Goodwin were colleagues for many years at the University of Sussex. Their relations were warm, friendly, and highly respectful on both sides. (We were members of a small dining club, so I witnessed them interacting many times.) Despite the mutual respect, however, their philosophical commitments were, and remained, fundamentally different.

Discussing the improbability of a species' settling on a high-but-narrow peak in the fitness landscape, Kauffman declared:

> Selection typically cannot reach and hold an adapting population in arbitrarily located or overprecise volumes of parameter space. Thus it is not foolish to suppose that the forms we see are largely those which are easily generated by the underlying developmental mechanisms. (Kauffman 1993: 642)

As for what is "easily generated" in development, he said:

> [My work, and Goodwin's, suggests that], contrary to intuition, morphogenesis may be deeply robust. Organisms, rather than being tinkered-together contraptions, may exhibit a nearly inevitable and stable order. Real morphogenesis is due not to the unfolding of any single developmental mechanism but to the beautifully ordered unfolding in time and space of some richly integrated combination of simpler mechanisms such as cell-sorting, sheet-folding, positional discontinuities, and reaction–diffusion mechanisms. . . . [These] *constrain* the morphologies which emerge to a small subset, each of which occupies a large volume of state space and parameter. Rather than causing complexity, integration of developmental mechanisms may generically yield simplicity and order. (Kauffman 1993: 637)

In short, natural selection is important, but not fundamental. If we want to know why certain imaginable morphologies have not appeared, even though they are represented at some point in the fitness landscape (the co-evolutionary phase space), the answer will sometimes be that fundamental developmental constraints prevent it. Like snowballs in hell, they are theoretically conceivable but dynamically impracticable.

This view was stated even more strongly in the concluding passages of Kauffman's *magnum opus*, *The Origins of Order* (1993):

> This book is an effort to continue in [D'Arcy] Thompson's tradition with the spirit now animating parts of physics. It seeks origins of order in the generic properties of complex systems. Those properties [I have] discussed have ranged [over many levels of biological self-regulation, including] the origins of spatial integration in multicellular systems when products of individual cells can reach their neighbours . . .
>
> I have tried [to characterize] the interaction of selection and self-organization. To some great extent, evolution is a complex combinatorial optimization process in each of the co-evolving species in a linked ecosystem, where the [fitness] landscape of each actor deforms as the other actors move . . . In sufficiently complex systems, much of the order seen in organisms is precisely the spontaneous order in the systems of which we are composed. Such order has beauty and elegance, casting an image of permanence and underlying law over biology. Evolution is not just "chance caught on the wing". It is not just a tinkering of the ad hoc, of bricolage, of contraption. It is emergent order honored and honed by selection. (Kauffman 1993: 644)

The reference to tinkering was a direct challenge to François Jacob, the co-discoverer of regulatory genes. He had famously described evolution as *bricolage*, or tinkering (Jacob 1977).

As if such downplaying of natural selection weren't maverick enough, Kauffman "continued in D'Arcy Thompson's tradition" even to the extent of paying homage to Goethe as well as to Kant. Many biologists had been entranced by D'Arcy Thompson's writing, as we've seen, but most readers had ignored his praise of Goethe and Kant. Even in the late eighteenth and nineteenth centuries, their holistic approach to biology had been scorned by most empiricists—though not, or not entirely, by Hermann von Helmholtz (see 2.vi.e). Now, Kauffman was resurrecting these long-dead philosophers.

His unfashionable remarks about them weren't mere fleeting asides. To the contrary, they were rhetorically prominent declarations of biological faith. The opening pages of his most important book expressed his kinship with the pre-Darwinian tradition of rational morphology; and the Epilogue began by praising Kant's philosophy of organisms (Kauffman 1993: 3–5, 643). As we saw in Chapter 2.vi.e–f, the rational morphologists had seen organisms as holistic self-organizing systems, shaped according to (unspecified) universal laws of form—an a-historical approach that was to be suddenly eclipsed by Darwinism.

Kauffman's appreciation of this outmoded theoretical tradition was largely due to his interactions with Waddington and Goodwin. Waddington had tried to combine D'Arcy Thompson's developmental approach with evolution and genetics. But his ideas about epigenetic landscapes were uncomfortably vague, and his concern with whole organisms was overshadowed by molecular biology. Eventually, his student Goodwin offered a theory (of developmental transformations within morphogenetic fields) that was better grounded in experimental data and more mathematically precise than Waddington's (Webster and Goodwin 1996, pt. II).

Goodwin identified even more strongly with the rationalist tradition in biology than Waddington had done. His major book took its motto from Goethe, and devoted many pages (written by his colleague Gerry Webster) to a philosophical discussion of these ideas (Webster and Goodwin 1996: vii, pt. I).

In his early research, Goodwin had used cognitive and informational concepts to try to capture some of the high-level, holistic features of organisms (Goodwin 1974, 1976; Boden 1981*b*). Later, and especially in his research with Kauffman, he relied more on dynamical mathematics. As an experimental biologist, he was closer to the actual data than Kauffman.

Kauffman's mathematical work on fitness landscapes, for example, showed that fundamental changes in body plan will become less probable as development or evolution proceeds, and that certain transformations will be possible (certain attractors available) only on certain trajectories. Consequently, certain types of organ will tend to be found together, in recognizable morphological groups (Kauffman 1993, ch. 6). But Goodwin asked just which transformations actually occur, and how they come about.

In an early discussion of slime moulds, for instance, he pointed out that a single chemical may have very different morphogenetic "meanings" in different developmental contexts (Goodwin 1976). And his later work hypothesized specific transformations and morphogenetic fields in a wide range of examples, from slime moulds to the pentadactyl limb (Webster and Goodwin 1996, pt. II).

Just as computational psychologists need to test their models against the empirical data, so mathematical biologists need to do this too. Accordingly, Kauffman sought evidence from various areas of biology to confirm his mathematical analyses. When discussing the significance of very low values of K, for example, he remarked that each gene or active molecule in living organisms (from bacteria to primates) appears to be directly regulated by only a few other molecules—often, as few as two. In general, his book referred to a large number of biological studies in support of his approach.

However, the evidence wasn't always available, nor even consistent. His remark just cited, for instance, is questionable: many genes can interact directly with at least ten others. If chaos theoretically ensues when K is greater than 5, this fact is an embarrassment. So is the fact that individual neurones may receive thousands of inputs from others, yet the brain generates highly ordered cognition. Again, his claim that *Drosophila* segmentation can be explained by reaction–diffusion models inspired by Turing (Kauffman 1993: 566–600) is disputed (see Section iv.b). And as for counting cell-types, it's not obvious that the best way to classify cells, as natural kinds, is by their morphological properties, as is done in histology textbooks (and by Ramón y Cajal): hidden properties, programmed in by the genes, may be just as important (Maynard Smith, personal communication).

Maynard Smith, for instance, allowed that Kauffman's work on autocatalytic protein networks perhaps throws light on the origin of life (Maynard Smith and Szathmáry 1995: 69–72). But he also argued that many questions about evolution call for more biological data, not more theoretical models. On his view, computer models of actual genetic systems are likely to be more useful than simulations of evolution in general (Maynard Smith 1996).

In short, sceptics often complained that Kauffman's models (and other A-Life abstractions, such as Ray's), though admittedly intriguing, were too far from the biological coalface to be really useful. This opposition was stoked by Kauffman's provocative views on natural selection, and his 'old-fashioned' philosophical holism.

d. Discussions in the desert

Not everyone was situated at the biological coalface, however. Many people remembered that, although a real neurone is very different from an abstractly defined McCulloch–Pitts unit (see Chapter 14.ii), McCulloch's simplification had inspired important empirical research. So, too, might Kauffman's. And for some readers, his dynamical approach seemed to offer a way of thinking about self-organization in many different disciplines besides biology.

That's why, in 1985, Kauffman was invited to join the Santa Fe Institute (SFI), in the research group on Complex Systems. SFI had been set up one year before, by a group of physicists and mathematicians at nearby Los Alamos. Their research focus was on non-linear dynamical systems.

Following in the footsteps of the physicists Schrödinger (1887–1961) and, especially, Ilya Prigogine (1917–2003), they asked how fundamental physical principles, concerning entropy and equilibrium, can allow—or even give rise to—self-sustaining order, especially biological order. Schrödinger (1944) had argued that living things survive not thanks to food, but thanks to negative entropy. And Prigogine himself had described his Nobel-winning work on "dissipative structures"—irreversible far-from-equilibrium processes—as a study of the "self-organization" of matter (Prigogine *et al.* 1969; Prigogine and Stengers 1984).

For instance, he'd shown that a heated liquid may form rotating "convection cells", patterns of physical activity that hugely outstrip the range of the intermolecular forces that cause them. It may be that he called this "self-organization" because he spent a good deal of time with the embryologists in Brussels (Dupuy 2000: 130). If so, biological morphology may have been close to his mind even when he was doing 'pure' thermodynamics.

The group at Los Alamos also worked on the theories of chaos and complexity. As Kauffman's work on NK networks had illustrated, these dynamical phenomena are intimately related. They'd been studied within mathematical physics since the mid-1960s, but had been recognized by the wider scientific community only recently—partly as a result of the activities of the Los Alamos group (Waldrop 1992; Kellert 1993). It was in light of this historical context that Kauffman had described his own work as "an effort to continue in [D'Arcy] Thompson's tradition *with the spirit now animating parts of physics*" (1993: 644; italics added).

The Los Alamos physicists encouraged a widely interdisciplinary approach. Their Center for Nonlinear Studies had initiated a series of conferences on various aspects of self-organization. One of these, focusing on emergent order and distributed computation, included non-physicists such as Holland, Kauffman, Hillis, Douglas Hofstadter, Stephanie Forrest, Stevan Harnad, and Paul Churchland (Forrest 1991). The new institute, SFI, was intended as a focus for interdisciplinary research concerned with complex systems *in general*.

One early SFI activity was a joint physics–economics workshop, held there in 1987. Two of the economists involved (Kenneth Arrow and Brian Arthur) had long stressed the non-linearity of economic systems—something ignored by classical economics. But that's not to say that all was plain sailing when these matters were discussed. The initial physics–economics culture clash has been tellingly described by Mitchell Waldrop (1992, ch. 4). Although this meeting ended fruitfully, even pleasantly, some later SFI workshops "degenerated into shouting matches and sulking" (p. 143). Interdisciplinarity isn't always easy—or good-tempered.

In short, the personnel at SFI were nothing if not open-minded. Over the next decade, they welcomed colleagues in physics, mathematics, computer science, AI, neuroscience, biology, psychology, economics, and philosophy. All were invited to discuss the emergence of order in complex systems. Many chose to focus on aspects of life and cognition.

As so often happens, the open-mindedness of the early days eventually became muted. By the end of the 1990s, both Kauffman and Langton had been sidelined by SFI, apparently for speculating too wildly (C. J. Langton, personal communication). The puzzle of life had been replaced in SFI's affections by puzzles about predicting the stock market.

15.x. Naming and Synthesis

A-Life was born healthy at mid-century, as we've seen. But the pattering of its tiny feet caused little disturbance. The infant field wasn't even mentioned for over thirty years. Some of its core *topics* were widely discussed, to be sure—for example, in Hofstadter's hugely popular *Gödel, Escher, Bach* (see Chapter 12.x.a). And the Media Lab's Vivarium (a simulated aquarium), alongside its work on animation, was attracting funding from Hollywood and computer manufacturers, and drawing huge numbers of visitors (13.v.a). But artificial life as such was invisible. Then, suddenly, it seemed to be everywhere.

The suddenness was due to a public act of naming in 1986, and a christening party in 1987. Before then, the term "A-Life" didn't exist. The seeming ubiquity was due to the wide range of disciplines represented at the party, and to media interest prompted by the infant's name—even more outrageous, evidently, than Fifi Trixibelle.

a. The party

Studies of self-organization prior to the 1990s had many underlying similarities, as we've seen. But most people at the time were aware of the links only dimly, if at all. Indeed, they were largely ignorant of each other's activities.

One wouldn't expect ethologists, for instance, to have come across Craig Reynolds's paper on flocking, published in the journal of *Computer Graphics* (Reynolds 1987). His model showed that the group behaviour typical of flocks of birds or schools of fish can emerge from three simple rules—so simple, that they might conceivably be 'hardwired' into animals' brains. Computer animation experts soon picked up on this work. It was used, for example, to generate the lifelike 'schools' of virtual fish described at the outset of this chapter, and to choreograph the flocks of bats in the film *Batman*. But people interested in real fish, and how they manage to do what they do, were hardly likely to have *Computer Graphics* on their reading lists.

This isn't an isolated example. Much of the relevant research was inaccessible to potentially interested readers, because of disciplinary boundaries.

The unification (such as it is) occurred in the late 1980s, when the term A-Life was coined by Langton. He first used it in print in 1986 (Langton 1986). But he'd thought of it in 1985, during a meeting organized at Los Alamos by the physicists Doyne Farmer and Norman Packard, and by the mathematicians Buton Wendroff and Alan Lapedes (Rasmussen *et al.* 2003*b*: 210). The official title of that meeting had been a mouthful: 'Evolution, Games, and Learning: Models for Adaptation in Machines and Nature'. The term *artificial life* was more pithy, more unifying—and, of course, much more provocative.

Langton was a Los Alamos physicist, especially interested in the relation between complexity and life. He had a wide intellectual background, and an iconoclastic attitude to disciplinary boundaries (Levy 1992: 93–103; Kelly 1994: 343–6). This would have counted against him in most academic institutions, but was no disadvantage in Los Alamos. The physicists there had highly interdisciplinary sympathies, as we've seen.

By 1986, Langton was embarking on his painstaking research involving the lambda parameter (see Section ix.a). But he'd already completed a number of important studies in what he now decided to call A-Life.

After working with Burks at Michigan, he'd recently achieved the first implementation of a self-replicator (Langton 1984). (Arbib's 1966 design for a self-replicating CA, mentioned in Section v.b, wasn't implemented.)

He'd also produced various models of ant trails (Langton 1986: 135–6). He showed that a disorganized group of ants, each one following a very simple rule, could give rise to many different trails (e.g. circles, spirals, straight lines . . .). Tiny rule changes could result in observably different larger-scale phenomena. And these phenomena were reminiscent of real insects. For instance, ants dropping pheromones as they walked could eventually converge on a small number of (relatively straight) paths leading to the best food sources.

This was a purely abstract study: Langton was no biologist. Later, however, others would use real-life data in modelling the effects on ant trails of different chemical concentrations, walking speeds, and antennae spans—another example of CNE (Sharpe and Webb 1998). Indeed, Langton's paper led A-Lifers to a general interest in "stigmergy", wherein social behaviour is mediated by individuals responding to environmental changes caused by other individuals (Bonabeau 1999). This phenomenon was named by a French biologist in the 1950s, and many empirical data had been collected. Now, biologists had the possibility of analysing it in rigorous terms.

Mathematicians were interested, too. The notion of "Langton's ant" was generalized in the CA/A-Life community, and led to many follow-up analyses(e.g. Gale and Propp 1994; Gale *et al.* 1995; Stewart 1994; Gajardo *et al.* 2002). In addition, AI work on agents and distributed cognition sometimes drew on Langton's ideas (e.g. O. Holland and Melhuish 1999: see 13.iii.d).

Already connected with the Santa Fe Institute, Langton helped found their A-Life research group. And it was he who organized "the first workshop on A-Life". This event took place in 1987, in Los Alamos. It's remembered by many who attended as a party, even a celebration, as much as a workshop.

The invitations had been issued by Langton, who sent them not only to a wide variety of selected individuals but also to Internet newsgroups in many different disciplines. He recalls this as an anxious period: he had no idea how many people would be intrigued by his invitation, nor how many would see the point if they actually came. After the event, he described it as producing "a growing sense of excitement and camaraderie—even profound relief—as previously isolated research efforts were opened up to one another for the first time" (Langton 1989*a*, p. xvi).

Langton's memory of the meeting is doubtless coloured by his own role in it. But the workshop did catalyse an increasing level of interest, and even cooperation, between scientists from different disciplines. Beer (personal communication), for instance, now remembers it as hugely important for consolidating his nascent interest in dynamical systems—first triggered by Hofstadter and Winograd, and recently strengthened by a best-seller on chaos theory (see xi.b, below). Many others today would tell similar stories. The general sense of excitement fostered by this meeting is clear from the many informal remarks of participants quoted by science journalists reporting on the new field (Levy 1992; Kelly 1994).

It also led to a great deal of 'hype'. Some of this was generated by the participating scientists themselves. Langton later admitted that there was "more good science and less 'gee-whiz' " in the second volume of A-Life Proceedings than in the first (Langton *et al.* 1992, p. xiv).

But much of the hype was generated by 'outsiders'. The people attending included a generous sprinkling of famous names, associated with a wide range of sciences. That was likely to be noticed. And Langton's vision of the future of the field was highly provocative: "The ultimate goal [of A-Life is] to create 'life' in some other medium, ideally a *virtual* medium" (see below). Such a claim was bound to—indeed, was intended to—attract attention. However, it also drew scepticism and scorn.

As for the name bestowed on the field, this too had its drawbacks. Irrespective of specific remarks made by Langton or anyone else, the term "artificial life", like "artificial intelligence" before it, invites literal interpretation from the media. I've encountered journalists from good newspapers who evidently think that the question "Does system X really use A-Life technology?" means the same as "Does system X contain creatures that are really alive?" In short, the meeting was a journalist's dream.

Langton was far from unhappy about that. And within a few years, A-Life had gained the attention of the counter-culture. For it exemplified a new image of science, more 'romantic' than the traditional image was.

The philosopher Stephen Clark (1995) contrasted "Faustean" and "Magean" science, the first being intelligible and largely predictable (though often bringing unwanted side effects) and the second opaque, almost like magic. Where GOFAI—and science in general—had been seen as dealing in preconceived plans and pre-defined goals, with "little use for spontaneity, trial-and-error, unplanned discovery, vaguely-defined ends, or informality" (Edwards 1996: 168), A-Life showed unpredictable evolution, emergence, and responsiveness to environmental details.

Stephen Clark cited it, accordingly, as an example of Magean science. Many professional artists apparently agreed with him. They were inspired by what they saw (often, wrongly) as the aims, and philosophical claims, of work in A-Life. By the turn of the century, hardly more than a decade after Langton's naming party, there was a still-growing body of art based broadly on these ideas (Whitelaw 2004). And students of contemporary culture, such as Sarah Kember, were making grand claims for A-Life as a cultural discourse describing "posthuman" ("cyborgian") life (2003, p. vii and *passim*).

But Clark saw Magean science as threatening, too. Just because of its opacity and unpredictability, it was unintelligible and beyond control—which is to say, it risked going out of control (cf. Kelly 1994). Soon, science-fiction novels/films such as Michael Crichton's (2002) *Prey* would give vivid expression to those cultural fears.

Worrying or not, however, A-Life became the talk of the town—much as Langton had hoped. The tone of "the talk of the town" wasn't always one of apocalyptic anxiety. Often, the *gee whiz* factor won out. Consider (for example) this University of Toronto press release of 2000, which describes the then latest version of the artificial fish mentioned in Section ii.a, above:

Surviving jaws: virtual merman 'thinks' his way to safety

Using a mythical merman and hungry sharks, a University of Toronto computer science professor and two former graduate students have pushed the notion of artificial intelligence and virtual life to a new level.

In his creation of a virtual underwater world, Professor Demetri Terzopoulos has fashioned more than just a cool screen saver—he has given his animated characters the ability to think. A hungry shark circles ominously, looking for a nice meal, while a nervous merman searches for a place to hide. When the shark swims away, the merman dashes from behind large rocks to open water with the shark in hot pursuit. Will his cleverly devised plan allow him to reach safety or not?

"This is more than artificial intelligence", says Terzopoulos. "It's artificial life. Computer graphics, animation and virtual reality have advanced dramatically over the past decade. We are now able to create characters that are self-animating with functional bodies and brains that have behaviour, perception, learning and cognition centres."

Terzopoulos and his former students have developed the cognitive modelling language that enables animated characters to reason. For example, it enabled the virtual merman to formulate a plan of action by reasoning about his situation given certain knowledge, such as the fact that he cannot outrun sharks but can use underwater rocks to hide. "With cognitively empowered graphical characters, the animator need only specify a behaviour outline and, through reasoning, the character will automatically work out a detailed sequence of actions."

The potential for future applications are immense, Terzopoulos says. Cognitive modelling and the cognitive modelling language can become powerful tools for scientists, animators and game developers. His paper, co-authored with John Funge and Xiaoyuan Tu, was published at the 1999 ACM SIGGRAPH conference, the premier forum for research in computer graphics. (University of Toronto web site, <http://www.utoronto.ca/>, 29 February 2000)

If this was the official, relatively restrained, account you can perhaps imagine the enthusiastically garbled versions that would appear in the less scrupulous newspapers.

(Excessive hype, from within and outside the field, is still a problem—and an embarrassing one, for those seeking professional legitimacy. In the A-Life community's turn-of-century self-assessment, the key failure was said to be "No rigor", and the greatest advantage of founding an International Society for A-Life—set up in 2003—was "reducing hype" by improving intellectual standards: Rasmussen *et al.* 2003*b*.)

Despite the hyperbole and misunderstandings, however, there was—and is—a serious intellectual core. The common scientific interest aroused at the workshop was afterwards described by Langton as

> an emerging consensus . . . of the "essence" of Artificial Life [based on] themes such as *bottom-up* rather than *top-down* modelling, *local* rather than *global* control, *simple* rather than *complex* specifications, *emergent* rather than *prespecified* behavior, *population* rather than *individual* simulation, and so forth. (Langton 1989*a*, p. xvi)

These themes are evident in the seminal work of Turing, von Neumann, Burks, and Holland (and in the wide range of later research mentioned in Sections vii–viii). And, if interpreted without reference to computer models, most of them can be seen in D'Arcy Thompson's writings, too.

Such people's pioneering work also fits the definition given in Langton's Call for Papers in 1986:

> Artificial life is the study of artificial systems that exhibit behavior characteristic of natural living systems. It is the quest to explain life in any of its possible manifestations, without restriction to the particular examples that have evolved on earth. This includes biological and chemical experiments, computer simulations, and purely theoretical endeavors. Processes occurring on molecular, social, and evolutionary scales are subject to investigation. The ultimate goal is to extract the logical form of living systems. (quoted in Levy 1992: 113)

D'Arcy Thompson and von Neumann, for instance, would have been quick to allow that the logical form of living systems isn't immediately evident. Discovering it might add something new to our accepted concept of life. In Langton's terms, we could come to see that "life as it could be" is a wider category than "life as it is".

For example, the crucial values of Langton's lambda parameter or Wuensche's Z (see Section ix.a) might come to be included within the definition of life.

Again, if reproduction is regarded as essential to life, then life might be defined as requiring von Neumann's dual (data/instructions) functionality. Von Neumann showed also that a universal replicator is possible: so perhaps some extra-terrestrial living organism could give birth to puppies as well as kittens—and to tadpoles and grass seeds, too? One might object that such a heredity-cheating creature could never have evolved. But that's relevant here only if one regards evolution itself as an essential criterion of life. Some biologists and philosophers do (Bedau 1996; Moreno and Ruiz-Mirazo 2004: 251–5; Luisi *et al.* forthcoming, 3). However, this decision isn't without drawbacks (see Chapter 16.x.d).

As for the logical form of living *bodies*, most biologists would probably say that there's no such thing. But not all: we've seen that recent work in D'Arcy Thompson's (and Goethe's) footsteps suggests that—given the laws of physics—only certain types of morphology are actually possible.

b. Simulation or realization?

For Langton, the logical form of living systems is such as to allow the possibility of *virtual* life. Contrary to D'Arcy Thompson, the constraints of physical embodiment are a distraction best avoided:

> The ultimate goal of the study of artificial life would be to create "life" in some other medium, ideally [*sic*] a *virtual* medium where the essence of life has been abstracted from the details of its implementation in any particular model. (Langton 1986)

Ray agrees. He argues that digitized creatures existing in cyberspace (very like his, described in Section vi) could be genuinely alive (Ray 1992, 1994).

This philosophical claim (discussed in Chapter 16.x) is called strong A-Life, in analogy to strong AI. Most A-Lifers reject it, believing that life in principle requires some sort of physical body and metabolism. Pattee, for instance, always insisted on a clear distinction between simulating life and realizing it, because he saw the laws of physics as crucial in explaining biological organization (Pattee 1972, 1976). So the journalists who excitedly reported that the production of cyberlife is the goal of A-Life *in general* were mistaken. So, too, were those who said that A-Life *in general* was aiming to produce "new forms of life", whether virtual or physical.

But their mistakes were understandable. For Langton, the instigator and organizer of the inaugural event, explicitly held both views. Moreover, as the official subtitle of the meeting he chose 'An Interdisciplinary Workshop on the Synthesis [*sic*] and Simulation of Living Systems' (Langton 1989*a*). And, in a paper published shortly before the first A-Life meeting, he'd said:

> We would like to build models that are so life-like that they cease to become *models* of life and become *examples* of life themselves. (Langton 1986)

Another point in mitigation was mentioned above: the likelihood that "artificial life" would be interpreted by outsiders to mean "life made artificially".

In fact, only a few insiders understood the term in this way. Indeed, some soon adopted less philosophically provocative (and somewhat less inclusive) language. They spoke of "adaptive systems", for example, or of "animats"—a conflation of "animal" and "robot" (Meyer and Wilson 1991). That enabled them to work on computer models of life without implying that simulation was intended as realization.

Because of the high profile of the 1987 meeting and its immediate successors, however, the term *artificial life*, like the term *artificial intelligence*, is here to stay. One must remember, therefore, that using artefacts to learn about life—even "life as it could be"—isn't the same thing as aiming to make life artificially.

Whether life could be made artificially remains an interesting question nevertheless. So does the question whether virtual life is in principle possible. So, too, do disputed claims about whether only life can support mind, whether adaptive autonomy (without representation) can suffice to do so, whether evolution is necessary for life and/or intelligence, and whether minds are distinct from living brains.

These are all partly philosophical questions (see Chapter 16.x), to which scientific work is dialectically relevant. That's why reports of experiments in this area often include discussions of the *definition* of life, and phrases like "the definition of life one wishes to use" (Luisi and Oberholzer 2001: 353).

If they were purely scientific questions then one, at least, would have been answered at the dawn of the new millennium. London's *Sunday Times* of 30 January 2000, under the headline 'First Artificial DNA Can Create New Forms of Life', reported that researchers in Texas "have made the world's first synthetic DNA [and] mapped out the exact way it will be configured to create synthetic organism one (SO1), the microbe destined to be the world's first man-made creature".

Three years later, the *New Scientist* excitedly reported that Craig Venter (director of the Human Genome project) is trying to build "the minimal genome" (by stripping down the genome of the simplest existing organism), and that the Harvard nanotechnologist George Church is aiming to do much the same thing (Ainsworth 2003). But the *New Scientist* had the philosophical edge over the *Sunday Times*, for its journalist acknowledged the controversial nature of the *concept of 'life'*. These matters aren't so straightforward as his *Sunday Times* rival had assumed.

That's evident from the conceptual arguments (i.e. not just the empirical disagreements) that arise within so-called "wet" A-Life. This form of A-Life *does* aim to create new life. Specifically, it aims to create living/lifelike systems out of chemical/biochemical substances. Sometimes, it even starts from what Kant would have termed "crude inorganic matter" (see 2.vi.b), although sometimes it starts from an already living thing.

The notion that this is in principle possible dates from the mid-1920s, in a paper by the Russian biochemist Alexander Oparin. Remaining untranslated, this had little impact at the time. But Oparin's late 1930s book caused huge excitement, not to say scandal (Oparin 1936/1938). The first Oparin-inspired experiments to succeed in synthesizing molecules characteristic of life, namely amino acids, from simple substances present on the primitive earth were done half a century ago (S. L. Miller 1953; Miller and Urey 1959), and sugars had followed by the late 1960s (Reid and Orgel 1967).

Today, such experiments continue. But most of them don't count as wet A-Life. For that, the aim must be not merely to synthesize unorganized biomolecules, but to create *entire, albeit simple, living things*. Several groups around the world are attempting this. And they're doing so in interestingly different ways, depending on their philosophical views about the nature of life as such.

Venter's team in Maryland—who drew the attention of the *New Scientist*—are working top-down. That is, they're starting from a living, independently replicating, cell (*Mycoplasma genitalium*) that has the smallest known genome: only 517 genes (Fraser *et al.* 1995; Hutchison *et al.* 1999). They aim to identify any non-essential genes (the so-called junk DNA), and to construct a new, stripped-down, genome that will generate a recognizably *living* organism. By the eve of the new century, they'd identified 480 protein-forming genes, of which between 265 and 350 seemed to be essential (including 100 of unknown function). If and when they achieve their aim, it's unlikely that there will be much disagreement over whether the result really is a living organism. After all, it will be a cleaned-up copy of a naturally evolved life form.

Things may be—indeed, they already are—rather different in cases where researchers aim (*à la* Oparin) to concoct life from the bottom up, as opposed to working top-down by removing redundant DNA. The problematic philosophical issues are more evident when people start from a non-biological base and try to build living systems anew. Such people often restrict themselves to molecules we already recognize as "biochemical".

But sometimes they don't, hoping to find very general biochemical principles which cover substances other than the ones found in terrestrial organisms. (Life as it could be.)

The biochemists who are seeking to model and/or produce physical analogues of the molecular origins of life don't always agree about just what molecules count as "biological" and what as "pre-biological", or about what is a "basically autonomous system" but *not* a living thing, or about how "biomolecules" can become sufficiently organized to merit the label of "full-fledged living beings" (Moreno and Ruiz-Mirazo 2004: 245–55).

Similarly, they disagree on just how the criterion of a *boundary* is to be interpreted, and what its philosophical significance is, when chemical compartments (micelles, vesicles . . .) arise within which specific chemical reactions can take place. For example, liposomes can form vesicles; if these contain an enzyme for replication, an RNA template, and ingredients for RNA synthesis, then the system may self-replicate (Biebricher *et al.* 1982). Some biochemists—notably Pier Luigi Luisi's group in Rome—interpret such chemical boundaries in terms of the philosophy of autopoiesis (Bachman *et al.* 1990, 1992; Walde *et al.* 1994; Luisi *et al.* 2004).

Luisi, a long-time friend and admirer of Varela, relates his experiments to an autopoietic interpretation of "the minimal cell" (Luisi and Varela 1989; Luisi and Oberholzer 2001: 349–50; Luisi 2002). This concept has been discussed for almost fifty years. For example, Harold Morowitz (1967) argued that the minimal cell need be no more than nine-tenths the size of *M. genitalium*. (For more recent estimates, see Islas *et al.* 2004; Szathmáry 2005.) But new biochemical techniques have led to a new spurt of interest, including two international conferences in 2004.

The minimal cell is the smallest possible living system, having "the minimal and sufficient number of components to be called alive" (Luisi *et al.* forthcoming, 3). It isn't thought to be a particular system, which *had to be* generated if life was to appear at all. Rather, the assumption is that there's a broad family of potential minimal cells. Which of these was/were actually realized is a question more for biological history than for abstract biological theory.

As for the term "living", this is usually understood in wet A-Life to mean: capable of self-maintenance (metabolism), self-reproduction, and evolution. Pre-cellular systems, or proto-life, may fulfil only one or two of these three criteria. And/or they may be reproducible for only a few generations, or they may be able to reproduce only part of themselves. Luisi calls such systems "limping life" (Luisi *et al.* forthcoming, 22). Workers in wet A-Life typically argue that they were essential stepping stones on the way to the emergence of fully fledged life.

Researchers differ in their commitment to what I just called biological history, as opposed to biological theory. So there's no consensus over whether we should be trying (like the Texas researchers who impressed the *Sunday Times*) to replicate the biomolecules already familiar to us. Some argue that wet A-Life may, or even should, attempt to build "chemical automata" made up of *alternative* chemical components (Drexler 1989; Bro 1997; Moreno and Ruiz-Mirazo 2004: 255).

In addition, of course, there are many empirical disagreements. These include one instigated at the University of Copenhagen by Peter Nielsen, the "inventor" of PNAs (polyamide-linked nucleic acids). PNAs are man-made "chimaera" molecules, having some of the structural properties of DNA and some of the chemical properties of proteins (Nielsen *et al.* 1991). Significantly, they follow the Watson–Crick base-pairing rules (Egholm *et al.* 1993). So the question arises whether PNA chemistry can replace RNA chemistry for the purposes of wet A-Life (Rasmussen *et al.* 2003*a*).

Here, as elsewhere, science and philosophy can interact. Work in wet A-Life often includes explicit discussion of how to define "life" (e.g. Moreno and Ruiz-Mirazo 2004: 254–7). And for those who believe that life and cognition are *essentially* linked, it may affect their philosophy of mind as well.

15.xi. After the Party

After the party was over, life went on. And A-Life went on, too. Only seventeen years have passed since Langton's epochal gathering. Nevertheless, a large body of work has been done.

A few examples drawn from the 1990s, or later, have already been mentioned. These include the evolutionary robots outlined in Section vi.c, the primitive open-ended evolution sensors of Section vi.d, the various models of neuro-ethology discussed in Section vii.b–d, and the biochemical A-Life briefly mentioned above. By and large, however, this chapter has followed the time-line, from the early work to the official founding/naming of the field. It may be appropriate, then, to end it by describing two more instances of *very* recent research.

These will bring things full circle, all the way back to mid-century cybernetics. But the circle is an ascending spiral, not a plane figure: besides lifting cybernetic understanding to a new level, these two examples open up exciting pathways for further study. Moreover, they raise fundamental issues that are relevant to cognitive science *in general*.

a. Resurrection of the Homeostat

Over a century ago, George Stratton (1896, 1897*a*,*b*) earned immortality by an experiment in which he wore inverting lenses for eighty-one hours, spread over eight days (see 14.viii.b). At first, his movements were utterly inappropriate to the physical environment around him. But his visuo-motor system gradually adapted until he was able to negotiate his world almost as successfully as before. Moreover, he no longer had to make allowances for the fact that the world looked upside down because, after the adaptation, the world *didn't* look upside down, but broadly "normal".

This phenomenon has intrigued psychologists and neurophysiologists ever since, and various similar experiments—some with left–right inverting spectacles—have been done. Now, further examples are cropping up in a wide range of VR applications (see 13.vi.b), including novel forms of motor and/or sensory prostheses (A. J. Clark 2003*a*).

Ashby's interest in it was mentioned in passing in Chapter 4.viii.c, and Andras Pellionisz's neurologically based explanation was described in Chapter 14.viii.b. Recently, another—more abstract—model of recovery from inversion has exploited Ashby's notion of a homeostatic dynamical system.

Ezequiel Di Paolo (2000*b*) evolved simulated light-seeking robots that could gradually recover phototaxis after their visual field was inverted. Putting it like that, however, may be misleading. For—like Stratton himself—they weren't evolved *to recover from* *visual inversion*. In other words, this adaptation didn't feature in the fitness function. Rather, they were evolved to perform phototaxis. But the 8-unit control network that enabled them to do this was of a type which *also* enabled them to recover when later subjected to the radical challenge of visual inversion.

Phototaxis was achieved by evolving the robots' control network, or "brain"—broadly, as described in Section vi.c, above. Two of the eight units ("neurones") in the network were attached to the left and right motors, and two to the sensors ("eyes"). In addition, every unit was connected to every other. But the excitatory/inhibitory nature of the connections could be varied by mutation, as could the numbers assigned to units and connections (see below). At each generation, six lights, of various intensities, were shown at different times and at different places. The robots that, on average, approached them most closely and most quickly, and that stayed by the light when they got there, were selected as parents for the next generation. (Noise was added to the system, to make the evolving circuits more robust.)

So far, so conventional (if such a still-recent approach can be called conventional). The difference from previous work was the nature of the units in the network. The key point is that each one of these was locally homeostatic. In other words, each artificial neurone had a permissible range of activation rates (not evolved, but assigned to it by Di Paolo). If it went above or below its limits, the weight on the incoming (presynaptic) connections would be changed so as to lower or increase its activity, respectively.

Unlike blood temperature then, where homeostasis restores a specific value, a neurone's acceptable activation rate could have various values—all lying within the relevant range. Given the numerical limits of each of the eight individual neurones, the numbers in the rule of presynaptic weight change were evolved. That is, the fitness function—besides measuring success in the behaviour of light seeking, as described above—measured success in homeostasis of all eight units.

In perfect homeostasis, no neurone would ever move outside its limits, so weight changes would never need to be made. But it turned out that neurones with very powerful homeostasis were bad news: they tended to do nothing, so that the robot hardly moved. For phototaxis to evolve, the unit homeostasis had to be less than perfect.

The result of 1,000–2,000 generations of evolution would be a light-seeking robot with a neural controller wherein each unit would recover its permissible activation rate if perturbed by some unusual input. An "unusual input" would normally be something like an especially strong (or weak, or distant) light source. But the robots' sensory anatomy enabled the malign roboticist to provide yet more unusual inputs.

Each robot had a left eye and a right eye. That is, its two sensors were placed relatively far apart on its body, so that the difference between the two visual inputs would depend on (in functionalist terms, would "encode") the specific location of the light source. A near-equivalent to Stratton's upside-down inversion, then, was to switch the anatomical positions of the two eyes, while leaving their neuronal connections intact.

As you might expect, each immediately post-operative robot was unable to approach the light. Indeed, it would move in the *opposite* direction. Di Paolo found, however, that about half of his robots eventually managed to adapt to their new situation, ending up with perfect phototaxis as before.

Although the behavioural outcome (reaching the light) was identical, the behavioural strategy wasn't. In one case, for instance, the pre-operative robot would begin by moving slightly to one side and then to the other, in order to centre the light in its visual field, and would then move directly towards it. After adaptation to inversion, the same robot moved in an arc of a circle to locate the source. (Di Paolo also found that the longer the period before inversion, the longer it took for adaptation to occur; he compared this to the "critical periods" for neural adaptation that have been observed in animals—2000*b*: 445.)

Senator Proxmire wouldn't have been impressed (see Chapter 6.iv.f). Granted, phototaxic robots could conceivably be useful. But if a researcher has managed to produce light-seeking robots, why waste the taxpayers' money in messing around with their sense organs so that they fall into confusion?

Put like that, the Proxmire complaint sounds persuasive. But, as was so often the case in his lifetime, the Senator would have been wrong. For the light-seeking robots were just an example. Famous though Stratton's experiment is, it wasn't Di Paolo's primary concern. What he was really engaged in was something even more interesting. Explicitly paying homage to Ashby, he was resurrecting the Homeostat (Chapter 4.viii.c). Specifically, he was demonstrating the sort of self-organization that Ashby had called ultrastability. Homeostasis of the system as a whole resulted from changes triggered by local homeostasis of its component units.

Moreover, his work was physiologically promising, in the sense that his general methodology allowed for the modelling of many other sorts of cerebral plasticity besides weight change (Di Paolo 2000*b*: 447–8). For instance, it could be used to model neuronal depression or potentiation, or modulatory synapses. It could even be used to simulate diffusive chemical neuromodulation—a phenomenon recently studied by his Sussex colleagues (cf. 14.ix.f).

Above all, for our purposes here, his chosen example was *psychologically* interesting, and highly relevant to cognitive science. Indeed, he sees his phototactic robots, like his earlier work on the evolution of communication (13.iii.e), as evidence supporting *a radical critique* of orthodox (functionalist) cognitive science (2000*b*: 448).

The reason is that his robots' behavioural homeostasis—that is, their successful adaptation to environmental perturbation—wasn't guided by any specific behavioural goals. Rather, it resulted from the underlying dynamical principles of the system. (This point was confirmed by the fact that adaptation also occurred when he made various "lesions" in one of the sensor or motor "organs"—2000*b*: 446.) In short, their behaviour was in effect *purposeful*, but wasn't directed by *purposes*.

b. Analysing dynamics

Di Paolo's work was experimental, not analytic. Each of his successfully adapting robots provided an existence proof of a homeostatic dynamical system with intriguing psychological overtones. (Only overtones: unlike the neurologist Pellionisz, he wasn't trying to say just how, in fact, Stratton's brain might have recovered from visual inversion.) But he didn't know just how many 8-unit networks were in principle capable of this adaptation. Nor did he know just what type, or types, they could be. He didn't even know whether as many as eight neurones were strictly necessary.

That's not unusual. In general, cognitive scientists who manage to get interesting behaviour out of a dynamical system typically don't know just why it occurred. (Up to a point, the same is true of GOFAI; but GOFAI researchers learnt long ago that they should try to analyse their programs, not just glory in them: see Chapter 11.iii.) Dynamicists rarely know just what it was about *this* dynamical system which made it capable of generating *that* behaviour.

Very recently, however, some elegant mathematical work has enabled researchers to say something along these lines, at least for very small networks of a certain general type. The systems concerned are called CTRNs, or CTRNNs: continuous-time recurrent (neural) networks.

This is a very broad class. (The "brains" controlling Di Paolo's robots were an extended version, as we'll see.) The first person to pay significant attention to them was the cockroach-building Beer (R. D. Beer 1995*c*). Beer first defined them in 1991 (he doesn't know whether anyone else had done so already: personal communication). He used the double-N label: CTRNNs.

When they're called CTRNs (by Di Paolo, for instance), the N for "neural" is omitted. Arguably, the single-N label is better (and I'll use it in what follows). For, as Beer himself pointed out when he defined the term in the first place, the units *need not* represent neurones, nor even neuronal cell assemblies. They can stand for *any* essential variable, *any* convenient dynamical building block, in the dynamical system being modelled.

In general physiology, these would be key aspects of metabolism. In neuroscience and psychology, they'd be features of neural function and behaviour, respectively. And in sociology or management studies, they'd be properties of the social communications concerned. Even Pask's ideas about the cybernetics of the criminal underworld (4.v.e) could be represented by a CTRN. (It's significant that his ambitious "conversation theory" was intended as a *general* account of feedback systems, in which the conversationalists identified by the rebarbative notation could be cells, people, or organizations.)

Today, CTRNs are increasingly used by people doing dynamical modelling in A-Life and neuroscience. One reason for this is a mathematical proof achieved in the early 1990s (Funahashi and Nakamura 1993). Much as a universal Turing machine can model any discrete computation, so these networks can in principle represent any dynamical phenomenon.

A CTRN is made up of units computing continuously in time. In effect, it's a cellular automaton in which the time-steps are infinitesimal in duration. (If it's implemented on a von Neumann computer, a CTRN will in fact involve *discrete* state transitions.) But each unit has its own numerical time-constant, which determines just how quickly/slowly it will give its output in response to a significant input. The units aren't binary, but sigmoidal. (Di Paolo's units are unusual in having a *range* of "permissible" activation rates that's smaller than the range of possible activation rates, and a homeostatic mechanism for bringing them back within the permissible range as appropriate.)

What counts as a significant input is determined by the unit's numerical "bias" parameter. This is broadly equivalent to the threshold of a McCulloch–Pitts neurone. But the bias (and the other numerical parameters of a CTRN) doesn't have to be an integer. Unlike McCulloch–Pitts thresholds, it can be defined to whatever degree of precision is required.

Each connection is either excitatory or inhibitory, and carries some numerical weight. The weights on the connections between any two units can be asymmetrical, in which case unit A will influence unit B more strongly than B influences A. (So a CTRN could model the asymmetrical communications between the various crew members aboard ship: see Chapter 8.iii.b.) Often, all the weights stay the same once they've been assigned. But sometimes, for instance in Di Paolo's phototaxic robots, the individual weights can change during the system's "lifetime".

Finally, the networks are completely connected. That is, each unit is connected to every other unit—and even to itself, by a recursive feedback loop. Because the network is completely connected, any change in one unit *directly* affects every other unit. (A CTRN is thus similar to a continuous Hopfield net, described in Chapter 12.v.f; but Hopfield units don't have self-loops, and Hopfield connections are all symmetrical.) Being completely connected makes a CTRN a dynamical system par excellence:

> Much of the unique flavor of dynamical systems is captured by the idea of *coupling*. [Two] variables are coupled when the way each *changes* at any given time depends directly on the way the other *is* at that time. In other words, coupled variables simultaneously, interdependently co-evolve, just like arm angle and engine speed in the centrifugal [Watt] governor. *Genuinely dynamical systems exhibit high degrees of coupling: every variable is changing all the time, and all pairs of variables are, either directly or indirectly, mutually determining the shapes of each other's changes*. (van Gelder 1997: 437; final italics added)

Complete connection is mathematically interesting, and it enables holistic study of dynamical systems in which every individual part influences every other. But it's rarely biologically plausible. For instance, the number of connections found in mammalian nervous systems scales roughly linearly as the number of neurones increases, instead of exploding as it does in fully connected networks (C. F. Stevens 1989).

Nevertheless, CTRNs can be used to model real biological circuits, for if a weight is set to zero then the connection concerned is in effect deleted. (Similarly, a huge bias parameter would deactivate the relevant unit.) So every conceivable network, whether fully connected or not, can be represented as a CTRN.

Beer's first artificial cockroaches, described in Section vii.c above, were situated robots inspired by Brooks and by Pengi. One might almost say that they were engineered rather than controlled. But Beer soon began to use dynamical system controllers to improve their walking.

His dynamical turn had been inspired by two near-simultaneous publications. One was James Gleick's (1987) popular book on chaos theory (personal communication), and the other was the Winograd–Flores book (see 11.ii.g)—which introduced Beer to the ideas of Maturana and Varela. From that time on, they were the major intellectual influences on his work.

Initially, Beer was most excited by how specific dynamical systems could help him build better robots. For example, he evolved a CTRN leg controller that repeatedly adapted its parameters to compensate for the fact that the (simulated) leg was *growing*, so changing the values of the physical constraints involved (J. G. Gallagher and Beer 1993, sect. 6). In 1991–2 he wrote his dynamicist's "manifesto" (personal communication) for the journal *Artificial Intelligence* (R. D. Beer 1995*a*). But he became increasingly interested in the abstract properties of his robot mechanisms *considered as dynamical systems*.

By the start of the new century, these general principles were his main focus. He now wanted to discover just which dynamical attractors were possible for different classes of CTRNs. This was a mathematical exercise (described below), but one with practical implications.

For instance, suppose he found that some class of CTRNs involved cyclic (oscillating) attractors. Intuitively, it seems that such networks would be specially apt for controlling rhythmic activities, like walking. If so, then one particular CTRN oscillator would need to be built into any actual robot. Of course, Beer could simply have picked out one example from the class of oscillators to use as the gait controller for an artificial cockroach leg. But his choice would have been largely arbitrary. So, instead, he picked a "parent" CTRN from this general class and then used GAs to evolve even more suitable descendants.

In other words, he was able to search the wide range of possibilities generated from the chosen seed by random mutation. (There was another advantage, too: biological evolution didn't have the luxury of picking something out of an explicitly analysed class, so Beer wanted to convince himself that these stable oscillators *could actually be evolved*.)

Beer's mathematics wasn't wholly a priori. As Gleick's book had pointed out at length, one can't predict (i.e. analytically prove) just how a complex dynamical system will behave. So Beer had to rely on experimental induction. That is, he defined/evolved a range of CTRNs, and studied their behaviour *post hoc* to see if he could find any regularities. Langton had done much the same thing when defining his "lambda" parameter in the mid-1980s, though using CAs rather than CTRNs (see ix.a, above). But whereas he'd looked at scores of different systems, Beer—with his students Sean Psujek and Jeffrey Ames—looked at several millions (Psujek *et al*. 2004).

Why so many? Well, the rich connectivity of CTRNs means that the range of possible architectures is huge. (An architecture is defined as a set of directed connections.) For a 3-unit network there are 64 architectures, for a 4-unit net there are 4,096, and 5 units surpass the half-million mark (to 528,384). Then, the numbers rapidly become unmanageable. Moreover, any one architecture has multifarious examples, because each parameter (the three connection weights, the bias, and the time constant) can have various numerical values.

Consequently, the Case Western team studied only CTRNs of up to five units. (For the 5-unit networks, they couldn't inspect every possibility so looked at a 1 per cent sample instead.)

In evolving CTRNs (with a population size of 100, and 250 generations), they soon discovered that convergence was common. In other words, CTRNs (like NK networks) are *apt* to fall into stable limit cycles—so are potentially promising as models of living/nervous systems. (Compare Wolfram's intuitions about which type of CA is capable of life and/or computation.) But they wanted to discover which architectures are most likely to converge to this or that stable oscillation.

For instance:

* What types of CTRN make oscillations possible?
* Or closed feedback loops?

