The Blackwell Guide to the

Philosophy of the
Social Sciences

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Edited by

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Introduction. Ghosts and the Machine: Issues of Agency, Rationality, and Scientific Methodology in Contemporary Philosophy of Social Science

Stephen P. Turner and Paul A. Roth

This anthology surveys an intellectual landscape vastly and importantly reshaped over the last 25 years. Historically, the philosophy of the social sciences has been an inquiry loosely organized around the problem of the scientific status of social knowledge. This problematic emerged with social sciences themselves in the latter part of the nineteenth century and continued, in one form or another, to dominate discussion through the better part of the next. A trio of core issues – the scientific status of intentional explanations (and agency), the nature of rationality, and the methodological hallmarks of science – seemingly persist through current discussion and debate. But the substance attached to these issues has fundamentally shifted and altered. Without examining details of the substantive changes, the shifts in the subject matter remain obscured. This introduction examines these shifts and proposes an explanation of how and why they occur.

Whatever science is thought to be, it is, at the minimum, a science of the natural world. The questions this formulation raises are: can we have scientific knowledge of the social world? If so, what does "scientific knowledge" mean? Philosophy of science focuses primarily on answers to the second question. Philosophy of social science traditionally has taken those answers and attempted to determine if the conditions making scientific knowledge possible in the natural realm obtain for the social order as well. The guiding assumption in all of this is that an answer to the question of what constitutes the nature of scientific knowledge

provides, *inter alia*, a demarcation criterion, a way of cutting the difference between scientific inquiry and mere pretenders.[1]

The structure of this anthology reflects the editors' views of the change in the underlying problematic governing philosophy of social science. The issues are no longer organized around the familiar topics borrowed from philosophy of science: what is a law, what is an explanation, what are the ontological units (e.g., holism v. individualism), which sciences are primary (reductionism), what is the structure of theories, and so forth. Rather, we now find a field organized around a poorly bounded collection of cross-cutting debates and issues. Some involve the appropriation of a natural science by the social sciences, some claim to incorporate explanations of how natural sciences function within a social scientific framework, and some simply propose new and better ways to do the traditional job of explanation and prediction. Many topics compete in the struggle to unify the understanding of how social science *does* function as well as how it *ought* to.

Debates about standards of rationality and causality remain, in interesting and important ways, central to concerns in the area, but with important shifts in epistemological emphases. Among the new problems are these. How do presumptions about agency, normativity, and value – those ghostly qualities thought to constitute and animate us – fit with the idea of a science of the social, of society as a stable, regularity-manifesting machine? Ironically, with the ascendancy of rational choice explanations, the natural sciences themselves have become objects of explanation, even of justification, by a methodology of decision theory most closely associated with the social sciences. The issue now is whether naturalism presupposes "rationality" in a normative space, or whether "natural" facts explain rationality and normativity.

The Origins of the Philosophy of Social Science

Natural science preceded social science, but in a sense, the philosophy of science and the philosophy of social science were born together. Questions of scientific methodology prior to the emergence of the social sciences had a distinctly different character.[2] The question of what is a law, what is an explanation, and many related questions did not take a well-defined general form until they had been faced with the problem of applying them to social science. The problems presented to notions of scientific inquiry by the social sciences are arguably what makes it intellectually important to answer questions such as what *in general* is "science," or "scientific explanation," or "scientific law." Not unreasonably, one may regard the philosophy of science and the philosophy of social science as both originating in the problem of the scientific status of the social sciences. The notion of science as consisting of a special method emerges only in the nineteenth century in the face of this problem. The question of what serves to define science and the issue of whether social inquiry conforms to the "method" were contested

from the start. The writings of those who set the agenda for what were to become rival conceptions of the philosophy of science, Auguste Comte and J. S. Mill, on the one hand, and on the other hand, John F. W. Herschel and William Whewell, divided on the subject of social science in the same way. The former were on the side of the suspicion of fictions, including the theoretical entities of social theory, which Comte argued belonged to the prepositive or prescientific "metaphysical" stage of the development of social thought; the latter on the side of the insistence that explanation required theories that made sense of data, going beyond it rather than merely summarizing it, as in fitting a curve to data points.[3]

Subsequent philosophical writing on the social sciences has never left these problems behind, though it has reproduced them in various different combinations. But it is more important that the social sciences have themselves never produced results that could be uncontroversially and unambiguously assimilated to the usual philosophical answers to these questions.[4] One indication of the significance of this is the fact that while little of the literature in the natural sciences concerns itself with philosophy, the situation in the social sciences is quite different. The problem of whether the social sciences are sciences has historically been closely bound up with the development of the subject matter itself.[5] The disciplines of the social sciences themselves produce a large literature on the various claims of these disciplines to be sciences, much of which is inspired by writings about the philosophy of science and the philosophy of social science. Not infrequently these are the writings of philosophers of science of generations past, and necessarily so, since the writings of recent generations, for example, Larry Laudan and Bas van Fraasen, are irrelevant to the problem that inspires them: of "justifying" social science or instructing it in how to become "scientific."

What this suggests is that the philosophy of social science stands in a fundamentally different relationship to its subject matter than does the philosophy of science. The philosophy of natural science can treat its subject matter as, if not a finished object of analysis, then at least one that is autonomous – existing independently from philosophical speculation on it. Philosophy of social science lacks precisely this kind of second order or "meta" relationship to its subject matter. Philosophical considerations, especially with regard to claims to scientific status, have been intrinsic to the identity of and to movements within the social sciences.

The historically central problem of the scientific status of social science no longer constitutes the core of contemporary philosophical discussions about the social sciences. One reason for this shift is apparent, and it occurred on the side of philosophy of science: 150 years of reflection on the elements of the "scientific method" has not resulted in a consensus that there is a "scientific method," much less a full-blown demarcation criterion. Confidence that there is a methodological essence to science has decreased as what counts as science has come to appear more historically plastic and contingent. The temptation or need to engage in ongoing disputes about the scientific status (or lack thereof) of putative knowledge of the social has, accordingly, waned.

During one period, however, philosophy of science left its mark on social science itself. The high tide of logical positivism and the "unity of science" movement, when positivism made highly influential and public attempts both to provide general answers to questions about the nature of science knowledge (including related notions of law, explanation, theory, and reasoning) and to apply these results to the social sciences, coincided with a period of rapid expansion in the social sciences. So, despite the fact, as Tom Uebel points out in his chapter, that the social sciences *per se* were never an important concern of positivism and the unity of science movement, positivism's impact was significant. Its formulations were thought to hold the methodological key to becoming a science at the time the social sciences were most anxious about doing so.[6]

Throughout the literature of this period of intense interest, which may be (very roughly) dated from the early 1930s through the 1960s, one nevertheless finds a telling tension between the analytic philosophical style of the logical positivists and the concerns of those who were "building science," for example, engaged in the actual business of using empirical data in the form of experiments in large data sets to actually generate scientific results. The tension is telling because it signals that something is seriously awry. Unlike the attitude towards the natural sciences – that is, that they were doing something quite right, and philosophy would help pinpoint exactly what that is – the approach of positivism to social science suggested that all disciplines so named required wholesale reform.

Ironically, those thinkers in the newly termed "behavioral" science who took up the call for reform needed to rely on some of the more arcane and problematic tricks in the positivist bag, for example, ideas about theoretical entities imported from philosophical reflections on the unobservable theoretical entities of microphysics. The contexts in which the positivist account of theoretical entities originated involved relationships between the entities and the measurements that indicated their existence; or, what was the same, the theoretical necessity or convenience of employing the entities was bound in a web of strict laws or idealizations that took that form. Lengthy discussions ensued about such topics as Craig's theorem. The odd objects that made up the microphysical world fit this model.

Hempel's own comments on these entities is emblematic of a deep problem. Hempel considers what he labels "the theoretician's dilemma," a puzzle that arises from the possibility of replacing theoretical terms by surrogates that involve only observables. The "dilemma" is this: "If the terms and principles of a theory serve their purpose they are unnecessary . . . ; and if they do not serve their purpose they are surely unnecessary. But, given any theory, its terms and principles either serve the purpose or they do not. Hence, the terms and principles of any theory are unnecessary." (Hempel [1958] 1965:186). Hempel goes on to reject the dilemma on the grounds that establishing deductive connections between observables is not the sole purpose of theories: theoretical concepts may enable greater simplicity of theoretical formulation, and may be more fruitful than formulations without theoretical concepts. His discussion of this issue, interestingly,

is elaborated in terms of psychological concepts, and the influential formulations of the philosopher Gustav Bergmann and the psychologist Kenneth Spence.

The ironic consequences of this approach can be seen in the work of the social psychologist Donald Campbell. Psychological entities, such as attitudes, had to be inferred from rough statistical material produced by difficult to interpret experiments that seldom produced quantitatively close results of the sort that could be idealized into laws. The problems produced were given clever solutions, and led to an experimental tradition of great richness and subtlety. In this sense the theoretical terms were "fruitful." As "science," however, this was a fiasco – the extent that the entities behaved regularly was so limited that they could not be usefully theorized about, and fundamental approaches could not be decided among. The project of attitude theory faltered, reduced to tautologies like "attitudes have behavioral effects if they are salient, and attitudes are shown to be salient by the fact that they have effects." The clues offered by positivism to the mystery of how to become a science led to a large body of practical activity, but not to successful theory.

The exponents of wholesale reform did nothing to alleviate the mismatch between the realities of experiment and social and historical research and the gold standard of physics-like explanatory theory. One need only consider Hempel's classic exposition, "The function of general laws in history" (1942). Here Hempel notoriously urges a quite abstract formal model for social science practice, a model which was not then, nor ever has been, applicable to that practice. The tensions did not go unnoticed. There were a good many dissenters to the project of the scientification of social knowledge even within the scientific community. A notable example here is Percy Bridgman. Bridgman, a distinguished physicist and the inventor of the concept of "operational definition," a concept that psychologists and behavioral scientists adopted and popularized, publicly doubted that the social sciences could ever have what he called "significant measurement." The uneasy relationship between social inquiry and conceptions of science produced numerous and sophisticated dissenters to the proposition that social science could ever be accommodated to the methods believed necessary for scientific status; and to the usual answers to the question of whether it was desirable to imagine social inquiry could have this status. These issues were forcefully raised by F. A. Hayek ([1942–44] 1952) and Karl Popper ([1944–5] 1961) in a manner that now seems prescient, for they focused on economic theory, a topic that positivists had special difficulty assimilating.[7]

Winch's Triad

Coeval with these discussions, which reached their peak of informativeness in the 1940s, and within "analytic" philosophy itself, there emerged another line of debate which further complicated the claims to scientific status made in the social

sciences. The problem of human action, and in particular the nature of agency, came to be defined in a debate on the relation between reasons and causes, between law explanations and intentional explanations, which had as its epicenter "ordinary language" philosophers (primarily at Oxford). As we shall see shortly, this prolonged and even obsessive debate had the unintended consequence of moving the problems of the philosophy of social science to an unaccustomed place at the center of general philosophical debate.[8]

Without question the galvanizing moment is to be found in two major writings by Peter Winch, his book *The Idea of a Social Science* (1958) and the essay "Understanding a primitive society" (1964). Winch offered arguments purporting to show why the core concerns of agency, rationality, and scientific methodology formed a logically inconsistent triad. The source of the inconsistency, Winch maintained, was clear. On his account, a science accommodating agency and the nature of human rationality, like a round square, could be shown to be an impossible object just by explication of the concepts involved. The concept of a science demanded a generalizability of relationships that the idea of a social science could not, in principle, provide. The "in principle" barrier turned out to be the notion of rationality itself, and Winch's reasoning here transformed the problem by showing the relativistic implication of the appeal to "reason." For in the case of human relations, Winch maintained, that which determines what counts as rationality is local and cultural. Things without thoughts move to universal rhythms; thinking things do not.[9]

Winch's account of the socially variable nature of rationality made that issue decisive to the possibilities regarding the character of social inquiry. Debates about rationality and relativism came to dominate the philosophy of social science. The period 1964–80 marked philosophy of social science's moment in the sun: its core issues mattered to other, more traditional central areas of philosophy. For a while, traditional problems of epistemology and metaphysics – problems of what we ought to believe and of what – coalesced around the puzzling anthropological data regarding an obscure African community of witchcraft believers, the Azande. What made the Azande so philosophically problematic was that they appeared to reason in ways that were "irrational" by our lights but which were nevertheless entirely functional and unproblematic within the context of their own form of life.

The significance of Winch's "Wittgensteinian" position in the debates over rationality was that, like Quine's earlier attack on and erasure of the analytic–synthetic distinction, Winch challenged the usual epistemic categories for identifying certainties. Categories thought to be certain *a priori* become, on Winch's handling of rationality, only relatively so, that is, given their location within the intellectual motifs of a given society. He granted to Azande thought a kind of logical primitiveness of the sort hitherto reserved for the absolute presuppositions of metaphysics.

Winch's reasoning is worth articulating in some detail. Edward Evans-Pritchard identified "contradictions" in Azande reasoning (e.g., their views on the heritability of witch-substance implied that almost everyone should be a witch, a view they

nonetheless denied). Winch's view implies that Evans-Pritchard pushes Azande reasoning in directions it does not naturally go and therefore constitutes a misunderstanding of Azande reasoning. (Other problems included whether the Azande belief in witchcraft and oracles counted as prescientific or nonscientific, in the sense that they could not be countered by experience.)

The debate's deep significance lies in the fact that it seems to rule out the idea that, at least with respect to the basic inferential patterns of a culture, there could be any such thing as a mistake, or falsity, or irrationality. A "mistake" is a concept relative to the rules pertaining to concept-use in that society. The "rules" of concept-use in the culture determined whether or not an application is correct. The basic inferential patterns of the society define for that society what rationality is. But if "irrationality" too is relative to the rules, a clear implication would be to locate relativism at the fundamental level of the *a priori* conditions of reasoning. There is nothing more fundamental. And this implicitly put an end to a certain conception of "analytic" philosophy in which analysis could provide a replacement for traditional epistemology and metaphysics by analyzing linguistic usage. Analyzing the language of the Azande led, not as G. E. Moore had expected, to the vindication of common sense, but straight to an epistemology of poison oracles and a metaphysics of witches.

The Legitimation of "Continental" Philosophy

The affinities between Winch's progressively more radical arguments and "continental" philosophy were apparent very early. Jürgen Habermas discussed them in *On the Logic of the Social Sciences*, originally published in 1967 (1988, esp.127–30, 135–7). One affinity arose between the idea that reasons were not causes and the idea, promoted by such turn of the century neo-Kantian figures as Wilhelm Dilthey and Heinrich Rickert, that the explanations of the *Geisteswissenschaften* were of a fundamentally different type from those of the *Naturwissenschaften*. But there was an even more powerful affinity to the neo-Kantian account of fundamental categories.

Consider the famous discussion at the origins of neo-Kantianism about the material and spiritual hypotheses in psychology (Fisher [1866] 1976:22), which is in some respects a simulacrum of the fundamental question of whether the social sciences can be sciences. One hypothesis holds that human psychology is purely a matter of material processes, the other hypothesizes a human soul. What could decide between these approaches?

The issue here, put in Quinean terms, is whether there is a fact of the matter between the two hypotheses. The neo-Kantians' response was that there is no fact of the matter, but they did *not* conclude so much the worse for the soul. Rather, the view was that there was no rational ground for deciding between the two. What constituted the factual or determined the facts of human psychology could

be determined in two different ways, consistent with each competing and incompatible hypothesis. The "facts" of psychology were theory-impregnated already, and therefore could not constitute evidence for the theories of which they were a part. It was simply an illusion to think that there were in some sense independent facts, or alternatively independent facts about the world on which reason operates directly.

This neo-Kantian line of argument posed the key problem around which "continental philosophy" developed. Edmund Husserl tried to solve it by attempting to return to a core basic level of "things themselves." The failure of this project led to the recognition in, for example, Martin Heidegger and Karl Jaspers, that no fundamental project, no project of establishing absolute presuppositions, could succeed. The Frankfurt School tried a different, Hegelian, approach: to see the succession of foundations as part of a larger historical project, which was to provide, at the end, intelligibility and emancipation from the false consciousness that previous foundations represented. Another path from this failure was to treat the process of interpretation, in which presuppositions are made and then revised, as the fundamental basis of knowledge. In "hermeneutical" approaches the *a priori* remained as a condition of interpretation open to revision.

One cannot ignore here the extraordinary success and influence of Thomas Kuhn's *The Structure of Scientific Revolutions* ([1962] 1996). By virtue of his much discussed notion of paradigms and how they operate within scientific communities, Kuhn recast the history of science itself as the succession of *a priori* assumptions guiding historically limited communities of scientists. The effect of this reasoning was to cancel the exemption of the internal development of natural science – tacitly accepted even by "continental" philosophy – from the problem of fundamental premises. And this in turn opened the door to a variety of relativisms based on the notion that differences of belief – which philosophy traditionally accounted for by rational considerations, such as new data, or the correction of erroneous beliefs – were the result of the different fundamental premises of different historical communities. In time, postmodernists argued that traditional notions of truth and meaning could only be an expression of a kind of tribal loyalty to one's own community's taken-for-granted standards (cf. Fish 1989, 1995); deconstructionists that these were conditions doomed to be concealed from their readers as a condition of understanding; and feminists that they were irremediably gendered and embodied, and traditional notions of truth and meaning were expressions of the limited understanding allowed by a particular standpoint within gendered society.

Enter Davidson

"Analytic" philosophy took a different turn. Through the debate over rationality and relativism in the 1970s and early 1980s, philosophy of social science had, as

we have suggested, posed a key challenge to mainstream analytic philosophy. Yet the challenge from the margins of philosophy to the core was eventually eclipsed by work on these very issues of rationality which occurred in the core of philosophy, notably Donald Davidson's "The very idea of a conceptual scheme" ([1974] 1984). Richard Rorty recently described Davidson's essay as "a paper which still strikes me as epoch-making. It will, I think, be ranked with 'Two Dogmas of Empiricism' and 'Empiricism and the Philosophy of Mind' as one of the turning-points in the history of analytic philosophy" (1999:575). Among other things, this essay normalized the notion that translating as rational is prior to judging of rationality:

> seeing rationality in others is a matter of recognizing our own norms of rationality in their speech and behavior. These norms include the norms of logical consistency, of action in reasonable accord with essential or basic interests of the agent, and the acceptance of views that are sensible in the light of evidence. (Davidson 1999:600)

At the same time, it muted the radically relativistic implications of this idea by insisting that the beliefs of the "Others" we are interpreting *must be* largely the same as ours: ". . . we cannot take even a first step towards interpretation without knowing or assuming a great deal about the speaker's beliefs. Since knowledge of beliefs comes only with the ability to interpret words, the only possibility at the start is to assume general agreement on beliefs" (Davidson 1984:196). A genuinely radical (but still comprehensible) alternative conceptual scheme was an impossibility, precisely *because* it would violate this precondition of comprehensibility. This led to the end of the dispute that had centered on the question of whether there was a single standard of rationality or a core of rationality common to all cultures. Davidson's (holistic and Quinean) point was that neither was necessary, and that the same considerations that led to the problem – the primary translation as rational over evaluation of rationality – excluded the possibility of the kind of radical "living in a parallel universe" relativism embraced by some of the more exuberant interpreters of Kuhn.

Largely as a consequence of Davidson's paper, issues that began the rationality dispute lost the sharp focus which debate on the anthropological cases had provided, and thus the close connection to cases peculiar to philosophy of social science. Philosophy of social science returned to a pastoral obscurity not unlike that of the Azande themselves. Yet the initial problems concerning the consistency of the Winchian triad did not go away. Two new sets of problems emerged: one with respect to relativism, the other with respect to intentional explanations of a particular kind, rational choice explanations, which we will consider in the next section.

Issues of relativism remain even if one accepts Davidson's claim that one must accept most of the beliefs of those we interpret as true in order to interpret them at all. For it is far from evident that this constraint does not exclude, but might actually warrant, relativism with respect to much of social science or social theory,

or indeed of social standpoints. The problems became evident with the rise of feminist epistemology, which in some versions argued that there is a special standpoint that was epistemologically privileged by virtue of the fact that it is not bound up with the assumptions of dominant groups and could thus enable its possessors to see what the dominant groups could not.

This was a possibility wholly consistent with Davidson's formulations: it did not need to represent a "parallel universe" relativism of the sort his arguments excluded. To say that the master shared beliefs about maps and floors with the maid is to say nothing about the beliefs that form their basic perceptions of one another, the social relationship they have, and consequently the experiences that would underwrite their theories of the world. To the extent that these understandings of the social world are themselves "social theories," and the experiences in question are the basis of social knowledge, the problems that earlier philosophy of social science as "science" had failed to solve, problems about what counts as good reasoning, are still there to be solved. Moreover, they are there in the problematic form of potential relativistic circularity: are the criteria of evaluation themselves a matter of one's "standpoint"?

Rational Choice: The Scientization of the Intentional

In the early days of the reasons and causes debates, there was an unresolved puzzle about what sorts of things "reasons" explanations were. On the one hand, they were typically taken to be fully sufficient – and particularly not to be in need of further explanation, such as a causal explanation which connected the reason as a motivation with an action that was its effect. Indeed, much of the literature focused on the argument that the connections between reasons and causes was of a different kind, "conceptual," and therefore noncausal, or examples of what Aristotle called "practical syllogisms." But explaining what this meant raised insoluble problems of objectivity. The practical syllogisms taken from Aristotle depended on beliefs, such as "dry food conduces to good health," that were local, connected to long supplanted categories of Ancient Greek thought and cuisine. These local beliefs were "relative" in at least three senses: they explained action only for those who shared these beliefs, and counted as explanations only for those who shared them, and they were true and fully intelligible only to those who shared them. Winch had bitten the bullet and acknowledged this: the conceptual necessities that replaced the causal necessities of action explanation were local, and the job of the social scientist was to explicate these local concepts.

Yet only two decades after these problems over objectivity had apparently proven fatal, we find such claims as these: rational choice accounts not only offer a form of interpretation, but also a form "which lets us make objective yet interpretive sense of social life" (Hollis 1987:7). Such accounts were, on this

view, their own explanation. They were, in James Coleman's words, the form of explanation "that we need ask no more questions about" (1986:1). They are the point at which the spade is turned; explanatory bedrock has been hit. Like the verifiability criterion of meaning, the syntax of the theory and the evidence to which it is applied are held to have a "self-evidence" which requires no further explanation. But this bedrock is most emphatically not "local," nor a matter of the epistemic preferences of an interpretive community.

What changed? In the first place, the subject changed. The endpoint of explanation was no longer a "reason" whose objective validity was in question, but a decision, whose rationality was taken for granted, but which needed to be explicated. Successful explication was successful explanation. The form of the explanation is still teleological and intentional. But the beliefs that go into the reasons are not, like beliefs about "dry food," local and true only in a relative way. Explaining in terms of rational choice opens up the possibility (indeed requires) that the beliefs themselves be accounted for in the same manner, that is, as rational choices. If beliefs themselves can be accounted for in this way, then even the rationality of natural science finds explanation in these terms. Explaining the rational preferability of science, on this rather startling view, requires no essential appeal to a purely "scientific" method, and no appeal to epistemology, realism, and the rest of the traditional philosophical justification of science.[10]

But the more thoroughgoing the reduction to rational choice, the stranger the results, and the more perplexing the question of what sort of thing this endpoint is. How can there be an endpoint of explanation that is not itself grounded in nature – as traditional teleological arguments, however defective, were? Is rationality an unmoved mover? And if it is not a "mover" or disposition at all, can its role in explanation be other than purely formal – perhaps a formal redescription of events that have a "real" but different causal explanation at a different level of description, such as the cognitive or the evolutionary biological? If we think of the axioms of rational choice in a Quinean way, as particularly basic and therefore unlikely to be revised parts of our theory of the world, don't they lose their objectivity, and become relative again, this time to the pragmatic purposes that our theory of the world satisfies, and doesn't this suggest that rational choice is merely another methodological perspective?

Philosophy of Social Science Today

The question of whether rational choice analysis requires a further foundation points to the continued importance of the traditional problem of the scientific character of the social sciences. It arises today in novel forms. Winch's concerns with the social embeddedness of notions of agency and rationality were directed at models of social science from the past – notably Mill's in *A System of Logic*

(1843), and Max Weber interpreted in the light of Mill – which attempted to compete with or supplant ordinary reasons explanations. The models were of nomological explanations, or Humean causation. Both are largely irrelevant arguments, given the form of present social and natural scientific explanations.

Structural equation models, which constitute a goodly portion of empirical social science, and employs basically the same methods as those used in such "natural sciences" domains as biostatistics and on such topics as global warming, seem to stand on their own. They do not bring in their train old questions about the interreducibility of domains of explanation because they do not take the form of laws and do not depend on laws, and yet are capable of producing secure causal results with a minimum of background knowledge.

But the problems of compatibility do not disappear. Instead they proliferate. What is the relationship between these models and other theories and the data produced by other methods? Is there reason to think that biological and statistical models can be squared with deeply held views about agency and rationality, or with the results of cognitive science? Can the supposed accounts of micro-foundations of behavior be squared with regularities noted at the macrolevel? These problems of compatibility are sufficiently complex and intractable to make one nostalgic for the older but better formed problem of reasons and causes.[11] The *Rationalitätstreit* of the 1960s and 1970s, similarly, has transmuted rather than disappeared. Rational choice models replaced positivist characterizations of rationality, colonizing key areas of philosophy just as they have the social sciences. Ethics, epistemology, and philosophy of science all draw on their presumed explanatory power. Once again, a model of rationality originating outside of philosophy drives the discussion of issues at the center of the discipline, and it is a model that pervades and transforms the topics to which it can be applied.

Philosophy of social science today may best be thought of as concerned with novel issues of compatibility between arguments generated within its powerful problem traditions. The Winchian claims of logical incompatibility, of a conceptual inability to do intellectual justice simultaneously to agency, rationality, and scientific methodology, have not been resolved. But new issues regarding how to characterize what is rational, and how such characterizations affect the scientific study of the social, have become more pressing. Among these are: conflicts on what to call rational (see Harding's chapter 13), efforts to incorporate the natural sciences with the purview of what social scientists may explain (see Fuller chapter 9), uses of generic notions of rationality to explain decisions in all areas of human activity (see Rawlings and Udehn chapters 5 and 6), subsuming apparently intentional actions to species of biological explanation (see Nelson's chapter 11).

There is an underlying theme to these issues of compatibility. When Quine articulated his vision of a naturalized epistemology, he controversially urged that we understand this relation of rationality and science as one of "reciprocal containment," though, he added, "containment in different senses: epistemology

in natural science and natural science in epistemology" (1969:83). Epistemology contains science insofar as epistemology is a study of the logic by which we build our theories of the world from the data available to us. But science contains epistemology to the extent that our account of how creatures like us turn this trick is tied to studies of how we develop under the stimuli to which we are exposed. The line between philosophy and science blurs.

Philosophy of social science today is primarily concerned with the implications of this blurring. Social science represents, philosophically, a form of "real existing naturalism," explaining or purporting to explain subjects that philosophers traditionally believed to be explicable within the province of reason alone. Today, the challenges typically relate to "normativity," which is understood to be that which stands beyond the reach of naturalistic explanation. And behind many of the issues are questions that arise with naturalism generally – does it conflate or confuse the normative and descriptive enterprise? Is the social phenomenon of normativity something unrelated to what philosophers call "normative" and if so what does this imply for social explanation itself? Is there something normative beyond the naturalistic that interacts with the causal world? Or is normativity in the "philosophical" sense another ghost in the machine? With questions such as these, the issues at the core of philosophy of social science once again move to philosophical center stage. For it is in competing conceptions of social science – is what we value simply part of the *explanans*, or is it an *explanandum* – that such debates are played out. Perhaps history here can run in reverse, at least in the following way. Just as the issues raised by anthropological cases became lost in more general debates about rationality, perhaps debates on the sources of normativity can be given sharper form by looking more closely at competing explanations of their social origins and character. Or perhaps with normativity we reach an incompatibility as profound as mind and body.

Notes

1 See, for example, the chapters in this volume by Hankinson, Humphreys, Rawlings, and Udehn for discussions of how explanatory models in the social sciences either stand free of concerns regarding what defines scientific method or how social scientists have worked to appropriate natural science (e.g., biological theories) for their own ends. Fuller and Harding in their chapters discuss challenges to the presumption that natural science is the royal road to explanation of social phenomena. Finally, although not discussed here, there is an interesting debate concerning the natural sciences as a "natural kind" (Rorty's phrase). See Dupre (1993), Galison and Stump (1996), and Rosenberg (1994).

2 As is evident in the collection of essays on "methodology" before and after Comte, published by Larry Laudan as *Science and Hypothesis* (1981).

3 Ironically, the *positivist* in this debate, Comte, rejected statistics. Herschel said this of the statistician Quetelet:

> What astronomical records or meteorological registers are to a rational explanation of the movements of the planets or of the atmosphere, statistical returns are to social and political philosophy. They assign, at determinate intervals, the numerical values of the variables which form the subject matter of its reasonings, or at least of such "functions" of them as are accessible to direct observation; which it is the business of sound theory so to analyze or to combine so as to educe from them those deeper-seated elements which enter into the expression of general laws. (Herschel 1850:22)

4   See especially in this regard the chapters by Roth and Turner in this volume.

5   For example, in his book *Constructing the Subject: Historical Origins of Psychological Research* (1990) Kurt Danziger treats the subject matter of academic psychology and the discipline itself as fundamentally the product of an erroneous conception of science and method.

6   The methodological manifestos of the logical positivists coincided with a time when the social sciences themselves found these questions particularly salient. Consequently, a number of philosophers and philosophically inclined social scientists found opportunity in this period for unusually close, intense, and fruitful interaction. "Positivist" social scientists such as George Lundberg were among the most supportive academic patrons of the Vienna circle as it re-established itself in the United States. Psychologists worked with Gustav Bergmann and revered Herbert Feigl.

  Indeed, the history of the movement itself might have been quite different without some pre-existing affinities in the social sciences on which it was able to draw. For example, Karl Pearson's *The Grammar of Science* ([1892] 1911) provided one of the crucial historical links between Comte and the logical positivists themselves. The tide of Nazism that swept logical positivists to the shores of the United States and Britain brought them in contact with its native social science positivism. Yet this tide also brought to Anglo-American attention the work of Popper and Hayek, and with them a strong stream of antiscientism originating in a certain conception of economics and economic life that opposed the idea of scientific planning and the scientifically organized reconstruction of social life. Thus, ingrained into some philosophy of science as it made itself felt in Anglo-American culture, was a skepticism about combining social inquiry and science. The admixture of the social and the scientific proved combustible everywhere they were found together, as they were in the 1940s, producing such texts as Lundberg's *Can Science Save Us?* (1961), Hayek's *The Counter-Revolution of Science* (1952), Morgenthau's *Scientific Man vs. Power Politics* (1946), and, most influentially and importantly, Popper's *The Poverty of Historicism* ([1944–5] 1961).

  Philosophical issues of agency and rational choice disappeared when American social science, fueled by a tremendous infusion of foundation money in the postwar period, embarked on a massive attempt to create for the first time a genuine "behavioral science." The stage was then set by the 1950s for searching discussions regarding the problem of constructing social scientific knowledge. The philosophical literature about the philosophy of social sciences that emerged in the 1960s drew on a literature from the 1940s and 1950s that was a product of the earlier interactions.

7   And continue to have difficulty with (cf. Rosenberg 1992).

8   On yet another front, much of the heady atmosphere of contention of the 1950s took on a striking and new political polarity within the student movement of the 1960s.

For positivism, despite its development by thinkers of liberal and leftist views (Ayer, Schlick, Neurath, and even the young Wittgenstein), became typed as politically reactionary. This formulation reflected Popper's vehement animus towards all notions of social engineering and the concomitant reluctance to assign to social science the ability to make policy prescriptions. This nonprescriptive view lay at the core of the *Methodenstreit*, a much discussed and open conflict between Popper and his tribe and the by then elderly theorists of the first generation of the Frankfurt School, notably Max Horkheimer and Theodor Adorno. The exchanges functioned more as opportunities for mutual condemnation than a reasoned exploration of differences. But here too the philosophy of social science was the active front in the growing conflict between continental and analytic philosophy (but before even these characterizations became standard usage).

9 The problem followed, ironically, quite directly from the way that ordinary language philosophers had made their case against causal explanation. If, for example, explanations of action were to be construed as explanations of practical syllogisms rather than laws, it was evident from the standard examples ("Dry food suits any human/Such-and-such food is dry/I am human/This is a bit of such-and-such food; yielding the conclusion: This food suits me" Anscombe [1957] 1963:58) were simply false – or at best "rational" in terms of the local criterion of rationality and the particular cosmology. And this posed the problem of relativism: if agents believed in witchcraft and "explained" their own actions in terms of practical syllogisms about witches, did this also count as explanation for us?

10 See Goldman (1999), esp. Chapter 8; Goldman and Shaked (1991); Kitcher (1993).

11 In retrospect, the intellectual shifts in the philosophy of social science problematic vindicate Weber. Weber was regarded in the 1960s by writers such as Winch and MacIntyre as an interpretivist entangled in a naive logical error. The error was rooted in his supposed attempts to apply the causal methods of Hume to the explanation of human action under the illusion that this would create a science free of the conceptual demands of generalizing natural science. Weber, however, had in fact grasped this problem, arguing that social science needed to rely on a probabilistic sense of causality appropriate to situations in which the causal categories are preconstituted, as occurs in the case of judgments about legal liability, and not to apply nonomological or Humean concepts of causality. He also posed the crucial problems of the relationship between all three sides of the triangle of action descriptions and the culturally relative constitution of the subject matter of the social sciences, and provided an answer: to be intelligible to a given cultural audience, actions needed to be described in terms of ideal-typifications which were understood by that audience Some typifications, such as that of instrumental rationality, that is, rational choice, were more readily applied transculturallly than others, which were more local. But nothing about this type made it uniquely valid or applicable. It was only a contingent historical fact, which he thought could be explained, that it was particularly applicable in certain times, and there was no assurance that in the future it would not only cease to be applicable and even cease to be comprehensible to future audiences, just as former typifications are no longer intelligible to us.

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Part I

Pasts

Cause, the Persistence of Teleology, and the Origins of the Philosophy of Social Science

Stephen P. Turner

The subject of this chapter will be the history of the problem of cause and teleology in the social sciences up to the early years of the twentieth century, especially as it appears in the thinking of several of the major founding figures of disciplinary social science. The topic is muddled. But the later history of social science is unintelligible without an understanding of the issues, which have never been fully resolved. The history of the problem is driven by the fact that overtly teleological forms of explanation have often been replaced by problematic or ambiguous forms. Older terminology was sometimes replaced with new (e.g., "function" or "meaning" for "purpose" and "self-organizing systems" for "organisms"), turning the issues into terminological disputes, and sometimes making the different positions difficult to distinguish. Whether the new forms are free of the problems of the old forms is a matter of continued controversy. I will begin with a brief introduction to this history, told largely from the point of view of problems that arose for those who made the history, and conclude with a discussion of the present status of the technical issues for the project of eliminating teleology and the (perhaps insurmountable) difficulties in carrying it through.

Teleology and the Scientific Revolution

The social sciences emerged over a long period, against the background of, and in opposition to, an inheritance from Aristotle and the natural law tradition. The inheritance was noisily rejected by some thinkers, and partly rejected and partly absorbed by others. The tradition was a teleological or purposive mode of

theorizing about the order of the world, including the social world, that considered all beings to be governed by law in a purposeful hierarchy. At the beginning of this revolution Richard Hooker formulated the idea of natural law thus: By "the law of nature . . . we sometimes mean that manner of working that which God has set for each created thing to keep" (1888:206–8). Both people and things were supposed to have an essence that reflected the purposes of nature or of God. The term "destiny" was used to characterize the process by which the ends were contained in the nature of a thing. "Every thing both in small or in great fulfilleth the task which destiny hath set down," as Hooker quoted Hippocrates. "Natural agents" do this "unwittingly"; for voluntary agents, the law is "a solemn injunction" to fulfill the tasks for which they are created (1888:206–8). This distinction marked the divide between the human and the physical.

Natural law theory held the world to consist of a variety of beings and objects whose essence disposed them toward the fulfillment of higher purposes. The larger hierarchy of purpose answered the question "why does thing x exist?" – the manifest "natures" of things were evidence of the purposiveness of creation. The model could be applied both to the physical and the human world, taking into account the differences in the essential character of humans and things, and the ways in which they were governed by natural law.

The key technical feature of these explanations was asymmetry. Charles Taylor formulated the issue in a way which has been influential by contrasting the law of inertia to the teleological conception of motion.

> The [Newtonian] Principle of Inertia does not single out any particular direction in which bodies "naturally" tend to move. . . . And thus it may be said to be neutral between the different states of any system of which it may be invoked to explain the behavior. But this cannot be said of a principle of asymmetry, whose function is precisely to distinguish a privileged state or result . . . that, in other words, this result will be brought about unless countervailing factors arise. (Taylor 1964:23–4)

The teleological account of motion thus involves a "notion of 'tendency towards' a given condition which involves more than just the universal and exceptionless movement of events" in a certain direction. It involves the idea of "a bent or pressure of events towards a certain consummation, one that can only be checked by some countervailing force" (Taylor 1964:24). In Hooker's language, this "bent" is the "manner of working" that leads to the destiny set down for the being by God; for others this becomes the notion of powers and essential natures, and such asymmetries as those between the "normal" and the "pathological."

Natural law thinkers were not naive, and it was of course evident that this way of solving intellectual problems could go wrong, even badly wrong. But they believed that there was a solution to the problem of arbitrariness in what might be called nesting: the fact that an end toward which something "tended" at one level served an end at a higher level, and was, from this higher point of view,

nonarbitrary. So the salvation of the conception was its hierarchical character and the fact that natural ends were arranged in a determinate and knowable sequence of higher purposes, leading up to the purposes of God.

A more stubborn problem, however, was circularity. "Natures" or inner purposes were theoretical properties of things that could only be understood by inferring them from their effects. Were these inferences bogus, and if so why? The *locus classicus* for the sense that these were bogus explanations that depended on linguistic flummery to conceal their emptiness is the ridicule heaped upon medics by Molière. He targeted the practice of physicians (which continues to this day) of giving a Latin name for a disease which is nothing more than the name of the unknown condition that is supposed to be the cause of the "disease," thus giving the illusion of explanatory knowledge. Molière's most famous example of this kind of humbuggery concerned the "dormitive powers" of opium. He has a bachelor of medicine recite the following: "*Mihi à docto doctore/ Domandatur causam et rationem quare/ Opium facit dormire/ A quoi respondeo;/ Quia est in eo/ Virtus dormitiva/ Cujus est natura/ Sensus assoupire*" ([1673] 1987: 'The Third Interlude'). Why does opium induce sleep? Because of its nature, its dormitive power. And sleeping, indeed, was an effect of opium. But to explain this effect by referring to the sleep-inducing powers of opium was merely to move in a circle. To say that "x has dormitive power" is to say nothing more than that x has the effect of inducing sleep.

So the parody had a powerful point. But as Leo Strauss later remarked, if opium did not have dormitive powers it would not have been able to produce sleep. The claim is not arbitrary. At best, it is classificatory. Such classifications pointed to what, for the most part, were genuine explanatory problems. They employed, and depended on, causal analysis, in the form of the theory that the effects in terms of which they ascribed powers or intrinsic natures to things were the result of something in the thing itself. What was missing was the actual mechanism, or any idea of what property of opium induced sleep, or what mechanisms produced the effect. To be sure, mechanisms and properties should be discovered. But, in many of the relevant cases, problems posed in these terms could not be solved by science. And in the numerous cases where intrinsic natures and the like could not be identified except in terms of their effects, this causal theory was circular.

The problem of circularity and the problem of the arbitrariness of the asymmetries are distinct problems, and they arise in different ways in the later history. To these may be added a third: the natures, powers, and the like which are mysterious inner properties, unobservable, and irreducible to ordinary material features of things. They are, as Descartes put it, "attached to substances, like so many little souls to their bodies" (1991:216), and their role in explanation is problematic in many of the ways that the operations of the soul in the material universe is problematic. "Philosophers," he said, "posited [them] only because they did not think they could otherwise explain all the phenomena . . ." (1991:217). They are, in short, believed to exist because they are needed to explain something. And this

reason, explanatory necessity, amounts to an open challenge to construct an explanation that can do without them.

The idea that these problematic accounts of "natures" could be replaced, either by an account of a mechanism (or property that is not arrived at merely by circular reasoning) or by a predictive law, is central to the long revolution against natural law. But in the social sciences mechanisms and predictive laws were elusive, and the replacement concepts were often themselves problematic. Such dispositional ideas as Adam Smith's notion of a "natural propensity to truck and barter" (Smith [1776] 1976:25) were difficult to distinguish from teleological explanations: trucking and bartering is, after all, a kind of end, and the truth content of statements about a "propensity" of this sort is difficult to distinguish from the content of simple descriptions, such as "people who are free to do so, trade."

Aristotle thought of the relations between causal and teleological explanations in a way that made teleology dependent on causality. For him, causal explanations formed a necessary *part* of teleological explanations and needed to be *completed* by teleological explanations. This nagging idea that knowing the purpose served by the causal phenomena persisted, and in some ways knowledge of purposes seems to be *required*, in order to make sense of the "laws" that a causal explanation appeals to. Consider the case of rational actions. We can account for them "causally" in the sense that we can give an analysis of explanations like "I went to the store to buy milk" which breaks the explanation down into a pro-attitude toward milk and the causal fact of our "rational" practical knowledge that it can be obtained by going to the store.

But if we then ask "why do people act rationally?" we pose an apparently legitimate question that is difficult to answer without reverting to teleology at least in the form of the idea that there is a tropism to the rational. We could appeal to the (asymmetric) principle: people tend to behave rationally when there is nothing to keep them from doing so, for example. But this is a characterization of an inner tendency whose effects we can observe, and which shows the nature or essence of people. In the early stage of this revolution resolutions of this kind were typical. Thomas Hobbes, for example, attempts to account for state authority in a nonteleological way, that is to say not by reference to the supposed purposes and nature of state power but in terms of the individuals who make up the state and authorize it.[1] When he does so, however, he runs into difficulties, such as the question of why people keep and are bound to keep the promises that they make when they contract to produce the state, which he can only resolve by attributing an inner promise-keeping *nature* to these individuals: its formulation is suspiciously ambiguous (Hobbes [1651] 1965:74). Is promise-keeping causal and directionless, or is promise-keeping an essentially purposive notion, and thus just as teleological as the account of the state he sought to replace? Dispositions of these kinds remained troublesome. They seemed to be incomplete or feeble explanations, that, when improved upon, either turned into causal laws or to directional teleological claims, a point which we will consider later.[2]

Teleology in the Enlightenment

Both the teleological explanation and the hierarchical teleological worldview came under increasing pressure during the eighteenth century. In large part this was a result of the proliferation and "abuse" of the concept of final causes. In Germany, especially, as theology became possible outside the control of the church, speculative teleological thinking was carried to conclusions that were logical, but even more ludicrous than anything Molière dreamed of. The philosopher Christian Wolff, for example, argued at some length that the sun shone so that people could more easily go about their work in the street or the fields (Wolff 1962, part I, vol. 7:74–5).[3] Voltaire mocked an unnamed contemporary work which held that "the tides are given to the ocean so that vessels may enter port more easily" (Voltaire 1924:133–5). The "so that" in the sentences was meant to be explanatory – the ends explained the facts of sunshine and tides.

Enlightenment thinkers nevertheless were torn in several directions in the face of these problematic arguments. They generally agreed that teleology had been abused in the past. But they were impressed with the idea that organisms seemed to be understandable only teleologically, in terms of some internal principle or nature that could not be reduced to mechanism. Moreover, they relied freely on the idea of human nature, characterized by inherent purposes, in their political reasoning. Even the most naturalistic Enlightenment *philosophes*, especially when they wrote about the inevitable course of history, wrote routinely and unself-consciously in teleological ways. They spoke of the "forces" that assured this inevitability, forces that often seemed difficult to distinguish from "dormitive powers." And when they insisted on the fundamental similarity of laws of social science to laws of physics or biology, they slipped into teleological language unself-consciously.[4] They nevertheless grasped that there were unsolved problems with these usages.

Questions about origins were particularly baffling puzzles for them. If the world was a clock-like mechanism, it seemed that it needed a maker, and a winder, and this created not only a role but a necessary role for God. Voltaire wrote that "If a clock is not made to tell the hour, I will then admit that final causes are chimeras" (1924:133), and he regarded as absurd the claim that "the mouth is not made for speaking, for eating, the stomach for digesting," and so forth. He pointed out that even those who denied final causes in nature "avow nevertheless that tailors make them coats to clothe them," and thus "deny to nature, to the great Being, to the universal intelligence, what they accord to the least of their workmen" (1924:133).

For these reasons, Voltaire, for one, refused to give up final causes, and sought to draw a line between acceptable uses of the concept and abuses. Voltaire's answer to the teleological account of tides was to say that "to be certain of the true end for which a cause functions, it is essential that the effect shall exist at all times and in all places. There were not ships at all times and on all the seas; hence

one cannot say that the ocean was made for the ships" (Voltaire 1924:133–4). But this is just to say that nothing can be a final cause unless it is universally a cause of its effect, which is to say that it is a causal consequence. It does not tell us how to pick out the "true end" from the various universal effects of a cause. To say that all humans exhale CO_2, for example, is not to say that humans exist for this purpose. So the problem of drawing a line proved difficult to solve in these terms. A new approach was needed.

The philosopher who provided it was Immanuel Kant, and the solution was different in kind. Kant began his career as an enthusiastic proponent of a teleological physics, but eventually rejected it.[5] His position on social science, which figures in his essay on universal history (Kant 1963), was novel. He refused to commit to the *reality* of teleological forces, but urged nevertheless that history had to be *understood* as a teleological process. How could Kant have it both ways? He had articulated in his mature writings an argument that teleological explanations were always circular and therefore different in cognitive status from mechanical laws. In his *Critique of Judgement*, he posed the question of whether an organism as a whole can be explained in an entirely causal way, as a mechanical system can. He denied that it can. This "insufficiency" argument was then, and continued to be, the basic argument in favor of teleological accounts. But he then argued that the notion of purpose can, properly speaking, apply only to the free actions of intelligent beings. So when we apply it to organisms we can do so only in a metaphorical or analogical sense, that is to say *as if* they had purposes. He then introduced the notion that "an organized natural product is one in which every part is reciprocally both means and ends" (1988:24–6).[6] But means and ends can only serve as analogical terms here.

Kant's solution to the conflict between cause (in the sense of mechanical causality) and teleology reflects a core problem. There is, he acknowledges, something spooky about teleology, but also something compelling, and the compulsion needs to be explained. To identify purposes in nature requires us to go beyond the sensible world, the world we can subject to observation or experiment. The need to think purposively about the world is ours. Purposes are matters of *our* concern, as intelligent beings, and the need we feel for them is our need. Comte radicalized this insight by historicizing it: he relegated teleological thinking to a stage in the historical development of thought, a "need" which would wither away.

The Replacement of Teleology

Comte's project

Auguste Comte was a self-conscious revolutionary: "The Positive philosophy is distinguished from the ancient . . . by nothing so much as its rejection of all inquiring into causes, first and final; and its confining research to the invariable

relations which constitute natural laws" (Martineau 1858:799). By this he meant the thorough-going elimination from all of science of "theological" and "metaphysical" notions, notably the notion of a purposive universe, in all its forms, manifest and hidden. Comte's project was unprecedented in scope, and relentlessly pursued. He distinguished himself as a thinker in ferreting out hidden teleological usages.

Comte's core sociological idea, his "law of the three stages," contained within itself the idea of the elimination of final causes. According to the law, each scientific area went successively through three stages. The first was one of superstition and animism, which he called "theological," marked by the appeal to "fictitious entities." There followed an intermediate stage, which he called "metaphysical," in which explanations appealed to abstract entities or forces, such as "momentum" (and "cause" itself, in any sense other than the strict sense of invariable relations). In the final stage, these ideas were eliminated, and purely predictive laws constituted the whole of what was taken to be scientific in that domain. Physics had, for the most part, arrived at the positive stage: one no longer asked what "caused" gravitation, for example, precisely because one recognized that the only answer to such a question would be either theological or metaphysical. Biology had not quite reached this stage. Final causes and other pseudo-explanations abounded, often in concealed form. Social science was more distant yet from liberation from pseudo-explanation. Comte took this as his own task.

The notion of the positive stage was a powerful critical tool, and produced a large number of questions about scientific concepts in the sciences that had not yet reached this stage. Were "life" and "organism" metaphysical notions? Could such notions be replaced, or rather be freed of their metaphysical connotations? These were problems that concerned Comte greatly in his accounts of the development of these fields, which occupy much of his major work, the *Cours de philosophie positive* (cf. Scharff 1995:73–91, Schmaus 1982:248–53). The ideas of fictions and hypotheses especially interested Comte, in part because of the contemporary controversy over the wave theory of light, in which he was an active disputant. He argued that the use of hypotheses, and even fictions, is often necessary in science at certain stages of inquiry, but he insisted that in the end hypotheses had to be supported by sensory evidence. Claims about "natures" and the like, by definition, cannot (cf. Laudan 1981:111–62).

The troubles with causality

The devil, for social science, was in the details, and the rather large detail that remained was to produce a collection of laws, auxiliary hypotheses, and so forth, that could replace metaphysical thinking. Comte made an effort to provide them, at least in the form of the law of the three stages and a sophisticated discussion of the conditions for intellectual progress, which were his auxiliary hypotheses. Comte also sought to restate what he thought was usable in the notion of the social

organism in causal rather than teleological terms, as well as to suggest ways in which "life" could be thought of nonteleologically. The strategy here anticipated the later approach of logical positivism – to criticize and reject previous philosophical ideas ruthlessly, but to accept and attempt to reinterpret existing "scientific" thinking. Comte did not do this consistently – he rejected "psychology" as a science and abominated statistics, while accepting the need to replace ideas about the organic character of social life. John Stuart Mill, whom Comte profoundly influenced, followed his strategy of selective appropriation, but selected different things to save, including economics, statistics, and psychology (which he located at the base of the social sciences and to whose laws he ascribed the true explanatory force of the other social sciences).

These differences reflected the basic problem with their position: there simply were no laws of social science, beyond the problematic case of the sociological laws of the three stages. There were statistical relations; actions could be and were explained "causally" in accordance with the model of human action employed in courts of law, and there were various secular trends in history. Economics had developed an impressive deductive structure, but it was questionable whether it was genuinely causal: to the extent that the structure rested on the fiction of the wealth-seeking agent it was apparently teleological, and operated to explain the teleology of the market by reference to the teleology of individuals, whose nature and ends had been attributed to them circularly on the basis of their actions.

The philosophical argument had its own troubles: troubles about causality. As we have seen, the case against teleology rested on the idea that there is something spooky about ends, things that lie in the future, pulling events in the direction of their culmination. Comte, however, recognized that the idea of causes pushing to an outcome was equally spooky, and for much the same reasons. Each notion points to something *permanently* hidden from empirical study and not merely a temporary "fiction." Comte made prediction on the basis of law the true mark of developed science in place of this metaphysical notion of causation. But this was open to objections, particularly from those, like Émile Durkheim, who were realists and argued that the point of experiment in such sciences as chemistry was not simply to predict but to reveal the underlying chemical reality that allowed one to account for the validity of the predictions (Durkheim 1982:199).

Apart from these philosophical objections there was a more painful problem. The social sciences could make predictions, but they were statistical predictions: the only results that resembled the quantitative laws of physics were curves fit to statistical results, like suicide rates, which were probabilities. But Comte rejected these. Medical statistics, he argued, were not laws at all, at least not in the sense of a fully developed positive science. And contemporary experience had borne this out. The great cholera epidemics of the middle part of the nineteenth century were studied by the best statisticians of the day and produced many predictive "results." But they proved to be misleading about the mechanism. The mystery of cholera was solved by John Snow, who focused, not on statistics, but on the substance that transmitted the disease.[7]

There was an easy way out of this unhappy choice between prediction and law – on the philosophical level. An admirer of Comte, the statistician Karl Pearson, at the end of the century thought these issues through and proposed, in his philosophical work *The Grammar of Science* ([1892] 1911), the following solution. In place of invariant succession Pearson argued that variation was the law of nature, that even the laws of physics were idealizations of relationships that, empirically, contained genuine and ineliminable empirical variation, and that, accordingly, there was no difference in principle between correlation and "predictive law." This line of argument exorcized the spooks and made data the determiner, in an especially direct way, of the predictive law. But it had quite a price of its own. If every correlation were equal in the eyes of science, and if there were no distinction, in principle, between correlation and cause, social science had plenty of scientific results: far too many to make sense of, in fact.

Pearson's argument could be accepted in the abstract, without being satisfying in concrete cases where one is nagged by the question of whether a given statistical association is accidental or represents something genuine, meaningful, or, in a word, causal. The intuitions (intuitions that a Comtist would say were conditioned by the metaphysics of causality), even of the social scientists who attempted to implement this program, conflicted with the idea that there was nothing but prediction at stake. So figures such as the early quantitative sociologist W. F. Ogburn, who accepted Pearson's arguments in principle, nevertheless acknowledged that it was necessary to place some additional sort of interpretation on the results to make them meaningful. But, Ogburn argued, in good Pearsonian fashion, these interpretations had no scientific status and were akin to the interpretations that could be placed on an editorial cartoon (Ogburn 1934:17). This line of reasoning was unpersuasive: the distinctions between cause and correlation were too difficult to let go of. The current form of Pearson's problem is the question of causal models, the subject of Paul Humphrey's chapter in this volume. The difficulties are similar: are there purely mathematical criteria for distinguishing between causal and noncausal statistical models composed of statistical correlations? Or do we need some sort of additional information, which adds the "causality" from another source, such as an independently grounded theory, "background knowledge," or hypothesis about "causal mechanisms"? It is not clear that these sources are free from the infirmities that Comte's and Pearson's austerity programs were designed to cure, including teleological usages.[8]

The Rest of Social Science

The organic analogy and function

If one did not choose to accept the implications of Pearson's austerely empirical route – and few social scientists did – there were alternatives. Many of the

alternatives represented a compromise with teleology – an attempt to make the two compatible, the acceptance of particular kinds of teleology, or an attempt to make causal sense of teleological arguments, and two arguments in particular: the idea that society was like an organism and the idea of historical processes with an inevitable outcome. One philosophical solution to the problem of compatibility is particularly appealing: to treat teleological systems as causal systems, to thus make teleological explanation a subtype of causal explanation. This amounts to reducing teleological explanations to causal explanations, to the extent that they are legitimate, and making their legitimacy depend on their reducibility to claims about causal systems.

"System" is a usage with deep potential for ambiguity. In these approaches the arbitrariness of the choice of the favored outcome in teleological explanations may in some instances be overcome by causally explaining why this outcome is favored. Thus an explanation of the teleological structure of a thermostat can be accounted for and made nonarbitrary by reference to the causal mechanisms making up the thermostat and the causal act of setting the thermostat. Human nature, a teleological notion, might be explained as a product of biological or evolutionary processes that are understood to be causal. "Collective" results, such as the spontaneous order that results from the signaling of information in a market, may result causally from the goal-seeking activities of individuals, without the "order" itself being teleological or goal-directed. But in the case of artificial systems the mechanisms are real, understood, and causal. Applying these ideas to human agents or social systems is analogical.

The explanatory language employed by the "organic analogy" in sociology was open to interpretation either causally or teleologically. As has been noted, Comte's struggle against teleology included many attempts to absorb and explain, in nonteleological terms, phenomena such as life. Life was the battleground that the defenders of teleology in the nineteenth century chose to take a stand on: the inadequacy of mechanical accounts of life was held to be proof positive of the ineliminability of purposes from natural science explanation. Against this Comte and Mill attempted to show how such notions as consensus between parts could be understood causally, and to substitute notions like harmony, a physical term, for teleological conceptions (Turner 1986:22–7, 53). One effect of these efforts was to turn organic analogies and talk of "function" into the common property of both sides. Another effect was to muddle the distinction sufficiently that some important thinkers in the next period, such as Herbert Spencer and Durkheim, are in the end difficult to classify. Both vigorously rejected teleology, but employed many terms also used by teleologists and suggested that they could be understood causally.

Spencer remarked of his own book *Social Statics* ([1892] 1954), that "there is everywhere manifested a dominant belief in the evolution of man and society. There is also manifested the belief that this evolution is . . . determined by the incidence of conditions – the actions of circumstances. And there is further . . . a recognition of the fact that organic and social evolutions, conform to the same

law" (Spencer 1901:137). But his discussions of the law have little to do with the incidence of conditions, and much to do with "general laws of force" (1901:138). These undergird the general principle that progress is "the evolution of the simple into the complex, through successive differentiations" (1972:40).

"Evolution" is a highly ambiguous usage in this context: is it teleological or causal? The question is similar for the "general laws of force." Are they symmetric, or teleological? They seem to state a disposition. But is it a directional disposition toward a particular end, or merely a stable but directionless causal feature, like Newtonian inertia? There is good reason to be confused. As his expositors have said, "In *Social Statics*, Spencer almost seems to see the social state as a fulfillment of a preexisting disposition, and he continually asserts an identity between processes in which the outcome is predetermined (like an embryo's maturation) and those in which it is not (like socialization or social evolution)" (Peel 1972:xxxviii). He freely employs the language of "essences" and "natures" (though apparently without regarding such usages as anything more than commonsensical), and even appears to fall into the teleologists' problem of circularity, as when he treats empirical exceptions to his generalizations as "incidental" facts which do not relate to the "nature" of society (Peel 1972:xxxviii–xxix).[9]

These confusions were not resolved by other writers who employed the analogy. The founding figure of French sociology, Émile Durkheim, was a careful reader of Comte and Spencer, as well as of German psychological and legal theorists who were concerned with issues of cause and teleology, and was philosophically tutored by a thinker who had sought to preserve a version of the teleological character of the physical universe, one in which physical law had – and was nested in – an ultimate purpose (Boutroux 1920:159–60, 193–4). Not surprisingly, Durkheim was sensitive to the implications of teleological usages, and especially to the issues of the reducibility of apparently purposive holistic phenomena to mechanistic explanation. His commitment to cause was clear, and he was more careful than Spencer had been. But he also attempted to account for collective phenomena, and intermittently employed an analogy between society and organisms. The meaning he *intended* for these "organic" usages should be clear from a comment he made about the "maintenance" of social institutions. He employed a notion we can recognize from Kant, who spoke of the reciprocity of means and ends. He suggested that "if more profoundly analyzed, [the] reciprocity of cause and effect might furnish a means of reconciliation which the existence, and especially the persistence, of life implies" (Durkheim 1982:144). Thus Durkheim promoted a causal interpretation of the social organism. And he expended considerable effort in redefining such concepts as normal and pathological in non-teleological ways, as well as using words like "function" rather than "purpose," and in construing these words causally.

But intending to be purely causal and fully succeeding are two different things. Durkheim's teacher had his doubts about the explanation in *The Division of Labor in Society*. Boutroux analyzed the argument thus: Durkheim says that the division of labor is necessary to bring about the cessation of the struggle for life. But the

problem "admits of other solutions, the simplest of which is the eating of one another. That is really the law of nature, and division of labor is instituted for the very purpose of impeding the fulfilment of this law" (Jones 1999:160, cited in Boutroux 1914:199). As Robert Jones explains,

> The division of labor is "necessary," only in the sense of being *preferable* – i.e., more in conformity with the idea of humanity, responding more completely to that sympathy with the weak which we assume to exist in man. What can this mean, Boutroux asked, except that "what we took to be a crude law of causality involves a relation of finality [i.e. of teleology], and that we are assuming the intervention of the human intellect and will. . . ." (Jones 1999:160, citing Boutroux 1914:199–200)

In short, among the causes and causal connections on which Durkheim's account depended were some which could not be readily understood nonteleologically.

Decision and intentionality: Weber and the marginalists

Classical economics was largely unconcerned with choice and decision, or for that matter "rationality." The focus was on "factors" of production and commodities, and on the constraints imposed by the physical difficulties of production or by Malthusian forces governing demand for food.[10] These are readily construed as "causes." The effect of the marginalist revolution was to shift attention to individual choices – an intentional term that is most easily understood teleologically: choices are made in order to achieve ends – and the purposive rationality of the individual.

The marginalists posited individual rational agents, pursuing self-selected purposes, whose separate decisions led to aggregate patterns of equilibrium. Thus they assumed a particular abstracted teleology at the individual level to explain the teleological properties of the market. The strategy raised the question of circularity, and indeed the question of whether these models had any empirical content at all. Contemporary critics, such as Thorstein Veblen, who had written his own dissertation on Kant's *Critique of Judgement* (cf. Veblen 1884), recognized that this amounted to a reversion to teleological thinking, and thus went against the general tide of the nineteenth century, which flowed against teleology in science.

There was, however, a very different methodological direction in which a focus on choice and intentionality could lead. Choices, after all, are made by people who conceive of the choices, and their concepts are not irrelevant to the outcomes of decisions. Indeed, in every domain other than the narrowly economic domain of price comparisons, understanding and explaining actions depends on understanding ways in which people conceive situations. Such conceptions vary culturally and historically. Even the questions of basic social science and history come in human terms, human terms that vary culturally and historically. Max

Weber raised the question of whether, even if one could have "a sort of 'chemistry' if not mechanics of the psychic foundations of social life," its results would have significance "for our knowledge of the historically given culture or any phase thereof, such as capitalism, in its development and cultural significance?" (Weber [1904] 1949:75). His answer was that it would not, because terms like "capitalism" are cultural.

Weber understood "culture" as "a finite segment of the infinity of the world process, a segment on which human beings confer meaning and significance." Different cultures or epochs confer different "meaning and significance" on different finite segments. The social sciences, he argued, are cultural sciences, and their questions, which begin with what is meaningful and significant for us, are in terms of the "language of life," that is to say, human terms. This language culturally and historically varies, so the "knowledge of cultural reality" the social sciences seek "is always knowledge from particular points of view" (1949:81). But he also argued that the social sciences were causal, and that the fact of causality itself was not relative to viewpoints. He rejected teleological thinking and spared no effort at rooting it out, violently attacking the teleological formulations of the German historical school in economics as well as the kind of teleology that appeared in collective concepts of the state and law (1975:55–91). But at the same time he defended explanation of what he called meaningful social action in terms of human intentions. These considerations led him to a complex position, which was a kind of compatibilism not unlike Donald Davidson's, which at present dominate the philosophy of action (Weber 1978:4–16).

Trained as a lawyer, Weber pointed out that legal reasoning about responsibility was causal, and argued that this kind of reasoning, properly understood, was relevant to and sufficient for the kinds of factual historical questions that arise within cultural points of view. The proper understanding of the causal character of these questions was this: determinations of causality or responsibility did not require scientific laws, but required a judgment that, in a class of similar cases, subtracting a given condition would have lowered the probability of the outcome. This kind of reasoning could be applied to such historical questions as the question of the contribution of Protestantism to the rise of capitalism, where of course it would necessarily be hypothetical. But the model also allowed explanations of ordinary intentional action as simultaneously intentional and causal. Attributing intentions was done by showing that the sequence of events of which the act was a part was intelligible or meaningful as an action of a particular kind (Weber 1978:8–9). Causal responsibility was shown by establishing that it would have some probability of producing the outcome (cf. Weber 1949:67–75).

Causal and "meaningful" or intentional considerations are coequal and compatible in Weber's model of social science explanation, at least in principle. For Weber action explanations had to be both valid at the level of interpretation or understanding and valid at the level of cause, in the sense that "subtracting" a cause would alter the outcome to be explained. This allowed for low-probability causal relations to be genuinely explanatory.

What does this have to do with teleology? When Weber employed the term "meaning," he did so in order to avoid using the teleological term "purpose," which also could be used in many of the same contexts. Eliminating "purpose" and using "meaning" is one approach to the problem of characterizing intentional action. In Weber's case there was no pretense that "meanings" were causes. Meaning attributions, as he understood, were like purpose attributions in that they were not arbitrary, but nevertheless had to be imputed indirectly, on the basis of, among other things, their consequences in the form of actions. Like the attribution of dormitive powers, this was circular. But, as Weber formulated it, the relevant evidence included the "course of events" around an act, which is to say more than the bare fact of the effect.

Because Weber did not rely exclusively on this circular reasoning, and did not pretend that this reasoning could do the work of causal explanation, he avoided the problem of circularity. But this solution to the problem of compatibility works because he allowed probabilistic relations between classes of "causes" and "effects" described in the language of life (including intentions or "meanings"). The price of this solution is that these explanations, unlike those involving laws, cannot be derived from and thus explained by other, broader or more basic laws. They are causal but with respect to causality they are an explanatory dead end. With respect to "meaning," however, they are not a dead end. Meanings can be further explained. For example, they can be explained historically, in terms of varying beliefs and values, using the same basic framework and type of explanation. The "causal" content taken alone has no more empirical significance than Pearsonian correlations arbitrarily selected between variables. What makes them different is that considerations of "meaning adequacy" introduce a nonarbitrary method of selection from the huge class of actual probabilistic relations.

The Persistence of Teleology

Causal systems that are composed of or involve human action and social objects, such as institutions, practices, and societies, have continued to be the subject of disputes. But the discussions of human objects and social objects have different trajectories. In the remainder of the chapter I will concentrate on the problems of teleology in relation to social objects. But the domains are not independent, and one of the central problems with "social objects" is this: to what extent are they "real," or, put differently, do they possess any explanatory force beyond the elements of human action and physical causality that compose them? One view, also associated with Weber, is that they do not. Similar conclusions can be reached from rational choice premises. But these arguments depend on intentional explanation. So what is intentional explanation? Cause, teleology, or something else? Whatever the answer, it cannot be generalized to the problem of social objects.

Intentions require minds and, although talk of group minds or social intelligence was not uncommon a century ago, it is rare today. But it is not clear that the concepts that have been proposed to explain the social phenomenon that group minds formerly "explained" are free of the problems of group minds.

A *causal system* is a set of interlocking causal mechanisms with certain features. *Teleological causal systems* include feedback mechanisms (which produce adjustment or equilibration such that the system maintains itself or progresses toward a goal that is built into the system). These systems can be characterized as purposive or end-seeking or teleological. But "teleology" does not, so to speak, reside in the mechanisms, as social purposes were once thought to reside in the group mind or in a collective "intelligence." End-seeking is a property that adds no explanatory content – everything that happens does so because of the arrangement of causal mechanisms such as the feedback mechanisms that do the work of directing the system toward the end state. The "ends" are a consequence of the arrangement of mechanisms, rather than something that adds predictive power or explanatory force to the explanation. If social objects could be analyzed as systems of this kind, two of the problems of teleology – the issues of arbitrariness and circularity – would not arise.

The requirement of specifying feedback mechanisms, however, is a high standard, and defenders of particular collective social science concepts have generally dismissed or ignored this standard. Consequently they become entangled with the traditional problems of teleology. Consider the notions of racism, sexism, and oppression. These notions are not on the surface teleological. Sexism and racism are understood as features of individual attitudes, beliefs, and so forth, as well as of institutional practices that have oppressive consequences. The persistence of oppressive structures and practices, and their resistance to reform, seems like a depressingly familiar empirical fact that demands explanation, not an artifact of a medieval explanatory strategy. But matters are not so simple. The phenomena of "racism" and "sexism" are theoretical entities, underlying causes of the attitudes or practices. And they are identifiable *as* racist, sexist, or oppressive not because they are on their face racist, sexist, or oppressive, but because they produce these particular kinds of outcomes. The outcomes are very closely connected to our understanding of the phenomena itself. Perhaps they are related in the circular way that teleological "natures" were connected to "ends."[11] Suppose that any practice that results in any gender or racial difference or inequality is understood and characterized as sexist or racist. If an attitude or practice is defined as racist because of its effects, the explanation becomes circular, or true by definition: racism is whatever produces racist effects.

An influential defense of these kinds of arguments is found in Gerald Cohen's *Karl Marx's Theory of History* (1978:285, 289–96). Cohen defends these as *functional* arguments. But whether functional arguments avoid the traditional difficulties of teleology is controversial. Ernest Nagel attempted to restate teleological terms in nonteleological language.

> Consider, for example, the teleological statement: "The function of the leucocytes in human blood is to defend the body against foreign micro-organisms." Now whatever may be the evidence that warrants this statement, that evidence also confirms the non-teleological statement: "Unless human blood contains a sufficient number of leucocytes, certain normal activities of the body are impaired," and conversely. (Nagel 1961:405)

But *is* this language nonteleological? As Victor Gourevitch, commenting on this passage asks:

> In what possible sense are such terms as "sufficient," "normal activities," and "impaired" less teleological than the terms "function" and "defend" which they replace? Taken in and by itself alone a blood-count is a mere number. As such, it is wholly meaningless and uninformative. It yields information only when it is compared to a normal blood-count, that is to say to the blood-count of persons who are known to be healthy and whose bodies are *therefore* said to exhibit "normal activities." The knowledge that someone is healthy precedes and is independent of our taking their blood-count as a standard. It is pre-scientific, or first for us. (Gourevitch 1968:293, emphasis in original)

"The only real difference between Nagel's two statements," Gourevitch concludes, "is that the so-called nonteleological statement takes for granted what the teleological statement renders explicit" (1968:293).

As it happens, this is a commonplace feature of many arguments that do not present themselves as teleological arguments, or even as "functionalist" arguments. Consider a usage of the sociologist Pierre Bourdieu, "reproduction." Bourdieu believes that the social practices that produce the domination of one group by another are not only passed on in the course of education, but preserve an underlying logic of domination over time. All that such a "logic" could be, if it is not simply a nonexplanatory description of the effects of practices, is a kind of self-perpetuating force whose end is the preservation of domination. Is this a teleological usage? Clearly it is. Perpetuation is not a fact that depends on the intentional preservation of the "logic." Indeed, the logic necessarily operates behind the back of social actors, who must "misrecognize" its significance in order to carry out the practice. If the persistence of the practices that secure domination was wholly a result of particular causal conditions, there would be no point in appealing to the notion of a hidden logic. The system would be causal, but while the causes would produce and sustain the practices and the practices would have the effects, the effects would not be purposes. For Bourdieu, however, the effects are purposes.[12] Cohen's defense of Marx's functional explanations is designed to avoid these obscurities. He observes that "a benefit-statement assigns beneficial consequences to some item." He then says, let us generalize the question "What makes benefit-statements explanatory?" by asking instead: "what makes citation of consequences, be they beneficial or not, explanatory? What are the truth conditions of what we may call a *consequence explanation*?" (1978:259).

His answer is that they must depend on an empirical "law" governing the relation between consequences and the preceding things that they explain, a *consequence law*, which can be understood through "an analogy between '*e* occurred because *f* occurred, since whenever *F* occurs, *E* occurs' and '*e* occurred because of its propensity to cause *F*, since whenever *E* would cause *F*, *E* occurs'" (1978:261).

An example of such a consequence law can be drawn from anthropological functionalism. "Whenever performance of rain dance *R* would bring about, shortly thereafter, a rise in social cohesion, rain dance *R* is performed" (1978:261). Cohen thus overcomes the spookiness of future events causing past events by saying that there *is* a prior fact, a disposition:

> It is false that, in an explanation relying on such a generalization, the resulting social cohesion is put forth as explaining the performance of the rain dance. Instead, the performance is explained by this dispositional fact about the society: that if it were to engage in a rain dance, its social adhesion would be increased. . . . *It can be explanatory to cite the effect of the rain dance, not because its effect explains it, but because the fact that it had that effect allows us to infer that the condition of the society was such that a rain dance would have increased its social cohesion, and it is implied that the inferrable condition occasioned the performance of the dance.* (Cohen 1978:261–2)

Claims about this disposition can be stated as true generalizations.

One difficulty with all such arguments is familiar: circularity – they involve processes or dispositional facts that are accessible only by inferring their existence from their effects. The means by which the society's dispositions produce intentional actions, for example, are deeply mysterious. Do they enter into the heads of the dancers as individual mental causes or urges? And if so, how? If such questions are unanswerable, these explanations cannot easily respond to the charge that they result only in analogies. Moreover, because the relations in question are not particularly strict, they are open to alternative explanations. It must be said that Voltaire would have recognized these problems as close kin to his own, and Molière would have recognized these explanations as subject to the same weaknesses as "dormitive powers." And Cohen, like Strauss, can make the same response: If there were no law-governed disposition of this sort, there would be no predictable consequences of this kind. Cohen thus solves the problem of teleology by making teleological explanations dispositional, which is a weak form of explanation in part because of underdetermination: typically many alternative explanations of a dispositional kind also fit the facts. In the case of Marxian explanations there are some other difficult problems to handle. Are Marxian "predictions" readily falsifiable if they are flexible with respect to the time at which the predictions are to be fulfilled? (Elster 1982:478 n.8). Can any amount of present evidence disconfirm a prediction about the indefinite and perhaps infinite future?

Strengthening the explanations, by specifying their mechanisms, making them into "causal system" forms of teleology, faces other obstacles. "Dormitive powers" can be explained by mechanisms involving opium receptors in the brain. Similarly,

in the social sciences, "mechanisms," such as the stylized intentional explanations of rational choice theory, used to represent aggregations of intentional acts, such as markets, are used to account for such things as the market's disposition to seek price equilibrium. These intentional actions together compose an "invisible hand." But other collective concepts used as explanations seem to be wholly analogical and incapable of being broken down into plausible mechanisms. Consider Bourdieu's concept of practices (1977:59–60) or Mary Douglas's defense of Fleck's notion of "thought collectives" (1986:12–17, 32), the supposed common mental frameworks of thought in a community. Such explanations seem to require that the collective objects – such as practices and thought collectives – have effects on individuals, or operate causally within them. The mechanisms by which this is supposed to occur are mysterious. So one suspects that these explanations are dead ends, analogies that, unlike dormitive powers, cannot be made into something better. And, because they are weak, the results they "explain" are also open to many equally weak alternative explanations.

Notes

1 For a detailed discussion of Hobbes' various struggles with teleology see R. S. Peters (1967:43–91, 129–77).

2 A standard discussion of the problem of circularity with respect to dispositional statements is Carl G. Hempel (1965). Hempel distinguishes a "narrow" human and a "broadly" dispositional approach. A "broadly" dispositional analysis of rationality, for example, is not circular since claims can rest on different grounds in addition to the evidence given by instances of rational action, which assume rationality by definition and would, taken alone, produce a circularity (1965:473). This reasoning was later revised by Donald Davidson (1976), who abandons dispositions in favor of probabilities, which are independent facts and thus avoid circularity.

3 Kant critiqued such arguments in *The Critique of Judgement* (1988:257–61).

4 For an example of the unself-conscious reliance on teleological usages even in thinkers who prided themselves on "the purity of their empirical method," freedom from the "*esprit de système*" of the Scholastics and the natural law thinkers of the seventeenth century, and insisted that "they looked only at facts," see Manuel and Manuel (1979:464–5).

5 Schönfeld 2000:ch. 5.

6 As Jon Elster points out with respect to Pierre Bourdieu, the "as if" is instantly used in these texts. But Bourdieu, unlike Kant, uses this language in contexts that are not limited to "subjective understanding" (cf. Elster 1982:453–82, esp. 456).

7 Cf. McKim and Turner (1997), Freedman (1999).

8 Pearson's competitor G. U. Yule refined an alternative manner of reasoning about causality that operated in terms of estimating effects on the basis of a given set of variables minus the variable to be assessed, and attributing the difference between the observed and estimated effects to the included variables (Turner 1997:23–45). The statistical sources used by Max Weber, who will be discussed shortly, used analogous reasoning, but applied it to dependent probabilities rather than correlations.

9 It is striking that the method of inverse deductions deals with exceptions in a similar way.

10 Cf. Mill (1929).

11 These need not be teleological or dispositional concepts. Some thinkers about "white privilege" have managed to specify what privilege consists in by reference to such considerations as the likelihood that other *individuals* will not judge my actions as representative of my race or as a result of racial characteristics. These are not circular (McIntosh 1988) But they are at the level of individual action rather than that of social objects.

12 Elster discusses many similar examples from Marxist social science (1982), and considers Marx in detail (1985).

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Phenomenology and Social Inquiry: From Consciousness to Culture and Critique[1]

Brian Fay

Transcendental Phenomenology

At its inception twentieth-century phenomenology was a most unpromising place to inspire or underwrite a philosophy of social science or actual social-scientific undertakings. Edmund Husserl is generally taken to be the founder of twentieth-century phenomenology, and one of his deepest intuitions was that to psychologize or historicize philosophy is a fundamental mistake. Philosophy should be concerned with a transcendental realm that is utterly different in kind from the mundane realm of human experience and behavior studied by social inquiry.

The nature of this transcendental realm can be appreciated by reflecting on the question: what is the basis of logic, mathematics, or science? This was one of the questions with which Husserl explicitly concerned himself,[2] but this question can be expanded to the more general question, what is the ground for the meaningfulness and normativity that operates in all human activity? Husserl believed that a suitable answer to this question could not be located in the contingencies of actual thinking, speaking, and acting because it is these contingencies themselves that are in need of grounding. To ground science by an appeal to scientific practice, for instance, simply begs the question at issue. What is needed is a realm beyond the contingent human world on which this world is in some sense dependent, and that could serve as a proper ground of the normativity and meaningfulness inherent in human experiences, doings, and practices.

Husserl proposed that to discover this realm philosophy needed to attend to consciousness. For Husserl the starting point of phenomenology is the fact that

we humans exist as conscious subjects. As such, we possess what Husserl called the "natural attitude" which is the point of view that all of us cannot but adopt in the course of our everyday lives. How is this natural attitude to be understood? Husserl argued that to do this we must hold aside the everyday notions of the natural attitude and reflect on the notion of consciousness that makes our world a *lived* world:

> *Instead of remaining at the (natural) standpoint . . . we set it as it were out of action. We disconnect it, "bracket" it.* It still remains there like the bracketed in the bracket, like the disconnected outside the connexional system . . . *but we make no use of it.* Thus all sciences which relate to this natural world . . . though I am far from any thought of objecting to them in any degree, *I disconnect them all, I make absolutely no use of their standards. . . .* (Husserl [1913] 1931: sections 27–32. Italics in the original)

This is the so-called *phenomenological epochē* that begins with actual consciousness but disconnects it from its involvement in the world and its concern for the reality of its objects. The result is an examination of consciousness without reference to any actual experience or any particular objects of that experience. To do this is to move away from the particular individual consciousness that I am to a transcendental conscious field in terms of which any individual consciousness is made possible.

What remains when this movement to transcendental consciousness occurs is a structural correlation between any conscious act or *noesis* and its noematic sense, the object as intended and grasped through the act. But even this structure can be bracketed in the so-called *eidetic reduction* to ascertain the basic forms (or "essences") in terms of which experience is constituted and made possible. It is these essences that provide the basis of any conscious experience whatsoever. For Husserl, phenomenology is therefore ultimately a science of essences rather than a science of concrete human experience.

Thus, to the question of the basis of normativity Husserl answered that only by rooting actual, temporally located human experiences in the atemporal, immaterial necessity of the world of essences could we understand the force of logic, science, and the other meaningful structures in virtue of which human experience and interaction operate. But it is precisely the material, temporal world with which social science is concerned. So phenomenology conceived transcendentally cannot be a guide for such a science (though Husserl argued for its normative role in clarifying its concepts and grounding its practices). By taking the transcendental turn of Husserl, phenomenology not only seems to leave the science of empirical, actually existing human beings bereft of any real guidance, but it suggests that a scientific study of empirical consciousness is a category mistake in which nonnatural phenomena are studied by strictly natural sciences.[3] (It is for this reason that Husserl was antipathetic to Gestalt psychology, which might appear to grow out of a broadly phenomenological approach.[4])

But phenomenology did not remain transcendental forever. Indeed, in its second generation crucial changes occurred whose effect was to render phenomenology useful for the study of actual human beings. Husserl's student Heidegger changed transcendental phenomenology into what has come to be called existential phenomenology. Alfred Schutz began on a different footing than Heidegger by attempting to work out problems in Weber's theory of action and *Verstehen*, but he turned to the task of making Husserl's work applicable to social inquiry as a way of solving these problems, and in the process fundamentally changed phenomenology into a phenomenology of the actual world of lived experience. And Maurice Merleau-Ponty, addressing what he saw as the inappropriate scientism of contemporary psychology (in both its sensationalist and Gestalt versions) focused phenomenology on the actual body and bodily comportment of human subjects; in so doing he opened up important resources for the study of human consciousness and activity. Through the work of these thinkers, transcendental phenomenology became existential phenomenology, and thereby became relevant to social inquiry.