* And when do these oscillators and/or loops involve connections to the "motor" neurones? (In their experiments, three units were "motor" neurones for the two opposing swing muscles and the foot, and the others—if any—were unassigned.)
* What classes will oscillate between three (or four . . .) states, rather than two?
* What's the minimum number of connections necessary to achieve successful walking?
* And what types of oscillator are best for achieving speed and stability in walking?
* Can any CTRNs produce several different oscillators?
* Or a combination of oscillator/s and point attractor/s?
* If so, what's needed for these oscillators to be smoothly coordinated?
* Last, but not least: for any given architecture, are there particular clusters of parameter-values that are especially promising?

They discovered, for example, which class of 3-unit CTRNs is best suited for controlling walking. There were three behaviourally distinct groups: 29 out of the 64 3-unit possibilities generated either no movement at all or only a single step; 8 produced slow, jerky, walking; and 27 resulted in fast, rhythmically smooth, stepping. On inspecting the connectivity patterns of the three groups, they found key differences in the existence and connectivity of feedback loops between the motor neurones for the foot and the two swing muscles. They then asked whether the same graph-theoretic properties could be used to *predict* similar classes of behavioural output in 4- and 5-unit nets.—Yes, they could. (There were some exceptions, but these could be explained: see 2004, sect. 4, paras. 7–8.)

In addition, some types of network were found to be *more easily evolvable* than others (2004, sect. 5). And qualitative descriptions like those mentioned above were supplemented by quantitative measures of fitness, distinguishing each individual motor neurone. These measures enabled the team to study an architecture's average results as well as its best ones. Some types were found to be more generally useful than others, even though their best performances were lower. (Di Paolo, you'll remember, had selected the robots which approached the lights most closely *on average*. But he couldn't say what type of controller was most likely to give a good performance-average.)

Even more recently, Beer's group have observed how the phase portrait of a dynamical system changes when the numerical value of one of the parameters is changed (R. D. Beer 2005). A "movie" of the effects of varying the bias in a 2- or 3-unit network, for example, shows how the system changes smoothly from one attractor to another, or diverges from any equilibrium. Plotted as a diagram of a two- or three-dimensional space, a particular region (or regions) can be identified where most of the "interesting" things happen, and others can be seen to be sterile. (Compare the pioneering fourfold classification of CAs mentioned in ix.a, above.)

This abstract analysis has intriguing biological and psychological implications. For example, given a specific CTRN, one can sometimes predict how its behaviour will change as the parameters are varied in certain ways. And Beer has recently used these ideas to explain the behaviour of a "minimally cognitive agent" with as many as fourteen artificial neurones (personal communication). Conceivably, such analysis might even help biologists to show why certain behaviours have evolved (i.e. are easily evolvable) from one origin rather than another.

However, "intriguing" and "conceivably" is as much as can be said, at present. And maybe you shouldn't hold your breath. The very recent (2005) analysis just described is certainly exciting, for it can be applied to CTRNs of *any* size. But readily intelligible 'Euclidean' diagrams can be drawn only for two or three dimensions. The larger CTRNs are like the multidimensional hyperspaces (including the imaginary scholarship rules) discussed in Chapter 14.viii.b: not beyond mathematical understanding, but fiendishly difficult.

As for Beer's *complete* descriptions of the space of architectures, these have been applied only to very tiny CTRNs. Even artificial CTRNs may have more units than can be dealt with in this way, and real organisms are huge by comparison: the minimalist. *C. elegans* has 302 neurones (14.v.b), and mammalian brains have many millions.

These biological facts may not be quite so devastating as they seem, for the Case Western researchers discovered that *more* need not be *better*: once there are five or more connections (connections, not units), the best fitness for the robot-walking task hardly alters (Psujek *et al*. 2004, sect. 3). That may be why, as remarked above, biological networks—even including the cerebral cortex—are very sparsely connected in comparison with full-blooded CTRNs. Nevertheless, the numbers remain awesome. (It's not clear that increased computer power will help much; but possibly quantum computers?—see 16.ix, preamble.)

In other words, the combinatorial explosion threatens work in dynamical systems just as it threatens GOFAI. So one shouldn't expect Beer to provide further exhaustive analyses of CTRNs: five units today, eight tomorrow, perhaps the elegant 302 next month . . . Rather, one should see his work—including the varying-parameter phase portraits—as a promising exploration of *the kinds of effects, properties, or concepts* that may be needed in thinking about realistic (i.e. larger) systems.

For example, rhythmic behaviour in general—not just walking, and not just cockroaches—depends on oscillator attractors, which in turn depend on there being closed loops in the architecture. With hindsight, that may be obvious. But it wasn't obvious, even to committed dynamical theorists (such as Di Paolo: personal communication), before Beer's analysis.

Much the same might be said about psychological AI. Consider, for instance, research on mental architecture (Chapter 7.i.e–f), on anxiety-ridden speech (7.ii.c), on the complex webs of relevance involved in everyday thinking (7.iii.d), or even on the development of the past tense (12.vi.e and x.d). The hope of the cognitive scientists concerned is not to map the computations involved in precise detail, but to understand *what sort of system* a personality (or a neurosis, or an associative memory, or an infant learning to speak) is, and *in what sorts of ways* it can be manifested, in *what sorts of circumstances*. Most computational psychologists believe that concepts drawn from classical and connectionist AI can answer those questions. By contrast, dynamicists believe that dynamical concepts and insights can best help us understand what sort of system an organism, or a mind, is. Similar bets, different horses.

In sum, dynamical systems are still more theoretically opaque than either GOFAI or orthodox connectionism. It's not clear how far Beer's analysis can be applied to the dynamics evolved by Nature. And, for all Di Paolo's 8-unit ingenuity, Ashby may have been right to fear that future Homeostats would be "too complex and subtle for the designer's understanding" (1948: 383). But some provocative beginnings have been made. If many open questions still remain, and others can't yet even be asked, that's par for the course in any creative science.

16

PHILOSOPHIES OF MIND AS MACHINE

Philosophies of mind and machine have been prominent since the 1630s. But for René Descartes and virtually all of his successors, that "and" really meant "not". Mind was contrasted with machines, not likened to them (see Chapter 2).

Philosophies of mind *as* machine didn't appear until over 200 years later (Chapter 4.ii and vi). Today, they still have many philosophical adversaries. And even those philosophers who are sympathetic to mind-as-machine disagree about *just which* machines these might be. Nor would they accept Herbert Simon's claim that, in writing the Logic Theorist (6.iii.c and 10.i.b), he and his colleagues had "solved the venerable problem of how a system composed of matter can have the properties of mind" (Simon 1991: 190).

Evidently, then, philosophical problems don't get solved in a hurry. One might even say that they don't get solved at all—for if they do, they're relabelled as "scientific" problems. AI often suffers a similar fate (see 13.vii.a). Having bravely located firm ground in unmapped and marshy territory, its successes are renamed "computer science" and what was once exploration becomes tourism. Moreover, some philosophers insist that their aim is not to solve problems but to *dissolve* them, "to show the fly the way out of the fly bottle" (Wittgenstein 1953, para. 309; cf. A. J. T. D. Wisdom 1952: 259).

Soluble or not, philosophical problems have already featured in our discussion. Chapter 2 outlined two broad philosophical movements with competing influences on the life sciences. And other chapters have included arguments on specific topics, such as:

* free will (Chapters 5.ii.a and 7.i.g);
* innate ideas and nativism (7.vi, 8.v–vi, 9.vii, 12.viii.c–e, 12.x.e, and 14.x.c);
* word meaning (4.iii.c, 7.ii.d, 9.iv and x.d);
* concepts (5.iv.c, 8.i.b, 9.x.d, and 12.x);
* non-conceptual content (12.x.f);
* representations (7.i.h, 7.v.a, 12.ix.e, and 14.viii–ix);
* scientific explanation (7.iii and 12.x.f);
* consciousness (7.i.g–h and 14.x–xi);
* relativism and scientific objectivity (1.iii.b);
* tacit knowledge (13.ii.b); and
* life (15.vii.b).

Some issues in the philosophy of religion were implicit in Chapter 8.vi, and even metaphysics has been mentioned (13.i.b–c and 7.iv.e—and see the discussion of computation in Section ix.e, below).

A fortiori, cognitive science is related to the philosophy of mind. That's the general topic of this chapter. Section i outlines the competing philosophies of mind dominant at the inception of cognitive science, and the various philosophical impasses involved. To understand those is to appreciate why, in the late 1950s and early 1960s, cognitive science seemed to be such a liberation.

Section ii considers the influence of the Turing Test on philosophy and AI. Various forms of functionalism are discussed in Sections iii and iv. Some objections to functionalism feature in Section v, and the founder's recantation in Section vi. Further neo-Kantian critiques are discussed in Sections vii and viii. Differing concepts of computation, and their relation to intentionality, are explored in Section ix. Finally, Section x deals with the relevance of life to mind.

Aperitif. Before starting, however, we need to ask why non-philosophers should bother with philosophy at all.

Many think they should not. For instance, one reviewer of an early outline of this book suggested that the proposed chapter on philosophy be entirely omitted. Cognitive science, he/she evidently believed, can get on very well without it. (And this *despite* the fact that most leaders in the field find it exciting largely because they believe that it solves the notorious mind–body problem.)

Even philosophers admit that "First-rate philosophy can be profoundly irritating, especially to non-philosophers" (Bringsjord and Zenzen 2002: 241). The slowness/insolubility mentioned above is one common source of irritation. But the authors just quoted were thinking of another:

> [Following Socrates' example, the philosopher] starts with innocent enquiry, pushes and probes, makes some seemingly innocuous inference . . . and boom!—suddenly she has shown that what scientists and engineers take for granted *shouldn't* be taken for granted. (ibid.)

Occasionally, to be sure, what was taken for granted is later confirmed by the philosopher concerned, with stronger arguments provided to buttress it. Even then, the common-sense belief may have been transformed in the process (see the discussion of free will in Chapter 7.i.g). That doesn't make for comfortable reading. But sometimes, what was previously assumed is rejected altogether. In that case, the question is left worryingly open. And there's no hope for a neat resolution in the next number of *Nature*.

Hard-headed scientists, as a result, are likely to agree that "There is nothing so absurd that it has not been said by some philosopher"—a remark of Cicero's, quoted by Descartes and by many others after him. (For Cicero and Descartes, of course, "philosophy" included what we would call science. It's ironic, then, that the biologist Lewis Wolpert, while repeatedly dismissing philosophy in the most contemptuous terms, celebrates the seeming absurdity, or "unnaturalness", of science: L. Wolpert 1992.)

Friedrich Nietzsche, some 2,000 years after Cicero, even remarked that a philosopher is "a terrible explosive, endangering everything" (Kauffmann 1969: 281). That wasn't a criticism, for he wasn't hoping for (epistemological) safety. Most practising scientists, by contrast, are.

Myriad beliefs normally taken for granted are questioned (*questioned*, not necessarily rejected) in the philosophy of cognitive science. For instance: that mind requires life; that zombies are logically possible; that human freedom can't be scientifically explained; and that dogs and monkeys have conscious experiences—and intentions, too. The list could go on, and on . . .

That's irritating enough, perhaps. But one challenge in particular annoys the "scientists and engineers": the denial that a real world exists independently of human minds, and that science can find out about it.

This counter-intuitive claim isn't new. It was lying in wait at the heart of Descartes's philosophy (he toyed with it himself, in suggesting that his whole life might be a dream: 2.iii.b), and was later forefronted by the neo-Kantians (2.vi and vii.c). Nor is it tucked away safely inside dusty philosophy books. To the contrary, it's often wheeled up to the scientists' front door. The "strong programme" in the sociology of knowledge defends this claim, and has led to vituperative "science wars" in which gallons of metaphorical blood have been angrily spilt (see 1.iii.b).

Scientists, however, have little patience for the anti-realist challenge. (That may not be true of quantum physicists, but in my experience it applies to most life scientists.) Even when they're discussing and/or dismissing philosophy as such, they often don't mention this aspect of it. Or if they do, they don't give it more than a glancing thought. Gerald Edelman is a case in point, as we've seen (14.ix.d).

Philosophers of cognitive science usually are willing to argue about realism (although sometimes they deliberately sidestep the question: see vii.d, below). But they almost always end by accepting it, *even while admitting that they can't decisively prove it*. That's what I did, provisionally, in Chapter 1.i.c and iii.b; and it's what Jerry (Jerrold) Fodor does too (see viii.b, below). Indeed, seven of the ten sections below ignore the realism/anti-realism dispute. It features only in Sections vi–viii.

So why ask non-philosopher readers to consider it at all? One reason is that some experimental cognitive scientists, the followers of Humberto Maturana and Francisco Varela (x.c, below), support views which—whether they realize it or not—are fundamentally anti-realist. (Occasionally they do realize it, and explicitly accept them: I. Harvey 2005.)

Another is that the counter-cultural opposition mentioned at various points in this book (e.g. 1.iii.b–d, 6.1.d, and 11.ii.f) is grounded not only in political suspicion of science's applications, but (often) also on neo-Kantian scepticism about its realist foundations. The counter-culture can be properly understood, and persuasively (if not definitively) rebutted, only if that is appreciated.

A third is that an interesting new twist on the realism/anti-realism debate is related to the recent AI technology of virtual reality, or VR (13.vi). Some philosophers' reactions to this draw on work in cognitive science (see viii.c, below).

The main reason, however, is that anti-realism is grounded in an alternative, and in non-scientific circles highly influential, philosophy of mind. If that philosophy is correct, then cognitive science is fundamentally wrong-headed—indeed, impossible. For to drop realism is to drop naturalism: the belief that there could, in principle, be a science of mind. "Cognitive science", on this alternative view, should be a very different kind of enterprise: hermeneutic, not naturalistic (Harré 2002). Intentionality is to be presupposed, not explained.

In sum, no serious consideration of cognitive science can ignore philosophy. Quite apart from the specific philosophical issues that arise *within* the field (concepts, representations, zombies, free will, self . . . and, of course, the mind–body problem), its very possibility, as a coherent intellectual enterprise, is in doubt. Those doubts must be understood, even if one ultimately rejects them—as I do myself. Moreover, the huge attraction of the field lies largely in its promise to help solve one of the greatest philosophical puzzles of all: the mind–body problem.

This chapter, then, explains why that promise seemed to many people to be convincing—and why many others are *still* not convinced.

16.i. Mid-Century Blues

In the 1940s and 1950s, four philosophies of mind competed for attention from English-language philosophers. ('Continental' philosophers favoured fundamentally different accounts, not taken seriously in analytic circles at the time: see Section vii.) Mind-as-machine wasn't one of them.

GOFAI, connectionist, and cybernetic approaches were already hovering in the wings, to be sure (see Chapters 4–15). But professional philosophers were looking straight ahead, on-stage.

There, the forerunners in the talent competition—in order of appearance—were epiphenomenalism, logical positivism, logical behaviourism, and identity theory. Alan Turing was cast as a support dancer for behaviourism's solo: not until the 1960s would he move onto centre-stage.

Each key contestant attracted many fans. But few onlookers sat comfortably in their seats. As we'll see, they had to admit to—or systematically repress (Feigl 1958/1967: 3)—worrying flaws in every performance.

a. Interactionist squibs

Besides these four, there was an also-ran: interactionism. This was dismissed out of hand by most mid-century thinkers, who shared Princess Elizabeth's scepticism about the mind's ability to move the brain (see Chapter 2.iii.b). An occasional philosopher, however, insisted that interactionism—and even substance dualism—hadn't actually been refuted (Ewing 1954: 114).

Some scientist–philosophers went even further. The chemist Michael Polanyi, for instance, suggested that "some enlarged laws of nature may make possible the realization of operational principles acting by consciousness", and that the mind might "exercise power over the body merely by sorting out the random impulses of the ambient thermal agitation" (Polanyi 1958: 397, 403 n.).

Similarly, the leading neuroscientist John Eccles (1903–97) argued that conscious choice is a matter of essentially mental influences causing changes in the brain (see Chapter 2.viii.f). This applied only to the cerebrum: as we saw in Chapter 14.v.c, Eccles was happy to describe the cerebellum as "a Neuronal Machine".

Eccles avoided speculations about the pineal gland, preferring to tell fairy stories in modern scientific language ("fields", "quantum effects", and "critically poised neurones"). But he encountered much the same logical difficulties that had bedevilled Descartes long before.

He said, for instance: "[My] hypothesis assumes that the 'will' or 'mind influence' has itself a spatio-temporal patterned character in order to allow it this operative effectiveness [on specific cells]" (Eccles 1953: 277–8). This was given almost as an aside: *how it is possible* for an essentially mental influence to be spatio-temporal wasn't discussed. Eccles's move, here, invites Peter Medawar's classic comment on Teilhard de Chardin's even woollier remarks about consciousness (especially how individual minds can be absorbed in the one Omega consciousness): "And so our hero escapes from his appalling predicament: with one bound, Jack was free" (Medawar 1961: 104).

Some twenty years later, a distinguished philosopher of science—one of the tiny handful of people elected to both the British Academy and the Royal Society (others are the cognitive scientists Uta Frith and Philip Johnson-Laird)—would join Eccles in promoting interactionism (Popper and Eccles 1977). Karl Popper's obituarist for the Academy, a close colleague for many years, said of him, "At heart he was a Cartesian interactionist" (Watkins 1997: 679), and mentioned his saying that he believed in what was then being dubbed "the ghost in the machine". This was at a talk given in Oxford, where Gilbert Ryle's ghost-denying approach was then dominant (see subsection c, below).

By the late 1970s, there were many other ghost-deniers besides Ryle. All of them upstaged Popper. Despite his high standing in the profession, the audience for his interactionist cameo was small, and the applause muted.

b. Puffs of smoke and nomological danglers

The first 'spot' in the contest was epiphenomenalism, singing a hymn to body–mind causation but trashing mind–body influence. This competitor had been ushered on stage by Thomas Huxley long before, in the 1870s (Chapter 2.viii.b). He'd costumed mental events as puffs of smoke from a steam-engine: unmissable, but ineffectual.

Now, eighty years later, many scientifically minded people—including most neurophysiologists—were still applauding it. Many expressed it in much the same (Cartesian) terms as Huxley had done. But the denial of mind-to-body—and even mind-to-mind—causation remained counter-intuitive, and the posited body-to-mind causation remained a metaphysical mystery.

Even when put less provocatively, in twentieth-century (phenomenalist) terms of lawful regularities rather than causal interaction, the doctrine was still problematic. Mental events are regularly connected with the physical story, but—given that the physical story is apparently seamless—they don't lead back into it. They don't even lead without interruption to each other, for the mental story isn't seamless. Nor is it regular: thought *x* is not predictably followed by thought *y*.

Herbert Feigl (1902–88) noted these difficulties:

Causality [i.e. regular succession] in the mental series is by far too spotty to constitute a "chain" of events sufficiently regular to be deterministic by itself. Epiphenomenalism in a value-neutral scientific sense may be understood as the hypothesis of a one-to-one correlation of [mental states] to (some, not all) [cerebral states], with determinism (or as much of it as is allowed by modern physics) holding for the [cerebral] series, and of course the "dangling" nomological relations connecting [the cerebral and mental events]. (Feigl 1958/1967: 15)

Feigl's nomological danglers were perhaps less puzzling than Huxley's body-to-mind causation, but they were a philosophical embarrassment nonetheless. They were especially problematic for those people—including many leading linguists and psychologists, as well as Feigl himself—who shared the logical positivists' faith in the unity of science (Chapter 9.v.a).

The logical positivists had bounded onto the boards in the 1930s. They saw their role not as cutting the mind–body Gordian knot, but as preventing it from being tied in the first place. They argued that the mind–body problem, like all metaphysical puzzles, is a "pseudo-problem" generated by misunderstanding of language. Although they held that all knowledge is based in sense experience, they insisted that their approach wasn't just another form of post-Cartesian idealism. And they briskly denied any fundamental difficulty in relating mind and body:

The answer to the question whether sense-contents are mental or physical is that they are neither; or rather, that the distinction between what is mental and what is physical does not apply to sense-contents. It applies only to objects which are logical constructions out of them. But what differentiates one such logical construction from another is the fact that it is constituted by different sense-contents or by sense-contents differently related . . . [There] is no philosophical problem concerning the relationship of mind and matter, other than the linguistic problems of defining certain symbols which denote logical constructions in terms of symbols which denote sense-contents . . . Being freed from metaphysics, we see that there can be no *a priori* objections to the existence of either causal [i.e. Humean] or of epistemological connections between minds and material things. (Ayer 1936: 123–4)

So far, so dismissive. Too dismissive, for many people. Even Feigl, who'd been involved with logical positivism since its inception in 1920s Vienna, refused to accept that mind–body is a mere pseudo-problem. (His own solution was a version of the identity theory: see below.)

c. Dispositions and category mistakes

A third philosophical contestant appeared in the late 1940s, eliciting both vociferous applause from the gallery and angry hisses from the stalls: logical behaviourism. This too, though for different reasons, saw the mind–body problem as an illusion based in misunderstandings of language.

The most well-known script used by this contender was Gilbert Ryle's book *The Concept of Mind* (1949), whose advance flyer had been posted a few years earlier (Ryle 1946). The book was immediately hailed in *Mind* as "probably one of the two or three most important and original works of general philosophy which have been published in English in the last twenty years"—*despite* (said this reviewer) its having fundamental flaws (Hampshire 1950: 237). Ryle's crisp sentences and humorous polemic were exceptionally widely read. There were five reprints within five years, and many more thereafter.

Ryle (1900–82) was an Oxford philosopher who often traversed the English countryside in the 1930s to hear Ludwig Wittgenstein (1889–1951) lecture in Cambridge.

Ryle was deeply influenced by him, and they became close friends. But the amity didn't last. Wittgenstein's 'later' work had circulated privately for some years (Chapter 9.x.d). To his annoyance, however, Ryle's book was published first and received enormous attention.

Wittgenstein's chagrin wasn't merely priority pique, nor simply plagiarism paranoia. (He was quick to accuse others of plagiarizing, and misrepresenting, his work: the young Richard Braithwaite, for example, was forced to write a letter to *Mind* 'apologizing' to him on this count: Wittgenstein 1933; Braithwaite 1933.) He felt that Ryle had not only benefited from his ideas, but also misrepresented—or anyway, altered—them. For instance, Ryle relied heavily on "dispositions", a term that Wittgenstein disliked (see below).

Their earlier friendship dissolved into thin air. Wittgenstein dismissed Ryle as not "serious". For his part, Ryle deplored Wittgenstein's "unhealthy" and "pedagogically disastrous" influence on his students (Monk 1990: 275, 495).

Ryle dismissed Descartes's dualistic "myth" of "the ghost in the machine" as radically incoherent and/or a source of countless examples of infinite regress. For example, it explained "intelligent" action in terms of "intelligent" thought—hardly a helpful move.

Descartes was cast as the villain in Ryle's script. However, the *Mind* reviewer pointed out that Descartes wasn't the only one to be blamed. As he put it, "the myth of the mind as a ghost within the body is one of the most primitive and natural" of all beliefs (Hampshire 1950: 239). Moreover, the words for mind, soul, or spirit in most European languages had the same etymology as the words for ghost "long before Descartes or modern mechanics". It was true, however, that Descartes's account had been especially clear, and especially influential.

But if Descartes had been talking nonsense, said Ryle, he hadn't been talking utter nonsense:

> A myth is of course not a fairy story. It is the presentation of facts belonging to one category in the idioms appropriate to another. To explode a myth is accordingly not to deny the facts but to re-allocate them. And this is what I am trying to do. (Ryle 1949: 8)

For Ryle, everyday psychological terms don't denote occurrences within some non-material world. (Compare Wittgenstein: "When I think in language, there aren't 'meanings' going through my mind in addition to the verbal expressions: the language is itself the vehicle of thought" and "Speech with and without thought is to be compared with the playing of a piece of music with and without thought"—1953, paras. 329, 341.)

Rather, such terms should be analysed as shorthand descriptions of actual *and possible* behaviour. That is, they concern behavioural "dispositions"—comparable to the brittleness of glass.

To say that glass is brittle is to say that it's disposed to break, in certain circumstances. A given piece of glass may never actually break, but it's brittle nonetheless. *If* it were dropped onto a hard surface, *then* it would break. Analogously, Ryle argued, to say that someone is jealous, or happy, or thinking about a problem . . . is to say that they are disposed to behave in certain ways, given certain circumstances. Even to say that someone is hot, or itchy, or seeing red (in either sense!) is to remark on their actual and/or potential behaviour.

He allowed that people can keep their thoughts and feelings to themselves. But he analysed this as a counterfactual claim. What it meant, he argued, was simply that *if* the circumstances had been different (if their interlocutor were less indiscreet and/or more trustworthy, or if they were on the point of death, or . . .), *then* they would have expressed them.

Words denoting "propositional attitudes" (Bertrand Russell's term: B. Russell 1918–19)—such as *know*, *believe*, *desire*, *prefer*, *fear*, and *hope*—were treated in the same way. To believe that *p* is to be disposed to say that *p*, and to behave in ways that would be appropriate (given the person's other beliefs and desires) if *p* were true. As in this example, psychological concepts are generally defined in terms of others. So this approach put a new gloss on what would previously have been thought of as mind–mind (or even mind–body) causation. Pity, for instance, is—among other things—the disposition to help and/or comfort ('behaviour') someone seen to be in distress ('circumstances'). So to say that Mary helped Jane "because" she pitied her is to mark a conceptual link, not a causal (contingent) one.

As for intelligence (said Ryle), this isn't some ghostly power causally responsible for behaviour, but rather a certain manner of behaving. Intelligent behaviour is carried out carefully, thoughtfully, skilfully, appropriately, and flexibly. Largely, the intelligence manifests itself only as tacit *knowledge how*. Sometimes, a person can comment on aspects of their behaviour, and answer questions or give advice, by verbal statements of *knowledge that*. Intelligence in general, however, isn't a matter of explicit reasoning—whether kept to oneself or shouted to the rooftops. In a nutshell:

> Overt intelligent performances are not clues to the workings of minds; they are those workings. Boswell described Johnson's mind when he described how he wrote, talked, ate, fidgeted and fumed. (Ryle 1949: 58)

In general, then, Ryle didn't try to forbid talk of mental states, but he gave an unfamiliar analysis of their nature. (Similarly, computational analyses of human freedom admit that people have a psychological characteristic which animals don't, but gloss this in an unorthodox way: see Chapter 7.i.g.)

All very plausible, possibly, for third-person remarks about mental states. But what about first-person statements? Surely these are reports of inner happenings?—Ryle was obdurate. He said (for instance) that first-person statements about one's feelings of emotion are "avowals" rather than reports:

> Avowing "I feel depressed" is doing one of the things, namely one of the conversational things, that depression is the mood to do. It is not a piece of scientific premiss providing, but a piece of conversational moping. That is why, if we are suspicious, we do not ask "Fact or fiction?", "True or false?", "Reliable or unreliable?", but "Sincere or shammed?" The conversational avowal of moods requires not acumen but openness. It comes from the heart, not the head. It is not discovery [by Cartesian direct access], but voluntary non-concealment. (Ryle 1949: 102)

(Compare Wittgenstein: " 'So you are saying that the word "pain" really means crying?'—On the contrary, the verbal expression of pain replaces crying and does not describe it"—1953, para. 244.)

For all his hard-headedness in rejecting the Ghost, Ryle had no interest in saving the Machine:

> [The] influence of the bogy of mechanism has for a century been dwindling because, among other reasons, during this period the biological sciences have established their title of "sciences". The Newtonian system is no longer the sole paradigm of natural science. Man need not be degraded to a machine by being denied to be a ghost in a machine. He might, after all, be a sort of animal, namely, a higher mammal. There has yet to be ventured the hazardous leap to the hypothesis that perhaps he is a man. (Ryle 1949: 328)

Ryle's reference, here, to a non-Newtonian paradigm in biology is puzzling. He may have been thinking of Claude Bernard's work on self-organization, or Walter Cannon's on homeostasis (Chapter 2.vii.a). Presumably, he wasn't referring to their 1940s equivalent, cybernetics. For his cybernetic contemporaries were arguing that all organisms are fundamentally like *some* machines, albeit machines unlike anything imagined by Descartes (see Chapter 4.v–vii).

Logical behaviourism was very different from the psychological variety discussed in Chapter 5.i and iii. Psychological behaviourists, in essence, were experimental epiphenomenalists. They claimed that mental (conscious) states are ontologically distinct from the body; that proper knowledge of mental states can be had only by the person whose states they are; and that, for scientific purposes, third-party concepts of mental states can be defined in the non-intentional terms of *stimulus* and *response*. As noted above, Ryle rejected every one of these claims.

So did Wittgenstein. He wrote:

> "Are you not really a behaviourist in disguise? Aren't you at bottom really saying that everything except human behaviour is a fiction?"—If I do speak of a fiction, then it is of a *grammatical* fiction.
>
> How does the philosophical problem about mental processes and states and about behaviourism arise?—The first step is the one that altogether escapes notice. We talk of processes and states and leave their nature undecided. Sometimes perhaps we shall know more about them—we think. But that is just what commits us to a particular way of looking at the matter. For we have a definite concept of what it means to know a process better. (The decisive movement in the conjuring trick has been made, and it was the very one we thought quite innocent.)—And now the analogy which was to make us understand our thoughts falls to pieces. So we have to deny the yet uncomprehended process in the yet unexplored medium. And now it looks as if we had denied mental processes. And naturally we don't want to deny them. (Wittgenstein 1953, paras. 307–8)

> And yet you again and again reach the conclusion that the sensation itself is a *nothing*.—Not at all. It is not a *something*, but not a *nothing* either! The conclusion was only that a nothing would serve just as well as a something about which nothing could be said. (para. 304)

Some cognitive scientists today, including Daniel Dennett and Aaron Sloman (both of whom encountered Ryle while studying at Oxford), see themselves as following in Ryle's footsteps (Dennett 1969, pp. xi, 2000; Sloman 1996*c*, Acknowledgements). This may seem strange. For his neo-Wittgensteinian assumption that talk of non-material processes can be understood only dualistically was very different from cognitive science—which is mentalistic, although not dualistic. However, there are two good reasons for arguing that Ryle should be seen as a precursor of cognitive science, not just a predecessor (see 9.ii).

First, Ryle's subtle analyses of psychological concepts, and of the rich interconnections between them, were highly illuminating as accounts of mental architecture. His account of emotions (1949: 83–115), for instance, was an inspiration for Sloman's work on the topic (Chapter 7.i.f). And in exhibiting the conceptual (rational) links between the many varieties of belief and desire, he prefigured Dennett's theory of intentional systems (see Section iv.b).

Second, Ryle's term "disposition" can be interpreted in a way that fits very well with cognitive science. Whether Ryle himself understood the term in this way is another matter.

Most talk of "dispositions" is systematically ambiguous, denoting behaviour and/or its underlying causes. This is why Wittgenstein preferred to avoid the term (1953, para. 149 and pp. 191–2). The psychologist William McDougall had explicitly defined the word in both these senses, and had kept them clearly distinct (McDougall 1923; see 5.ii.a). Ryle didn't.

His term is usually read *descriptively*, as denoting *an observable tendency to behave in a certain manner* (O'Shaughnessy 1970). This sense was dominant in the 1950s, partly because analytic philosophers of mind didn't see it as their role to offer explanations. That, Wittgenstein had taught them, was the business of brain scientists. However, Ryle's term was later read also as *explanatory*, denoting *the mechanism responsible for the relevant behaviour* (Squires 1970).

Whether Ryle himself understood it in this way is doubtful. But cognitive scientists in general favour the dual reading. And, while any "mechanism" which Ryle might have intended would have been neurophysiological, they allow computational explanations too. (Whether they all expect these to map neatly onto the propositional attitudes is another question: see Section iv.)

If Ryle's bravura performance was hailed by many of his contemporaries, it was fiercely criticized by others. Even the adulatory *Mind* reviewer was ambivalent. Quite apart from his analytic philosophical method in general (which he later confessed was the *real* topic of the book, mind–body being a "notorious and large-size Gordian knot" that he could use as an illustrative example—Ryle 1971: 12), there were two major problems.

First, it wasn't clear that dispositional analyses could be fully spelt out in practice, not least because so many psychological concepts are defined in terms of others. Moreover, many people felt that consciousness was still missing—or, to put it in linguistic terms, that philosophical behaviourism didn't give a satisfactory account of first-person psychological statements. Ryle's analyses of sensations, and Wittgenstein's dismissal of private languages (1953, paras. 243–76) and "the beetle in the box" (1953, para. 293), failed to persuade them that Cartesian mental processes are dispensable. On the contrary, they seemed—to many critics—to be recalcitrantly real. (A dialogue between Ryle/Wittgenstein and the obstinate "Otto" might have been very similar to that given in Chapter 14.xi.b.)

d. Questions of identity

In 1956 a fourth challenger entered the limelight—and, especially for scientific audiences, threatened to win the contest. This was mind–brain identity theory.

The new theory accepted dispositional analyses for most psychological predicates, adopting the dual interpretation that posits explanatory mechanisms in the brain. But sensations, or experiences, were not analysed in this way. Nor were they understood as epiphenomena. Denying any special realm of conscious events, identity theory held that sensations are *identical* with brain states. The brain state doesn't cause the sensation, nor is it correlated with it. Rather, it constitutes it. Ontologically there is only one thing. But it can be described, and known, in two ways.

Identity theory was first published in a scientific journal, the *British Journal of Psychology*, by Ullin Place (1924–2000). (As usual, there were precursors: Feigl, for instance, had been thinking along similar lines for some time.) Place insisted that the theory was "a reasonable scientific hypothesis, not to be dismissed on logical grounds alone" (Place 1956: 44). Only future work in neurophysiology could confirm its truth, he said, although suggestive empirical evidence was already available.

"Logical" objections were sure to be made, of course, and Place anticipated two. The first was that our introspective access to our conscious states proves that they are events of a different order. Place called this "the phenomenological fallacy". He argued that it rested on "the mistaken idea that descriptions of the appearances of things are descriptions of the actual state of affairs in a mysterious internal environment" (1956: 44).

The second was that mental and physical states are radically different, so can't possibly be identical. Or, expressed less 'metaphysically': the meanings of mental and physical terms are very different, which is why people can talk about their experiences without knowing that the brain is in any way involved.

Distinguishing "the 'is' of definition" from "the 'is' of composition", Place replied that things falling under very different concepts may in fact be compositionally identical. Some such identities are superficial and contingent (Mary's table *is* an old packing case). But some are deep and systematic, and discoverable only by science. People had talked about lightning for thousands of years, but scientists eventually discovered that lightning *is* electric charges in motion. It follows, Place argued, that the fact that our concepts of mental states are logically very different from our concepts of brain states doesn't prove that they pick out non-identical things.

Stated thus, Place was right. Feigl made the same point by appealing to Gottlob Frege's distinction between *sense* and *reference* (Chapter 2.ix.b). But even two "very different" concepts may share an important feature. All Place's examples relied on shared spatio-temporal dimensions. (Think of how one discovers that a table is a packing case, or that lightning is electric discharge.) And, *pace* Eccles, conscious states—notoriously—are not in space.

Accordingly, the identity theory was widely regarded as paradoxical. Even David Armstrong, who published the best-developed version of it—known as central-state materialism—ten years later, admitted: "Certainly I myself found the theory paradoxical when I first heard it expounded" (Armstrong 1968: 73).

The next move—taken by Jack Smart (1920–), in whose Adelaide department Place had written his paper—was intended to resolve the paradox. Smart suggested that we wield Occam's razor and *decide* to treat experiences and brain states as identical. In that case, an experience could be said to have the same location as the brain process which constitutes it. He even said:

> [We] may *easily* adopt a convention (which is not a change in our present rules for the use of experience words but an addition to them) whereby it would make sense to talk of an experience in terms appropriate to physical processes. (Smart 1959; italics added)

Many critics could hear Jack (the giant killer) bounding again. Norman Malcolm (1911–90), for instance, insisted that Smart *was* suggesting a change in usage, and one that would be fundamentally incoherent (Malcolm 1964). But at least Smart, unlike his successor Francis Crick (14.x.d), recognized that any such change would be a scientifically grounded philosophical decision, not a scientific discovery.

Whether the scientific grounding required could ever be delivered was unclear. The identity theorists held that each type of mental state is, as a matter of fact, identifiable with a certain type of brain state. Yet there were many reasons to doubt this—some scientific, some philosophical.

One was Karl Lashley's (1950) recent discovery that memories seem not to be located in any specific part of the cortex (Chapter 5.iv.a). Nowadays, the term "brain state" could include some highly distributed pattern of activity (see Chapters 12 and 14), making Lashley's findings less problematic. At mid-century, however, "brain state" usually intended the activity of a single neurone or localized cell assembly. In those terms, hardly any positive evidence was available. Single-cell feature-detectors were yet to be discovered (see Chapter 14.iv). It had recently been found that complex conscious states can be elicited by stimulating points in the temporal cortex. But their content was predictable only, if at all, in the individual concerned (Penfield 1952, 1958).

Natural language, too, presented a problem. Except on an extreme Humboldtian view (Chapter 9.iv.b), two people can sometimes express the same thought. But if they speak in different tongues (different phonemes, etc.), their brain states must differ in many ways. Even dogs gave pause for thought. Presumably dogs, and possibly Martians, can feel (non-conceptualized) pain, much as a baby can. But a dog's or Martian's brain is very different from a baby's—which would imply (according to the identity thesis) that it *cannot* feel pain (David Lewis 1980).

Besides 'scientific' hesitations such as these, more fundamental attacks were mounted by Wittgensteinian philosophers. Malcolm (1964), for instance, argued that only someone whose culture includes milkmen can have the sudden thought that they haven't put out the milk bottles. This thought therefore can't be identified with a brain state, for that could conceivably occur (perhaps due to microsurgery) in anybody's head. Any brain state could even happen, as Smart himself had pointed out (1963), in a brain *in vitro*. But for Malcolm, the notion that a brain—in a head or in a vat—can *think* is literally absurd. Thoughts can be sensibly ascribed only to embodied and enculturated persons.

At the end of the 1950s, then, none of the four main rivals had monopolized the applause, and all were receiving some highly critical reviews. Despite the near-universal rejection of mental substance, Descartes's mid-twentieth-century heirs faced much the same mind–body problems as he had done (see Chapter 2.iii.b).

Some voices off-stage—across the Channel—were arguing that "mind" and "body" are Cartesian illusions anyway (see Sections vii–viii below, and Chapter 14.xi). But most of the analytic audience wasn't listening. Those few—notably Ryle and Wittgenstein—who had heard them were expressing this view in different terms.

(About ten years later, at a meeting held in France, Ryle admitted that his book "could be described as a sustained essay in phenomenology, if you are at home with that label"—1962: 188. In the discussion, which also included A. J. Ayer and Willard Quine, one of the leading Continentals replied: "[What Mr Ryle] was saying was not so strange to us, and the distance, if there is a distance, is one that he puts between us rather than one I find there"—Merleau-Ponty 1960: 65. And the 'analytic' reviewer of the meeting's Proceedings apparently agreed: C. M. Taylor 1964*b*. All this isn't too surprising, for as a young man Ryle had been an exponent of Edmund Husserl, and had reviewed Martin Heidegger's *Being and Time* with respect. The respect was coloured with scepticism, however. Ryle's review opened with "This is a very difficult and important book . . . though . . . I suspect that this advance is an advance towards disaster," and closed by saying, "Phenomenology is at present heading for bankruptcy and disaster and will end either in self-ruinous Subjectivism or in a windy mysticism"—Ryle 1929: 355, 370.)

As for mind-as-machine, the topic had barely arisen. On the rare occasions when mind—or rather, adaptive behaviour—was likened to machine in the philosophical journals (Ashby 1947; J. O. Wisdom 1951), it was cybernetic machines which were in question (see Chapter 4.v–vii). Digital computers were hardly mentioned until 1950, when a provocative squib in *Mind* written by Alan Turing prompted a flurry of discussion about the possibility of machine intelligence.

16.ii. Turing Throws Down the Gauntlet

Turing himself was more interested in the machine than in the intelligence. In other words, his *Mind* paper was primarily intended as a sketch of a research programme for AI (see 10.i.f), not as a conceptual analysis of "intelligence". However, virtually all the philosophical authors who responded to it addressed what they saw as Turing's behaviourism, rather than his imagined machines.

Moreover, those who did mention his machines didn't do so sympathetically, or in detail. Not until the 1960s would philosophers of mind start to use computational ideas in a constructive manner (Sections iii–iv, below).

a. Sketch of a future AI

Well before mid-century, Turing had been considering mind in a very different way from the writers discussed above (see Chapters 3.v.b and 4.ii.b). From the late 1930s, he'd believed that many, perhaps all, thoughts were formal–computational in nature. And soon after the war, he wrote a technical report on how to make "thinking machinery"—and there outlined what was later called the Turing Test. But the Official Secrets Act prevented publication.

Some of Turing's philosopher contemporaries were aware of his general position. In October 1949 he took part in a Manchester seminar on 'The Mind and the Computing Machine', with the philosophers Dorothy Emmet and Wolfe Mays (1912–2005), and the chemist–philosopher Polanyi. But none of them was persuaded (Manchester Philosophy Seminar 1949; Polanyi 1958). Other discussants on this occasion included the mathematician Max Newman, the zoologist John Z. Young, and the psychologist Frederic Bartlett. They were more ready to consider his ideas seriously, and to try to relate them to what was known about the brain (he'd mentioned both logical and 'neurone-based' models: Chapter 10.i.a).

Many philosophers, however, still knew of Turing only as a mathematical logician and/or a slave of Manchester's newfangled calculating machine, vulgarly dubbed a "giant brain" by the newspapers. This was the world's first modern computer—but why should they be interested in *that*? Many more hadn't heard of him at all.

In 1950 Turing's philosophical profile suddenly became more prominent, when he published a paper in *Mind* on 'Computing Machinery and Intelligence'. It ignored all the mind–body 'isms' outlined in Section i—although it did rebut several potential objections, including the argument from Gödel's theorem. (For the record, that argument had originated with Emil Post some thirty years earlier, and had featured in a paper of 1941 rejected by the *American Journal of Mathematics* as too "historical": Post 1965.) Nevertheless, Turing's 1950 essay was rich fare. It served up two intellectually tasty dishes—one of which was left untouched by most of its philosophical readers.

Primarily, the *Mind* paper comprised an outline programme for AI research, based on Turing's still-secret report. It provided a tutorial on digital computers, describing programs in terms of "tables of instructions" (rules for linking Input plus Internal State to Output). And it suggested how these machines might be useful in thinking about thinking.

It said, for example, that if we want to find analogies between computers and brains, we should focus on "mathematical analyses of function", not superficial similarities such as using electricity. Charles Babbage, after all, had "all the essential ideas" but hadn't used electricity. And it suggested that intelligence could be modelled by these machines (even though Turing knew there are *some* questions they can't answer: Chapter 4.i.c).

The paper provided "recitations tending to produce belief", as opposed to "convincing arguments of a positive nature", about what practical advances might be made—and *how*. So in the august pages of *Mind*, Turing discussed possible AI work on game playing, perception, language, and learning, giving tantalizing hints about what had already been achieved (for details, see Chapter 10.i.a and f). He even envisaged cloning—which he said would *not* be regarded as "constructing a thinking machine".

Most philosophers at the time paid no serious attention to Turing's substantive argument about the possibility of AI. They focused, rather, on his tongue-in-cheek claim that it would be natural to attribute thought to a computer that succeeded in the "imitation game" (a chess version of which he'd already carried out on a "paper machine": A. M. Turing 1947*a*: 23).

The game (soon dubbed the Turing Test: see subsection c, below) had been proposed in order to avoid dismissive arguments based on the meanings of words. These were wheeled out, nonetheless, some citing the authority of the *OED* (Mays 1952: 149). Turing himself resolutely refused to define either "machine" or "thinking":

> I propose to consider the question, "Can machines think?" This should begin with definitions [framed to reflect] the normal use of the words, but this attitude is dangerous. [It implies that the meaning and the answer are] to be sought in a statistical survey such as a Gallup poll. But this is absurd. Instead of attempting such a definition I shall replace it by another [namely, the

Turing Test], which is closely related to it and is expressed in relatively unambiguous words. (A. M. Turing 1950: 433)

He added two provocative predictions, based on his expectation of practical advance in AI. The first was that "in about fifty years' time" machines would be able to play the imitation game with at least a 30 per cent probability of fooling "an average interrogator" for five minutes (p. 442). (What he actually wrote was: "[the] interrogator will not have more than 70 per cent chance of making the right identification". This is very often misread as predicting that the computer would *fool* the interrogator in 70 per cent of cases; but, as Blay Whitby pointed out to me, it means that the interrogator would be *correct* up to 70 per cent of the time.)

This prediction gave grounds for the second:

The original question, "Can machines think?" I believe to be too meaningless to deserve discussion. Nevertheless, I believe that at the end of the century the use of words and general educated opinion will have altered so much that one will be able to speak of machines thinking without expecting to be contradicted. (p. 455)

This was slippery. It could be read as a philosophical claim (an "educated opinion"), or as an empirical prediction about "the use of words". Moreover, the term "contradicted" was ambiguous. To allow someone—without reproof—to speak of computers thinking, or even to do so oneself, isn't necessarily to believe that computers actually think (see Sections v–vii, below).

By 1952, Turing's ideas had been featured in the newspapers, and he was debating them on BBC radio (for the BBC transcripts, see Copeland 1999). And the first philosophical replies had been published (Pinsky 1951; Mays 1951, 1952). One of these, written by Mays, would have appeared even earlier, had it not been censored by Ryle:

When Turing published his paper in *Mind* in 1950, Gilbert Ryle sent me the proofs and asked me to comment on them. I did, but Ryle rejected it on the ground that it was too polemical. (I used the word consciousness too many times, which in those days was heresy. It's now well-trodden ground by Searle, Dreyfus, and others.) Instead he asked Minsky to reply [who, in the end, didn't]. My reply got published in *Philosophy* some time later. (Mays, personal communication)

(Mays wasn't anti-machines: he'd co-designed an electrical logic device in the late 1940s—see Preface, ii. He'd also arranged for two digital computers to be shown at a 1950 philosophy conference, which could transform strings of up to 2,048 binary digits into logical symbolism: Mays *et al.* 1951. However, he *was* anti-formalist and anti-behaviourist with respect to the philosophy of mind. Indeed, he later founded the British Society for Phenomenology.)

Initially, these papers were a trickle, not a flood. In the 1950s, the attention of scientifically minded philosophers was largely captured by the identity theory (see Section i.d). One Cambridge apple orchard housed a group of philosophers, and others, deeply influenced by Turing. But they were more concerned to put his scientific ideas into effect than to publish philosophical papers about them (see Preface, ii, and Chapter 9.x.d).

It wasn't until the 1960s—thanks partly to functionalism and partly to results in early AI—that Turing began to be cited regularly by philosophers of mind. Usually, they ignored the main thrust of the piece and focused only on the Turing Test. But a few picked up Turing's gauntlet, by asking whether certain aspects of mind could, in fact, be simulated in computers.

Keith Gunderson, then at Princeton, was one of the first to pay Turing the compliment of disagreeing with him about his *main* point: that AI could (at least) simulate intelligence.

Gunderson had been alerted to AI/functionalism in the 1950s, even before either of those names existed. The first wake-up call was Turing's classic essay. The next was Michael Scriven's (1953) paper denying consciousness to robots (because they aren't alive: see Section x, below). The third was Paul Ziff's (1959) discussion of feelings in robots. And the last was a seminar given at Princeton by Hilary Putnam in 1959 (personal communication). Now, in the early 1960s, Gunderson published a paper on the imitation game (1964*b*), another touching on language in computers (1964a, sect. iii), and another—in reply to Ziff—on emotions in robots (1963).

With respect to Turing's core claim, Gunderson was sceptical. He distinguished various forms of "computerophily" (1964*a*: 211), in which philosophers claimed that this or that human capacity could be simulated, perhaps even instantiated, in computers. On the one hand, he shared Descartes's doubts about the provision of language and *general* intelligence to machines (see 2.iii.c), although he did consent to leave open the question of whether computers could ever use language successfully (1964*a*: 222). On the other, in thinking about feelings in robots he'd begun to develop the distinction between "program receptive" and "program resistant" properties—later spelt out at length (Gunderson 1971; cf. 1985: 166–247). Some psychological phenomena, he said, could never be understood in AI/functionalist terms, whereas others could: emotions and problem solving, respectively (see 7.i.d).

Computer scientists, by contrast, had appreciated the main point of Turing's piece in *Mind* immediately. By 1963 it was already "one of the best-known papers" on AI, and was included in the influential collection on *Computers and Thought* (Feigenbaum and Feldman 1963: 9). Interest in it continued to grow. By the century's end, it had been anthologized scores of times, and cited on countless occasions. It still features regularly, both in professional journals (of philosophy and cognitive science) and in the media.

b. The gauntlet spurned

For ten years or more, most philosophers (as opposed to computer scientists) considered Turing's paper as a version of behaviourism, not as a substantive claim about thought. (Honourable exceptions included Mays, Gunderson, Scriven, Ziff, and John Lucas: see Section v.a–c.) In that sense, the philosophical community declined to pick up the gauntlet he'd thrown down. Indeed, many of these early replies said nothing specific about computers—which were still fairly inaccessible (see 3.v).