From Transcendental Phenomenology to
Existential Phenomenology

These changes were not merely external to phenomenology, but in fact were already implicit in Husserl's phenomenology. Or so I shall now attempt to show.[5]

The problem that Husserl's transcendental phenomenology faces is this. The necessary structures that it uncovers must be instantiated in actual social life in order for them to do the work for which they were intended, namely to ground logic, science, and the normativity in virtue of which the *Lebenswelt* is possible. Actual logicians, for example, have to be able to at least implicitly employ the necessary structures of transcendental consciousness in order to differentiate between logical and illogical inferences, and actual everyday speakers have to be able to recognize at least implicitly the normativity of linguistic rules in order to be able to distinguish between meaningful speech and meaningless gibberish. But what is the nature of this instantiation? Perhaps it is itself a necessary relation, as Husserl thought. But then the question arises: how is *this* necessity instantiated? Moreover, it is completely unclear how *a priori* necessities of transcendental consciousness can have any psychosocial reality.

Heidegger forcefully made this point. He showed this by demonstrating that Husserl's account of perception cannot achieve what it set out to do. To adopt his phenomenological *epochē*, Husserl had to claim that the phenomenologist could separate out from an act of perception various elements of it – such as the being of the object of perception, and the various parts of an object distinct from one another. But this isn't in fact possible: when we perceive an object we necessarily perceive it as existing, and its various parts as necessarily related to

each other as parts of a whole. This means that phenomenological reduction in the case of perception makes no sense. As Joseph Rouse puts it:

> On any plausible account, perception gives its objects *as* being materially present, as existing in space and time. . . . So the material existence of the perceived object and the perceived body belongs to the sense of what is perceived . . . the material existence of perceived objects and perceiving bodies is an *essential* component of any perceptual presentation whatsoever. And that means that the phenomenological reduction, which purported to suspend any concern with the actual spatio-temporal existence (or any other mode of "actual" existence or non-existence) in order to reveal the intending and intended *sense* of perception, instead abolished its perceptual character. (Rouse forthcoming)[6]

The result of this is not just a minor point about perception, but a devastating critique of the entire enterprise of transcendental phenomenology. Perception undergirds most human experience, so that the inability to "bracket" it in the relevant way means that the turn to an atemporal, immaterial realm of necessity makes it impossible to ground actual practices, which is phenomenology's aim. In order to accomplish this aim phenomenology must perforce turn to the concrete, physically and social situated realities in which we find ourselves – to the phenomenology of actually existing beings – in order to render it clear how such beings experience, think, and relate to their world.[7]

This is precisely what Heidegger does in *Being and Time* ([1927] 1962). There he analyzes not "consciousness as such" but *Dasein*, that is, an active being engaged in the world whose being is a question for itself. *Dasein* is not pure mind, or consciousness, nor can the fact of its interaction with the world be bracketed in order to illuminate its nature. To understand *Dasein* one cannot suspend its practical engagement in order to discern the "essential structures" in virtue of which it has the being it has; to do so would make it invisible precisely because it *is* these engagements.

The shift in terminology from Husserl's "transcendental consciousness" to "*Dasein*" indicates the profound shift in phenomenology brought about by Heidegger. Phenomenology is brought back to the real world of active beings interacting with their environment (whose nature is itself revealed by means of their concerns and actions on it). The job of phenomenology now becomes to reveal the basic elements of *Dasein*'s existence, not to ground this existence in some realm of necessity beyond it. Hence the term "existential" comes to replace "transcendental" as the modifier that describes what phenomenology is up to.

Of course, Heidegger didn't just show that phenomenology must be existential; he also put forward his own account of the fundamental structures of being for a creature whose being is a question for itself. Thus he offered a detailed analysis of "being-in-the-world" and "being-with-others." He interpreted the existence of *Dasein* as fundamentally temporal, as being engaged in a constant

projection to the future and a constant reassessment of the past. Situated in time and aware that it is, *Dasein* is necessarily aware of the possibility of its own death, and this awareness structures all of its feelings and activities. A general anxiety saturates *Dasein*'s projects; indeed, the driving force behind its everyday activities is the attempt to escape the possibility of its own nothingness (though for these activities to be successful they must be disguised as something else: otherwise, in the very act of trying to forestall a fall into nothingness one would be constantly reminded of it). But throwing oneself into projects in this forgetful way is a mark of inauthenticity, and the goal of existential phenomenology is to reveal the anxiety at work in *Dasein*'s life and thereby open up the possibility for it to live a genuinely authentic existence.

Given this general approach, it should not be difficult to see how it inspired what came to be called *existential psychology*.[8] This is an attempt to understand the human psyche in terms of the basic problems and resources that it has "existentially" – that is, not as a result of its being a particular person at a particular time but of being a conscious being engaged in the world. Existential psychology attempts to understand the activities and conscious states of people – including their neurotic activities and feelings – in terms of their coping with basic human problems that occur simply by virtue of their being human.

Meanwhile, in the realm of phenomenology proper, while Heidegger was working his way out of (or down from) the transcendentalizing of Husserl, Alfred Schutz essentially ignored this aspect of Husserl's thought. Schutz set himself the task of elaborating on Weber's sociology and putting it on a firm footing, and for this he turned to Husserl. But in this attempt to make phenomenology useful for social analysis the transcendental features of Husserl's thought were beside the point. From the outset Schutz conceived phenomenology as the investigation of the "natural attitude" and the *Lebenswelt* themselves.[9]

Schutz's greatest and most influential book appeared in 1932, later translated as *The Phenomenology of the Social World*.[10] Following Weber, Schutz roots his analysis of social inquiry in the concept of social action, and since actions are constituted by and express meanings of the social actor, it is the concept of meaning which is at the center of his thought. I shall discuss his account of meaning in the next section, but the crucial point here is that for Schutz *meaning is constructed by social agents themselves*. This is crucial because it leads to a fundamental point regarding social life: social behaviors and relations are active creations of the social agents involved. Actors construct social reality, such that any view of social agents as mere passive objects on which social structures imprint themselves is deeply mistaken.

The question for Schutz, given his approach, is how others besides the individual agent are involved in the construction of meaning. A good deal of the *Phenomenology* is devoted to showing how individual subjects can and indeed must understand the meanings of other individual subjects. In this, the role of the *Lebenswelt* is critical. The life-world encompasses the cultural, taken-for-granted framework of social life in virtue of which agents can understand and interact

with one another. This life-world consists of the knowledge of certain skills, typifications, and routines that are socially prescribed and distributed. These can all be studied by means of ideal-types that are the chief tool by means of which social analysis can uncover the meanings operative within the social world.

Schutz's work has led to two different, though broadly related, approaches in sociology: phenomenological sociology and ethnomethodology. The founders of each of these approaches studied with Schutz, and his influence is readily apparent in their work. Both approaches attempt to make Schutz's work operational in a rigorous, empirically grounded study of social actions and its products; also, both attempt to move social analysis away from the individual social actor and more toward social structures and institutions. Phenomenological sociology received its classic expression in Peter Berger and Thomas Luckman's *The Social Construction of Reality* (1967)[11] The title alone suggests its phenomenological roots and its main point: that even when considering the so-called "objective" cultural products such as language, religious systems, art objects, and so forth, at work in and through them is the creative activity of social agents interpreting and applying them to their particular social situations.

Ethnomethodology literally means "the study of ethnomethods" – the procedures by which people interact with one another and make sense of these interactions. Its founder was Harold Garfinkel, who developed this approach in the 1940s; it received its classic statement in his 1967 *Studies in Ethnomethodology*. Ethnomethodologists seek to render the taken for granted in everyday social life problematic in order to reveal the operations at work in ongoing social interactions and the rules and assumptions that underlie these operations. In order to do this ethnomethodologists have constructed ingenious procedures (e.g., so-called "breaching experiments") that interrupt the normal flow of social life in order to reveal the nature of its taken-for-granted routines. They have also studied social situations (such as the death camps, or the experience of immigrants) in which the taken for granted is absent as a way both to throw light on this taken for granted, and as a way to study the means by which social actors construct meaning.

Ethnomethodology is also intent on rendering its own renderings problematic – the operations by means of which social analysts conduct their enterprise, and the rules and assumptions in virtue of which these are done. Ethnomethodology is therefore inherently self-reflexive. Ethnomethodologists study not only everyday people, but they study themselves studying everyday people – as they must, since both groups are engaged in the operations by means of which they make sense of their world and make it an ongoing reality.

In addition to *The Phenomenology of the Social World*, the other great work in phenomenology during the period in which phenomenology became focused on the lived world of actual people is Maurice Merleau-Ponty's (1945) classic *The Phenomenology of Perception*.[12] Although phenomenology had not neglected the body – Schutz, for instance, discussed what he called "bodily presence" or "corporeal givenness" in trying to account for the possibility of understanding

the subjective meanings of other agents, and Husserl made bodily perception a subject of *Ideen II* ([1989] 1952) – it is in this book of Maurice Merleau-Ponty that the body is given a full phenomenological treatment, and is given a central role in understanding the nature of consciousness.

Merleau-Ponty agreed that the phenomenological *epochē* should be applied to natural science and its findings, but insisted that it could not be applied to the prescientific knowledge of the world, particularly that provided through perception. (In this he echoes Heidegger, as I showed above.) When this is done, and phenomenologists turn their attention to the lived experience of an embodied subject, a number of important truths reveal themselves. First, the body is not a purely physical entity to which consciousness is attached, but is the medium through which consciousness lives and breathes. Second, the body is not merely an individual thing (and certainly not a thing among things) but is as much a social entity as it is an individual one: the body is always a body-for-others. And third, consciousness in general and perception in particular is not simply a mental operation on the basis of which consciousness directs or uses the body: consciousness is always situated in space and located in time; indeed, "situated" is too passive a description, for consciousness is always a practical engagement with the world. Far from being an add-on to consciousness as the Cartesian tradition insisted, the body is the center of mental life. Human being for Merleau-Ponty is always "being-in-the-midst-of-the-world," essentially social, essentially practical, and essentially embodied. Husserl's attempt to get at the core of human existence by abstracting out these features is therefore fundamentally misguided because it abstracts out what cannot be abstracted out if one wants to understand human existence.

Merleau-Ponty has had an important influence on later psychologists and social scientists. One of the more interesting of these is the loosely defined group of "anti-Cartesian" cognitive scientists.[13] Those who subscribe to this approach argue that understanding the human mind cannot be done without according the human body a central role. In so doing they take explicit aim at the widespread view that intelligence in general and cognition in particular can be understood in terms of information processing à la digital computers, in which the body and bodily skills have no essential role to play. Thus, for example, in a book representative of this approach, Kenneth Shapiro (1985) explicitly attempts to transform Merleau-Ponty's insights into a full-fledged empirical psychology, asking how experience is meaningful – it is in part because it is embodied – and applying his answer to this question to the phenomenon of ambivalence.

In the philosophical work of Heidegger, Schutz, and Merleau-Ponty the transcendental phenomenology of Husserl, concerned with grounding the actual world in an atemporal realm of necessity, was transformed into an existential phenomenology concerned with the structure of the lived world of actually existent, embodied agents. This transformation rendered phenomenology directly suitable for empirical social analysis.

From Phenomenology to Hermeneutics

Despite the inspiration that phenomenology provided to social analysis in the twentieth century, a curious fact shows itself if one looks at relatively recent important works in the philosophy of social science: phenomenology is scarcely mentioned.[14] Part of this may be because some of its lessons have been so well learned that it no longer seems necessary to repeat them; here phenomenology has become a figure hidden in the carpet of contemporary philosophy of social science.[15] The contemporary silence about phenomenology might also be the result of the domination of English-speaking philosophers ignorant of or anti-pathetic to "continental" philosophy. But while it is true that some have regarded phenomenology as a European tributary in which somewhat eccentric European émigrés swam, a perusal of the works cited in note 14 shows this is not the case with them. A deeper and more philosophically interesting reason for this neglect might be at work. In our time language has replaced consciousness as the basic focus of analysis, with profound consequences for how the task of social inquiry is conceived.

Phenomenology is essentially centered on the analysis of consciousness. This means that linguistic meaning in particular, and meaningfulness of experience, actions, and relations in general, must be accounted for in terms of the activity of consciousness. This is precisely what Husserl and Schutz did when they provided a theory of meaning.

For them, meaning derives from internal time consciousness. Humans not only live in a world of change – as does everything else on the planet – but are aware of this temporality. Indeed, the awareness of humans is temporal in its very core. As essentially intentional, human consciousness is directed toward projects into the future, and aware of where it has been in the past. Meaning derives from this essentially temporal character of consciousness; as Schutz put it:

> I can turn my glance toward the intentional operations of my consciousness . . . then I no longer have before me a complete and constituted world but one which only now is being constituted and which is ever being constituted anew in the stream of my enduring ego: not a world of being, but a world that at every moment is one of becoming and passing away – or, better, an emerging world. As such, it is meaningful for me in virtue of those meaning-endowing acts of which I become aware by a reflexive glance. (Schutz [1932] 1967:35–6)

Accordingly, Schutz defines meaning as "*a certain way of directing one's gaze at an item of one's own experience*" ([1932] 1967:42, italics in the original).

Notice here a feature of this account of meaning that makes it particularly unsuitable for social analysis. This feature is its solipsism. Husserl's project was to build up the meaning of human doings (science, logic, speech, actions) by bracketing them in order to gain entrance into the sphere of the transcendental ego. In *The Phenomenology of the Social World* ([1932] 1967) Schutz followed this same

path, commencing with the solitary ego and then introducing features to construct the social world.[16] But starting out with the isolated ego in this way, the problem arises as to the existence and role of other isolated egos and the possibility of communication among them.

Schutz's and Husserl's conception of meaning rests on the individual experience of the solitary ego. But this conception is seriously deficient: the consciousness of an isolated individual cannot be the basis for an adequate theory of meaning. Moreover, this means that the basis for their general theory of society – and for the accounts of social inquiry that draw on this theory – is problematic.[17] Moreover, in order to correct this problem the very basis of phenomenology is brought into question, and the entire enterprise is transformed into something else. This something else is hermeneutics or interpretive social science.

The problem with Schutz's account of meaning is that the intentionality that serves as its basis is an essentially social phenomenon, in a very particular way. I can have intentions only because I am already situated within a web of meaning provided by my language and the culture within which I reside. Far from linguistic meaning deriving from my intentionality and my subjectivity, the opposite is the case; as Habermas has put it:

> Just as for Cassirer language, as *one* symbol system among others, is grounded in the representational function of consciousness, and the structuring of consciousness cannot be derived from linguistic communication, so for Husserl and Schutz as well, linguistic symbols are grounded in the comprehensive appresentational activity of the transcendental ego. Monadological consciousnesses spin linguistic intersubjectivity out of themselves. Language has not yet been understood as the web to whose threads subjects cling and through which they develop into subjects in the first place. (Habermas 1988:116–17)

In the English-speaking world the philosopher most responsible for showing the essentially social and public character of meaning is the Wittgenstein of the *Philosophical Investigations* ([1953] 1958).[18] Though Wittgenstein does not address Husserl or Schutz, what he says there is devastating for their entire project of accounting for meaning in terms of the acts of an individual consciousness. In the so-called private language argument Wittgenstein shows that even with regard to one's own inner conscious states an isolated subject could not construct a meaningful language. Language requires rules, and rules require others as checks to insure they are being followed.

But even within the phenomenological tradition the notion of meaning underwent a change, one wrought by Heidegger. For Heidegger understanding is a mode of *Dasein*'s being in the world, not simply an epistemological stance *Dasein* takes towards this world. Moreover, understanding and the meaning it uncovers are embedded within the fabric of social relations. For this reason the interpretation of meaning consists of rendering explicit what is implicit in these

relations. Far from disengaging ourselves from our involvement in the everyday world by repairing to the world of pure consciousness in order to grasp the (sources of) meaning of this world, as Husserl demanded, Heidegger argued that we must in fact involve ourselves in this world to understand how meaning is in it.[19]

Meanings do not exist within the minds of (transcendental) egos, but are located in the shared domains of activity on the basis of which human beings act and relate to one another. The source of sense is not the individual (transcendental) consciousness, but *Dasein* in its ongoing engagements with the world and its ongoing social practices.

But this means that the focus of social investigation must move away from the consciousness of the individual to the broader social world and its history on which the individual draws to be conscious in the first place (insofar as consciousness involves sense-making). To understand an individual consciousness one must situate it within the broader cultural and historical world in which it lives and breathes. This cultural world consists of language, of social rules and institutions, of cultural objects like houses and city streets and artworks, of ideologies, and so forth, all of which are situated within ongoing historical traditions. Meaning is a broadly historical-cultural phenomenon, not an (individual) psychological one, and the interpretation of meaning (and the search for its source of meaning) must turn from the minds of people to the historically situated public world of shared practice.[20]

How nicely this is all put in the famous essay "Thick description" by Clifford Geertz, the anthropologist most responsible for initiating the so-called interpretive turn in anthropology in particular and the social sciences in general:

> . . . To say that culture consists of socially established structures of meaning in terms of which people do such things as signal conspiracies and join them or perceive insults and answer them, is no more to say that it is a psychological phenomenon, a characteristic of someone's mind, personality, cognitive structure, or whatever, than to say that Tantrism, genetics, the progressive form of the verb, the classification of wines, the Common Law, or the notion of a "conditional curse" . . . are. (Geertz 1971:12–13)

As a result interpretive social analysis does not consist of delving into the psyches of individuals but of deciphering the public world of culture which consists of structures of signification. This involves attention not to the experience of social agents hidden in the inner recesses of their minds but to the concrete actions, relations, and objects of particular people in particular places. Nor is the ground of meaning to be located in the individual (transcendental) consciousness, but in the public world of ongoing social interaction. Though actually addressing structuralists and linguistic formalists, Geertz seems to be speaking directly to Husserl when he rejects the transcendental approach to meaning that Husserl proposed:

> To set forth symmetrical crystals of significance, purified of the material complexity in which they were located, and then attribute their existence to autogenous principles of order, universal properties of the human mind, or vast a priori *weltanschauungen* is to pretend a science that does not exist and imagine a reality that cannot be found. Cultural analysis is (or should be) guessing at meanings, assessing the guesses, and drawing explanatory conclusions from the better guesses, not discovering the Continent of Meaning and mapping out its bodiless landscape. (Geertz 1971:20)

This passage might well serve as the inscription on the tombstone of a social science inspired by phenomenology devoted to rooting the world of meaning in some atemporal realm of necessity instead of the shared practices of concrete individuals, or of directing attention inward to the conscious mind of the individual instead of outward to public behavior and public objects.

From Phenomenology to Cultural Analysis

Besides the replacing of consciousness by language and culture as the focus of social inquiry, another reason for the apparent irrelevance of phenomenology for the theory of social inquiry is its deeply ahistorical character. In recent times the historicity of human being in general and human consciousness in particular – historicity understood as being a product of a *specific* historical situation – has come to be appreciated.[21] The result is that enterprises that think that human beings can be understood independently of their particular sociocultural and historical location have come to be suspect.

The phenomenology of Husserl is obviously ahistorical – it seeks to ground human being in the transcendental ego and the realm of essences. Even when Husserl turned his attention to the *Lebenswelt* and history his goal was to reveal the basic structure of consciousness as such. The case of Schutz is more ambiguous on this score, but generally his aim was to uncover structures so fundamental as to be constitutive of any form of recognizably social life. Moreover, this ahistorical goal is to be found even in Heidegger's early existential phenomenology, with its insistence that the being of *Dasein* is essentially situated within time. Heidegger, too, wanted to say something about human being as such. He spoke about *Dasein* in the abstract, not this or that particular *Dasein* (or instantiation of *Dasein*). And Merleau-Ponty, for all his appreciation of the temporal and spatial stiuatedness of bodies, still spoke of "the human body" and "human perception." In all these cases, the idea is for phenomenology to distinguish structures that are universal, permanent, and even necessary.

But given what I said in the previous section about the ways subjectivity is constituted out of the web of cultural meanings in which it is situated, this idea

is doomed to fail or to be of limited value. The citizens of ancient Rome significantly differ from the citizens of modern Rome, and the Azande live importantly different lives from the habitues of Rodeo Drive in Beverly Hills. True, there may be some important ways all of these people are similar, but only if one abstracts out what is distinctive and interesting about them; and even then, what is left are general observations that often seem relatively contentless. Historians or anthropologists or sociologists want to know how the structures that characterize different periods and cultures, and that underwrite particular identities, arose, are maintained, and flourish or decline. Even with the human body itself, specific cultural location is critical – as Foucault (1979) has shown.

Moreover, contemporary philosophers and social scientists are suspicious of talk of "we" in phrases such as "what we take for granted" or what "we regard as normal." Who, they want to know, is this "we"? Class, gender, national, racial, religious, and other differences – not to speak of differences deriving from different historical epochs – can so distinguish people (indeed, can so delimit the range of possible options for the kind of people they can be) that to speak as if "we" are all of a piece is to be profoundly insensitive to these differences. In an intellectual (and political) world where the importance of difference is one of the key points on its agenda, any phenomenology that sees its task as describing the basic structures of consciousness, human being, or the human body "as such" will seem irrelevant and ultimately wrongheaded.

Or, to put the point more charitably, what will be emphasized in phenomenology is its call to ferret out the schemes of interpretation, forms of typification, and systems of relevance at work within particular forms of life. In particular, what will be emphasized is the taken for granted in a form of life, and the ways this taken for granted can be revealed as constitutive of it. After all, one of the deepest impulses of phenomenology is its call to render the given problematic, and thereby to reveal its character. Ethnomethodology, with all its brilliant ways of bringing social actors up short and thereby revealing the nature of what they think is "commonsensical" and "obvious," is a particularly vivid use of phenomenology to engage in cultural analysis.

Moreover, phenomenology also has a particular insight to add to this project of illuminating the taken for granted, and indeed for all cultural analysis. This is the idea that consciousness is not a mere container waiting to be filled up, nor is it an object upon which, for instance, culture leaves its imprint. Consciousness is an activity, and the meanings in terms of which it lives are constituted out of this activity. Too often cultural analysis today speaks of people as mere things (or "dopes," to use Garfinkel's colorful term) waiting to be turned out rather like cookie dough waits for the cookie cutter to turn it into a shape of a particular sort. This sort of approach is an example of what Sartre aptly called "bad faith," in which the responsibility for personal identity and one's own actions is denied, fobbed off onto others or one's "society" or "the system." Sartre's term is a direct expression of the phenomenological tradition within which he worked, its insistence on the active character of consciousness.

From Phenomenology to Critique

Phenomenology is essentially descriptive: its goal is to lay bare the fundamental structures of consciousness and the instantiations these structures assume in the human body as well as social institutions and objects. But this has seemed to some too narrow a conception of what social analysis should consist of. For it assumes the veridicality of these structures and their embodiments in the sense that by grasping them the nature of the experiences, relations, actions, and practices of the social agents involved is revealed. But mightn't these structures of meaning *conceal* reality as much as reveal it?

To think so is to return to an earlier conception of phenomenology, that found in Hegel's great book whose title uses the term.[22] Indeed, in the *Phenomenology of Spirit* ([1807] 1977) Hegel had already both laid the foundation for phenomenology as it came to be conceived in the twentieth century, and at the same time provided a basis for a fundamental criticism of it. Hegel practiced phenomenology in a way that rendered it not only descriptive of what had historically occurred, but critical as well.

In the *Phenomenology* Hegel traces the journey of Spirit (reason, freedom) as it matures. Maturation on this account consists of two aspects of Spirit finally coming together at the moment of maturity: one is the objective meaning of the process as various conceptions of reason and freedom are developed over time and take their place in the world; the other is the awareness of this process as being the process it is. The former Hegel calls the "in-itself"; the latter the "for-itself." For history up to the time of Hegel the "in-itself" has diverged from the "for-itself"; that is, what people thought was the case about themselves in particular and history in general was at odds with what in fact was occurring. People didn't know that they were part of a process whereby Spirit self-consciously came to maturity through their own efforts. Only at the moment of enlightenment do individuals – and therefore Spirit itself, since Spirit is only conscious of itself in and through the consciousness of individuals – realize that their story is the story of coming to know what they are and embodying this identity in their actions and practices. At this moment the "in-itself" becomes identical with the "for-itself."

Since maturation consists in part of the story whereby Spirit becomes conscious of itself in and through the consciousness of individuals, any account of this process must include a description of the process of the various stages whereby consciousness developed. In other words, any account of maturation must consist in part of a narration of the stages of the "for-itself," of the way individuals' identities and reality appeared to the protagonists of this process – of, therefore, a *phenomenology* of Spirit. (Hegel also believed that this process of development unfolded according to its own internal logic, and that, therefore, any phenomenology must also depict the inner principles of development that underlay the movement from one stage in the growth process to the next.) So here is a basis for the importance of phenomenology for philosophy and for history: in order to

understand the nature of reality we must understand the nature of the way reality appears to those who experience it. Failing this, we would fail to comprehend a vital aspect of reality itself, since reality is in part comprised of the way it appears and reaches its fullest expression in and through the consciousness of particular beings capable of recognizing that reality is in part their coming to be aware of its nature. Phenomenology is therefore a necessary part of any philosophical account of reality.

But it is not sufficient in itself, if Hegel is correct. For during the long process of development individuals' consciousness of reality is at variance with reality itself – the "in-itself" is different from the "for-itself." On Hegel's view, in its later stages Spirit only exists in and through the consciousness of individuals, but these individuals don't know this, don't know that they are agents whereby Spirit is moving to its own completion. They misidentify who they are, and they misunderstand the nature of the process of which they are an important if unwitting part. For this reason any account of Spirit must include, in addition to a delineation of the way it appears to its agents, an account of the way these agents are in ignorance of its (and consequently their) nature. The *Phenomenology of Spirit* is not only about the appearance of reality but of the true path whereby reality comes into its own – it is not only an account of the consciousness of those who are agents of its maturation, but of the way this consciousness is mistaken. In other words, the *Phenomenology* is not only about the "for-itself" but also about the "in-itself," and about their divergence throughout history. (Only in the end do the "in-itself" and the "for-itself" coalesce, such that a depiction of one is *ipso facto* a depiction of the other.)

Understood in this way, the *Phenomenology* offers a critique of consciousness as much as a depiction of its basic structures and content. The message here is clear: though any account of reality must include a fundamental role for an outline of the way reality appears to its participants, it must also include a critical evaluation of this appearance. Students of history must therefore adopt a double vision: one part enters into the lived world of its participants and portrays the way this world is experienced and made sense of by them; the other part observes this lived world from the outside, noting the illusions of its members and the ways their sense of who they are and what reality is diverges from who they actually are and what its true nature is. Philosophers, or any students of history, must contend with the experience of those who are part of it, but they cannot be content with this: they must also note the ways this experience is deficient, hiding from itself what in fact it is.

Put succinctly, on Hegel's view phenomenology (as understood in the later, twentieth-century manner) is not enough if one wants to understand reality; any phenomenology must also be accompanied by a critique of phenomenology if the full picture is to come into view. Thus, though Hegel laid the groundwork for phenomenology, showing how and why it must be a crucial feature of any attempt to understand social reality, he also laid the basis for a criticism of phenomenology, namely, that the way reality appears may be at odds with what

reality is. In these cases students must go beyond phenomenology to criticize its contents and presuppositions of the experience of those they are studying. One lesson of Hegel's *Phenomenology* might be summed up as "from phenomenology to ideology-critique." (It is thus no accident that Hegel inspired a raft of so-called "left-Hegelians" whose program consisted of revealing the ways in which the ideas constitutive of social practices were illusions, with the hope that in so doing these illusions would be replaced with a consequent revolution in the social practices they underwrote.)

It is worth recalling here Marx's description in the Preface to *The German Ideology* (1972) of what he called the "innocent and childlike fancies" of these self-same left-Hegelians. He claimed that they naively assumed that genuine critique could consist merely of the critique *of ideas* – as if social reality consisted entirely of ideas about it. He accused critical phenomenology – if one may use this term – of failing to situate consciousness and its ideas in material and social processes marked by unequal power relations, and thereby failing to grasp the way social structures called for social illusions and thereby reinforced them.[23] In general, Marx argued that a genuine understanding of the meaning of ideas required an account of their social role and their social basis.[24]

This was a call to transcend phenomenology, however critically conceived, by marrying it with economic and sociological analysis of the social structures in which these ideas are situated. Phenomenology, once it has grasped the possibility of illusion and the need to interpret structures of meaning as deceptive as well as revelatory, points to a form of analysis for which it itself does not provide a basis: the causal analysis of the conditions of meaning, of the conditions in which meanings become illusions, and of the conditions that must be changed to enlighten those in the grip of such illusions. Phenomenology thereby becomes not just ideology-critique but a full-fledged critical theory that combines meaning and causal analysis in a very distinctive way.

Something of this same movement – from Hegel to Marx – can be observed in the recent history of ethnomethodology. In his famous presidential address to the American Sociological Society in 1975 Lewis Coser attacked ethnomethodology, claiming that the ethnomethodological approach ignores "institutional factors in general, and the centrality of power in social interaction in particular" (1975:696). This clearly echoes Marx's criticism of the left-Hegelians in *The German Ideology*, and it is perhaps no accident that it is this same text of Marx's that Dorothy Smith cites as a source of inspiration, along with Garfinkel's *Studies in Ethnomethodology,* for what she calls "standpoint theory." Standpoint theory attempts to marry phenomenology and critical theory to create a critical account of women's experiences as being located within the larger structures of male domination (Smith 1989).

> I began thinking through how to develop sociological inquiry from the site of the experiencing and embodied subject as a sociology from the standpoint of women . . . we began to discover that we lived in a world put together in ways in which we had

very little to say . . . (that) we had taken from the cultural and intellectual world created by men the terms, themes, conceptions of the subject and subjectivity, of feeling, emotion, goals, relations, and an object world assembled in textually mediated discourses and from the standpoint of men occupying the apparatuses of ruling. (Smith 1990:1–2)

Having recognized the ways in which the experience of women, as well as the ways this experience is described by them, are tainted by being left out of the gendered relations of ruling, sociology cannot be content simply with elucidating the basic structures of women's lived experience. It also makes these structures problematic by showing the ways in which they distort what is really going on, and by showing that these distortions have causes in the structures of power operative in a patriarchal society. Thus, though beginning with a phenomenological approach, standpoint sociology must go beyond it. As Smith has put it, in a way that clearly reveals the movement from a purely phenomenological approach to one that marries it with a critical theory that focuses on structural conditions of power:

> The knowing subject is always located in a particular spatial and temporal site, a particular configuration of the everyday/everynight world. Inquiry is directed toward exploring and explicating what she does not know – the social relations and organization pervading her world but invisible in it. (Smith 1992:88)

Conclusion

At the beginning of the twentieth century phenomenology emerged out of the desire for a pure philosophy which would establish an utterly clear and self-evident basis for human knowledge and activity. In this it was essentially Cartesian in inspiration. But as the century moved on, phenomenology was dragged down from the empyrean heights into the muck of human existence. It was transformed into an inquiry into the basic structures of human existence, central as these are for understanding the activity that is human consciousness. So transformed, phenomenology inspired a number of important attempts to grasp the basic structures of human existence and human social life. But even this was too abstract, and later thinkers would insist on the location of consciousness within a particular historical cultural and social situation. This led those convinced of the importance of meaning for an understanding of human life to turn away from individual consciousness and from the search for timeless structures to the interpretation of particular cultures. Furthermore, phenomenology's essentially descriptive aspirations to uncover the basic structures of consciousness and society were seen by others to be too limiting, given that these structures, contingent and located within relations of unequal power, might hide as much

as reveal the nature of the life of those living in terms of them. Consciousness can be false as well as true, and any endeavor attempting to understand it had to be able to draw this distinction. Social analysis cannot just be descriptive, it must also be (possibly) critical. Phenomenology must therefore give way to the critique of ideology and to the critique of the power relationships that sustain this ideology.

The movement from describing the transcendental subject to the critique of particular forms of false consciousness is the history – or at least one way of construing the history – of phenomenology in the twentieth century.

Notes

1 I would like to thank Joseph Rouse for his help in writing this essay, most especially for his perceptive comments on an earlier draft.

2 In asking and attempting to answer this question, Husserl first tried in his 1891 *Philosophie der Arithmetik* to apply Brentano's account of psychology to mathematics (see the relevant parts in *Early Writings in the Philosophy of Logic and Mathematics* 1994). But under the criticism of Frege he came to reject this Brentanian approach, attacking the psychologism of trying to base logic on empirical psychology. See *Logical Investigations* ([1900–1] 1970). (For more on Brentano, see note 9 below.)

3 See Heap and Roth (1973) for the claim that attempts by sociologists to use Husserlian concepts of phenomenology have fundamentally misunderstood their meaning. Husserl did think that there is an appropriate scientific correlate to transcendental phenomenology – phenomenological psychology – but he insisted that the latter should not be confused with the former.

4 In the preface to his major work, *Ideas*, Husserl complained that Gestalt psychology was as guilty of a misplaced naturalism as was the sensationalism it was intended to replace ([1913] 1931:24). He concluded this despite the claim by the founders of Gestalt psychology that phenomenology was one source of inspiration for their psychology. On this, see Koffka (1935:570–1, and 18–21).

5 The argument of this section draws heavily on chapter 1 of Joseph Rouse's forthcoming: *How Science Practices Matter*.

6 Rouse cites an article by Hubert Dreyfus (1972a "The perceptual noema: Gurwitsch's crucial contribution") as presenting this line of immanent criticism of Husserl particularly clearly and forcefully. Gurwitsch tried to respond to the problems in Husserl by treating perceptual *noema* as perceptual *gestalts*; in this he relied heavily on Gestalt psychology, thereby connecting phenomenology and Gestalt psychology back together (see Gurwitsch 1966). Rouse traces the argument back to Heidegger's 1925 Marburg Lectures (Heidegger 1985).

7 Husserl himself seems to have drawn the same conclusion toward the end of his life (in part as a result of Heidegger's criticisms). In his 1936 *The Crisis of European Sciences and Transcendental Philosophy*, the transcendental ego is said to be "correlative" to the world, and the world is no longer what it is for any transcendental ego but for an intersubjective community. Given these changes, phenomenology now comes to be a search for the ways in which communal experience occurs, and for the

conditions of coherence of these ways. The degree to which this change represents a fundamental alteration in Husserl's views is much debated. For a good overview of the issues and positions involved, see David Carr's "Introduction" to the English version of *The Crisis* ([1936] 1970).

8 The father of existential psychology, Ludwig Binswanger, explicitly acknowledged his debt to Heidegger. See his *Being in the World (*1963), and "The existential analysis school of thought" (1958). For a good account of phenomenology's contribution to psychology, see Spiegelberg (1972).

In France Heidegger's sometime student Jean Paul Sartre also developed an existential analysis of consciousness in his 1943 *Being and Nothingness*. The psychological consequences of this work are further developed in Sartre's *Existential Psychoanalysis* ([1943] 1953). Sartre's approach fed directly into the work of R. D. Laing and his associates (see *The Divided Self: An Existential Study of Sanity and Madness*, 1965).

9 In a certain way Schutz was proposing to return to the conception of phenomenology offered by Husserl's teacher, Franz Brentano. In his *Psychology From an Empirical Standpoint*, Brentano ([1874] 1972) attempted to classify and categorize modes of consciousness and types of consciousness of real human beings, not transcendental egos. In the process he insisted that consciousness was not a container full of contents but an activity constituted in relations between an active subject and an object of this activity. Consciousness is always consciousness of something, and in this way its central feature is its intentionality. The notions of intentionality and of consciousness as an activity are cornerstones of all subsequent phenomenological thought.

10 This book is Schutz's major systematic statement. He continued to develop his ideas – in some cases rejecting his earlier views; his later thought is to be found in his *Collected Papers*, published in three volumes under the editorship of Maurice Natanson, Arvid Brodersen, and Ilse Schutz (1962–66).

11 This is one of the most widely read books in contemporary sociology.

12 This book grew out of, among other sources, a study of Husserl's unpublished manuscripts; on this, see Tilliette (1970).

13 For a good overview of this see Dreyfus and Dreyfus (1999). Dreyfus's *What Computers Can't Do* (1972b, esp. chap. 7) is also directly relevant. In addition, the following give a good sense of the "anti-Cartesian" approach in cognitive science: Varela et al. (1991), McCannell and Rakerin (1994), and Giorgi (1970).

14 See Braybrooke (1987), Rosenberg (1988), Bohman (1991), Martin and McIntyre (1994), Hollis (1996), Blaike (1993), Hollinger (1994), Manicas (1987), Fisk and Shweder (1986). An exception to the widespread ignoring of phenomenology in recent philosophy of social science is Bernstein (1976). For an excellent account of phenomenology as it relates to concerns in the social sciences, see Roche (1973). For the best treatment of the influence of phenomenology on social science see Natanson (1973) and Psathas (1973). For a recent account of phenomenology and contemporary sociological theory, see Wallace and Wolf (1999) and Ritzer (1988: chap 6). Wallace and Wolf claim that phenomenology as a method of sociological practice has been increasing in popularity (see Wallace and Wolf: 291–2).

15 Thus, for example, the insistence that social analysis must begin with the concepts and categories of the social actors being studied in order to see how they conceive of

what they are doing has become a commonplace. This was a central point of phenomenology from Schutz onwards (see Schutz 1962:48–66).

16 In his later writings Schutz came to reject this way of doing things, and to take "intersubjectivity" rather than subjectivity as the starting point. See Schutz, (1966).

17 For a viewpoint opposed to the one I espouse here, see Mary Rogers (1983). Rogers argues that ethnomethodology is insufficiently phenomenological precisely because it attends to rules, interactions, and practices rather than to the foundation of these in consciousness. Rather than moving toward a social and public conception of meaning and social behavior, as I indicate in this section, Rogers would have phenomenologically inspired social science turn all the more vigorously back to consciousness and its analysis.

18 Ludwig Wittgenstein, *Philosophical Investigations* (1953), especially the opening sections (#1–88), and those devoted to following a rule and the famous private language argument (sections #172–280). For a powerful exegesis of these sections, see Kripke (1982). For a brilliant account of linguistic meaning in particular and normativity in general along broadly though substantially enriched Wittgensteinian lines, see Brandom, *Making It Explicit* (1994). The title of Brandom's book suggests a phenomenological impetus (though it does not discuss phenomenology explicitly): to render clear what is implicit. This book is devoted to establishing the source of normativity, just as Husserl's *Logical Investigations* was at the beginning of the twentieth century. But Brandom does so with completely different results, locating it in the ongoing life of actual social interactions. The movement from Husserl to Brandom mirrors the changes in phenomenology itself over the course of the century.

19 Note that Schutz, too, was compelled in the *Phenomenology* ([1932] 1967) to discuss the objective world of typifications and recipes that comprise culture (see especially chapter 4). In his later works this aspect became even more prominent.

20 The philosopher who has developed these ideas, first enunciated by Heidegger, into a full fledged philosophy of understanding and indeed a philosophy of human being is Hans Gadamer. His masterwork, *Truth and Method* (1992), is the deepest and most complete statement of hermeneutics in our time. Part II, section 1 of this work offers an account of how hermeneutics has developed, and Part II, section 1 subsection 3 explicitly discusses the work of Husserl in this regard.

21 Or reappreciated, since this view was already articulated by Herder at the end of the eighteenth century. See Herder (1969).

22 *The Phenomenology of Spirit* (1977). In this work Hegel drew on Kant's distinction between *noumena* (things in themselves) and *phenomena* (things as they appear). It was Kant's use of "phenomena" that was the basis for the development of the term "phenomenology."

23 As Marx put it: "The call to abandon their illusions about their condition is a *call to abandon a condition which requires illusions*" (1963:44, italics in original).

24 Some phenomenologists have claimed that properly understood phenomenology provides the basis for a critical assessment of particular social forms. The basic idea is that by adumbrating what is genuinely universal in human sociality, phenomenology thereby provides a basis for a critique of that which is only contingent and therefore alterable. See, for instance, O'Neill (1972) and Zaner (1973). For a good overview for the tensions involved in bringing Marx and phenomenology together, see Dallmayr (1973).

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Twentieth-century Philosophy of Social Science in the Analytic Tradition

Thomas Uebel

Introduction

Looking over the course of twentieth-century philosophy, it would appear fair to conclude that the tradition of analytic philosophy's concern with social science tended to be a minority interest, even among its not insignificant number of philosophers of science. To assert this is hardly controversial – more so is to give answers to the question "Why?" Might it be "in character" for analytic philosophy to have lagged behind for so long (at least in this respect)? But first there is work to be done in chronicling the apparent neglect and noting what was neglected. Once that is clearer, it may be found that there are no reasons for the relative neglect that are intrinsic to the analytic nature of some philosophical approaches. Moreover, it may be that the neglect of philosophy of social science is by no means as extensive as might be thought. What this chapter will attempt is, first, an initial overall survey of the relevant work in analytic philosophy, and, second, a rendition of the important early work that did go on, its apparent lack of impact notwithstanding.

Before we begin, a note on terminology. Hoping to be excused from giving necessary and sufficient conditions for work belonging to the analytic tradition, suffice it to say that for present purposes a work belongs by virtue of making reference in method/style and/or content to the works of philosophers like Gottlob Frege, Bertrand Russell, G. E. Moore, Ludwig Wittgenstein, Rudolf Carnap, W. V. O. Quine, Donald Davidson, R. M. Hare, John Rawls (thus positioning the analytic tradition as encompassing far more than logical empiricism.) To delimit "analytic" further one may note as borderline cases Edmund Husserl who, having failed to take the linguistic turn (Michael Dummett's criterion)[1], started continental phenomenology – against which analytic philosophy is commonly pitched – and Richard Rorty whose conversationalism strains the bounds

of philosophy as truth-valuable discourse in his flirtation with postmodern theory; of the neo-Kantians we may note that Ernst Cassirer would be in and Heinrich Rickert out (again by Dummett's criterion). Finally, by the terms "postpositivism" and "antipositivist turn" is meant the rejection of three doctrines characterizing the positivism that preceded them: the "received view" in philosophy of science, verificationism in the theory of meaning, and reductionism in the philosophy of mind. Not untypically, their rejection was coupled with the acceptance of reflexive equilibrium as a path to conceptual discovery. This antipositivist turn can be dated to the end of the 1960s (with precursors and latecomers as usual) and may be deemed sealed in the philosophy of social science by the appearance of Fred Dallmayr and Thomas McCarthy's anthology of 1977 giving pride of place to an analytic philosopher affirming the hermeneutical imperative (Taylor 1971).

Overview of Twentieth-century Analytic Philosophy of Social Science

It is plain that among the figureheads of analytic philosophy of science up to mid-twentieth century (roughly the end of World War II), simple neglect or even disregard of social science was commonplace: it was instead the formal and the physical sciences that demanded attention. This orientation unites philosophers as different as Frege, Russell, the early Wittgenstein, Moritz Schlick, Carnap, and Quine. (Predictably, of course, Wittgenstein's later work breaks the early mold, whereas Frank P. Ramsey's short life left no time for him to develop the promising hints he made, leaving that for Donald Davidson.) That as yet unnamed foot soldiers of the self-styled analytic revolution should have done important work in the generally neglected field is not thereby ruled out, of course, nor that one important figurehead of the analytic tradition is conspicuous by his absence from the above list (more on both presently), but a dominant trend is undeniable.

What may have been the causes of this neglect? Even if it is true that the social sciences of the day were in a less developed state than physics, this does not mean that they lacked philosophically interested theorists altogether. Some of the founding fathers of modern social science, for instance, were no less philosophically versed than Henri Poincaré, Ludwig Boltzmann, and Albert Einstein. Yet none of these turn-of-the-century social scientists – for example, Max Weber, Georg Simmel, Ferdinand Tönnies, Émile Durkheim, Vilfredo Pareto – were major influences on the analytic philosophers noted, nor did these philosophers think of themselves as consonantly aligned. It would appear that, with exceptions still to be noted, both sets of theorists operated "in different worlds." For some, this may suggest an indictment of the self-declared distance from traditional philosophical problems which many analytic practitioners prided themselves on. (With analytic philosophy generally abstaining from substantial questions of the "good life," what else was

to be expected from its philosophers of science?) But we must be careful not to generalize too much.

For, of course, there were exceptions to the general picture even before we reach the second half of the twentieth century. Here we will limit ourselves, as it happens, to Viennese contributors to the analytic tradition, without wishing to declare these to be the only ones. Significantly, the most important of these exceptions – as far as the wider philosophical public was concerned – did not emerge as such until the very end of the period we are considering: Karl Popper, the self-styled "killer" of logical positivism. Along with his *Logik der Forschung* of 1934 (untranslated until 1959), it was his article series of 1944–5, "Poverty of historicism" (reprinted in book form in 1957) and his two-volume *The Open Society and its Enemies* of 1945 that brought him into the emerging Anglo-American mainstream and back to Europe from exile in New Zealand. Significantly too, Popper called on influences noted above but generally shunned by analytic colleagues, like Carl Menger. With its most characteristic theses, in any case, Popper's work in this field towers over analytic philosophy of social science throughout the 1950s into the 1960s. Of late, prompted by the "fall of communism," it has received renewed interest.

The other exception we shall focus upon below is a member of the Vienna Circle more often remembered for his organizational efforts for the unity of science movement and some all-too-catchy names and slogans (for example, "physicalism"): Otto Neurath. A social scientist by training, with wide and unusual interests in history and economics, Neurath had long complained in the Vienna Circle itself about its lack of concern for the metatheory of social science. Already somewhat marginalized philosophically, Neurath's writings fell into oblivion after his death in 1945, just as Popper's star was rising. While his epistemological thought has been afforded some reconsideration of late by historians of general philosophy of science, Neurath's theory of social science remains largely unknown. In effect, together with his general epistemology, it represents a road not taken – or rather one only very belatedly and then independently taken – in the development of analytic philosophy.

Yet another exception is Karl Menger, son of Carl, a mathematician who before his emigration, having been a sometime member of the Vienna Circle, convened the famous Mathematical Colloquium, many of whose members were drawn from economics. With his *Morality, Decision and Social Organization. Toward a Logic of Ethics* of 1934 he introduced formal models of decision making into philosophy and initiated a path that ultimately led to the game theory of John von Neumann and Oskar Morgenstern – both of whom had participated in the Colloquium – thus pioneering one of the most important paradigms of current social science. Since this work did not, however, significantly influence many analytic philosophers until the closing decades of the twentieth century, so not in the period mainly at issue here, we shall not discuss him further.[2]

Also for the sake of completeness, mention should be made of two more theorists partially associated with the Vienna Circle – Edgar Zilsel and Felix

Kaufmann (their work is not characteristic of analytic philosophy and they had little influence before or since their deaths in 1944 and 1949 respectively). Zilsel was already an acute critic of racialist ideologies in the 1920s, and from the 1930s on pioneered an approach to the sociology of science that was overshadowed by Robert Merton and his school.[3] Kaufmann attended meetings of both the Vienna Circle and F. A. Hayek's *Geistkreis* and sought to mediate between logical empiricism and phenomenology. In 1936 he published *Methodology of Social Sciences*, the English version of which (1944) was completely rewritten to take account of his assimilation of pragmatic influences resulting from his work at the New School for Social Research in New York.

Clearly then, there were notable exceptions to the disregard of social science among analytic philosophers up to mid-century, even when only philosophers hailing from Vienna are taken into account. However, with the exception of Popper, they are hardly household names. Moreover, analytic philosophy of social science of the next period paid very little if any attention to the prewar work of these philosophers, again with the exception of Popper.

We now turn to the analytic philosophy of science in the period from mid-century (the end of World War II) up to the antipositivist turn. We may wonder, of course, whether the latter was required for analytic philosophy of social science to take off. In a quantitative sense the answer is clearly "no": the period at issue saw a considerable rise in analytic philosophy of science concerned with social science in particular. In a more qualitative sense along current criteria, the answer may seem to be that the antipositivist turn was required; however, this holds only insofar as its prepostpositivist form tended to be reductive in orientation. We may leave open whether as a matter of fact all of its forms were inclined that way; in any case, postpositivist philosophy of social science is by definition antireductionist. Sometimes this is regarded as a consequence of antipositivism in related fields of inquiry, but it need not be. The not often remarked upon fact that antireductivist philosophy of social science can have momentous consequences across other areas of philosophy is shown by none other than Donald Davidson (1974), who relates how he developed his decidedly postpositivist philosophy of mind on the basis of a broadly hermeneutical insight gleaned from social science fairly early on.

Yet what was the standard fare in postwar analytic philosophy of science? Short of making a survey of all relevant philosophy journals, we may take the important anthologies of the day as our initial guide to what "played" in analytic philosophy of social science from the late 1940s to the 1960s. The first thing to notice is that it was not until the mid-1960s that anthologies dedicated to the subject appeared. Until then, Herbert Feigl and Wilfried Sellars' influential *Readings in Philosophical Analysis* (1949) in its section of philosophy of science contained only one directly relevant and two more broadly relevant papers, while Feigl and May Brodbeck's *Readings in the Philosophy of Science* (1953) at least contained an entire section of philosophy of social science with eight papers ranging from general methodology and soon-to-be standard topics to specifics of economics. It was another 10 years' wait for general philosophy of social science anthologies,

of which a number were published up to the early 1970s, after which again no major general anthology was published until 1994 (even though more narrowly oriented ones continued to appear). Similarly, no widely read textbooks dedicated to analytic philosophy of social science appear to have been published before Richard Rudner's of 1966 and Alan Ryan's of 1970, and then again very few until the more recent wave that began with David Braybrooke's replacement monograph for Rudner's in 1987.[4]

These data are consonant with the thesis of the relative neglect of the philosophy of social science in the analytic tradition and the "slow thaw" from the 1950s onwards. But let us consider what "broke the ice," as it were. By the early 1960s enough material appears to have been available to merit Maurice Natanson's anthology of 1963, which sought to confront "naturalistic" (analytic) and phenomenological contributions. (In this respect it was hardly a typical analytic publication.) Braybrooke's smaller anthology of 1965 was more decisively analytically oriented and covered also early formal-technical developments. Along almost exclusively analytic lines both May Brodbeck's and Leonard Krimerman's large anthologies of 1968 and 1969 respectively set out to be as comprehensive and cover as many topics of the subject as possible. The smaller anthologies by Dorothy Emmett and Alasdair MacIntyre (1970) and Alan Ryan (1973) added incrementally to this stock.

What were the topics covered in these anthologies? Of specifically philosophy of social science themes, Feigl and Sellars had featured only Hempel on the deductive-nomological model in history, while Feigl and Brodbeck featured papers on laws in social science generally and economics and history specifically, on the clash of naturalistic and phenomenological approaches, on the virtues or otherwise of dialectics, on methodological individualism, and on the issue of objectivity and value judgments. Maurice Natanson in particular featured the clash of "naturalism" and phenomenology with regard to the issue of concept and theory formation and objectivity and value judgments. Brodbeck and Krimerman then attempted to cover the entire field: the possibility of a social *science* and its relation to the natural sciences as regards scope and strictness of generalization and complexity of subject matter, the relation between causal and rational explanation, the status of teleological and functional explanation, the peculiarities of social scientific concept formation and modeling, methodological individualism, objectivity and value judgments, and realism vs. instrumentalism in economics. The smaller collections by Dorothy Emmett and Alasdair MacIntyre and Alan Ryan distinguish themselves mainly through the introduction of questions of rationality and of relativism as topics in their own right – which around this time began to attract also single-issue anthologies spanning the antipositivist turn (Wilson 1970, Hookway and Pettit 1978, Hollis and Lukes 1982).[5]

It is not possible to discuss or even list all the individual authors featured in these anthologies. Briefly though we may record C. G. Hempel and Ernest Nagel as leading empiricists (Popper's position is mostly represented not by himself but

by John W. N. Watkins), and Alfred Schutz, Maurice Mandelbaum, Peter Winch, and William Dray as leading social science critics of the nomological method. Of these Nagel and Hempel (as well as Popper) represent veterans of the prewar period as do Mandelbaum and Schutz (the latter another Viennese commonly counted in the phenomenological camp), while Winch and Dray, for instance, represent the younger opposition, with writers like MacIntyre and Leon Goldstein taking a differentiated position between the two poles.

In retrospect it is notable that in opposing the presumption of law-governed social science on the model of natural science Winch, in his much-quoted *The Idea of Social Science* (1958), and Dray, in his monograph on historiography (1957), denied altogether that reasons are causes (along with other philosophers less concerned with social science). It is tempting to speculate that, unlike their older fellow opponents Schutz and Mandelbaum, who were still able to draw on the German historicist tradition, they were driven to this radical position (in Winch's case partly derived from Wittgenstein) by the overwhelming naturalist hegemony they perceived to hold sway in 1950s philosophy of social science.[6] In more positive terms, however, we may see in these discussions the beginning of the still continuing debates about rational explanation and rationality. The assimilation of reasons to causes and into the nomological framework turned out to be not as unproblematical as naturalists had first assumed – as Davidson himself, an early combatant for causalism (1963) came to stress (1970).

On the "naturalist" side meanwhile it is notable that the opposition within the camp that sought for laws in social science between those who expected these laws to be reducible to psychology or even more basic sciences and those who rejected this expectation remained curiously underdiscussed. Broadly speaking, logical empiricists and their allies tended to concentrate on the defense of the applicability of the model of deductive-nomological explanation[7] in social science whereas critical rationalists battled for methodological individualism.[8] Both stances appeared to represent different aspects of mainstream opinion, but their underlying disagreement found little expression in these anthologies. It is only in retrospect that their conflict becomes more immediately obvious.[9]

It is also instructive to compare with these anthologies the two large volumes published by the leading analytic philosophers of science, Nagel and Hempel, in the 1960s. This allows us to stress, first of all, that in this post-World War II period, unlike the previous one, we can at best speak of the *relative* neglect of philosophy of social science by the mainstream of this tradition: after all, roughly a quarter of both Nagel's *The Structure of Science* (1961) and Hempel's *Aspects of Scientific Explanation* (1965) concern issues in this area. (Some of the work, built upon or reprinted in these books, dates back to the beginning of the 1950s and earlier.)

A particularly prominent place among Nagel's and Hempel's contributions to the philosophy of social science, moreover, is occupied by the issues of functionalism and functional explanation. Nagel and Hempel rejected the former and delineated precise conditions where the latter was legitimate, pioneering its

grounding in self-regulatory processes.[10] There can be little doubt that this concern reflects the state of social science of the day when functionalism still appeared to provide the main systematic alternative, as a distinctively sociological mode of explanation, to class analysis and historical materialism. Nagel's and Hempel's criticism not only engaged with the work of the earlier anthropologists Malinowski and Radcliffe-Brown, but also that of the contemporary sociologists Parsons and Merton.[11] Thus we may note secondly that in this period major players were paying close attention to actual work in the field, and not only to fellow metatheorists or philosophers. While Rudner's textbook may have become emblematic for the era, with its astounding absence of live examples, it is no means wholly representative. A third point is that some of this mainstream work stood the test of time rather well. It is customary to credit Steven Lukes (1968) with making the point that it is not at all clear just what the type of individualistic predicate is supposed to be upon which methodological individualism rested its claims. Many terms applicable to individuals presuppose a realm of social relations without which they would not be intelligible – but are all of these terms properly "individualist"? A similar point was already made by Nagel against Watkins in order to separate the acceptable "ontological" thesis of methodological individualism from its unacceptable "reductive" thesis.[12] Another notable example of lasting relevance is Russell Keat's recourse in his assessment of Critical Theory to Nagel's distinction of "characterizing" and "appraising" value judgments.[13] To the extent then that some of Nagel's contributions may be regarded as steps in the development of what nowadays is called "nonreductive naturalism," Hempel's late-1970s encomium of Nagel's *Structure of Science* also holds for the philosophy of social science contained in it: "Recent controversies between analytic and historic-sociological approaches to the philosophy of science have not diminished its significance; in fact, it seems to me that the pragmatist component in Nagel's thinking may be helpful for efforts to develop a rapprochement between the contending schools."[14]

Virtually all the anthologies noted, especially Brodbeck's and Krimerman's, clearly belong to, and indeed represent, prepostpositivist analytic philosophy of social science. Of all the older topics it is perhaps not surprising that many of the issues of contemporary analytic philosophy of social science are most closely related to then recently emerged issues of rationality and of cultural relativism.[15] Yet what of the other topics discussed in these prepostpositivist anthologies – do they still excite contemporary practitioners?

Though most of them are less frequently, certainly less heatedly, discussed nowadays, they are still part of the agenda. Indeed, how could these topics not form part of its subject matter: (1) the possibility of a social *science* and its relation to the natural sciences as regards scope and strictness of generalization and complexity of subject matter; (2) the peculiarities of social scientific concept formation and/or mathematical modeling; (3) the relations between causal and rational explanation; (4) the nature of functional explanation and its relation to teleological explanation; (5) objectivity and value judgments; (6) methodological

individualism; (7) realism versus instrumentalism in individual disciplines? Short of change of subject or philosophical orientation, some stance on a number of these issues defines an analytic philosopher of social science. Certainly all of them still feature more or less directly in Michael Martin and Lee C. McIntyre's anthology of 1994. But this is not to say, of course, that the stance taken has not changed considerably since the antipositivist turn or that no other main topics emerged. Moreover, by no means all the papers in the important Midwest Studies volume on *The Philosophy of the Human Sciences* (French et al. 1990) fall into these established categories.

What tends to distinguish contemporary treatments of the older topics are two clusters of characteristics. There is, first, our contemporaries' self-conscious and identity-bestowing antipositivism. Second, there is the increased attention to actual examples from social science. The antipositivism can be many-faceted. Beyond the nowadays *de rigeur* antireductionism and the affirmation of the autonomy of social science, which has both a conceptual and a nomological aspect, antipositivism of late also tends to be characterized by a pluralistic attitude toward the methodology of social science as argued by Roth (1987). There is no longer thought to be just one correct way of doing social science: methodological pluralism itself has become the new paradigm. But postpositivism is also pro something. Its analyses of the wide variety of actual work in social science contrast markedly with the level of abstraction at which previously the issue of the distinction of social science tended to be discussed – when social science itself admitted less methodological variety.[16] While this, for reasons noted above, is not a categorical distinction, we may conclude that contemporary analytic philosophy takes social science as it is practiced – though by no means uncritically so – rather than legislate *ex cathedra* and from principle alone how a proper social science would proceed. This pluralist approach to social science and the decidedly practice-based theorizing is well illustrated by a number of recent textbooks or survey volumes.[17] Likewise, the journal *Philosophy of Social Science* has expanded from its Popperian base with articles discussing distinctly "continental" themes.

Most importantly perhaps, along with the antireductionism came, as already noted, the acceptance of the hermeneutic challenge. This did not only open the field for a variety of reflexive inquiries (methodology of social studies of science, race and gender in natural and social science) but also affected the reception of writers from outside the analytic tradition. Thus whereas before the turn, Weber was viewed mostly as challenging the empiricist perspective, he now tends to be read as (at least in part) providing suggestions for a more broad-minded empiricism. Still today the challenge for postpositivist analytic philosophy of social science as a whole would appear to be the "integration" of the by now irreversible recognition of the causal efficacy of subjective perspectives and participants' categories (at least in principle – irrespective, that is, of the subtleties of their interplay as investigated in Cultural Studies) in the "natural order" of the material world – where what it means to "integrate in the natural order" is precisely one of the issues up for grabs.

So it seems we can conclude that even though some of the older debates – reasons versus causes, say, or methodological individualism – no longer rage as they used to, it is not yet clear just what the currently presumed consensus – that reasons can be causes and metaphysical holism is to be viewed with suspicion – comes to in the larger picture concerning the issues (1)–(3) in the list above. For it is also the picture of natural science that the antipositivist turn in analytic philosophy has set into a certain flux which has not yet settled. The "anomalies" of social science may well turn out less anomalous than was long thought.

Behind the Scenes of Prepostpositivist Analytic Philosophy of Social Science

Since the sources noted above are by no means obscure, there is little need here to characterize the standard debates further. It may be more to the point to explore what went before, namely the above mentioned neglected work of logical empiricist philosophers of social science. Considering Neurath's contribution in particular promises to be instructive in several respects. Doing so will, first, highlight the sociopolitical context of philosophy of social science not only in principle but also in special cases – so providing a surprisingly early instance of the reflexivity so prized in postmodern theory – and thereby, second, provide some important but long-neglected prehistory of one of the older disputes, methodological individualism. Even though he shared a not inconsiderable part of Popper's animus, Neurath served as the mostly unnamed opposition against which many of Popper's argument were directed.

It may be wondered whether such an inquiry could be of more than archival interest. The answer pursued here is that the example of Neurath's nonstandard theorizing about social science sharply contradicts the view that orthodox prepostpositivist analytic philosophy of social science was of necessity what it was, for it shows that there in fact were early attempts at what basically are postpositivist positions – attempts that for a variety of reasons were subsequently neglected.

Two of these reasons, it may be noted right away, are the far-flung nature of Neurath's oeuvre as well as, for those who could lay their hands on it, his sometimes discursive and programmatic style.[18] Given that there are other philosophers prized in the analytic tradition who also could be accused of obscurity, however, the neglect of Neurath is hardly fully explained thereby. One of the additionally relevant factors is surely that a most striking contrast class was represented by the writings of his sometime colleague-in-arms Rudolf Carnap; another that Neurath's basically pragmatist orientation sat only very uneasily with the more formalist one of his mainstream logical empiricist colleagues; yet another that the association with the latter precluded a sympathetic reading of Neurath by like-minded theorists outside that movement. Thus it is likely that even if some

readers saw beyond Neurath's supposed positivism, his loose formulations (in some of his criticisms of Weber, for instance) suggested him to be a much cruder materialist than he actually was (as I argue below).

The difference between the appearance and reality of Neurath's philosophy of social science is best appreciated with regard to his key slogan "physicalism." Long thought to signify reductionism at its most rabid, its use by Neurath actually hides different doctrines of quite different intent. For our purposes we can distinguish between three of them: the epistemological, the metalinguistic, and the nomological conception of physicalism.

The first of them, important though it is, need not detain us here. This is his conception of physicalism as a "comprehensive attitude," which has been identified as the position of what is nowadays called "epistemological naturalism," the denial of epistemological aprioricism.[19] Roughly, such a position seeks to explain – and legitimate – scientific knowledge claims in a scientific manner. Importantly, Neurath's early version of this doctrine differs from Quine's later one in allowing social science to play a role in this "explanation" of scientific knowledge. Moreover, Neurath's epistemological naturalism does not rely, like many contemporary versions, on a scientific realism that invokes the correspondence theory of truth, but incorporates certain constructivist elements on the metaepistemological level concerning notions such as "justification."[20]

The second conception of "physicalism" that we need to distinguish is the metalinguistic notion that concerns the guiding conception of the language of science. It is instructive to compare Carnap here. For him, "physicalism" simply meant that every language of science, that is the languages of all its different disciplines, can be translated into the language of physics (1932). This is easily read as materialism clad in metalinguistic garb, but Carnap intended to make no ontological claims whatsoever: notoriously, he rejected all attempts to interpret his physicalism as a form of materialism. Carnap never saw himself able to go beyond questions of the internal consistency and mutual compatibility of possible languages and derive ontological claims from pragmatic decisions concerning current or future language use. (On this point he has been criticized since Quine.) His thesis of the "universality" of the language of physics accordingly merely marks the elevation of the thesis of the linguistic conformity and unity of science to the status of a constitutive principle of the theory of science. Carnap's physicalism, we may add, originally required the complete translatability of the languages of all the sciences into that of physics, but this was gradually relaxed.

Neurath's metalinguistic physicalism was centered differently. "Every scientific statement is a statement about a lawlike order of empirical facts" ([1931] 1981:424) Neurath linked his metalinguistic thesis of physicalism closely to the empiricist criterion of meaningfulness and at a very early stage sought to allow for non-reductive forms of it: "Physicalism . . . only makes pronouncements about what can be related back to observation statements *in some way or other*" ([1931] 1981:425, italics added). Beyond this, Neurath determined meaningfulness as

inextricably linked to the availability of intersubjective evidence and he had already rejected the possibility of private (protocol) languages in 1931. Importantly, he determined that the language in which such test procedures are formulated (the protocol language) was to be not the theoretical language of physics itself, but the "physicalistically cleansed" everyday language. Neurath's conception of the physicalistic language was not bound to the language of physics as such; what it was bound to we shall see presently.

In concert these points result in a conception of metalinguistic physicalism that compensates for its explicit lack of precision in comparison to Carnap by presenting an alternative. In place of Carnap's translatability Neurath put testability. For him, metalinguistic physicalism did not represent a logical condition on the relation of individual expressions of high theory in the different disciplines of unified science, but an epistemological condition on the admissability of whole statements into unified science. Two points are notable here.

First, for Neurath, metalinguistic physicalism expressed the condition of empiricism. For him, physicalistic statements are statements about "spatiotemporal structures" ([1931] 1981:425). Only those statements are admissible that can be tested – or, as Neurath put it, "controlled" – by direct or indirect reference to intersubjectively available observational facts. It follows that social scientific theories must allow for derivations that can be formulated in the everyday language speaking of spatiotemporal structures and can be tested as such.

> Physicalism encompasses psychology as much as history and economics; for it there are only gestures, words, behavior, but no "motives," no "ego," no "personality" beyond what can be formulated spatio-temporally. It is a separate task to ascertain what part of traditional material can be expressed in the new strict language. Physicalism does not hold the thesis that "mind" is a product of "matter," but that everything we can sensibly speak about is spatio-temporally ordered. (Neurath [1931] 1973:325)

Neurath's adoption of the term "behaviorism" is also to be understood in this spirit.

> There is no longer a special sphere of the "soul." From the standpoint advocated here it does not matter whether certain individual tenets of Watson, Pavlov or others are maintained or not. What matters is that only physicalistically formulated correlations are used in the description of living things, whatever is observed in these beings. (Neurath [1932a] 1983:73)

"Behaviorism" for Neurath meant simply the limitation to physicalistic statements, that is, to statements about human activities as taking place in space and time. While he did not stress it, we may note that this includes talk of many of the "intervening variables" which for the psychologists mentioned had become illegitimate. Thus note not only that Neurath was open in principle to Freud's

psychoanalysis – he headed a working group dedicated to the "physicalization" of Freud's texts – but that his own theory of scientific evidence statements (protocols) makes explicit reference to intentional phenomena via locutions like "speech thinking," "thinking person," and so forth.[21] Thus Neurath wrote:

> While avoiding metaphysical trappings it is in principle possible for physicalism to predict future human action to some degree from what people "plan" and "intend" ("say to themselves"). But the practice of individual and social behaviorism shows that one reaches far better predictions if one does not rely too heavily on these elements, which stem from "self-observation," but on others which we have observed in abundance by different means. ([1936a] 1981:714)

It was in this sense that Neurath expounded a "social behaviorism" that "ultimately comprehends all sociology, political economy, history etc." ([1932b] 1981:565). We may be skeptical about the value of the exclusive use of overtly behavioristic procedures, of course; the point here rather is that Neurath's physicalism was not limited to them.

The second point to be noted is that Carnap's original physicalism required the translatability of individual terms. This amounts to the reducibility of all the terms of the special sciences to the terms of the language of physics (say, of "market" to some extremely complex description of the behavior of atoms). Neurath's epistemological take requires only that admissible statements be logically related to statements that can be correlated as wholes with statements of the physicalistic common language of observation. From Neurath's metalinguistic physicalism therefore does not follow what follows from Carnap's: that all the individual terms admissible into unified science be definable in the terms of physical theory.

I believe we do not overextend our sympathy if we interpret Neurath's metalinguistic physicalism as at least in intention a partial form of what nowadays is called "nonreductive physicalism" (minus its ontological dimension). Of course, Neurath did not employ many of the terms used in the exposition of the latter, like "supervenience" (ontological dependence without reducibility), but a careful assessment of his admittedly contrapuntal writings strongly suggests that his metalinguistic physicalism allowed for the conceptual autonomy of the special sciences (within the framework of empiricism).

The third aspect of physicalism to be mentioned here concerns nomological reducibility, the supposed reducibility of the laws of the various individual sciences to those of some basic science. It throws into relief Neurath's nonstandard conception of the idea of the unity of science itself. The standard conception of the unity of science, of course, envisaged that unity as an (inverted) pyramid of reductively related disciplines with physics at the base, and accordingly demanded, at least in principle, the reduction of sociological laws to those of physics (such that sociological laws are but shorthand for a complex amalgam of physical laws). By contrast, Neurath wrote:

> The development of physicalistic sociology does not mean the transfer of the laws of physics to living things and their groups, as some have thought possible. Comprehensive sociological laws can be found as well as laws for definite narrower social areas, without the need to be able to go back to the microstructure, and thereby to build up these sociological laws from physical ones. ([1932a] 1983:75)

Two things are important here: the rejection of the postulate of the reducibility of the laws of social science to those of physics and the rejection of the postulate of methodological individualism (in one of its guises).

The rejection of the reducibility of the laws of social science follows from the rejection of the reducibility of the individual terms of social science to those of physics (through the implications canvassed above). Neurath owes us an explicit argument to this effect, but significantly enough he wrote: "One can understand the working of a steam engine quite well on the whole without surveying it in detail. And indeed, the structure of a machine may be more important than the material of which it consists" ([1931] 1973:333). The explanatory structural kinds invoked need not be reducible to those concerning material constituents. This suggests that it is not only the distinction between the contexts of discovery and justification that Neurath was exploiting – such that only in the latter intertheoretic reductions of laws are required – when he concluded: "The sociological laws found without the help of physical laws in the narrower sense must not necessarily be changed by the addition of a physical substructure discovered later" ([1932a] 1983:75). Indeed, he noted: "According to physicalism, sociological laws are not laws of physics applied to sociological structures, but they are also not unproblematically reducible to laws about atomic structures" (1933:106).

Yet nomological antireductionism also has a still more specific dimension of relevance to social science, namely the rejection of methodological individualism in its conceptual and nomological sense. Concerning sociological laws Neurath wrote: "Naturally certain correlations result that cannot be found with individuals, with stars or machines. Social behaviourism establishes laws of its own kind" ([1932a] 1983:75). Given the strenuous opposition to metaphysical social science in his *Empirical Sociology*, where he explicitly opposed the invocation of the supraindividual entities that populated the rising *völkisch* ideologies, it is clear that Neurath did not aim for an ontological holism of any kind. Rather, he once again stressed the conceptual autonomy of social science.

For Neurath then, the claim of unified science was minimalist – what has been called "unity at the point of action"[22]: "All laws of unified science must be capable of being linked with each other, if they are to fulfill the task of predicting as often as possible individual events or groups of events" ([1932a] 1983:68) He did not require that social science be conducted just like natural science. "The program of unified science does not presuppose that physics can be regarded as an example for all the sciences to follow" ([1937] 1981:788). Neurath also issued warnings against the consequences the neglect of the peculiarity of social

science would have for the general theory of science, but to little avail. His *Foundations of the Social Sciences* (1944), where he argued his case against reductionism once more, was mostly disregarded.

In the 1930s then Neurath was already involved in a struggle for a proper empirical social science on two fronts: on one side against the claims of metaphysical *Geisteswissenschaft* and on the other against the claims of a reductionist type of unified science promoted still vigorously in the 1950s – among others by philosophers who gained fame in their later maturity for denouncing the failings of precisely this "positivism" (see Oppenheim and Putnam 1958). There are still further aspects of Neurath's philosophy of social science that are of postpositivist interest, but let us now turn to his counterpart, Karl Popper.

Even though Popper did not start out as a philosopher of social science – his *Logic of Scientific Discovery* really concerned general methodology and natural science – he did venture into this field at a relatively early stage, in the second half of the 1930s when faced with the political catastrophe in Central Europe. He did not publish his work, however, until he found occasion to join F. A. Hayek's campaign for methodological individualism (Hayek [1942–4] 1952) – and to reorient the latter (remarkably discreetly so) away from Hayek's own antiscientistic stance of social scientific separatism – in the series of articles called "Poverty of Historicism," noted above. (These articles provided the methodological counterpart to the social philosophy argued for in *The Open Society* [1945] 1966.)

Historicism was castigated as "the approach to the social sciences which assumes that historical prediction is their principle aim, and which assumes that this aim is attainable by discovering the 'rhythms' or the 'patterns,' the 'laws' or the 'trends' that underlie the evolution of history" (Popper [1944–5] 1961:3) For Popper, historicism underwrote totalitarianism by providing its theoretical legitimation and epistemological foundation ([1944–5] 1961:159).[23] Methodological individualism represented the only alternative to the failings of historicism and, like Hayek, he sought to nail his opposition on the cross of holism, that is, anti-individualism.

Popper distinguished naturalist and antinaturalist anti-individualists, where "naturalism" means something like adherence to the unity of science thesis and opposition to the separation thesis between natural and social science. (Popper himself has to be counted a naturalist only in this sense.) Antinaturalist historicists are mistaken in thinking there to be essences of social wholes that could in some way be intuited like the properties of a *Gestalt*. They rely either on an anti-experimentalism that is ungrounded (there obtains no principled difference from natural science in this respect) or they rely on the particularization of generalizations to certain periods or ages, a view that shows, so Popper thinks, a misunderstanding of the task of theory (to discover strict universal laws). Naturalistic historicists are characterized by their view that the only true social science is history ([1944–5] 1961:39) and that social science is "the study of the operative forces and, above all, of the laws of social development" ([1944–5] 1961:45),

with supposedly objective teleologies grounding the normative claim of such studies to guide action ([1944–5] 1961:50). Later they were shown to be mistaken in thinking there to exist laws involving social wholes: so-called laws of development are just statements of trends and these designate a singular fact at best. Popper held that historicism sought laws in the wrong places: there are no historical laws of the development of collectives, but only laws of the aggregation of individual actions. What laws there are to be discovered must be universal with variables ranging only over individuals or states thereof.

What then was Popper's understanding of the doctrine of methodological individualism? Quite obviously, it was directed at the supposition that entities not reducible to individuals, their relations, and combinations thereof could be considered causal agents. So Popper opposed ontological holism. But was he also a methodological individualist in the sense of requiring that the concepts and laws of all social sciences be reducible to individual psychology? That is by no means clear – indeed, there is good reason to consider Popper an anti-individualist in this respect. He defined methodological individualism ontologically as "the important doctrine that all social phenomena, and especially the functioning of all social institutions, should always be understood as resulting from the decisions, actions, attitudes, etc., of human individuals, and that we should never be satisfied by an explanation in terms of so-called 'collectives' (states, nations, races, etc.)" ([1945] 1966:vol. 2, 98). And he went on: "The mistake of psychologism is its presumption that this methodological individualism in the field of social science implies the programme of reducing all social phenomena and all social regularities to psychological phenomena and psychological laws. The danger of this presumption is its inclination towards historicism" ([1945] 1966:vol. 2, 98).

This passage clearly rejects the demand that the concepts and laws of social science be reducible to psychology. Popper conceded the "autonomy of sociology" – just in this respect he lauded Marx over Mill – and gave social science "the task of analysing the unintended social repercussions of intentional human actions" ([1945] 1966:vol. 2, 95). Other social sciences differed from psychology inasmuch as they studied the aggregational effects of such unintended consequences of intentional actions across society, whereas psychology studied the motivations and intended consequences of intentional action. (Notably for Popper, even "psychology – the psychology of the individual – is one of the social sciences, even though it is not the basis of all social science" ([1945] 1966:vol. 2, 97), given that it too has to take social context into account.)

Yet Popper was an individualist not only in an ontological but also in a moral and political sense. He defended liberal economic freedoms (the right to own the means of production) as a precondition of liberty and/or progress and followed Hayek in pronouncing the impossibility of comprehensive social and economic planning. The market performs an indispensable signaling role in allowing individuals to rationally conduct their affairs in the light of their limited and fallible

knowledge of market values, and thus serves to coordinate individual actions. To think otherwise and attempt a comprehensive "planned economy" was to take the first step towards totalitarianism.

Note here the undisguised political agenda that Popper gave to philosophy of social science. Originally intended to combat both Nazism and communism, it was in the service of Cold War opposition to the latter that Popper's arguments in *Historicism* – together with those in *The Open Society* – gained the widest audience in post-World War II Western Europe. Popper alleged wholesale holism on his opponents' part and argued that this gross but widespread misunderstanding of the nature of scientific knowledge – together with utopian designs – had fatal consequences in real life. Despite what in some quarters is deemed a scientistic insistence on the value-neutrality of social science, even some prepostpositivist analytic philosophy of social science reserved for itself a morally and politically engaged position.

Who were the historicist-utopian theorists of social science that Popper condemned to irrationality? Did Popper not overintellectualize the hacks churning out party propaganda? Popper's opposition remains a strangely shadowy bunch indeed. Mannheim was convicted by association, but other thinkers behind the Stalinist *Gestalt* Popper outlined are not identified. Neurath is only once referred to – most curiously, in the chapter dismissing antinaturalist historicism – for his supposed confusion about the scientific method ([1944–5] 1961:103). That he may nevertheless be one of the intended objects of Popper's polemic only becomes clear once we note Popper's deferential reference to Hayek's *Collectivist Economic Planning* (1935) in support of his own argument for the inherent irrationality of utopian, holistic planning: there Neurath is the central villain. This suggests that Neurath is one twentieth-century theorist upon whom Popper thought he could foist, with at least *prima facie* plausibility, the joint acceptance of historicism and utopianism (just as Hayek foisted his notion of scientism on him). Whatever the biographical importance of Neurath to his methodological project, however, Popper placed the "fascinating intellectual structure" of historicism – as he put it in the preface to the book edition, first published in 1957 ([1944–5] 1961:vii) – into center stage.

The important question here is: are Popper's charges against Neurath correct? For present purposes we can put the thorny issue of Neurath's proposals for "administrative economies" to one side and concentrate on whether his social scientific methodology does fall prey to Popper's antihistoricist arguments. If, as I shall argue, it does not, then Popper's critique misfires, whatever the nature of Neurath's contentious economics. Note that this question is of systematic interest in that a negative answer would provide an alternative conception of social science to that of Popper, who true to his Mengerian roots can only conceive of one true social science. (For Popper, Carl Menger's conception of theoretical deductive science is correct for *all* of theoretical science, not just, *pace* Hayek, economics or social science ([1944–5] 1961:131).) In other words,

Neurath would show us by example both one price that we may have to pay for dissent from individualism as conceived by Popper, and the alternative that could be attained for that price.

As noted, we must guard against misunderstanding the point of Neurath's "physicalism" and his "social behaviorism" as a reductivist doctrine that forbade all references to mental states. All it did forbid was reference to disembodied mental states and contents such that attributions would become in principle untestable. Neurath's naturalistic methodology did not preclude reference to intentional phenomena. But that was not really Popper's complaint (though it had been Hayek's); rather that he was a "historicist." This meant that Neurath fell foul of the strictures of Popper's methodological individualism. So what about Neurath's "holism"?

While his debt to Pierre Duhem undoubtedly indicates a fair dose of epistemological holism (scientific knowledge is supported coherentially), which is not at issue here, Neurathian social science does not condone ontological holism or anti-individualism. Neurath rejected reference to the irreducible collective entities like *"Volk"* or *"Volksgeist"* – indeed, to combat such talk within the academia and even more outside of it may be said to constitute the most central motive force of his social scientific naturalism. It was just by allowing such talk that contemporary proponents of separatist *Geisteswissenschaft* were regarded by him as providing metaphysical pseudofoundations for fascism and Nazism. This is a major point of agreement of Neurath's with Popper's later arguments which the latter never cared to mention.[24] It is true, of course, that Neurath, as shown above, did not require that social scientific concepts and laws be reducible to psychology, but that could also hardly count as a point of criticism for Popper, who in fact shared this view.[25] Moreover, the later Neurath in particular was concerned – somewhat to the chagrin of his colleagues – to stress the principled limits to predictability in the social sciences, which he saw as arising in part from the phenomenon of reflexive predictions. This phenomenon – another mainstay of Popper's later arguments against historical prediction – is one which Neurath had already commented upon before World War I (1911) and repeatedly since (e.g., 1931, 1944).[26] Neurath, we can conclude, plainly escapes Popper's charge of historicism in the strict sense and also cannot be counted an anti-individualist in the ontological sense; moreover he pioneered some aspects of the views Popper later propounded.

Yet as noted, Popper deemed him caught in the wider net of the arguments he spun. He convicted him of mistaking the nature of theoretical science by allowing social science to aim for generalizations that only hold "within 'the present cosmological period'" (Popper [1944–5] 1961:103). The quotation is not exact, but Popper's attached footnote correctly refers to Neurath's paper at the Second Congress for Unified Science where a "relativization and historicisation" of social scientific generalizations is suggested ([1936b] 1981:772). For Popper, Neurath counted as a historicist because he still hankered after what might be called "soft" historical laws. But are such relativized laws laws of history at all? At least a

different reading of the Neurathian enterprise is possible, but it is not one Popper was inclined to pursue.