Even after computers became familiar, anti-Turing arguments—for instance, that robots must be non-conscious "zombies"—usually focused on his approach in the abstract (Section v.b, below). Its detailed implementation was ignored.

Ned Block's (1942–) influential argument about the giant look-up table was seemingly an exception (N. Block 1982). "Seemingly", because it ignored the intractable problem of the combinatorial explosion—which no computer scientist would have done (Dennett 1988). Interpreted literally, the Turing Test allowed the possibility that the computer's individual answers had been pre-stored, each one directly triggered by a particular question. But that wouldn't count as intelligence, which as Descartes had pointed out (Chapter 2.iii.c) is able to generate new answers to new questions.

However, Block's objection was well aimed only at the letter of Turing's paper, not at its spirit. In his wartime work—trying to determine what coding machine could be responsible for the observed 'behaviour'—Turing had indeed been seeking look-up tables: *this* letter of the message alphabet corresponds to *that* letter of the code alphabet. (It had turned out that there were two successive look-up tables, not one.) But by 1950, when his paper appeared in *Mind*, computing had already moved on. Programs now defined internal computations and structured spaces of generative possibilities, not mere lists of input–output pairs. In other words, Turing had excluded look-up tables implicitly, if not explicitly.

Despite Turing's coyness about defining thought, his paper implied that some conceivable digital computers could actually think. It was one of his philosophical colleagues at Manchester who first published substantive reasons for denying this.

Mays argued—like John Searle, thirty years later—that computer programs are all syntax and no semantics (Mays 1951, 1952). Or, as he put it:

> But if we grant that logical machines are complex pieces of symbolism, a development of the visual aids to thinking which we have known for centuries, in order that the signs may acquire a significance they need to be given a specific logical or mathematical interpretation. As Whitehead [in his *Universal Algebra* of 1898, pp. 3–5] tells us, though we can study the art of practical manipulation of these signs without needing to assign any meaning to them, abstract calculi only possess a serious scientific value when they can be given an important interpretation.
>
> Neglect of the pragmatic or instrumental aspects of such machines, leads to the tendency to attribute to them a capacity for thinking which they have only by proxy. The transformation of formulae according to a fixed set of logical rules, is not, however, a sufficient criterion of thinking. Unless the resultant formulae or patterns of symbols are retranslated in terms of their referents, the transformation remains a meaningless array of marks. [We need a human] intelligence to programme the machine and interpret the end-result . . . (Mays 1952: 159)

He failed to point out that Turing himself had made the same point in his *Mind* contribution—not once, but twice. We can speak of computers making "mistakes", he said, only "when some meaning is attached to the output signals from the machine". Similarly: "[it needs to] be shown that the machine has *some* thought with *some* subject matter. Nevertheless, 'the subject matter of a machine's operations' does seem to mean something, at least to the people who deal with it" (1950: 445).

Mays added a scientific remark, citing Gestalt psychology and Lashley's psycho-physiology (Chapter 5.i.b and iv.a). Thinking is very unlike logic, he said, since "the nervous system operates as an organic whole". And he blamed Wittgenstein for putting AI on the wrong track:

> The basic assumption in applying the calculating machine analogy to the mind is that thinking operates in the form of an atomic system. It accepts Wittgenstein's view of the world as a structure of atomic facts . . . The progenitor is, of course, Wittgenstein [in the *Tractatus*] . . . Indeed one might say that modern digital computors [*sic*] are electrified pieces of Wittgensteinian logic. (Mays 1952: 161)

Here, Mays was right. It had been Warren McCulloch's youthful infatuation with logical atomism which eventually led him and Walter Pitts to propose AI, and influenced the design of the von Neumann computer (see Chapter 4.iii–iv).

But that was the early Wittgenstein. The later Wittgenstein saw those ideas as fundamentally misguided: thought was *not* a matter of atomistic logic (Chapter 9.x.d). Nor was language: on the contrary, it's part of our biology, one of our "forms of life"—so "If a lion could speak, we would not understand him" (1953: 223). As for machines, he said:

> Could a machine think?—Could it be in pain?—Well, is the human body to be called such a machine? It surely comes as close as possible to being such a machine.
>
> But a machine surely cannot think!—Is that an empirical statement? No. We only say of a human being and what is like one that it thinks. We also say it of dolls and no doubt of ghosts too. Look at the word "to think" as a tool. (Wittgenstein 1953, paras. 359, 360)

One might say that Turing agreed. For he was arguing that if a machine behaved like us, we would say it was thinking. But here the analogy ended. Wittgenstein was presupposing the presence of a human body and human "forms of life" as criteria for thinking, whereas Turing explicitly discounted these (see Section vii.a).

Mays was one of the very few commentators to pick up Turing's *main* philosophical challenge. Most philosophers were diverted by the Turing Test.

c. The Turing Test: Then and now

The phrase "Turing Test" wasn't used by Turing himself, and it suggests rather more philosophical weight than he'd intended. His friend Robin Gandy (1919–95) recalled the paper being written as lighthearted "propaganda", inviting giggles as much as serious philosophical critique (Gandy 1996: 125).

We saw, above, that Turing's teasing discussion of the imitation game had been aimed at avoiding endless definitions of intelligence. In fact, it spawned a minor—and still continuing—philosophical industry doing just that (e.g. Millican and Clark 1996). The lengthy entry in the online *Stanford Encyclopedia of Philosophy*, for example, lists over forty essential publications—and six Internet sources too.

Some saw the Test as indicating (if not *proving*) not intelligence *as such* but embodied, enculturated, human intelligence—which no computer could achieve, even though it might be genuinely intelligent in some other way (R. French 1990; Boden 1995*b*). And some elaborated it as the Total Turing Test (Harnad 1989), and even as the Total Total Turing Test (Harnad 1991, 1994; Bringsjord 1995). The TTT included full sensori-motor behaviour, or robotics. And the TTTT added "Turing indistinguishability right down to the neurons and molecules"—cloning, again? Most commentators, however, were content to discuss the original pen-pal version.

Despite this huge body of literature, one can't say briefly what the Turing Test *is*. The all-human version of the imitation game was clear enough: an interrogator exchanges written messages with a man and a woman, trying to distinguish them (only the man is allowed to lie). However, Turing's paper suggested three different 'machine' versions.

The first two involved a computer, an interrogator, and another human being—whose output text is compared with the computer's to decide which is which.

The computer plays the part of (1) a man pretending to be a woman, or (2) a man; the other human being is (1) a woman, or (2) a man. The third version involved only a computer and a human interrogator. The human knows that he/she is conversing with a computer, and must decide whether it is thinking. In discussions of the paper, the first definition was almost always forgotten. The second was often ignored. Usually, philosophers—like Turing himself—focused on the third.

Further confusion arose after AI started to achieve practical results. Then, people often considered a fourth case, where the interrogator does *not* know that a computer might be involved.

Sometimes, this situation arose accidentally. One famous example involved a prototype of the ELIZA program described in Chapter 10.iii.a, interacting with the NewFAI researcher Danny Bobrow. Slightly different reports of this dialogue have appeared in print. The version given in (Kaneff 1970: 32–3), and reproduced below, was personally confirmed to me by Bobrow. (However, as the editor of the *SIGART Newsletter* sagely remarked, "it's a good enough story that it probably deserves repetition even if there is not a word of truth in it!": *SIGART Newsletter* 1968.)

A computer salesman had a teletype interchange with Bobrow—or so he thought:

SALESMAN: DO YOU THINK THAT I CAN USE THE COMPUTER THIS MORNING?

TELETYPE RESPONSE: WHY DO YOU ASK?

SALESMAN: I MIGHT BE ABLE TO MAKE SOME ADDITIONAL TELCOMP SALES.

TELETYPE RESPONSE: WHY AREN´T YOU SURE?

SALESMAN: MY PROSPECTIVE CUSTOMERS HAVE NEVER SEEN A DEMONSTRATION OF THE SYSTEM.

TELETYPE RESPONSE: DOES THIS MEAN VERY MUCH TO YOU?

SALESMAN: OF COURSE IT DOES.

TELETYPE RESPONSE: YOU SEEM VERY POSITIVE ABOUT THAT.

SALESMAN: THIS IS RIDICULOUS.

TELETYPE RESPONSE: WHAT DO YOU MEAN THIS IS RIDICULOUS?

SALESMAN: IS THERE ANY REASON WHY I CAN´T USE THE COMPUTER NOW?

TELETYPE RESPONSE: TELL ME MORE ABOUT YOUR PROSPECTIVE CUSTOMERS.

SALESMAN: PLEASE DIAL ME UP ON 491–1850

At that point, no teletype response occurred. (The salesman hadn't put a full stop at the end of his curt request, and ELIZA always needed a full stop to trigger a response.) And, of course, no phone call was forthcoming either. Eventually, the furious salesman phoned Bobrow direct: "Why are you being so snotty to me?" Bobrow, still half-asleep, naturally asked: "What do you mean, why am I being so snotty to you?" The reply was unprintable.

Journalists and others, some of whom should have known better, celebrated this episode as ELIZA's having "passed the Turing Test". However, it didn't fit any of the definitions of the imitation game given in Turing's paper.

More to the point, the fourth-case situation sometimes arose by design. A few years after Bobrow was so rudely awakened, an experiment was carried out to "validate" Kenneth Colby's computer model of paranoia (Chapter 7.i.a). A random group of psychiatrists were asked to give diagnostic interviews by teletype, and some were connected not to a mental patient but to PARRY (Colby *et al.* 1972). Another group were asked to rate the transcripts for paranoia. And a third group were sent the transcripts, told that some had involved a computer, and asked to say which.

Sure enough, no one in the first group realized they were interviewing a program. The "weak" and "strong" versions of Colby's model were diagnosed, by the first two groups, as slightly and highly paranoid, respectively. (Occasionally, PARRY's linguistic limitations led to a diagnosis of physical brain damage.) And the *man or machine?* guesses of the third group achieved only chance level.

Colby himself had the wit to see that this wasn't a Turing Test as the master had defined it. He pointed out that, had the interviewers been warned, they would have asked different questions, designed to discover whether the interviewee was a program. In a follow-up experiment, he did warn the judges beforehand—and they were mistaken almost as often (Heiser *et al.* 1980). Again, however, this wasn't the unrestricted test imagined by Turing: the psychiatrists' freedom to question was limited by medical ethics (*one* communicant was a human patient) and professional focusing (on paranoia, and its simulation). And again, Colby was explicit about this, referring to "*Turing-Like* Tests" and stressing the *limitations* of such experiments in assessing simulations. Similarly, John Clippinger (1977, ch. 9) pointed out that the plausibility tests done on ERMA weren't reliable as validations of his psychological theory (Chapter 7.ii.c). Other commentators were often less careful, describing the programs in these (and similar) examples as having passed the Turing Test.

The high media profile given to this type of case not only brought the Turing Test into the layman's vocabulary, but also reinforced its presence in the vocabulary of AI. The early AI researchers had been inspired by the scientific vision of Turing's 1950 paper, and its many challenges to their ingenuity. In addition, most had read it as providing a criterion of their success. Especially in the 1960s and early 1970s, AI work was often judged in terms of some version of the Turing Test. Colby's experimental "validation" was a case in point. And most philosophers agreed that the Test was an appropriate criterion. Hubert Dreyfus, for instance, declared it to be "just what was needed" for evaluating AI (H. L. Dreyfus 1972, p. xxi).

Today, outsiders still use TT comparisons to evaluate AI's achievements—usually, to criticize them. By 1980 however, serious AI professionals were much less likely to judge their work in these terms. Bernard Meltzer, first Editor of the journal *Artificial Intelligence*, had already argued that the Turing Test was not only irrelevant but constraining, since it *limited* AI to considering human intelligence (Meltzer 1971). From 1980 onwards, AI researchers in general came to agree with him.

John McCarthy, in a presidential pep talk to his fellow AAAI members, specifically said that improving standards in AI research *did not* require passing, or even considering, the Turing Test: it was a "challenge", not a "scientific criterion" (1984: 8; see 11.iii.b). A few years later, when AI was no longer in its infancy and so-called "failures" had multiplied, several writers described evaluation by Turing Test as positively damaging to AI (Hayes and Ford 1995; Whitby 1996*a*; Sloman 2002). The main exception concerned people writing games and entertainments in the 1990s, who talked about "believable" agents and valued virtual-reality systems that could 'fool' the user to some extent (Chapter 13.vi).

To be sure, in 1991 the Boston Computer Society sponsored a public run of the Turing Test, the American Association for AI reported it in their magazine (R. Epstein 1992), and the event was subsidized by the National Science and Sloan Foundations. This was the first in an annual competition (visible live on the Web since 1999) funded by Hugh

Loebner. The yearly prize is $2,000, for the "best" entry. But the rules state that $25,000 awaits "the first computer whose responses [are] indistinguishable from a human's" (see the official web site: <http://www.loebner.net/prizef/loebner-prize.html>). And entrants at that level will also be able to compete for the Grand Prize of $100,000, which requires "audio-visual capabilities" too.

(One might regard this as somewhat niggardly. For Ed Fredkin had set the same sum aside in 1980 for the first program to beat a reigning world champion in chess—no audio-visuals required. The Fredkin prize was won in 1997, by IBM's Deep Blue: Hamilton and Hedberg 1997; Michie 1997. Then Deep Blue was disassembled, never to play again: Hsu 2002.)

By the end of the century, the Loebner competition had moved around the world, to be sponsored by Flinders University in South Australia and then by the London Science Museum. However, the event always was—and still is—more publicity than science.

The original planning committee involved philosophers as well as computer scientists (Shieber 1994). Early members included Dennett, Quine, and (initially) Bernard Cohen; and the philosopher Block was one of the first referees. (Quine might almost be an honorary computer scientist, for two of his papers on logic spawned a widely used method for minimizing the number of gates or interconnections in computer circuits and chips: Quine 1952, 1955.)

A genuine Turing Test being out of the question, the competition used a highly restricted version: only one topic per conversation, and a non-probing conversational style on the part of the human judge. The programs (and hidden humans) were rated on how "human-like" they were, as well as on whether or not they were human. It turned out, to the judges' surprise, that some programs were mistaken for people—and vice versa!

Dennett eventually resigned as chairman of the prize committee, because they couldn't (then) be persuaded to toughen the test—to include fancier syntax and pronominal reference, for instance (Dennett 1998: 27–9). A few years later, the restrictions on topic hopping and probing were lifted. And the judges now include experts in NLP and other areas of AI.

But the competition still attracts little interest from AI professionals (although the 1997 prize was won by a team led by Yorick Wilks, a leading NLP researcher whose "preference semantics" was mentioned in Chapter 9.x.d). It won't make anyone's fortune. Even the intermediate prize will probably remain forever unclaimed, and achieving apparently human performance *in the general case* is impossible in practice (and perhaps also in principle).

More important, it's now recognized, at least by insiders, that the Turing Test isn't an appropriate way of judging AI's progress. This was made crystal clear in Turing's suggested landmark year. The Loebner prizewinner in 2000 was a program much nearer to ELIZA than to SHRDLU or LUNAR. And NLP research had already progressed hugely since all of those (Chapter 9.x–xi). In any event, AI's aim isn't *épater les bourgeois*. Highly human-like performance is only rarely required, even in technological AI.

The exceptions include certain applications of speech processing. Someone needing an artificial voice will normally want one that sounds as human-like as possible—not to deceive their interlocutors, but to put them at their ease. (I say "normally" because there is at least one such person who's said he doesn't want a human-sounding speech synthesizer: see 9.xi.g.) These voices may be not only humanlike, but plausibly idiosyncratic: early-millennial speech-synthesizers can pronounce written text in many different local accents (9.xi.g). Similarly, someone wanting a computerized "companion" may well prefer to hear a pleasing human-like voice, and familiar regional pronunciation (see 13.vi.d). In the general case, however, the Turing Test is irrelevant for AI.

Or rather, the Turing Test *as philosophers normally understand it* is irrelevant. In a recent issue of the *AI Magazine* devoted to a twenty-five-year retrospective on AI, one of the invited articles did indeed discuss the Turing Test. It even declared that "the test still stands as a grand challenge for artificial intelligence, it is part of how we define ourselves as a field, it won't go away" (P. R. Cohen 2005: 61). But its title was a give-away: 'If Not [*sic*] Turing's Test, Then What?' For the writer was arguing that "Turing's test is not irrelevant, *though its role has changed over the years* . . . [Among] AI researchers, the question is no longer, 'What should we do to pass the test?' but, 'Why can't we pass it?' " (italics added). These shifting attitudes to the TT in AI's professional community, despite its unchanged role in the minds of the general public (and most philosophers), have been noted also by Whitby (1996*a*) and by Robert French (2000). In short: although the test "won't go away", passing it isn't regarded as AI's core goal, or as the key criterion of its success.

After AI programs had entered the public domain, the AI community asked whether passing the Turing Test—even in the attenuated sense of a program's evoking trust in the user—is actually desirable (cf. Boden 1977, ch. 15). Professional codes of practice were suggested, to prevent the production of deceptively 'human' systems and to limit marketing hype (Council for Science and Society 1989; Whitby 1988). Marketing hype continued, nonetheless.

But the potential for deception depends on the sophistication of the user. An early medical-assistant program (dealing with issues in medical ethics) explicitly reminded its users that they might have relevant knowledge which it didn't, and that only they could make the decision required (Sieghart and Dawson 1987). Whether such warnings are appropriate at any given point in AI's history depends on what the users (in this case, medics and paramedics in Great Britain) can be assumed to know about AI.

This is why Turing's prediction about the 30 per cent fooling of "the average interrogator" failed. It might, perhaps, have been borne out in the underdeveloped world. But, by the year 2000, people in technological societies already knew too much about what computers can and can't do. A computer salesman accidentally connected to ELIZA today wouldn't ever reach the stage of raging at Bobrow.

Admittedly, language generation—needed for the imitation game—is still the least impressive aspect of NLP research (see Chapter 9.xi.c). Deception in other modes is sometimes possible. A real-time jazz improvisation system can fool many people for much longer than five minutes (Paul Hodgson, personal communication). A program can compose music indistinguishable (by all but experts) from that of Bach, and other famous composers (see Chapter 13.iv.b). And I've found that a program that interprets marks of expression in musical scores (a more complex problem than one might think: Longuet-Higgins 1994) can play Chopin's romantic Fantaisie-Impromptu in C sharp minor—which lasts about four minutes—so convincingly that even musically experienced AI researchers often can't tell that a computer is 'at the keyboard'. To be sure, the Turing Test (*sic*) allows *any* comparison, other than bodily form. But Turing's imitation game (*sic*) allowed only comparisons made via teletyped language.

By contrast, Turing's prediction about changing word usage came true long before he expected. By the turn of the millennium, the new usages had spread far beyond science. People now speak about computers in psychological terms as a routine, everyday, matter.

For instance, as Scriven (1953: 235) foresaw, they speak about "consulting" the computer, not just "using" it. Indeed, changes have occurred in the opposite direction too, computing terminology being used to describe human thought. My own favourite example (you probably have yours) came from the mouth of a British Museum curator of Pacific artefacts—hardly a paradigm member of the computer culture. While talking about how to tell indigenous peoples about the ways in which Aids spreads, she said of one suggested approach that "People just can't compute it."

But was psychological language being understood literally, when applied to computers? Had spontaneous belief and/or educated opinion changed, along with the use of words?

The sociologist Sherry Turkle tried to find out. In the late 1970s, when some children had already had regular access to computers for some years, she found that these youngsters were happy to describe computers as (really) "thinking" and "intelligent", but not as having "feelings" or "emotions", nor as being "alive" (Turkle 1984).

Some years later, when A-Life technology was widely available, she found that both children and adults still insisted that computers aren't "really alive, as we are" (Turkle 1995). But they vacillated (as well as disagreeing) over whether robots, and/or VDU-screen creatures, are alive merely as ants are, sort of alive, or not alive (though admittedly in control of their own movement). Sometimes, these distinctions were brushed aside with the remark "It's just a machine".

That is, the people interviewed by Turkle tried to draw a principled distinction between computers and human beings—though not necessarily in terms of *thinking*. But they often did so by giving mutually inconsistent definitions in different cases. The rest of this chapter outlines how philosophers of cognitive science have tried to address the man–machine distinction more rigorously—whether to insist on it or to deny it.

16.iii. Functionalist Freedoms

Functionalism was first defined as such by Putnam (1926–), in 1960. (He didn't actually use that word, but he did define mental events in terms of causal–computational functions, as we'll see.) But it had been more or less explicit in other thinkers too—who weren't professional philosophers, and whose work was read hardly at all by the philosophical community.

One shouldn't assume, then, that there were no functionalists before Putnam. However, all self-styled philosophical "functionalists" were inspired by Putnam's early work.

That word "early" is important. Many years later, Putnam would roundly reject his earlier approach, as we'll see in Section vi. Here, and in Sections iv–v also, "Putnam" means early Putnam.

a. Just below the surface

Functionalist approaches to mind were sketched as early as 1943 by McCulloch and Pitts, and also by Craik (see Chapters 4.iii–iv and vi, and 14.viii). I don't mean merely that their psychological research was functionalist in spirit. In addition, all of them engaged in explicitly *philosophical* argument, relating their work to the views of Kant, for instance, and to the nature of universals, cause, and number.

Craik, for example, repeatedly described his book as offering a new "philosophy" (see 4.vi.c). His position was as much brain-as-machine as mind-as-machine, for he identified "the nature of thought" as the use of cerebral models whose *physical* properties paralleled those of external reality. Anticipating the inevitable anti-materialist objections, he declared:

> [People who scorn materialism] deserve criticism for blindly refusing to consider whether things are always what they seem—whether their own powers of introspection are able to tell them the secrets of their own mental processes and whether many of their own acts are not very similar to those of the machines, natural and man-made, which lie all round them and which they will not take the trouble to understand. (Craik 1943: 98)

> [There] is something wonderful in the idea that man's brain is the greatest machine of all, imitating within its tiny network events happening in the most distant stars, predicting their appearances with accuracy, and finding in this power of successful prediction and communication the ultimate feature of consciousness. (p. 99)

And he drew a robust epistemological moral:

> Further, I see no difficulty in understanding how anything so "different" from physical objects and concepts and reasoning can tell us something more about those physical objects; for I see no reason to suppose that the processes of reasoning *are* fundamentally different from the mechanism of physical nature. On our model theory neural or other mechanisms can imitate or parallel the behaviour and interaction of physical objects and so supply us with information on physical processes which are not directly observable to us [and which may even be imaginary]. Our thought, then, has objective validity because it is not fundamentally different from objective reality but is specially suited for imitating it . . . (p. 99)

It followed that the classic questions of philosophy were to be interpreted in a new way. One example (out of many):

> Our question . . . is not to ask what kind of thing a number is, but to think what kind of mechanism could represent so many physically possible or impossible, and yet self-consistent, processes as number does. (p. 55)

Despite claiming to have glimpsed "the secrets of [our] own mental processes", however, Craik didn't explicitly address the hot topics outlined above in Section i. Or rather, he made many remarks about the *topics*, but very few references to the contemporary philosophical *literature*. (By 1943, of course, only two of the four key contestants had appeared on stage: see Section i.b.)

It's not clear that many philosophers of the 1940s read his work, although the Cambridge-based and scientifically minded Braithwaite and Margaret Masterman occasionally mentioned him. If they had, they might have found it highly suggestive. But they might not have afforded it the status of "yet another philosophy" (Craik

1943, p. vii) because, by philosophers' standards, Craik's argument wasn't sufficiently rigorous.

A functionalist approach was implicit also in Turing's *Mind* paper of 1950. And, in his anticipation of various objections, he said a little about some questions in the philosophy of mind. Even so, he didn't engage with the philosophical literature, and he deliberately avoided discussion of the currently competing 'isms'. Philosophers did read his paper, of course—indeed, they flocked to it in droves. But as we've seen, they focused on the semi-joking behaviourism, not on the wholly serious functionalism.

As a result, these early functionalist ideas were initially picked up by psychologists and computer scientists, not philosophers. By 1960, the Logic Theorist had been proving the theorems of *Principia Mathematica* for a full four years (see Chapters 6.iii.c and 10.i.b). Moreover, its authors explicitly denied that they were making any claims about the structure of the brain:

> Discovering what neural mechanisms realize these information processing functions in the human brain is a task for another level of theory construction. *Our theory is a theory of the information processes involved in problem solving and not a theory of neural or electronic mechanisms for information processing.* (Newell *et al.* 1958*a*; italics added)

In the terminology soon to be introduced by another functionalist philosopher, this was a statement of *multiple realizability*. That is, a psychological (computational) theory can be expressed, and may be true, without making any commitment whatever as to the physical mechanisms that implement it. Put baldly: psychologists can ignore the brain. Put more precisely:

> whether the physical descriptions of the events subsumed by [psychological] generalizations have anything in common is, in an obvious sense, *entirely irrelevant* to the truth of the generalizations, or to their interestingness, or to their degree of confirmation, or, indeed, to any of their epistemologically important properties. (Fodor 1974: 14–15; italics added)

Machine-inspired "structural explanation" had already entered both experimental and Freudian psychology (J. A. Deutsch 1953, 1960; K. M. Colby 1955). Internal models, enabling one to "go beyond the information given", were posited by the 'New Look' cognitive psychologists (Chapter 6.ii). And the provocative *Plans and the Structure of Behavior* was coming off the press (Chapter 6.iv). These writings teemed with philosophically relevant remarks. Even so, hardly any philosophers at the time had their eye on the computational ball.

Masterman did (and Braithwaite, too). But she was thinking about conceptual structure and language, not mental processes as such (Chapter 9.x.d). Across the Atlantic, Putnam did too—and he *was* concerned with the metaphysics of mind.

Many years later, he said, "I may have been the first philosopher to advance the thesis that the computer is the right model for the mind" (Putnam 1988, p. xi). Given the views of McCulloch, Craik, and Turing, perhaps he should rather have said he was the first *card-carrying* philosopher to do so. However, as a card-carrier he could offer the rigorous argument, and the attention to philosophical 'isms', which Craik couldn't. It's not surprising, then, that Putnam's work, unlike Craik's, *was* seen as "yet another philosophy". Functionalism, at last, had appeared above the surface of the water.

b. The shackles loosened

Putnam's statement of functionalism in 1960 was hugely influential. His paper on 'Minds and Machines' didn't catch philosophers' attention through informing them of technical results. For AI—including Putnam's own recent work on automatic theorem proving (10.iii.b)—wasn't even mentioned, though Noam Chomsky was. Rather, it excited them because it promised an escape from the mid-century philosophical impasse described in Section i.

Indeed, it dismissed the previous philosophies as "empty"—and irrelevant to scientific psychology. This was Putnam's verdict even on Smart's identity theory, which he saw as the best of a bad bunch. And, as though to put icing on the cake, his paper picked up Turing's gauntlet at last—treating the challenge not as a threat but as an inspiration.

Putnam studied at Princeton, taught philosophy of science at MIT in the early 1960s, and crossed the Charles River to Harvard in 1965. He was especially interested in the philosophy of logic and mathematics, and in computational logic too. For instance, it was he who proved that Chomsky's transformational grammar was equivalent to a Turing machine (see Chapter 9.vi.d). Moreover, his (LT-inspired) paper on computational theorem proving was cited in Marvin Minsky's 'Steps Toward Artificial Intelligence' (10.i.g), and later helped lead to resolution theorem proving in AI (10.iii.b).

Situated in Princeton and MIT, the leading centre for automata theory and AI, Putnam was well aware that these disciplines might have implications for psychology. Initially, he shared the positivists' belief in the "unity of science" (see Chapter 9.v.a, and Oppenheim and Putnam 1958). But he soon recanted, for his functionalist writings depicted psychology as a science in its own right.

Putnam argued that there's nothing metaphysically special about mind. Every mind–body puzzle, including subjectivity, would also arise in an automaton that could inspect some of its own states and reason inductively about them. Even introspective infallibility would be paralleled, if the machine's Turing table led it to print *I am in state A* whenever it entered state A: since this would be automatic, there would be no room for "mistake". As for souls, he tartly remarked that if dualism proves that people have souls, it proves that Turing machines do too—and he was as loath to ascribe souls to computers as Descartes had been in respect of "worms, gnats, [and] caterpillars" (see Chapter 2.iii.e).

So far, so negative. But to establish these conclusions, Putnam made five substantive claims:

* First, that human beings and other organisms are finite automata, describable by Turing machine tables—including input–output rules specifying the action of sensory/motor organs.
* Second, that these descriptions focus on the machine's (abstract) "logical" states, not its (physical) "structural" states.
* Third, that a given logical state "may be physically realized in an almost infinite number of different ways".
* Fourth, that the logical and structural descriptions of an automaton are analogous to psychological and physiological descriptions of a person.
* And last, that "further exploration of this analogy may make it possible to further clarify the notion of a 'mental state'".

In sum:

> The *functional organization* (problem solving, thinking) of the human being or machine can be described in terms of the sequences of mental or logical states respectively (and the accompanying verbalizations), without reference to the "physical realization" of these states. (Putnam 1960, sect. 3; italics added)

The "further exploration" was provided a few years later (1967*a*,*b*). Abstract Turing machines, Putnam then declared, could in principle be implemented in Descartes's mental substance, if it existed. Actual mental states, however, are in fact implemented in brain states. These may vary. The physiology of pain (for instance) in a dog, or an octopus, differs from ours. But pain as such is defined psychologically, or functionally, not physiologically.

It follows, he said, that psychological predicates can't be understood as Rylean dispositions (see Section i.c). On the contrary, functionalist analyses explain such dispositions. As for the Turing Test, this is unreliable because two very different programs might generate exactly the same behaviour.

Putnam admitted that his appeal to Turing machines was vague—not least because he held that *everything* is a Turing machine, under *some* description. (Both he and Searle would later use this point to attack functionalism: see Sections v–vi.) Even so, Turing machines—or programs—are much easier to study than brains are:

> This hypothesis, in spite of its admitted vagueness, is far *less* vague than the 'physical–chemical state' hypothesis [i.e. identity theory] is today, and far more susceptible to investigation of both a mathematical and an empirical kind. Indeed, to investigate this hypothesis is just to attempt to produce "mechanical" models of organisms—and isn't this, in a sense, just what psychology is about? (Putnam 1967*b*: 435)

Moreover, computer models appeared to carry scant metaphysical baggage. The causal relations between hardware and software seemed to be clear, unlike those between "mind" and "body". (This happy assumption would eventually be questioned: see Section ix.c–f.)

The relevant computational models, said Putnam, should simulate not only perception and motor action but also reasoning, preferences, values, and beliefs. The implication was that a scientific psychology would broadly conserve the psychological relations implicit in everyday language.

These include specific conceptual links, such as those between *pity*, *help*, and *comfort* (see Section i.c), and the general architecture of rationality that's presupposed by the language of intention, accident, and excuse (cf. Austin 1957). But whereas the ordinary-language philosophers saw these relations as conceptual, Putnam saw them as explanatory. He even hoped to find some "normal form" for psychological theories, so that animals' hunger, aggression, and pain (though not their physiology) could be explained in much the same way as ours.

Functionalism, then, was not only a philosophy of mind but also an empirical theory—or rather, a 'schema' for generating empirical theories—about mental states and processes. Such a combination was highly suspect in certain circles, being seen as an example of scientism: the view that scientific facts are metaphysically fundamental, and that all important questions have some scientific answer (Stenmark 2001; cf. N. Maxwell 1984).

Putnam, following his teacher Quine (1908–2000), who favoured a "naturalized epistemology" (Quine 1952), saw philosophy as continuous with science. But Wittgensteinians didn't. He'd already crossed swords with them on the subject of dreaming, arguing—contra Malcolm—that novel scientific discoveries (in this case, REM sleep) may affect the meanings of everyday concepts (1962*a*).

Moreover, he'd already answered Turing's question (see Section ii) in much the same way. Although it's "false" or "improper" to say *baldly* that machines think, or use language, the strong analogy between digital computers and human behaviour makes it reasonable to extend the meanings of those words to cover both (Putnam 1960). Now, in his discussion of 'Psychological Predicates' (later retitled 'The Nature of Mental States'), Putnam was imbuing *all* psychological vocabulary with ideas drawn from computer science.

For philosophers not dismayed by charges of scientism, functionalism was a heady liberation. It promised to loosen the shackles imposed by the problems mentioned in Section i, and some remarked in Section ii.

Not all post-1960 versions of it were solely, or even partially, inspired by Putnam. Fodor's functionalism, for instance, owed much to Chomsky and Jerome Bruner as well as to Putnam (his tutor). Kenneth Sayre's maverick version leant on cybernetics more than on logic and AI (Sayre 1965, 1969, 1976). And my own followed Masterman and *Plans and the Structure of Behavior* (Boden 1965; 1970; 1972, esp. 52–9; and see Preface, ii).

But Putnam's functionalism, which focused on the metaphysical questions without referring to specific empirical data, was hugely important for many philosophers. It licensed talk about mind, while avoiding metaphysically mysterious events, processes, or substance. And it offered many other freedoms, too.

For instance:

* It underwrote the explanatory links between (and within) mind and body that we normally take for granted.
* Thanks to multiple realizability, it saved the main claim of identity theory (that each mental state is identical with some brain state), without stating—falsely—that all mental states of type *x* type are identical with brain states of type *y*.
* It was consistent with materialism but left space for an autonomous, non-reductionist, psychology—intentionality being grounded in causal–computational mechanisms.
* It ruled out psychological behaviourism,
* while saving the subtle conceptual analyses of logical behaviourism.
* But it reinterpreted these analyses as explanatory, identifying mental states in terms of their mutual causal relations.
* It valued what Dennett (1981*a*) would later call "folk" psychology, seeing our everyday talk as a proto-theory, a starting point for scientific study of both human and animal minds.

* Above all, it promised a novel research programme (for both philosophy and psychology) that was free to exploit the rigour, richness, precision, and testability of computational theories and models.

It's no wonder, then, that—for audiences leaning towards science—this new player swept its predecessors from the philosophical stage. Turing was posthumously promoted from spear-carrier to matinée idol, and functionalism soon became the orthodox position in the philosophy of cognitive science.

It wasn't universally applauded, to be sure. Quine, for example, held that psychology is *not* autonomous, because belief–desire vocabulary is in principle reducible to non-intentional terms (Quine 1953*b*, 1960). Others complained that functionalism can't allow for consciousness. Yet others were radically opposed to the entire 'scientistic' project. Some of these objections will be discussed in Sections v–viii.

First, let's look at some of the ways in which functionalism was expressed by its supporters.

16.iv. Three Variations on a Theme

Even orthodoxy allows reinterpretation. Functionalism is "the thesis that the essence of our psychological states resides in the abstract causal roles they play in a complex economy of internal states mediating environmental inputs and behavioral outputs" (P. M. Churchland and Churchland 1981: 121). But this general thesis developed into many mutually disputatious positions.

The most influential philosophers of functionalism, *post* Putnam, were Fodor, Dennett, Paul and Patricia Churchland, and Andy Clark (whose PDP-based philosophy was discussed in Chapter 12.x.e).

Other important accounts, functionalist in spirit if not in philosophical jargon, were offered by Chomsky (Chapter 9.ii–vii), Allen Newell and Herbert Simon (Section ix.b, below), and—later—Minsky (Chapters 7.i.g, 10.i.f, and 12.iii.d). Their scientific research influenced all the philosophers described in this section. (Sloman developed an interesting version in the 1970s too, but this was less influential than it deserved: see Section ix.c, below.)

The late 1960s saw extended statements of functionalism being provided by Fodor (1935–) and Dennett (1942–). Both men eventually became famous—even notorious. But their near-simultaneous books were very different. In part, the differences reflected the fact that Fodor was a cognitive scientist with a strong interest in philosophy, whereas Dennett was a philosopher with a nascent interest in cognitive science.

Fodor was an interdisciplinary animal. He had a joint appointment in psychology and philosophy at MIT, had attended both Chomsky's and Bruner's seminars in the late 1950s, and gave detailed tutorial seminars on Chomsky's *Syntactic Structures* at that time (Chapters 6.i.e and ii.b–c, and 9.viii.c). His first book, *Psychological Explanation* (1968), defended this new type of research. It supported Putnam's claims about multiple realizability and the autonomy of psychology, and spelt out the notion of "functional equivalence" in theoretical psychology. With respect to the philosophy of mind in general, it accused Ryle and Wittgenstein of conflating mentalism and dualism (see Section i.c).

His arguments against behaviourism were well received by sympathetic souls. But for those who'd already rejected it, there were no eye-openers here. Later, Fodor would offer surprises aplenty, with the publication of *The Language of Thought* (1975). Before then, the surprises came rather from Dennett.

a. Content and consciousness

When Dennett's book hit the scene in 1969, he himself was a surprise. Teaching in California since leaving Oxford in 1965, he was still unknown. His first three papers had appeared only a few months earlier, and were critiques of others—including Dreyfus (Dennett 1968)—rather than free-standing statements of his own position. Now, *Content and Consciousness* addressed the topics that would preoccupy him throughout his career: intentionality, consciousness, free will, evolution, and the comparison between human and animal minds.

Integrating challenging philosophical claims with wide-ranging scientific argument, many readers found it an exhilarating book. I remember writing to this total stranger across the seas, to say so—one of only four times I've ever done that. Anti-functionalists, it must be said, demurred. The *Times Literary Supplement* ran a damning review by a Wittgensteinian, who described it as "not a happy piece of work" and complained about the "many references to scientific findings and, it must be confessed, scientific speculations" ([Hamlyn] 1970). (This anonymous review was written by the philosopher of psychology David Hamlyn: Dennett, personal communication.) But many others were excited.

The central-state materialists welcomed this new version of centralism. So Smart, asked by Ryle to review it for *Mind*, decided to write a Critical Notice instead, and a fellow Australian did likewise (Smart 1970; R. L. Franklin 1970). In the USA, Thomas Nagel (1970) wrote a sympathetic, though unconvinced, review—and Gilbert Harman immediately ran a seminar on the book at Princeton. (Harman had already published on the psychological implications of Chomsky's work, and he later co-founded Princeton's Program/Laboratory for Cognitive Science with George Miller: Harman 1988.)

Dennett was soon invited to speak to Harman's group (his first public presentation) and elsewhere, so ending his "five anonymous years of apprenticeship" at Irvine. (He moved to Tufts University in the early 1970s, where he's remained ever since.) His 1969 book, and the paper he read at Princeton in December 1970, set much of the agenda in the philosophy of cognitive science for years ahead.

Dennett was nothing if not heretical. Despite being deeply influenced by Ryle (his ex-tutor) and Wittgenstein, he disagreed with them in important ways. He posited a "sub-personal" (functionalist) level of explanation as the key to understanding mental phenomena, and saw meaning as rooted not in public language and society but in mechanisms evolved in human and animal brains. He disagreed with Putnam, too. He leant heavily on Putnam's logicist account of minds as Turing machines, and often used AI examples in discussing sub-personal mechanisms (e.g. Dennett 1969: 101–13). However, the "functions" he was most interested in weren't purely logical, or even causal, but at base evolutionary (see below).

Moreover, he refused to identify thoughts with either functional states or brain states. To identify *x* with *y* is to say that both terms refer to the same thing—but Dennett argued that mentalistic terms don't refer to anything at all. *Thought*, *belief*, *intention*, *fear*, even *pain*: all these are non-referential (like the word *sake*, in "She did it for my sake"). Specifically, they are items in a mode of discourse we use to ascribe meaning, not to discover it. To be sure, "people *do* have beliefs, intentions, and so forth" (and machines don't: "a computer is no more *really* an information processor than a river *really* [desires to reach the sea]") (Dennett 1969: 89–90). But meaning, or content, isn't an extra item over and above bodily behaviour or brain states.

Here, Dennett was leaning on Quine (1908–2000) and on Wilfrid Sellars (1912–89). Sellars had distinguished the "manifest" and "scientific" images of man (1956). The former sees people as having beliefs, desires, and intentions, while the latter describes them in terms of physics and neurophysiology. These two images were seen by Sellars as distinct logical "spaces"—of reasons and of causes, or norms and facts. They provided different ways of talking about people—with no guarantee of a neat mapping between the two. And no guarantee, either, of reliability: Sellars specifically attacked "the myth of the Given", according to which experiential "data" are an unassailable foundation for knowledge (see Section viii.b).

As for Quine, he'd already argued that the language of belief and desire is an "essentially dramatic idiom" (Quine 1960: 219). Although Quine was Dennett's other philosophical mentor (besides Ryle) (Dennett 1994*b*), Dennett rejected his view that intentional language is in principle reducible to physics. If we stopped using this language, said Dennett, no existing thing need be ignored. But we could no longer interpret anything as meaningful.

Such interpretations assume rationality. When we say that someone carrying an umbrella wants to keep dry, we presuppose a rationally interrelated set of meanings which result in "appropriate" behaviour. This insight wasn't new: we saw in Chapter 5.iii.a that Thomas Dewey and Ralph Perry had said it long before. And (following Wittgenstein) Elizabeth Anscombe (1957) and Charles Taylor (1964*a*) had said it more recently.

But whereas Anscombe and Taylor had taken it for granted that meaning is intrinsic to people and/or language, Dennett asked what it is about human beings that makes it 'natural' to ascribe meaning to them. His highly unfashionable answer, in a nutshell, was: brain states and evolution.

The "crux" of any centralist theory of mind, he said, is "the ascription of content or meaning to particular central states of the brain" (Dennett 1969: 71). For Dennett, this couldn't be done (as both Putnam and Smart had tried to do) by assigning the content of mental states to the relevant brain states: the semantic traffic goes in the opposite direction.

The semantic content of neural states, for Dennett, is based in evolutionary function (pp. 48 ff.). And since evolution works on whole organisms, "one can only ascribe content to a neural event, state or structure when it is a link in a demonstrably appropriate chain between the afferent and the efferent" (p. 78). In calling a retinal cell a bug-detector, for example (see Chapter 14.iv.a), it's crucial that it's linked to a motor circuit that causes the frog's sticky tongue to shoot out appropriately (*sic*) or its leg muscles to make it jump to the right (*sic*) place.

Moreover, neural content is the basis of linguistic meaning:

> [Philosophers have asked] whether events and states of the nervous system could be assigned meanings or ascribed contents, and assigning meanings was seen as associating events or states

with verbal expressions. Verbal expressions, however, are not the ultimate vehicles of meaning, for they have meaning only in so far as they are the ploys of ultimately non-linguistic systems. (Dennett 1969: 88)

To posit meanings in the brain wasn't to posit languages in the brain. Dennett criticized scientists' talk of "brain-writing", and of neural "codes" and "languages", because it usually relied on unexplained mechanisms for understanding such languages (pp. 86–8). This, he said, is to postulate "little men" in the brain. (Bruner and Ulric Neisser had already argued that computational theories could avoid homunculism: see Chapters 6.ii.c and 7.i.b. But they hadn't focused on the problem of meaning, or intentionality, as such.)

As for comparing the brain to "a community of correspondents", this—said Dennett—was "the most far-fetched and least useful" of all the common analogies, since it appears "merely to replace the little man in the brain with a committee". (Ironically, he would later favour Minsky's "society of mind" in his mature theory of consciousness (Dennett 1991): see 12.iii.d.)

This Rylean disdain for homunculi extended also to philosophers' talk of *acts of will*, *volitions*, and the *self* (and to ego, id, and super-ego), when presented as "explanations" of mental life. In Dennett's words: "The solitary audience in the theatre of consciousness, the internal decision-maker and source of volitions or directives, the reasoner, if taken as *parts* of a person, serve only to postpone analysis" (p. 190).

However, to assign a neural basis to linguistic meaning wasn't to say that mental states could be neatly mapped onto neurophysiology: "[we cannot] find precisely worded *messages* for neural vehicles to carry . . . [but this] is merely an inability to map the fundamental onto the derived, and as such should not upset us" (p. 88). It will upset us, however, if we hope that neuroscience will eventually identify the neural vehicles of particular propositional attitudes.

Besides multiple realizability and the complexity of the brain, said Dennett, there's the difficulty that intentional vocabulary "has no real precision of its own". We talk about beliefs, for instance—but just what is it to believe a proposition? How many of its logical implications are involved? And just what does a child believe who announces "Daddy is a doctor"?

These questions, for Dennett, can have no clear answers. His account of belief resembled Wilhelm von Humboldt's of language (Chapter 9.iv.b). That is, what we call the "same" belief is probably different in every individual—and, even more important, can't be precisely pinned down in any particular case.

Despite insisting that ordinary-language terms like *belief* and *desire* are unavoidably imprecise, Dennett didn't deny their usefulness. His view on *conscious* and *aware* was very different:

[The philosopher who seeks to understand consciousness is] forced to do psychology rather than "pure" philosophy.

. . . Can a machine be conscious? This question cannot be answered until we arrive at a conclusion about what it is to be conscious, and ordinary language does not tell us . . . [We need] more than a solution to a conceptual problem via analysis of language, for language is deficient in this area. The way out is an analysis of phenomena at the sub-personal level, and although this leads one into areas many philosophers would prefer to avoid, the alternative is the perpetuation of traditional confusions. (p. 130)

The question *What is consciousness?* has no single answer, because this term is irredeemably confused, "an unhappy conglomerate of a number of separable concepts" (p. 114). Philosophers have assumed ever since Descartes that it's the most accessible concept of all. In fact, said Dennett, it's useless for philosophical or scientific purposes and should be replaced by new concepts that make the relevant distinctions clear.

Among the cases marked by the everyday term are those where someone can give an introspective report of their current thought, and those where they're evidently affected by some stimulus but can't report it (like the car-driver who negotiates a corner while engrossed in conversation). Dennett offered two new concepts, defined by sub-personal criteria, to distinguish such cases.

Following Putnam's example of the introspective automaton (see Section iii.b), he suggested that some functional systems—such as human brains—include a "speech centre". If a mental state is input to that centre, the immediate (Turing-tabled) output is a verbal expression whose content is the same as the content of the input state. This expression, when input to other subsystems, will cause further changes. Often, it leads to speech movements; but these may be inhibited, as in reading silently or simply keeping one's own counsel. For such a system, two senses of awareness can be defined:

(1) A is aware-1 that *p* at time *t* if and only if *p* is the content of the input state of A's "speech centre" at time *t*.
(2) A is aware-2 that *p* at time *t* if and only if *p* is the content of an internal event in A at time *t* that is effective in directing current behaviour. (pp. 118–19)

The sub-personal processes involved in awareness-2, said Dennett, may be very like those in AI models of perception, language, and problem solving (p. 151).

As for whether dogs and other dumb (*sic*) animals are conscious, we must distinguish the two senses. Understood as aware-2, animals are indeed conscious—sometimes, in highly discriminating ways. But understood as aware-1, they are not: in this sense of the term, only language-using creatures can be conscious.

So far, so good. Dennett's distinction was useful when applied to human beings, and even seemed to make retrospective sense of Descartes's views on animal consciousness (Chapter 2.ii.d–e). But there was a sting in the tail—or rather, two.

First, it followed that computers could be conscious. A computer can clearly be aware-2 of things. And it can also be aware-1, if it is set up so as to give verbal reports of some of its internal states. Dennett was unabashed:

> [This] may seem to be an intolerable situation, but only if one clings to the folklore that has accrued to the ordinary word "aware" . . . [There] is no important residue in the ordinary concept of awareness that is not subsumed under either awareness-1 or awareness-2. There is no room . . . for a concept of awareness-3, which would apply only to people and rule out all imaginable machines. (p. 121)

Second, it followed that there are no conscious epiphenomena, no experiential qualities, pure sensations, raw feels, or *qualia*. These denizens of the mind, about which there had been so much argument (see Section i), were mere metaphysical phantoms. This conclusion had been implicit in Putnam's functionalism, but Dennett (like Ryle before him) was resolutely explicit.

Even pain, he insisted, is a wholly functionalist phenomenon:

> Where discriminating is an analysable personal activity, like discriminating good apples from bad by checking for colour and crispness, we can distinguish the qualities from the discriminating of them. But in the case of distinguishing sensations as painful, the act of discrimination itself is the only clue to the localization (in space and time) of the presumed quality. Insisting that, above and beyond our ability to distinguish sensations as painful, there is the quality of painfulness, is thus insisting on an unintelligible extra something. (p. 92)

His comment here was reminiscent of Wittgenstein's, that "a nothing would serve just as well as a something about which nothing could be said" (see Section i.c). For Dennett, if there is no further function there is indeed nothing more to be said. But at the personal level, a verbal expression of pain is "a *bona fide report*", not—*pace* Wittgenstein—"an outcry of sorts" (p. 112 n.). (Some years later, Dennett would argue that the concept of pain is actually incoherent—and that *this* is the reason why one can't build a computer to feel pain: 1978*a*. See also Wall 1974, 1979.)

b. From heresy to scandal

Dennett's multiply heretical volume was strong meat—and a strong sauce would soon be added. Most early discussions of his position considered not just his book but also (sometimes, only) the paper first given at Princeton (Dennett 1971). The book had named two explanatory styles, or "stances" (e.g. p. 101): personal (intentional) and sub-personal (functional/physical). The paper named three: intentional, design, and physical.

The physical stance seeks explanations couched in physical, or purely neurophysiological, vocabulary. This is often infeasible in practice, even where artefacts are concerned, and is functionally uninformative in principle. Indeed, neurophysiologists were already using non-physical concepts, such as bug-detectors and orientation detectors (Chapter 14.iv).

The design stance, by contrast, "relies on the notion of *function*, which is purpose-relative or teleological" (Dennett 1971: 88). It's a form a reverse engineering, which assumes that the system is well, even optimally, designed to perform certain tasks. Artefacts, computers included, are typically understood in this way. The design stance can operate at various levels of abstraction, such as electronic circuitry, transistors and switches, multipliers and dividers, strategic units (like planners and line-finders), or programmed instructions. In other words, there may be many different levels of virtual machine (see Section ix.c).

But Dennett held that natural systems can be understood in this way too. Indeed, the design stance is "the proper direction for theory builders [in psychology and neuroscience] to take whenever possible" (1971: 96). Not only does it avoid unanalysed ascriptions of intelligence, but it can compensate for the *lack* of rationality that bedevils actual intentional systems.

The intentional stance was defined as before. But there were four reinforcements of points already made in *Content and Consciousness*.

First, Dennett declared that "Intentional theory is vacuous as psychology because it presupposes and does not explain rationality or intelligence", and wherever a theory uses intentional language, "there a little man is concealed" (Dennett 1971: 99, 96).

Second, he enlarged upon the fact that people's "rationality" is limited, which causes both principled and practical difficulties in assigning beliefs and desires to individuals. Third, he stressed the instrumental status of intentional discourse, as interpretation not discovery: "a particular thing is an intentional system *only in relation to the strategies of someone who is trying to explain and predict its behaviour*" (Dennett 1971: 87; italics added). And last, he pointed out that it was already impossible, even for the system's designer, to explain or predict the behaviour of the best chess-playing computers except by using intentional vocabulary.

To say of the computer that *It's trying to get its queen out early* is thus not an avoidable metaphor, but the only practicable way of understanding its performance. This description is (pragmatically) justified, even though there may be no specific programmed instruction, or program goal, to *get the queen out early*. The instructions and goals, whatever they are, were designed (*sic*) to have this overall effect. Nevertheless, even the programmer can't remember and/or run through the relevant rules so as to understand what's going on or predict what's likely to happen next.

Previously, Dennett would have said that no computer can *really* try to get its queen out early (see above). Now, he insisted:

> Lingering doubts about whether the chess-playing computer *really* has beliefs and desires are misplaced; for the definition of intentional systems I have given does not say that intentional systems *really* have beliefs and desires, but that one can explain and predict their behavior by *ascribing* beliefs and desires to them, and whether one calls what one ascribes to the computer beliefs or belief-analogues or information complexes or Intentional whatnots makes no difference to the nature of the calculation one makes on the basis of the ascription. (Dennett 1971: 91)

All very moderate, perhaps . . . until one realized that human beings were being classified as intentional systems *in just the same sense*. His ready admission that even the best computers were crude and narrow-minded, by human standards, didn't alter that.

Understandably, Dennett's provocative views sparked lively debate, both inside and outside the functionalist camp. His scandalous dismissal of *qualia*, in particular, aroused huge dissent (see Chapter 14.xi.b). Nagel, for instance, believed that no objective science could possibly explain the subjectivity of consciousness. Hence his unconvinced review of Dennett's first book (1970), and his memorable paper 'What Is It Like To Be a Bat?' (1974)—later answered by one of Dennett's students with 'What Is It Like To Be Boring and Myopic?' (Akins 1993).

Today, with respect to *qualia*, Dennett remains in the minority. But he's not alone. Sloman, for many years, has analysed consciousness in terms of a complex virtual machine—and *qualia* as computational phenomena within it (Sloman 1978, 1999; Sloman and Chrisley 2003).

At the end of the century, with most of his professional colleagues and lecture audiences still unpersuaded on *qualia*, Dennett would tell the (true) story of a card trick called 'The Tuned Deck'. This trick bemused even groups of expert magicians for many years. As an old man, its inventor revealed the secret: instead of creating *one exceptionally ingenious* card trick, he had craftily hopped about between *many familiar* ones. Once his expert audience had discounted trick A, because he'd fooled them into expecting it just before using trick B, he could use trick A at any time without their noticing it—and similarly for tricks B to Z.

'Pure' experience, for Dennett, is eternally elusive for much the same reason. Once we have detailed *all* the many functions—verbal naming and description, discriminatory behaviour, emotional responses, conceptual associations . . .—involved in seeing red, or smelling lilac, *nothing more remains* to be explained. By the same token, he now says, if you took a pill to get rid of only your toothache's *qualia* but not its causal effects—neural stimuli, wincing, rubbing your jaw, lack of concentration, bad temper . . .—then *you wouldn't even know*.

Dennett has become convinced, however, that disagreements on this issue are often rooted in a clash of intuitions quite impervious to argument (lecture at University of Sussex, May 2000). He's penned various imaginary conversations with a common-sense sceptic called Otto (one was given in Chapter 14.xi.b). But Otto is never persuaded, despite Dennett's responding to every one of his outraged objections.

Dennett put other issues on the agenda, too. His comments on the imprecision of belief led some functionalists to recommend that cognitive science abandon folk-psychological language entirely (P. M. Churchland 1979; Stich 1983). And his instrumentalism with respect to mental states—along with his "flagship" renunciation of it (in terms of "real patterns", and "*abstracta*" such as centres of gravity) (1981*b*; cf. 1987: 3)—caused gallons of ink to be spilled.

(Most of that ink was philosophical. But psychologists' pens were eventually wielded too. The intentional stance was applied in empirical studies of the "Theory of Mind" in young children, non-human primates, and other species: see Chapter 7.vi.f.)

Several of Dennett's later works received enormous attention (e.g. 1978*b*, 1987, 1988, 1991, 1996). This was especially true of his "multiple drafts" theory of consciousness, which pictured the mind as a virtual machine vastly more subtle (and more scientifically grounded) than his aware-1/aware-2 distinction had done (see 14.xi). A notorious aspect of his theory of consciousness was his attack on *qualia*; this had been a long time a-brewing, for it was first delivered late in 1978 and was widely circulated in draft before being published ten years later. Only his analysis of free will (1984*a*), in my opinion, received less regard than it deserved (see Chapter 7.i.g).

Today, the debate still rages: for an indication, see *Behavioral and Brain Sciences* (1988); Dahlbom (1993). But despite the elaboration of his views on consciousness, there's been no fundamental change in Dennett's position. On rereading *Content and Consciousness* after twenty years, he commented: "I am struck more by my doctrinal consistency than by my developments. Most of the changes seem to me to be extensions, extrapolations, and further arguments, not shifts" (1987, p. xi) (see also Dennett 1994*b*).

c. Must angels learn Latin?

In the 1970s, while Dennett's early writings were setting the philosophy of mind alight, Fodor was continuing his own attack on the Ryle–Wittgenstein approach (Fodor 1975: 2). His first book had appeared shortly before Dennett's, as we've seen. But his visibility soared in 1975, when his volume on *The Language of Thought* offered a form of functionalism very different from Dennett's.

It differed partly in its philosophical claims and concerns, for it was unambiguously realist about mental states and it ignored consciousness and freedom. But it differed also in its attitude to the practice of cognitive science.

Dennett had used chess programs and memory research as examples, but he wasn't especially concerned to help cognitive scientists do their job. Or rather, his help consisted in trying to dispel the conceptual confusions he felt they shared with many philosophers. Only much later would he collaborate closely with psychologists, ethologists, neuroscientists, and roboticists (see e.g. Dennett 1994*a*, 1996, and Chapter 15.vii.a). Fodor, by contrast, was committed to computational psychology and to Chomskyan linguistics, as such.

His early work referred to highly detailed research on perception, concept learning, decision making, problem solving, and—above all—psycholinguistics. He was a leading champion of Chomsky, and had already co-authored a textbook on the psychology of language (Fodor *et al*. 1974). He was also inspired by GOFAI, though more for its reliance on formal symbolism than for its modelling of specific mechanisms. And he adopted—and never relinquished—the implication of Putnam's 1960s papers, that "we have no reason to doubt that it is possible to have a scientific psychology that vindicates commonsense belief/desire explanation" (Fodor 1987*b*: 16).

In short, Fodor took the dual role of functionalism—as philosophy and theoretical psychology—especially to heart. That had been evident in his first book (1968), but it was even clearer in *The Language of Thought*. This aimed to provide clear theoretical underpinnings for the cognitive revolution already under way. It was a paradigm of 'classical', formalist, cognitive science. For it described intentional processes as syntactic operations defined on mental representations (see also Fodor 1978*b*).

This view was boldly presented as the only "even remotely plausible" candidate for a scientific psychology. There was no middle way, either then or later. At century's end, Fodor had developed serious doubts about whether all mental processes are computations. Nevertheless, he still held the same opinion about what a scientific psychology must be like: "[computational psychology] is far the best theory of cognition that we've got; indeed, the only one we've got that's worth the bother of a serious discussion" (Fodor 2000*b*: 1).

The book quickly became influential, even infamous. Like Newell and Simon before him (1972), Fodor insisted that the mind (or brain) is "literally" a computational system. Functionalist talk shouldn't be, though it often was, interpreted as a mere *façon de parler* (1975: 51, 76). And he drew two major implications, summarized as "no computation without representation" and "there's no point in angels learning Latin" (pp. 34, 86).

The first of these, if not the second, might prompt one to ask "Why the fuss?" After all, cognitive psychologists, Chomskyan linguists, and functionalist philosophers had been positing computation and internal representations for over fifteen years.—Yes, said Fodor, but they hadn't been clear about what these are. They hadn't realized that "Computation presupposes a medium of computation: a representational system," and that "The pressing question is what properties does the system of internal representations have." This wasn't a purely philosophical question: the answer must fit the facts, as any scientific theory must do (pp. 27, 33, 156).

Even this may seem fairly anodyne. But Fodor also said:

> [What] I am proposing to do is resurrect the traditional notion that there is a "language of thought" [later often referred to as LOT] and that characterizing that language is a good part of what a theory of the mind needs to do. (1975: 33)

That was guaranteed to raise many hackles. As we saw in Chapter 9.ii–iv, philosophers in past centuries had posited an internal language of thought to explain adult reasoning and/or to explain how children manage to learn their mother tongue. To adopt the first goal would upset the Wittgensteinians, who saw no room for any level of explanation between natural language and neurophysiology (which they assumed wouldn't employ any computational notions): see Section v.f, below. To adopt the second would outrage not only Wittgensteinians, with their commitment to public language, but also empiricists—who had already attacked Chomsky's nativism con brio (Chapter 9.vii.c). Never one to fear controversy, Fodor adopted both.

His central claim about mental representations was uncompromising, and very different from Dennett's:

> [Modern cognitive psychology assumes that] the computational states ascribable to organisms can be directly explicated as relations between the organism and *formulae*; i.e. formulae in the internal code . . . [And, even more importantly, it assumes that] for any propositional attitude of the organism (e.g. fearing, believing, wanting, intending, learning, perceiving, etc., that *P*) there will be a corresponding computational relation between the organism and some formula(e) of the internal code such that (*the organism has the propositional attitude if the organism is in that relation*) is nomologically necessary. (p. 75)

These formulae, said Fodor, are required not primarily for (public) communication, but for representing concepts, goals, and the like. Even animals, if they can represent such things, must have a language of thought. Internal representations are real structures, apt for discovery rather than mere interpretation. They are composed of atomic primitives, whose computational (semantic) properties determine the properties of the whole—as in the formulae of symbolic logic, or the sentences of natural language.

As for just what those primitives might be, they certainly included the basic concepts of the *innate* language of thought. And if his early 1960s work in generative semantics (Chapter 9.viii.c) had been correct, these would suffice (see below). However, worries about computational efficiency had now convinced him that natural-language words can't, in practice, be analysed into semantic atoms every time they're used. Rather, they're still present (as *functional* primitives) in the adult's internal code:

> [There] is no process for [analytic] definition *at all*; i.e., both the defined expression and its definition appear as items in the primitive vocabulary of the representational system. (1975: 133)

> Learning a definition principally involves learning a meaning postulate. It thus adds to the constraints (not on computing memory but) on long-term memory; it adds a rule of inference to the list that is stored there. That is why . . . abbreviatory definition and other recoding schemes make formulae easier to understand: Computing memory is expensive, but long-term memory is cheap. (p. 150)

In other words, an adult's language of thought includes all the words they know in their mother tongue. Hence Fodor's wry salute to his philosophical opponents: "There may, then, really be some point to the late Wittgensteinian insistence upon the surface richness of natural languages" (p. 156).

He also conceded that recent psychological research on the nature of concepts seemed to support the later, rather than the earlier, Wittgenstein (see Chapters 8.i.b and 9.x.d). That is, concepts appeared to be stored as stereotypes, exemplars, or even images rather than definitions. But he saw this as "terribly difficult", asking "How, for example, does one *access* an exemplar? If your concept of a dog is . . . a representation of a stereotypic dog, how do you go about determining what *falls under* the concept?" (p. 153). Ten years later, PDP connectionism seemed to many people to have answered his question (see 12.x). But Fodor was unconvinced, and later defended a strictly atomistic theory of concepts (Fodor 1998*a*).

Just what representations are actually involved in thinking was still unclear. Fodor allowed that they might include images (1975: 174–94). But although visual imagery could be a "vehicle of reference" it couldn't be a "vehicle of truth"—in other words, a thought. Thought, he said, is the ascription of properties to things, and is essentially propositional and rational: "The sequence of events that causally determines the mental state of an organism will be describable as a sequence of steps in a derivation if it is describable in the vocabulary of psychology at all" (p. 198).

This claim carried another surprise. Putnam had implied that all mental states, and a fortiori all propositional attitudes, are grist to the functionalist mill. But Fodor denied this. For him, psychology required computational causes. It included perception and concept learning, since these (or so it was thought at the time: see below) involved the generation and testing of hypotheses. And it might include some Freudian examples (p. 200), such as neurotic beliefs semantically generated by defence mechanisms (Chapter 7.i.a). But much of our most systematic and interesting mental life, he argued, was probably excluded.

Sensation, he said, isn't caused by computation. Creative thoughts might not be. Many associative processes probably aren't. And nor are emotional influences on perception and belief. Such matters might be explained by biology (neurophysiology), but not by psychology. In sum, functionalism's scope had been significantly reduced: "It may be that we are laboring in quite a small vineyard" (p. 202).

What about the notoriety? This arose because Fodor claimed that the innate language of thought was "as powerful as any language that one can ever learn" (p. 82).

He confessed that this claim was "scandalous", but insisted that no serious alternative had been proposed: "The only coherent [account of language learning] is one which presupposes a very extreme nativism" (p. 96). Human babies don't have adult concepts—such as *air-plane*—ready-formed. But they are born with the representational capacity (concepts and combinatorial operations) to express the meaning of any concept that can be learned later (p. 152). More accurately, they're either born with these concepts and operations or they mature, biologically, so as to attain them. What they do *not* do, is learn them.

Fodor's argument for this highly counter-intuitive position leant crucially on Bruner's research on learning and Chomsky's views on innate grammar (see Chapters 6.ii.b and 9.vii). Following Bruner, he saw concept learning as "essentially inductive extrapolation, [which] presupposes a format for representing the experiential data [and] a source of hypotheses for predicting future data" (p. 42). Since language learning involves learning not only what counts as a sentence but also the semantics of the vocabulary, it follows (he said) that "one cannot learn a first language unless one already has a system capable of representing the predicates in that language" (p. 64). Every word must be translatable into the innate language of thought, even though (as remarked above) it isn't normally translated into it.

The very possibility of an innate language, whatever its representational power, was highly controversial. Ryleans would scent an infinite regress of understanding (Dennett's "little man"), and Wittgensteinians would reject any pre-cultural private language. Fodor countered both objections by a brain–computer analogy:

> though the machine must have a compiler if it is to use the input/output language, it doesn't *also* need a compiler for the machine language. What avoids an infinite regression of compilers is the fact that the machine is *built* to use the machine language. Roughly, [the formulae of the machine language] correspond directly to computationally relevant physical states and operations of the machine . . . (p. 66)
>
> On this view, what happens when a person understands a sentence [in a language he has learned] must be a translation process basically analogous to what happens when a machine "understands" (viz., compiles) a sentence in its programming language. (p. 67)

He used this computer analogy also to explain why babies have to learn their mother tongue—even though, according to him, they already have the capacity to represent the meaning of every dictionary word. People need natural languages for much the same reasons that they need programming languages (see 10.v). We don't have world enough, or time, to express everything in terms of primitives—or to remember it, if we did.

And this brings us, finally, to the angels:

> If an angel is a device with infinite memory and omnipresent attention—a device for which the performance/competence distinction [see 7.iii.a] is vacuous—then, on my view, there's no point in angels learning Latin; the conceptual system available to them by virtue of having done so can be no more powerful than the one they started out with. (1975: 86)

Only a philosopher would have sailed so close to the wind as to risk this seeming absurdity. Like Dennett's denial of *qualia*, it exemplified the fourth cause of (non-philosophers') irritation identified in the preamble to this chapter. Fodor himself admitted blandly, even happily, that it looked like a *reductio ad absurdum* (p. 82).

Many critics noted that his argument was based in ignorance, for he assumed that no non-inductive theory of concept learning was conceivable. Philosophers also sought other flaws in his argument (e.g. Dennett 1977: 273; P. S. Churchland 1978; Sloman 1987*b*). As for the psychologists, most simply ignored this self-confessedly "scandalous" claim.

But both communities—and eventually, neuroscientists too—hotly debated many of his other claims (see, for example, Chapters 7.v.i, 12.x, 14.ix.e, and 15.vi–viii). That is, they questioned the existence, and the nature, of each of the following:

* an innate language of thought;
* real, individuated, representations;
* formal–computational (and compositional) internal states;
* mental states whose semantic relations depend on their syntactic structure;
* scientifically describable carriers of specific propositional attitudes;
* a psychological science approximated by common-sense belief–desire psychology;
* non-computational associative thought;
* and non-conceptual semantic content.

Angels aside, then, Fodor's influence was enormous. Even Dennett, who disagreed with him on many points, said: "Fodor challenges us to find a better theory, and I fully expect that challenge to be met, but when better theories emerge they will owe a good deal to Fodor's reconnaissance" (Dennett 1977: 280).

d. Fodorian frills

If Fodor's reconnaissance had led to surprises, so did his next scouting party. In 1980 he undertook "a meditation upon the consequences of assuming that mental processes are formal processes" (Fodor 1980*a*: 64).

He concluded that a "rational" psychology (Chomsky's term: see Chapter 9.ii–iv) must be "methodologically solipsist" (Putnam's term, but first used by Russell). And this implied that psychology will *never* do what it's normally intended to do—namely, explain how we gain knowledge of, or even beliefs about, the world.

Putnam's part in this philosophical drama was to have set the stage by distinguishing two general approaches to intentionality (Putnam 1975*a*). These differ over whether the meaning (and therefore the identity) of every mental state depends on the existence, and nature, of anything outside the subject whose state it is.

For proponents of "wide" meaning, such as the recently converted Putnam himself, it does. (As he put it, "'meanings' just ain't in the *head*!"—Putnam 1975*a*: 227.) For champions of "narrow" meaning, it doesn't. They are *methodologically* solipsist, said Putnam (p. 220): for them, the content of our mental states could be just the same even if nothing else existed. (Theories of wide and narrow meaning are often termed externalist and internalist, respectively.)

Fodor, *qua* computational psychologist, danced on the narrow side of the stage. Formal computation, being purely syntactic, excludes semantic concepts such as truth and reference, so can say nothing about the world. Terry Winograd's SHRDLU exemplified this, he said, being a simulated robot within a purely virtual reality (see Chapters 9.xi.b and 10.iv.a). The dire implication was that psychology can't ever tell us how we see tigers, smell lilacs, understand speech, or learn that politicians aren't to be trusted.

There seemed to be a chink of light, for Fodor granted that a "natural" (neurophysiological) psychology could discover the physical causes of sensation. However, to know the causes of *perception* is impossible in the general case. Science might tell us what a cat is, but not what a mat is—for the concept *mat*, like most concepts, doesn't name a natural kind. Someone's perception (belief, fear, desire . . .) that the cat is on the mat could be *naturally* explained only by explaining every causal detail involved. To do this for all objects of thought would require "the theory of *everything*" (Fodor 1980*a*: 70). As Fodor remarked, Leonard Bloomfield had used similar reasons in arguing that a naturalistic semantics is impossible (see Chapter 9.v.b). In short, despite its principled failure to engage with the real world, "computational psychology is the only one that we are going to get" (1980*a*: 66).

All was not lost, however: *philosophy* could come to the rescue. If we complemented a functionalist psychology by a theory of semantics then we could, after all, bring the lilacs and politicians into the picture. For we could then say how perception and belief are possible.

The rest of Fodor's career, with one important exception, was—and still is—devoted to defending his previous views (on intentional realism, "Rationalist" nativism, and the adult's rich language of thought) and to developing a philosophical semantics to fit (e.g. Fodor 1981*a*,*b*, 1987*b*, 1998*a*, 2000*a*).

The exception concerned the notion of mental modularity. When he wrote *The Language of Thought*, Fodor still followed the 'New Look' psychologists (6.ii) in seeing perception and learning as the formation and testing of hypotheses. "For all we know", he believed, "cognition may be saturated with rationality through and through" (1975: 173)—a view shared by Zenon Pylyshyn (1973), for whom even mental imagery reflected beliefs (7.v.a). By the early 1980s, however, the New Look was under fire from several directions.

For example:

* Chomsky had posited "mental organs", of which the language-acquisition device—underlying the language "faculty"—was only one (see Chapter 9.viii.b).
* GOFAI was increasingly relying on domain-specific knowledge (see 10.iv).
* David Marr, speaking of "the sensorium of sight" (Marr and Nishihara 1978), had detailed a variety of fast-acting, automatic, bottom-up processes in low-level vision (7.v.b–d).
* Developmental psychologists were discovering to their surprise that newborn babies have "knowledge" of—or, better, predispositions to attend to—various aspects of the world (including faces: see 14.ix.c). (To their surprise, because Jean Piaget, no less than the behaviourists, had denied this: Boden 1994*a*.)
* And Pylyshyn (1980) had defined the mind's "fixed functional architecture", whatever this turned out to be, as inescapable. It was "cognitively impenetrable", or uninfluenced by beliefs (see 7.v.a).

All these cognitive scientists agreed that the inborn propensities could generate actual ideas, or mental contents, *only* given the relevant environmental triggering. In sum, Immanuel Kant's views on the inherent structuring of the mind had seemingly been broadly vindicated (see Chapter 9.ii.c).

Fodor's response was *The Modularity of Mind* (1983). Like *The Language of Thought*, this book interested psychologists as well as philosophers. (It later received a detailed rebuttal from the developmental psychologist Annette Karmiloff-Smith: 1992.)

With his usual flair for scandal, Fodor picked up Chomsky's term and recommended "faculty psychology". He wasn't resurrecting the outmoded nineteenth-century faculty psychology, with its notions of Will, Perseverance, and Morality. Rather, by faculty psychology he meant "the view that many fundamentally different kinds of psychological mechanisms must be postulated in order to explain the facts of mental life" (1983: 1).

Following Pylyshyn, he defined mental modules as innate input systems whose computations were automatic, and couldn't be influenced top-down by concepts or beliefs. (One doesn't become immune to the Müller-Lyer illusion by learning that it is an illusion.) If rationality implies belief, then *perception*—despite being computationally complex at all levels—wasn't "saturated with rationality *through and through*" after all.

Cognition, however, was—and Fodor now saw this as a major problem (Chapter 7.iii.d). He argued that since any belief (hope, desire . . .) can be influenced by indefinitely many others, there can be no principled theory of problem solving or belief fixation. (Or, one might add, of 'New Look' perception, or of neurotic repression: see Chapters 6.ii.a and 7.i.a.) The best we can hope for is an informed natural history: a relatively unsystematic collection of problem-solving procedures and suggestive anecdotes. This would be an advance on common sense primarily in recognizing that propositional attitudes are (literally) GOFAI-computational.

But are they?—As we saw in Chapter 12, GOFAI would soon be challenged by PDP connectionism. (And Fodor would defend his position accordingly: Fodor and Pylyshyn 1988.)

e. Eliminative materialism

The earliest philosophers of PDP-based functionalism were the Churchlands. Their views on concepts, mental states, and consciousness were indicated in Chapters 12.x.b–c and 14.x.d.

As we saw there, they differed from Putnam, Dennett, and Fodor not only in rejecting GOFAI (abandoning logical deductions and intentional inferences for vector-to-vector transformations), but also in trying to avoid mentalistic language entirely. "Folk psychology" was to be discarded, not reduced. But their eliminative materialism was buttressed by PDP and computational neuroscience, not originated by them. To the contrary, it had long-standing philosophical roots.

Paul Churchland's earliest work pre-dated the resurgence of connectionism by several years. It had argued that advances in neurophysiology could conceivably lead us to think about minds in such a radically different way that beliefs, desires, fears, hopes . . . would be "eliminated", or "disappear" (P. M. Churchland 1979, 1981).

Even then, this idea wasn't new. The possibility had been implicit in Sellars's 1950s notion of the manifest image (see above), and had been made explicit in the early 1960s by Paul Feyerabend (1963*a*,*b*) and Richard Rorty (1965). Moreover, philosophers of science such as Russ Hanson (1958) and Thomas Kuhn (1962), and New Look psychologists such as Bruner and Richard Gregory (Chapter 6.ii), had already suggested that our empirical theories affect our perceptions.

The novelty in Churchland's approach was that he made a serious attempt to imagine how scientific knowledge could alter perception—including introspection—in a fundamental, all-encompassing, way.

Rorty too, in a book published in the very same year as Churchland's, considered the possibility of radically different types of perception. He imagined alien beings who never reported having *perceptions*, *ideas*, or other *mental representations*. Instead, they spoke in terms of neurophysiology. So, for instance, "When their infants veered toward hot stoves, mothers cried out, 'He'll stimulate his C-fibers' "; and a visual illusion might make them say "How odd! It makes neuronic bundle G-14 quiver, but when I look at it from the side I can see that it's not a red rectangle at all" (Rorty 1979: 71).

But Rorty wasn't arguing for scientific realism: far from it. Besides trying to banish the Cartesian concept of mind (partly by showing that intelligent creatures could do without it), he was denying that any form of knowledge, science included, is more fundamental than any other. According to Rorty, philosophy isn't an investigation but a "conversation", more like literary criticism than logic or science.

To Churchland, that was anathema. He insisted on the realist nature of science—he called his book *Scientific Realism and the Plasticity of Mind*—and used science (i.e. New Look psychology: Chapter 6.ii) to show that our perception actually is theory-laden.

Moreover, he tried hard to imagine *what it might be like* to have a radically different type of experience.

So, for instance, he described how he had trained himself to *see* the night sky so that certain astronomical facts, concerning the geometry of the solar system, were directly perceived, not inferred. These facts are usually regarded as counter-intuitive, precisely because they conflict with our normal perceptions. He explored other cases too, suggesting how children in an imaginary culture—whose 'ordinary' conception of reality was the one embodied in modern physics—might be taught to perceive the world in a very different, and more veridical, manner:

It is important for us to try to appreciate, if only dimly, the extent of the perceptual transformation here envisaged. These people do not sit on the beach and listen to the steady roar of the pounding surf. They sit on the beach and listen to the aperiodic atmospheric compression waves produced as the coherent energy of the ocean waves is audibly redistributed in the chaotic turbulence of the shallows . . . They do not warm themselves next the fire and gaze at the flickering flames. They absorb some EM energy in the 10^{-5} range emitted by the highly exothermic oxidation reaction, and observe the turbulences in the thermally incandescent river of molecules forced upwards by the denser atmosphere surrounding.

These observational descriptions, so arcane to us, are in no way arcane to the people under discussion. This is the only idiom they know. (P. M. Churchland 1979: 29–30)

As this passage suggests, a perceptual upheaval might involve not just seeing things differently, but seeing different things. In other words, our perception of physical reality might be informed by a different physical ontology. (And, *pace* Rorty, this alternative ontology might be *nearer the truth about reality*.) Instead of merely making us see/think of various "material objects" in a new way, science might lead us to discard such categories entirely:

The "facts", as currently conceived and observed by us, form the starting place for theoretical inquiry, but its successful pursuit may well reveal that we should vacate that starting place as hastily as possible. Large-scale intellectual progress will involve the wholesale rejection of old *explananda* as frequently as it involves the wholesale introduction of new *explanantia*. (P. M. Churchland 1979: 44)

And if the ontology of physical objects wasn't sacrosanct, neither was the ontology of mind.

It was even more difficult, however, to imagine how introspection might be systematically transformed, for the relevant neuroscientific theory wasn't yet available. (His "phase-space sandwiches" would come later: see Chapter 14.x.d.) Nevertheless, said Churchland, introspection is as theory-laden as any other form of perception. And the common-sense theory of mind—a "thoroughly thumb-worn theory whose cultural assimilation is complete" (p. 2)—didn't appear to map neatly onto what was already known about the brain.

Moreover, he predicted, this failure of mapping would probably deepen as neuroscience advanced:

[The] prospect we face is that a detailed neurophysiological conception of ourselves might simply displace our mentalistic [belief–desire] self-conception in much the same way that oxidation theory (and modern chemistry generally) simply displaced the older phlogiston theory of matter transformation. That we are long in the habit of making non-inferential introspective judgments in the terms of the theory to be displaced affects the matter not at all. (p. 5)

A halfway house, empty of physics and neurochemistry, would be a functionalist account. But this might be very different from what orthodox functionalists assumed:

Even as a functional characterization of ourselves, [folk psychology] may turn out to be taxonomically cockeyed, radically incomplete, and altogether too confused to merit continued use, when compared to the much superior functional characterizations that an adequate theory of the central nervous system can be expected to provide. (p. 113)

In short, folk psychology may be a *false* theory, rather than an approximate and/or incomplete one. (Incomplete, because—as Churchland pointed out—it works only for normal human agents, not for brain-damaged patients or animals.) To protest that we know it to be true through introspection (Cartesian "direct access") is futile. If our perception of physical things is theory-laden, and perhaps mistaken, so also is introspection.

These ideas hadn't yet quite gelled in Churchland's mind. Eliminative materialism was presented in his book merely as a coherent philosophical option, although his sympathies were clear. Two years later, however, he argued that it should definitely be adopted (P. M. Churchland 1981).

If there was no place for belief and desire in the philosophy of mind, there was no place for them in a scientific psychology either. But what was to replace them? What was the psychological equivalent of oxygen? Or, if we define ourselves (functionally) as "epistemic engines", how should we conceptualize the "epistemic states" through which we pass?

Churchland denied that these states can be helpfully seen as sentences, processed by some "ideal sentential automaton" (ISA). Orthodox functionalism, and logical positivism too, had adopted a "vision of rational intellectual activity as consisting essentially in a dance of propositional states, a dance whose form preserves certain propositional relations" (1979: 126). But he argued that the *normative* aspects of thought and knowledge ("epistemic virtue") can't be grounded in such an account. Since very young children don't use sentences, we can't explain the development of rationality in sentential terms—despite Fodor's "recent and noteworthy attempt to make a go of a linguistic interpretation of the infant's cognitive activities" (p. 131).

The fundamental constraints on what constitutes rational activity must therefore lie elsewhere: "we must try to penetrate to that deeper intellectual kinematics of which our manipulation of sentences is just the occasional and superficial reflection" (p. 141). Beyond assuming that they must contribute to evolutionary survival, he didn't pretend to know what these constraints are. But he used thermodynamic analogies in speculating about them.

He described animals as "informational sponges", absorbing information from the physical environment *and somehow using that information to get more information*. As for how that added value might be achieved, he imagined an intertidal creature, whose survival depends on its predicting the superficially chaotic variations in water level. This could be done, he said, by means of three internal oscillatory parameters, whose interactions "model" the relevant dimensions of the outside world—and he sketched the mathematics involved. (Compare Kenneth Craik's approach, described in Chapters 4.b–c and 14.viii.)

Churchland's closing words showed how far he already was from GOFAI functionalism. They also showed why he would later be so sympathetic to PDP connectionism, and to dynamical (vector transformation) approaches derived from neuroscience:

> And finally, it appears likely that the thermodynamics of "irreversible" processes will provide the underlying framework . . . for whatever genuine progress gets made here. For it is this theory that renders physically intelligible such things as the process of synthetic evolution in general, and the Sun-urged growth of a rose in particular. And what is human knowledge but a cortically embodied flower, fanned likewise into existence by the ambient flux of energy and information? (p. 151)

From the late 1980s on, accordingly, he and his wife treated those areas of cognitive science as sources of ideas about what cognition is, and how it works—two questions they saw as inseparable.

16.v. Counter-moves

So the functionalists reigned supreme, bickering among themselves about the rules of the club but encountering no serious opposition?—Not at all.

Outsiders protested that the functionalist enterprise was flawed in various ways. Indeed, one influential outsider was an ex-insider: Putnam resigned from the club some thirty years after founding it. As a result, the heady relief of the 1960s, on apparently escaping from the metaphysical impasse outlined in Section i, was gradually tempered.

'Friendly' objections, raised by people who spoke the same language as the functionalists, are discussed in this section. One influential critique was Block's (1978) 'Troubles With Functionalism'—but functionalism's troubles were many, and only a few can be mentioned here.

'Hostile' complaints grounded in a radically different perspective, a development of the neo-Kantianism sketched in Chapter 2.vi, are outlined in Sections vii–viii. Wittgensteinians were represented in both camps, as we'll see. So, too, was Putnam (Section vi).

a. Gödel to the rescue?

Disputes based in Gödel's theorem were voiced from the start. The general form of such objections is that computers are inherently limited in a way in which human minds are not.

Kurt Gödel had proved in 1931 that for any consistent logical system rich enough to contain elementary arithmetic, there is at least one meaningful sentence that cannot be proved in the system, but which humans can see to be true. And Turing himself had shown, independently of Gödel, that there are some well-defined questions that a given Turing machine can't answer (see 4.i.c).

For Turing, however, there was a crucial difference between a *given* Turing machine and *any* Turing machine. Accordingly, he rebutted "the mathematical objection" in *Mind*. He pointed out that "questions which cannot be answered by one machine may be satisfactorily answered by another". Moreover, no one had *proved* that we aren't subject to the same limitations. Certainly, we can sometimes see that a machine is giving the wrong answer (or failing to answer at all), which makes us feel superior. But we make mistakes too. And we can't triumph simultaneously over all machines: "There might be men cleverer than any given machine, but then again there might be other machines cleverer again, and so on" (A. M. Turing 1950).

Lucas (1929–) disagreed. A few years later, at a meeting of the Oxford Philosophical Society in 1959, he argued that "The Gödelian formula is the Achilles' heel of the cybernetical machine" (J. R. Lucas 1961: 116). This claim was the core of his seminal paper on 'Minds, Machines, and Gödel', published in *Philosophy* in 1961.

Gödel's proof, said Lucas, showed not that minds are "superior" to machines, but that they're different. However, describing the difference in positive terms was tricky:

> We are trying to produce a model of the mind which is mechanical—which is essentially "dead"—but the mind, being in fact "alive," can always go one better than any formal, ossified, dead system can. Thanks to Gödel's theorem, the mind always has the last word. (J. R. Lucas 1961: 116)

Despite the canny scare quotes, the terms *dead* and *alive* didn't really help.

Towards the end of his paper, however, Lucas expressed the difference by reference to the "unity" of consciousness. The crux of Gödel's theorem is self-reference, and it can be escaped only by moving up a level—at which point, an equivalent logical difficulty arises. But, said Lucas, in recognizing that when a conscious being knows something he also knows that he knows it, and knows that he knows that he knows . . . we aren't positing "an infinite sequence of selves and super-selves and super-super-selves". Rather, we're recognizing that a conscious mind, since it has no true parts, "can both consider itself and its performance and yet not be other than that which did the performance . . . [It] is already complete, and has no Achilles' heel" (p. 125). This argument took the place of the *proof* that Turing had asked for to show that humans aren't subject to the same limitations as machines.

Most readers saw Lucas's argument as a weapon in the battle against functionalism. But he hadn't originally intended it in that way: "[I was] innocent of functionalism rather than anti-functionalist. Like many other isms, it just passed me by" (personal communication). He'd been led to Gödel's theorem not by functionalism but by the problem of free will (see also J. R. Lucas 1970: 114–23).

That's why his paper ended in the claim that mechanism is false. He allowed that a super-computer might come to behave unpredictably, as if it had "a mind of its own". But in that case, "it would cease to be a machine, within the meaning of the act" (1961: 127). It followed, he said, that "no scientific inquiry can ever exhaust the infinite variety of the human mind" and that our concepts of freedom and morality are safe from the challenge of mechanism.

(Functionalists argued, to the contrary, that a computational approach can not only allow for what we call freedom but also explain how it's possible: Boden 1972, ch. 7 and pp. 330–4; 1978; Sloman 1974; Dennett 1984*a*.)

For every person who was convinced by Lucas's paper, someone else wasn't. It became hugely influential, and is still attracting readers in the new century (it's on his web site, and is viewed by around fifty people a week—see J. R. Lucas 2000: 5). At a meeting held at the University of Sussex in 1990 to mark the fortieth anniversary of Turing's

Mind paper, Lucas looked back on how the argument had progressed (J. R. Lucas 1996). Wryly recalling the many replies that had been "lacking in either courtesy or caution", he pointed out that a large number of different objections had been raised by his critics (including cognitive scientists Dennett, Clark Glymour, and Douglas Hofstadter). He didn't regard any as decisive, and ended as he'd done before: roundly proclaiming the demise of mechanism.

Lucas had written the paper at Princeton, where he went (in 1957–8) specifically to work on Gödel. One of the people he encountered there was Putnam, but he didn't manage to convince him: in his 1960 paper, Putnam dismissed the Gödelian objection as a fallacy. Thirty years later, however, he saw it as a problem—not a knock-down formal argument, but an epistemological reminder that our reason enables us to go beyond whatever it can formalize (Putnam 1988: 118; cf. Putnam 1997: 39).

Roger Penrose, too, used Gödel's theorem to attack strong AI and computational psychology (R. Penrose 1989: 102–8). As a Fields medallist (the mathematical equivalent of a Nobel prizewinner), his voice received a very wide hearing—not least, in the media worldwide. (It doubtless helped that his conclusion, the superiority of human minds over computers, was what the vast majority of people wanted to hear.) Nevertheless, like Lucas before him he was challenged repeatedly, and at length (e.g. Sloman 1992).—This show, as they say, will run and run.

b. Consciousness and zombies

A second objection to functionalism that was anticipated (teasingly) by Turing concerned consciousness. As various people would later point out, there's no such thing as *the* problem of consciousness (see Chapter 14.xi.a, and Section iv.a below). The most hotly disputed of the *several* problems of consciousness concerns qualitative experience, which was discussed in Chapter 14.x–xi. Here, we need add only two points.

The first is that the mid-century arguments about consciousness (Section i, above) acquired new versions in the context of functionalism. Often, they were couched in terms of "zombies": android robots behaving *exactly like us*, but lacking all sensation or feeling. The objection typically ran as follows: since such creatures are obviously logically possible, functionalism must be false.

Dennett (given his views outlined in Section iv.a–b, and in Chapter 14.xi.b too) didn't see this as obvious at all. On the contrary, he said, the concept of zombies is incoherent, and belief in their possibility "ridiculous" (Dennett 1991, ch. 10.4; 1995*b*). Sloman (1999) argued, similarly, that nothing could have the same computational architecture as us (necessary for it to behave exactly like us), yet lack sensation.

An early variant of the zombie argument was due to Ziff (1920–2003). He claimed that "no robot could sensibly be said to feel anything", because we could program it to act any way we liked (Ziff 1959). So, for instance, we could make it act "tired" when lifting a feather but not a ton, or blue things but not green things. Again, the functionalist would ask whether these apparent "possibilities" assume a virtual machine that is at base incoherent. (A robot might tap into reserves of energy when lifting green things, because of some general motivational attitude to green; the ton-weight case, ignoring 'emergencies', is more problematic.)

Sometimes, it was claimed not that androids *needn't* be conscious, but that they *couldn't* be conscious. Scriven (1953), for instance, argued that no robot could be conscious, because it wouldn't be alive (see Section x.a). A few years later (Scriven 1960), however, he recanted: "I now believe that it is possible so to construct a supercomputer as to make it wholly unreasonable to deny that it had feelings."

The second point to be noted here is that the problem of *qualia* caused even some functionalists to admit defeat. If Dennett and the Churchlands—and Sloman (see Section ix.c)—thought they'd solved it, Chalmers and Fodor didn't.

Chalmers (1995, 1996*a*) called it the "hard" problem, which neither functionalism nor neuroscience could solve (see 14.x.d). But at least, he suggested, there was a glimmer of light. If we were to admit a "dual-aspect" notion of *information* as a fundamental feature of the natural world, then conscious experience could be accommodated in science. Sceptics complained that this wasn't explaining consciousness, but slipping it in through the cellar door—a millennial version of *élan vital* (see Chapter 2.vii.b).

As for Fodor, he had no hope of an answer. Saying that consciousness couldn't be a functional ("relational") phenomenon, he declared:

> Nor do we know, even to a first glimmer, how a brain (or anything else that is physical) could manage to be a locus of conscious experience. This last is, surely, among the ultimate metaphysical mysteries; don't bet on anybody ever solving it. (Fodor 1995*a*: 83)

This was about as pessimistic as Colin McGinn's (1989, 1991) claim that consciousness is as far beyond our cognitive capacity to understand as algebra is for dogs. Indeed, for "pessimistic" one might read "defeatist". (Whether neuroscience might one day enable us to understand *qualia* was discussed in Chapter 14.x–xi.)

c. That room in China

A third cluster of counter-moves concerned intentionality. These also were made from the inception of functionalism, and were mentioned (in passing) by Turing himself (1950).

One of these, emanating from Searle (1932–) at the University of California at Berkeley in 1980, soon became notorious. This was a sparkling intellectual hatchet job, featuring the Chinese room.

People unfamiliar with Searle's other writings often assume that he had no sympathy for cognitive science in general and AI in particular. That's not so. "Since the beginnings of the discipline", he has said, "I have been a practicing 'cognitive scientist' " (1992: 197). And he's described AI—and computer functionalism—as "one of the most exciting developments in the entire two-thousand-year history of materialism" (1992: 43).

Moreover, he welcomed the fact that NLP researchers studying conversation had used his own work on speech acts (see 9.xi.f), not least because it enabled him to improve the theory. As he put it:

> Now the beauty of AI, and this I really do admire, is that it forces you to pose those questions [about rules for understanding language] precisely and forces you to state your theory precisely. In fact the things I've written about—metaphors and indirect speech acts and so on—a great deal of it has been programmed by people working in various AI labs. So I think, in fact, that AI

is an immensely useful tool in the study of language and the study of the mind . . . (in Pagels *et al.* 1984: 356)

However, there's cognitive science and then there's cognitive science. In Searle's view, "most mainstream cognitive scientists simply repeated the worst mistakes of the behaviorists [by ignoring] the essential features of the mind" (1992, p. xii). Those features, he said, are intentionality (the focus of his 1980 paper) and consciousness (the focus of his 1992 book).

The central point of Searle's provocative paper—the semantic emptiness of AI programs, considered as uninterpreted symbols—wasn't new. As we've seen, Fodor had embraced this in his "meditation" on formalism published a few months before, and Mays had noted it as early as 1949 (Manchester Philosophy Seminar 1949; Mays 1951, 1952). What Searle originated was an elegant slogan ("all syntax and no semantics"), a terminological distinction ("weak" versus "strong" AI), and an extraordinarily seductive—and slippery—thought experiment.

The terminological distinction quickly entered the discourse of cognitive science. However, it wasn't usually understood in just the way that Searle defined it. Both "weak" and "strong", in this context, have been interpreted in different ways. Searle put it like this:

According to weak AI, the principal value of the computer in the study of the mind is that it gives us a very powerful tool. For example, it enables us to formulate and test hypotheses in a more rigorous and precise fashion. But according to strong AI, the computer is not merely a tool in the study of the mind; rather, the appropriately programmed computer really *is* a mind, in the sense that computers given the right programs can be literally said to *understand* and have other cognitive states. (Searle 1980: 417)

So defined, weak AI—as Searle was quick to point out—is on a par with using computers to test theories about rainstorms. It claims no more theoretical affinity with minds than with anything else.

"Weak AI" is usually understood, however, as the claim that some of the computational features—processes, structures, virtual machines, architectures—involved are substantive theoretical terms in psychological explanations. As explained in Chapter 1.ii.a, this is the core thesis of cognitive science. Searle implied that to go this far and no further (that is, to claim explanatory power while drawing the line at strong AI) was disreputable, an example of muddled—even pusillanimous—thinking based on some fuzzy "computer metaphor". For him, *programs are tools* and *programs are minds* were the only intellectually honourable positions—and he praised Newell and Simon for their "straightforwardness", accordingly (see Section ix.b).

As for "strong" AI, this too is ambiguous. There are at least eight readings of it: some true, some false, some requiring empirical investigation (Sloman 1992). So even if the thought experiment 'worked', anyone who said baldly that strong AI had been dismissed would be too quick.

Searle's thought experiment entered the discourse of cognitive science—and everyday discourse, too. It was featured on TV and radio around the globe, including the BBC's annual Reith Lectures (Searle 1984), and in many newspapers and popular science magazines.

This, it seems, was a surprise to Stevan Harnad, the editor of *BBS*—where (on the say-so of the associate editor, Pylyshyn) Searle's paper was first published, alongside twenty-seven peer commentaries. He hadn't been enthusiastic:

> I cannot say that I was especially impressed . . . [It] seemed to be yet another tedious "Granny Objection" about why/how we are not computers. . . . Across the ensuing years, further commentaries and responses continued to flow as, much to my surprise, Searle's paper became *BBS*'s most influential target article (and still is, to the present day) as well as something of a classic in cognitive science. (At [a Rochester Conference in 1982] Pat Hayes went so far as *to define cognitive science* as "the ongoing research program of showing Searle's Chinese Room Argument to be false"—"and silly", I believe he added at the time. (Harnad 2002: 294–5; italics added)

That last addendum was a dig at Searle, not Hayes. But one could be forgiven for thinking otherwise, having read Harnad's own writings on the topic:

> As the arguments and counter-arguments kept surging across the years I chafed at being the only one on the planet not entitled (*ex officio*, being the umpire) to have a go, even though I felt that I could settle Searle's wagon if I had a chance . . . [When I eventually joined the online discussion on "comp.ai"] I found comp.ai choked with such a litany of unspeakably bad anti-Searle arguments that I found I had to spend all my air-time defending Searle against these non-starters instead of burying him, as I had intended to do. (p. 296)

To Harnad's annoyance, even Searle believed him to be on his side, and urged him "to keep fighting the good fight". Now, however, Harnad himself admits that on the one "essential point" they'd actually agreed all along (p. 296). This historical vignette is just one illustration of the slipperiness of Searle's thought experiment.

Aspiring writers may take comfort in knowing that a paper disparaged by one editor was published nonetheless—and sped around the world immediately. But to match this phenomenon they'll need to think up an example that's equally seductive.

Just in case you can't already recite it in your sleep, here it is:

Situation: Searle in a windowless room; a slot through which paper slips with "squiggles" and "squoggles" on them are occasionally passed in; a box of slips carrying similar doodles, of various shapes; and a rule book, saying that if a squiggle is passed in then Searle should find a blingle-blungle and pass it out, or perhaps go through a long *sequence* of doodle pairings before passing some particular shape out.

Denouement: Unknown to Searle-in-the-room, the doodles are Chinese writing; the rule book is a Chinese NLP program, comparable to Wendy Lehnert's question-answerer (see Chapter 9.xi.d); and the Chinese people outside the room are happily using Searle to answer their questions about some topic or other.

Punchline: Searle entered the room unable to understand Chinese and, no matter how long he stays there, he still won't understand a word of it when he comes out.

Conclusion: Formal computation alone (which is what Searle-in-the-room is doing) can't generate intentionality. Therefore strong AI is impossible.