Popper wrote:

> It would not be a sign of laudable scientific caution if we were to add such a condition, but a sign that we do not understand scientific procedure. [Here follows the footnote reference to "historicists" like Neurath.] For it is an important postulate of scientific method that we should search for laws with an unlimited realm of validity. If we were to admit laws that are themselves subject to change, change could never be explained by laws. It would be the admission that change is simply miraculous. And it would be the end of scientific progress; for if unexpected observations were made, there would be no need to revise our theories: the ad hoc hypothesis that the laws have changed would "explain" everything. ([1944–5] 1961:103)

Clearly, that it is "an important" task to find exceptionless laws does not mean that it is the only task for science – unless it is Mengerian theoretical science that we have in mind. But why should that be accepted without argument?

To unhinge Popper's argument one must challenge the presupposition on which his prescription proceeds. Contemporary debate has achieved this by challenging Popper's conception of what scientific laws are worth aiming for (albeit not under this heading). Contemporary theorists argue persuasively that universal laws of the Mengerian variety are by no means the exclusive aim of social science (e.g., Kincaid 1996). It is perhaps too early still to predict the end of this debate, but note that with this issue we are squarely back to discussing the issues that enlivened the notorious *Methodenstreit* over 100 years ago between Menger and Schmoller. One of the sharpest distinctions between his own "theoretical" and Schmoller's "historical economics" for Menger was the absolute universality of theoretical laws.

It does not lack a certain irony therefore that not only did Neurath make this "contemporary" move against Popper's prescriptions, but that he made that move being very well acquainted with the classical positions in the *Methodenstreit*. Concerning that debate, Neurath noted early on that in principle, "there is no reason to think of historical and theoretical research as opposites, it would not even be practical to conceive of each in isolation from the other" (1911:113). Far from betokening a simple misunderstanding of what science is all about, Neurath disagreed with Popper's Mengerian conception of scientific knowledge that the regularities to be discovered by science must be universal laws. Without his Mengerian presupposition Popper's final argument against Neurath also collapses. For if it be granted to social science to aim for less than universal laws, then there is no bar to investigating "midrange" generalizations concerning social phenomena and *ceteris paribus* laws qualified by temporal indexes. Such generalizations may well refuse to trade in ontologically irreducible wholes – and that is all that is required to break the deadlock that Popper imposed on the social sciences: be individualist and universalist or unscientific.

Conclusion

Let me stress that the argument just given is not directed against the possibility of pursuing social science in the way favored by Popper, but his contention that it *has* to be done his way. It was on the neglect of this small but significant difference that his rhetoric and apparent success rested. This points to the "moral" of our story. Before the "received view" of scientific theories had become "received" in the later 1940s (and remained so throughout the 1950s into the 1960s), there were alternative conceptions in analytic philosophy of science, in particular conceptions that anticipated postpositivist positions in philosophy of social science. That its own progress in this respect may appear delayed for so long obviously has little to do with intrinsic features of the analytic tradition.

It would be tempting to conclude that a break occurred in the 1940s that introduced a discontinuity between the work actually done in philosophy of social science before and that done afterwards. But we also had occasion to note the lasting relevance of Nagel's work. If memory of it is somewhat clouded nowadays, it is so perhaps because we tend to regard everything associated with the received view as formalist in orientation. But just as Neurath shows a protopragmatist side of early logical empiricism, so Nagel shows the pragmatist side of mid-century American scientific philosophy. Thus there did obtain a certain continuity of work on or toward nonreductive naturalism, but it was not signaled, perhaps not even noticed, as such at all. Nowadays, of course, Nagel's own pioneering work risks neglect despite the resurgence of interest in pragmatism, being a representative of its scientific wing and thus the victim of Rorty's postmodern rhetoric.

Besides the problem of accessibility for English-language readers, noted above, in the case of Neurath the neglect appears very much to have been a *politicum* – at least insofar as Popper's arguments either misfire or (like Hayek's) grossly misrepresent his position (which is very similar as regard to methodological individualism is concerned). The political dimension of philosophy of social science may here be seen reflected in the academic debate (or rather nondebate) itself. What was in effect lost – as a further investigation of Neurath's work would show – was not merely one early logical empiricist philosophy of social science, but rather a logical empiricist conception of "critical" social science that the prepostpositivist mainstream could hardly dream of.

How severe that loss was need not be decided here; what should be clear, however, is that the widely perceived postwar alternative in analytic philosophy of social science between logical empiricist reductionism and critical rationalist universalism by no means exhausted what actually had been on offer. Enough has been said, I hope, to show that there existed, at least in outline, a postpositivist analytic philosophy of social science *avant la lettre* – not only during the postwar period of dominance by the received view, but still before World War II. Whether this qualifies as a postmodern irony I must leave the reader to decide.[27]

Notes

1 Dummett has expressed the criterion as follows: "What distinguishes analytical philosophy, in its diverse manifestations, from other schools is the belief, first, that a philosophical account of thought can be attained through a philosophical account of language, and, secondly, that a comprehensive account can only be so attained. . . . On this characterization, therefore, analytical philosophy was born when the 'linguistic turn' was taken" (1993:4–5).

2 See Leonard (forthcoming) for an extended discussion of Menger's influence.

3 See Zilsel (2000), especially the editors' introduction.

4 It is consistent with my analysis that the one journal specifically dedicated to the field, *Philosophy of Social Science*, was not founded until 1971 with I. C. Jarvie, a former student of Popper's, and J. O. Wisdom, John O'Neill, and Harold Kaplan as editors.

5 One area not covered in the present survey is the philosophy of psychology, since it is commonly understood as a scientific variant of philosophy of mind. Feigl and Brodbeck (1953) thus reserved a special section for "Philosophical Problems of Biology and Psychology" beside that of "Philosophy of the Social Sciences." Smith (1986) found that throughout most of the century the relations between both practicing psychological theorists and philosophers of behaviorist persuasion were not close – less close, we may add, than what is suggested by the work that emerged from Feigl's Minnesota Center for the Philosophy of Science in the late 1950s and early 1960s. Similarly, the emergence of interest on the part of analytic philosophers in psychoanalysis can only be mentioned here (Wisdom 1953, MacIntyre 1958).

6 Winch conceded the mistake in the 1990 preface to the second edition of his 1958 work.

7 As canonized by C. G. Hempel (cf. 1965), the deductive-nomological (DN) model defines scientific explanation as an argument according to which a statement expressing an observation or a predicted consequence of a theory (the *explanans*) is to be regarded as following deductively from statements of the relevant laws and a statement of initial conditions obtaining. For the debate about the DN model see Salmon (1989).

8 The thesis of methodological individualism most broadly understood forbids appeal to social wholes or collectives that cannot be further reduced to their individual constituents and their relations (its opposition being methodological holism). The thesis can be subdivided into ontological, epistemological and semantic or conceptual aspects or subtheses of methodological individualism, the various combinations of which are plausible in different degrees, as will be noted in the text below.

9 The exception is Brodbeck (1958). One other public instance of this dispute will be briefly mentioned below (Nagel vs.Watkins), and one "underground" instance of it will be considered in detail in the following section (Neurath vs. Popper).

10 Since it is customary to cite R. B. Braithwaite alongside Nagel as a pioneer of interpreting teleological explanations as causal explanations applicable to self-regulatory systems (e.g., Hempel [1959] 1965:326n), it may be noted that Braithwaite's original lecture of 1946 (which became chapter 10 of his book of 1953) did not yet contain the footnote quotation of Rosenblueth et al.'s "Teleological behavior becomes synonymous with behavior controlled by negative feedback" (quoted in Braithwaite 1953:328), nor did it or its reprint use the term "self-regulating system" (yet it

employed "goal-directed behavior" abundantly). While Braithwaite pioneered the idea of a causal but nonreductive interpretation of "functional explanations" and as such left Nagel "indebted" to him (1953 reprint: 546n), it was left, it seems, to Nagel in his 1952 and 1953 works to make fully explicit the connexion with self-regulation – moreover, in his 1952 and in great detail in his 1957 works, to link this interpretation with the issue of functional explanations in social science (which Braithwaite did not touch upon).

11 See Nagel (1957, 1961:ch. 14, sect.2) and Hempel (1959).

12 See Nagel (1961:ch. 14, sect. 3), a passage also cited in the bibliographical footnote 12 in Lukes ([1968] 1994:458).

13 See Keat (1981:40) with its reference to Nagel (1961:ch. 13, sect. 5).

14 See the back cover of the 1979 reprint of Nagel (1961). From the later 1970s onwards, reflecting interactions with his then colleague T. S. Kuhn, Hempel himself was much concerned to effect such rapprochement; see, for instance, Hempel (1988).

15 As noted above, the theory of rational choice, now so influential, was then still only gathering the momentum documented by the anthologies of Campbell and Sowden (1985), Elster (1986), and Moser (1990).

16 That the latter still occasions concern and periodic need for reorientation that looks back to the classics can be seen from anthologies like Turner (1996).

17 Most prominently so, perhaps in Braybrooke (1987), Little (1991), Hollis (1994), Fay (1996).

18 Concerning the former point it may be noted that up to the postpositivist turn (as it happens) Neurath's contribution (1944) to the *International Encyclopedia of Unified Science* series was the only piece of his on philosophy of social science (besides his earlier and equally programmatic 1932a work translated first in 1959) that was available in English: rewritten in some haste after the first manuscript was left behind in Holland when he fled the Nazi invasion, its nonstandard perspective and arguments were not helped by his idiosyncratic English. Carnap was moved to disclaim coeditorial responsibility and Nagel made clear to Morris (the other co-editor of the series) that he was scandalized by the monograph (ASP RC102-55-25, 102-55-24). Compare also Hempel 1969 (that is, before Hempel's pragmatist turn (see note 14 above) on the orthodox reception of Neurath's philosophy of social science.

19 The original term used by Neurath in a letter to Carnap was "*Gesamthaltung*" (ASP RC029-09-45). For a discussion of the interpretation of Neurath as an epistemological naturalist see Uebel (1991).

20 This point is developed further in Uebel (1996).

21 On the former point see Frenkel-Brunswik (1954), on the latter Uebel (1993).

22 So called in Cartwright et al. (1996).

23 This is clearly not the same type of view as the more common historicism which asserts the historical individuality of cultures and cultural products like concepts and theoretical conceptions (and is sometimes attached to the work of Thomas Kuhn).

24 Compare also Neurath's opposition to Plato's social theory (Neurath and Lauwerys 1945), which was applauded, incidentally, by Russell (1945), who did not shy from ridiculing other theses of Neurath's (Russell 1940:186). In distinguishing his own critique of Plato from Neurath's, Popper rather tendentiously tarred him with the brush of having "defended Hegel" ([1945] 1966:vol. 1, 325).

25 It may also be noted that the later Neurath, albeit only in his correspondence, even criticized fellow logical empiricists like Carl Hempel and Edgar Zilsel for their seeming attempts to delineate strict laws of history.

26 Also in Neurath (1936b) which Popper cites in a different connection.

27 I thank the editors for helpful comments and suggestions.

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Part II

Programs

Critical Theory as Practical Knowledge: Participants, Observers, and Critics

James Bohman

When considered in light of the history of the philosophy of the social sciences, Critical Theory occupies a distinctive place. It has long sought to distinguish its aims, methods, theories, and forms of explanation from standard understandings in both the natural and the social sciences. Instead, it has claimed that social inquiry ought to combine, rather than separate or eliminate, the poles of explanation and understanding, structure and agency. Such an approach, Critical Theorists argue, permits their enterprise to be practical in a distinctive sense. They do not aim at some independent goal, but rather (as in Horkheimer's famous definition) seek "to liberate human beings from all circumstances that enslave them" (Horkheimer 1982:244). This task requires the interplay between philosophy and social science as well as multidimensional and interdisciplinary social research (Horkheimer 1993).

If it is to address *all* such circumstances, it must employ explanations and interpretations from a variety of perspectives. Sometimes the critic who has this aim must employ practical knowledge in adopting a stance that goes beyond the limits of agents' local practical knowledge. Such a stance is often called "objective." Most proponents of Critical Theory in the broad sense of the term (both inside and outside the Frankfurt School) now see this claim to be misleading at best, given the epistemic situation of the critic as simply one practical agent among others. The question then is not only how inquiry can be both interpretive and explanatory but also descriptive and normative at the same time. This alternative avoids both a pure "insider's" and participant's standpoint (in the manner of hermeneutics) and a pure "outsider's" or observer's standpoint (in the manner of naturalistic social theories). The distinctively normative standpoint characteristic of critical social inquiry has been called the "perspective of a critical-reflective participant" (Habermas 1984; McCarthy and Hoy 1994:81). As the hyphen indicates, this is not some particular perspective or the exclusive domain of the social theorist, but a combination of various perspectives at different levels.

Horkheimer's definition does not suggest a specific end to be achieved, but rather identifies a distinctively practical activity. The act of social criticism provides the basic structure of inquiry for a certain type of social science. The distinguishing mark of critical social science is not that it produces some novel form of social scientific knowledge. Rather, by transforming reflexive social inquiry into practical knowledge (that is, the knowledge of practical knowledge), agents gain precisely the sort of knowledge needed for effective social agency and freedom in the social world.[1] This practical knowledge is tied up with the capacities of agents to adopt and to employ a variety of social perspectives, and this capacity in turn provides the epistemic basis for social criticism. Theories are to be understood for their practical implications.

Such a practical account of social inquiry has much in common with pragmatism, old and new (Bohman 1999a, 1999b). As with pragmatism, Critical Theory came gradually to reject the demand for a scientific or objective basis of criticism grounded in a grand theory. This demand proved hard to square with the demands of social criticism directed to particular audiences at particular times with their own distinct demands and needs for liberation or emancipation. The first step was to move the critical social scientist away from seeking a single unifying theory to employing many theories in diverse historical situations. Rather, it is better to start with agents' pretheoretical knowledge. The issue for critical social inquiry is not only how to relate pretheoretical and theoretical knowledge of the social world, but also how to move among different irreducible perspectives. The second step is to show that such a practical alternative not only provides the basis for robust social criticism, but also that it better accounts for and makes use of the pluralism inherent in various methods and theories of social inquiry. My argument here is that only this practical form of inquiry can meet the epistemic and normative challenges of social criticism.

Critics, Observers, and Participants: Two Forms of Critical Theory

There are two common, general answers to the question of what defines the distinctive features of critical social inquiry: one practical and the other theoretical. The latter approach claims that critical social inquiry employs a distinctive theory that unifies such diverse approaches and explanations. On this view, Critical Theory constitutes a comprehensive social theory that will unify the social sciences and underwrite the superiority of the critic. The first generation of Frankfurt School Critical Theory sought such a theory in vain before dropping claims to social science as central to their program (Wiggershaus 1994). By contrast, according to the practical approach, theories are distinguished by the form of politics in which they are embedded and the method of verification that this politics entails. I defend a version of this approach here, grounded in the

pragmatic conception of democracy as a mode of inquiry that seeks to find solutions to problems that are acceptable to all those affected. But to claim that critical social science is best unified practically and politically rather than theoretically or epistemically is simply not to reduce it to democratic politics. It becomes rather the mode of inquiry that participants may adopt in their social relations to others.

Before turning to such a practical interpretation of critical social inquiry, it may be appropriate to consider why the theoretical approach was favored for so long and by so many Critical Theorists. First, it has been long held that only a comprehensive social theory could unify critical social science and thus provide a "scientific" basis for criticism that goes beyond the limits of lay knowledge. Second, not only must the epistemic basis of criticism be independent of agents' practical knowledge, but also an explanation must be correct regardless of its political effects on a specific audience. So conceived, social criticism is then a two-stage affair: first, inquirers independently discover the best explanation using the available comprehensive theory; then, second, they persuasively communicate its critical consequences to participants who may have false beliefs about their practices.

Starting with Marx's historical materialism, large-scale macrosociological and historical theories have long been held to be the most appropriate explanatory basis for critical social science. However, one problem is that comprehensiveness does not ensure explanatory power. Indeed, there are many such large-scale theories, each with its own distinctive and exemplary social phenomena that guide their attempt at unification.[2] A second problem is that a close examination of standard critical explanations, such as the theory of ideology, shows that they typically appeal to a variety of different social theories (Bohman 1996). Habermas's actual employment of critical explanations bears this out. His criticism of modern societies turns on the explanation of the relationship between two very different theoretical terms: a microtheory of rationality based on communicative coordination and a macrotheory of the systemic integration of modern societies in such mechanisms as the market (Habermas 1987).

Not only does the idea of a comprehensive theory presuppose that there is one preferred mode of critical explanation, it also presupposes that there is one preferred goal of social criticism, a socialist society that fulfills the norm of human emancipation. Only with such a goal in the background does the two-step process of employing historical materialism to establish an epistemically and normatively independent stance make sense, indeed one that is independent such that the possibility that participants may reject such explanations does not matter for the correctness or incorrectness of criticism. Pluralistic inquiry suggests a different norm of correctness: that criticism must be verified by those participating in the practice and that this demand for practical verification is part of the process of inquiry itself.

Despite his ambivalence, Habermas has given good reasons to accept the practical and pluralist approach. Just as in the analysis of modes of inquiry tied to distinct knowledge-constitutive interests, Habermas accepts that various theories

and methods each have "a relative legitimacy." Indeed, like Dewey he goes so far as to argue that the logic of social explanation is pluralistic and elides the "apparatus of general theories." In the absence of any such general theories, the most fruitful approach to social scientific knowledge is to bring all the various methods and theories into relation to each other: "Whereas the natural and the cultural or hermeneutic sciences are capable of living in mutually indifferent, albeit more hostile than peaceful coexistence, the social sciences must bear the tension of divergent approaches under one roof..." (Habermas [1967] 1988:3). In *The Theory of Communicative Action*, Habermas casts critical social theory in a similar pluralistic, yet unifying way. In discussing various accounts of societal modernization, for example, Habermas argues that the main existing theories have their own "particular legitimacy" as developed lines of empirical research, and that Critical Theory takes on the task of critically unifying the various theories and their heterogeneous methods and presuppositions. "Critical Theory does not relate to established lines of research as a competitor; starting from its concept of the rise of modern societies, it attempts to explain the specific limitations and relative rights of those approaches" (Habermas 1987:375). Habermas argues that methodological pluralism is a pervasive feature of social inquiry. The goal of Critical Theory is to bring the results of these methods together "under one roof."

This tension between unity and plurality leads in the two directions that I have already developed, one practical and the other theoretical. What might be called the "Kantian" approach proceeds case by case, seeing the way in which these theories run up against their limits in trying to extend beyond the core phenomena of their domain of validity (Bohman 1991:ch. 2). This approach is not theoretical in orientation, but more akin to "social science with a practical intent" to use Habermas's older vocabulary (Habermas 1971). The Kantian answer is given sharpest formulation by Weber in his philosophy of social science. While recognizing the hybrid nature of social science as causal and interpretive, he sought explanations of particular phenomena that united both dimensions. For example, in his *Protestant Ethic and the Spirit of Capitalism* he brought the macroanalysis of institutional structures together with the microanalysis of economic rationality and religious belief (Weber 1958). According to this contrasting approach, "the relative rights and specific limitations" of each theory and method are recognized by assigning them to their own particular (and hence limited) empirical domain rather than establishing these judgments of scope and domain through a more comprehensive theory that encompasses all others.

The second approach I term "Hegelian." Here theorists seek to unify social scientific knowledge in broad comprehensive theories that produce a general history of modern societies. But general theories provide "general interpretive frameworks" on which it is possible to construct "critical histories of the present" (McCarthy and Hoy 1994:229–30). Even this account of a comprehensive theory hardly eliminates competing histories that bring together different theories and methods. Rather than aiming at a single best history, "Hegelian" theories of this

sort are instead better seen as practical proposals whose critical purchase is not moral and epistemic independence but practical and public testing according to criteria of interpretive adequacy.

What then is the basis for normative social criticism? Could it be a more comprehensive theory of rationality, as Habermas develops in his account of communicative reason? Yet even this theory of rationality has to be understood as a practical reconstruction if it is to avoid what Rorty calls "the ambiguity of rationality," between its statuses as "a cognitive faculty and a moral virtue." Rorty wants to keep them distinct. "The epistemological notion of rationality concerns our relation to something nonhuman, whereas the moral notion concerns our relations to our fellow human beings" (Rorty 1996:74).

The problem for the practical conception of critical social inquiry is then to escape the horns of a dilemma: it should be neither purely epistemic and thus overly cognitivist, nor purely moralistic. Neither provides sufficient critical purchase. In the case of the observer, there is too much distance, so much so that it is hard to see how the theory can motivate criticism; in the case of the pure participant perspective, there is too little distance to motivate or justify any criticism at all. It is also the same general theoretical and methodological dilemma that characterizes the debates between naturalist and antinaturalist approaches. While the former sees terms such as rationality as *explanans* to explain away such phenomena as norms, the latter argues that normative terms are not so reducible and thus figure in both *explanans* and *explanandum*. The best practical account here reconciles Rorty's ambiguity by putting the epistemological component in the social world, in our various cognitive perspectives toward it that include the normative perspectives of others. The ambiguity is then the practical problem of adopting different points of view, something that reflective participants in self-critical practices must already be able to do.

Social Inquiry as Practical Knowledge

This shift to "perspective taking" is already implicit in the reflexivity of practical forms of Critical Theory. Rather than look for the universal and necessary features of social scientific knowledge, Critical Theory has instead focused on the social relationships between inquirers and other actors in the social sciences. Such relationships can be specified epistemically in terms of the perspective taken by the inquirer on the actors who figure in their explanations or interpretations. The two dominant and opposed approaches adopt very different perspectives. On the one hand, naturalism gives priority to the third-person or explanatory perspective; on the other hand, the antireductionism of interpretive social science argues for the priority of understanding and so for an essential methodological dualism. Critical Theory since Horkheimer has long attempted to offer an alternative to both views.

Habermas and other Critical Theorists rightly apply the term "technocratic" to social inquiry that only develops optimal problem-solving strategies in light of purely third-person knowledge of the impersonal consequences of all available courses of action. Pragmatists from Mead to Dewey offer similar criticisms (Habermas 1971, 1973; Dewey 1988). Such inquiry models the social scientist on the engineer, who masterfully chooses the optimal solution to a problem of design. For the social scientist *qua* an ideally rational and informed actor, "the range of permissible solutions is clearly delimited, the relevant probabilities and utilities precisely specified, and even the criteria of rationality to be employed (for example, maximization of expected utilities) is clearly stated" (Hempel 1965:481). This technocratic model of the social scientist as detached observer (rather than reflective participant) always needs to be contextualized in the social relationships it constitutes as a form of socially distributed practical knowledge.

By contrast with the engineering model, interpretive social science takes up the first-person perspective in making explicit the meaningfulness of an action or expression. Interpretations as practical knowledge are not based on some general theory (no matter how helpful or explanatory these may be when interpretation is difficult), but reconstruct an agent's own reasons, or at least how these reasons might seem to be good ones from a first-person perspective. This leaves an interpreter in a peculiar epistemic predicament: what started as the enterprise of seeing things from others' points of view can at best approximate that point of view only by providing the best interpretation *for us* of how things are *for them*. As a matter of interpretive responsibility, there is no getting around the fact that ethnography or history is our attempt "to see another form of life in the categories of our own" (Geertz 1971:16–17, Bohman 1991:132). The only way out of this problem is to see that there is more than one form of practical knowledge.

Naturalist and hermeneutic approaches see the relationship of the subject and object of inquiry as forcing the social scientist to take either the third-person or first-person perspective. However, critical social science necessarily requires complex perspective taking and the coordination of various points of view, minimally that of social scientists with the subjects under study. The "second-person perspective" differs from both third-person observer and the first-person participant perspectives in its specific form of practical knowledge. It employs the know-how of a participant in dialogue or communication (Bohman 2000a). This perspective provides the alternative to opposing perspectives especially when our first-person knowledge or third-person theories get it wrong (Bohman 2000b).

When faced with interpreting others' behavior we quickly run into the limits of first-person knowledge *simpliciter*. From a third-person perspective, it is indeterminate whether behavior follows some common rule or merely represents some idiosyncrasy. Third-person accounts face the same "gerrymandering problem" as made clear in the private language argument (Brandom 1994:28ff). That is, it is always possible to interpret some behavior as related to many different and even incompatible contextual factors. Neither the interpreter's nor the observer's perspectives are sufficient to specify these opaque intentional contexts for others.

Since actions therefore underdetermine the interpretations that may be assigned to them by third-person or first-person interpreters, there is no alternative to offering interpretations of agents' self-interpretations that can be determined practically in on-going dialogue and interaction. For social scientists as well as participants in practices more generally, the adjudication of such conflicts requires mutual perspective taking, which is its own mode of practical reasoning.

Theories of many different sorts locate interpretation as a practice, that is, in acts and processes of ongoing communication. Communication is seen from this perspective as the exercise of a distinctive form of practical rationality. A critical theory of communicative action offers its own distinctive definition of rationality, one that is epistemic, practical, and intersubjective. For Habermas, for example, rationality consists not so much in the possession of particular knowledge, but rather in "how speaking and acting subjects acquire and use knowledge" (Habermas 1984:11). I call any such account "pragmatic" because it shares a number of distinctive features with other views that see interpreters as competent and knowledgeable agents. Most importantly, a pragmatic approach develops an account of practical knowledge in the "performative attitude," that is, from the point of view of a competent speaker. A theory of rationality can be a reconstruction of the practical knowledge necessary for establishing social relationships. This reconstruction is essential to understanding the commitments of the reflective participant, including the critic.

There are two general arguments for a theory that assumes the irreducibility of such a perspective. The first is that interpreting is not merely describing something. Rather, it establishes commitments and entitlements between the interpreter and the one interpreted. Second, in doing so the interpreter takes up particular *normative* attitudes. These "normative attitudes" must be those of the interpreted. In interpreting one is not just reporting, but rather expressing and establishing one's attitude toward a claim, such as when the interpreter takes the interpreted to say something to be true, or to perform an act that is appropriate according to social norms. Some such attitudes are essentially two-person attitudes: the interpreter does not just express an attitude in the first-person perspective alone, but rather incurs a commitment or obligation to others by interpreting what others are doing (Brandom 1994:79). To offer an interpretation that is accepted is to make explicit the operative social norms and thus to establish the normative terms of a social relationship.

The critical attitude shares with interpretation a structure derived from the second-person perspective. Here an agent's beliefs, attitudes, and practices cannot only be interpreted as meaningful or not, but must also be assessed as correct, incorrect, or inconclusive. Nonetheless, the second-person perspective is not yet sufficient for criticism. In order for an act of criticism itself to be assessed as correct or incorrect, it must often resort to tests from the first- and third-person perspectives as well.

Like the third-person perspective, the second-person perspective holds out the possibility of employing other normative standards. However, unlike the

third-person critic who must claim epistemic superiority over other participants, the second-person perspective is interperspectival rather than transperspectival. Reflective participants must take up all stances; they assume no single normative attitude as proper for all critical inquiry. Only such an interperspectival stance is fully dialogical, giving the inquirer and agent equal standing.

It is this political relationship of equal agency that critical inquirers seek to establish. They address those they criticize as fellow reflective participants in practices, including practices of inquiry. The critic engages in the inquiry into the basis of the cooperative character of the practices that the first-person interpreter simply presupposes, as "prejudgments" or "prejudices" in the hermeneutic sense. If indeed all cooperative activity "involves a moment of inquiry" (Putnam 1994:174), then they also need a moment of self-reflection on the prejudgments of such inquiry itself. It is this type of reflection that calls for a distinctively practical form of critical perspective taking. If critical social inquiry is inquiry into the basis of cooperative practices as such, it takes practical inquiry one reflective step further. The inquirer does not carry out this step alone, but rather with the public whom the inquirer addresses. As in Kuhn's distinction between normal and revolutionary science, second-order critical reflection considers whether or not the framework for cooperation itself needs to be changed. Such criticism is directed at current institutions as well as toward formulating new terms of cooperation under which problems are solved.

Various perspectives are appropriate in different critical situations. When participants hold norms constant, then instrumental and strategic modes of inquiry are useful in determining the objective and social consequences of policies and practices. In this case, critical reflection is not confined to the limits of first-person knowledge, nor does it suffer from the gerrymandering problem of independent standards of rationality. In order to occupy the normative realm between the first- and the third-person perspectives, detailed social science (now understood as second-order cooperative inquiry into cooperative practices) is necessary. If it is to identify all the problems with cooperative practices of inquiry, it must be able to occupy and account for a variety of perspectives. Only then will it enable public reflection among free and equal participants.

Consider the problem of the availability of experimental drugs for the treatment of AIDS. It is clear that the development and testing of drugs is a matter for scientific experts who learn more and more about the virus and its development in the body. So long as experts unproblematically engage in first-order problem solving, AIDS patients are willing to grant these experts a certain authority in this domain. Nonetheless, the continued spread of the epidemic and lack of effective treatments brought about a crisis in expert authority, an "existential problematic situation" in Dewey's sense (Dewey 1988:492). By defining expert activity through its social consequences and by making explicit the terms of social cooperation between researchers and patients, lay participants reshape the practices of gaining medical knowledge and authority. Indeed, as Stephen Epstein points out, the effects of AIDS activism did not just concern extrascientific problems

such as research funding, but rather challenged the very standards of statistical validity employed in experimental trials (Epstein 1996:Part II). The affected public changed the normative terms of cooperation and inquiry in this area in order that institutions could engage in acceptable first-order problem solving. It required reflective inquiry into scientific practices and their operative norms.

This public challenge to the norms on which expert authority is based may be generalized to all forms of research in cooperative activity. It suggests the transformation of some of the epistemological problems of the social sciences into the practical question of how to make their forms of inquiry and research open to public testing and public accountability. This demand also means that some sort of "practical verification" of critical social inquiry is necessary. How do we judge its practical consequences, especially if it is second-order rather than first-order problem solving? For such second-order reflection involves testing existing norms. Thus, it also must test norms of social inquiry, including norms of public justification. Here the emphasis will be on practical orientation rather than theoretical unification. Such an account incorporates the problem of pluralism as a moment within critical inquiry itself.

Pluralism and Critical Inquiry

A practical approach to Critical Theory responds to pluralism in the social sciences in two ways, once again embracing and reconciling both sides of the traditional opposition between epistemic (explanatory) and nonepistemic (interpretive) approaches to normative claims. On the one hand, it affirms the need for general theories, while weakening the strong epistemic claims made for them in underwriting criticism. On the other hand, it situates the critical inquirer in the pragmatic situation of communication, seeing critics as making strong claims for the truth or rightness of their critical analysis. This is a presupposition of the critic's discourse, without which it would make no sense to engage in criticism of others.

But once again this strong moral cognitivism is offset by an equally strong epistemic concession concerning the situated character of such moral claims, particularly in light of cultural pluralism and the attendant diversity of normative perspectives. In light of these facts of complex and pluralistic modern societies, it is hard to see how critics as reflective participants would not face the same basic epistemic constraints regardless of how they employ theoretical knowledge, including the knowledge of some moral theory or of a social theory of moral norms. Such knowledge is not only interpretive in the application, but the theories themselves embody various social perspectives.

A good test case for the practical and pluralist conception of Critical Theory based in perspective taking would be to give a more precise account of the role of general theories and social scientific methods in social criticism, including moral

theories or theories of norms. Rather than serving a justifying role in criticisms for their transperspectival comprehensivness, theories are better seen as interpretations that are validated by the extent to which they open up new possibilities of action that are themselves to be verified in democratic inquiry. Not only that, but every such theory is itself formulated from within a particular perspective. General theories are then best seen as practical proposals whose critical purchase is not moral and epistemic independence but practical and public testing according to criteria of interpretive adequacy. This means that it is not the theoretical or interpretive framework that is decisive, but the practical ability in employing such frameworks to cross various perspectives in acts of social criticism. The second-person perspective provides the framework for the justification in any such process, since practical success in democratic terms is to establish terms of social cooperation that all can accept. In the above example, it is accomplished in taking the patients' perspectives seriously in altering practices of medical inquiry into AIDS.

Why is this practical dimension decisive? There seems to be an indefinite number of perspectives from which to formulate possible general histories of the present. Merely to identify a number of different methods and a number of different theories connected with a variety of different purposes and interests leaves the social scientist in a rather hopeless epistemological dilemma. Either the choice among theories, methods, and interests seems utterly arbitrary, or the Critical Theorist has some special epistemic claim to survey the domain and make the proper choice for the right reason. On the one hand, the former, more skeptical, horn of the dilemma is endorsed by "new pragmatists" like Richard Rorty (Rorty 1991) who sees all such knowledge as purpose-relative, as well as by Weber at more decisionistic moments of his methodological writings. The latter, perhaps Hegelian, horn demands objectivist claims for social science generally and for the epistemic superiority of the Critical Theorist in particular – claims that Habermas and other Critical Theorists have been at pains to reject (Weber 1949, Habermas 1973:38). Is there any way out of the epistemic dilemma of pluralism that preserves the possibility of criticism without endorsing epistemic superiority?

The way out of this dilemma has already been indicated by a reflexive emphasis on the social context of critical inquiry and the practical character of social knowledge it employs. It addresses the subjects of inquiry as equal reflective participants, as knowledgeable social agents. In this way, the asymmetries of the context of technical control are suspended; this means that critical social inquiry must be judged by a different set of practical consequences, appealing to an increase in the "reflective knowledge" that agents already possess to a greater or lesser degree. As agents in the social world themselves, social scientists participate in the creation of the contexts in which their theories are publicly verified. The goal of critical inquiry is then not to control social processes or even to influence the sorts of decisions that agents might make in any determinate sort of way. Instead, its goal is to initiate public processes of self-reflection (Habermas 1971:40–1, Bohman 1996:ch. 5). Such a process of deliberation is not guaranteed success

in virtue of some comprehensive theory. Rather, the critic seeks to promote just those conditions of democracy that make it possible for such a process to be the best one that is available upon adequate reflection.

When understood as solely dependent upon the superiority of theoretical knowledge, the critic has no foothold in the social world and no way to choose among the many competing approaches and methods. But such a conception is based on the wrong model of verification for critical social inquiry. In practical verification, agents may not in the end find these insights acceptable and thus may not change their second-order self-understandings. The publicity of such a process of practical verification entails its own particular standards of critical success or failure that are related to social criticism as an act of interpretation addressed to those who are being criticized. An account of such standards then has to be developed in terms of the sort of abilities and competences that successful critics exhibit in their criticism. Once more this reveals a dimension of pluralism in the social sciences: the pluralism of social perspectives. As addressed to others in a public by a speaker as a reflective participant in a practice, criticism certainly entails the ability to take up the normative attitudes of the second-person perspective.

But publicity alone still does not identify what is distinctive about critical social inquiry. Once again, it is doubly distinctive. It is criticism addressed in the normative attitude of the second-person perspective, but in relation to other perspectives: the first-person, second-person, and third-person perspectives are all interrelated in different ways in different forms of criticism. The public process of testing for agreement or disagreement by those addressed is the ultimate practical verification; this testing process requires self-reflectively employing multiple pragmatic perspectives in communication as well as reflexive knowledge of the limits of the social and epistemic practices in which acts of criticism are embedded.

Reflexivity, Perspective Taking, and Practical Verification

If the argument of the last section is correct, a pragmatic account is inevitably methodologically, theoretically, and perspectivally pluralistic. Any kind of social scientific method or explanation-producing theory can be potentially critical. There are no specific or definitive social scientific methods of criticism or theories that uniquely justify the critical perspective. One reason for this is that there is no unique critical perspective, even for a reflexive theory that provides a social scientific account of acts of social criticism and their conditions of pragmatic success.

Looking at what critics do rather than at their theories suggests that the more typical feature of socially situated acts of criticism is their cross-perspectival character: criticisms cross over from the third-person to the first-person, the second-person to the first-person plural perspective, and so on. Such multiperspectival criticism allows for the reflexive distance necessary for criticism, even in the case of a single person reflecting upon the possibility of self-deception and manipulation.

The second-person perspective does have justificatory priority, but no priority as the source of criticism. It has justificatory priority because of what a critic does in acts of criticism: C (the critic) communicates CC (the critical claim) to S (the audience of the criticism). Such a claim must be such that S could accept CC given the proper changes in the normative attitudes of S. The critic then is successful in the act of criticism only by adopting the attitude of S as the second person (not of S as an object of manipulation in the third-person perspective), that is, by adopting the normative attitude of the second person to whom CC is directed.

Those Critical Theorists who have favored the view that there are specific and comprehensive critical theories have tended also to argue that criticism ultimately must be given from an objective, third-person perspective. Standard theories of ideology see agents' social beliefs as systematically false in ways that require the independent standpoint of a theory to be untangled. Such a theory in turn requires that criticism be objective in the sense that it has no perspective: it is nonperspectival in the sense that it has no point of view; it is a view from nowhere. Instead, third-person, explanatory criticisms of ideology are trans-perspectival; they show the limits of participants' first-person perspectives from the point of view of a theoretical explanation grounded in their own experience of the social world (as did Marx in discussing the exploitation of workers both from the perspective of situated historical experience and an explanatory economic theory). Rejecting such nonperspectival theories for interperspectival critical explanations provides the basis for going beyond a "mere" pluralism and to a "critical" pluralism that is able to adjudicate among the often contradictory claims of theories and explanations in the social sciences and among often conflicting perspectives in reflective practices.

The indeterminacy of the third-person perspective (its inadequacy for non-arbitrarily selecting among competing interpretations) suggests that the normative attitude of the critic cannot be confined to a single social perspective or the supposed correctness of the particular content of any theory. Rather, whatever objectivity a theory can achieve for criticism is "a matter of perspectival form and not of non-perspectival or cross-perspectival content" (Brandom 1994:600). If they see themselves in this way, theorists do not claim that their theory is true because it is a uniquely accurate description of the structure of the social world. The appropriate form of objectivity is a critical one: that the social scientist can identify the proper combination of perspectives that reveals the moral and epistemic inadequacies of any particular perspective. As is the case for the theory of ideology's claim that a belief is false, it shows those under the sway of an ideology such as racism that this falsehood can be seen if the agent adopts a perspective different from his or her own, say that of different social actors whose perspective denies the justificatory force of such beliefs. The unmasking role of social criticism is then a matter of relating the proper perspectives in order to expose the normative pretensions of various sorts of claims to truth or moral correctness that are made in various practices.

Rather than claiming objectivity in a transperspectival sense, Critical Theorists have always insisted that their form of social inquiry takes a "dual perspective" (Habermas 1996:ch. 1, Bohman 1991:ch. 4). This dual perspective has been expressed in many different ways. Critical Theorists have always insisted that critical approaches have dual methods and aims: they are both explanatory and normative at the same time, adequate both as empirical descriptions of the social context and as practical proposals for social change. This dual perspective has been consistently maintained by Critical Theorists in their debates about social scientific knowledge, whether it is with regard to the positivism dispute, universal hermeneutics, or micro- or macrosociological explanations.

In the dispute about positivist social science, Critical Theorists rejected all forms of reductionism and insisted on the explanatory role of practical reason. In disputes about interpretation, Critical Theorists have insisted on social science not making a forced choice between explanation and understanding. Even if social scientists can only gain epistemic access to social reality through interpretation, they cannot merely repeat what agents know practically in their "explanatory understanding." Here we might think of explanations that create micro and macro linkages, as between intentional actions pursued by actors for their own purposes and their unintended effects due to interdependencies of various sorts. Such dual perspective explanations and criticism both allow the reflective distance of criticism and the possibility of mediating the epistemic gap between the participants' more internal and the critics' more external point of view. Given the rich diversity of possible explanations and stances, contemporary social science has developed a variety of possible ways to enhance critical perspective taking.

Such a dual perspective provides a more modest conception of objectivity: it is neither transperspectival objectivity nor a theoretical metaperspective, but always operates across the range of possible practical perspectives that knowledgeable and reflective social agents are capable of taking up and employing practically in their social activity. It is achieved in various combinations of available explanations and interpretive stances. With respect to diverse social phenomena at many different levels, critical social inquiry has employed various explanations and explanatory strategies. Marx's historical social theory permitted him to relate functional explanations of the instability of profit-maximizing capitalism to the first-person experiences of workers. In detailed historical analyses, feminist and ethnomethodological studies of the history of science have been able to show the contingency of normative practices (Epstein 1996, Longino 1990). They have also adopted various interpretive stances. Feminists have shown how supposedly neutral or impartial norms have built-in biases that limit their putatively universal character with respect to race, gender, and disability (Mills 1997, Minnow 1990). In all these cases, claims to scientific objectivity or moral neutrality are exposed by showing how they fail to pass the perspectival tests of public verification of all those affected.