Arguably, Searle was here attacking a straw man. Newell and Simon, and Minsky and John McCarthy too, certainly believed that AI systems of a certain complexity would be *really* intelligent, and *really* have beliefs. But, as we'll see in Section ix.b, they didn't have abstract Turing-computation in mind.

Searle, however, did (1980: 417). He argued that programs are mere shufflers of meaningless shapes. Indeed, he said, any AI program could in principle be interpreted as (mapped onto) many different activities: tax laws, dance routines . . . whatever. As regards the program itself, the choice between "meanings" is arbitrary. Any meaning it appears to have is derived entirely from us. So although we can't help speaking of computers in intentional terms (as Dennett had said), they don't "intrinsically" merit such ascriptions—whereas people do. So strong AI is an illusion.

As for weak AI, Searle doubted that brains generally implement formal computations. If they don't, then (GOFAI-based) computational psychology—far from being, as Fodor had claimed, the only psychology on offer—can't even *begin* to explain our mental life. But even if they do, he said, the Chinese room argument showed that *something more* is needed for intentionality.

The Chinese room, like the Turing Test, spawned a minor philosophical industry. A goodly number of readers agreed that the Chinese room argument proved just what he said it proved, while just as many saw it as fundamentally wrong-headed. Critique and reply alternated in many different places, and in varied forms. For example, the Chinese *room* was transformed into the Chinese *gym*, to deal with connectionism (Searle 1982, 1984, 1990*a*,*b*, 1992). And, twenty years after the original publication, an entire volume of new critiques was specifically commissioned by a major university press (Preston and Bishop 2002).

The barrage of responses to Searle's paper mostly ignored the mysterious "something more"—with good reason, as we'll see below. Instead, they focused on his novel way of expressing the old empty-symbolism argument.

Many objections were versions of a position he'd anticipated—and rejected—as "the Robot reply". This said that Searle-in-the-room would acquire understanding of Chinese if sensori-motor mechanisms were added. In other words, symbols (or concepts) must be grounded in world-engaging activities in order to be meaningful, and to refer to individually discriminable things (e.g. Fodor 1980*b*; Dretske 1984, 1995; Harnad 1989, 1990).

Some philosophers of language in the 1980s held that causal processes aren't enough, that they have to be grounded in evolutionary history—in which case only *evolved* robots, at most, could possess intentionality (see Section x.d). And a number of critics argued that causation is intimately linked with *computation* as it is understood in practice, so that Searle's—and Turing's—abstract concept of computation is largely irrelevant (see Section ix).

d. Neuroprotein and intentionality

Searle, too, had stressed the importance of causation—but in a most unilluminating way. This concerned the "something more" needed for intentionality.

According to him, neuroprotein has "causal powers" capable of generating intentionality, whereas metal and silicon don't. As he put it some years later: "I think it is empirically absurd to suppose that we could duplicate the causal powers of neurons entirely in silicon" (1992: 66). The brain, he granted, is a digital computer—because *everything*, including your bedroom wallpaper, can be seen as a digital computer.

(We needn't discuss that point here: for rebuttals, see Chrisley 1995; Chalmers 1996*b*; Copeland 1996.) But its formal structure isn't the point:

> Whatever else intentionality is, it is a biological phenomenon, and it is as likely to be as causally dependent on the specific biochemistry of its origin as lactation, photosynthesis, or any other biological phenomena. No one would suppose that we could produce milk and sugar by running a computer simulation of the formal sequences in lactation and photosynthesis, but where the mind is concerned many people are willing to believe in such a miracle . . . (1980: 424)

We could even simulate brain functions (he said), by having the Chinese room contain a system of water pipes and adjustable valves corresponding to neurones and synapses. But still there'd be no understanding, because "[the brain's] causal properties, its ability to produce intentional states, wouldn't even have been simulated, never mind reproduced" (p. 421).

One might be tempted to describe this as a fairy story about the brain, except that fairy stories have some positive content. Even Penrose's shaky speculations about microtubules would offer more than this (Chapter 14.x.d).

Searle's move here, besides being empty, ignored the nature of explanation in neuroscience. Certainly, we have strong empirical evidence—as Descartes did too—that the brain is causally implicated in mental life. But at the level of *material stuff* (compare: lactose, chlorophyll), this is intuitively unintelligible.

In other words, we're in much the same philosophical position as the alien in the science-fiction story, if not *quite* so surprised by the brute facts:

> "They're made out of meat."
>
> "Meat?"
>
> "Meat. They're made out of meat."
>
> "Meat?"
>
> "There's no doubt about it . . . They're completely meat."
>
> "That's impossible. What about the radio signals? The messages to the stars."
>
> "They use the radio waves to talk, but the signals don't come from them. The signals come from machines."
>
> "So who made the machines? That's who we want to contact."
>
> "They made the machines. That's what I'm trying to tell you. Meat made the machines."
>
> "That's ridiculous. How can meat make a machine? You're asking me to believe in sentient meat."
>
> "I'm not asking you, I'm telling you . . . We probed them. They're meat all the way through."
>
> "No brain?"
>
> "Oh there is a brain all right. It's just that the brain is made out of meat."
>
> "So . . . what does the thinking?"
>
> "You're not understanding, are you? The brain does the thinking. The meat."
>
> "Thinking meat! You're asking me to believe in thinking meat!"
>
> "Yes, thinking meat! Conscious meat! Loving meat. Dreaming meat. The meat is the whole deal! Are you getting the picture?"
>
> "Omigod. You're serious then. They're made out of meat." (Bisson 1991)

Even neuroscientists can empathize with the alien's amazement. For in so far as we understand how neuroprotein (aka meat) grounds intentionality, we do so in terms of functional concepts. We speak of *messages*, *excitation*, *thresholds*, *codes*, *bug-detectors*, *orientation detectors*, and other types of *computation* (see Chapter 14). The neurotransmitters (meat juices) aren't interesting primarily for their biochemistry, but for the fact that they function to alter thresholds, for instance. Even the crucial sodium pump, whose material chemistry is thoroughly understood, is significant for its functional properties, in allowing an electrical impulse (message) to propagate along the axon. Similarly, we know a great deal about the chemistry of the synapse—but what's psychologically relevant is how this affects the neurone's message-passing functions (Chapter 14.ix.d and f).

Many of us may share Searle's unargued hunch about the inadequacy of metal and silicon. Neuroprotein may indeed be the only substance on earth that can support intentionality. But if so, why? Perhaps only neuroprotein allows the combination of chemical complexity, stability, and flexibility that's required for implementing the virtual machines that generate behaviour and thought? To say this, however, is—again—to make a functional point: what matters is what a particular metabolite enables the brain to *do* (see 14.ix.f).

Although this aspect of Searle's paper didn't engender much debate, it did encourage the fourth counter-move against functionalism: scepticism about multiple realizability.

e. How multiple is multiple?

Putnam's initial claim had been that all the functional properties of human and animal minds are in principle realizable in indefinitely many ways. But whether they were all multiply realizable in practice was another matter.

If philosophers were apparently licensed to ignore neuroscience, other cognitive scientists weren't. Indeed, even some philosophers felt that they would have to take notice of the brain after all, if it turned out that computations typical of brains could do things which other types of computation (in practice) could not.

This is why PDP connectionism, despite its many 'unnatural' features (such as back propagation), was often seen as relevant to *philosophical* theories of concepts and thinking (Chapter 12.x). It's why even connectionism was soon criticized for being insufficiently close to neurobiology, and urged to take facts about the brain on board (14.ii.d). It's why the Churchlands used ideas drawn from neuroscience, such as "phase-space sandwiches", to help justify their version of eliminative materialism (see 14.x.d). And, in the context of computer science, it's also why some AI scientists see Searle's argument as fundamentally irrelevant to their research (see Section ix).

Encouragement, however, isn't initiation: it didn't need Searle to make cognitive scientists consider neurophysiology. John von Neumann himself had surmised that the "logic" of the brain was very different from that of the von Neumann computer, and in 1947 McCulloch and Pitts had said much the same—offering hypotheses about what the alternative might be (see Chapter 12.i.c). Newell and Simon had long designed AI models that reflected specific properties of the brain, and Marr (among others) had asked what types of computation certain neural mechanisms could perform

(Chapters 7.iv.b and 14.v). In short, the functionalist dogma of multiple realizability was already being taken with a large pinch of salt.

Even so, the explanatory focus was more on *the types of computation* involved than on their biochemical embodiment. As the years passed, it became increasingly clear that neuroscience can provide crucial information about the functional organization of a cognitive system (the virtual machine), not just about how it happens to be materially implemented (Bechtel and Mundale 1999; Keeley 2000*a*). And at the turn of the twenty-first century, a connectionist textbook appeared which paid greater attention to neurobiology than the fledgling PDP had done (O'Reilly and Munakata 2000: see 14.ii.d).

f. Subconsciousness attacked

The fifth counter-move was more radical. The functionalists described brains as implementing computations, and even Searle was willing to entertain this as a hypothesis. But the more deeply committed Wittgensteinians saw it as absurd.

Their problem wasn't specifically with computation, although they thought that "supposing the brain to be a computer is mere fashion" (Anscombe 1974: 235). They were suspicious of *any* attempt to explain mental life in terms of sub-personal mechanisms that weren't consciously acknowledged by the subject. Even Freudian theory was suspect for this reason. Brain mechanisms there must be, of course—but those were the neurophysiologist's business.

This attitude was evident in the uncompromising rejection of New Look psychology by two of Wittgenstein's leading disciples, Anscombe (1919–2001) and Malcolm. At a meeting held in 1971 at the University of Kent at Canterbury, Gregory (1974*b*; cf. 1975) summarized his 'New Look' view of perceptions as hypotheses (see 6.ii.e). Anscombe would have none of it:

> Now an hypothesis is something answerable to evidence, to data. To what data could the perceptual hypotheses that Gregory speaks of be answerable, but to perceptions? *Are* these perceptions then in turn hypotheses, and so on *ad infinitum*? (Anscombe 1974: 213)
>
> *What* is framing the hypothesis? Is [it] the conception which Gregory suggests, that the framer is some mechanism that produces hypotheses that are answerable to input? For *we* who perceive don't know what the input is.
>
> Hypotheses are *that* things are the case . . . [It] looks as if Gregory's theory involved our perceiving apparatus itself as entertaining judgments. Hypotheses are predictive by way of inference. They also involve the logical constants, can be negative, universal, particular, conjunctive, and disjunctive. Perception can be all of these things except for being disjunctive . . . By perception's being alternative I would mean two perceptions presenting themselves as alternatives at the same time. (p. 218)
>
> When there being a tree here . . . is the description of what is plainly the case, plainly and visibly to the perceiver—it is absurd to call the perceptual belief an hypothesis. (p. 243)

Rather than speaking of hypotheses, she suggested, he should have spoken of "models", "patterns", or "schematic sketches of possibilities".

Gregory responded with some asperity:

> [How] much surprise are we allowed before a word becomes philosophically suspect? . . . I do not want a normal word, because I do not want to express a normal thought . . . The "framer" I

regard as brain mechanisms . . . Surely we do not *have* to say that total human beings alone have the prerogative to frame hypotheses? (Animals might do so—and are *they* conscious? Computers might do so—and surely *they* are not conscious?) Again, why should we be captured by the inertia of language? (Gregory 1974*b*: 232, 234)

Anscombe fought back: "I don't mind a bit if he uses the word 'hypothesis' in a novel way . . . I merely want to understand what he says and assess its value" (pp. 236–7). Gregory had found out some interesting things, she conceded, "but how are his enquiries logically facilitated by having the scientific hypothesis paradigm rather than by thinking of patterns and models?"

This defence would have rung more true if her initial paper had been more constructive in spirit. As Gregory put it, "[Do] these philosophers have rival theories of perception up their sleeves? If so, they must surely justify their linguistic preferences not from common usage but by reference to their theories" (Gregory 1974*b*: 234).

Malcolm (1971) went even further than Anscombe, rejecting the cognitive approach wholesale. Much as he'd earlier denied that scientific evidence could justify our saying that dreams occur at certain times during sleep (see Section iii.b), so he now dismissed cognitive structures and processes as "myth". He wasn't saying that *these* structures and processes (hypotheses, perhaps) are implausible whereas *those* (models, perhaps) are better founded. Rather, he was outlawing all theorizing about subconscious psychological mechanisms.

A powerful counter-attack, analogous to Putnam's (1962*a*) critique of Malcolm's views on dreaming, appeared immediately (M. Martin 1973). It argued that scientists typically extend linguistic usage, and that cognitive psychologists had good grounds for positing various types of unconscious process and structure. But Malcolm wasn't persuaded.—It's small wonder, then, that Fodor introduced his own approach in the mid-1970s by observing that "many philosophers [still believe] that Ryle and Wittgenstein killed this sort of psychology some time about 1945, and there is no point to speculating on the prospects of the deceased" (Fodor 1975: 2).

16.vi. Betrayal

Already under fire from these many directions, functionalism then had to face the unkindest cut of all. Putnam himself started to have doubts in the early 1970s, and explicitly disowned his brainchild some fifteen years later.

Ironically, GOFAI suffered a similar trauma at much the same time, being betrayed in 1986 by the erstwhile wunderkind Terry Winograd (Chapter 11.ii.g). But Putnam's disaffection with functionalism was the more shocking. For whereas Winograd had picked up someone else's ball before running with it, Putnam had sewn the ball together before throwing it onto the pitch.

In 1973, at a meeting on 'Computers and the Mind' at Berkeley, Putnam still defended the autonomy of psychology (Putnam 1975*b*). But he rejected his previous claims that the whole human being is a Turing machine, and that a mental state is a state of a Turing machine, saying he had been "too much in the grip of the reductionist outlook".

That was more a complaint about oversimplification than a rejection of the spirit of functionalism. By the late 1980s, however, rejection had won out:

> The desire that grips Fodor, then, as it once gripped me, is the desire to make belief–desire psychology "scientific" by simply identifying it outright with computational psychology . . . Mentalism is just the latest form taken by a more general tendency in the history of thought, the tendency to think of concepts as scientifically describable ("psychologically real") entities in the mind or brain. And it is this entire tendency that, I shall argue, is misguided. (Putnam 1988: 7)

> Functionalism, construed as the thesis that propositional attitudes are just computational states of the brain, cannot be correct. (p. 73)

One might compare this change of viewpoint with Bruner's contemporaneous flight from experimentation and computation to interpretation and narrative (Chapters 6.ii.c and 8.ii.a). Or one might see it as a Damascene conversion typical of the "counter-cultural" trend so prominent in the 1970s–1980s (see 1.iii.c–d). But it was much more carefully argued than most such examples (including Bruner's), and much longer drawn out than the episode on the road to Damascus.

The three pillars supporting Putnam's recantation were universal realizability, externalism, and meaning holism. Each one contradicted a central aspect of his 1960s position. And each one had arisen from *friendly* territory, in the sense that they developed while Putnam still had faith in functionalism.

In thinking further about the nature of language, he didn't improve functionalism but rejected it. Indeed, he ended up in what was traditionally seen as the enemy camp.

a. Friendly fire

Universal realizability was multiple realizability gone wild. The doctrine of mind–brain multiple realizability, said Putnam, "still seems to me to be as true and important as it ever did" (1988, p. xii). But now, he added a mind-computation version, which forbade the identification of mental states with specific computations.

Using an argument so highly technical that it was relegated to an Appendix, he claimed that *any* program is implemented in *any* object. So mental states supervene on computations much as they supervene on brain states: they can't be identified with each other.

This—if correct—put paid to computational psychology: if every pebble implements Newell and Simon's GPS, and every leaf Chomsky's grammar, why bother?

Searle later added this argument to his own arsenal (1992: 208–9). But others saw it as invalid, on the grounds that it ignored the causal aspects of computation. That, of course, was a heretical view—indeed, for most philosophers it qualified as *nonsense*. For their normal assumption was—and still is—that computation is a purely formal notion, not a causal one. This disagreement will be discussed at length in Section ix.

The second pillar of Putnam's self-critique was externalism. This is the type of philosophical semantics—whose popularity grew in the late 1970s and 1980s—which favours "wide" content (see Section iv.d).

Sometimes, wide content is understood in naturalistic terms, such as agent–environment relations (G. Evans 1982), natural kinds (Kripke 1980), or evolutionary history (Millikan 1984). And Putnam himself argued that the meaning of nouns like "water" is fixed by the physical constitution of the natural kind concerned—whether we know about it or not. (This was the import of his much-disputed "Twin Earth" example—1975*a*: 223–7.)

But he also saw interpretative norms as crucial. Indeed, he even argued that causal accounts are inherently non-naturalistic, because what we count as "the" cause (like "the" explanation) depends on our interests (Putnam 1982).

Wittgensteinians, of course, had long stressed the relevance of cultural practices and norms in assigning meaning to language. Now, Malcolm's milk bottles had come home to roost (see Section i.d). If we can't individuate concepts and beliefs without reference to the physical and cultural environment, then neither brain states nor computational states *as such* can carry the content that identity theorists and functionalists, respectively, had ascribed to them. As Putnam put it: " 'meanings' just ain't in the *head*!" (1975*a*: 227). (Compare Wittgenstein: "If God had looked into our minds he would not have been able to see there whom we were speaking of"—1953: 217.)

As for the third pillar, meaning holism, this was a neo-Quinean position which held that the meaning of an individual term (concept) isn't independent of the overall framework of beliefs in which it's involved.

It's not just that meanings are fuzzy, incapable of definition by necessary and sufficient conditions. Worse, they're essentially interconnected. No one can think *that there are churches in Vienna*, for example, unless they possess concepts like *city*, *building*, *religion* . . . and so on. The reason is that the meaning of the concept *church* involves the meanings of many other concepts.

Quine had developed this theory in his attempt to find an adequate philosophy of science—indeed, to ground philosophy *in* science. But it could also be seen as a modern version of the humanist Humboldt's view that language is inseparable from culture (Chapter 9.iv.b). You and I may use words we're happy to regard as 'equivalent', translating the Cook Island Maori *tamariki* as the English *children*, for example. But Western and Polynesian beliefs about children are subtly different, so (on this view) our concepts are different too. This scientific/humanist ambiguity explains why Putnam eventually crossed over to 'the other side' (see subsection b).

Dennett, you'll remember (I hope!), had argued that *beliefs* can't be pinned down, for this very reason. But it hadn't turned him away from a neo-functionalist view of cognitive science. However, it worried Putnam more than Dennett. It was puzzling over this fact, Putnam said later, which was "the beginning of the end of my attachment to functionalism" (1999: 118).

Eventually, it led Putnam to the conclusion that the very notion of a "functional state" is "not fully intelligible". (It also led him to conclude that the notion of an "internal phenomenal state" isn't fully intelligible either, another point on which he agreed with Dennett: Chapter 14.xi.c–d.)

Mental states (propositional attitudes), Putnam now argued, are similarly interconnected, and similarly impossible to pin down. No two people thinking "The cat sat on the mat" are thinking exactly the same thought, because their beliefs about cats and mats aren't identical. When we say that they *are*, we're relying on cultural norms that decide *what we are willing to count as* the "same" thought. (*Tamariki/children*, again.) It follows that no single computational account, howsoever complex, can fit every instance of "one and the same thought".

Whether Putnam, Quine, or anyone else (even Humboldt) ever took this doctrine literally is arguable. Putnam specifically allowed that two terms may have the same *denotation* (though not the same *sense*, or *meaning*): "rational animal" and "featherless biped", for example. But what's important here is that Fodor's functionalism, and GOFAI modelling in general, took a very different approach to meaning.

Even PDP models, which allowed for fuzziness and family resemblances in individual concepts (12.x.a–b), didn't reflect mutualities of meaning between beliefs. Indeed, Putnam's holism implied that *no* scientific theory could use meanings (concepts, mental states) as scientific objects, playing an explanatory role. (For Fodor's rebuttal, see Fodor 1987*b*, ch. 3.) That is, psychology—because it deals with meanings, or *content*—can't be naturalized:

> There is nothing in [my mature position on perception and conception] that is "antiscientific" in the sense of *standing in the way* of serious attempts to provide better models, *both neurological and computational*, of the brain processes upon which our perceptual and conceptual powers depend, processes concerning which we still know very little. Moreover, it is *a profound mistake* to equate serious science with Cartesianism cum materialism that has for three centuries tried to wrap itself in the mantle of science. (Putnam 1999: 48; first italics in the original, others added)

At the end of the century, Putnam remained unrepentant. Cognitive science, he said, had turned out to be more science fiction than science:

> There is no harm in speculating about scientific possibilities that we are not presently able to realize; but is the possibility of an "ideal psychological theory" of this sort [i.e. expressed in a "normal form": see Section iii.b] anything more than a "we know not what"? . . . Do we have any conception of what such a theory might look like? Even if we had a candidate . . . how would we go about verifying that it does implicitly define the unreduced psychological properties? One hears a lot of talk about "cognitive science" nowadays, but one needs to distinguish the putting forward of a scientific theory, or the flourishing of a scientific discipline with well-defined questions, from the proffering of promissory notes for possible theories that one does not know even in principle how to redeem. (Putnam 1997: 36)

One must take care, however, to note just what he meant by cognitive science. He *wasn't* saying that the research described in Chapters 6–15 is worthless. Nor was he complaining (like N. Stuart Sutherland: see Chapter 1.ii.c) that some self-styled cognitive scientists focus not on empirical work but on the philosophical disagreements within the field.

Rather, he was saying that those (like Fodor) who sought a computational theory that would map neatly onto individual propositional attitudes, and that would identify their content independently of our interpretative practices and the context of use, were doomed to failure. The "narrow" side of the semantic stage, for Putnam, can support no real action. And even wide content—*pace* Dretske, Dennett, or Ruth Millikan—can't be analysed in naturalistic, non-normative, terms.

b. Crossing the river

Putnam's 'friendly' three-pronged attack on functionalism, described above, was only part of his reason for abandoning it. By the late 1980s, he'd become committed also to a fundamentally hostile (neo-Kantian) critique. As we'll see in Section vii, various versions of this had been lurking in the background—specifically: across the English Channel—for well over a century. They reached Putnam via the later Wittgenstein, and eventually turned his ideas upside down.

Putnam described his apostasy as "a matter of being torn between opposing views of the nature of philosophy" (1988, p. xii). This opposition is evident, for instance, in the difference between the "early" and "late" Wittgenstein. One way of describing it is in terms of attitudes towards metaphysical realism.

The typical empiricist–scientific position is that a real world exists independently of us, with its own intrinsic properties and a determinate built-in structure. Our thoughts represent it, and are *true* in so far as they correspond to it. The business of science is to describe the world and our relation to it, and philosophy—including the philosophy of mind—must be constrained by the findings of science. As Bertrand Russell put it, in criticizing the later Wittgenstein:

> [Philosophers should resist] the desire to separate [philosophy] sharply from empirical science. I do not think such a separation can usefully be made. A philosophy which is to have any value should be built upon a wide and firm foundation of knowledge that is not specifically philosophical. (B. Russell 1956*b*: 329)

This form of realism, however, appears to put a metaphysical gulf between thought (i.e. representation) and world (reality). The typical neo-Kantian position, outlined in Chapter 2.vi, aims to avoid that difficulty.

It holds that things in the empirical world aren't mind-independent, but constituted by the concepts informing human minds. For Kant himself, there was also the noumenal world. Some of his followers dropped this unknowable thing-in-itself, and moved on to idealism: the view that *all* reality is constituted by human thought. Some implications for the philosophy of language and biology were noted in Chapters 9.iv and 15.viii.b, respectively. As for the philosophy of mind, the implication highlighted by the Continental phenomenologists in the first half of the twentieth century was that there is no metaphysically fundamental Subject–Object or mind–body (or even body–environment) distinction. The notion that there is, they argued, is a Cartesian illusion.

Illusion or not, this Cartesian view underlay most early cognitive science—some cyberneticians perhaps excepted (see Chapter 4.v–vii). In general, the mind was conceptualized without reference to the body, and Subject and Object were taken as given. There was no suggestion that the nature of mental states depends on the fact that we have bodies (as opposed to brains). Certainly, sensory transducers and motor effectors entered this functionalist story. But the prime theoretical focus lay inside the head. Mental life was seen as a sensori-motor sandwich (2.iii.a, 10.iv.b), a repeated cycle of *perception*, *thought*, *action*. And *thought* was where the interest lay.

This is why most early AI (as we saw in Chapters 9.x–xi and 10) confined itself to text-based examples, and discarded much of the real-world information even when sensori-motor aspects were included. In effect, seeing ('scene analysis') was treated as a species of reasoning. Even robotics, in GOFAI, concentrated on abstract representation and planning. The messy details of the real world, and of bodily movement in it, were excluded whenever possible. By contrast, many workers in situated robotics, such as

Barbara Webb for instance, would welcome these messy details as a way of testing their theories (see Chapter 15.vii.c).

Putnam had started out as an unabashed realist. Before formulating functionalism, he'd even believed that all sciences reduce to physics (see Section iii.b). But, like Wittgenstein before him, he changed his mind. By the early 1980s, he was convinced that there are no metaphysically intrinsic properties, no mind-independent facts, or "ready-made world" (Putnam 1982).

Properties (he now argued) are "essential" only *relative to a description*. If one crushes a statue, it is no longer a statue. But think of it as a piece of clay, and the crushing doesn't matter: shape isn't essential. In the final chapter of *Representation and Reality* (1988), Putnam developed this neo-Kantian theme. He avoided out-and-out idealism, allowing that minds are in the world, not the fundamental creators of it. But he avoided straightforward realism, too. He called his theory "*internal* realism":

> [My idea] is not the view that it's all *just* language. We can and should insist that some facts are there to be discovered and not legislated by us. But this is something to be said when one has adopted a way of speaking, a language, a "conceptual scheme". To talk of "facts" without specifying the language to be used is to talk of nothing; the word "fact" no more has its use fixed by the world itself than does the word "exist" or the word "object". (1988: 114)

He conceded that *within* any given conceptual scheme we need to distinguish between the natural world and our judgements about it. But it is we who determine the nature of our concepts, including *mind*, *body*, *self*, and *material object*. And it is we who construct normative and/or interest-relative notions such as *good*, *rational*, *justified*, and even *cause*. Since a fundamentally mind-independent (naturalistic) account of the world is impossible, mind and language are philosophically prior to science and can't be explained by it.

In short, Putnam had committed the ultimate betrayal: he'd crossed the metaphysical divide to join the neo-phenomenological camp. To be sure, he soon moved to the outskirts of the encampment: by the turn of the millennium, he had modified his "internal realism" of the 1980s to a less extreme (less idealistic) "common-sense realism", according to which we are *directly* aware of real, independently existing, things (Putnam 1999). But still, he had no truck with the notion of internal phenomenal states (*qualia*, sense data), nor with talk of "correlations" between mental and physical states (see 14.x.c–d).

Putnam wasn't alone in pitching his tent in the neo-Kantian compound. One philosopher of cognitive science was already there waiting, and others would soon arrive—as we'll now see.

16.vii. Neo-Phenomenology—From Critique to Construction

Some philosophers used neo-phenomenological ideas not to deny the possibility of a scientific psychology, but to suggest new ways of doing it. Susan Oyama (1985) was a case in point. As we saw in Chapter 14.ix.b, she conceptualized the mind not as a mysteriously pre-structured input–output machine, but as the outcome of a series of developmental shifts in a self-organizing system. However, her key interest was *the origin of form*, of which 'knowledge' is one example. Here, we'll focus on philosophers coming to neo-phenomenology from a concern with *the nature of representation*.

a. Where Dreyfus was coming from

If Putnam had done a philosophical volte-face, one important critic of cognitive science had been consistent in his neo-Kantianism since the early 1960s. Indeed, Dreyfus was already launching his polemic against AI and computational psychology while Putnam was still formulating functionalism (Dreyfus 1965, 1967), and his highly influential *What Computers Can't Do* appeared soon afterwards (1972).

Dreyfus's fourfold diagnosis of AI's failings was discussed in Chapter 11.ii.a–c. Here, our interest is in the philosophy underlying it. This was drawn primarily from the phenomenologists Heidegger (1889–1976) and Maurice Merleau-Ponty (1908–61), from the (closely related) Gestalt psychologists (5.ii.b), and from the later Wittgenstein. Another influence was Polanyi, whose philosophy of science had stressed the scientist's "tacit" knowledge and practical skills (Polanyi 1958; cf. H. L. Dreyfus and Dreyfus 1986; and see 13.ii.b)—and who'd countered Turing face to face in the 1940s by positing unformalizable knowledge (see Preface, ii).

All these thinkers inspired Dreyfus's rejection of AI's ontological assumption "that what there is, is a set of facts each logically independent of all the others" (H. L. Dreyfus 1972: 68). The early Wittgenstein, who had strongly influenced McCulloch (4.iii.c), had put it like this: "The world is the totality of facts . . . The world divides into facts . . . What is the case, the fact, is the existence of atomic facts" (1922: 31). But the later Wittgenstein had roundly rejected it, commenting for instance that "It makes no sense at all to speak absolutely of the 'simple parts of a chair' " (1953, para. 47). Dreyfus echoed this remark:

> Even a chair is not understandable in terms of any set of facts or "elements of knowledge". To recognize an object as a chair . . . involves a whole context of human activity of which the shape of our body, the institution of furniture, the inevitability of fatigue, constitute only a small part. And these factors are no more isolable than the chair. (Dreyfus 1972: 122)

The phenomenologists also inspired Dreyfus's claim that the objects we experience—and to which we have direct access: no representations required—are inherently meaningful, or significant. They exist within a field of human interests (what the Gestalt psychologists called a "life-world") that gives them *relevance*, and which informs and guides our thinking.

Heidegger, in particular, had highlighted the immediate "situation"—in which the phenomenological subject has its Being, and *creates* the "beings" in the world and its coping response to them. The person's situation isn't a part of some objective external environment, but a lived context for practical action. Similarly, Merleau-Ponty had said "Motility [is not] a handmaid of consciousness, transporting the body to that point in space of which we have formed a representation beforehand . . . [We must] avoid saying that our body is *in* space, or *in* time. It *inhabits* space and time" (1945/1962: 139).

It follows, Dreyfus insisted, that we don't need to posit a third level of discourse, between phenomenological experience and neurophysiology. He followed Merleau-Ponty's example in saying that "what the learner acquires in experience [of the world] is not *represented* in the mind at all but is *presented* to the learner as a more and more finely discriminated situation" (1998: 3). Indeed, he argued—like Malcolm: see Section v.f—that it's not even clear what expressions like "mental processing" or "mental operations" could possibly mean (1972: 91). He was especially critical of GOFAI, but he rejected *any* theory of internal representations. In short: "To avoid inventing problems and mysteries we must leave the physical world to the physicists and neurophysiologists, and return to our description of the human world which we immediately perceive" (1972: 183).

As for where the meanings come from, he said, they're grounded in the body. Here, he was following Merleau-Ponty, who had written extensively on the difference between one's own body and mere physical objects. The body, in the phenomenologist's sense, cannot be described in purely naturalistic terms since it is the *origin* of meaning, or value:

> My body is the fabric into which all objects are woven, and it is, at least in relation to the perceived world, the general instrument of my "comprehension."
>
> It is my body which gives significance not only to the natural object, but also to cultural objects like words. (Merleau-Ponty 1945/1962: 235)

Hence, said Dreyfus (1967), computers must have bodies in order to be intelligent. But this requires more than being a robot: "no one understands how to begin to program primitive robots, even those which move around, to *have a world*" (H. L. Dreyfus 1972: 211; italics added).

On this view, we cope with the world by exercising our "bodily skills", which range from pattern recognition, through language, to tool use. (Tool use was a favourite example not only of Heidegger, Merleau-Ponty, and Polanyi but also of George Miller in his cognitive science manifesto: Miller *et al.* 1960. It's instructive to compare what Heidegger and Miller said about what's involved in using a hammer: Chapter 6.iv.c.)

Skilled behaviour, for Dreyfus, isn't the result of computation or information processing, and nor is it the execution of a plan. We may (consciously) follow rules or make plans when we're learning the skill, but "there is a moment when we finally transfer control to the body" (H. L. Dreyfus 1972: 160). Then, we just do it.

For cognitive scientists reading Dreyfus's critique, there was no "just" about it. They wanted to know *how* behaviour, skilled or unskilled, happens. (Later, they would also want to know how bodily skills *develop*: 12.viii.c–e and 14.ix.a–d.) The intellectual chasm between them and Dreyfus was huge:

> The alternative [i.e. phenomenological] view has many hurdles to overcome. The greatest of these is that it cannot be presented as an alternative scientific explanation . . . [We] shall have to propose a different *sort* of explanation, a different sort of answer to the question "How does man produce intelligent behavior" or even a different sort of question, for the notion of "producing" behavior instead of simply exhibiting it is already colored by the [Cartesian/scientific] tradition. For a product must be produced in some way; and if it isn't produced in some definite way, the only alternative seems to be that it is produced magically. (H. L. Dreyfus 1972: 144)

[The alternative answer] takes the form of a phenomenological description of the behavior involved. It, too, can give us understanding if it is able to find the general characteristics of such behavior . . . Such an account can even be called an explanation if it goes further and tries to find the fundamental features of human activity which serve as the necessary and sufficient conditions for all forms of human behavior. (p. 145)

Compare Merleau-Ponty:

[Phenomenology] is a matter of describing, not of explaining or analysing . . . The whole universe of science is built upon the world as directly experienced, and if we want to subject science itself to rigorous scrutiny and arrive at a precise assessment of its meaning and scope, we must begin by reawakening the basic experience of the world of which science is the second-order expression. (1945/1962, p. viii)

Cognitive scientists could happily allow that phenomenologists often provide subtle descriptions of experience. One excellent example, published a few years later (with the book motto drawn from Heidegger), concerned the changes in consciousness and bodily skills that happen when one learns to improvise jazz on the piano (Sudnow 1978/2001). But they wanted to know, in scientific terms, *how* the body enables this to happen.

Dreyfus's insistence that it's simply a matter of physical energies affecting the brain, while true at one level of discourse, didn't help. As Sutherland (1974) soon pointed out, "leaving it to the neurophysiologists" might itself involve the use of what Dreyfus had called third-level concepts. Indeed, Marr had already developed a computational theory of the cerebellum—specifically aimed to show how bodily skills can be learnt, smoothed, and controlled (Chapter 14.iv.c). To be fair, this was a connectionist account, not a GOFAI one. Nevertheless, it was a computational theory (see Section ix, below).

Given this intellectual chasm, Dreyfus-versus-X—where X is virtually any pre-1990s cognitive scientist—was less a meeting of minds than the simultaneous snarling of different species of beast. Dreyfus's book caused some bewilderment even among philosophers, for the relation between Anglo-American and Continental philosophers in the early 1970s was more like ships passing in a foggy night than opponents engaging in the arena.

In other words, most Anglo-Americans (with notable exceptions, such as Mays and Taylor) paid scant attention to anything said by the Continentals. When they did, they weren't impressed.

The realist Russell had expressed this intellectual chasm in uncompromising terms. Dismissing the later Wittgenstein as "completely unintelligible", he complained, "I have not found in Wittgenstein's *Philosophical Investigations* anything that seemed to me interesting and I do not understand why a whole school finds important wisdom in its pages" (B. Russell 1956*b*: 319). In his obituary of Wittgenstein a few years before, he'd been more tactful. The later philosophy was mentioned only indirectly: "Getting to know Wittgenstein was one of the most exciting intellectual adventures of my life. In later years there was a lack of sympathy between us . . . " and—as the closing words—"Of the development of his opinions after 1919 I cannot speak" (B. Russell 1951: 297, 298). Despite this conventional observance of *de mortuis nil nisi bonum*, the message was clear. In Russell's eyes, Wittgenstein had betrayed everything he (with Russell) had previously stood for.

On the other side, Heidegger and Merleau-Ponty were no less hostile to realism. And Heidegger was hostile to clarity too. He notoriously declared that "Making itself intelligible is suicide for philosophy" (Blackburn 2000).

Russell wasn't the only one to bridle at such a sentiment. By the 1940s, Heidegger was "a figure of fun, too absurd to be taken seriously as a threat" by analytic philosophers (Dummett 1975: 437), and accused by Anthony Quinton (personal communication) of "ponderous and rubbishy woolgathering". The one-time editor of *Mind* Simon Blackburn (2000) has provided a hilarious critique of Heidegger, and speaks of "the purgatory of trying to read [him]" (2005: 78). Similarly, Paul Edwards delivers some savage sarcasm in his attempt "to stem this [current] tide of unreason" (2004: 9). Noting that Heidegger's work has appeared in about twenty languages, he remarks that there's no Hebrew translation: "This is perhaps just as well—the Jews have surely suffered enough already" (p. 11).

Given such entrenched hostility within the Anglo-American camp, it's doubly strange that this German philosopher should now be so fashionable in some areas of cognitive science. For besides eschewing "intelligibility", he scorned the cyberneticists' view of man-as-machine as the ultimate metaphysical illusion. (He was also a savage critic of technology in general.) One might say that he foresaw what Donna Haraway (1986/1991) would call the "cyborg" culture, and fought against it with all the contempt he could muster.

Ryle had been unusual among analytic philosophers, having read fairly widely in phenomenology and being at least partly sympathetic to it (Ryle 1929, 1962). And Taylor (1971) had recently followed his critique of behaviourism (see Chapter 5.iii.b) by drawing on Heidegger, and on hermeneutics, to argue that science—because it's constructed by human subjectivity—can't hope to explain meaning.

But most Anglo-American philosophers at that time—and as noted above, many even now—judged Heidegger to be "a prolix charlatan and poisoner of good sense", whose prose style was "an abomination", mere "bombastic, indecipherable jargon" (Steiner 1978: 13, 16). When analytic philosophers such as Putnam strayed into the enemy territory of anti-realism, they were more likely to start from paths laid down by Wittgenstein (or even Quine) than Heidegger. So, for instance, they would follow Wittgenstein (and Ryle) in seeing "folk psychology" not as a psychological theory, adequate or otherwise, but as a network of conceptual necessities, constitutive of the very concept of mind.

The prime exception, and the person who was most responsible for bringing Heidegger's work back into Anglophone philosophy, was Rorty (although Dreyfus would come a close second). Rorty's first attempt at resurrecting Heidegger was in a conference paper read in 1974, and published soon afterwards (Rorty 1976). But the most influential was his book *Philosophy and the Mirror of Nature* (1979). His accusation there that Descartes had "invented" the mind (cf. 2.iii.a) was largely due to Heidegger. So, too, was his focus on *practice* as opposed to *cognition*.

b. Hands-on Heideggerians

Rorty's efforts were by no means universally welcomed. But nor were they in vain. By the end of the century, more people were willing to take anti-Cartesian, even Heideggerian, ideas seriously—and to use them *constructively* in generating new approaches in cognitive science. (Whether they were faithful to the originals in so doing is another matter: see below.) Even Dreyfus was now recommending a ‘‘reconciliation’’ between phenomenology and AI (H. L. Dreyfus and Dreyfus 1990).

Some of these anti-Cartesians were scientists doing hands-on research in computer science, AI, A-Life, psychology, or neuroscience. They included a number of people mentioned elsewhere in this book (including in Section x, below): Winograd, Michael Arbib, Brian Smith, Philip Agre, Rodney Brooks, Inman Harvey, Randall Beer, Howard Pattee, Esther Thelen—and Maturana and Varela, whose assault on Cartesian realism dated back to the 1960s.

None of these people was originally inspired by Dreyfus, though some cited him in support after developing their own position. Indeed, with the part-exception of Pattee they weren't initially inspired by the philosophical literature at all. Their unorthodox views arose, rather, from reflecting on their own scientific research.

Winograd, for instance, had been frustrated by the limitations on his GOFAI wizardry in NLP (Chapter 9.xi.b). Later, he was influenced partly by Dreyfus (a Stanford colleague from the mid-1970s) and Heidegger, and also by the Frankfurt School, Maturana and Varela, and Fernando Flores (11.ii.g). Indeed, he and Flores co-authored a book in which they explicitly rejected classical realism (Winograd and Flores 1986: 30–1). So did Varela, who had recently applied autopoietic theory to cognitive psychology (Varela *et al.* 1991).

The psychologist Thelen saw the mind/brain as a dynamical system closely coupled with the environment, making this seemingly obvious brain–environment distinction problematic (Thelen 1985; Thelen and Smith 1994) (see Chapter 14.ix.b). Harvey, too, had questioned Cartesian realism, in connection with his work in evolutionary robotics (Husbands *et al.* 1995; Harvey 2000, 2005). And Smith's research—a heady mix of hard-headed computer science and highly speculative metaphysics—had surpassed all but Maturana and Varela in its commitment to criticizing realism.

Others such as Brooks (1987) had rejected representation (and reason too: 1991*b*), if not realism. (That's not to say that Brooks *argued for* realism: he simply assumed it, not being concerned with metaphysics.)

The situated robots described by Brooks, Harvey, Beer, and Arbib were all featured in previous chapters (13.iii.b, 14.vi.c, and 15.vi.c and vii.c). Here, we need add only that our discussion (above) suggests that situated robots are ill-named. To be sure, they respond relatively directly to specific environmental cues, in virtue of their physical constitution. That's why they were sometimes described in terms of phenomenological ideas about the body and the lived situation (e.g. Haugeland 1995). But they aren't situated in the full sense, because they aren't genuinely *embodied*.

As Merleau-Ponty would have been quick to point out, being an autonomous mobile robot isn't the same as having a body. This follows, too, from the philosophical biology of Maturana and Varela (Chapter 15.vii.b, and Section x.c, below). As for Agre and Smith, they're considered in Sections ix.c and ix.e, below.

c. Flights from the computer

Other late-century anti-Cartesians were philosophers of cognitive science—such as John Haugeland, Timothy van Gelder, and Clark. In general, they were sympathetic to the 'insider' attacks just mentioned. Like these scientists, they identified with cognitive science as an explanatory enterprise. If it was faulty, they wanted to fix it—not abandon it.

Today, Haugeland (at the University of Pittsburgh) is a leader of the neo-phenomenological movement in the philosophy of cognitive science. He took Dreyfus's work seriously from the start, and wrote a paper with him for the Kent meeting where Malcolm and Anscombe had also spoken (see Section v.f) (H. L. Dreyfus and Haugeland 1974). But his phenomenological sympathies were then largely implicit. They became fully explicit only in the 1990s.

Whereas Dreyfus's attacks on AI were unremittingly hostile, in both substance and tone, Haugeland's critique was more measured, and more constructive. In his first monograph, he complained:

> Debunkers want to shoot AI down before it takes off, regardless of any actual research, as if the very idea were somehow crazy or incoherent. But the prospects for cheap victories strike me as slender. (1985: 249)

So, for example, he doubted whether computational psychology, or "cognitivism" (1978), could conceptualize moods, and ego involvement (see Chapter 7.i.d). But instead of dismissing cognitivism outright, he said that such phenomena "deserve a lot more study, even in AI" (1985: 238). Similarly, while suggesting that GOFAI couldn't model imagery, he allowed that some non-GOFAI computers could (p. 228).

In the 1980s, Haugeland saw cognitive science as "a great intellectual revolution", based on "a profound and distinctive empirical hypothesis" (pp. 2, 249). Its "paradigm", namely GOFAI—defined so as to include the claim that thought *is* symbol manipulation (pp. 112–13)—was "the most powerful and successful approach to psychology ever known" (p. 250). It had already made some important discoveries, and might—or, as the sceptics claimed, might not—make many more:

> By disciplining theory, the computational model also liberates it: issues can be formulated and accounts developed that were hitherto essentially inconceivable. Certainly these early explorations have encountered unanticipated complexities and difficulties, but even they constitute a wealth of unprecedented empirical knowledge. (p. 211)

> We hate to withhold judgment: scepticism is intellectual anemia. How much more fun, more vigorous, more apparently forthright to take sides! Yet, sometimes, the results are just not in. I am not really convinced that GOFAI is impossible; on the other hand, I'm certainly far from persuaded that it's inevitable. I am dubious: near as I can see, after thirty years, the hard questions remain open. (p. 254)

In short: "no 'disproofs' of AI are proposed, but at most some issues to ponder" (p. 213).

One of the issues to ponder was intentionality. In questioning whether one can "make sense" of AI programs, Haugeland wasn't merely saying (like Searle in 1980) that AI meanings are derivative, but (like Dreyfus) that meaning originates in the lived context and is expressed by embodied, situated, action.

In the 1980s, he put this in Dennettian—not phenomenological—terms. To ascribe meaning, he said, we must "ascribe beliefs, goals, and faculties [to mice as well as to men] so as to maximize a system's overall manifest competence . . . [namely] the ability to achieve goals by making rational decisions in the light of knowledge and perception" (1985: 214–15, 264). Even then, however, phenomenology was in his mind (and his endnotes). He wasn't sorry, he said, to have neglected phenomenology in his text—but he regretted not having more space to discuss "the 'background' of everyday practice" (1985, p. ix). This concept was prominent in Continental philosophy. It had also been stressed by Searle, who defined it as "a set of nonrepresentational mental capacities ['know-how'] that enable all representing to take place" (1983*b*: 143).

Ten years later, Haugeland's phenomenological sympathies were more explicit—and more committed. Previously, he'd felt no qualms in ascribing intelligence to mice (1985: 214). Now, in a glowing review of Dreyfus's unrepentant update, *What Computers Still Can't Do* (H. L. Dreyfus 1992), he declared: "There's only *one* world, *this* one—and it's ours . . . In my own view (and I suspect also in Dreyfus'), there is no such thing as animal or divine intelligence" (Haugeland 1996: 127). Even if there were, he said, that would only extend the scope of who "we" are—for "the *world* just is the realm of the meaningful; in other words, it is where intelligence abides" (p. 127). And he explicitly rebutted the realist's challenge:

> But what about the physical universe: countless stars and galaxies, vast mindless forces, fifteen billion years of atoms and the void? Isn't that the *real* world, a fleeting speck of which we happen to throw an interpretation over, and regard as meaningful? No, that's backwards. The physical universe is *a part of* our world . . . it is *not* primary. Accordingly, cognitive science would be trying to build from the roof down if it began its investigation of intelligence with our understanding of physics. The foundations of intelligence lie not in the abstruse but in the mundane. (p. 127)

Card-carrying functionalists couldn't be expected to agree. They preferred his earlier dismissal of "zany idealism" (1985: 39).

As for Haugeland's opinion of functionalists, he now accused them of presupposing "tendentious and substantive metaphysics" (1996: 120). In other words: Descartes's metaphysics, minus his substance dualism.

Haugeland's metaphysics was phenomenological. It started out from the subject's lived presence in the immediate situation, and was unremittingly externalist with regard to meaning. It followed, he argued, that the scope and boundaries of intelligent systems are not what is usually assumed—by functionalists, as by common sense. Study of "the brain alone" is insufficient for understanding intelligence, and even "an entire individual organism may not be, by itself, encompassing enough" (1996: 121).

Compare Dreyfus:

> But what about *my* experience, one may ask; my private set of facts, surely that is in my mind? This seems plausible only because one is still confusing this human world with some sort of physical universe. My personal plans and my memories are inscribed in the things around me just as are the public goals of men in general. My memories are stored in the familiar look of a chair or the threatening air of a street corner where I was once hurt . . . After all, personal threats and attractions are no more subjective than general human purposes. (H. L. Dreyfus 1972: 178)

Through the 1990s, Haugeland's philosophy grew increasingly Heideggerian (dare one say "zany"?). It eventually included claims such as "The general form of free human commitment—of care or faith—is love'' (Haugeland 1998: 2). Smith might have sympathized, as we'll see (cf. Section ix.e). But most cognitive scientists wouldn't.

Van Gelder (1962–), then based at the Australian National University and Indiana University (now, in Melbourne), exhorted the philosophers of the 1990s to consider not the computer, but the Watt governor (1992/1995, 1998; van Gelder and Port 1995). The old opposition between AI and cybernetics had recently been reawakened by wide-ranging work in dynamical systems (see 4.v–vii, 14.viii–ix, and 15.viii.c, ix, and xi). Van Gelder took this work as his model.

In arguing (as Dreyfus had done) that the brain is an analogue system of constantly varying energy levels, he tried to be constructive. Unlike most dynamical theorists, he allowed for internal representations. But he described them in a novel way. Challenging Fodor's claim that formalist psychology was ''the only one that we are going to get'' (see Section iv.d), he asked, 'What Might Cognition Be, If Not Computation?'—and answered: state transitions in dynamical systems (1992/1995). He described this hypothesis as a vindication of David Hume's dream of a scientific psychology based on mathematical laws like those of physics (see Chapter 2.x.a).

Unfortunately, to try to be constructive isn't necessarily to succeed. Several philosophers protested that van Gelder was saying little of any substance.