More modest forms of third-person criticism need not be so narrowly interpreted. The third-person perspective identifies nonintentional processes, feedback

mechanisms, by-products, and other features of complex social structures not under fully voluntary control. This perspective may reveal the unintended consequences of profit maximization, self-defeating strategies or policies, the macroconsequences of microbehavior, and other social scientific mechanisms that operate through agents' intentional actions but produce aggregate and unintended effects that agents do not anticipate. An example is here Thomas Schelling's analysis of tipping points in nonintended residential segregation or of various perverse consequences of state regulatory schemes (Schelling 1971).

The theory of ideology provides a more directly instructive example within traditional critical social inquiry. As the target of dual- or cross-perspective criticism, ideology might be rethought in terms of distortions of communication that give rise to justification under conditions that undermine successful communication, or in terms of limits of the experiences of agents due to social positions. All such biases may also constitute general impediments to the achievement of a democratic form of life (Kelly and Bohman 2000). The common thread of all such examples is the way that they may be embedded in acts of social criticism.

Theories are not the only means by which social scientists are able to expose such normative biases and cognitive limitations. Agents can adopt the first-person singular perspective to criticize the limitations of various other perspectives and by employing acts of criticisms that go across the limits of various perspectives. In this way excluded actors can point out the biases and limitations of traditions that have collective authority, showing how the contours of their experiences do not fit the self-understanding of an institution as fulfilling standards of justice (Mills 1997, Mansbridge 1991). Such criticism requires holding both one's own experience and the normative self-understanding of the tradition or institution together at the same time, to expose bias or cognitive dissonance. It uses expressions of vivid first-person experiences to bring about cross-perspectival insights in actors who do not otherwise see the limits of their cognitive and communicative activities.

Criticism should initiate an act of communication in which the critic takes the role of the representative second person. The "second-person" perspective is particularly useful in practices of justification, to the extent that it comprehends the justification on its own terms as well as why it then fails to convince those to whom it is addressed. For example, offering testimony is an act of social criticism, not simply as an act of self-expression but as embedded in a complex form of communication that exhibits how such true testimony can help agents cross various perspectives and achieve reflective distance from the unreflective and implicit assumptions of their practices.

In these cases, why is it so important to cross perspectives? Here the second-person perspective has a special and self-reflexive status for criticism. Consider the act of crossing from the first-person plural or "we perspective" to the second-person perspective in two reflexive practices: science and democracy. In the case of science the community of experts operates according to the norm of objectivity, the purpose of which is to guide scientific inquiry and justify its claims to

communal epistemic authority. The biases inherent in these operative norms have been unmasked in various critical science studies and by many social movements. As Stephen Epstein has shown for the case of AIDS activism in the 1980s, the claim that scientists had sole authority over the design of medical experiments proved to be much less objective than the scientific community had believed from their perspective. In appealing to this "we perspective," scientists initially refused to address the criticisms and concerns of patients until they were able to show that standards of experimental validity were more contingent and variable and failed to pass the test of the "second-person perspective" of patients involved in trials (Epstein 1996:ch. 5). Helen Longino has also shown that gender bias undermines some of the causal claims of behavioral research. For Longino, this criticism is not merely negative but suggests the need for a better norm of objectivity, "measured against the cognitive needs of a genuinely democratic community" (Longino 1990:236). Similarly, critical social inquiry can not only reflexively call into question the implicit empirical assumptions of liberal democracy (Kelly 2000), but also the central ideal of some of its critics: the benefits of political participation. Empirical studies have shown that existing forms of participation are highly correlated with high status and income, that lower income and status citizens were often unwilling to participate in a public forum for fear of public humiliation, and that public hearings do not have much influence on administrative decision making (Verba et al. 1995, Mansbridge 1991). Adopting the second-person perspective of those who cannot effectively participate does not simply unmask the egalitarian or meritocratic claims and understandings of existing forms of public participation, but rather also suggests normative inquiry into alternative forums and modes of public expression (Bohman 1996). In this way, crossing from the we perspective of the existing community and its practices to the second-person perspective of those affected by its norms initiates reflexive inquiry into more normatively robust practices of science and democracy.

Put in such a context of inquiry, it is possible to see that the normative attitudes of the second person are neither true descriptions nor self-expressive claims. Rather, by treating what others are saying as true, or taking them to be correct or incorrect in their performance, the interpreter establishes nothing more than the possibility of more and perhaps better interpretations and thus the possibility of future dialogue or interpretive exchange. Gadamer puts this in a practical way: "every interpretation establishes the possibility of a relationship with others" (Gadamer 1992:397). Gadamer goes on to say that these relationships institute obligations, since "there can be no speaking that does not bind the speaker and the person spoken to" (Gadamer 1992:397). From the perspective of the participant, competence entails this binding power of interpretations and the implicit know-how of establishing normative relations to which critics appeal. Dialogue so conceived opens up or closes off various practical possibilities with others with whom we are engaged in the process of mutual interpretation or of reaching understanding.

This practical and second-person account of understanding suggests that certain debates about the relationship between criticism and interpretation need to be reformulated. If my argument about the cross-perspectival nature of criticism is correct, then it is a mistake to see such debates as about tradition and its authority on the one hand and social scientific and critical explanations such as the critique of ideology on the other. The plurality of perspectives suggests that there is no inherent conflict between a first-person plural (or the "we" perspective of the internal participant) and a third-person perspective of the external critic. It is rather about correctives needed to expand the normative attitudes of interaction within the second-person perspective. Certainly, it is sometimes necessary to invoke the standards of the community in order to make them explicit enough to judge them; at other times it is necessary to refer to features of the situation of interpretation that may limit its practical possibilities. The real problem is the role of reflection in the context of interpretation. Reflection establishes the possibility of epistemic improvement even when know-how cannot be transformed into theoretical knowledge.

Once we view critical social inquiry as fundamentally practical and employing practical knowledge from a variety of perspectives, then we can see the limits of hermeneutic and instrumental accounts of practical knowledge. The third-person perspective shifts the superiority of the expert in means/ends reasoning to the interpersonal domain; the hermeneutic perspective limits reflection by granting final authority to the community over its norms. Both suggest particular political relations of authority that the social scientists could borrow, even if they might from another perspective be subject to criticism. There are no appeals to suspect claims to the epistemic superiority of general theoretical knowledge that the social sciences cannot underwrite. Rather, theorists stand as participant/critics in the public sphere, establishing social relations of obligation and authority only by addressing their audience as a public. This weak idealization of the possible future audience that can appropriately verify a critical claim is all that a practical and pluralist Critical Theory requires. This idealization is at heart dialogical, articulating an expanded we-perspective of free and equal citizens in a more open and self-reflective democratic practice that is the generalized second-person perspective evaluating our current community.

Conclusion: The Politics of Critical Social Inquiry

My argument here develops a contrast between two interpretations of critical social science: the one is theoretical and dependent on the heritage of German Idealism, and the other is practical and pluralistic in the spirit of pragmatism. The main epistemic weakness of the first interpretation is that it depends on the overly ambitious goal of a comprehensive social theory that unifies all the diverse

methods and practical purposes of social inquiry. This approach fails, due to the fact that all comprehensive theories face the following dilemma: either they privilege one particular perspective as a simplifying assumption or they simply consist of irreducibly many subtheories, each with its own perspective.

The practical alternative offers a solution to this problem by taking critical social theory in the direction of a pragmatic reinterpretation of the verification of critical inquiry that turns seemingly intractable epistemic problems into practical ones. The role of critical social science is to supply methods for making explicit just the sort of self-examination necessary for on-going normative regulation of social life. This practical regulation includes the governing norms of critical social science itself. Here the relation of theory to practice is a different one than among the original pragmatists: more than simply clarifying the relation of means and ends for decisions on particular issues, these social sciences demand reflection upon institutionalized practices and their norms of cooperation. Critical social science is also reflexive in the sense that among such practices is included the examination of their modes of inquiry, including their underlying social norms and relationships established in inquiry itself. Reflective practices cannot remain so without critical social inquiry, and critical social inquiry can only be tested in such practices. One possible epistemic improvement is the transformation of social relations of power and authority into contexts of democratic accountability among political equals (Bohman 1999a, Epstein 1996).

Properly reconstructed, critical social inquiry is the basis for a better understanding of the social sciences as the distinctive form of practical knowledge in modern societies. Their capacity to initiate criticism not only makes them the democratic moment in modern practices of inquiry, but also reflexive inquiry into the basis of social inquiry itself. Normative criticism is thus not only based on the moral and cognitive distance created by relating and crossing various perspectives; it also has a practical goal. It seeks to expand each normative perspective in dialogical reflection and in this way make human beings more aware of the circumstances that restrict their freedom and inhibit their practical knowledge.

Notes

1 I use the term reflexive in two senses. Social scientific knowledge is reflexive to the extent that it promotes reflection on social conditions of knowledge. It is also reflexive in that it can account for itself. As a theory of practical knowledge, it provides knowledge of such knowledge, and as such can show how and when agents can become critical of their own practical knowledge.

2 When considering the complexity of modern societies, a theorist could appeal to Parsonian structural functionalism to explain social differentiation, or to Luhmannian systems theory to explain polycentric and unintended social order.

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Decision Theory and Degree of Belief[1]

Piers Rawling

Rationality is standardly divided into the practical and the theoretical. Practical rationality concerns what we should do; theoretical rationality concerns what we should believe. In telling us to perform that act with the greatest "expected utility" (a maxim associated with Bayes 1763, although the idea predates him – see Jeffrey [1965] 1983:1, 20–1), decision theory concerns the practical. But, in many of its guises (see Fishburn 1981, for an extensive sampling of decision theories), the theory also apparently concerns what we should believe, since it tells us to conform our degrees of belief (intuitively, degrees of confidence that things are or will be so) to the standard Kolmogorov (1933) probability axioms.[2] In one common approach a key goal is to lay down the minimal qualitative conditions on the rationality of preference that are sufficient to prove a representation theorem to the effect that, if an agent obeys the qualitative preference axioms, then certain quantitative facts are true of that agent: not only are his or her preferences ranked in accord with their expected utilities, but also the agent's degrees of belief obey the standard probability axioms. An issue that arises, however, is the extent to which degrees of belief are practical as opposed to theoretical. I shall focus initially, however, on a theory, due to von Neumann and Morgenstern ([1944] 1953), that takes degrees of belief as given.

The Theory of von Neumann and Morgenstern

To say that length can be measured is to say that there is a map from physical objects to the nonnegative reals that preserves the qualitative relation "is at least as long as." This relation conforms to certain qualitative criteria: reflexivity (any object is at least as long as itself), transitivity (if A is at least as long as B, and B at least as long as C, then A is at least as long as C), and connectedness (given any

two objects, one is at least as long as the other). If certain structural conditions are added to the essential qualitative criteria, then the relevant representation theorem can be proven: there is a mapping of the entities to be measured (the possessors of "lengths") to the nonnegative reals that represents the relation "is at least as long as." And, since in the case of length there is a natural zero, we have a ratio scale: the mapping is unique up to multiplication by a positive real (the only difference between any two scales of length is a mere matter of multiplication by a positive constant – e.g., a yard is 36 inches, or a meter is 100 centimeters). Note the ingredients: we have the entities to be measured, a qualitative relation on those entities, and a mapping (unique up to some transformation) from the entities to the measuring domain (the nonnegative reals in the case of length) that preserves relevant structure (so that, in the case of length, the longer the object, the greater the associated real).

In measuring utility, there is some debate concerning the entities over which preference ranges. In the case of von Neumann and Morgenstern (1953), the entities are lotteries or gambles (I follow here Resnik's accessible treatment of the von Neumann–Morgenstern theory in Resnick 1987:88–98).

Each nontrivial lottery has two prizes (which can be either "basic prizes" or other lotteries) with associated probabilities that sum to one. The theory supposes that there are a finite number (greater than one) of basic prizes on offer between some of which the agent is not indifferent (so there is a best prize (B) and a worst prize (W)). Each basic prize is considered a trivial lottery. If L_1 and L_2 are lotteries and $0 \leq a \leq 1$ then there is a lottery assigning L_1 as a prize with probability a and L_2 as a prize with probability $1 - a$ (such a lottery is written: $L(a, L_1, L_2)$, which is the same lottery as $L(1 - a, L_2, L_1)$). Nothing else is a lottery.

Quotidian decisions can be viewed as lotteries. Let:

a = probability of rain
"L_1" abbreviate "wet and with umbrella"
"L_2" abbreviate "dry and with umbrella"
"L_3" abbreviate "wet with no umbrella"
"L_4" abbreviate "dry with no umbrella"

In deciding whether to carry an umbrella, a person decides between:

$$L(a, L_1, L_2) \text{ and } L(a, L_3, L_4)$$

Von Neumann and Morgenstern lay down criteria on preference among lotteries sufficient to prove a representation theorem – that is, they prove that, provided the set of lotteries satisfies the criteria, there exists a function u on the set of lotteries into the real numbers satisfying the following properties (where R is the agent's weak preference relation – that is, $L_1 R L_2$ if and only if the agent strictly prefers L_1 over L_2 or is indifferent between them).

(1) $u(L_1) \geq u(L_2)$ if and only if L_1RL_2
(2) $u(L(a, L_1, L_2)) = au(L_1) + (1 - a)u(L_2)$
(3) Any u' satisfying (1) and (2) is a positive linear transformation of u
(That is, for any u' satisfying (1) and (2) there are reals m ($m > 0$), b such that for all L: $u'(L) = mu(L) + b$).

Statement (2) is known as the expected utility property, for a reason illustrated by the following example. Suppose you believe a certain die to be fair, then what would you expect to win, on average, over repeated trials in which you are paid $11 each time a six is rolled, and you pay $1 each time another number is rolled? Every six trials you would, on average, receive $11 once, and pay $1 five times, yielding $1 per trial. Here $a = \frac{5}{6}$, $L_1 =$ receive $11, $L_2 =$ pay $1. The dollar value of

$$L(a, L_1, L_2) = [\tfrac{1}{6} \times (\text{the dollar value of } L_1)] + [\tfrac{5}{6} \times (\text{the dollar value of } L_2)]$$
$$= [\tfrac{1}{6} \times (\$11)] + \tfrac{5}{6} \times (-\$1)] = \$1$$

Utility is introduced to replace dollar values because, even in the cases where the prizes are monetary, how the agent values a prize need not be a linear function of its dollar value. This is illustrated by the diminishing marginal utility of money: for most of us, the first million dollars would be more valuable than the second.

Statement (3) states that von Neumann and Morgenstern measure utility on an interval scale: the case is similar to that of measuring temperature in degrees Fahrenheit and Celsius.

The criteria to be satisfied are as follows.

(A) An ordering condition: R is assumed to be a weak ordering – that is, R is reflexive (for all lotteries L, LRL), transitive (for any lotteries L_1, L_2, L_3, if L_1RL_2 and $L_2R L_3$, then L_1RL_3) and connected (for any lotteries L_1, L_2, either L_1RL_2 or L_2RL_1). (Note that indifference, I, and strict preference, P, can be defined in terms of R: L_1PL_2 if and only if not $[L_2RL_1]$; L_1IL_2 if and only if L_2RL_1 and L_1RL_2).
(B) The better prizes condition.
  For any real a ($0 \leq a \leq 1$), for any lotteries L_1, L_2, L_2':

$$L_2PL_2' \text{ if and only if } L(a, L_1, L_2)PL(a, L_1, L_2').$$

Roughly, preference between two lotteries that are identical apart from a difference in one of the prizes must be due to a preference concerning that one difference.
(C) The better chances condition.
  For any reals a, b ($0 \leq a, b \leq 1$), for any lotteries L_1, L_2:

$$\text{if } L_1PL_2 \text{ then } a > b \text{ if and only if } L(a, L_1, L_2)PL(b, L_1, L_2).$$

Roughly, if one lottery prize is preferred to the other, then the lottery that gives the best chance of that prize is preferred.

(D) A continuity or Archimedean condition. Suppose there is a lottery you consider so awful (call it "H" for hell) that the following holds: there are two other lotteries L_1 and L_2 such that L_1PL_2, yet provided $0 < a$ in $L(a, H, L_1)$, you would still strictly prefer L_2 over $L(a, H, L_1)$ (i.e., $L_2PL(a, H, L_1)$) no matter how close to (but still greater than) zero a becomes. Then your preferences cannot be represented with real valued utilities.[3] Thus we need a condition to rule out such cases. It runs as follows:

$$\text{if } L_1PL_2 \text{ and } L_2PL_3, \text{ then there is a real number } a \ (0 < a < 1)$$
$$\text{such that } L(a, L_3, L_1)IL_2.$$

(E) The reduction of compound lotteries condition. What are your chances of winning L_1 in the lottery $L(a, L(b, L_1, L_2), L(c, L_1, L_2))$? There are two routes: you have a chance a of winning $L(b, L_1, L_2)$, which then presents you with a subsequent chance b of winning L_1 – total chance of winning L_1 via this route is ab; and you have a chance $1 - a$ of winning $L(c, L_1, L_2)$, which then presents you with a subsequent chance c of winning L_1, yielding a total chance via this route of $(1 - a)c$. Summing over the two routes yields an overall total chance of winning L_1 of $ab + (1 - a)c$. Your total chance of winning L_2 is thus $1 - [ab + (1 - a)c]$. In light of these calculations, it is required that:

$$L(a, L(b, L_1, L_2), L(c, L_1, L_2))IL([ab + (1 - a)c], L_1, L_2).$$

Criteria A-E are proposed as conditions on rational preference among lotteries. The next step is to show that a preference ranking that obeys these criteria can be represented by a utility function u satisfying (1), (2) and (3) above. The proof of the existence of this function begins by making $u(B) = 1$, $u(W) = 0$ and $u(L(a, B, W)) = a$; an accessible version of the proof is to be found in Resnik (1987:88–98). This representation theorem serves to justify the rationality of maximizing expected utility. If one agrees that it is rational to meet criteria A-E, then one must agree that the rational ordering of the lotteries goes by expected utility. This is not to say that rationality requires calculation; but if an agent's preferences over lotteries meet criteria A-E, then that agent ranks lotteries as if calculating expected utility (see, e.g., Luce and Raiffa 1957:31–2).

The notion of rationality invoked here is "thin," in the sense that nothing substantive is said concerning the ordering of the basic prizes: criteria A-E do not distinguish between, for example, self-interested and altruistic orderings. More is said on this below in the discussion of rational choice theory.

Even this thin notion of rationality is subject to challenge, however. Consider Broome's (1991:26) variant of an example due to Diamond (1967). You are faced with an agonizing decision – you have to distribute a kidney to one of two

patients: A and B. Whoever gets it will live; the other will die. Among the available options are giving the kidney to A or tossing a fair coin, and giving it to A on heads, B on tails. You are indifferent between the basic "prizes" [A lives, B dies] and [A dies, B lives]. Hence, by the better prizes condition, you should be indifferent between $L(\frac{1}{2}, $ [A lives, B dies], [A lives, B dies]) (which is tantamount to giving the kidney to A) and $L(\frac{1}{2}, $ [A lives, B dies], [A dies, B lives]). But you are not: you strictly prefer $L(\frac{1}{2}, $ [A lives, B dies], [A dies, B lives]) because of fairness considerations – and this seems quite rational.

One way to try to save the theory is to individuate outcomes more finely (see, e.g., Broome 1991:95–117), by maintaining that there are really three basic prizes featured in the two lotteries: [A lives, B dies], [A dies, B lives], and [A lives, B dies and you treated her unfairly]. And you prefer [A lives, B dies] over [A lives, B dies and you treated her unfairly]. Hence, by the better prizes condition and transitivity of preference you should prefer $L(\frac{1}{2}, $ [A lives, B dies], [A dies, B lives]) over $L(\frac{1}{2}, $ [A lives, B dies and you treated her unfairly], [A lives, B dies and you treated her unfairly]). But now we have the difficulty of realizing such lotteries as the following (which the theory requires to be a lottery): $L(\frac{1}{2}, $ [A lives, B dies and you treated her unfairly], [A lives, B dies]).

This difficulty will resurface in the discussion of Savage ([1954] 1972) below, under the guise of the "constant act problem." A more general shortcoming of von Neumann and Morgenstern (1953) from the Bayesian perspective (see below) is that, rather than emerging as a result of the representation theorem, numerical probabilities are present at the start. Von Neumann and Morgenstern suggest that "the simplest procedure is . . . to insist upon the . . . interpretation of probability as frequency in long runs" (1953:19).[4] (In the case of, say, tossing a coin, the idea – roughly – is that the probability of heads is the proportion of heads that appear in a long run of tosses.)

Theorists such as Ramsey (1926) and Savage (1972) (both of whom I discuss below) pursue the goal of getting measures of *both* subjective probability (or degree of belief) and utility from a purely qualitative axiomatization of preference.

It is perhaps helpful in understanding Savage's approach (1972:31) to note that von Neumann and Morgenstern's better chances condition (1953) can yield, from information about an agent's preferences, a guide to an agent's ranking of his or her degrees of belief. Why, if L_1PL_2, would a rational agent prefer

G_3: receive L_1 if event A occurs, L_2 otherwise

to

G_4: receive L_1 if event B occurs, L_2 otherwise?

Reasoning backward, as it were, along the lines of the better chances condition yields the thought that (with certain provisos) the agent must have a higher degree of belief in A than in B.

Ramsey's (1926) method for deriving degree of belief from preference begins with a similar line of reasoning. Why, if $L_1 P L_2$, would a rational agent be indifferent between G_3 and

G_5: receive L_1 if event ~A occurs, L_2 otherwise?

Surely because (with certain provisos) the agent has equal degrees of belief in A and ~A.

Before discussing (among others) Ramsey and Savage in more detail, I turn first to the distinction between decision theory and rational choice theory.

Rational Choice Theory

In Plato's *Phaedo* we find Socrates claiming that his physical constitution serves merely as the enabling condition for the cause of his sitting in prison, that cause being mental: his belief that he is doing the right thing in succumbing to the punishment that the state has seen fit to inflict upon him. Two aspects of Socrates' view are relevant here. First, he appeals to psychological states in the explanation of behavior. Second, he believes that there are reasons for preferring one end to another: namely, that some ends are better than others.

This second feature is in sharp contrast to Hume, who claims that:

> Where a passion is neither founded on false suppositions, nor chooses means insufficient for the end, the understanding can neither justify nor condemn it. 'Tis not contrary to reason to prefer the destruction of the whole world to the scratching of my finger. 'Tis not contrary to reason for me to chuse my total ruin, to prevent the least uneasiness of an Indian or person wholly unknown to me. 'Tis as little contrary to reason to prefer even my own acknowledge'd lesser good to my greater, and have a more ardent affection for the former than the latter. (Hume [1739] 1968:416)

On a Humean account rationality determines only the means, and has nothing to say concerning the relative preference ranking of any two ends. On the Socratic account, rationality determines the preference ranking of the ends also.

One take on decision theory is to view it as a way of accounting for degrees of belief (rather than merely beliefs *simpliciter*) in the psychological explanation of behavior, while at the same time furthering a Humean account of preference. On one common approach, briefly canvassed above, a key goal is to lay down minimal qualitative conditions on the rationality of preference that are sufficient to prove a representation theorem to the effect that, if an agent obeys the qualitative preference axioms, then her or his degrees of belief conform to Kolmogorov's (1933) axioms, and her or his preferences are ranked in accord with their expected utilities.

Suppose one agrees with Hume that, given any two outcomes, rationality has nothing to say vis-à-vis their relative ranking in terms of preference. Does rationality then have anything to say about preference among outcomes? The standard decision-theoretic view is that rational preference among outcomes must be at least connected (given any pair of outcomes A and B, either A is preferred to B, or B to A, or they are ranked together) and transitive (given any trio of outcomes A, B, C, if A is preferred to B and B to C, then A is preferred to C). (It is a matter of controversy whether a Humean can take on such constraints on preference while remaining a thoroughgoing Humean. (See, e.g., Broome 1993, Hampton 1998, Rawling 1997.)

However, decision theory is not inconsistent with an anti-Humean insistence that reason does dictate a preference for the scratching of one's finger over the destruction of the world. Decision theorists simply do not speak to such issues because they lie outside the scope of their project. This is in marked contrast to rational choice theory, at least as it is used by economists working in what some perceive to be the tradition of Adam Smith doctrine (see Walsh 1996:114–19 for a challenge to the view that Smith advocated an unalloyed assumption of self-interest). Rational choice theorists typically insist that rational agents pursue their self-interest above the interests of others.

Smith's notion of the "invisible hand" can be seen as a response to Hobbes, who argued (roughly speaking) in the *Leviathan* (1651) that it is rational to live under the subjugation of a sovereign. This is the only way to free ourselves from the "state of nature," in which our relentless pursuit of self-interest leaves us communally worse off. In game-theoretic terms, the state of nature confronts us with a prisoner's dilemma. The sovereign allows the possibility of cooperation via the enforcement of contracts.

Strictly speaking the prisoner's dilemma is the following two-person "game" due to A. W. Tucker (see Jeffrey [1965] 1983:ch. 1). You and your confederate have been told the following by the arresting officer before being placed in separate cells: If neither of you confesses, we have enough evidence to put you both away for a year. If both of you confess, you will each be imprisoned for five years. However, if one of you confesses, and the other does not, the confessor will be released, but the other will be sent down for ten years.

You reason as follows. If my confederate confesses, I should (five years in prison is better than ten). And if he doesn't confess, I should (release is better than one year in prison). I don't know which he is going to do, and I have no way of influencing him, or communicating with him to make a deal. Hence I should confess. Your confederate reasons similarly. Thus you both confess, and each of you goes down for five years. But had neither of you confessed you would both have got away with only one year.

The problem is quite general. Suppose Anne has options A and B, and Cheryl has options C and D, resulting in the table of outcomes shown in table 5.1, where Anne has the preference ranking (where first is best): β, δ, α, γ: and Cheryl has the ranking: γ, δ, α, β.

Table 5.1

	C	D
A	α	β
B	γ	δ

Assuming that Anne and Cheryl cannot trust one another to keep any agreements they might make, Anne will choose A since it *dominates* B (that is, she prefers the outcome of A regardless of whether Cheryl picks C or D: cf. von Neumann and Morgenstern's better prizes condition), and Cheryl will choose C since it dominates D. Thus they will converge on which is the third choice for each. Yet they could have arrived at δ (the second choice for each) had they been able to make a binding agreement that Anne opt for B and Cheryl for D. Hobbes can be seen as arguing that the rational thing to do is to adopt a mechanism for enforcing agreements that will enable us to converge on δ.

Adam Smith (in the *Wealth of Nations* 1776) is interpreted by some as arguing, contrary to Hobbes, that there are conditions not involving authoritarian government under which the pursuit of self-interest will promote the general good:

> As originally conceived by Smith, [rational actor] theory provided a powerful and creative mechanism whereby the pursuit of individual self-interest would lead to collective welfare. The genius of Smith's invention – the market mechanism, regulated by an invisible hand – solved a problem which had troubled philosophers since Hobbes made his famous argument that there was one basic human nature and that this nature was self-centered: How can a society of selfish citizens produce collective welfare without authoritarian government? (Monroe 1991:1)

Smith's notion, rudimentarily put, is that capitalists will pursue their self-interest by reinvesting profits into their businesses to enlarge them, which will result in economic growth that will also benefit society as a whole.

Even if this latter is true, however, one might challenge the extent to which capitalism has vitiated Hobbes's argument for the necessity of authoritarian government. Clearly, enforceable and enforced contracts are crucial for the functioning of a capitalist society, thus there must be an enforcing authority. Perhaps the neo-Smithian vision is that, given the existence of such an authority (presumably democratic), we can all do good by doing well (to invert Tom Lehrer's lyric). In support of this vision, much has been done in the attempt to show that self-interest has the consequence of societal benefit. However, the idea that reason dictates a preference for one's own good over that of others is anti-Humean, and is no part of decision theory (although not inconsistent with it). Decision theory prescribes a certain coherence among preferences, but it does not prescribe ends.

It might be complained that there is a confusion of the prescriptive with the descriptive in the foregoing. Is rational choice theory descriptive or prescriptive? If the former, then it claims that, regardless of how people should act, they actually do act out of self-interest at least most of the time. However, this claim too is outside the purview of decision theory: insofar as decision theory has descriptive pretensions, these are limited to claims about such matters as the actual transitivity of people's preferences. If rational choice theory is prescriptive, then it outruns the prescriptions of decision theory in maintaining that people should act out of (and only out of?) self-interest.

Prescription and Description

This discussion points to an interesting question: to what extent, in decision theory, can the prescriptive be distinguished from the descriptive? Davidson "doubt[s] that there is an interesting way of understanding the purported distinction" (1985b:89).

Rationality is an evaluative concept: to claim that a rational agent's preferences are transitive, or that his or her degrees of belief are coherent, for example, is to claim that an actual agent's preferences *should be* transitive, or that his or her degrees of belief *should be* coherent. The (broadly speaking) Humean line on decision theory is that it gives part of an instrumental account of rationality: whatever your ends, they are best achieved by conforming your preferences to the dictates of the theory. Decision theory does presuppose that you have ends, and, of course, the fact that the ends are yours entails that they are, in a certain sense, of value *to you*. But this is distinct from maintaining that your ends are self-interested: they might be altruistic (for you to value your being altruistic is distinct from your altruism being in your self-interest).

More radical is the claim that decision theory is both prescriptive and descriptive: that agents conform to the theory's prescriptions. This is a claim that has been championed by Davidson (see, for example, 1985b and 1990:section III; see also 1980, 1984, 1985a, 1989). Consider first the case of belief *simpliciter*[5] and "standard folk-psychological explanation." On the one hand we have prescriptive views about how people should act and what they should prefer given what they believe and desire. Indeed, we have prescriptive views, both hypothetical and categorical, about what people should believe and desire (hypothetical views concern, for example, what people should believe and desire given their other beliefs and desires; categorical views concern what we should believe and desire *tout court*).

On the other hand we explain (a descriptive enterprise) people's actions and preferences by adverting to their beliefs and desires. But this kind of explanation involves rationalization (in a nonpejorative sense): to explain an action is to place it in the context of an agent's beliefs and desires in such a way that we make sense

of that agent and her or his action – we make the agent out, as far as possible, to be rational. To do any less is to fail to treat the person as an agent. But to make an agent out to be rational is to see the agent as conforming to our prescriptive norms. Amanda's belief that David is preparing beef for supper, in combination with her preference for red wine over white with beef, explains her purchase of the red because it is what she *should* do, given her belief and her preference.

To explain an action or an attitude we must describe it as conforming to prescriptive norms. (This is not to say, of course, that every action and attitude is endorsed; but even Hitler's anti-Semitism, if it is psychologically explicable, will be explained as fitting some norms of rationality, however minimal.) Where we fail in this endeavor, and make someone out, to a certain extent, to be internally inconsistent or irrational, to that extent we fail adequately to explain. The point is Davidson's, and is a development of Quine's "principle of charity"[6]: ". . . [in interpreting linguistic behavior] one's interlocutor's silliness, beyond a certain point, is less likely than bad translation – or, in the domestic case, linguistic divergence" (1960:59).

In interpreting intentional behavior in general, flagrant irrationality is less likely than a flawed attribution of propositional attitudes. To interpret someone is to attribute mental states to them. And, Davidson argues, any mental state is partly constituted by its rational connections to other mental states of the agent. Thus my belief that it's lunchtime is partly constituted by the fact that it combines with my intention to meet a colleague for lunch to give rise to the appropriate action on my part. And the belief–intention pair both rationalizes and explains my action.

On Davidson's account, then, rationality plays a constitutive role in psychology: nothing is a propositional attitude or an action unless it is part of a rational pattern. Not only does the explanation of an action involve appeal to propositional attitudes that rationalize it, but rationalizing attitudes must be present in order for the behavior to be an action. And, furthermore, the attitudes themselves rely for their existence on their place in a larger rational pattern of attitudes.[7]

In the case of the full range of degrees of belief, Davidson suggests that the prescriptive norms concerning choice and preference are constituted by (our best) decision theory. And, by analogy with the case of belief *simpliciter*, we appeal (implicitly) to this theory in the attribution of degrees of belief and preferences, in the interpretation of behaviors as actions, and in the explanation of those actions.

Decision theory, on Davidson's view, is not so much an empirical theory of brain and behavior as it is part of our constitutive theory of mind and action – that is, to conform (roughly) to the canons of our best decision theory is part of what it is to have a mind and to act. And it is not as though constitutive theories are absent in other realms. While it could have turned out, and might yet turn out, that $E \neq mc^2$, it could not be, as Davidson points out, that the relation *longer than* is intransitive (given various provisos) (1980:220–21). (Or, at least, if transitivity fails, "we cannot easily make sense of the concept of length" (Davidson 1980:220).) Indeed, Davidson compares the transitivity of preference to the transitivity of length:

If length is not transitive, what does it mean to use a number to measure a length at all? We could find or invent an answer, but unless or until we do we must strive to interpret "longer than" so that it comes out transitive. Similarly for "preferred to." (Davidson 1980:273)

As things stand, then, the attribution of intransitive preferences is one of the last resorts of the skilled interpreter.

On the one hand, decision theory is explicitly prescriptive: much debate over the axioms of preference concerns the extent to which we should adhere to them. On the other hand, decision theory has a descriptive component: it is recognizably addressing preference and degree of belief. Can someone have "degrees of belief" wildly at odds with Kolmogorov's axioms? Is someone who feels absolutely no pressure toward transitivity really expressing preferences? Nevertheless, Davidson must respond to the myriad well-known challenges that have been mounted against both the prescriptive and descriptive adequacy of all extant decision theories. Concerning challenges to prescriptive adequacy, he might maintain that we have not yet formulated our best theory (he currently favors Jeffrey [1965] 1983); and he might also avail himself of some of the responses discussed below.

On challenges to descriptive adequacy, the first thing to note is that conforming to decision theory does not require calculation on the part of agents. The decision-theoretic claim need only be that the preferences of an agent conform (roughly) to a set of qualitative preference axioms[8] (of our best theory of decision). It then follows (as we saw analogously in the case of von Neumann and Morgenstern), by virtue of the representation theorem (for the relevant theory), that the agent acts (roughly) as if maximizing utility (that is, as if he or she assigns approximate numbers as utilities and degrees of belief, and calculates his or her optimal choice).

I do not, however, want to overemphasize the role of representation theorems. In explaining choices we often make direct appeal to degrees of belief, at least qualitatively. If Amanda returns home with white wine, we might explain this by saying that she thought fish more likely, and prefers white wine over red, with fish. In more complicated situations, we have to furnish more complex explanations (we might, of course, have to do this in Amanda's case) – and invoke, for instance, the "balancing" of likelihood against preference over outcomes.

One might view representation theorems as showing merely that the norm of coherence among degrees of belief, and the norms of preference (such as transitivity) are mutually reinforcing; and as pointing to methods for uncovering information about our degrees of belief from our preferences, and vice versa. But if this is all that one requires of representation theorems, it is but a short step to Joyce (1999), who (building on a suggestion of Jeffrey 1992:229–30 and the work of, among others, Bolker 1966, 1967, de Finetti 1937, Jeffrey 1983, Savage [1954] 1972 and Villegas 1964) derives utilities and degrees of belief from qualitative restrictions on: the relation "is not more probable than"; the relation "is not preferred to"; and the relation between these relations.

When it comes to approximate numbers, Davidson does not want to maintain that attribution of degrees of belief requires perfect coherence (where one's degrees of belief are said to be coherent if and only if they conform to Kolmogorov's 1933 standard probability axioms and definition of conditional probability: see note 2), but a degree of coherence is required lest we be tracking the wrong feature of the agent. If what we thought were someone's degrees of belief showed no tendency to cohere, we would conclude that we were misinterpreting.

Here is a trite example to illustrate Davidson's thinking as it applies to degree of belief: the case of Monty Hall's three doors. Anne knows the following:

There is a prize behind one of three doors. She will get the prize if and only if she has chosen this door at the end of following procedure. She will first choose a door. Monty will then open one of the other two doors, but not the one hiding the prize. She can then stick with her original choice or opt for the other unopened door.

What should she do? Well, she stands the best chance of winning if she switches. But many fail to see this. My purpose, however, here is not to convince the reader that Anne should switch. It is, rather, to show that it is easy to slip into incoherent degrees of belief about the case, and how this illustrates the fact that Davidson need not insist on perfect coherence in order to interpret someone as having degrees of belief.

Suppose Anne has initially chosen door A. Let:

> A abbreviate the proposition that the prize is behind A;
> C abbreviate the proposition that the prize is behind C;
> M abbreviate the proposition that Monty opens B.

Some may hold degrees of belief as follows:

$$p(A) = \tfrac{1}{3} \text{ and } p(A \text{ given } M) = p(C \text{ given } M) = \tfrac{1}{2}.$$

It is the latter that presumably underlies the thought that Anne has no reason to switch to C from A should Monty open B.

However, these same people might well also have the following degrees of belief: $p(M) = \tfrac{1}{2}$ (Monty will not open A because Anne chose it; B and C are equally likely candidates for opening in the absence of further information) and $p(M \text{ given } A) = \tfrac{1}{2}$ (if A conceals the prize, then there is no reason to suppose Monty more or less likely to open C than B). But from $p(A) = \tfrac{1}{3}$, $p(M) = \tfrac{1}{2}$ and $p(M \text{ given } A) = \tfrac{1}{2}$ it follows that $p(A \text{ given } M) = \tfrac{1}{3}$, not $\tfrac{1}{2}$ (use (4) from note 2). Therefore anyone who holds $p(A) = \tfrac{1}{3}$, $p(M) = (\tfrac{1}{2})$, $p(M \text{ given } A) = \tfrac{1}{2}$ and $p(A \text{ given } M) = \tfrac{1}{2}$ violates coherence.

Such a violation, however, does not force a verdict of misinterpretation: we can attribute incoherent degrees of belief. The agent has simply made a mistake, and

Davidson need not deny this. He would, however, correctly insist that the attribution of such error or irrationality is impossible unless the agent has sufficient probabilistic coherence to warrant the attribution of the degrees of belief in the first place – in particular we need to be able here to attribute the following:

$$p(A) = \tfrac{1}{3}, \; p(M) = \tfrac{1}{2}, \; p(M \text{ given } A) = \tfrac{1}{2} \text{ and } p(A \text{ given } M) = \tfrac{1}{2}.$$

As he puts it:

> Crediting people with a large degree of consistency . . . is unavoidable if we are to be in a position to accuse them meaningfully of error and some degree of irrationality. Global confusion . . . is unthinkable, not because imagination boggles, but because too much confusion leaves nothing to be confused about . . . (Davidson 1980:221)

It is not that there is no room for irrationality; but it cannot be too pervasive.

How pervasive is too pervasive? Davidson does not owe us a precise answer: I am bald and Paul is not, yet we lack precision concerning the boundaries of the bald. But there is good evidence that people are prone to make *systematic* errors when making judgments under uncertainty (see Kahneman et al. 1982) – does this not challenge the descriptive adequacy of standard probability theory? The same point applies: what makes, say, purported probability estimates judgments of *probability*?

Canons of rationality place restrictions on what constitutes interpretation. But rather than make interpretation a more difficult enterprise, these restrictions are what make it possible (see, e.g., Davidson 1990:325). For interaction to be mutual interpretation, the parties must make assumptions about each other that could not turn out to be false lest their enterprise fail to be interpretation at all, so that no interpretation is built entirely from scratch. As Davidson acknowledges, another of his progenitors, in addition to Quine, is Ramsey. Ramsey's theory predates that of von Neumann and Morgenstern (an accessible treatment of this pair of theories is given in Jeffrey 1983:ch. 3). It was a signal achievement, not least because it was (as far as I know) the first theory to yield both utilities and degrees of belief from qualitative restrictions on preference. It is also important as a model of Davidsonian interpretation, and how it is that the restrictions of rationality make interpretation possible.[9]

Ramsey's Theory

In Ramsey (1926), one of his main concerns is the notion of degree of belief, and how to measure it on a numerical scale. He sees two possible ways to begin. We might "suppose that the degree of a belief is something perceptible by its

owner; for instance that beliefs differ in the intensity of a feeling by which they are accompanied, which might be called a belief-feeling or feeling of conviction, and that by the degree of a belief we mean the intensity of this feeling" (Ramsey [1926] 1931:169). Ramsey rejects this account: "it seems to [him] observably false, for the beliefs which we hold most strongly are often accompanied by practically no feeling at all; no one feels strongly about things he takes for granted" ([1926] 1931:169). His favored account is that "the degree of a belief is a causal property of it, which we can express vaguely as the extent to which we are prepared to act on it" ([1926] 1931:169). This he cashes out in terms of preferences among gambles. There are, however, two determinants of such preferences: in addition to the degrees of belief in the propositions on which the outcomes of the gambles hinge, the degree to which those outcomes are desired is clearly also crucial. Here Ramsey invokes his ingenious technique for getting both utilities and degrees of belief from information merely about preferences among gambles.