For instance, he insisted that he wasn't ''vainly attempting to do *without* complex internal structures [but was] dramatically reconceiving how they might be instantiated'' (1998: 626). As remarked in Chapter 15.vii.c however, it's doubtful whether the dynamical approach can distinguish specific propositional content. Even Clark, who credited the dynamical approach with significant explanatory power, complained that the system's internal functional organization (and the adaptive roles of its components) is obscured by the highly abstract vocabulary of dynamical theory (A. J. Clark 1997: 118–28).

Likewise, Sloman argued that dynamical theory can't represent any interestingly complex mental state (1993, sects. 7–8). If one construes a mind/brain (or a computer) as having a single ''atomic'' state—a point in a high-dimensional, numerically defined, vector space—with a trajectory in the phase space of possible global states, one loses all the important structure in the system. One can't identify the many coexisting, more or less independent, subsystems (and sub-subsystems . . .), or their various, and largely asynchronous, interactions.

Of course, said Sloman, one can try to characterize these in detail—perhaps by constantly varying the number of dimensions, adjusting the numerical parameters, and/or defining attractors on different levels. But once one starts doing this, the distinction between a dynamical systems view and a more conventional AI view begins to disappear. It's no accident, then, that computer scientists speak of computational substructures and sub-processes, not trajectories of global patterns of bits or electric pulses. We need the concept of the virtual machine (see Section ix.c). In short, whereas Hume can be forgiven for taking physics as the model for psychology, today's philosophers cannot.

Yet others objected that the notion of a ''dynamical system'' is so wide that it includes whirlpools and windmills—and digital computers, too (Giunti 1997). Unless one can say just which types of dynamical system are specifically cognitive, one isn't saying much (Chrisley 1998).

Moreover, current computer models of dynamical systems aren't actually continuous, even if—like CTRNs (15.ix.b)—they're called continuous. Rather, they involve discrete state transitions (they're implemented in von Neumann machines). So, contra van Gelder, they *cannot* do things which classical computation can't do.

Finally, van Gelder defined "computation" in terms of Turing machines, arguing (correctly) that this abstract notion can't support various aspects of real information-processing systems, such as temporality. But, as both Sloman (1993) and Ronald Chrisley (1999) pointed out, there are good reasons for adopting a very different definition of computation, immune to van Gelder's criticisms (see Section ix.c and f).

To dismiss highly general philosophical claims for a certain theoretical approach isn't to say that it's never useful (especially if enriched by others: cf. Sloman's remarks, above). A meeting on 'Dynamic Representations in Cognition', co-organized by van Gelder at Indiana University in 1991, considered situated robotics, neuroscience, and psychology (Port and van Gelder 1995). The approach seemed helpful, for instance, in explaining the sudden shifts, or "phase transitions", between different gaits (walk, trot, canter, gallop), or the development of skills such as reaching, grasping, and coordinated kicking (Thelen 1995; see 14.ix.b).

One paper even applied dynamics to Simon's old stamping ground: decision making (Townsend and Busemeyer 1995; cf. Busemeyer and Townsend 1993). It defined a "decision field theory", to reflect the fact that decision making is temporal (unlike logic or probability theory). Continually shifting considerations—preferences, values, expectations, inferences, potential conclusions—"push" and "pull" against each other as the mind moves through the decisional state space; and the trajectory varies with the time available. (Similarly physics-based language was used by William James, in a startlingly apposite passage comparing the phenomenology of choice with the elasticity of a material object—quoted in Townsend and Busemeyer 1995: 102.)

Prima facie, this example seems to contradict the criticisms (above) about dynamics being unable to deal with propositional content and cognitive structure. But to show that the appearance and interaction of "atomic" states X, Y, and Z can be dynamically described isn't to use those states' specific internal structure to explain *why* they arise or *why* they influence each other in the ways they do. The decisional (semantic) trajectories defined by Townsend and Busemeyer were suggested by their prior understanding of rationality, not by the 'physics' of their field theory. That is, the various possible options, and the reasons for and against, weren't actively generated but were taken as given. What was being studied was how the mind then vacillated between them.

Analogously, Haugeland (1978, sect. 6) suggested long ago that a holographic system could act as an associator of visual patterns, and thus underlie (human) chess playing. The first member of a pair of chess positions would be "an important common substructure" in chess, and the other a move (or two alternative moves) that's generally powerful in response to such structures. That's plausible, perhaps, as a description of what goes on in the practised player's mind. But it's not clear that it can explain why response X or response Y was tried out in the first place. That, too, would have to be explained as a series of pattern associations (see 12.x.g).

d. A computational philosophy of embodiment

Clark, like van Gelder, was impressed by the late-century research in dynamical systems—and also by work in animate vision and A-Life (14.vii and 15.v). By the early 1990s, he was already well known as a philosopher of connectionism (Chapter 12.x.e). Then, he 'intermitted' from Sussex for seven years to work at the University of Washington, St Louis—a world-leading base for neuroscience. (Now, he's back in the UK, at the University of Edinburgh.) The intellectual environment in Washington helped him to shine a biological searchlight onto connectionism, and to relate the philosophical question of what counts as a "representation" to various types of neural mechanism (see Chapter 14.viii.d).

The diversity of biological representations, and their difference from the static entities posited by Fodor and GOFAI, was one theme of his book *Being There* (1997). This punning title linked Heidegger with both Woody Allen and Peter Sellers (not easy!). But it was the provocative subtitle, *Putting Brain, Body, and World Together Again*, which announced the main theme.

Clark aimed to counter the disembodied, individualistic picture inherited from Descartes. He did more than insist that "'meanings' just ain't in the head". He described his position as "active externalism", because it focused on the active role of the environment in driving cognitive processes (what he later called "cognitive technology": A. J. Clark 2001, ch. 8)—see Chapter 12.x.e. Indeed, he went further: he saw the (natural and cultural) environment as *constituting* minds and mental capacities.

The empirical roots of this position were deep and long-standing (Chapter 5.ii.b–c). Gibsonian "ecological" psychologists had long stressed the role of the agent's actions (and interests) in exploring its environment, and the specific bodily adaptations involved. Jean Piaget had applied similar insights in the context of "epigenetic" development. Even GOFAI workers had often remarked, though also often forgotten, that the external world provides a rich source of information, lessening the need for detailed on-board memory.

All these ideas had recently been revivified by an upsurge of research on enactive cognition, cognitive and developmental neuroscience, situated and evolutionary robotics, and dynamical systems (see Chapters 8.iii, 14.vi–vii and ix.d, and 15.vi–viii). The picture of the mind/brain as a *self-organizing* system had been reinforced by connectionism on the one hand and developmental neuroscience on the other. Moreover, dynamical approaches had stressed the difficulty, even arbitrariness, of locating the boundary between closely coupled systems.

Clark was the leading philosopher of cognitive science to draw on these later studies in detail. Others included Horst Hendriks-Jansen (1996) and Michael Wheeler (1996, 2005), both also from the Sussex stable. Clark distinguished a range of explanations found in this literature, and defined criteria for deciding when one is appropriate rather than another. But as well as offering such methodological morals, he painted a picture of "active, embodied, cognition"—and of "the *extended* mind".

The latter idea had already been bruited by neo-Kantians such as Dreyfus and Haugeland (see above), and by phenomenologists in general (Rowlands 2003). It had been argued also by postmodernist and/or feminist philosophers such as Donna Haraway (1986/1991). It had even been supported by 'pure' functionalists. Newell and Simon, for instance, had spoken of the environment as providing an "external memory" (see 7.iv.a). Indeed, the notion that individual minds, or selves, are constituted—constituted, not merely influenced—by their social–cultural relations with other selves was a commonplace of much social psychology and social–political philosophy (Hollis 1977—and see Chapter 13.iii.e).

But this view of the mind hadn't been prominent in the functionalist camp. In his first book, Clark had quoted the PDP group's comment that "the external environment [is] a key extension to our mind", and described it as "highly revealing" (A. J. Clark 1989: 133). There, he had related it to the mind's creation of a multiplicity of *virtual* machines. Later, he developed it at length, drawing insightfully on the new empirical evidence mentioned above. And he gave a *computational* justification for it.

So he described language and culture as "natural" artefacts. Like ants' pheromone trails and termites' nests, they are part of the natural history (cf. "forms of life") of the creatures who made them. Their function, said Clark, is to restructure the computational tasks we face into a form better suited to the human brain—a basically connectionist, pattern-recognizing, system.

He'd already argued this (in terms of "representational trajectories") in relation to PDP research in general, as we saw in Chapter 12.viii.d. Now, he focused on the specifically human examples. We naturally construct a system, or "scaffold", of cultural situations. That system makes our situated (i.e. largely automatic) actions more efficient, given the computational resources we possess. (This is why chairs are imbued with human, and even personal, meanings: see above.)

These social and physical artefacts, Clark argued, are so closely coupled with our intelligence that they are in effect integral to it. They inform, and are informed by, our thoughts, memories, decisions, emotions, and desires. In a very real sense, then, a human mind, or self, isn't encapsulated within one individual's skull, or even (skin-delimited) body, but is dynamically extended across the human environment:

> [The] boundary between the intelligent systems and the world [is] more plastic than [has] previously been supposed—in many cases, selected extra-bodily resources constitute important parts of extended computational and cognitive processes. Taken to extremes, this seepage of the mind into the world threatens to reconfigure our fundamental self-image by broadening our view of persons to include, at times, aspects of the local environment. (A. J. Clark 1997: 213–14)

Clark's "externalism" was very similar to Clifford Geertz's 'non-psychological' anthropology (Chapter 8.ii.a). But it was even more shocking, for whereas Geertz had spoken of the "mind" being located outside the head, Clark said this also about the "self"—even closer to the bone, as one might say. Minus the scientific trimmings, his view soon reached a much wider philosophical audience, in a prizewinning paper co-authored with his Washington colleague Chalmers (Clark and Chalmers 1998). The prize was awarded by *The Philosopher's Annual* for one of "the year's ten best papers in philosophy"—*any* area of philosophy. Not everyone was convinced, however: for a critique, see (Butler 1998, ch. 6).

The implication for cognitive science was that we must think of, and therefore study, mental capacities or skills in a new way. The skill of *ship navigation*, for instance, emerges from (is situated within) a complex coupling of individual personalities, social roles and conventions, maps and instruments, mariners' knowledge, problem solving (often spread across several crew members), and a variety of bodily skills (for details, see Chapter 8.iii).

Similarly, said Clark, "much of what we commonly identify as our mental capacities may . . . turn out to be properties of the wider, environmentally extended systems of which human brains are just one (important) part" (1997: 214). Those systems, of course, include computers. Clark's friend van Gelder (2005) has explicitly linked current PC functionality to changes in the "self-constitution" of the rationality of the people who use them—changes which he regards as largely welcome (see Chapter 10.i.h).

A few years later, Clark would have mentioned the intriguing performance artist Stelarc, who came as Visiting Artist to Sussex's Centre for Computational Neuroscience and Robotics. Stelarc was the first person to explore the possibilities and implications of a wide range of man–machine interconnections (M. Smith 2005). One project initiated with CCNR, for instance, was to use electrical signals from his muscles to control a specially built robot many miles away. Indeed, he'd already done this in simple ways (and we saw in Chapter 14.x.b that comparable results have been achieved by linking monkeys' brains to far-distant robots).

The distinction/link between 'real' and 'virtual' worlds (Chapter 13.vi) is a prime interest not only of Stelarc (1986, 1994, 2002*a*, *b*), but also of Clark. He recently returned to it in his popular book describing human beings as "natural-born cyborgs"—in which he did, indeed, mention Stelarc (A. J. Clark 2003*a*: 115–38; cf. Haraway 1986/1991). Mainly, however, he showed how various novel types of computing technology—wearable computers, environmental computers, extrasensory prosthetics, virtual reality, augmented reality . . . —were destined to change people's thinking, experience, and self-image as much as language, writing, and printing already have done.

For all his sympathy with the new methodologies, however, Clark refused to share their all-encompassing scorn for the old orthodoxy:

> The true lesson . . . is not that we somehow succeed *without* representing (or, worse, without computing). Rather, it is that the *kinds* of internal representation and computation we employ are selected so as to complement the complex social and ecological settings in which we must act. (A. J. Clark 1997: 221)

In one important respect—and despite his occasional references to Heidegger, Merleau-Ponty, and Dreyfus—Clark doesn't 'fit' into this section. Putnam, Dreyfus, and Haugeland had all rejected realism. Even Putnam's "internal realism" held that our concepts of mind, body, and self are prior to science, not grounded in it.

By contrast, Clark's theory of "the extended mind" rested as much on scientific research as on philosophical argument. (Russell would have approved.) Like the vast majority of cognitive scientists, he presupposed realism. Indeed, he specifically declined to engage in the realism/anti-realism debate:

> Heidegger was opposed to the idea that knowledge involves a relation between minds and an independent world . . . —a somewhat metaphysical question on which I take no stand. (A. J. Clark 1997: 171)
>
> Varela *et al.* use their reflections as evidence against realist and objectivist views of the world. I deliberately avoid this extension, which runs the risk of obscuring the scientific value of an embodied, embedded approach by linking it to the problematic idea that objects are not independent of the mind. My claim, in contrast, is simply that the aspects of real-world structure

which biological brains represent will often be tightly geared to specific needs and sensori-motor capacities. The target of [my] critique is thus *not* the idea that brains represent aspects of a real independent world, but rather the idea of those representations as *action-neutral* and hence as requiring extensive additional computational effort to drive intelligent responses. (p. 173)

Other *fin de siècle* analytic philosophers were less wary of these metaphysical issues. By the end of the century, anti-realist voices were increasingly heard in the general philosophy of mind—as we'll now see.

16.viii. Mind and "Nature"

By the dawn of the twenty-first century, the "opposing views of the nature of philosophy" still caused radical dissent—even mutual contempt. And the contempt wasn't new.

Realists and anti-realists exemplify what William James (1907) called "tough" and "tender-minded" philosophers, respectively—and little love is lost between them. As James put it, "The tough think of the tender as sentimentalists and soft-heads. The tender feel the tough to be unrefined, callous, or brutal" (p. 13). James's diagnosis was confirmed by Russell's tone of exasperation, when criticizing the later Wittgenstein (see Section vii.a, above).

Later still, it was illustrated by the "science wars" (see 1.iii.b), and by arguments about postmodernism in general (Gross and Levitt 1994/1998; Gross *et al*. 1996; Sokal and Bricmont 1998). Some examples in social psychology and anthropology were mentioned in Chapters 6.i.d and 8.ii.b–c, respectively.

Not least, this mutual mistrust has imbued discussions of mind-as-machine. For example, Guy Robinson (1972) ridiculed functionalism and strong AI by borrowing Thomas Hobbes's contemptuous rhetoric: "When men write whole volumes of such stuff, are they not mad, or intend to make others so?" Since Hobbes himself was a committed mechanist (see 2.iii.b), Robinson was being more than a little mischievous. But he was also being sincere.

By the millennium, many philosophers—in private, if not in print—either shared Robinson's reaction or rejected it with equal passion. Nevertheless, the two approaches had recently become more closely intertwined in some people's work than anyone would have imagined, thirty years earlier.

The new recruits to neo-Kantianism included some hard-headed—but not tough-minded—philosophers raised (like Putnam himself) in the analytic tradition. They respected science but not scientism, and regarded functionalism, and most of cognitive science, as passing illicitly from the former to the latter. Indeed, they shared Rorty's hope for "the disappearance of psychology as a discipline distinct from neurology" (1979: 121). They saw mind (the realm of rationality) as distinct from nature (the realm of the physical and life sciences), and scientific data as philosophically irrelevant. Russell didn't live to see this: presumably, he was turning in his grave.

a. No representations in the brain

One example of this new approach appeared as a volume in a popular series of student-oriented textbooks on 'Problems of Philosophy'. Gregory McCulloch (1952–2002) described the mind as inseparable both from the body and from its surroundings (McCulloch 1995).

Gregory (unlike Warren!) McCulloch leaned heavily on Wittgenstein and Jean-Paul Sartre, and to a lesser extent on Heidegger and Dreyfus. He rejected "mentalism"—by which he meant functionalism, and in particular the language-of-thought hypothesis. Fodor's position, he said, was probably not even the only available scientific psychology—and, for sure, it gave "no account at all of the folk psychological [everyday] mind" (McCulloch 1995: 155). (Clark's 2001 textbook on the philosophy of cognitive science was similar in some ways, but lacked the anti-realism.)

Michael Morris (1955–), a colleague of Clark's (and mine) at Sussex, sympathized with this position. He, too, insisted on the inseparability of the world and our conceptions of it. He avoided idealism, he said, because his theory was *externalist* about meaning. (He later changed his mind about that: see below.) And he had no time for functionalist defences of folk psychology. The notion of folk psychology, on his view, is a scientistic "myth"—one that is "wrong in every particular" (Morris 1992: 111). To say that the death penalty acts as a deterrent is to make a psychological claim (one that's often made by the folk). To regard people as having beliefs and desires, he insisted, is not.

To treat beliefs and desires as theoretical entities, said Morris, is to adopt a fundamentally mistaken (Cartesian) conception of the mind. When we say "She did it because she thought it would help", we are not—*pace* Fodor and his functionalist friends—identifying a causal relationship, but a rational one (pp. 108–9).

As for brain processes, these can cause other bodily happenings, but not behaviour. Or at least, brain processes can't cause the kind of behaviour for which *persons* are *responsible* (namely, *actions*):

> But what *does* cause the behaviour? Nothing. The relevant kind of behaviour [i.e. behaviour which someone is responsible for] is not caused; it is just done. This does not violate such principles as that every event has a cause. People's deeds are not events . . . Events happen; they occur; they are caused. Deeds are just done . . . [This] does not cut deeds adrift from the order of event causation. There is no problem, for example, with thinking that that bit of neuron activity's causing *my hand to rise* is partially constitutive of my action when *I raise my hand*. (p. 136; italics added)

In short, propositional attitudes aren't causes, as they were said to be by functionalist-Putnam. Rather, they are aspects of a system (a human mind) imbued with values. An orthodox Wittgensteinian might put this by saying that they're semantic pieces in the game of rationality, whose rules aren't causal laws but epistemic norms of coherence, evidence, and warrant. Morris prefers to say that there are only two fundamental norms, or "values": namely, moral goodness and truth—intelligible only in combination and contrast with each other. (Hence the title of his book: *The Good and the True*.) The precondition for both of them, he argues, is our personal responsibility.

It follows from this that beliefs and desires can't be explained by *any* naturalistic account. Neuroscience, computational psychology, evolutionary semantics (see Section x.d) . . . none of these can account for rationality. Our evaluation of truth (and our respect for whatever is properly termed *knowledge*) is a philosophical precondition for science, not something that can be explained by it.

This anti-naturalistic position was rooted in the mid-century (pre-functionalist) Wittgensteinian arguments that *intentions*, since they are characterized in terms of the action intended, can't be causes (Melden 1961; cf. Boden 1972). But, writing some thirty years later (and with Clark and myself as colleagues), Morris felt that he had to say something about cognitive science.

This made him unusual, since most neo-Kantian philosophers—including the much more well-known John McDowell (1942–), discussed below—simply ignored its detailed claims. By the same token, it made him interesting for our purposes here. For surely, there should be rather more detailed intellectual contact between philosophy of mind and neuroscience than all-encompassing references to "the brain". Granted that science can't answer philosophical questions, philosophers (of mind) should attempt to situate those scientific theories (of the brain) which at least *appear* to be relevant.

Morris argued that mental representations *as understood in functionalist cognitive science* don't exist—not in Fodor's realist sense, nor in Dennett's instrumentalist sense either (Morris 1991). In other words, there are no *non-semantically individuated objects or states which have meaning, or content*, because no naturalistically identified event can be inherently meaningful. Whereas Dreyfus had implied that cognitive science is a waste of time, Morris didn't. But he argued that cognitive science—*qua* science, not metaphysics of mind—can manage perfectly well without positing representations in the sense defined above (1991: 28–30).

For example, he said, it can investigate certain abstract properties of cognitive capacities, such as the differences between connectionism and other AI architectures. This needn't have any direct bearing on the human mind, but it may—perhaps in respect of the nature of concepts, for instance. (Here, Morris was tacitly agreeing with Churchland's claim that neuroscience may be relevant even to a normative epistemology: Chapter 12.x.c.)

Again, it can study the capacities of actual cognitive systems, using computational concepts of various kinds to discover *functional* relations between *semantically* individuated states.

And third, it can offer partial explanations of psychological capacities, if it can find non-semantically identified types of event that correspond with semantically identified ones—whose meanings are then *stipulatively* assigned to the relevant scientifically identified events. That is: given that we already understand what it is for the cat to sit on the mat, we can decide (*sic*) to assign this meaning to certain neural or computational states. Accordingly, Morris would have no quarrel, for instance, with Clark's claim that neuroscience has discovered various types of representation. (He did suggest, however, that representations would actually be found only for low-level cognitive capacities.)

(For the record, Morris now thinks that he *was* idealist then, even though he believed that he wasn't: personal communication. His view today, in a nutshell that I shan't attempt to crack open, is this: "(1) That the world as it is in itself is entirely independent of our conceptions of it. (2) That there is no correspondence between our conceptions of the world and the world as it is in itself. (3) That most so-called realists—i.e. the ones who believe in correspondence—are in fact committed to idealism (because only an idealist can think that the world as it is in itself has to correspond to our conceptions of it). And (4) that we can perfectly well describe the world as it is in itself—though not by means of any correspondence, of course." What's interesting here, given that Morris is broadly of the anti-Russellian camp, is his continued effort to repudiate idealism. That, clearly, he sees as a step too far.)

b. Mind as second nature

Similar evaluations of science and scientism, and so of the "proper" scope of cognitive science (though without the attention to theoretical details) informed McDowell's highly influential John Locke Lectures.

These were given in Oxford in 1991, and published a few years later (McDowell 1994*a*). McDowell, then at the University of Pittsburgh but originally from the Oxford stable, shared Rorty's view that Descartes had "invented" the mind, not described it (cf. Chapter 2.iii.a).

He was strongly influenced by Sellars's dualism of logical spaces, as Dennett had been too (see Section iv.a). But he rejected Dennett's functionalist analysis of consciousness (see also McDowell 1994*b*). For McDowell, conscious experience is an irreducibly *rational* category—like belief, intentional action, and all other mental phenomena. Accordingly, he rejected any suggestion that the laws of natural science could explain human psychology.

He also refused to follow Nagel—or Everyman—in ascribing subjective consciousness to bats. Subjectivity (he argued) requires meaning, or content, which requires concepts. Without concepts, there can be no particular view of the world, nor any understanding of its general properties. So, despite what most cognitive scientists assume, there can be no such thing as non-conceptual content (12.x.f). And, for McDowell, there can be no such thing as animal experience. He allowed—contra Descartes (Chapter 2.ii.d–e)—that it would be "outrageous" to deny that animals have feelings (such as pain) or "perceptual sensitivity to their environment". But these aren't rational phenomena, so aren't experiences. (Whether they can be anything over and above the patterns of animal behaviour to which Descartes himself drew attention is unclear.)

McDowell wasn't alone in drawing a sharp distinction between humans and animals: most neo-phenomenologists did so. Haugeland, for instance, changed his mind about intelligence in mice as his position became more Heideggerian. And Wittgensteinians in general argued that animals have no concepts, or thoughts. At the Kent meeting, for instance, Peter Geach (1916–) announced to a crowded lecture hall that "You can't really say that dogs are intelligent." This elicited fierce growls from a blind philosopher's normally silent guide dog—and hoots of laughter from the rest of the audience (and Peter, too).

(Wheeler was an exception, here. Although his view of cognitive science was strongly influenced by Heidegger, who had specifically denied *Dasein* to animals, Wheeler argued that they, too, construct their own "worlds": 1996, 2005. Clark, similarly, rejected Heidegger's "thoroughly social", language-based, view of embodied action—A. J. Clark 1997: 171.)

To claim, as McDowell did, that there is no non-conceptual content is to say there are no mind-independent data (no "Given") to act as the foundation of our knowledge of the world. In other words, no purely causal processes could justify, or even provide rational grounds for, our empirical beliefs. Although causal mechanisms (of perception, for example) are necessary for our having beliefs, no scientific psychology—on this view—can explain how these beliefs emerge.

When cognitive scientists study low-level vision, for instance, they typically describe causal (neurophysiological) processes in computational–intentional terms (see Chapter 7.v.b–d). They say that DOG detectors and the like provide information (cf. 'content') about light-intensity gradients, edges, and depth. For McDowell, such talk may be scientifically defensible: "It would be dangerous to deny, from a philosophical armchair, that cognitive psychology is an intellectually respectable discipline, *at least so long as it stays within its proper bounds*" (1994*a*: 55; italics added). But, he insisted, it's also "a recipe for trouble", since it implicitly encourages the myth of the Given. Perceptual mechanisms as such can provide no meaning, no semantic content, whatsoever.

Moreover, the brain can't generate—still less, justify—beliefs, hunches, inferences, or knowledge. All these are matters of normative rationality (the "demands of reason" imply that we "ought" to believe the truth, and to reflect on our own ideas). They therefore lie forever outside science. In that sense, McDowell declared, cognitive science is impossible.

Like Putnam and Morris, McDowell wanted to avoid idealism—including Rorty's version of it (see Section iv.e). He tried to do this not by resurrecting Kant's noumenal world, but by positing a direct (non-representational), and *natural*, relation between mind and the empirical world.

What he meant by "natural", however, was highly unusual. Certainly, his form of naturalism—like the usual forms, which he termed "bald" naturalism—implied *not supernatural*. (On this point, he aligned himself with Aristotle, who had seen rationality as part of human nature: see Chapter 2.ii.a.) But he distinguished between "nature" and "disenchanted nature".

The latter (modernist) notion is the world as described by the laws of science, conceptualized as existing independently of us. The former (as he defined it) includes also "second nature": the habits of rational thought and action into which human infants are initiated by adults as they mature, and which involve the emergence of "resonance" or "responsiveness" to meaning.

Babies, he said, are the only animals (*sic*) to possess the natural, inborn, capacity to acquire concepts, epistemic norms, and other cultural practices—including the interpretation of behaviour in terms of rational relations between beliefs and desires. This "natural capacity" can't be explained by science, because the logical space of causal laws (as opposed to rational norms) has no room for meaning. If neuroscientists discovered innate brain mechanisms underlying some Chomskyan language acquisition device, they wouldn't have explained our capacity for language. And evolutionary biology couldn't do this either:

> It would be one thing to give an evolutionary account of the fact that normal human maturation includes the acquisition of a second nature, which involves responsiveness to meaning; it would be quite another thing to give a constitutive account of what responsiveness to meaning is. (pp. 123–4)

So far, so familiar: neo-Kantians in general drive a wedge between reason and cause. But McDowell, using the concept of second nature, claimed to be able to reconcile them. Philosophy, he said, must be sharply distinguished from science. Nevertheless, philosophers inclined—mistakenly, but understandably—to a *bald* (scientistic) naturalism could rest easy:

> This should defuse the fear of supernaturalism [including various forms of Platonism]. Second nature could not float free of potentialities that belong to a normal human organism. This gives human reason enough of a foothold in the realm of law to satisfy any proper respect for modern natural science. (p. 84)

Whether Russell, for example, would have seen this as satisfying a "proper respect for science" is highly doubtful. For sure, Fodor didn't. He praised McDowell's book for raising "a number of our deepest perplexities"—but defiantly added, "Which, however, is not to say that I believe a word of it" (Fodor 1995*b*: 3).

Nor was it to say that he could actually disprove it, on pain of contradiction. (This was a paradigm case of the "profoundly irritating" nature of first-rate philosophy: see preamble, above.) Claiming that the two logical spaces hadn't been reconciled, Fodor identified several aspects of McDowell's position that he felt were mistaken, unjustified, or merely metaphorical. What is it, for instance, to "resonate" to meaning? And he stated his own, alternative (but not strictly provable), claims.

McDowell, he said, was "as good a contemporary representative of this [anti-naturalistic] philosophical sensibility as you could hope to find". But, he insisted, "it's all wrong-headed. Science isn't an enemy, it's just us" (Fodor 1995*b*: 8).

c. Mind and VR-as-nature

In the preamble to this chapter, I said that a new twist on the realism/anti-realism debate is related to developments in VR (13.vi). This "twist" combines debate about the existence/non-existence of a real world outside us with debate about just what Fodor's word "us" (in the quotation above) really means. The latter question isn't asking what a particular individual's self is "really" like (a problem raised with respect to the different types of VR avatar in Chapter 13.vi.e). Rather, it's asking what it is to be a human being, and to have (or if you prefer, to be the origin of) genuinely human experience.

These deep philosophical questions have been triggered by the fictional exploration of VR in the hugely popular film of 1999, *The Matrix*. (And I really do mean *hugely* popular: a Google search on 10 January 2006 elicited 104 million items.) The human beings in this story are supposed to be experiencing a purely virtual world, while their living but inert bodies are farmed for energy by the machines in charge. The machines are clever enough to fool the people (the brains?) into thinking that they are walking, talking, and eating in a world much like our own.

Among the leading philosophers of mind who've written about the implications of *The Matrix* are Clark, Chalmers, McGinn, and Dreyfus—with his son Stephen (H. L. Dreyfus and Dreyfus 2002; cf. Dreyfus 2000, 2003). Led by Dreyfus (who remarked that, for the first time, the students were now assigning significant homework to their professors), they all contributed online papers to the Warner Brothers *Matrix* web site. Searle and Dennett gave interviews on the web site, too.

Besides these big guns, the cinematic version of Descartes's worry about the *malin génie* (see 2.iii.b) has prompted many other philosophical discussions, some specifically placed in the context of "popular culture" (Irwin 2002). A clear-headed account that's especially helpful for non-philosophers has been written by Matt Lawrence (2005). But the most relevant in the context of this book are the initiating paper by Dreyfus and son and the later essay by Clark.

Dreyfus uses the distinction between telepresence and VR in arguing that the experience of the *Matrix* inhabitants isn't authentic, and—even more "worrying"—isn't fully human either. In telepresence, such as the distance surgery outlined in Chapter 13.vi.b, radio signals from the real world reach the human participant. The person may have the experience of looking at a TV screen, and of hearing sounds through earphones. So far, so VR. But this virtual reality is closely based on, indeed causally linked with, a genuine reality. Moreover, as Dreyfus points out, that reality can fight back—as reality does, all too often. For instance, the surgeon may receive signals that are not only uncontrollable but highly unwelcome, if the tele-operated robotic tool (located many miles away) encounters unexpected flesh or bone in the real patient's body.

In purely fictional VR, however, as depicted in *The Matrix*, any apparent fighting back on the part of the virtual reality is pre-programmed by the designer. It follows, says Dreyfus, that certain sorts of creativity, which are characteristic of real humans in real environments, are blocked off. Radical changes in the person's mode of interaction with the VR world simply aren't possible, because the possibilities have been strictly limited by the VR program. In that sense, the experience of the *Matrix* inhabitants isn't fully human, even though it may be indistinguishable from genuine experience almost all of the time.

Clark's (2003*b*) paper is more closely linked to cognitive science as such. He sets out the filmgoer's three interpretative options: *The Matrix* as "dream, simulation, or hybrid". Citing various experimental findings (some of which were mentioned in earlier chapters), he defends the cognitive science view of human experience as shifting, constructed, and embodied—and dependent on specific brain mechanisms, including the neurophysiology of wakefulness and dreaming. And he draws specific comparisons, including some important contrasts, between our experiences when awake or dreaming and our experiences when involved with VR. The cinematic humans do indeed have experiences "much like our own", as remarked above. But they aren't entirely like our own, and nor are they compatible with what's known about the neurophysiology involved. (The film is science fiction, to be sure: but human physiology was supposed to be unchanged.)

On the issue that's central to this section, Clark finally opts for a version of realism. That is, he argues that *if* it were the case (which it isn't, not quite) that the humans depicted in *The Matrix* had *exactly* the same experiences of perceptual and bodily engagement with a real, and often resistant, world as we do when we're fully awake, *then* they'd be "embodied intelligences through and through", each having/being "a real body, realized in the non-standard medium of bits of information". In other words, having the experience of engaging with a resistant world, by way of one's senses and motor actions, is (so Clark claims) *what it is* for the mind to be "embodied".

However, it's by no means evident that Clark's arguments (or those of another would-be-realist interpreter of *The Matrix*: Chalmers 2003) would satisfy a more down-to-earth realist. In short, the realism/anti-realism dispute hasn't been settled yet. Maybe it never will be—a possibility that makes the "scientists and engineers" mentioned in the preamble distinctly uncomfortable.

16.ix. Computation as a Moving Target

For all his passionate dissent from McDowell's anti-realist position, Fodor didn't pretend to have a knock-down argument. This isn't surprising: as we've seen, we're dealing here with "opposing views of the nature of philosophy".

It's little wonder, then, that shock and excitement—not to mention scepticism—greeted the claim that one can dissolve this long-standing metaphysical impasse by reflecting on the nature of *computation* (see subsection e, below).

It's almost as wonder-worthy, you might think, that anyone bothered to reflect on the nature of computation in the first place. Hadn't Turing settled that question, once and for all? Wasn't he the one (or he and Alonzo Church the two) who'd claimed that the intuitive notion of "computation" could be formally defined (see Chapter 4.i.c)? Unless someone was going to find *extra* meanings hidden in mathematicians' intuitive use of the term (as, in a sense, Penrose tried to do: 1989; cf. Sloman 1992), what more was there to say?

Well, quite a lot—as we'll now see. And much of that has relevance for what we're prepared to count as "cognitive science", because (by definition) one of the two intellectual footpaths followed by cognitive science is computation (see Chapter 1.ii.a).

a. Three senses of computation

The seminal definition, in this context, was Turing's (see Chapter 4.i.c). He saw computation as the step-by-step symbol manipulation carried out by a Turing machine, conceptualized in abstract mathematical terms. Although he held that these operations could in principle be implemented in physical mechanisms, his concept of computation was a purely formal one.

His definition remains fundamental to theoretical computer science, and no cognitive scientist would dispute its importance. Indeed, it's still the only *rigorous* account of what computation is.

However, as the practices of AI and computer science became increasingly varied, two additional senses arose. (A recent paper distinguishes about a dozen different senses, but for our purposes here that's over-egging the pudding: B. C. Smith 2002.) Neither of these is well defined, as we'll see. But to ignore them is to miss much of the intellectual excitement that's driven the field over the past thirty years or more.

Turing (1936) defined computation in formal terms, as an exercise in pure mathematics. His definition was a formative influence on cognitive science, in the sense that McCulloch and Pitts (1943) used it in proving the logical equivalence of their abstractly defined neural networks and Turing machines (Chapter 4.iii.e). But when (a few years later) digital computers appeared on the scene, enabling logical computations to be implemented in electronic machinery, the notion of computation became less clear.

The notion of proof itself—and GOFAI's stress on theorem-provers (Chapter 10.iii.b)—was questioned (even by lawyers) when mechanized 'proofs' of more than a million inferences came on the scene (MacKenzie 1991*b*, 1993). These programs, it was argued, could be informally checked but not formally verified. Yet (so one eminent software expert declared) their conclusions should be accepted as *proofs* nonetheless:

> We must beware of having the term "proof" restricted to one, extremely formal, approach to verification. If proof can only mean axiomatic verification with theorem provers, most of mathematics is unproven and unprovable. The "social" processes of proof are good enough for engineers in other disciplines, good enough for mathematicians, and good enough for me. (cited in MacKenzie 1993: 177)

For our purposes, what's important is that the notion of computation absorbed some of the features of the machines in which it was implemented. Certainly, a program could still be thought of as a series of well-formed formulae of some logical calculus, or programming language—in other words, as a set of uninterpreted mathematical expressions. But it could also be considered in a very different way, as essentially connected with causal processes in computers (see subsections b–c, below).

With the rise of functionalism in the 1960s, Turing's mathematical definition, already featured in the pages of *Mind* in 1950, became more familiar to philosophers. Indeed, it was for many years the only concept of computation they considered. Several philosophers discussed it at length, and used it as a defining criterion of cognitive science and/or AI (e.g. Haugeland 1985; Fodor 1980*a*; Pylyshyn 1980; Copeland 1993). We've already seen that it's the notion relied on by the authors of various key papers:

* by Mays (1952) in rejecting the Turing Test;
* by Putnam (1960) in defining functionalism;
* by Fodor (1975, 1980*a*) in defining cognitive psychology, and in arguing for methodological solipsism;
* by Searle (1980) in saying that AI programs are "all syntax and no semantics";
* and by van Gelder (1992/1995) in criticizing computationalism and recommending dynamical models instead.

The relevance of Turing's definition wasn't argued by these philosophers, so much as assumed by them. Putnam, for instance, took it for granted in his discussion of multiple realizability. And Searle treated it as an unassailable premiss in his attack on strong AI. He even took it to be a premiss shared by his philosophical arch enemies Newell and Simon, the high priests of strong AI. He had good reason for this. Nevertheless, we'll see (in subsection b) that one might read Newell and Simon's work in a different way.

By the 1990s, however, some philosophers and logicians were puzzling over the definition of "computation", and asking *just which* actual and notional machines were equivalent to Turing machines, and which were not (Bringsjord 1994; Calude *et al.* 1998; Copeland and Sylvan 1999).

AI practitioners were moving beyond Turing-computation too. For instance, certain types of neural network (in which the weights could be represented as irrational numbers) were said to be capable of analogue computation (Siegelmann 1995, 1999). Indeed, even *without* such maverick methods, AI scientists weren't interpreting computation as rigidly as outsiders assumed. Sloman—a skilled AI programmer, as well as a philosopher—has said:

> No programmer or computer engineer has, to my knowledge, ever thought of programs in [Searle's] way, and as a programmer myself I have never thought of programs that way. (This criticism of Searle was made by many computer scientists and AI people around 1980.) (Sloman, personal communication)

Sloman goes even further, for he sees Turing machines as largely "irrelevant" for the history of computers and AI (Sloman 2002). Both of these, he argues, are natural developments of two types of artefact: machines for controlling machines (including mechanical looms, musical boxes, and Hollerith machines), and machines for operating on abstract entities (numbers, logical terms, or other symbols). These existed long before Turing was even a gleam in his mother's eye (see Chapters 2.iv and ix, 3, and 4). They blossomed—in power, sophistication, and range of application—in the 1940s–1950s because of electronic technology, not because of Turing's mathematics. Much as Babbage didn't need Turing machines to design a close equivalent of a von Neumann computer (Chapter 3.iii.b), so the modern pioneers of computing and AI didn't strictly *need* them either. The people for whom Turing machines were crucial were those doing *theoretical* computer science.

That's all true. (And Turing himself, before learning about electronics, thought that physical computers were infeasible because they'd have to be as big as the Albert Hall: Chapter 4.i.d.)

One must add, however, that Turing used his theoretical ideas when designing the ACE, and that the designers of MADM—the first stored-program general-purpose electronic digital computer—did so too (3.v.b–c). One must add, also, that AI and computational psychology—and the von Neumann computer itself—were recognized as exciting possibilities in the mid-1940s partly because of a marriage between Turing machines and Russell's logic (4.iii–iv). Furthermore, Turing's paper in *Mind*, which was deeply influenced by his earlier work, was intended as a manifesto for AI and was hugely influential in the young AI community (see Section ii, above). In short, although computers and AI would have happened even without Turing machines, the way in which they did happen was deeply influenced by them.

What's especially relevant in this context is that *philosophers* focused on Turing's formal definition of computation. This said nothing about implementation, whether the physical or the virtual machine. But he'd made provocative claims in the philosophers' trade journal about (future) AI's ability to model the mind, and was (wrongly) believed by them to have offered a behaviouristic Test for successful AI. Putnam, when defining functionalism, used the abstract concept of Turing machines and didn't even mention AI.

Moreover, philosophers in general could cope with mathematical logic, but knew little about computers and even less about the experience of using them. For these reasons, they assumed (and most still do) that Turing-computation was *essential* to the inception and practice of symbolic AI. Having this (first) sense of computation in mind, then, they usually see "computation" as typified by GOFAI and often regard "cognitive science" as essentially symbolic.

The second sense of computation is less restrictive, for it amounts to *Whatever methods are actually used in computer modelling*. Because the list of practicable methods has lengthened with the advance of AI/A-Life, just what this second sense picks out has changed over the years—and will continue to do so.

GOFAI still qualifies, of course. But so do the several kinds of connectionism (Chapter 12); the coordinate transformations of tensor network theory (14.viii.b–c); the various styles of evolutionary programming (15.vi); and the many types of cellular automata (15.v and viii). Situated robotics and dynamical models inspired by physics are covered too (15.vii–viii and xi).

Indeed, new and notional forms of computation are now being discussed. Some of these involve "quantum computers" (D. Deutsch 1985), which—in case they are practically feasible—could do Turing-computation much faster than conventional computers do. (In November 2001, scientists at MIT reported having used a "quantum computer" to find the factors of 15; some of the large banks, dependent as they are on prime-number cryptography, were sufficiently worried to employ consultants to look into these issues immediately: C. T. Ross, personal communication.)

Others involve what Jack Copeland and Diane Proudfoot have called hypercomputers (Copeland 1998*b*; Copeland and Proudfoot 1999; Copeland 2000). These, like Turing's imaginary O-machines mentioned in Chapter 4.i.c, would compute the uncomputable (the key terms being interpreted in the intuitive and Church–Turing senses, respectively). The philosopher Selmer Bringsjord, for instance, has argued that hypercomputation is already being performed by human minds, and that it might one day be performed by computers too (Bringsjord 1992; Bringsjord and van Heuveln 2003: 64–7; Bringsjord and Zenzen 2003). Although he criticizes Penrose's statement of the argument from Gödel, he provides a "modal" version of it which also rejects current AI as the core of a science of mind (Bringsjord and Arkoudas 2004).

At present, ideas about hypercomputers are more speculative than practical (Bringsjord 2001; Copeland 2002*a*; M. Scheutz 2002). But if they're implemented in some future technology, they too will be included under this second, catch-all, sense of "computation".

The more inclusive notion of "computational" concepts has become increasingly prominent since the mid-1980s, in discussions of mind as machine. But it was there right from the start. When cognitive science first emerged, most of the people involved were active both in logical-computational theorizing and in dynamical cybernetics—which they saw as part of the same general enterprise. It wasn't until some years later that these two types of computer modelling were restricted to different research communities (see 4.v–viii).

McCulloch and Pitts, for instance, saw their seminal connectionist paper of 1947 as a new way of developing the ideas they'd put forward in 1943 (Chapter 12.i.c). So did their contemporaries. The sociological separation of GOFAI and connectionist AI happened later. Defining their collective project in a way that excluded the non-GOFAI activities (as Fodor, for instance, did) would have struck them as bizarre.

But perhaps it's better to be bizarre than to be boring? This undiscriminating definition of computation may seem to have no bite, for it doesn't imply any particular philosophical position. Indeed, some philosophers prefer to define cognitive science in terms of formal representations and Turing-computation largely *because* they then know just what it is that they're talking about. If all non-symbolic enterprises fall by the referential wayside, too bad.

What's more, the second definition may even seem perverse. For the proponents of some of the methodologies just listed made a point of distancing themselves from classical AI, as we've seen. They insisted that their models involved a "new form of computation, one clearly based upon principles that have heretofore not had any counterpart in computers" (Norman 1986*b*: 534); and they spoke of "*sub-symbolic* processing" (Smolensky 1988), of "intelligence *without* representation" (R. A. Brooks 1991*a*), and of "cognition *not* computation" (van Gelder 1992/1995). Why, then, did a growing number of people come to think of computation—and therefore cognitive science—in such an all-inclusive way?

The answer is that these modelling approaches—like different programming languages (see 10.v)—involve significantly different virtual machines, with different capabilities.

* A PDP system, for example, has strengths and weaknesses largely complementary to those of a GOFAI program.
* Localist connectionist networks are different again.
* Cellular automata differ from classic sequential programs, and their performance varies according to the types of rule they embody.
* One evolutionary model may be significantly different from another, and all can do things which non-evolutionary programs in practice can't.
* Dynamical systems, such as CTRNs, are unlike classical AI, and can traverse their state space in diverse ways.

Moreover, one virtual machine can be implemented in a different virtual machine . . . and so on. We saw in Chapter 12.viii–ix, for example, that a *sequential* virtual machine can be emulated by a *parallel* virtual machine implemented in a *serial* computer (which in turn may involve several 'layers' of programming languages, ultimately implemented in the machine code).

A host of interesting questions have already arisen about the types of virtual machine that could model human minds—or, more provocatively, what type(s) of virtual machine(s) the mind might actually *be*. Others haven't yet been asked, because they (will) concern types of computer model still to be developed. As remarked above, these may involve highly unconventional machines, including hypercomputers (Calude *et al*. 1998).

To restrict "computation" or "cognitive science" only to Turing computation and GOFAI is as unreasonable as restricting "physics" to the mathematics available to Galileo. When Galileo said (in 1623) that the Book of Nature is "written in the language of mathematics", he didn't mean that the concepts used in *his* mathematics could solve all the problems of physics, but that physicists need to travel along mathematical pathways. And indeed, modern physics isn't definable in terms of only one type of theory (van Fraassen 1980; Cartwright 1983).

Analogously, cognitive science employs a variety of concepts and models, which are "computational" in a general sense. (Even dynamicists speak of the "computations" performed by their models.)

The third sense of computation is philosophically the most controversial. It's also the most unclear. Indeed, it's not so much one sense as a group of rather different senses, all informed by the same general aim.

This aim is to present computation as intentional, and meaning as computational—in part, by focusing on the causal relations involved. The third-style approach conflates *what computers do* with *what minds do*, by way of an account of intentionality that supposedly applies to both. In the rest of this section, we'll consider four—or, more accurately, three and a bit—examples of this third sense of the term.

b. Physical symbol systems

The earliest, and most influential, third-style example was due to Newell and Simon (Newell and Simon 1961, 1976; Newell 1980).

Their AI programs specified formal computational systems of the type defined by Turing. As psychologists, however, they were interested in computations that are grounded in the world and capable of directing behaviour in it. Accordingly, they outlined a semantic theory designed to avoid what Putnam and Fodor called methodological solipsism, and to support what Searle would term the Robot reply.

Their central claim was that intentionality is achieved, in both minds and computers, by implementing certain types of formal computation. Such implementations, or Physical Symbol Systems (PSSs), were seen as "the necessary and sufficient means for general intelligent action". In other words, the mind *is* a PSS. Moreover, said Simon (towards the end of the century):

> [Some programs can] demonstrably understand the meanings (at least some of the meanings) of their symbols, and [they] have goals . . . In contrast [to a simulation of digestion], a computer simulation of thinking thinks . . . We need not talk about computers thinking in the future tense; they have been thinking (in smaller or bigger ways) for forty years. They have been thinking "logically" and they have been thinking "intuitively"—even "creatively". (Simon 1995*a*)

In arguing their case, Newell and Simon defined computation in causal terms. A *symbol*, they said, is a physical pattern with causal effects. The meaning of a symbol is the set of changes it enables the information-processing system to effect, either *to* or *in response to* some object or process (outside or inside the system itself). Analogously, concepts such as *representation, interpretation, designation, reference, naming, standing for*, and *aboutness* were causally defined. For example:

> Two notions are central to this structure of expressions, symbols, and objects: designation and interpretation.
>
>> *Designation*. An expression designates an object if, given the expression, the system can either affect the object itself or behave in ways depending on the object.
>
> In either case, access to the object via the expression has been obtained, which is the essence of designation.
>
>> *Interpretation*. The system can interpret an expression if the expression designates a process and if, given the expression, the system can carry out the process.
>
> Interpretation implies a special form of dependent action: given an expression, the system can perform the implicated process, which is to say, it can evoke and execute its own processes from expressions that designate them. (Newell and Simon 1976: 116)

Whereas the meaning of an atomic symbol depends on its causal history and effects, that of a complex symbolic expression depends on the meaning of its components *and the relations between them*. It was the complex expressions which interested them, for they saw these as the inner core of mentality. But how were these relations depicted? Within any implemented symbol structure, they said, the component symbols are "related in some physical way (such as one token being next to another)".

However, implementation details can vary (multiple realizability, again). So computational psychologists may choose to concentrate on symbol types, rather than symbol tokens. In practice, then, Newell and Simon usually focused on the formal properties of their psychological theories, not on their physical implementation. ("Usually", because they did sometimes take account of neuroscientific evidence: see Chapter 7.iv.b.)

This is why they're typically seen—by Searle (1980), for instance—as proponents of the first sense of computation, not the third. On that view, PSS theory holds that causation grounds the interpretation of computations once implemented, not that it's involved in defining computation *as such*.

c. From computation to architecture

In emphasizing the causal basis of meaning in computational systems, Newell and Simon weren't alone: Sloman did so too.