Ramsey's method, roughly speaking, begins as follows (see Ramsey [1926] 1931:177 ff). Call a possible event, E, "ethically neutral" for an agent if, for all outcomes (for simplicity's sake, I assume all outcomes to be compatible with both E and ~E – cf. Ramsey's footnote on p.178), the agent is indifferent between [α & E] and [α & ~E]. We must find an ethically neutral possible event E and two outcomes, α and β, such that our agent, Anne, is indifferent between the two gambles:

$$G \quad \alpha \text{ if E}; \beta \text{ if } \sim\text{E}$$
$$H \quad \beta \text{ if E}; \alpha \text{ if } \sim\text{E}$$

yet strictly prefers α to β. Now we suppose that if any other outcomes are substituted uniformly for α and β in G and H, Anne is indifferent between the resulting gambles; and we define (p.177) Anne's degree of belief (p) in E to be one half. This comports with the notion of expected utility. We have:

$$u(G) = u(\alpha)p(E) + u(\beta p(\sim E))$$
$$u(H) = u(\beta)p(E) + u(\alpha p(\sim E))$$

Since $p(\sim E) = 1 - p(E)$, and $u(\alpha) \neq u(\beta)$, equating $u(H)$ and $u(G)$ entails (by simple algebra) that $p(E) = \frac{1}{2}$.

From here, Ramsey goes on to show (this is his representation theorem) how to measure utilities, and degrees of belief generally – on the proviso that the agent's preferences satisfy certain axioms. And Ramsey proves that if an agent satisfies these axioms, then, given any proposition p, we can attribute to that agent a degree of belief in p (and measure it); and his or her degrees of belief across the multiplicity of propositions are coherent. (For some objections to Ramsey's approach, see Jeffrey 1983:ch. 10.)

Ramsey sees degrees of belief as necessarily related to preference in a certain way: note that he *defines* degree of belief one half in an ethically neutral proposition in terms of preferential indifference. A restriction on the relation between degree of belief and preference enables measurement of the former on the basis of the latter.

However, is Ramsey claiming that to have certain degrees of belief just is to have certain preferences? Recall his idea that "the degree of a belief is a causal property of it, which we can express vaguely as the extent to which we are prepared to act on it" ([1926] 1931:169). Perhaps Ramsey sees both preferences and degrees of belief as mere dispositions to behave. But his adversion to action implies a role for reason. Plausibly, on Ramsey's view, degrees of belief are independently pre-existing psychological states that constitute a key component in our reasons for our preferences; and rather than constituting our degrees of belief, preferences result in part from them. Ramsey and Davidson (see Davidson 1990:317) then agree that preferences provide evidence for degrees of belief (Anne's indifference above is very strong evidence for $p(E) = ½$); and degree of belief plays a role in the explanation of preference.

Decision theories, such as Ramsey's, that derive coherent degrees of belief from rationality conditions on preference provide one justification for the coherence requirement: if your preferences conform to the rationality conditions, then coherent degrees of belief are attributable to you. Another argument for coherence, due independently to Ramsey (1926) and de Finetti (1937), is the Dutch book argument.

Dutch Books and the Epistemic Objection[10]

If we assume (as does de Finetti 1974:ch. 3; see also Seidenfeld and Schervish 1983:408–9) value to be a linear function of dollars for small amounts, then we can equate $p(A)$ (your degree of belief in the proposition A) to your dollar value for the guarantee: [receive \$1 if A, \$0 if not]. Note that, in von Neumann and Morgenstern's terms, if $u(L_1) = 1$ and $u(L_2) = 0$, then

$$u(L(p(A), L_1, L_2)) = p(A)u(L_1) + (1 - p(A))u(L_2) = p(A).$$

You are said to be susceptible to a "Dutch book" if you are willing to take on a set of gambles that guarantees you will lose come what may. The Dutch book argument, due independently to Ramsey (1926) and de Finetti (1937), purports to show the irrationality of incoherent degrees of belief (i.e., degrees of belief that violate Kolmogorov's 1933 axioms: see note 2) by showing that your degrees of belief are incoherent if and only if you are vulnerable to a Dutch book.

I begin with an example. Let $p(A) = x$; $p(A\&B) = y$; $p(A\&\sim B) = z$. You should be indifferent between

$x and the guarantee of $1 if A, $0 if not; and
$y and the guarantee of $1 if A&B, $0 if not; and
$z and the guarantee of $1 if A&~B, $0 if not.

Since A is equivalent to [(A&~B) or (A&B)], an obvious consequence of Kolmogorov's axioms is that:

$$p(A) = p(A\&B) + p(A\&\sim B)$$

But suppose your degrees of belief are such that:

$$p(A) > p(A\&B) + p(A\&\sim B) \text{ (i.e., } x > y + z)$$

Then I, as Dutch bookie, will pay you $(y + z)$ in exchange for your guarantees to pay me $1 if A&B and $1 if A&~B; and get you to pay me x in exchange for my guarantee to pay you $1 if A. However the world turns out, I will net $[x - (y + z)]$. The example also illustrates Ramsey's ([1926] 1931:182) remark that "if anyone's mental condition violated [Kolmogorov's axioms], his choice would depend on the precise form in which the options were offered him, which would be absurd": you would choose the guarantee of $1 if A over the guarantee of [$1 if A&B and $1 if A&~B], even though the two guarantees are identical.

More generally, as I show in the appendix:

(T) Your degrees of belief violate Kolmogorov's axioms if and only if there is a set of propositions such that the sum (**s**) of your degrees of belief across its members is either greater than the greatest (**g**), or less than the least (**l**), logically possible number of truths among them.

In either case, the Dutch book is clear. If **s** > **g**, the Dutch bookie gets you to pay $**s** in exchange for her or his guarantee to pay you at most $**g**; if **s** < **l**, the Dutch bookie pays you $**s** in exchange for your guarantee to pay him or her at least $**l**.

There are many objections to Dutch book arguments. For example, Schick complains that they presuppose "value additivity" (1986:113). You might be indifferent between y and the second guarantee in the example above, and between z and the third, and yet be quite rational in declining to accept $(y + z)$ in exchange for providing the two guarantees conjointly, because, say, you are so poor that money has diminishing marginal utility above either y or z.[11]

Another objection to the Dutch book justification of coherence is what might be dubbed the "epistemic objection." This objection, which applies to the decision theoretic approach to degree of belief more generally, questions the probative force of a practical justification of a theoretical matter. The Dutch book argument appeals to the practical issue of financial loss in justifying coherence. But, the epistemic objectors claim, degrees of belief belong in the realm of theoretical

reason, hence we should have a theoretical justification for coherence (see Joyce 1998, Kaplan 1996).

For example, we might try the thought that degree of belief is belief about objective chance,[12] and we know that objective chances cohere. This fails, however, for a variety of reasons. First, as Jeffrey (1992:46) notes, you might think the objective chance of A is either 10 percent or 70 percent with odds of 5:1 on 10 percent, hence your degree of belief in A is 0.2 (= [⅚ × .1] + [⅙ × 0.7]) even though you are confident that 0.2 is not the objective chance of A.

Second, pure Bayesians, such as de Finetti, disavow objective chances. (One might, however, adopt a "mixed Bayesianism" (see Jeffrey 1992:197), according to which there are some objective probabilities (chances).) Bayesianism, in this context, is an interpretation of probability[13] – or, more correctly perhaps, a family of interpretations: according to Edwards et al. "there must be at least as many Bayesian positions as there are Bayesians" (1963:195). One key feature of Bayesianism is the idea that degree of belief is a property of the believer rather than of the object of belief: if your degree of belief that a tossed coin will come up heads is one half, this is a property of you rather than the coin. On pure Bayesianism, probability assignments held by an agent are criticizable if and only if they violate Kolmogorov's axioms: no single subjective probability is criticizable in isolation (provided, of course, it falls within the range between and including the probabilities of necessary truths and falsehoods).

Another feature of Bayesianism is the claim that "all uncertainties are measured by probabilities" (Edwards et al. 1963:196); note also Lindley (1983): "Bayesian statistics is based on one, simple idea: *the only satisfactory description of uncertainty is by means of probability.*" Thus, for the Bayesian, whenever you are uncertain about a proposition you have a degree of belief in it. But what is degree of belief? The following goes back at least to Bayes (1763) (see Jeffrey 1983:72): $p(A)$ equals the value to you of the guarantee of a unit of value if A is true. At the outset of this section we assumed value to be a linear function of dollars for small amounts, and equated $p(A)$ to your dollar value for the guarantee: [receive $1 if A, $0 if not]. This *defines* degree of belief as a practical notion. Ramsey (1926 [1931]:169) concurs that degree of belief is practical: "the degree of a belief is a causal property of it, which we can express vaguely as the extent to which we are prepared to act on it." But if degree of belief is a practical matter, then perhaps a practical justification of coherence is to be expected.

In the case of belief, we have a theoretical constitutive aim, namely truth (i.e., it is part of what it is to be a belief that it aims at the truth). This aim yields a theoretical justification of consistency: inconsistent beliefs cannot all be true together. Furthermore, however, since intentions also constitutively aim at the truth, we have a theoretical justification of intention consistency (inconsistent intentions cannot succeed together), and intention is certainly a practical notion. One of the standard ways of distinguishing between belief and intention (following Anscombe [1957] 1963) is to point out that although both aim at the truth of their propositional content, they do so with different directions of fit: belief by

conforming to the world, intention by reforming the world. If beliefs are inconsistent they cannot all fit the world; if intentions are inconsistent the world cannot be reformed to fit all of them.

What, for the Bayesian notion of degree of belief, is analogous to the theoretical constitutive aim of truth? Joyce (1998) argues for what he dubs "gradational accuracy, " but space precludes discussion of this question here. I shall instead attempt to outline a simple theoretical case for coherence based on the idea that consistency is the limiting case of coherence. Jeffrey (1986:52) sees Kolmogorov's axioms as, ultimately, "as obvious as the laws of logic." In a sense, what I shall now do is attempt to bolster this claim.

De Finetti draws a crucial distinction between "prevision" and prediction: given one's *prevision* that the average household has, say, 2.1 children, one's best *prediction* concerning the progeny in a random household is 2 (de Finetti 1974:98). Jeffrey (1986) follows de Finetti in drawing this distinction, but uses the terms "estimate" and "guess, " respectively, for prevision and prediction. The relation between full belief and degree of belief is controversial (see note 5). But we might view your having full beliefs in the truth or falsity of all the elements of some set of propositions as entailing that you have (whole number) predictions concerning the number of truths in the set and every subset of it. In the case of inconsistent beliefs, the agent has predicted, of the set of propositional contents of those beliefs, that it contains more truths than the greatest logically possible number. More generally, consistency requires exactly that one's prediction of the number of truths in any set of propositions be at most the greatest number logically possible and at least the least number logically possible.[14] A reasonable condition on one's *degrees* of belief, then, is that one's *prevision* (or estimate) of the number of truths in any set of propositions be at most the greatest number logically possible and at least the least number logically possible. Consistency is then simply this latter condition applied to whole number previsions. And, provided your previsions admit a degree of belief function, this latter condition, by (T) (see p.00 above), is equivalent to coherence:

Suppose your estimates of numbers of truths in sets of propositions are such that there exists a function, p, which assigns to each proposition under consideration a number in such a way that your estimate of the number of truths in any set under consideration $\{A_1, \ldots, A_n\}$ is the sum $p(A_1) + \ldots + p(A_n)$[15] (Call a function p with this property a "degree of belief function"; p need not be unique). Where p is a degree of belief function, we can now, with only a minor abuse of notation, let, for any set $\{A_1, \ldots, A_n\}$, $p(\{A_1, \ldots, A_n\}) = p(A_1) + \ldots + p(A_n)$. And we have the following restatement of (T):

A degree of belief function p violates Kolmogorov's axioms (that is, fails to cohere) if and only if there is a set of propositions $\{A_1, \ldots, A_n\}$ such that $p(\{A_1, \ldots, A_n\})$ is either greater than the greatest, or less than the least, logically possible number of truths in $\{A_1, \ldots, A_n\}$.

Even if, however, we succeed in justifying coherence satisfactorily, a related difficulty (discussed in the previous section) still lurks. Degree of belief remains a pragmatic notion, and one might worry that this vitiates one seemingly plausible interpretation of decision theories that derive numerical degrees of belief from qualitative restrictions on preference alone. One might regard such theories as showing how preference and degree of belief are related – how degrees of belief rationally combine with certain preferences to yield others, and how we might gain insight into agents' degrees of belief from their preferences (the route to this insight being provided by the derivation of degrees of belief from preference). However, if degree of belief is itself practical notion, there is the lingering doubt that agents might have to see what they prefer in order to see what their degrees of belief are. (And perhaps the same applies to belief *tout court*: to believe something is to be prepared to act on it under the appropriate circumstances.) I shall not further address this issue here, however, but turn now to another case of deriving degrees of belief from preference: Savage's decision theory, regarded by many, including Fishburn (1981:194), "as one of the best."

Savage's Theory

Savage's axiomatization resembles von Neumann and Morgenstern's approach in that he conceives of acts as similar to lotteries assigning consequences to possible states of the world. He supposes there to be a set of consequences and a mutually exclusive and exhaustive set of states, with acts as functions (1972:14) on the set of states (S) (as a consequence of Savage's axioms, S is uncountable (Fishburn 1981:161); any subset of S is an event) into the set of consequences (F) (there may be as few as two consequences (Fishburn 1981:161)). The set of acts is "a large subset of" the set of functions on S into F (Fishburn 1981:160, 162). Savage's axiom P1 (Savage 1972:18) asserts that "the relation [is not preferred to] is a [weak] ordering among acts" (cf. von Neumann and Morgenstern's criterion (A)). Consequences get ranked in the weak preference ordering via the device of a "constant" act – that is, an act "whose consequences are independent of the state of the world" (Savage 1972:25).

Von Neumann and Morgenstern's better prizes condition finds expression (roughly speaking) as Savage's theorem 3 (1972:26), also known as the principle of dominance (PDOM):

Let $\{B_i\}$ be a partition of event B (a partition of an event is its division into finitely many mutually exclusive events). Let $\mathbf{f}(s)$ ($\mathbf{g}(s)$) be the consequence of act $\mathbf{f}(\mathbf{g})$ in state s. Suppose that for any i, for any $s, t \in B_i$, $\mathbf{f}(s) = \mathbf{f}(t) = f_i$, $\mathbf{g}(s) = \mathbf{g}(t) = g_i$. Then: If, for all i, $g_i R f_i$, then $\mathbf{g}R\mathbf{f}$ given that B obtains. If, in addition, $g_j P f_j$ for some j such that B_j is not null, then $\mathbf{g}P\mathbf{f}$ given B.

(B is a null event if and only if, for all acts $\mathbf{f}$ and $\mathbf{g}$, $\mathbf{g}R\mathbf{f}$ given that B obtains, so that the agent considers B to be "virtually impossible." [Savage 1972:24])

We saw an example of dominance reasoning in discussion of the prisoner's dilemma above. Another example is given by Savage (1972:21): if you consider that the result of the next presidential election is relevant to the issue of whether you ought to purchase a particular piece of property, but believe that either the Republican or Democratic candidate will win, and conclude that you should buy the property in either case, then you should buy the property. We saw a potential counterexample to dominance reasoning as it applies to indifference (if, for each event E, you are indifferent between the consequences of two acts under E, then you should be indifferent between the two acts) when discussing von Neumann and Morgenstern: Broome's (1991:26) variant of Diamond's (1967) kidney distribution case.

One of the postulates on which PDOM rests in Savage's system is his "sure-thing principle" (STP):

> >If acts **f**, **g**, and **f′**, **g′** are such that:
> 1. for all $s \notin B$, $\mathbf{f}(s) = \mathbf{g}(s)$, $\mathbf{f'}(s) = \mathbf{g'}(s)$,
> 2. for all $s \in B$, $\mathbf{f}(s) = \mathbf{f'}(s)$, $\mathbf{g}(s) = \mathbf{g'}(s)$,
> 3. **gRf**
> then **g′Rf′** (1972:23, P2)

To give necessary and sufficient conditions in terms of preference for one event to be "not more probable than" another (Savage 1972:31) requires a postulate (1972:31, P4) to ensure that the latter relation is well-defined. Define $\mathbf{f}_A$ as the act that yields consequence f in case A, consequence f′ in case not A; $\mathbf{f}_B$ as the act that yields f in case B, f′ in case not B; $\mathbf{g}_A$ as the act that yields g in case A, **g′** in case not A, etc. The postulate requires that:

$$\text{if fP f′, gPg′, } \mathbf{f}_B R \mathbf{f}_A, \text{ then } \mathbf{g}_B R \mathbf{g}_A.$$

This renders kosher the following:

A is not more probable than (≤) B if and only if: if fPf′ then $\mathbf{f}_B R \mathbf{f}_A$.

To ensure that the relation ≤ is nontrivial, Savage adds a postulate (1972:31, P5) to the effect that there are consequences f, f′ such that fPf′. He then shows that the relation ≤ is a "qualitative probability" (1972:32; see also de Finetti 1931, 1937; Koopman 1940):

(1) ≤ is transitive and connected;
(2) given any events B, C, D: if D is incompatible with B and with C then

$$B \leq C \text{ if and only if } [B \text{ or } D] \leq [C \text{ or } D]$$

(3) an impossible event is not more probable than any event, and the certain event is more probable than an impossible event.

A further condition must be added, however, to force a unique assignment of probabilities to events. To this end (see Villegas 1964:1787) de Finetti and Koopman postulate arbitrarily fine partitions of the certain event into equally probable events. Savage does not make direct appeal to equiprobability (1972:33); he adds instead another axiom of preference (1972:39, P6), which states that no consequence f is so good or so bad that we cannot find a partition fine enough to nullify its ability to affect strict preference – that is, suppose **h**P**g**, then there is a partition of the certain event such that: if **h** is modified to yield f at every state in one element of the partition, [modified **h**]P**g**; and if **g** is modified to yield f at every state in one element of the partition, **h**P[modified **g**] (cf. von Neumann and Morgenstern's criterion (D)).

I have not discussed all seven of Savage's qualitative preference axioms, but I hope to have given the general flavor of his approach. His approach to utility is essentially that of von Neumann and Morgenstern (see, e.g., Savage 1972:75). But unlike von Neumann and Morgenstern's representation theorem, which takes degrees of belief as given, Savage's representation theorem encompasses both degrees of belief and utilities: he proves (roughly speaking) that if agents meets his qualitative conditions on preference then they have degrees of belief that obey Kolmogorov's axioms, and utilities (u is now a function on the set of acts and consequences) that meet the following analogue of von Neumann and Morgenstern's properties (1), (2) and (3), where p is a derived coherent and unique degree of belief function:

(1) $u(\mathbf{f})\ u(\mathbf{g})$ if and only if $\mathbf{f}R\mathbf{g}$

(2) $u(\mathbf{f}) = \Sigma_i p(E_i)u(f_i)$
     (f_i is the consequence of $\mathbf{f}$ in event E_i; $\Sigma_i p(E_i)u(f_i) = p(E_1)u(f_1) + \ldots + p(E_n)u(f_n)$, where n is the number of events in the partition $\{E_i\}$[16])

(3) Any u' satisfying (1) and (2) is a positive linear transformation of u
     (i.e., for any u' satisfying (1) and (2) there are reals m ($m > 0$), b such that for all acts $\mathbf{f}$ and consequences f:

$$u'(\mathbf{f}) = mu(\mathbf{f}) + b \text{ and } u'(f) = mu(f) + b).$$

I turn now to a catalogue of issues and difficulties concerning Savage's theory.

What is Preference and Why is it More Basic than Qualitative Probability?

Savage axiomatizes preference and then derives qualitative probability (before moving on to quantitative probability), but why not axiomatize qualitative probability directly? Savage's response, in part, is to argue that assessing qualitative

probability through direct interrogation is too far removed from "the [extraverbal] behavior of a person in the face of uncertainty" (1972:27). Assessing preference, on the other hand, is closer to the behavioral mark.

Savage suggests that: "Of two acts **f** and **g**, it is possible that the person *prefers* **f** to **g**. Loosely speaking, this means that, if he were required to decide between **f** and **g**, no other acts being available, he would decide on **f**" (1972:17). Preference can be determined either by offering the agent the relevant alternatives, and seeing which he or she chooses, or by asking agents what they would do if faced with such a choice (1972:28). This latter interrogation, Savage claims, is just the right procedure for a theory that endeavors, among other things, to set forth "criteria of consistency for us to apply to our own decisions" (1972:28).

Savage acknowledges that there are difficulties with his behavioral approach. For instance, how is a ranking over more than two acts to be determined? (1972:29). And other authors are also critical of Savage's behavioral approach. Joyce (1999:99), for example, notes that one can prefer A over B where one could never have a choice between them (consider the weather; see also Jeffrey's (1983:ch. 10) critique of Ramsey).

Another issue concerning the nature of preference is its relation to desire. Joyce, for instance, follows Seidenfeld and Schervish (1983:398) in (apparently) equating preference with desire, and sees Savage as in danger of viewing "belief as a second-class propositional attitude that can only be understood in terms of its relationship to desire" (Joyce 1999:89). But perhaps Savage can be given a more sympathetic reading akin to my reading of Ramsey. Preference is not the same as desire: I might have a brute desire for chocolate that is greater than my desire for an apple, but I prefer the apple over the chocolate because of my beliefs about their relative effects on my health. Preference is a product of desire and belief, and Savage is pointing to a method whereby we can measure degree of belief from information about preference: if you prefer an apple to chocolate, then you prefer [apple if six is thrown, chocolate if not] to [apple if five is thrown, chocolate if not] if and only if you believe six more probable than five. This neither renders belief a "second-class propositional attitude" nor precludes you from examining the die in estimating probabilities.

Context Freedom and the Transitivity of Preference

There is much debate over the transitivity of preference. I urge here only that its discussion should not take place in isolation from discussion of context freedom.

Context freedom (CF) is the normative principle that adding or deleting alternatives in the domain of preference should not alter one's preferences over the original domain – consider Savage's remark: "Fancy saying to the butcher, 'Seeing

that you have geese, I'll take a duck instead of a chicken or a ham'" (1972:206). CF is regarded by McClennen (1990:1) as a key component of decision theory (see McClennen 1990 for relevant history). And Sen (1970:17) characterizes a version of context freedom (the "independence of irrelevant alternatives," or "property") as "a very basic requirement of rational choice." But this principle is certainly not uncontroversial: there are examples of (putative) violations of it which do not appear irrational. In Rawling (1997) I argue that Savage's axiomatization does not assume it (although there is evidence that Savage viewed context freedom as a rational constraint.)

On a pairwise dispositional conception of preference, according to which you prefer **f** over **g** if and only if you prefer **f** over **g** when only {**f**, **g**} are on offer (cf. the previous section), violation of CF is simply ruled impossible by definition. Pairwise dispositional accounts of preference yield, of course, an easy empirical test for "preference" – offer the agent the relevant pairwise choices. And there is a concomitant simple test for violations of "transitivity" – on a context-free account of preference, transitivity is equated with pairwise transitivity:

If **f** is preferred over **g** when only {**f**, **g**} are on offer; and **g** is preferred over **h** when only {**g**, **h**} are on offer; then **f** should be preferred over **h** when only {**f**, **h**} are on offer.

However, empirical tractability should dictate neither concepts, nor norms of rationality. And if we abandon a context-free conception of preference, the discussion of transitivity becomes a good deal more subtle. Space precludes discussion here, however.

The Causal Independence of Acts and Events

As the following shows, dominance reasoning (see above) is only valid in cases where "acts are without influence on events" (Savage 1967:307 – call this the "Act–Event Independence Criterion" or AEIC).[17] Consider a smoker deciding whether to quit in light of the cancer risk. Applying PDOM, she reasons as follows (where S is the act of continuing to smoke, Q is the act of quitting, and (~)C is the event of her (not) developing cancer): I prefer S&C to Q&C, and S&~C to Q&~C, hence I should continue to smoke. But the cancer risk is causally dependent upon her smoking behavior, so clearly the reasoning is fallacious. Naturally, Savage does not endorse it: his theory presupposes AEIC (see, e.g., Savage 1967). But this does not limit the range of the theory's application, because cases in which AEIC is not met can always be modified so that it is (see Savage 1972:15; Fishburn 1964:53–5, Jeffrey 1976).

For example, we can fit the smoking example into the Savage framework as follows. There are four relevant events that are independent of the agent's action: she will remain free of cancer regardless of whether she smokes or quits (S->~C, Q->~C); she will develop cancer if she smokes, but not if she quits (S->C, Q->~C); she will develop cancer if she quits but not if she smokes (S->~C, Q->C); she will develop cancer regardless of whether she smokes or quits (S->C, Q->C). We can represent matters in table 5.2:

Table 5.2

	S->~C, Q->C	S->~C, Q->~C	S->~C, Q->C	S->C, Q->C
S	S&~C	S&C	S&~C	S&C
Q	Q&~C	Q&~C	Q&C	Q&C

Contrast this with table 5.3.

Table 5.3

	C	~C
S	S&C	S&~C
Q	Q&C	Q&~C

In table 5.2, but not table 5.3, AEIC is met. Thus applying PDOM to recommend S in the latter table is fallacious. Applying PDOM in table 5.2 is legitimate, but yields no conclusion since neither act dominates.

The Constant Act Problem

The constant act problem is clearly illustrated by table 5.2 (see Savage 1967:306, Fishburn 1981:162–3). Recall that consequences get ranked via the device of a "constant" act – that is, an act "whose consequences are independent of the state of the world" (Savage 1972:25). Thus the Savage axiomatization requires that there be, for instance, an act in table 5.2 with the uniform consequence S&~C, which contravenes the stated nature of the second and fourth events (in which S->C).

Allais's and Ellsberg's Examples

Allais (1953, 1979) and Ellsberg (1961) pose independent (and now classic) challenges to Savage's sure-thing principle (STP: see above).

Allais considers the gambles shown in table 5.4, where the numbers at the tops of the columns are those written on lottery tickets to be drawn at random, and the prizes can be thought of as monetary, say in dollars in units of hundreds of thousands.

Table 5.4

	1	2–11	12–100
G1	5	5	5
G2	0	25	5
G3	5	5	0
G4	0	25	0

STP dictates that G1RG2 if and only if G3RG4, but many prefer G1 to G2 and G4 to G3 – is this irrational? Arguably not (although Savage thinks it is, 1972:103). However, as with the Diamond/Broome example above, we can respond to Allais by individuating outcomes more finely (see, e.g., Broome 1991:98, and the references cited there). Receiving $0 having chosen G2 over the sure $500,000 of G1 is a different experience to receiving $0 having chosen G4 over G3. But now we have the constant act problem again: on Savage's axiomatization, there must be an act with the constant consequence, "$0 having chosen G2 over a sure $500,000."

Whereas Allais's example trades on the value of certainty (the sure $500,000), Ellsberg's (1961) example trades on the value of certain chances (see also McClennen 1990:70–1). There are 90 solid color balls in an urn: 30 are red and 60 are black or yellow in an unknown proportion. You are presented with the gambles shown in table 5.5. STP dictates that g1Rg2 if and only if g3Rg4. But many prefer g1 over g2 and g4 over g3. One might claim that this is irrational, but if not, a cogent response to save STP is tricky, I think.

Table 5.5

	Red	Black	Yellow
g1	$100	$0	$0
g2	$0	$100	$0
g3	$100	$0	$100
g4	$0	$100	$100

Conclusion

Savage's theory has its difficulties, then. But I know of no decision theory that does not (see Fishburn 1981). Jeffrey (1983) (for which the representation theorem was proved by Bolker, 1966, 1967) is a theory that does not require the device of constant acts (Savage), nor the device of gambles (Ramsey; see Jeffrey 1983:ch. 10), and has various other advantages. But it too has its difficulties (see, e.g., Fishburn 1981:187, Joyce 1999, Rawling 1993). As noted above, Joyce (1999) derives utilities and degrees of belief from qualitative restrictions on: the relation "is not more probable than"; the relation "is not preferred to"; and the relation between these relations.[18] This is to abandon a certain Bayesian dream of deriving degrees of belief from rational preference alone. But perhaps the dream is unattainable.

Appendix

To show:

(T) Your degrees of belief violate Kolmogorov's axioms if and only if there is a set of propositions such that the sum of your degrees of belief across its members is either greater than the greatest, or less than the least, logically possible number of truths among them.

Kolmogorov's axioms:

(1) For any proposition A, $0 \leq p(A)$.
(2) If A is certain, then $p(A) = 1$.
(3) If A and B are mutually exclusive, then $p(A \text{ or } B) = p(A) + p(B)$.

(I) Assume your degrees of belief violate Kolmogorov's axioms.
To show:

there is a set of propositions D such that the sum of your degrees of belief across its members is either greater than the greatest, or less than the least, logically possible number of truths among them. If either:

(i) there is a proposition A such that $p(A) < 0$, or
(ii) there is a certain proposition A such that $p(A) \neq 1$,
let D = {A}.

If there are propositions A and B that are mutually exclusive, and $p(A \text{ or } B) \neq p(A) + p(B)$, then

I $p(\text{A or B}) + p(\text{not[A or B]}) \neq p(\text{A}) + p(\text{B}) + p(\text{not[A or B]})$

Consider $D_1 = \{\text{A}, \text{B}, \text{not[A or B]}\}$ and $D_2 = \{\text{A or B}, \text{not[A or B]}\}$. In each set there will be exactly one truth. But either $p(\text{A}) + p(\text{B}) + p(\text{not[A or B]}) \neq 1$ or $p(\text{A or B}) + p(\text{not[A or B]}) \neq 1$. If the former, let $D = D_1$; if the latter, let $D = D_2$. QED

(II) Assume your degrees of belief conform to Kolmogorov's axioms.
To show:

there is no set of propositions D such that the sum (**s**) of your degrees of belief across its members is either greater than the greatest (**g**), or less than the least (**l**), logically possible number of truths among them.

There is a standard way (see, e.g., Scott 1964:246) of expressing (finite) propositional logics using vectors. If we start with n sentential letters, then the relevant truth-table has 2^n rows, and we have 2 raised to the power of 2^n propositions (modulo logical equivalence). Each of these propositions can be represented as a vector with 2^n entries, all zeros and ones, in which a one appears in the ith position if and only if the proposition is true on row i. Any set of degrees of belief that conforms to Kolmogorov's axioms can be represented by a vector, b, with 2^n entries, all greater than or equal to zero, that sum to one, where the degree of belief in a proposition A is given by the scalar product of b and the vector representing A.

(The scalar product of $(A_1, \ldots, A_n)$ and $(B_1, \ldots, B_n)$ is the real number:
$$[(A_1 \times B_1) + (A_2 \times B_2) + \ldots + (A_n \times B_n)])$$

An arbitrary set of propositions D can be mapped to a vector, d, with 2^n entries in which the ith entry equals the number of truths in D on the ith row of the truth table. The sum, **s**, of your (coherent) degrees of belief across D is the scalar product of b and d. Let **g** be the greatest number of possible truths in D and **l** the least; hence **g** is the largest entry in d, and **l** is the smallest.
Since the sum of the entries in b is one with all entries greater than or equal to zero, $\mathbf{l} \leq \mathbf{s} \leq \mathbf{g}$.
QED

Notes

1   Thanks to Paul Roth for making this chapter, I hope, more comprehensible.
2   One's degrees of belief are said to be coherent if and only if they conform to Kolmogorov's (1933) standard probability axioms and definition of conditional probability (see de Finetti, 1972:67–9). These axioms capture the formal properties of

probability, and, leaving the issue of countable additivity aside (omitting de Finetti 1972:69, axiom VI), can be expressed thus (see, e.g., Jeffrey [1965] 1983:80, Resnik 1987:47–54):

A probability function p into the real numbers has as its domain propositions.

(1) For any proposition A, $0 \leq p(A)$.
(2) If A is certain, then $p(A) = 1$.
(3) If A and B are mutually exclusive, then $p(A \text{ or } B) = p(A) + p(B)$.

Definition of conditional probability:

$$p(A \text{ given } B) \text{ (written `} p(A|B) \text{')} = p(A \& B)/p(B), \text{ provided } p(B) \neq 0$$

The following is an immediate consequence (see Resnik 1987:53):

(4) If $p(B)$ is nonzero, $p(A|B) = [[p(B|A) \times p(A)]/p(B)]$

3 The proof of unrepresentability is straightforward. Assume, for purposes of *reductio*, that there is such a representation and $u(L_1) = r$, $u(L_2) = s$, $u(H) = t$, where r, s, t are real numbers. Then: $r > s > t$ and $s > at + (1 - a)r$ for all a such that $0 < a \leq 1$. Hence $a > [(r - s)/(r - t)]$. Yet a can be chosen such that $0 < a \leq [(r - s)/(r - t)]$. Such hellish lotteries can be ruled out of your preference ranking by insisting on the Archimedean condition that, if $L_1 P L_2$ and $L_2 P L_3$, then there exists an Archimedean fulcrum in the form of a real number a $(0 < a < 1)$ such that $L(a, L_3, L_1) I L_2$. (In particular, this condition implies that for any L there is a real number a $(0 \leq a \leq 1)$ such that $L I L(a, B, W)$; and it is key in ensuring the *continuity* of utility: $\{r: r = u(L)$, some $L\} = \{r: r$ is a real number, $u(W) \leq r \leq u(B)\}$.)

 Hellish lotteries give rise to a form of lexicographic preference: in comparing lotteries, you would first rule out any in which H appears with nonzero probability before going on to compare components in those lotteries (assuming there are such) in which H does not appear. According to Debreu, "it has often been assumed in economics that if a set X . . . is completely ordered by the preferences of some agent, it is always possible to define on that set a real-valued order-preserving function (utility, satisfaction). This is easily seen to be false" (1954:164, n.1). Debreu asks us to "consider the lexicographic ordering of the plane: a point of coordinates (a', b') is better than the point (a, b) if $[a' > a]$ or if $[a' = a$ and $b' > b]$." He shows (same note) that this ordering cannot be mapped in order-preserving fashion to the real line (that is, there is no function α from the plane to the real line such that for all w, x, y, z; $\alpha[(w, x)] > \alpha(y, z)]$ if and only if (w, x) is better than (y, z)). The lexicographic ordering of the plane is ruled out by the Archimedean condition if we make the assumption that $L(a, (w, x), (y, z))$ is the point $([aw + (1 - a)y], [ax + (1 - a)z])$. (Note that the ordering is also ruled out by the assumption of a finite number of basic prizes.)

4 Although they do note (Debreu 1954:164, n.2) that "if one objects to the frequency interpretation of probability then the two concepts (probability and preference) can be axiomatized together."

5   The relation between belief *simpliciter* and degree of belief is controversial (see, e.g., Kaplan 1996). The most obvious proposal is to define full belief as degree of belief one. But this has problems. For instance, on the standard account of how one should change degree of belief in the face of evidence, full beliefs should then survive any evidence.

6   Quine attributes the phrase and the basic idea to Wilson (1959).

7   Some might worry that this holistic interdependence of the attitudes has counterintuitive consequences. For example, if all attitudes are interdependent, then it might appear that an agent cannot move from believing that *p* to believing that *not p*, because interdependence entails that the proposition that *p* can be no part of the content of the new belief. This, however, is to misconstrue the nature of the interdependence (I am not the first person to note this: Priest 1981:78, for example, makes the point). There are dependencies in which changes in one factor can leave other factors stable. Consider Ohm's law: voltage (in certain ideal circuits, at least) is the product of resistance and current (V = RI). Here we have three mutually interdependent quantities, and certainly a change in one of them must result in a change in *one* of the others; but the third can remain fixed (e.g., by increasing the voltage in a circuit of fixed resistance, we increase the current). In the case of the contents of propositional attitudes and utterances, of course, we have a vast number of variables – but the same point applies: changes cause disruptions, but their scope will typically affect only a very small portion of the web of propositional attitudes and meanings. And I can share part of your web without sharing all of it (we can have two circuits at three volts, with differing currents and resistances). The number of connections can be as large as you like; it is the nature of the dependence that is crucial. Holism *per se*, then, is no threat to the propositional attitudes.

8   As with several other foundational points in this essay, Donald Davidson impressed this one upon me in conversation. (See also Luce and Raiffa 1957:31–2.)

9   Davidson (1990:318–19) notes a striking parallel between Ramsey and Quine. Central in the work of Quine is the following problem and resolution. Suppose we have worked out that an agent holds true one of his sentences, S, under certain circumstances, but holds it false under others. How are we to work out what he means by it? We confront the following difficulty: the agent holding S true is dependent both upon what he means by it, and upon what he believes the circumstances to be. Meaning and belief must be accessed simultaneously. "Quine's solution [is to] hold [belief] steady [via the principle of charity:] correct interpretation . . . cannot intelligibly admit certain kinds and degrees of difference between interpreter and interpreted with respect to belief" (Davidson 1990:319). Just as Quine confronts the problem of accessing both meaning and belief from "holding true," so Ramsey (1926) confronts the problem of moving from preference to both utilities and degrees of belief.

10   Thanks to Paul Weirich for pointing out various mistakes in an earlier version of this section. Remaining errors are, of course, mine.

11   Schick (1986:113, n.4) also contends that the difficulty arises even if we substitute "utiles" (for which I shall use the symbol "#") for dollars. But this is not so clear. Suppose $y = z = 0.3$, $u(\$0.3) = \#0.3$. This is consistent with $\#0.5 = u(\$0.6)$. Value is not additive in the sense that $u(\$(y + z)) \neq u(\$y) + u(\$z)$. But it is more difficult to establish the rationality of a strict preference between something of value $\#(y + z)$ and the guarantee of #1 if A, #0 if not. It is bootless to establish, for example, that

although the rational agent is indifferent between some object α and the guarantee [#1 if A&B, #0 if not], another object β and the guarantee [#1 if A&notB, #0 if not], yet the agent strictly prefers α and β conjointly to the two guarantees conjointly. β might, for instance, have a value of greater than #z if the agent already holds α, so that the value of α and β conjointly is greater than #(y + z).

12 What objective chances are, if such there be, is a matter of controversy. We have seen one possible suggestion: long-run frequency.

13 Bayes's theorem (so called because implicit in Bayes 1763) is a straightforward consequence of Kolmogorov's axioms and the definition of conditional probability (see Resnik 1987:53), thus accepting Bayes's theorem is not what makes one a Bayesian (see Edwards et al. 1963:193–4), although the theorem does play a crucial role in the Bayesian program (see Resnik 1987:55–6).

14 Let S be a set of n propositions; let $\sim S$ = {notP: P ∈ S}. Let p = your prediction of the number of truths in S; let l = the least logically possible number of truths in S. Suppose $p < l$. Then $n - p > n - l$.

 And $n - p$ = your estimate of the number of truths in $\sim S$; $n - l$ = the greatest number of possible truths in $\sim S$. Thus your beliefs across $\sim S$ are inconsistent.

15 Cf. what Jeffrey calls "DE FINETTI'S LAW OF SMALL NUMBERS: your estimate of the number of truths among the propositions $A_1, \ldots, A_n$ must equal the sum of the probabilities you attribute to them" (1986:54). See also Jeffrey (1992:60) and de Finetti ([1937] 1964:ch. 2).

16 With the aid of his P7 (1972:77), Savage generalizes utility to acts with an infinite number of consequences.

17 To avoid complications not germane here, I suppose that: AEIC holds if and only if the agent believes it does.

18 Joyce builds on Jeffrey (1983), and I am not convinced that he avoids all the putative difficulties with that theory.

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The Methodology of Rational Choice

Lars Udehn

One of the most noteworthy phenomena in recent social science is the upsurge of a theory, and/or methodology, called "rational choice." Originally confined to economics, it has recently spread to political science and sociology, and even to anthropology. Rational choice is so called because it is based on the assumption that human beings are rational in their choice of means to reach their preferred ends (see Elster 1986). It has been common, among rational choice theorists, especially economists, to assume that the "ultimate end" of human beings is utility-maximization (Arrow 1987:204ff.), but there is also the less demanding assumption of consistent preferences (Riker 1990:172). There is no single, commonly accepted, version of rational choice, but a family of different versions, united by resemblance, and based upon different conceptions of rationality (Sen 1990, Sugden 1991). It is not my business, here, to scrutinize the different versions of rational choice, or add to the existing plethora of definitions of rationality. My interest, in this article, is in the use, rather than the exact meaning, of rational choice theory. More precisely, my interest is in certain philosophical or methodological aspects of rational choice, as used by social scientists. For this purpose, "rational choice" is, roughly, whatever goes by that name in the history of social science.