He dismissed causal theories of meaning (like theirs) that assume some physical relation between a symbol and its referent, because we can refer to non-existing things (e.g. 1986*a*). But he did stress the *virtual* causal processes required for understanding. Moreover, whereas Newell and Simon had taken the concept of cause for granted, Sloman argued that we don't yet understand computation largely because we don't yet understand causation.

Sloman initiated his third-style approach in the 1970s, and is still developing it (Sloman 1978, 1992, 1996*a*,*b*,*c*, 1999, 2002). Throughout this period, he was seeking a new way of thinking about computation, even a new concept of computation—but not a new *definition* of it. In his view, we shouldn't expect to find any precise definition of computation that could function as *the* core notion of cognitive science.

Nor should we expect to find a precise definition of mind (or any other category of folk psychology—such as intelligence, for instance). Philosophers who propose candidates for this illusory honour prompt endless disputes about whether this or that system *is* a mind, *really*. Sloman, by contrast, always stressed the complexity, variety, and many degrees of overlap between different actual or conceivable systems.

He now recommends that we "abandon the notion that the concept of computation is the only or even the central foundation for the study of mind" (1996*a*: 215). He expects the concept to be eventually "replaced by a new taxonomy of designs, covering a more general class of architectures and mechanisms" (1996*a*: 216). This taxonomy will include both minds and mindlike systems, with no clear boundary between them. Computational mechanisms—including many types of virtual machine—are of course involved. But so are physical mechanisms, including neurochemical influences (14.ix.f).

In short, Sloman is a philosopher who takes the design stance seriously. He offers "not a philosophical argument about 'correct' concepts to use, but an engineering argument about the appropriateness of different (animal or artificial) designs for different tasks" (1996*a*: 216).

As remarked in Chapter 2.i.a, the word *engineering* can be a powerful turn-off. Sloman is well aware of this. But he was already arguing in the 1970s, in his book *The Computer Revolution in Philosophy*, that philosophers need to understand computation if they are to solve a raft of philosophical problems (1978, esp. ch. 6). These concern every area of philosophy, but especially the philosophy of mind, science, and language (Sloman 1978, chs. 2–3 and 10; 1986*a*).

He defiantly predicted: "[Within] a few years philosophers, psychologists, educationalists, psychiatrists, and others will be professionally incompetent if they are not well-informed about these [AI's] developments" (1978, p. xiii). At that time, the content of his claim was considered as provocative as its tone. Now, even with "engineering" added, it would be taken rather more seriously.

Many philosophers, however, would still discount it. For they think they already know what computation is—because Turing told us. Sloman allows that Turing's definition is the only precise one, and the only one associated with a clear and solid body of theory. It suffices for dealing with abstract mathematical matters (such as decidability, incompleteness, and complexity), where neither time nor causality is involved.

He allows, too, that Turing's definition has acted as a powerful "catalyst" to cognitive science. But—as Newell and Simon implied in defining PSSs—that notion of computation isn't really what we're interested in, when we ask whether minds are computational machines.

Partly in response to Searle's attempt to divorce computation from intentionality, Sloman argued in the 1980s that computers of appropriate complexity could understand. As for what "appropriate" meant, or "understanding" either, this required further research. But the computers of that time already embodied simple examples of some of the processes needed, such as building and comparing structured representations. The important question, he said, isn't "When does a machine understand something?"—which misleadingly implies some clear cut-off point at which understanding ceases. Rather, it is "What [many] things does a machine—whether biological or not—need to be able to do, in order to be able to understand?" (Sloman 1986*b*).

Although Sloman shared Newell and Simon's impulse to think about computation in terms of causal relations, he differed from them on two crucial points. First, he criticized them for ignoring causal processes within the virtual, as opposed to the physical, machine. For the philosophy of mind, as for psychology, it's the virtual machine which counts (see Section iv). Sloman's later writings explored the increasingly diverse computational processes and ontologies that are instantiated in AI and other forms of computer science. Even hands-on professionals, he said, don't fully understand what their machines are doing.

Second, he didn't take the concept of cause as a philosophical given. On his view, no current analysis captures all our intuitions about causality. It's closely linked with notions of possibility and conditionals, which is why Humean analyses fail. But "possible worlds" accounts, he believed, aren't satisfactory either (Sloman 1996*c*). Nor are the (closely related) attempts in recent theoretical computer science to understand concurrent interacting processes, such as are found on the Internet or in control systems for complex machinery (Sloman, personal communication). But such interactions need to be understood, if we are to achieve a satisfactory notion of causation *as such*.

Besides considering everyday examples, Sloman discussed the causal relations within a wide variety of virtual machines, and between the virtual and the physical machine (Sloman 1992, 1996*b*,*c*). One of his conclusions was that a virtual process can properly be said to cause a physical one, so that *qualia*—which he analyses in computational terms (Chapter 14.xi)—aren't epiphenomenal, but really do cause changes in the brain. But while he has suggested various desiderata for a comprehensive theory of causation, he's under no illusion that he has found one. Nor has he achieved the comprehensive taxonomy of designs necessary, he believes, for understanding both minds and computers. His third-style work is a promissory note rather than a definitive theory.

For many years, Sloman's publications were few and far between, and rarely in mainstream journals. His ideas were highly valued, nevertheless, by a small but discriminating international audience. As we saw in Chapter 10.iv.b, his work on computer vision involved several ideas that were ahead of their time, being rediscovered later by others. He was personally acquainted with most of the AI pioneers, and played an important role in the institutional development of cognitive science in England (where he worked at the University of Sussex for almost thirty years).

In the 1990s, various aspects of his research suddenly became more visible. His long-standing work on motivation and emotion (7.i.f) attracted increasing international attention, as did his approach to consciousness—which, like Dennett's, explicated it in terms of a complex virtual machine. And his ideas on computation, though not yet gathered into the long-awaited book, became more readily accessible through increased publication in print, and on the Web. In short, his influence grew. Many philosophers, however, were still wary of his (principled) refusal to offer crisp definitions, and bemused by his semi-technical discussions of computer architectures and virtual machines.

Sloman, then, is one of those—few, but growing in number—who regard computation (even within GOFAI) as a deep philosophical puzzle. And, as the other side of the coin, he's convinced that even computer scientists don't properly understand what computers are already doing—never mind what they might do in future.

d. The bit in "three and a bit"

This doubly problematic position—that computation is a puzzle, and that even hands-on users don't understand it—is held also by the AI scientist Agre (1997). His 1980s critique of GOFAI planning (see 13.iii.b) was the seed of his later ideas on computation. These are far from what one might expect from a hands-on AI professional.

Like Sloman (and, or so I've argued above, Newell and Simon too), Agre sees computation and intentionality as essentially connected. But he reaches this conclusion in a very different way, being a member of the neo-phenomenological camp discussed in Section vii.

Drawing on postmodernist authors such as Jacques Derrida as well as on Heidegger and Wittgenstein, Agre developed an "interactionist" approach to computation that is radically opposed to classical functionalism. He applied his interactionist vocabulary—or metaphors: his account was far from clear—not only to the computer as the human observer experiences it, but even to the electronic circuitry involved (e.g. Agre 1997: 92–102). And he insisted that computation can't be understood without considering the technical specifics of real examples:

> Computational principles, on my view, relate the analysis of a form of interaction to the design of machinery. These principles are irreducibly intuitive, taking precise form only in the context of particular technical proposals. (Agre 1997: 20)

> The tokens of mechanized logic are not exactly meaningless, since they are useless without the meanings that they are routinely treated as carrying. (p. 96)

Must combinational logic be understood within the mentalistic framework of abstraction and implementation? The happy answer of no can be sustained only by cultivating an awareness of the choices that lie implicit within day-to-day technical practice . . . In order to formulate an alternative, let us start by speaking about digital circuitry in a different way, rescuing it from the discourse of abstraction and implementation and reappropriating it within the alternative discourse of intentionality and embodiment. (pp. 103–4)

[One] should not confuse the articulated conceptions that inform technical practice with the reality of that practice. As [Brian Cantwell] Smith (1987) has pointed out, any device that engages in any sort of interaction with its environment will exhibit some kind of indexicality . . . AI-research needs an account of intentionality that affords clear thinking about the ways in which artifacts can be involved in concrete activities in the world. (pp. 241–2)

Despite his potentially off-putting references to Derrida (hardly an iconic hero of cognitive science: 8.ii.b), Agre—like Sloman—was later invited to contribute to a volume seeking to go beyond Turing-computation (Agre 2002). Nevertheless, I've chosen to describe him, unflatteringly, as "the bit in 'three and a bit' ". Why? The reason is not just his almost perverse unclarity, but the fact that his writing is heavily dependent on another AI researcher, who surpasses him in thoroughness—and, it must be said, in perversity too.

e. A philosophy of presence

As the last quotation indicates, one of the 'technical' authors who strongly influenced Agre was the Canadian computer scientist and philosopher Brian Cantwell Smith (1950–).

Originally trained at MIT, Smith's work was mostly done at Xerox PARC and Stanford (in the Philosophy Department), where he helped found Stanford's CSLI (Center for the Study of Language and Information). Later, he moved to the cognitive science programme at Bloomington, Indiana, and thence to Duke University, North Carolina. Now, he's Dean of Information Science at the University of Toronto.

His interests were always wide, combining intellectual interdisciplinarity with sociopolitical commitments. For example, he was co-founder—with Winograd—of Computer Professionals for Social Responsibility, and CPSR's first President (see Chapter 11.i.c). But no one could have foreseen that this MIT graduate would attempt to turn the notion of computation upside down—or, if you prefer, to enrich it immeasurably. I offer you that choice, because Smith's work is still hugely controversial.

Smith's research, developed over the past quarter-century, is the most far-reaching attempt to address philosophical puzzlement about the nature of computation. One might say that there's nothing it doesn't *attempt* to reach. (Some would add that there's nothing it *does* reach.) It is rich fare, though still a minority taste. Among the gourmets attracted to it, however, are some respected philosophers of cognitive science—and the computer scientist Matthias Scheutz, editor of the *Computationalism*: New Directions volume mentioned above (see B. C. Smith 2002).

This puzzlement grew out of Smith's hands-on experience as a computer professional, where knowledge of the material implementation often didn't enable him to understand what was going on. As he put it:

> [With] respect to computer systems *we already know the answers to all the physiological questions* (we have the source code and wiring diagram), without that necessarily leading to any serious understanding, at the right explanatory level, of "what the program is doing". (B. C. Smith 1996: 148)

He drew the obvious moral: "we should be cautious about any hope that by understanding the brain we will thereby automatically understand the mind" (p. 148).

As for the notion that *Of course we know what it is that computers are doing, because Turing told us*, Smith dismisses that as far too quick. For him, what Silicon Valley treats as computation *is* computation. And the denizens of Silicon Valley, he says, construe "computation" in at least six distinct ways—each of which allows for further distinctions within it. These different meanings are: formal (uninterpreted) symbol manipulation; effective computability; execution of algorithms; digital state machines; information processing; and physically embodied symbol systems. Certainly, one may be tempted to chide the Valley residents for their unclarity—and he does. But for Smith it is what their computers actually do which is important. Turing-computability (the first item on his sixfold list) doesn't capture that.

Smith's mature position—which he dubs "a philosophy of presence"—is another example of interactionist, neo-phenomenological AI. But it's more philosophically sophisticated than most, and much more provocative. Indeed, many professional philosophers (though by no means all, as we'll see) would regard "provocative" as an overly polite description.

Deeply influenced by his technical experience of Silicon Valley's various artefacts, Smith stresses the causal relations between program states (as Sloman had done too), the controllability of various devices, their basis in physics, and their being inescapably situated in the real (and the virtual) world. He sees computation as "inherently participatory", and computers as having "intentional capacities ultimately grounded in *practice*"—analogous to the human practices stressed by Heidegger and Dreyfus (B. C. Smith 1996: 305, 149).

His first publications, in the early 1980s, were on the semantics of programming languages. There, he distinguished Turing-computability from (implemented) computation as such, which *makes things happen* in computers. In other words, computation is a causal concept. He was thinking primarily not of the program's electronic implementation, but of causation in the *virtual* machine.

On that view, the expressions of any given programming language aren't mere empty syntax. They refer (*sic*) to virtual objects and causal processes such as variables (whose values may or may not differ at different times), numbers, procedures, strings, and larger structures containing these. Computer scientists, said Smith, should define their basic terms (such as "variable") accordingly (B. C. Smith 1982). He even designed a novel programming language, and a new semantics for LISP, on this principle.

This early work was seen by some readers as pure computer science—and it was included in an influential collection on knowledge representation in AI (B. C. Smith 1985). But Smith already had two philosophical aims: to put intentional flesh onto dry logicist bones, and to show how self-reflective knowledge is possible. His unorthodox approach whetted some philosophers' appetites, and his promised book was eagerly awaited.

Meanwhile, his ideas were developing in discussion with colleagues at Xerox PARC and Stanford who were working on situational semantics for AI and linguistics (for instance, Stanley Rosenschein, Jon Barwise, and John Perry), and with philosophers such as Haugeland, Adrian Cussins, and Bruno Latour.

His work began to influence the philosophy of cognitive science. It was cited, for instance, in an attempt to escape from the Chinese room (Boden 1988: 247–51); in an argument that non-conceptual content registers the world without articulating it as objects and properties (Cussins 1990); and in a rebuttal of Putnam's claim that computational states are universally realizable (Chrisley 1995). In 1991 Smith indicated his current position in a radical critique of Douglas Lenat's encyclopedia project (see 13.i.c and ii.a):

> [Lenat views] representation as explicit—as a matter of just writing things down. I take it as an inexorably tacit, contextual, embodied faculty, that enables a participatory system to stand in relation to what is distal, in a way that it must constantly coordinate with its underlying physical actions. (B. C. Smith 1991: 285)

But this was a mere taster. Five more years were to pass before the promised book appeared (a brief "introduction" appeared six years after that: B. C. Smith 2002).

Significantly, the title had changed—twice. Originally heralded in 1982 as *Is Computation Formal?*, it was flagged in 1991 as *A View from Somewhere: An Essay on the Foundations of Computation and Intentionality*. It was eventually published (in 1996), however, as *On the Origin of Objects*. In short, the enquiry into computation had become an enquiry into metaphysics.

Smith described the book as an overview, a philosophical "story stripped of its computational heritage" (1996, p. x). What he meant by that was that (like Chomsky in the 1950s: Chapter 9.vi.a) he was holding back most of the technical research that led to his ideas. However, four foundational volumes on *The Origins of Computation and Intentionality* were announced as "in preparation", and (again, like Chomsky) drafts were already informally available.

Now convinced that one must understand metaphysics to understand computation (and vice versa), Smith had developed a novel approach to the nature of objects, individuation, particularity, subjectivity, and meaning. All these matters, he argued, are inescapable if one wants to say *what computers actually do*.

On his view, neither physical objects nor intentional subjects are metaphysically given (as they were for Descartes). They all arise from the "participatory engagement" of distinguishable regions of the metaphysically basic dynamic flux. This flux, described as "riotous fomenting fields" (1996: 281), is the subject matter of field-theoretic physics, and—having no objects—involves neither individuality nor particularity. Objects emerge, or are constructed, as a result of dynamic participatory relations. Smith used the analogy of an English garden, whose order-in-wildness has to be both constructed and continuously maintained (p. ix).

So where others were broadening *Dasein* from humans to animals (see Section viii.b), Smith broadened constructive dynamical interaction from animals to rocks, and even to atoms. All these, he argued, are regions of the flux which achieve particularity and identity through participatory activity. But only some involve *minds*.

Intentionality is at base a form of registration (alias representation: cf. Bennett 1976) that requires a relatively high degree of disconnectedness, or autonomy, as well as connectedness. (The neural emulators and other types of representation discussed in Chapter 14.viii exemplify various combinations of connectedness/disconnectedness.) This type of engagement gives rise to subjectivity and objectivity alike. And its participatory aspect can't be escaped:

> [One] of the most important facts about the inherently participatory picture of registration being painted here is that the form of objectivity available to it cannot be achieved by mistakenly trying more and more to completely disconnect from the world, in a vain attempt to achieve the infamous view from nowhere [Nagel 1986]. On the contrary, the ability to register—*the ability to make the world present, and to be present in the world*, which is after all what this is a theory of—requires that one inhabit one's particular place in the deictic flux, and participate appropriately in the enmeshing web of practices, so as to sustain the kinds of coordination that make the world come into focus with at least a degree of stability and clarity. (pp. 305–6)

In other words, 100 per cent disconnection—pure objective contemplation—is neither possible nor useful: Smith spoke of "the utter futility [that results] when one pulls word and world too far apart" (p. 306).

Smith's third-style position on computation was even more evident now than it had been in the early 1980s. He explicitly denied any fundamental metaphysical distinction between intentionality in human beings and computers:

> The constitutive patterns of partial connection and partial disconnection, of interwoven separation and engagement, while in detail so infinitely various as to defy description, nevertheless reveal regularities across a wide range of cases—from high-level political struggles for autonomy to simple error-correction regimens in low-level computer circuitry. (pp. 347–8)

One's mind can't fail to boggle on reading Smith's book. Quite apart from specific claims such as those quoted above, his overall philosophical agenda—to reconcile the "opposing views of the nature of philosophy" outlined in Section vi.b, above—couldn't be more ambitious. In a recent paper intended as an "introduction" to his book, Smith says as much himself:

> For sheer ambition, physics does not hold a candle to computer or cognitive . . . science. Hawking and Weinberg are wrong. It is we, not the physicists, who must develop a theory of everything. (B. C. Smith 2002: 53)

And "everything", here, really does mean everything. In his book, Smith claims to have retained the major insights of both Continental and empirical–analytic traditions, without any of their problematic ontological assumptions. He also claims that his metaphysics gives us norms as well as facts, and that his account of objectivity can be "ultimately construed as a way of *living right*, rather than merely as a way of speaking truthfully" (1996: 108). (Compare Morris on the good and the true: Section viii.a, above.)

As one might expect, Smith's readers were mostly bemused. And even those who appreciated his effort were often highly ambivalent. For instance, the Washington University computer scientist Ronald Loui, who reviewed the book in *Artificial Intelligence*, described it thus:

> [It is] an important book, even a beautiful book. It reasserts its author as one of the deepest and [most] erudite thinkers of computing. It is also, to this reviewer, an intellectually uninteresting book and thoroughly frustrating to read. (Loui 1998: 353)

After listing some of the "frustrations", which included Smith's decision to spend so much time on metaphysics ("I would have demanded an apology rather than an admission": Loui 1998: 353), he continued:

> Along the way, gems are dropped that reflect an incredibly rich understanding of computing. I consider these gems to be more valuable than the details of the main line of Smith's investigation. . . . *If Brian Smith were not paralysed by metaphysical problems*, he could deliver the seminal work on the philosophy of computing just by collecting and displaying these gems of his insight. (Loui 1998: 354; italics added)

Nevertheless, *On the Origin of Objects* was published carrying high praise from philosophers and computer scientists alike. Haugeland, for instance, saw not paralysis but fundamental, all-engrossing, metaphysical insight. He was quoted on the dust jacket as saying: "Smith recreates our understanding of objects essentially from scratch—and changes, I think, everything."

As remarked at the outset of this chapter, however, philosophical problems don't get solved in a hurry. One shouldn't expect that Smith's highly unorthodox metaphysics would—or will—convince everyone. Some may disdain its 'engineering' origins in his practical experience of computing (see Chapter 2.i.a). Even setting such snobbery aside, however, agreement won't come easily.

Phenomenologists won't readily endorse his use of field-theoretic physics to arrive at conclusions largely similar to theirs. For as we've seen, they believe that a naturalistic philosophy of mind is impossible. Similarly, Searle attacks Smith's ascriptions of intentionality to computation, arguing that syntax (pattern) isn't really "in the physics" but is "observer-relative"—so that Smith's work provides no escape from the Chinese room (Searle 1990*a*; 1992: 209).

Even those who—like Smith himself—reject formalism while favouring a mind-as-machine credo may disagree with him in various ways. My own view, for instance, is that he helped himself to the "dynamic flux"—his version of Kant's noumenal world—without proper licence. He claims that he's pulled this concept up by its own bootstraps (in the final sixty pages), to form a "constructivist" metaphysics of objects and intentionality (cf. also pp. 188–9). But it seems to me that he begs this fundamental philosophical question instead of answering it.

In addition, many readers will be bemused—perhaps even repelled—by the extraordinary mix of dry argumentation and intoxicating (or embarrassing) rhetoric. The rhetoric is evident in his promise to do the metaphysical bootstrapping just mentioned:

> I have been assuming an underlying space of physical feature fields—fields that were never paid for, and which anyway are about to be discarded . . . The fields will go. But the patterns will remain: restless patterns of stabilization and coordination, of invention and description and activity and design, of struggle and submission and conquest and peace, sometimes collaborative, sometimes singular. All these participatory activities arise out of ineffable connection and subside back into it, at a different place or different time or under different circumstances, often benefitting from the perspective of abstraction and registration, but never escaping from the located, the directed, and the exquisitely particular. (1996: 312–13)

The crucial point, however, is that it's not only Smith's *rhetorical style* that's surprising. (Surprising, that is, to analytic philosophers and computer scientists. Devotees of Heideggerian metaphysics would be less bemused, for he too constantly appealed to freedom, commitment, poetry, and love.) In short, Smith's *account of computation* is very different indeed from what most philosophers expect.

Different or not, Smith is adamant that understanding computation is crucial for cognitive science:

> It is sobering, in retrospect, to realize that *our preoccupation with the fact that computers are computational* has been the major theoretical block in the way of understanding how important computers are. They are computational, of course; that much is tautological. But only when we *let go of the conceit that that fact is theoretically important* . . . will we finally be able to see, without distraction, and thereby, perhaps, at least partially to understand, how a structured lump of clay can sit up and think. (B. C. Smith 2002: 52–3)

f. The moral of the story

The general moral, here, is that the rise of third-style discussions (such as those of Sloman, Agre, and Smith) strongly suggests that we don't yet know what computation—understood intuitively as what computers do—*is*. In particular, we can't assume without question that it's utterly distinct from intentionality. It follows—if the mind is a computational machine—that we don't yet know what the mind is, either.

The core thesis of cognitive science—that mental processes are computational—should therefore be interpreted transparently, not opaquely (Chrisley 1999). In other words, the claim isn't that mind can be explained by our current ideas about computation, but that it's explicable by *whatever theory turns out to be the best account of what computers do*.

"Computers", as suggested above, may turn out to include hypercomputers, computing functions that aren't Turing-computable. So Copeland (2000, 2002*b*) has argued for much the same position as Chrisley, in opposing "wide" to "narrow" mechanism. Narrow mechanism sees the mind as some sort of Turing machine, whereas wide mechanism allows for types of computation that aren't Turing-computational—but which are executed in *machines* nonetheless. If wide mechanism is true, then classical functionalism bites the dust. But mind-as-machine doesn't.

There's a clear analogy, here, with physicalism in metaphysics. Physicalists don't normally tie themselves down to today's physics, but have in mind *whatever turns out to be the best account of the physical world*. The best physics may turn out to be surprising (yet again). And, as Chrisley points out, the best theory of computation may turn out to be surprising too.

It's clear from the rise of the second (inclusive) and third (causal/intentional) senses discussed above that the concept of computation has been a moving target since its introduction in the 1930s. Turing's definition is still the clearest, to be sure, and is understandably still cited by philosophers. But even Searle now allows (largely because of Smith) that the mathematics and electronics haven't been clearly connected, and that "there is little theoretical agreement among the practitioners on such absolutely fundamental questions as . . . What exactly is a computational process?" (1992: 205).

In sum: the still widespread notion that philosophers can give—or just assume—a quick definition of computation, and then get on to the *really* interesting philosophical issues, takes far too much for granted.

16.x. What's Life Got To Do With It?

All the minds we know about are found in living things. But why?

* Couldn't there be mind and meaning without life?
* And what is life?
* Given that it involves self-organization, just what sort of self-organization is it?
* Must it involve evolution, for instance?
* Is embodiment essential? And what's that? Is mere physicality enough for embodiment?
* Or is metabolism needed too—and, again, what's that?
* What's the link, if any, between metabolism and mind?
* Could life be generated artificially, and if not why not?
* Is strong (i.e. virtual) A-Life impossible in principle?
* If so, does it follow that strong AI is an illusion too?

These questions took a long time to surface in cognitive science. Or perhaps one should rather say to *resurface*. For in the very early days, life and mind were both discussed—and were treated largely on a par.

The cyberneticians of the 1940s applied their theories of self-controlling machines to both living organization and purpose (see 4.v–vii). Life wasn't regarded as necessary for teleology, for self-guided missiles were said to exemplify goal seeking (Rosenblueth *et al.* 1943). However, since life and mind were supposed to consist in fundamentally similar principles of control, it didn't seem surprising that all the minds we know about are grounded in living things.

McCulloch had been a member of this life-and-mind movement since its inception in the 1930s. But he'd been deeply interested in logical analyses of language even before then (see 4.iii.c). In the event, his paper of 1943 turned attention away from life in favour of mind.

Control by feedback gave way to logic-based computation. The NewFAI computer-modelling community saw mind and meaning as matters for propositional logic, their origin in adaptive behaviour and living embodiment being downplayed. Even unreconstructed cyberneticians now sometimes spoke of mind without mentioning life: when William Ross Ashby wrote a paper on cybernetics for *Mind*, he concentrated on defending materialism against mind–body dualism, not on exploring the philosophy of self-organization (Ashby 1947).

In short, by mid-century the link between life and mind—though still widely accepted, even taken for granted—wasn't explicitly considered. The people on the NewFAI side of the emerging cybernetics/symbolic schism (4.ix) ignored life and spoke only of mind. The people on the other side said very little about mental phenomena beyond perception and goal seeking: self-reflection and reasoning were mostly ignored.

That life/mind split within cognitive science lasted for several decades. Much the same was true in philosophy, especially the analytic variety. It's still the case, in 2006, that the link between life and mind is ignored (beyond mere lip-service) by the mainstream. However, these issues are now arousing interest—largely as a result of the rise of A-Life.

a. Life in the background

Even in the early 1960s, there were a few exceptions to what I've just said. Most important, with respect to their historical role in cognitive science, were the people impressed by cybernetics who were developing holistic philosophies—and computer models—of life and/or mind (15.vi–vii).

The most influential of these (all discussed later in this section) were to be the Chilean neuroscientist Maturana (with Varela and Milan Zelený), whose similarity to Dreyfus was mentioned in Section vii.b above, and the philosopher Howard Pattee, with his students Robert Rosen and Peter Cariani. Both Maturana and Pattee were working on the concept of life from the 1960s. Although they had some early disciples, their ideas didn't become widely known until the 1990s. By the new millennium, however, Maturana and Varela's ideas had been published in semi-popular form, and an entire issue of the journal *BioSystems* was devoted to Pattee's work and influence (Rocha 2001).

All of the above were either scientists (like Maturana) or philosophers very close to science, so close that they sometimes got involved in scientific work (Pattee, for instance). None were "pure" philosophers.

In general, the mid-century philosophers who were interested in the puzzle of life-and-mind came from the Continental, not the analytic, side of the fence. As a result, they were largely ignored by the scientific community. Moreover, they were exceptions even within their own, neo-Kantian, tradition. To be sure, phenomenologists in general took the human being's embodied living-in-the-world as philosophically basic. And Wittgenstein saw language as part of our "natural history", declaring: "What has to be accepted, the given, is—so one could say—*forms of life*" (1953: 226). Most of them had scant interest, however, in what biologists mean by life—which includes oak trees and barnacles, as well as human beings.

One exception to this was the existentialist theologian Hans Jonas (1903–93), who developed a new philosophy of biology in the 1950s. Unlike Maturana and Varela, he wasn't interested in biology for its own sake, but as an aspect of what he saw as the disastrous cultural denouement of Descartes's materialism (Jonas 1966: 58–63).

An ex-pupil of Heidegger, Jonas fled Germany for England when the German Association for the Blind expelled its Jewish members (Jonas 1966, p. xii). From the mid-1950s to the mid-1970s he worked at the New School of Social Research, in New York. Despite still regarding Heidegger as "the most profound and . . . important [proponent] of existential philosophy" (p. 229), he rejected his philosophical dichotomy between humans and other living things—and his pro-Nazi sympathies, too. He explained the latter in terms of "the absolute formalism of [Heidegger's] philosophy of decision", in which "not *for* what or *against* what one resolves oneself, but *that* one resolves oneself becomes the signature of authentic Dasein" (Jonas 1990: 200). And that, in turn, he saw as a result of the stripping-away of value from nature, its "spiritual denudation" by Descartes and modern science (Jonas 1966: 232).

It was in response to this disenchantment of nature that Jonas published various essays on life in the post-war years, and collected them as *The Phenomenon of Life* in 1966. They outlined a framework for a biology that would admit value as an intrinsic feature of life in general. ("Outlined" and "framework" are important here: he discussed almost no specific examples.)

Embodiment, and in particular metabolism, was seen by Jonas as philosophically crucial (1966: 64–91). Not only was life essential for the emergence of mind (pp. 99–107), but *all* self-organized matter was, in a sense, ensouled—though where Maturana and Varela spoke of life as involving *cognition*, Jonas spoke of life as involving *self-concern*. (He lauded Heidegger for having "shattered the entire quasi-optical model of a primarily *cognitive* consciousness, focusing instead on the wilful, striving, feeble, and mortal ego"—1996: 44.) As he put it:

> One way of interpreting [the ascending scale of life] is in terms of scope and distinctness of experience, of rising degrees of world perception. . . . Another way, concurrent with the grades of perception, is in terms of progressive freedom of action. . . . [One] aspect of the ascending scale is that in its stages the "mirroring" of the world becomes ever more distinct and self-rewarding, beginning with the most obscure sensation somewhere on the lowest rungs of animality, even with the most elementary stimulation of organic irritability as such, in which somehow already otherness, world, and object are germinally "experienced," that is, made subjective, and responded to.
>
> [We spoke, above, of freedom.] One expects to encounter the term in the area of mind and will, and not before: but if mind is prefigured in the organic from the beginning, then freedom is. And indeed our contention is that *even metabolism, the basic level of all organic existence, exhibits it: that it is itself the first form of freedom*. (Jonas 1966: 2–3; italics added)

Even in "the blind automatism of the chemistry carried on in the depths of our bodies", there is "a principle of freedom . . . foreign to suns, planets, and atoms". For living organisms have a special type of identity and continuity: a stable dynamic form made of an ever-changing material substrate. In short, "mind is prefigured in organic existence as such" (p. 5). Plants, too, have "metabolic needs", although they stand in an "immediate" relationship to their environment. And metabolism is the necessary base of all forms of mediation: perception, motility (action), emotion, and—ultimately—conscious imagination and self-reflection. (These phenomena emerge as a result of evolution: Darwin, *despite* his materialist assumptions, had enabled us to understand this: pp. 38–58.) Life and mind are ontologically inseparable: "the organic even in its lowest forms prefigures mind, and . . . mind even on its highest reaches remains part of the organic" (p. 1).

In other words, Jonas was offering an answer to the question of *why* all the minds we know about are found in living things. At the same time, he was offering an answer to the question of *what life is*.

He explicitly refused to speculate about the origins of life (p. 4), even though this was already being discussed by biochemists (Chapter 15.x.b). His interests were ontological, not scientific: metabolism was "the break-through of being" from mere physicality to "the indefinite range of possibilities which hence stretches to the farthest reach of subjective life" (p. 3). (Accordingly, he retained a Heideggerian hostility to technological theories/analogies of life or mind: pp. 108–26.)

The book was reissued (by several different publishers) in 1979, 1982, and 2001, and translated into German in 1994. So one can't say that it was wholly ignored. Indeed, because of his stress on the intrinsic value of life and humankind's responsibility towards it, Jonas's work—especially his volume on ethics (Jonas 1984)—has recently become better known thanks to the environmentalist movement.

In the 1950s and 1960s, however, his philosophy of biology was ignored by analytical philosophers and mainstream biologists alike. (And by cyberneticists too, whose analysis of living purpose and rocket teleology *in the very same terms* he'd rejected as "spurious and mainly verbal"—1966: 111.) The same was true of Maturana and Varela's early work, but they have now earned a clear, if still marginal, place in the history of cognitive science. Jonas has not (but see Di Paolo forthcoming). He's relevant here not as a protagonist in that historical drama, but as a mid-century philosopher who tried to argue the case that mind requires life, rather than taking it for granted.

Another philosopher who'd done this was Henri Bergson (Chapter 2.vii.c). By the end of the twentieth century, Bergson's views on "creative evolution" were being revived in some philosophical circles—especially in "process" philosophy/theology (Sibley and Gunter 1978; Papanicolaou and Gunter 1987). This emulated Bergson alongside the even greater hero Alfred North Whitehead (4.iii.b). But some unorthodox scientists were taking an interest too. The physicist–philosopher Henri Bortoft (1996) put Bergson second only to Goethe as a precursor of current dynamical theories in science and philosophy (see 2.vii.c). And a few neo-Bergsonian philosophers even tried to relate his ideas to cognitive science and/or A-Life.

For example, Gilles Deleuze (1925–) revived certain aspects of Bergson's philosophy by stating them in terms of ideas about dynamical systems (Deleuze 1966/1988). I'm saying that at second hand, I must confess, for Deleuze himself is nigh unreadable by anyone more accustomed to analytical philosophy. Much as Richard Montague's work couldn't spread among linguists until a clear account of it had been provided by Barbara Partee (see 9.ix.c), so Deleuze's has been made accessible to cognitive scientists by his expositor Manuel DeLanda (2002).

Although he rejected Bergson's dualist interpretation of *élan vital*, Deleuze offered a "re-enchantment" of matter, nevertheless. He even (confusingly) used the term "spirituality" in talking about matter and life. But this wasn't intended as transcendent spirituality: rather, it referred to the abstract principles of self-organization, and the structured spaces of possibilities, that are inherent in matter/energy.

He saw matter not as inert stuff subject to external influences, but as the source of formative material *processes*. A soap-bubble, for instance, actively minimizes the surface tension at every point (it dynamically "computes" its own shape). The dynamical structures generated by matter were said to be constrained, in part, by abstract topological principles describing connectivities and attractors of various kinds (compare Stuart Kauffman's work on NK networks, and Randall Beer's on CTRNs: 15.ix.b and xi.b).

On this view, life was a special case of matter, and mind a special case of life. It followed that there's no *special* difficulty about giving a naturalistic, even a materialistic, account of mind or intentionality, even though spelling one out in detail may be highly challenging.

However, these intriguing analogies weren't helpful in furthering scientific understanding. (Or anyway, they haven't been helpful yet: DeLanda's relatively accessible version of Deleuze appeared only two years ago, and it remains to be seen whether many scientists will take it up.) Admittedly, the ever-maverick neuroscientist Karl Pribram (1987)—accused in the early 1960s of actually *believing* the MGP manifesto (6.iv.c)—described the cerebral basis of some cognitive processes in Bergsonian terms. But that's not to say that he *used* Bergson's ideas to make discoveries which otherwise would not have been made. Rather, he pointed out an analogy between Bergson's views on memory and his own (long-standing) holographic/holonomic theory (cf. 12.v.c).

Cognitive scientists who weren't already sympathetic to dynamical systems and/or Kauffman's approach to A-Life weren't likely to be interested in Bergson's work at all, even if they encountered it. And that was unlikely: as remarked in Chapter 2.vii.c, it had been more or less forgotten since mid-century—especially by philosophers of an analytic cast of mind.

For over thirty years, then, the concept of life was usually ignored in discussions of mind as machine. To be sure, the psychologist Miller raised the topic—but he immediately dropped it like a hot potato. He was, he said, "unclear" whether epistemic (cognitive) systems should be defined as animate or inanimate. The advantage of defining them as animate was that "we cut artificial intelligence free to develop in its own way, independent of the solutions that organic evolution happens to have produced" (G. A. Miller 1978: 9). (This remark pre-dated the concept of strong A-Life by a decade: clearly, Miller thought it obvious that computers and life are incompatible.) But whether it made "any real difference" in conceptualizing the study of *mental* processes was "unclear".

Most analytic philosophers tacitly assumed some life–mind linkage—which would imply that if computers aren't alive then they aren't psychological systems. They evidently thought this point so obvious that, even when they bothered to state it explicitly, they didn't offer any arguments for it.

Scriven, for instance, confidently declared—without giving reasons—that "Life is itself a necessary condition of consciousness" and that "Robots . . . are composed only of mechanical and electrical parts, and cannot be alive" (Scriven 1953: 233). Lucas hinted at a similar position in his own reply to Turing's 1950 paper (see Section v.a). Geach insisted that AI systems can't have beliefs and intentions because they're "certainly not alive" (Geach 1980: 81). And some, such as Searle (1980, 1992) and Ruth Millikan (1984), explicitly linked intentionality with biology (neurochemistry and evolution, respectively). But even they didn't discuss the nature of life as such.

Two exceptions that proved (i.e. tested) the rule were Putnam's (1964) paper on 'Robots: Machines or Artificially Created Life?' and Geoffrey Simons's (1983) book *Are Computers Alive?*

Despite its title, Putnam's paper focused mainly not on life, but on consciousness. At one point, Putnam endorsed Ziff's claim that it's an "undoubted fact" that if a robot isn't alive then it can't be conscious. But he was relying on "the semantical rules of our language", not on any quasi-explanatory relationship between life and mind.

He also said (this time, disagreeing with Ziff) that something which is clearly a mechanism might be alive. Again, however, this was linguistic philosophy in action. Sometimes, Putnam heretically recommended *changes in meaning* due to new scientific data, as he did when countering Malcolm's account of dreaming (Putnam 1962*a*). But in the paper on robots and life, he was talking only about what *current usage* allowed one to say (or imagine) without contradiction. The nearest he got to discussing a substantive claim about life was to scorn the suggestion that the primary difference between a robot and a living organism is the "softness" or "hardness" of the body parts (1964: 691).

Much later, Putnam's paper was discussed at length, and accused of incoherently combining Aristotelian and Cartesian views (Matthews 1977). At the time, however, it didn't prompt philosophical interest in the concept of life.

Simons, writing twenty years after Putnam, used concepts drawn from GOFAI and cybernetics to claim that computers can be *really* alive, and *really* intelligent. He specifically denied that the genesis of the system is relevant to whether it's alive: "A mechanically *assembled* [i.e. not evolved or self-constructed: see below] system may reasonably be regarded as living . . . " (1983: 23). However, his argument was neither deep nor convincing, and (deservedly) attracted little attention.

b. Functionalist approaches to life

With the rise of A-Life in the late 1980s, the nature of life became an inevitable topic for computational research. Inevitable, but in practice not central: most A-Life workers focused on other questions, maintaining a diplomatic silence on this one. Some of their colleagues, however, were more bold.

The relevant discussions were guided by two radically opposed philosophies. (Sounds familiar?—see Section vi.b.) These were functionalism and metabolic holism, a special case of dynamical systems theory.

Functionalism, in this context, is the view that the characteristics of life (see Chapter 15.ii.b) can be described by informational concepts. So self-organization involves the appearance of new levels of order, abstractly defined. Autonomy, emergence, development, adaptation, responsiveness, and evolution concern various types of structure, process, and control. Even reproduction (on this view) can be defined informationally, as self-copying.

The one exception is the concept of metabolism, which concerns not information but energy. Thoroughgoing A-Life functionalists weren't worried by this, as we'll see. But their opponents argued that they should be.

It's often assumed (wrongly) that all A-Life workers are thoroughgoing functionalists. This is largely because Christopher Langton, following John von Neumann's lead, wrote this position into his definition of the field in 1986 (Chapter 15.ii.b and ix).

Moreover, he drew the obvious implication: a licence for strong A-Life. If living self-organization is definable in logical terms, then a virtual "creature" implemented in computer memory that satisfied these abstract criteria—whatever they are—would be genuinely alive. ("Whatever they are", because definitions differed. For instance, Langton suggested including the lambda parameter, Andrew Wuensche the Z-parameter: Chapter 15.viii.a.)

Some A-Life colleagues were quick to join Langton in this claim. Thomas Ray, for instance, declared:

> The intent of [my] work is to synthesize rather than simulate life . . . To state such a goal leads to semantic problems, because life must be defined in a way that does not restrict it to carbon-based forms. It is unlikely that there could be general agreement on such a definition . . . Therefore, I shall simply state my conception of life in its most general sense. I would consider a system to

be living if it is self-replicating, and capable of open-ended evolution [generating] structures and processes that were not designed-in or preconceived by the creator. (Ray 1992: 372)

As we saw in Chapter 15.vi.b, Ray's Tierra system did indeed generate phenomena not designed-in by Ray. These included co-evolving parasites, hyper-parasites, cheaters, and symbionts. Ray's response was a curious combination of modesty and hubris:

[The] results presented here are based on evolution of the first creature that I designed, written in the first instruction set that I designed. Comparison with the [virtual] creatures that have evolved shows that the one I designed is not a particularly clever one . . . It would appear then that it is rather easy to create life. (p. 393)

As for the problematic concept of metabolism, Ray said two things. On the one hand, the computer consumes physical energy too. On the other, the equivalent of metabolism can be functionally defined:

In studying the natural history of synthetic organisms, it is important to recognize that they have a distinct biology due to their non-organic nature. In order to fully appreciate their biology, one must understand the stuff of which they are made. To study the biology of creatures of the RNA world would require an understanding of organic chemistry and the properties of macro-molecules. To understand the biology of digital organisms requires a knowledge of the properties of machine instructions and machine language algorithms. (p. 397)

I will discuss the inoculation of evolution by natural selection into the medium of the digital computer. This is not a physical/chemical medium; it is a logical/informational medium . . . Evolution is then allowed to find the natural forms of living organisms in the artificial medium. These are not models of life, but independent instances of life. (Ray 1994: 179)

For some broadly functionalist A-Life scientists, this was a step too far—and *much* too far for most philosophers (e.g. Harnad 1994; Olson 1997). Those A-Life colleagues were content to interpret most of the characteristics of life in informational terms—but not metabolism, which is irredeemably physical. However, since they defined metabolism as mere energy dependency, their rejection of Ray's position was intuitive rather than strongly argued (see below).

Many A-Life colleagues simply avoided the question, by way of the "diplomatic silence" mentioned above. They were interested in studying specific aspects of life, such as evolution or flocking, not in discussing its general nature. They were even less interested in considering the "strong A-Life" scenarios sketched by Langton and Ray—and later by Steve Grand (1958–).

Grand's first claim to fame was that he designed the hugely popular computer game Creatures. This swept the world in the early 1990s (see Chapter 13.vi.d), and was still being widely celebrated—for broadly counter-cultural reasons—in the new century (Kember 2003: 91–105).

Creatures enabled the user to evolve unusually sophisticated computer creatures (Grand and Cliff 1998). Their neural-network brains supported simple learning, and included 'neuromodulators' as well as several types of 'neurone'. The creatures also had a simulated biochemistry, with the potential to model a large number of metabolic and hormonal functions, from digestion to ovulation. As a piece of lifelike software engineering, it was way beyond the general state of the art when it appeared, and is still impressive. Indeed, it could conceivably be used as a powerful test bed for AI models of motivation and emotion such as those discussed in Chapter 7.i.e–f (Boden 2000*b*).

Grand's current technical aim is to build an "imaginative" robot called Lucy, whose intelligence will emerge "naturally"—and holistically—from its 100,000-neurone hardware (Whitby and Grand 2001). As he points out, this attempt to build Dennett's (1978*c*) "whole iguana" is very different from MIT's Cog project, with which Dennett himself was involved (see 15.vii.a).

The Cog robot was carefully designed module by module, bits of its "intelligence" being successively bolted on. Grand, by contrast, wants an already integrated intelligence to emerge from a relatively unorganized base. Rather than providing Lucy's brain with spatial maps or orientation columns, for instance, he hopes that these would emerge spontaneously (much as ocular dominance columns arose in the work of Christoph von der Malsburg and Ralph Linsker: see Chapter 14.vi.b and ix.a). And the robot would learn to perform "voluntary" actions by associating the image (representation, model) of the desired action with the muscle movements required to achieve it (compare Marr's theory of the cerebellum: 14.v.c).

The Lucy project is startlingly ambitious—I'm tempted to say, utterly impracticable. But the A-Life expert David Cliff (personal communication) believed Creatures to be utterly impracticable too, when first consulted by the games company to whom Grand had offered it. Given what Grand had told them it could do, it must—so Cliff thought—be either hype and/or a superficial con trick, carefully tailored to present a convincing 'demonstration'. (Even the impressive SHRDLU, you'll remember, could handle only the one conversation without tripping over its toes: 9.xi.b.) And the fact that it had been two-finger-typed on Grand's bedroom computer wasn't promising. Not until he got down into the machine code was Cliff convinced—at which point he suggested how it could be improved still further, using some of the ideas discussed in Chapters 7.i.f and 15.vi–ix.

Grand didn't know about those ideas already, because he's an autodidact. As such, he's undeterred by received academic opinion. And he's a highly creative computer engineer, who's already designed one apparently impossible system that does just what it was intended to do. He's thus in an entirely different class from the self-publicizing roboticists Kevin Warwick and Hugo de Garis, on whose 'research'—technical no less than philosophical—I forbear to comment, for fear of scorching the page.

I wouldn't bet a large sum of money on Lucy. And I don't agree with those cultural commentators who claim that Grand is "one of the 18 scientists most likely to revolutionise our lives in the coming century" (ICA 2000). Nevertheless, as Richard Dawkins has remarked, "If anybody can pull off a spectacular breakthrough, it'll probably be him" (Whitby and Grand 2001: 13). (For the most recent status report on Grand's progress, see his web site at <http://cyberlife-research.com>.)

At the turn of the century, Grand (2000, 2003) made a number of highly provocative claims about the philosophical significance of his own past and future work. He sees his virtual creatures as more than merely *lifelike*: they are "sort of alive", or even "a sort of life". When challenged on this point, he insists (personal communication). Grand is an autodidact in philosophy too, but here there's no good reason to give him the benefit of the doubt. Whereas Creatures (considered as technology) clearly does what he said it would do, his philosophical arguments are challengeable—and, in my view, as mistaken as Ray's. Strong A-Life is no more plausible in Creatures than in Tierra—and even Grand's predicted robot Lucy wouldn't count as genuinely *alive* (see the discussion of metabolism, below).

Where the general public were concerned, Lucy made something of a splash. Although it must be said—and often is said, by other roboticists—that if Grand hadn't fitted a furry gorilla-face onto the head, and if he'd called it Robot 37 instead of Lucy, people wouldn't have been quite so interested. (Similarly, the young Minsky's robot arm aroused no attention until he put a shirtsleeve on it: see 1.iii.h.) Quite apart from the overexcitement of the journalists (the same old story!), a number of commentators have picked up on it as an expression of wider cultural concerns.

The anthropologist Lucy (*sic*!) Suchman, for example, who cast doubt on GOFAI planning some twenty years ago and focused on *human–machine communication* soon after that (13.iii.b), described her robotic namesake as one among the disturbing category of the "almost human" (Castañeda and Suchman forthcoming; cf. Suchman 2004).

Besides the familiar anthropologists' fare of totems and other things "doing duty as persons", these include children, non-human primates, and AI/A-Life machines. The cultural status of children has been a focus of commentary at least since Jean-Jacques Rousseau (1712–78), and twentieth-century developmental psychology has helped fuel this fire. As for primates, advances in field ethology have led to the culturally problematic Great Ape programme (7.vi.f). The eighteenth-century automata (2.i.b) challenged contemporary notions of the person (Riskin 2003). Now, as Suchman pointed out, various actual and imaginary AI projects are exciting comment not only in the philosophy of mind but in our wider culture too.

Lucy (which Suchman discusses at length) is only one example of the "almost human" produced by AI/A-Life. Cog, and especially its successor Kismet (see 13.vi.d), are others. The feminist philosopher Evelyn Fox Keller (forthcoming), for instance, sees some "serious anxieties" with respect to providing Kismet and the like with facial expressions that reliably elicit emotional reactions in human viewers. (She's particularly worried by the plan to use robots like Brian Scasselatti's Nico to *test* theories in human developmental psychology.) Still other almost-humans—all media darlings in their day—include ELIZA, expert systems, AI agents ("softbots"), VR avatars, Turing's computer conversationalist, Stanley Kubrick's HAL, and Steven Spielberg's David.

The behaviour—and man–machine interactions—of many of these systems is far more human-like than Lucy's is. But because Grand, besides providing the superficial furry face, speaks of *life* as well as *mind*, his work aroused more outside comment than most. In addition, his A-Life system is not virtual/intellectual (as softbots are) but *embodied*—or at least, *material*. It's therefore of interest to those commentators, including phenomenologists and many feminist philosophers, for whom the downplaying of embodiment in the analytic–scientific tradition has been a fundamental mistake (Haraway 1997: 186, 302–3, and *passim*; Kember 2003: 105–15, 198 ff., and *passim*).

Suchman (like Haraway, and also Clark: vii.d, above) takes personhood, in whatever culture, to be constituted not by an individual person-in-the-mind but by the nexus of social relations and interactions available. On that view, the cultural status of robots, and other AI/A-Life systems, is determined less by their seeming intelligence than by the pattern of interactions we choose to engage in with them. But the influence is reciprocal: in so far as we do engage with them, we modify our own self-image in various subtle ways (cf. 13.vi.d).

c. The philosophy of autopoiesis

Some A-Life researchers dismissed all these science-fictional scenarios *because they were fundamentally opposed to functionalism in the first place*. Among these were the proponents of Maturana's theory of "autopoiesis".

This was perhaps the best-developed philosophy of metabolic holism. (The competing candidate is the work of Pattee's group: see below.) It even inspired several computer models of biochemical autopoiesis (Zelený 1977; Zelený *et al*. 1989), and a wide range of work in A-Life (McMullin 2004). This included work in "wet" A-Life, in which biochemical autopoiesis *as such* was studied too (see Chapter 15.x.b; Bachman *et al*. 1990; Walde *et al*. 1994).

Originated in the 1960s, Maturana's theory was strongly influenced by Heinz von Foerster's cybernetics. Despite the fact that an English translation was published over a quarter-century ago in the highly respected Boston Studies in the Philosophy of Science (Maturana and Varela 1972/1980), it has remained a minority taste. It's clear from my personal acquaintance that many philosophers have never even heard of it.

One reason, no doubt, is its rebarbative vocabulary and unrelenting abstraction. Also, it has some highly counter-intuitive implications, as we'll see. Nevertheless, it offers a principled way of grounding mind in life. Rather than arguing (like Searle) that neuroprotein happens to cause intentionality, as chlorophyll happens to cause photosynthesis, this view grounds intentional categories in an essentially autopoietic biology.

For Maturana and Varela (1972/1980), life is "autopoiesis in the physical space". Autopoiesis in general is defined as the continuous self-production of an autonomous entity. As they put it (you're advised to take a deep breath here):

> An autopoietic machine is a machine organized (defined as a unity) as a network of processes of production (transformation and destruction) of components that produces the components which: (i) through their interactions and transformations continuously regenerate the network of processes (relations) that produced them; and (ii) constitute it (the machine) as a concrete unity in the space in which they (the components) exist by specifying the topological domain of its realization as such a network. (Maturana and Varela 1972/1980: 79)

Or more colloquially, an autopoietic system "pulls itself up by its own bootstraps and becomes distinct from its environment through its own dynamics, in such a way that both things are inseparable". This type of self-organization can occur in the world of human communication, in which case we have some kind of social institution (cf. Teubner 1987, 1993). But when it happens in the physical world, we have a living organism.

The autopoiesis concerned here is a special case of homeostasis (see Chapter 4.v.c), where what's preserved isn't one feature, such as blood temperature, but the organization of the system as a unitary whole. This requires the self-creation of a unitary physical system, by the spontaneous formation of a boundary—at base, the cell membrane—and the continuous generation and maintenance of the body's own components.

For Maturana and Varela, *body* and *embodiment* are autopoietic categories. So too are *cognition*, *communication*, *meaning*, and *language*, all of which they defined in terms of the interactions of living things. In the more accessible version of their theory that appeared around 1990 (Maturana and Varela 1987, 1992), and in Varela's book co-authored with cognitive psychologists (Varela *et al.* 1991), they focused on human language, understanding, society, and consciousness—all described as necessarily rooted in our biology.

In fact, they were overly liberal with their ascriptions of intentionality (Boden 2001), for they declared that "Living systems are cognitive systems, and living as a process is a process of cognition" (1972/1980: 13). Taken seriously, this extends knowledge even to algae and oak trees. One can—and should—express the idea that algae and acorns are pre-adapted to their environment without using the concept of *knowledge*. Such over-liberality was an occupational hazard for cyberneticians: as we saw in Chapter 4.v.e, Gregory Bateson had similarly attributed *knowledge* to redwood forests, and *mind* to whirlpools and oscillating electrical circuits.

From the autopoietic viewpoint, both strong A-Life and strong AI are absurdities. For computers aren't autopoietic systems. Even self-assembling robots, if assembled from manufactured parts as opposed to being self-organized by some alien biochemistry, wouldn't be alive. (Nor would they have bodies.) Consequently, robotic intelligence is impossible too.

Autopoietic theory is a special case of the general (anti-functionalist) position that metabolism is essential for life. Believers in strong A-Life (such as Ray, quoted above), when confronted with this view, typically pointed out that computers consume energy too. They sometimes added that the "physics and chemistry" of their virtual creatures is constituted by the computer's memory and operating system.

A number of philosophers, some of whom weren't committed to autopoietic theory, replied that metabolism is more than mere energy dependency. Rather, it's the self-production and self-maintenance of the physical body by energy budgeting, involving self-equilibrating energy exchanges of some *necessary* complexity (Pattee 1989; Cariani 1992; Sober 1992; Boden 1999). They argued that strong A-Life is possible only if virtual systems can metabolize *in the sense just given*, or if metabolism is inessential for life. But neither alternative is tenable.

Living 'tin-can' robots are also excluded by this approach. Only robots powered by complex biochemical cycles of synthesis and breakdown would be truly alive, and truly embodied. This is the basis of the intuition scorned by Putnam, that "softness" and "hardness" matter (see above).

Elliott Sober (1948–) cited other biological properties, besides metabolism, in arguing against strong A-Life (Sober 1992). Digestion and predation (for example) each relate an organism to something outside itself, where that "something" is essentially physical. Both can be realized in multiple ways (defined by biochemistry and behaviour), but in every instance some physical organism has to interact with—hunt, eat, transform—another. Like metabolism itself, these features can be usefully simulated by A-life models. But they can't be replicated, so strong A-Life is impossible.

Sober's argument would be endorsed by autopoietic theorists. But to see metabolism as essential for life isn't necessarily to accept autopoietic philosophy. For this has some surprising implications, which many people reject. One was noted above, namely, the conflation of life and cognition. Another was remarked in Chapter 15.viii.b: the embargo on terms such as *input*, *output*, *function*, *feature-detector*, and *representation*. Two more concern features often listed in definitions of life: reproduction and evolution.

Maturana and Varela's claim that the formation of the cell membrane is *the* fundamental phenomenon of biology, and that life involves the "total subordination of [all the processes of change within] the system to the maintenance of its unity" (1972/1980: 97), implied that reproduction isn't essential for life. For them, this process is not (as functionalists claim) informational self-copying, but the formation of new autopoietic unities from previous ones. It follows that life is prior to reproduction (pp. 105–7). This wasn't a merely conceptual point, but a substantive biological hypothesis: that the earliest living organisms needn't have been able to reproduce (Boden 2000*b*).

Evolution, also, was seen by them as inessential, because it requires reproduction. (Inessential for life, but not for what's normally regarded as knowledge: they admitted that only evolution can generate the complex organisms typically credited with cognition.)

This conclusion, though unusual, is less controversial. For, *pace* Ray, and many theoretical biologists too (e.g. Maynard-Smith 1996), there are three independent arguments against defining life in terms of evolution. First, populations, not individual organisms, would be paradigm cases of life. Second, creationism would be conceptually incoherent, not just false. And third, a population in evolutionary equilibrium wouldn't count as alive.

d. Evolution, life, and mind

Some philosophers of A-Life, nevertheless, took evolution (together with metabolism) to be the sort of self-organization which characterizes life. Pattee was an early example, followed by his students Rosen (1985, 1991) and Cariani (1992, 1997). He'd modelled co-evolution in the 1960s (see Chapter 15.vi.a). Subsequently, he focused on the emergence of new phenotypic structures and functions.

A crucial example, for Pattee (1985), was novel types of "measurement", or classification. These were understood as ranging from enzyme activity to sensory perception—as in the evolution of new sensory organs (see Chapters 4.v.e and 15.vi.d). Pattee's concept of measurement was intriguingly similar to Smith's "participatory registration"—but, unlike Smith, he retained the first definition of computation distinguished in Section ix.a. So he specifically dismissed strong A-Life, arguing that measurement requires physical interaction, which can't be realized by formal computational systems. A fortiori, no novel biological functions can emerge in formal evolutionary systems (15.vi.d). He did allow, however, that "weak" A-Life modelling (simulation) could help clarify central biological and psychological concepts.

In the 1990s, another philosopher of A-Life argued that evolution is an essential criterion. Mark Bedau (1954–) explicitly accepted the three counter-intuitive implications mentioned above, because of the explanatory power gained by defining life in evolutionary terms (Bedau 1996). And this explanatory potential, he said, was augmented by A-Life. In presenting his account of "supple adaptation" (alias evolution), he argued that A-Life modelling can deepen our understanding of life as such, because it helps us to study evolution in dynamic and quantitative terms. Moreover, he extended his evolutionary argument from life to mind (Bedau 1999, in preparation).

A-Life philosophers weren't the only ones to link life and mind. Others, too, had grounded knowledge and meaning in biological evolution. Dennett had sketched an evolutionary account of meaning in *Content and Consciousness*, although philosophers then were more interested in other aspects of his work (see Section iv.a). By the mid-1980s, however, two influential examples of teleological or evolutionary semantics had appeared.

The philosopher of science David Papineau (1947–) argued that the content of beliefs depends on how they guide actions to satisfy desires, whose content is basically determined by natural selection (Papineau 1984, 1987). Similarly, Millikan (1933–) grounded intentionality in evolutionary history (Millikan 1984). Her book title was deliberately provocative: *Language, Thought, and Other Biological [sic] Categories*. This was guaranteed to raise philosophical hackles in devotees of the later Wittgenstein, and neo-Kantians in general (see Sections vi–viii, above).

Millikan upset many science-inclined naturalists too, by giving more philosophical weight to evolution than to neuroscience. Thus she argued that a perfect simulacrum of a human being, magically constituted in the middle of a swamp by a sudden combination of the relevant molecules, would have *no* beliefs, desires, or other intentional properties (1984: 93, 337–8; 1996; cf. Boorse 1976). It would, of course, utter the very same words as a human being would, if engaged in 'conversation'. For all the language-relevant events in the swamp-man's brain (and ears, and lips . . .) are, by hypothesis, identical with those of a person. But it wouldn't be a genuine conversation—for, on the swamp-man's side, no meanings or intentions would be being expressed. (In her defence, one could point out that we accept thermodynamics *even though* it allows the theoretical possibility of a snowball in hell: is swamp-man any more implausible?)

This imaginary example highlighted her central—and controversial—claim, that current meanings depend in part on events that happened millions of years ago. Millikan was saying, in effect, that Searle had been wrong about the "something more" that's needed *in principle* for intentionality. According to her, it's not neurochemistry as such that grounds meaning—nor even neurochemistry in interaction with the body and environment. Only evolutionary history can fix the system's semantics.

If Millikan's (or Papineau's) version of biological semantics is correct, then no 'ready-made' AI system, nor even a self-organizing—but non-evolutionary—A-Life system, could enjoy mind, intelligence, or meaning.

However, evolutionary semantics was later related to research in evolutionary robotics (Boden 2001). We saw in Chapter 15.vi.c that a robot's neural-network 'brain' may evolve 'feature-detectors' analogous to those found in mammalian visual cortex. So a mini-network may evolve that's sensitive to a light–dark gradient at an orientation matching one side of a white cardboard triangle, and that's used by the robots as a navigation aid (Harvey *et al.* 1994; Husbands *et al.* 1995). Such examples challenge Searle's (1980) view that the "meaning" of a computer model must always be derivative, and arbitrary to boot (see Section v.c).

One could debate whether the feature-detector means "light–dark gradient sloping up and to the right" as opposed to "left side of the white triangle". But similar difficulties attend the ascription of non-conceptual content to animals. (Are bug-detectors really *bug*-detectors, whether for the frog or for the frog's brain?—see Chapter 12.x.f and Cussins 1990: 416–17.)

The important point is that the various meanings one might want to ascribe to the robot aren't arbitrary. Nor are they derivative, based only in the human purposes involved in their design. They aren't based purely on causal regularities, either. They spring to mind as candidate meanings because the mini-networks concerned have evolved, within that task environment, to discriminate certain visual features and guide the robot's movements accordingly. That is, they're environmentally, enactively, and evolutionarily grounded.

However, to say these A-Life "meanings" aren't arbitrary isn't to say they're genuine. There's no consensus among A-Life researchers on whether evolutionary robotics could produce real intentionality. For the pure A-Life functionalist, it could: the triangle-detector is a primitive case, and more advanced (animal-like) examples would embody richer meanings. For Maturana, it couldn't: evolution and intentionality can occur only in biological organisms—so quasi-evolved robots can quasi-embody only quasi-meanings.

Nor is there a consensus among philosophers unconnected with A-Life, for the nature of life, mind, and the life–mind relation remain controversial.

Not everyone accepts an evolutionary semantics, for example. A causal semantics can't support the common-sense intuition that mind can arise *only* from life, unless the relevant causal relations can be shown to arise *only* in living things. And a model-theoretic semantics can't support it at all.

The competing A-Life methodologies of the early 1990s were systematically compared, and related to earlier philosophies of life, by Peter Godfrey-Smith (1994). He distinguished three dimensions of variation: internalism and externalism; asymmetrical and symmetrical externalism; and weak and strong versions of the continuity of life and mind.

Internalist approaches see life as autonomous self-organization, wherein internal constraints govern the history and interactions of the constituent units of the system. Examples include autopoietic theory and Stuart Kauffman's autocatalytic networks (15.viii.b). Externalist approaches explain the system's internal structure primarily as a result of its adaptive interactions with the environment. Work on evolutionary robotics is one example.

The asymmetric externalist emphasizes the organism's adaptive responses to its environment. By contrast, the symmetric externalist pays attention also to the active role of the adaptive organism in shaping that environment. Examples are situated robotics, and Ray's or Pattee's models of co-evolution, respectively.

Finally, the weak continuity theorist sees mind as emerging only from life, but as significantly different from it, whereas the strong continuity theorist regards mind and life as ontologically similar, sharing basic organizational principles. Descartes wasn't a continuity theorist at all, for he saw mind and living bodies as utterly distinct (see Chapter 2.iii and Matthews 1977). Examples of strong continuity theorists include the Naturphilosophen (Chapter 2.vi), the cybernetics movement (Chapter 4.v–vii), and autopoietic theorists. Arguably, they also include philosophers of non-conceptual content (Chapter 12.x.f) and participatory computation (see Section ix.e, above). And someone who argues that not all living things are cognitive systems (see above) is supporting weak continuity in that respect.

However, "mind" covers a number of abilities, and some of these may be strongly continuous with life whereas others aren't. Language has often been seen as a cut-off point. For instance, we saw in Chapter 2.ii.a, g, that Aristotle was a strong continuity theorist for perception and autonomous movement, but perhaps not for human reason (cf. Matthews 1992). Heideggerians who confine *Dasein* to human beings, or Wittgensteinians who ascribe intentionality only to linguistic concepts, count thus far as weak continuity theorists. But some neo-phenomenologists (such as Clark and Wheeler) ascribe intentionality to non-human animals, too.

Analogously, many AI connectionists allow that GOFAI insights will be needed to model the 'logical' aspects of human thinking (see Chapter 12.viii–ix), whereas some dynamical theorists deny this (Section vii.c, above). And nouvelle AI (a label recalling the minimalism of nouvelle cuisine) insists that AI must be grounded in 'lower' abilities, like those of our evolutionary precursors, whether or not it has to add GOFAI methods on top.

In sum, the relation between life and mind is still highly problematic. That applies to work in AI/A-Life, and to philosophy too. The common-sense view is that the one (*life*) is a precondition of the other (*mind*). But there's no generally accepted way of proving that to be so.

17

WHAT NEXT?

This chapter might have joined chapter XI of *Through the Looking Glass* as one of the shortest in the English language. For in response to "What next?", what is there to say but "Who knows!"? Fundamental advances, in particular, are unforeseeable. As Captain Cook's biographer put it, "Genius, of whatever sort, takes us unawares: is not, even in retrospect, deducible" (Beaglehole 1974: 3).

One doesn't have to adopt a Romanticist view of creativity, nor a literal interpretation of "genius", to agree with that. Creative ideas are unpredictable for a number of very different reasons, not all of which will be mentioned here (but see Boden 1990*a*, ch. 9). And some are more unpredictable than others. Even a carefully designed technological artefact will have some unpredictable features, as we saw in Chapter 8.v.b (Ziman 2000*b*). A relatively speculative creative idea can be more surprising still. (And its social impact is even less predictable: Tim Berners-Lee himself couldn't have foreseen that in a mere three years, from 1993 to 1996, the Web would grow from 130 sites to over 600,000—Battelle 2005: 40.)

More precisely, *historically* creative ideas, never generated by anyone before, are unforeseeable. Creative ideas that are new only for the person concerned can sometimes be foreseen, and even deliberately brought about, by other people—think of Socratic dialogue, for example. In what follows, I'll use "creative" to mean historically creative.

17.i. What's Unpredictable?

One source of unpredictability is serendipity: the finding of something valuable without its being specifically sought. Since this is unexpected by definition, prediction simply isn't on the cards when it's involved. The classic case in science is Alexander Fleming's noticing the dirty dish of agar-jelly, which led eventually to the discovery of penicillin. In cognitive science, the part-accidental discovery of several visual-feature-detectors is another illustration (Chapter 14.iv.a–b). In both cases, of course, a good deal of careful and systematic work had to follow the initial observation, before anything worth calling a "discovery" could be achieved.

Another source of unpredictability is change in the wider cultural context. The generation—and still more, the acceptance—of new scientific ideas can be discouraged, encouraged, and even part-guided by social–political factors (see 1.iii.b–d and 2.ii.b–c).

The positive reception of heterarchy (10.iv.a) and distributed cognition (13.iii.d–e), for instance, was influenced by political ideology, and of expert systems by nationalism and economics (11.v). Even if cultural changes could be predicted (which they can't), their effects on contemporary scientific thinking could not.

This applies also to shifts in the power relations between the various groups/disciplines within cognitive science, and so to *what will be seen as cognitive science* in the future:

> Some 25 years after its various beginnings there still is no such thing as a core cognitive science. Depending on where one looks, which departments one queries, who one's friends are, the core of cognitive science will be asserted to be neurophysiology, psychology, artificial intelligence, linguistics, or some more vague concept like human/machine interaction or symbolic or connectionist modeling. The result may not have been cognitive science, but it has been exciting and scientifically fruitful. It has created a community of interests and increased interdisciplinary communication. But as of now there are still viable independent cognitive sciences such as neurophysiology, linguistics, and psychology that flourish with or without the cognitive science label or affiliation. *It is difficult to say at this point where this will lead*. (G. Mandler 1996: 23; italics added)

A specially important type of cultural change concerns new technology. Sometimes, the new instruments are needed in order to do things that obviously needed doing—for instance, developing micro-electrodes to record from the cell body of a single neurone—or anyway, a very small number thereof (2.viii.e). Such cases are relatively predictable: it was clear that people would try, and probable that someone would eventually succeed. Other technological tools may come as more of a surprise, at least to people in other areas of science: a few biophysicists may have been able to predict brain-scanning techniques, but psychologists couldn't.

Technology can be used to prompt new concepts, as well as to find new data. Indeed, technical ideas have been transmuted into psychological theories on many occasions (Gigerenzer 1991*b*, 1994). Moreover, machines have been used as analogies for the brain for hundreds of years (Fryer 1978)—jukeboxes included (2.viii.f). The latest, of course, is the computer itself. So sceptics often say that it's just the latest in a long line of such analogies, to be displaced eventually by some unforeseen invention coming who knows when.

Well, yes and no. In cognitive science, the computer isn't merely a superficial analogy, a metaphor fished out of the memory—perhaps for purposes of popularization—after the real scientific work has been done. On the contrary, it provides substantive concepts in psychological and neuroscientific theories.

The computational concepts concerned were diverse even at the outset (4.ix), and have multiplied over the years (16.ix). Besides symbolic, connectionist, and evolutionary AI, they include dynamical systems described by differential equations (14.vi, 15.viii–ix and xi). The future may well hold unpredictable new machines, even less imaginable today than quantum computers are. But cognitive scientists believe that only *some sort of computational machine* will be relevant. For their key claim is that mind can be explained (not by today's ideas about computation, but) by *whatever theory turns out to be the best account of what computers do* (Chrisley 1999; see 16.ix.f). In that sense, they would endorse Philip Johnson-Laird's remark that "The computer is the last metaphor; it need never be supplanted" (1983: 10).

Other psychological reasons for unpredictability apply to all instances of creativity. They include the rich idiosyncrasy of human minds and the relative—though only *relative*—freedom of creative thinking (13.iv; cf. 7.i.g). Introspectively, it may seem as though almost anything can happen—at least, according to the molecular biologist François Jacob:

> Day science employs reasoning that meshes like gears . . . One admires its majestic arrangement as that of a da Vinci painting or a Bach fugue. One walks about it as in a French formal garden . . . Night science, on the other hand, wanders blindly. It hesitates, stumbles, falls back, sweats, wakes with a start. Doubting everything . . . It is a workshop of the possible . . . where thought proceeds along sensuous paths, tortuous streets, most often blind alleys. (Jacob 1988: 296)

If the creative scientist himself "wanders blindly" much of the time, so much less can his thoughts be foreseen by other individuals—who don't even know his *present* thinking in much detail.

Moreover, some new ideas strike us as paradoxical, not to say crazy, even *after* they've occurred. That often happens in instances of "transformational" creativity, in which one or more dimensions of the previously accepted style of thinking is/are radically altered or dropped (Boden 1990*a*, chs. 3–4). The more basic the dimension, the more fundamental the conceptual change will be. In such cases, it's hard for the new idea to be understood, and even harder than usual for it to gain acceptance. A fortiori it's harder to predict.

In particle physics, that's par for the course. Freeman Dyson reported an encounter between Niels Bohr and Wolfgang Pauli, who'd given a lecture on his new theory:

> Bohr rose to speak. "We are all agreed", he said to Pauli, "that your theory is crazy. The question which divides us is whether it is crazy enough. My own feeling is that it is not crazy enough." (Dyson 1958: 74)

And Dyson commented:

> The objection that they are not crazy enough applies to all the attempts which have so far been launched at a radically new theory of elementary particles. It applies equally to crackpots. Most of the crackpot papers which are submitted to *The Physical Review* are rejected, not because it is impossible to understand them, but because it is possible. Those which are impossible to understand are usually published. When the great innovation appears, it will almost certainly be in a muddled, incomplete and confusing form. To the discoverer himself it will only be half-understood; for everybody else it will be a mystery. *For any speculation that does not at first glance look crazy, there is no hope.* (italics added)

Cognitive science as a whole is less rococo, less conceptually bizarre, than particle physics. But several seemingly crazy ideas have found a respected place within it, after being fiercely resisted as "absurd"—perceptual defence, for example (Chapters 6.ii and 16.v.f), and object-oriented programming (10.v.d and 13.v.d). And remember the punchline of the quip about *Plans and the Structure of Behavior*: ". . . and Pribram believed it!" (6.iv.c). When people said that, they weren't dismissing those new ideas as worthless. They were allowing that they were weird-but-interesting, so worth thinking about. (Karl Pribram was made the fall guy because he'd recently defended a holographic theory of memory—hardly the usual bread-and-butter fare: 12.v.c. Nor is Bergsonian philosophy, but Pribram later dallied with that as well: 16.x.a. His reputation as a maverick was deserved. However, even those who called him "crazy Karl Pribram" later admitted that his strange ideas had "caught on", and that "his neurophysiological speculations are decades beyond other physiological work"—Walter Weimer, interview in Baars 1986: 309–10.)

Many future contributions, too, will seem weird initially—though just how weird they'll need to be remains to be seen. The "particle physics" of the field is conscious experience. This has already prompted many highly counter-intuitive theories, including some crackpot publications. I argued in Chapter 14.xi.e that a currently undreamt-of (i.e. crazy) approach will be needed to explain it.

Close runners-up in order of difficulty, and so in licensed craziness, are intentionality and computation. We saw in Chapter 16.ix.e how an extraordinary (crazy?) theory of those-two-together has come from an AI scientist–philosopher who thinks that "For sheer ambition, physics does not hold a candle to computer or cognitive . . . science" (B. C. Smith 2002: 53).

Sometimes, experts declare future progress to be not so much unpredictable as impossible. This view was implicit in Thomas Watson's notorious remark in 1943, as IBM chairman, that "I think there is a world market for maybe five computers." (He died in 1956, so never knew just how wrong he was. But he wasn't alone: Howard Aiken, of all people, said, "there will never be enough problems, enough work, for more than one or two of these computers"—Edwards 1996: 66.) And it was explicit in the advice given to Konrad Zuse in 1937 by Kurt Pannke, a manufacturer of specialized calculators:

> "Someone informed me", Dr. Pannke began, "that you have invented a computing machine. Now, I don't want to discourage you from continuing to work as an inventor and from developing new ideas, but I must go ahead and tell you one thing: in the field of computing machines, practically everything has been researched and perfected to the last detail. There's hardly anything left to invent . . ." (Zuse 1993: 42)

(To be fair to Pannke, he later changed his mind. He provided money to fund Zuse's home-based research, and recommended his machine to the German military—fortunately, with no effect: see 11.i.a.)

17.ii. What's Predictable?

I can't imagine anyone suggesting that there's "hardly anything left" to be discovered in cognitive science. But I've just allowed that creative ideas can't be predicted, only awaited. So perhaps I should now present you with an empty page, and leave it at that? After all, that's a respected rhetorical device. Laurence Sterne did it 250 years ago, when he declined to describe a beautiful woman in *The Life and Opinions of Tristram Shandy*, leaving it to the reader's imagination instead.

I don't have the courage to follow Sterne's example. But it wouldn't be appropriate in any case. For there is something that can be said.

All scientific research, in whatever domain, is located within some identifiable conceptual space where further creative exploration (and transformation) is clearly possible, and where some dimensions seem especially rich in potential with respect to current unsolved problems (Boden 1990*a*, 2004). Peer review, especially of proposals for future research, depends on that fact. We can't predict the detailed outcome of such explorations and transformations, much as David Livingstone couldn't foresee his discovery of the Victoria Falls. But we can reasonably expect that if we follow *these* dimensions of the space (compare: the Zambesi River, the mountains glimpsed ahead . . .), we'll find something of interest. That is, we can have intellectually defensible, if not infallible, hunches about where future discoveries are likely to occur.

In so far as such predictions are possible, I've indicated mine already. The previous chapters have told "the story so far"—but always with an eye to possible future episodes. So the relatively small volume of *recent* work that I've mentioned was chosen not just because it's recent, nor even because it's intriguing. It was selected because I think it's promising, capable of development in ways that seem likely to be fruitful.

One way of justifying our hunches about where interesting new ideas are likely to arise is to rely on sub-hunches about *how* those ideas might be generated. In other words, some specific exploratory pathways are recognizable as familiar ones, because they've often been found to be fruitful.

* For instance, once a simple deterministic space has been defined, it's very likely that people will eventually try to complexify it in certain ways. So when John von Neumann defined the basic cellular automaton, he knew very well that probabilistic and even evolutionary CAs would be explored later (Chapter 15.v–vi).

* Again, once problem solving had been seen as a simple hierarchy (6.iii), it was inevitable that more complex and/or 'open-execution' plan hierarchies would be explored (10.iii.c). It was even a good bet that theories of problem solving would eventually be transformed by hierarchy's being made less pure (10.iv.a), or perhaps deliberately dropped (13.iii.b, 15.viii.a).

* Third, when Alan Turing wrote his morphology paper, he knew that increasingly complex systems of reaction–diffusion equations would be explored, once computer power allowed (15.iv).

(Similar remarks apply to creativity in artistic contexts. So, for example, it was nigh inevitable that post-Renaissance composers would progressively complexify tonal harmony. And it was always on the cards that someone—it happened to be Arnold Schoenberg—would eventually transform the space of tonal music by dropping the home-key constraint altogether: C. Rosen 1976; Boden 1990*a*, ch. 4.)

In short, the common notion that creative thought is unpredictable because it's chaotic (in the everyday sense) is mistaken. There's significant method in creative madness. It's our tacit recognition of this fact which enables us to identify certain work as promising, even though we can't spell out the promises.

17.iii. What's Promising?

The recent empirical research that I see as promising in these terms includes the following (listed here in no particular order):

* hybrid systems I: symbolic/connectionist (Chapters 12.iii.d and ix.b, 15.viii.a)
* hybrid systems II: situated/deliberative (7.iv.b and 13.iii.c)
* hierarchical networks (12.viii.b and ix.b)
* connectionist work on the role of imagery of words (12.ix.e)
* statistical approaches to NLP (9.x, preamble, and 9.x.f)
* integration of connectionist learning with detailed neurophysiological data (14.ii.d)
* modular and/or time-based neural networks (12.ix.a, 14.ix.g)
* programmed/evolutionary neuromodulation (14.ix.f)
* AI-evolved organic–silicon computing networks (14.ix.f)
* computational neuro-ethology (14.vii, 15.vii)
* insect navigation strategies (15.viii.a)
* types of cerebral representation, especially emulators (14.viii)
* the epigenesis of thought and language in normal and brain-damaged children (7.vi.g–i)
* models of clinical apraxia and aphasia (12.ix.b, 14.x.b)
* theories of control in hypnosis (7.i.h)
* brain-scanning, *provided that* it's related to specific psychological theories (14.x.b)
* developmental trajectories (12.viii.c–e and x.e)
* fast/simple heuristics (7.iv.f–g)
* the origin of specific bounds on human rationality (7.iv.h)
* cognitive technology, including virtual reality (10.i.h, 13.vi, 16.vii.d)
* computational theories of creativity (9.iv.f, 13.iv)
* evolutionary modelling (14.ix.d and f, 15.vi)
* achieving open-ended evolution and/or creativity (13.iv.c, 15.vi.d)
* mathematical analyses of dynamical systems (14.vi and ix.b, 15.ix.b and xi.b)
* homeostasis in CTRNs (15.xi.a)
* distributed cognition and agents (8.iii, 12.ii–vi and x, 13.iii.c, 15.viii–ix)
* computational architectures integrating knowledge, motivation, and emotion (7.i.e–g and 7.iv.b–c).

If forced to choose only one of these items, I'd pick the last: work on integrated mental architectures. Indeed, I did that on the fiftieth anniversary of the 1953 discovery of the double helix, when the British Association invited several people to write 200 words for their magazine *Science and Public Affairs* on "what discovery/advance/development in their field they think we'll be celebrating in 50 years' time". This choice reflected my own long-standing interests in personality and psychopathology (Preface, ii). But it wasn't idiosyncratic: two years later, the UK's computing community voted for "The Architecture of Brain and Mind" as one of the seven "Grand Challenges" for the future (<http://www.ukcrc.org.uk/grand_challenges/index.cfm>). One member of the five-man committee carrying this project forward is Aaron Sloman, who's been thinking about architectural issues since the 1970s (7.i.f, 10.iv.b, and 16.ix.c). If progress is to be made on this front, my hunch is that his team will be in a good position to make it.

The Grand Challenges grew out of the UK government's 'Foresight' Programme (instituted in 2003 for a ten-to-twenty-year planning horizon), and in particular out of its Cognitive Systems Project. Naturally, government ministers aren't falling over themselves to help solve the problems of cognitive science for their own sake. For them, applications are all—whether in health, education, business, transport, arts and entertainment, or (of course) the military. But as the Project's official Report (DTI 2004) makes clear, scientific and technological motives are often very closely related (and can be satisfied only by interdisciplinary thinking). It should be no surprise, then, that architectures to support "emotional" robots and "social" human–computer interactions are now being investigated at the behest of Whitehall—and, naturally, of the Pentagon too.

The strength and range of the list of "promises" given above show that cognitive science is still a fruitful "scientific research programme" (Lakatos 1970). *Mind-as-machine*, in both its incarnations (1.ii.a), has generated many suggestive theories. These have been amended—and sometimes dropped—on the basis of further advances in our understanding, but in many cases the central insights remain. Marr's work on vision is one obvious example (7.v.b–f), but others have been described in previous chapters.

The field's potential won't be unlocked without new psychological–computational *theories*. Greater computer power may well be necessary, but it won't be sufficient. Even quantum computers and hypercomputers won't suffice to fill the bill (16.ix.a). Fundamental scientific advance will need more Ideas, not just more Bytes. Likewise, more and/or fancier PET/fMRI brain scanning won't suffice either, even though it will often be useful (14.ii.d and ix.c).

On a higher plane of abstraction, I've discussed some recent philosophical research concerning

* the nature of computation (16.ix)
* the variety of virtual machines (16.ix.a)
* conscious experience (14.x–xi)
* the nature of intentionality (16.x.d)
* the origin of conceptual content (12.ix.e and x.f)
* the nature/existence of non-conceptual content (12.x.f and 16.viii.b)
* mind and/as embodiment (16.vii)
* the boundary between self and world (16.vii.d)
* the nature of life, and its relation to mind (15.i and 16.x)
* the resolution/reintegration of neo-Kantian and analytic philosophical viewpoints (16.vii.b–d, ix.d–f, and x.a).

All of these matters will be key foci of effort and controversy in the foreseeable future. Indeed, they're so difficult, and so deep, that I expect them to remain key foci well over 100 years from now. For as remarked at the outset of Chapter 16, philosophical problems don't get solved in a hurry.

Nor, in these cases, will they get solved in disciplinary isolation. They'll require fundamental *and reciprocal* advances in up to five fields: philosophy, psychology, anthropology, neuroscience, and AI/A-Life. (Theoretical linguistics, as opposed to the philosophy of language, is less relevant here—unless we include *cognitive* linguistics: see 7.ii, preamble, 9.ix.g.)

17.iv. What About Those Manifesto Promises?

In Chapter 6.iv.c I said that a good way of judging how far cognitive science has succeeded is to compare it with the hopes/promises expressed in *Plans and the Structure of Behavior* (Miller *et al.* 1960). By the turn of the millennium, virtually all of MGP's promises had been at least partially met. The "satellite images", and the Newell Test, outlined in Chapter 7.vii surveyed many different examples.

To mention just two:

* hypnosis has been demystified (along with multiple personality and religious experience): (7.i.h–i, 8.vi.b, and 14.x.c), and
* MGP's distinction between Plans as animal instincts and as human purposes is now far better understood (7.i and iv, 14,vi.c, and 15.vii).

Although discussions of these matters have been hugely complicated since they wrote their manifesto, today's answers are broadly consistent with theirs. For TOTE units were—deliberately—defined so abstractly that they covered *both* the inbuilt sensori-motor skills of crickets and hoverflies *and* the deliberative (and hypnotic) planning of human beings.

Neither "demystified" nor "far better understood" implies that all the relevant questions have been answered. Far from it. But we're much clearer now about just how MGP's questions can be profitably put.

Consider, for example, their nature–nurture distinction mentioned above. This simplistic duality has given way—within cognitive science, if not yet in the minds of the general public—to an epigenetic view of development. This view was already waiting in the wings before cognitive science got started (5.ii.c). Now, it's prominent in disciplines as varied as psychology, neuroscience, philosophy, A-Life, and robotics (7.vi, 14.vii and ix.c, and 15.viii.a).

There's no reason why this process should cease now. And it doesn't require every psychological question to be answerable by a simulation. For MGP's futuristic remarks concerned a general approach to the mind: computational theorizing, not necessarily computer simulation as such. We'll surely see many new computer models (some of which will reflect new findings in neuroscience). We'll probably see radically new *types* of model (16.ix). Functioning computer models can test a theory's implications and coherence more rigorously than any other method (7.iii.c). But the novel theoretical concepts are what's important, in understanding what sort of system, or virtual/physical machine, the mind/brain is.

One thing is beyond dispute: that the rich subtlety of human minds is even more awe-inspiring than the arch-humanist Wilhelm von Humboldt (9.iv) believed it to be. Indeed, I've already identified this realization as the major result of computational psychology as a whole (7.vii.a). It follows that it will never be possible to capture every psychological detail, whether in a theory or a simulation. Predicting, explaining, or interpreting the specific thoughts/actions of individual people will always be largely "idiographic" (7.iii, preamble), a matter for the unargued intuitions of psychologists *qua* human beings, not for their deliberate conclusions *qua* scientists.

However, that doesn't spell disappointment for MGP. For on the one hand, idiographic insights can often be enriched, and sharpened, by considering general mechanisms. Remember, for instance, the varied ways of expressing different types of anxiety in speech (7.ii.c). On the other hand, the prediction/explanation of highly particular personal matters wasn't what MGP were aiming for. (Nor is this the aim of scientific psychology in general: 7.iii.d.) Rather, they hoped to understand how such phenomena are *possible*.

It's not only MGP's questions which can now be posed more fruitfully. The familiar puzzles that opened this story (1.i.a), many of them centuries old, have all been illuminated—and some even solved—by the successors of the visionary manifesto.

More answers will doubtless be found: the future of cognitive science will be as exciting as its past. But to say *what they'll be* would be like an eighteenth-century Admiralty Board foreseeing James Cook's extraordinary achievements in navigation and map-making: impossible.

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LIST OF ABBREVIATIONS

AAA	American Anthropological Association
AAAI	American Association for Artificial Intelligence
ABC	Agent-Based Computing
ABSET	Aberdeen set-theory programming language
ABSYS	Aberdeen compiler system
ACE	Automatic Computing Engine
ACT	adaptive control of thought
ACTF	a version of ACT, in a series named by successive letters of the alphabet
Adaline	Adaptive Linear Element
ADIOS	Automatic Distillation of Structure
AI	artificial intelligence
AISB	Society for the Study of Artificial Intelligence and Simulation of Behaviour (UK/Europe-based)
ALICE	Algorithmically Integrated Composing Environment
ALPAC	Automatic Language Processing Advisory Committee (of the US government)
AM	Automated Mathematician
AMG	Architecture Machine Group (the MIT group that preceded the Media Lab)
AN	auditory neurone
APA	American Psychological Association
APU	Applied Psychology Unit (University of Cambridge)
ARPA	Advanced Research Programs Agency (of the DOD)
ASL	American Sign Language
ATF	axon transfer factor
ATN	augmented transition network
BBC	British Broadcasting Corporation
BBN	Bolt, Beranek & Newman, now called BBC Technologies
BBS	*Behavioral and Brain Sciences*
BGA	Bruner, Goodnow, and Austin (1956)
BNA	British Nationality Act
BPS	British Psychological Society
BR	belief representation
BVSR	Blind Variation and Selective Retention
C3	command, control, and communications
CA	cellular automaton
CAD/CAM	computer-aided design/manufacture
CAS	Center for Adaptive Systems (Boston University)
CASTE	Course Assembly System and Tutorial Environment
CBC	Computer Based Consultant
CD	conceptual dependency
CDU	cathode-display unit
CF	context-free
CFPS	context-free phrase-structure
CFPSG	context-free phrase-structure grammar
CHIP	Center for Human Information Processing (UCSD; now renamed Center for Brain Cognition)
CIA	Central Intelligence Agency (USA)
CLRU	Cambridge Language Research Unit
CLS	Concept Learning System
CMAC	Cerebellar Model Arithmetic Computer

CMU	Carnegie Mellon University
CNE	computational neuro-ethology
CNM	Connectionist Navigational Map
CNS	central nervous system
COGS	Cognitive Studies Programme/School of Cognitive and Computing Studies (University of Sussex)
CORA	Conditioned Reflex Analogue
CPSR	Computer Professionals for Social Responsibility
CQG	complete quantum gravity
CREA	Centre de recherche en épistémologie appliquée (Paris)
CRT	cathode-ray tube
CS	context-sensitive
CSLI	Center for the Study of Language and Information (Stanford, Calif.)
CSPSG	context-sensitive phrase-structure grammar
CTR(N)N	continuous-time recurrent (neural) network
CUNY	City University of New York
DARPA	Defense Advanced Research Programs Agency (USA) (formerly ARPA)
DCB	Direct-Conflict-Brother
DEC	Digital Equipment Corporation
det (*or* Det)	determiner
DNA	deoxyribonucleic acid
DOD	(US) Department of Defense
DOG	Difference of Gaussians
DPA	Darwinian psychological anthropology
DTF	dendrite transfer factor
EAM	Elementary Adaptive Mechanism
EDSAC	Electronic Delay Storage Automatic Calculator
EDVAC	Electronic Discrete Variable Automatic Computer
EE	evolutionary epistemology
EEG	electroencephalograph
ELMER	Electro-Mechanical Robot
ELSIE	Electro-Mechanical Robot, Light-Sensitive with Internal and External Stability
EM	external memory
EMCSR	European Meeting on Cybernetics and Systems Research
EMI	Experiments in Musical Intelligence (now Emmy)
ENIAC	Electronic Numerical Integrator and Computer
EPAM	Elementary Perceiver and Memorizer
EPSRC	Engineering and Physical Sciences Research Council (UK)
ESRI	elementary self-replicating instructions
FAHQT	fully automatic high-quality translation
FANs	folding architecture networks
FAP	fixed action pattern
FAQ	frequently asked question
FARG	Fluid Analogies Research Group
FFA	fixed functional architecture
fMRI	functional magnetic resonance imaging
FORTRAN	Formula Translation programming language
FRAME	Fund for the Replacement of Animals in Medical Experiments
FREDDY (FREDERICK)	Friendly Robot for Education, Discussion and Entertainment, the Retrieval of Information, and the Collation of Knowledge
FRS	Fellow of the Royal Society
FSG	finite-state grammar
GA	genetic algorithm
GB	government-binding
GEB	Hofstadter, *Gödel, Escher, Bach: An Eternal Golden Braid* (1979)
GNP	gross national product

GOFAI	good old-fashioned AI
GOFAIR	good old-fashioned AI and robotics
GPS	General Problem Solver
GSIA	Graduate School of Industrial Administration (Carnegie Mellon University)
GSOH	good sense of humour
GSPG	generalized phrase-structure grammar
HAM	human associative memory
H&H	Hunt and Hovland
HCI	human–computer interaction
HOT	higher-order thought
HPSG	head-driven phrase-structure grammar
HRR	holographic reduced representations
IAAI	'Innovative Applications of AI'
IAS	Institute of Advanced Study (Princeton)
IBE	inference to the best explanation
ICA	Institute of Contemporary Arts (London)
IEEE	Institute of Electronics and Electrical Engineers (international)
IFIP	International Federation for Information Processing
IFIPS	International Federation of Information Processing Societies
IJCAI	International Joint Conference on Artificial Intelligence
IJCNN	International Joint Conference on Neural Networks
IKBS	Intelligent Knowledge-Based Systems
IMHO	in my humble opinion
INNS	International Neural Network Society
IPL	Information Processing Language
IR	information retrieval
IRCAM	Institut de recherche et coordination acoustique/musique
IRE	Institute of Radio Engineers
IRM	Innate Releasing Mechanism
IRMA	Innate Releasing Mechanism Analogue
ISA	ideal sentential automaton
IT	information technology
JASSS	*Journal of Artificial Societies and Social Simulation*
K&T	Kahneman and Tversky
KIPS	knowledge information processing systems
KMB	Kilmer, McCulloch, and Blum
KR	knowledge representation
LAC	Lattice Artificial Chemistry
LBP	loopy belief propagation
Leabra	Local, Error-driven and Associative, Biologically Realistic Algorithm
LFG	lexical-functional grammar
LGN	lateral geniculate nucleus
LISP	list-processing language
LMS	least mean square
LOT	language of thought
LRAAM	labelling recursive auto-associative memories
LRB	*London Review of Books*
LT	Logic Theory Machine, or Logic Theorist
LTM	long-term memory
MAC	Multi-Access Computing (time sharing); Man and Computers; Machine-Aided Cognition
MACT	*Mathematical Anthropology and Culture Theory*
Madaline	a group of Many Adalines
MADM	Manchester Automatic Digital Machine
M&V	Maturana and Varela
MARIAL	Center for Myth and Ritual in American Life

MCC	Microelectronics and Computer Technology Corporation
MENACE	Matchbox Educable Noughts And Crosses Engine
MGP	Miller, Galanter, and Pribram (1960)
MIT	Massachusetts Institute of Technology
MITECS	*MIT Encyclopedia of the Cognitive Sciences*
MM	massive modularity
MMM	moderately massive modularity
MMORPG	Massively Multi-Player Online Role-Playing Game
MMPI	Minnesota Multiphasic Personality Inventory
MN	motor neurone
MOMA	Museum of Modern Art (New York)
MOP	memory organization packet
MRC	Medical Research Council (UK)
MT	machine translation
MTCLA	Machine That Can Learn Anything
MUD	Multi-User Dungeon
NASA	National Aeronautics and Space Administration (USA)
NCST	National Centre for Software Technology (Bombay)
NE	neuro-ethology
NEA	National Endowment for the Arts (USA)
NEH	National Endowment for the Humanities (USA)
NERISSA	Nerve Excitation, Inhibition, and Synaptic Analogue
NewFAI	newfangled AI
NIH	National Institutes of Health (USA)
NIPS	Neural Information Processing Systems
NK	network with N units, each with K inputs
NLP	natural language processing
NLS	online system
NOAH	Nets of Action Hierarchies
NOMAD	Neurally Organized Multiply/Mobile Adaptive Device
NORAD	North Atlantic Defence
NP	noun phrase
NPL	National Physical Laboratory (London)
NSF	National Science Foundation (USA)
NSL	Neural Simulation Language
NYU	New York University
OED	*Oxford English Dictionary*
ONR	Office of Naval Research (USA)
OPEC	Organization of Petroleum Exporting Countries
OPS	Official Production-System language
P&W	Premack and Woodruff
PARC	Palo Alto Research Center
PC	personal computer
PCB	Prerequisite-Conflict-Brothers
PCBG	Prerequisite-Clobbers-Brother-Goal
PDP	parallel distributed processing
PET	positron emission tomography
PM	Prerequisite-Missing
PNA	polyamide-linked nucleic acid
PROLOG	Programming in Logic
PS	production systems
PSG	control structure for production systems: see Newell (1973*a*) (The "G" is alphabetical: the 7th, and last, in the series)
PSS	Physical Symbol System
PURR-PUSS	Purposeful Unprimed Real-world Robot with Predictors Using Short Segments
RAAM	recursive auto-associative memory

RAF	Royal Air Force
RAND	Research And Defence Corporation (USA)
REM	rapid eye movement
RLE	Research Lab of Electronics (MIT)
RN	residual normality
RNA	ribonucleic acid
RR	representational redescription
RRE	Royal Radar Establishment
RT	run and twiddle
RTF	reticular formation
SAGA	Species Adaptation GA
SAGE	Semi-Automatic Ground Environment
SAIL	Stanford AI Laboratory
SAM	Sound Activated Mobile
SARA	Simple Analytic Recombinant Algorithm
SASci	Society for Anthropological Sciences
SCB	Strategy-Clobbers-Brother
SCI	Strategic Computing Initiative
SDI	Strategic Defense Initiative
SERC	Science and Engineering Research Council (UK)
SETI	Search for Extra-Terrestrial Intelligence
SFI	Santa Fe Institute
SFN	Society for Neuroscience
SIGART	Special Interest Group on Artificial Intelligence (of the ACM)
SIP	semantic information processing
SNARC	Stochastic Neural-Analog Reinforcement Calculator
SOAR	Success Oriented Achievement Realized
SPA	Society for Psychological Anthropology
S–R	stimulus–response
SRC	Science Research Council (UK)
SRI	Stanford Research Institute
STeLLA	Standard Telecommunications Laboratories Learning Automaton
STEVE	SOAR Training Expert for Virtual Environments
STM	short-term memory
STRIPS	Stanford Research Institute Problem Solver
SUNY	State University of New York
SYCO	*Syritta computatrix* (simulated hoverfly)
SYNICS	Syntax and Semantics programming language
TAU	thematic abstraction unit
TG	transformational grammar
THES	*Times Higher Education Supplement*
TINLAP	Theoretical Issues in Natural Language Processing
ToM	Theory of Mind
TOP	thematic organization point
TOTE	Test–Operate–Test–Exit
TPE	temporal propositional expression
TREAC	Telecommunications Research Establishment Automatic Computer (UK)
TT	Turing Test
TtB	Take the Best (Ignore the Rest)
TTT	Total Turing Test
TTTT	Total Total Turing Test
UC	University of California
*UCI	unit of cultural instruction
UCLA	University of California at Los Angeles
UCSD	University of California at San Diego
Umass Amherst	University of Massachusetts, Amherst

US	United States
UWE	University of the West of England
VDU	visual-display unit
VLSI	Very Large-Scale Integration
VP	verb phrase
VR	virtual reality
WHO	World Health Organization
XOR	exclusive or (i.e. *p* or *q*, but not both)

SUBJECT INDEX

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NAME INDEX