Classical Economics

It is possible to trace rational choice back to Greek antiquity: to the Sophists, to Aristotle, and to Epicurus. In the later history of social theory important contributions to rational choice were made by Thomas Hobbes, David Hume, and Jeremy Bentham.

The first, and still the main, social scientific use of rational choice is to be found in economics. The founding father of economics, Adam Smith, did not state his

theory of the market explicitly as a theory of rational choice, but he certainly seems to imply that individuals engaged in economic exchange seek their own good in a rational manner. David Ricardo was even less explicit about his methodology, but he used a method, which has been called "abstract-deductive," or "geometric," because it was supposed to mimic the method of Euclidian geometry. The real champion of this method, however, was Ricardo's friend and teacher in matters philosophical, James Mill, who used this method himself in his *Essay on Government* ([1820] 1955).[1]

It was only gradually that the assumption of rationality was recognized as fundamental to economics and made explicit. An important step in this direction was taken by James Mill's son John Stuart Mill in his article "On the definition of political economy and the method of investigation proper to it" ([1836] 1950). In this article, Mill suggests that political economy "is concerned with him [man] solely as a being who desires to possess wealth, and who is capable of judging of the comparative efficacy of that end" (1950:420). According to Mill, then, people's end as economic beings is wealth maximization and this end is achieved by calculation of the different means to that end, or in a rational manner.

Mill's article was the first serious or, at least, successful, attempt to clarify the nature of economics, as a science. According to Mill, economics as a *science* is to be distinguished clearly from economics as an *art*. Economics is a *moral*, or psychological, as distinguished from a *physical* science, and it deals with human beings as existing in a state of society, not in a state of nature. As with all moral sciences, political economy is based upon laws of human nature, but not on all of them. Political economy abstracts from every human motive, except the rational pursuit of wealth (Mill [1836] 1950:420). Human beings are assumed always to prefer a greater portion of wealth to a smaller. "Not that any political economist was ever so absurd as to suppose that mankind are really thus constituted, but because this is the mode in which science must necessarily proceed" (1950:421).

According to Mill, then, political economy does not tell the whole and absolute truth. The laws of human nature, on which it is ultimately based, are only approximations to the truth and it disregards many disturbing causes known to influence economic phenomena in the complex social reality. In the terminology introduced by economists in the generation after Mill, economic laws hold only *ceteris paribus* (other things being equal).[2] According to Mill himself ([1836] 1950:438), social laws, economic laws included, take the form of *tendencies* (see also Mill [1843] 1974:898). Since, in reality, other things are rarely equal, and tendencies are usually counteracted, sometimes pre-empted, verification is difficult, if not impossible. For all these reasons, economics is, in the opinion of Mill ([1843] 1974:844ff.), not an exact science. Its conclusions "would be true, without qualification, only in a case which is purely imaginary" (Mill [1836] 1950:425). Even so, Mill conceived of political economy as an abstract, or deductive science *a priori* and defended this view against the radical empiricists (inductivists) of his day. It is important, however, to be clear about the meaning of *a priori* in the methodology of Mill. He did not claim that economic laws are

true, or valid, *a priori*; only that they are deduced from certain hypotheses about human nature, assumed to be approximately true, but not arrived at by induction and not subjected to test. Mill rejected the inductivist claim that it is possible to arrive at general laws by means of upwards induction, and maintained that all science proceeds by reasoning downward from assumed premises. By means of induction, one arrives only at empirical laws, or generalizations, not at causal laws.

According to Mill ([1843] 1974:861ff., 879, 907ff.), there is only one type of causal law in the social, or "moral," sciences: the psychological laws of human nature. There may be social laws connecting social states, but these empirical laws must be deduced from causal laws about human nature. In more recent terminology, Mill was a methodological individualist, who demanded that all macrotheories in social science be provided with microfoundations. Since these microfoundations are to be found in psychology, Mill is usually seen as adhering to the doctrine of psychological reductionism, or psychologism.

As we saw in the first quotation from Mill, above, political economy is concerned with human beings only to the extent that they are engaged in wealth maximization. Since this motive is expressed mainly in market activity, we may draw the conclusion that Mill saw economics as limited, in its scope, to the market. This used to be the common view among economists until some decades into the twentieth century. A well-known example is Alfred Marshall ([1890] 1920:1ff.), who is close to Mill in his view of the nature of economics, but who adds that the superiority of economics over the other social sciences is due to the fact that it relies on the measuring rod of money.

Neoclassical Economics

Despite his association with utilitarian philosophy, Mill did not introduce the assumption of utility maximization in political economy. This move was taken by Stanley Jevons, who maintained that "pleasure and pain are . . . the ultimate objects of the calculus of economics" or, in other words, "to *maximize pleasure*, is the problem of economics" ([1871] 1979:101). Jevons was one of three economists who made the marginalist revolution[3] – the other two were Carl Menger and Leon Walras. If Jevons introduced the idea of rationality as utility maximization, it was Menger and the other Austrians who introduced the idea of economizing, or the best (optimal) use of scarce resources in the satisfaction of needs.

Austrian economics has led a life of its own beside mainstream economics, but it did exert some influence on other neoclassical economists, who adopted the idea of economics as rational choice. One example is the English economist Philip Wicksteed, who maintained that economics is based on a "psychology of choice between alternatives" (1910:2f.). Another example is the founder of the Chicago School, Frank Knight, who may have been the first, explicitly, to suggest

that economics is a theory of "rational choice" (1921:89). More important, however, is his distinction between risk and uncertainty. In the case of risk, the probabilities of different outcomes are known and rationality is possible. In the situation of uncertainty, however, we know nothing about these probabilities and the ground for rational action is lacking. Modern economics, however, tends to obscure, or deny, the distinction between risk and uncertainty and treat both as cases of risk.

A third important economist, heavily influenced by the Austrians, was Lionel Robbins. He adopted the Austrian idea of economizing and used it in his famous definition of economics as "the science which studies human behavior as a relationship between ends and scarce means which have alternative uses" ([1932] 1935:16). He was also among the first to conceive of rationality as consistent preferences. "If I prefer A to B and B to C, I also prefer A to C . . ." (1935:92).

Walras, finally, had little to say about rational action, but more about the overall result of the economic action of many individuals on a market. Walras is the founder of general equilibrium theory in economics and this is, of course, no small contribution to rational choice. The idea of equilibrium – not only general equilibrium – is used to achieve determinacy in rational choice models of interaction between two or more individuals (Murphy 1995) and has been fundamental to neoclassical economics as a science (Hausman 1992:ch. 3). Recent contributions, however, show that rational choice theories frequently fail to arrive at determinate outcomes (Bicchieri 1993:15–18, ch. 2). Among the more problematic aspects of rational choice models is the existence of multiple equilibriums (Kreps 1990:95–107).

A second revolution occurred in economics with the publication of John Maynard Keynes's *The General Theory of Employment, Interest and Money* in 1936. This event marks the birth of modern macroeconomics, which is a theory about relations between aggregates, such as output, investment, savings, and employment. From early on, Keynesian macroeconomics was considered suspect, or lacking, by more orthodox neoclassical economists and especially by the Austrians. What was lacking in particular were solid, individualistic, microfoundations in the form of rational choice (Weintraub 1979, Janssen 1993).

A third revolution started in economic theory when John von Neumann and Oscar Morgenstern published their *Theory of Games and Economic Behavior* (1944). The main difference between traditional economics and game theory is that the latter takes social interaction between rational individuals into account and assumes that each individual acts on the basis of his or her expectations concerning the actions of other individuals. In traditional economics, individuals act only impersonally and anonymously on the market, but in game theory they interact with other individuals, who are also assumed to be rational. Game theory, then, deals with "mutually interdependent reciprocal expectations by the players about each other's behavior . . ." (Harsanyi 1977:10). In the terminology of Jon Elster, game theory works with the assumption of "strategic" rationality, as distinct from "parametric" rationality of traditional neoclassical economics (Elster 1979:18f., 117f.).

Neoclassical Methodology

Of the three branches of marginalist, or neoclassical, economics, it was the least orthodox – Austrian economics – that took most interest in the philosophical aspects of economics. Carl Menger, for instance, wrote a book on the method of political economy (Menger [1883] 1963), which was largely in line with the views of Mill, but also influenced by German philosophy and social science. Like Mill, Menger made a distinction between two types of law: laws of nature or exact laws, and empirical laws. He also defended abstract theory and methodological individualism, or atomism, against the historicism, holism, and empiricism of the German Historical School in economics. Unlike Mill, however, Menger claimed that the laws of economics are *exact* laws.

What Menger understood by "exact laws" is not easy to grasp, especially not for a reader trained in the Anglo-Saxon philosophical tradition. The philosophical background is to be found in Aristotle and in the phenomenology of the Austrian philosopher and psychologist Franz Brentano (Kauder 1958, Smith 1986). For my present purposes, it will suffice to say that exact laws, according to Menger, are not just *a priori*, in the sense of Mill (see above), but absolutely true, or "valid" *a priori*. They are "*laws holding for an analytically or abstractly conceived economic world*" (Menger [1883] 1963:72f., emphasis in original), but not a less real world for all that. According to Menger, exact laws are about the very essence of things (see Mäki 1990).

Menger is also the main founder of methodological individualism. His own name for it was "atomism," and it says that "whoever wants to understand theoretically the phenomena of 'national economy' . . . must . . . go back to their true elements, to the singular economies [individuals] in the nation, and to investigate the laws by which the former are built up from the latter" (Menger [1883] 1963:93).

Menger's book on the methodology of economics gave rise to a heated controversy, known as the "Battle of Methods" (*Methodenstreit*). It raged for decades, but did not bring much light to the issues involved. One exception was the contribution of Max Weber, which helped to clarify the concept of "rationality" in terms of means and ends (Weber [1903–6] 1975:186ff.), and suggested that the assumptions of economics, including that of rationality, are *ideal types*: abstract constructs "arrived at by the analytical accentuation of certain elements of reality" (Weber [1904] 1949:90). In other words, ideal types are simplified models of reality, reached by "isolating abstraction" (Weber [1917] 1949:43). As such, they are "pure fictions," and yet not entirely fictitious, since they approximate reality. "Economic theory," for instance,

> . . . makes certain assumptions which scarcely ever correspond completely with reality but which approximate it in various degrees and asks: how would men act under these assumed conditions, if their actions were entirely rational? It assumes the

> dominance of pure economic interests and precludes the operation of political and other non-economic interests. (Weber [1917] 1949:44)

Weber is usually seen as situated somewhere between the Austrian and the Historical School in economics, but I believe that his methodological views were closer to those of the Austrians, since he defended both rational choice and methodological individualism. He did not, however, accept Menger's view that the laws of economics are exact laws. According to Weber, they are not laws at all, at least not on a par with the typical laws of natural science. The rationality assumption, for instance, is not a hypothesis in the ordinary sense, since it cannot be falsified by an appeal to the facts (Weber [1905–6] 1975:188–91).

For Weber, the use of the rationality assumption is mainly heuristic and expository (Weber [1904] 1949:90, [1905–6] 1975:188ff.), but he assigns to it a methodologically privileged position.[4] The ideal type of rational action has the merit of being easier to understand than other actions and may, therefore, be used as a methodological point of departure. Other types of action, or at least irrational actions, are easiest to understand when compared with rational actions. Weber's advice, therefore, is to start by assuming that individuals act rationally, and then to treat other actions as deviations from this ideal type (Weber [1922] 1978:5–7).

It is common, if unfortunate, to single out Ludwig von Mises as the main representative of Austrian methodology (see, e.g., Caldwell 1982:103f., 117ff.). One reason for doing so is that he took the extreme position that the laws of economics are true *a priori*. Another reason is that he influenced Lionel Robbins, who wrote one of the main classics on the methodology of economics (see below). But Ludwig von Mises is not representative of Austrian methodology and he was not the only one to influence Lionel Robbins.

The point of departure for Mises's methodological reflections was Max Weber's interpretive sociology and, especially, his notion of ideal types. Mises accepted most of Weber's methodology, but rejected his conception of "rationality" and his suggestion that the basic assumptions of economics are ideal types (Mises [1929–33] 1976:75ff.). For Mises, all action, as distinguished from reactive behavior, is necessarily rational. Action is a category ultimately given to human experience and the general theory of this experience is called "praxeology." Theoretical economics is a branch of praxeology, and as such it is made up of statements known to be true *a priori* (Mises [1929–33] 1976:12–17, 23ff., [1949] 1966:11ff.). Some believe that Mises conceived of economics as made up of analytic statements, which are true *a priori*, but not about reality (see Smith 1990:279–82), but the most common interpretation is that he was a Kantian, suggesting that economics consists of synthetic statements, known to be true *a priori*, but, nevertheless, about reality (Caldwell 1982:121, Selgin 1988:21). The latter interpretation is much more likely, but both may very well be wrong. It has also been suggested that Mises's *aprioristic* interpretation of the principle of rationality derives from the idea of an *eidetic* science, in the sense of the phenomenological philosophers Franz Brentano and Edmund Husserl (Smith 1990:279ff.).

Be that as it may, Mises's interpretation of economics was not accepted by the most influential Austrian economist of this century, Friedrich von Hayek. In his classic article "Economics and knowledge" (1937), Hayek rejected both equilibrium analysis and praxeology, or the logic of choice. The problem with both is that they are tautological, and so fail to say anything at all about reality. Hayek's preferred alternative is to conceive of economics along the lines suggested by Max Weber: as an interpretive (*verstehende*) science working with ideal types as its main analytical tool (Hayek [1937] 1948:47, n.12, 52, n.18). Hayek's main contribution to economic methodology was not his clarification of the nature and status of economic generalizations, but his explication of methodological individualism along the lines first suggested by Menger (Hayek [1942–4] 1952:36–43).

Austrian economists have been far more influential in economic methodology than in economic theory proper, but their influence has been largely indirect. One channel for this influence was Frank H. Knight; another was Lionel Robbins, who wrote the main classic in this field: *An Essay on the Nature and Significance of Economic Science* ([1932] 1935). Robbins made no secret of his allegiance to Austrian economics and, in the first edition, especially, to Ludwig von Mises (Robbins [1932] 1935:xvi). In the second edition Max Weber replaces Mises as the main authority ([1932] 1935:xi–ii).

The reason for this change may very well be a change in Robbins's own views about the status of economic generalizations. There are significant differences between the first (1932) and second (1935) editions. In the former, economics is said to rest on certain self-evident truths, based on immediate experience, from which are deduced economic generalizations of a purely formal character (1932:96ff.). With reference to Frank H. Knight, who was also influenced by Mises, Robbins points to the affinity of economics with logic and mathematics (1932:110). In the second edition, however, this affinity is emphatically denied. Unlike logic and mathematics, the generalizations of economics are about reality, and therefore not formal in character ([1932] 1935:104). Economic analysis now "consists of deductions from a series of postulates, the chief of which are almost universal facts of experience present whenever human activity has an economic aspect . . ." ([1932] 1935:99f.). Economic theory is about reality, but it is not realistic. It is in the nature of abstract theory to be unrealistic. The question to be asked about theory is not whether it is true, or realistic, but whether it is or applicable to a given situation ([1932] 1935:116ff.). The main function of realistic studies, according to Robbins, is to decide which theories are applicable, what auxiliary assumptions are needed, and to suggest reformulations of the theories themselves.

Positivism, Popper and Beyond

Austrian methodology did not remain unchallenged and most difficult to accept was Mises's suggestion that the generalizations of economics are true *a priori*.

The dominating philosophy of science from the 1930s to the 1960s was logical positivism or, more broadly, logical empiricism. According to this philosophy, there are only two kinds of meaningful statement: analytic and empirical. The first are necessarily true, but not about reality; the latter are about reality, but not necessarily true. In this philosophy, there is no place for synthetic statements that are both *a priori* and about reality. If economics claims to be an empirical science, therefore, it cannot be *a priori*, and its laws must be verified or, at least, confirmed by facts.

This argument was made most forcefully by T. W. Hutchison in *The Significance and Basic Postulates of Economic Theory* (1938), who claimed that economics must be turned into an empirical science based upon the observation of economic behavior, but a majority of neoclassical economists seem to have adopted a positivist, or some other empiricist, position. Paul Samuelson's efforts to replace utility by revealed preferences were motivated by an empiricist wish to avoid unobservable theoretical entities (Samuelson 1938) and A. G. Papandreo was worried about the consequences of the attempt to build a universally valid economic theory. The result, he suggested, will be "operational meaninglessness" and lack of empirical relevance (1950:721).[5]

The most famous piece on economic methodology ever written, Milton Friedman's "The methodology of positive economics" (1953), has also been conceived of as "positivist" but it is really a piece on its own. If a label is needed, "instrumentalism" is probably the best to characterize his position (Boland 1979, Caldwell 1982:ch. 8). According to Friedman, the assumptions of economics are neither *a priori*, nor empirical, but more or less convenient instruments for making predictions about economic phenomena. The assumptions of economics are not even true, but nor are they supposed to be. The truth or realism of assumptions is immaterial, since the goal of science is prediction, not explanation.[6] Although much read, Friedman has few followers in matters methodological. Most economists seem to believe that the "realism" of assumptions is a matter of some importance (Koopmans 1957:137–42, Samuelson 1963).

By the time of Friedman's essay positivism was already on the wane and Karl Popper's falsificationism would soon replace positivist verificationism as the main methodology in economics.[7] Popper's ideas were first advanced in *Logik der Forschung* (1934) and were mentioned by Hutchison as early as 1938, but it was only after the English translation of this work (1959) that Popper's falsificationism became popular among economists (De Marchi 1988). By this time, Popper had also written *The Open Society and Its Enemies* (1945) and *The Poverty of Historicism* ([1944–5] 1961), where he launched a method called the "logic of the situation" as the most important tool of social science. According to Popper himself, situational logic is the method of economics. It would appear, therefore, that Popper was one of the earliest advocates of rational choice in social science. He was also a methodological individualist, who advocated institutionalism (Popper [1945] 1966, vol. 2:135ff.). The program of combining rational choice, methodological individualism, and institutionalism has been realized in the new

institutionalism in economics (see below). To the extent that social institutions are treated as exogenous variables, we have to do with a new form of methodological individualism, which has been called institutional individualism (Agassi 1975, Boland 1982:ch. 2).

It would soon turn out, however, that Popper's methodology was no more reassuring to economists than was positivism. It was pretty obvious to most observers that economists are not falsificationists in their scientific practice (Blaug 1980). The impression is, instead, that no amount of negative evidence would make economists give up their basic assumptions and theories. In Popperian parlance, they seem to avoid refutation, by engaging in "conventionalist stratagems" (Popper 1959:82).

But should economists try to refute their basic assumptions? As we saw in the previous section, many economists have denied that the assumptions of economics are open to falsification. If they are deliberate simplifications, or ideal types, known to be only approximately true, how can they be falsified? And what about the notorious *ceteris paribus*? Can economists really do without this clause, which is known to make falsification highly problematic (Grunberg 1966)? The answer to both these questions is probably no, and naive falsificationism, at least, does not seem to be a suitable methodology in economics (Caldwell 1982:chs. 11–12, Hausman 1992:ch. 10), or in any other social science for that matter. It may be added that Popper probably never was a naive falsificationist and eventually admitted that falsificationism is of limited use in the social sciences (see Caldwell 1991).

In a lecture delivered in the Department of Economics at Harvard University in 1963, Popper explicitly denies that the rationality principle plays the role of an empirical explanatory theory, or a testable hypothesis (Popper 1994:169). He admits that it is *a priori* (1994:171), but denies that it is true or valid *a priori*. According to Popper (1994:172), it is "clearly false." Like all models, it is a "vast oversimplification," but also a "lucky oversimplification" (1994:173). This does not mean, however, that Popper accepts Friedman's instrumentalism. The aim of science is truth and it is possible to conceive of it as approaching the truth. The best theory, or model, is that which is the best approximation to the truth. The rationality principle is a good approximation to the truth, but its status is somewhat special. According to Popper, it is an integral part of almost every testable social theory, but it is itself exempted from refutation. "My thesis is that it is *sound methodological policy* to decide not to make the rationality principle, but the rest of the theory – that is, the model – accountable" (1994:177). As I understand Popper, he defends a position that has much in common with the views of Mill, Weber, Robbins, and Hausman.

The actual practice of neoclassical economists seems to be more in line with Thomas Kuhn's idea of a paradigm and, especially, with that of normal science (Kuhn [1962] 1996). Instead of questioning the foundations of their science, neoclassical economists typically engage in puzzle-solving activity within the accepted paradigm. More dear to economists, themselves, however, is the idea of

scientific research programs as developed by Popper's follower Imre Lakatos (see, e.g., Latsis 1976). According to this idea theories are never rejected because of a single falsification, or two, but survive as it were in an "ocean of anomalies" (Lakatos 1970:133, 138). Theories are part of research programs that may be progressive, or degenerating, but there is "*no falsification before the emergence of a better theory*" (Lakatos 1970:119, emphasis in original). This claim was good news to economists and other adherents of rational choice, who rest assured in the conviction that their own approach is, at least, much better than any of its rivals (see, e.g., Chong 1995, Ordeshook 1995:186). In the words of Jon Elster (1986:27) "the continued dominance of neoclassical theory is ensured by the fact that one can't beat something with nothing."

With few exceptions, economists today do not believe that the basic assumptions of their science are true *a priori*, but it may be argued that they use them *as if* they were and in effect turn them into analytic statements. As Joseph Agassi (1970:50) has suggested, "most of traditional economics, to say the least, is in a no-man's-land between analytic and empirical," but there is a constant tendency towards the analytic or tautological (see also Sen 1979). This view has been stated most forcefully by Alexander Rosenberg, who maintains that economics has stopped being an empirical science and turned into "a branch of mathematics somewhere on the intersection between pure and applied axiomatic systems" (Rosenberg 1983:672).

Daniel Hausman is of another opinion. He denies that economists are overly dogmatic. Their method is *a priori* in the sense of Mill (see p.144 above), but they do not treat their basic assumptions as analytic. With Hausman we are back, full circle, to John Stuart Mill and the idea of economics as an inexact and separate science, which is not so much true or false, as more or less applicable to different phenomena and most applicable to the market. I find much to agree with in Hausman's analysis, but it is normative, rather than descriptive. In actual fact, lots of economists today see economics as a general, rather than a separate, social science.

The New Institutional Economics

In traditional neoclassical economics social institutions, such as firms and states, laws and other norms, are treated as exogenously given and neglected. In the 1950s, however, a number of economists, belonging mainly to the Chicago and Virginia Schools in economics, began to include social institutions in their analyses.

The new institutionalism in economics did not take off until the 1960s, but its real origin lies in two famous articles by Ronald Coase: "The nature of the firm" (1937) and "The problem of social cost" (1960). In the first article, Coase asked why there are firms at all, and suggested the heterodox answer that market exchange involves costs other than those covered by the price. There are costs of

negotiating and concluding contracts, which may make it rational to replace the exchange relation on the market with authority relations in a firm. Coase called them "contract costs," but they were later termed "transaction costs" and have come to include things not mentioned by him, most importantly, information costs. The idea of transaction costs was later exploited most fruitfully by Oliver E. Williamson (1975, 1985), who tried to identify the general causes behind the development of various "governance structures." Markets and hierarchies are the two extremes on a continuum of economic relations, but with many other forms of integration in between.

In his second article, "The problem of social cost," Coase advanced the controversial argument that externalities lead to inefficiency only because there are transaction costs. In a world without transaction costs, economic agents would have to buy their right to produce negative externalities and the end result would be efficient for the economy as a whole. This argument appealed, especially, to some members of the Chicago School, who turned to the analysis of property rights. The pioneers of the property rights tradition are Armen A. Alchian (1961) and Harold Demsetz (1964, 1966, 1967), who also made important contributions to the neoinstitutional theory of the firm (Alchian and Demsetz 1972). Another important figure, at some distance from Chicago, is the economic historian Douglass C. North, who has argued that the existence and stability of suitable ground rules, including property rights, is an important precondition for economic growth. No less important is a state, since this is the institution that creates and maintains a proper legal framework – the rules of the game – for economic activity (North 1981, 1990).

The analysis of social institutions in the property rights tradition is too complex and variegated to be expounded here, but the underlying message is often simple: social institutions are – or ought to be? – the results of maximizing behavior. They emerge and remain because they are rational for individuals and efficient for society. This message is often found in the writings of members of the Chicago School and probably reflects a commitment to the strong version of methodological individualism: social institutions are treated as endogenous variables, or something to be explained.[8] In other cases, however, including the analyses made by Williamson and North, social institutions are often treated as exogenous variables that explain. In these cases, we have to do with a weak version of methodological individualism, called institutional individualism.

At first sight, the new institutional economics might seem to increase the realism and empirical content of economic theory, but things are not that simple. Concepts such as "information costs" and "transaction costs" denote costs that are real enough, but hard to measure. In the end, they may very well lead to a decrease, instead of an increase, in empirical content (cf., for example, Elster 1979:156). It is important to distinguish between the informative and empirical content of scientific theories. The former has to do with how much a theory says about the world; the latter refers only to its testability. These magnitudes are related, but not identical.

The new institutional economics goes beyond neoclassical economics not only in its approach, but also in the domain to which it is applied. This transgression of the traditional boundaries of economics has been called "economic imperialism" (Tullock 1972, Stigler 1984). The most radical example of economic imperialism is the economic approach of Gary S. Becker (1976), which is launched as a general theory of human behavior and used to explain phenomena as varied as discrimination, criminality, and marriage. To achieve his goal, Becker introduces new types of costs and benefits in his utility functions. Among his innovations, we find things like "psychic costs" and a "taste for discrimination."

Like many other economists, Becker (1976:7, 13) believes that a more general approach to human behavior is a way to avoid ad hoc explanations. According to many critics of economic imperialism, however, Becker's own approach is notorious for its excessive use of ad hoc explanations (Rosenberg 1979:513ff., Blaug 1980:242f., Simon 1986:28ff.). The explanation for this difference of opinion is that Becker believes that only explanations that rely on other motives than utility maximization are ad hoc, whereas the critics believe that it is equally ad hoc to introduce all sorts of costs and benefits (arguments) in people's utility functions. I believe that the critics are right. The economic approach to human behavior has produced a lot of ad hoc explanations and it has serious problems with empirical content.

A general theory, however, leads to a loss not only of empirical content, but of informative content as well. A theory that attempts to explain everything usually ends up explaining nothing, or very little (Weber [1904] 1949:80, Popper [1944–5] 1961). This is the case at least when phenomena are heterogeneous, as they typically are in social science (Roberts 1974:58). In this case, there is an inverse relation between generality and content. This is a simple fact of logic known as "the principle of the inverse variation of extension and intension" (Nagel 1961:575).

Public Choice

A particular form of economic imperialism is public choice, or the economic theory of politics. Public choice may be divided into two branches: (1) the Virginia School, whose members are economists; and (2) the Rochester School, made up of political scientists. Both branches are explicit applications of rational choice and based on methodological individualism.

The main founder and head of the Virginia School is James M. Buchanan, who claims that public choice is based on the three assumptions of self-interest, exchange, and methodological individualism (Buchanan 1988:104). Most important in the history of the school has been the first assumption: public choice took off from the idea that political man is really economic man,[9] that individuals are equally self-interested in politics as they are in the market. This assumption has

generated a host of theories about various political actors, such as politicians, voters, bureaucrats, and interest groups, who are all assumed to seek their own, rather than the public, good. The conclusion of virtually all these theories is that politics is inefficient.[10]

The founders of the Rochester School are William H. Riker and Peter H. Ordeshook, who were also pioneers in using game theory for the purposes of political analysis (Riker and Ordeshook 1973). Political science was the first among the social sciences to make extensive use of game theory (see Ordeshook 1990, 1992). The reason for this is probably that strategic interaction is more common and more obvious in politics than in other forms of social life.

Like the new institutional economics, public choice is a huge field, which cannot be covered in this chapter. I will make brief mention only of two of its main classics, both of which raise some important questions about rational choice.

In *An Economic Theory of Politics* (1957), the economist Anthony Downs set out to study democratic politics, with the tools of economic theory. First, he assumed that political man is really economic man, a human who acts rationally and for her/his own selfish ends (1957:27ff., 282ff.). Members of political parties "act solely in order to attain the income, prestige, and power which come from being in office" (1957:28), which is achieved by winning elections. The immediate end of politicians, therefore, is to maximize votes. Voters, on the other hand, act so as to maximize their own utility, the benefits they expect to receive from voting for a certain party (1957:37).

Downs's most lasting contribution to an economic theory of politics is his spatial theory of voting (1957:ch. 8). In this theory, parties are seen as distributed along a one-dimensional ideological spectrum from left to right. The most well-known hypothesis, derived by Downs, is that parties in a two-party system tend to converge on the ideological center. The reason for this is that they can usually rely on the support of voters on the same side of the middle, but more extreme in their political views. The chance for parties of winning elections, therefore, usually lies in attracting the voters close to the center.[11]

In the economic theory of democracy, it is assumed that voters maximize their own utility. But on this assumption, it is hard to see why people should vote at all. As observed already by Downs, the costs of voting often exceed the expected returns. In these cases, it is rational to abstain from voting (Downs 1957:ch. 14). Considering the usually infinitely small probability that a particular vote will decide the outcome of a mass election, it is almost always "irrational" to vote, at least on the assumptions of rational choice, and yet most citizens do vote. This fact is called the "paradox of voting." To explain this paradox within the framework of rational choice has been a main preoccupation of its defenders.

Gordon Tullock (1967) suggested that people vote because of a social pressure to do so. Riker and Ordeshook (1968, 1973:60f.) believe that there are a number of additional benefits of voting, including the satisfaction derived from doing one's duty. Ferejohn and Fiorina (1974) took an altogether different route and claimed that it might be rational to vote after all, if people minimize their maximum

regret rather than maximize their expected utility. So far, however, no convincing rational choice explanation of voting in mass elections has been put forward, and it is probably vain to expect one to be forthcoming in the future. This is the conclusion also of some rational choice theorists (Brennan and Buchanan 1984, Fiorina 1990).

The paradox of voting may be seen as an instance of a more general problem of collective action: why do people participate in collective undertakings when the contribution of each is too small to make a significant difference to the outcome, and when they can often reap the fruits even when they have not sown. These considerations led the economist Mancur Olson to conclude, in his classic *The Logic of Collective Action* ([1965] 1971), that "unless the number of individuals in a group is quite small, or unless there is coercion or some other special device to make individuals act in their common interest, *rational self-interested individuals will not act to achieve their common or group interest*" ([1965] 1971:2, emphasis in original). As already indicated, a first reason for this is size. In a small group, it is often rational even for a self-interested individual to contribute to the provision of collective goods but, as the size of a group increases, there is a decrease both in the relative importance of each individual's contribution and in her or his share of the produce. A second, and better known, reason is that, according to Olson, collective or public goods are typically characterized by nonexcludability. It is impossible or, at least, difficult to exclude people from enjoying the benefits of such goods. Important examples are such things as laws, regulations, and wages, which are the typical results of the collective action of various interest groups, but which are usually provided to all in a specific group, or category, of people. According to Olson, then, people participate in large-scale collective action only when there is some selective incentive added to the benefit derived from the public good itself.

The paradox of voting and the problem of collective action are anomalies that represent failures, on the part of rational choice, to explain important social phenomena. As we have seen, this is admitted also of some leading rational choice theorists. In this situation, there are two basic options open to rational choice theorists: (1) to attempt to solve the problems in an ad hoc manner, within the rational choice approach; (2) to conclude that there are limits to rational choice, that it does not apply equally well to all domains of social life. Most rational choice theorists have opted for the first solution. The costs of this solution are high, however, and consist in a loss of empirical content that accompanies so many uses of rational choice. The first to point this out, in a clear and convincing manner, with respect to the theories of Downs and Olson, was the political scientist Brian Barry ([1970] 1978:19–23, 33–37). Among the last to do so were Donald P. Green and Ian Shapiro (1994), from their more "empiricist" point of view.

The second solution is to introduce other causes than the rational choices of individuals in the explanation of voting and collective action. This is the solution of Jon Elster, who argues that collective action is best explained as the result of mixed motivations (Elster 1985, 1989).

Rational Choice Sociology

Eventually rational choice reached sociology, a discipline at first not very favorably disposed to this approach (Heckathorn 1997:6). Sociologists tend to believe that human beings are followers of various rules, rather than being instrumentally rational actors. As we have seen above, Max Weber was an exception, since he assigned an important place to the ideal type of instrumentally rational action in his interpretive sociology ([1922] 1978:24ff., see also Elster 2000). The real pioneer of rational choice sociology, however, is James Coleman, who launched a theory of exchange based on rational choice, back in the 1960s, to replace the then dominating structural-functionalist approach (Coleman 1964).

Coleman's work culminated with *Foundations of Social Theory* (1990), which is the single most important contribution to rational choice sociology so far. In many respects, Coleman's theory of exchange is economics generalized and applied to social exchange. But there are differences as well. The most important difference is probably that Coleman, like many other social scientists, sees the need for a concept of "social structure" that is absent from economic theory.

From a methodological point of view, the most important aspect of Coleman's sociology is the use it makes of rational choice as a microfoundation for various forms of macrosociology (Coleman 1986), including the statistical correlations typically produced by empiricist sociologists (Hedström and Swedberg 1996). Coleman himself sees his quest for microfoundations as a manifestation of methodological individualism (Coleman 1990:5), but it is an extremely weak form of methodological individualism, since it admits social structure as a determinant of the social action of individuals (Coleman 1994:166f.).

Summary and Conclusion

According to one interpretation, rational choice theories are deductive systems, made up of statements that are true *a priori*. According to some these statements are analytic; according to others they are synthetic. Today, few are willing to accept the Kantian idea of synthetic statements *a priori*. Scientific theories are either analytic or synthetic, but not synthetic *a priori*. To the extent that they are true *a priori*, they are not about reality, and to the extent that they are about reality, they not true *a priori*. According to logical positivism, all synthetic statements are, or must be, empirical, but today most philosophers of science recognize a "metaphysical element" in science, made up of synthetic statements that are not empirical. Today, few adherents of rational choice claim that their theories are analytic and true *a priori*, but it is common among critics of rational choice to argue that they are.

It is also common to conceive of rational choice theories as existing in two forms: (1) as a formal calculus, which is, indeed, analytic; and (2) as an interpreted theory, which is empirical. It is even more common to make a distinction between rational choice as a *normative* theory about rational or "correct" choice or action, and as a *positive* theory about human action (see Harsanyi 1977:16, Elster 1986:2). In the normative version, rational choice theory defines rationality in various situations. In the positive version, it assumes that human beings actually make rational choices, at least in certain situations.

If we believe that social science is supposed to say anything about reality, it is the positive version of rational choice that is important. If, in addition, we believe that the main task of social science is *explanation*, Milton Friedman's methodology of positive economics is inappropriate, since it aims at prediction, rather than explanation. If, in other words, rational choice is intended to be an empirical, and explanatory, theory about social phenomena, it must have some empirical content and explanatory power. This raises the much-discussed issue of the relation between theory and reality, or the realism of rational choice theory.

The most common interpretation, since the days of John Stuart Mill, has been that rational choice theory, or economics, is an inexact science, based on assumptions that are not realistic but simplified, not true but approximations to the truth. The relation between theory and reality is one of applicability, rather than truth. Rational choice theories are more or less applicable to different social situations. Until about the 1950s, economists used to believe that rational choice theories are applicable to the market but not to other social phenomena. With the rise of game theory and economic imperialism, rational choice has been applied to all sorts of social phenomena. It is still arguable, however, that rational choice is best applied to the market, since people's motives tend to be more mixed in other institutional settings.

It is a matter of controversy how realistic rational choice theories are, but even more controversial is the question how realistic they should be. All scientific theories and models are abstract and simplified representations of reality. Are not abstraction and simplicity methodological virtues, rather than vices? There is no simple answer to this question, but it is difficult to see how rational choice can provide explanations of social phenomena, unless they are fairly good approximations to the truth. A common approach to this problem is the method of "decreasing abstraction" (Lindenberg 1992): to start with very abstract and simplified models and, then, to approach reality in a succession of increasingly realistic models. This method may, but need not, use a consistent rational choice approach. It stays within the bounds of rational choice, as long as it is based wholly on the principle of rationality, but goes beyond it if it introduces other principles of behavior.

A similar use of rational choice was suggested by Max Weber, who saw it mainly as a heuristic device for arriving at "true," or more adequate, explanations of social phenomena. In this case rational choice itself does not provide an explanation, at least not a full explanation, of the phenomenon to which it is

applied. A full explanation of most social phenomena requires the introduction of nonrational motives, such as (moral) values, affections, and traditions. For Weber, human action is usually the result of mixed motives.

By way of conclusion, I suggest that rational choice is more or less applicable to different social situations. The extent to which it is applicable to a certain social situation is an empirical matter. There are strong reasons to believe, for instance, that rational choice is less applicable to politics than to the market. If so, there are limits to rational choice, at least as an explanatory theory of human action. As a heuristic device, it may still be applied universally.

But rational choice seems to be insufficient also in another way. In most cases, the situation in which individuals act includes social institutions and social structures, not reducible to the choices of individuals. In these cases, rational choice analysis is not enough, but has to be supplemented with institutionalism and structuralism in order to be fruitful. In these cases, also, rational choice is no longer a manifestation of strict methodological individualism, but of methodological hybrids, like institutional and structural individualism.

Notes

1   Mill's analysis in *An Essay on Government* has almost everything in common with contemporary public choice theory and, like the latter, it was accused of lacking empirical content. The accusation came from the historian T. B. Macaulay (1829) in a famous review, which has lost nothing of its topicality.

2   The term *ceteris paribus* was used by economists before Mill, but apparently not by Mill himself. It was Mill's follower John E. Cairns, who made it popular, by using it in his influential *The Character and Logical Method of Political Economy* (1875:63).

3   The "marginalist revolution" refers to the switch from explaining market behavior in terms of the total utility of an economic good to explaining it in terms of the utility of an additional unit of that good. To take a simple example: the first piece of cake gives you a large amount of pleasure, the second some pleasure, and the third very little or none at all. This is the law of diminishing marginal utility, which is at the basis of the marginalist revolution.

4   Weber's view is similar to the "principle of charity" defended by Jon Elster (1979:116f.).

5   An exception was the Austrian economist Fritz Machlup, who rejected the empiricist interpretations of economics. According to him (in an article first published in 1954 and reprinted in his 1978 book), the basic assumptions of economics are ideal types and not independently testable (Machlup 1978:ch. 5).

6   There are obvious problems also with the interpretation of Friedman as a methodological instrumentalist (see, e.g., Mäki 1989:184–7, Hausman 1992:162–9). One problem is that Friedman suggests that assumptions should be "sufficiently good approximations for the purpose in hand" (Friedman 1953:15). According to instrumentalism, there is no need for assumptions to be even approximately true.

7   Popper rejected logical positivism mainly on the ground that verification of universal laws is impossible. No finite number of confirming instances can ever establish a

universal law to be true. There is always the possibility of a disconfirming instance. In sharp contrast to this, it is enough with one disconfirming instance to falsify a universal law. Therefore, according to Popper, try the best you can to falsify your scientific hypotheses.

8 It should be mentioned, though, that the explanations made by members of the Chicago School sometimes turn into functionalism and a breach with methodological individualism. The emergence and survival of social institutions are explained in terms of their rationality, or functionality for society, as a whole, but without an account of the rational choices of individuals, which are the ultimate causes of all social phenomena. The work of Richard Posner is replete with examples of this perversion of rational choice theory. See, for example, his article "A theory of primitive society, with special reference to law" (1980:5), where he explicitly renounces rational choice and endorses functionalism.

9 I use here the noninclusive language (e.g., "economic man," "political man") that was current at the time and at the very center of the arguments made at the time I am writing about.

10 See, however, Buchanan (1989), where he denies that the assumption of self-interest is a necessary element in rational choice models. In this article, it is suggested that rational choice models have the following three elements: methodological individualism, utility maximization, and constraints, including those posed by social institutions.

11 This hypothesis holds only if voters' preferences are evenly distributed, or concentrated at the center, not if there is strong ideological polarization.

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Mathematical Modeling in the Social Sciences

Paul Humphreys

Mathematical modeling in the social sciences uses a large, heterogeneous, and occasionally effective set of techniques. From the philosophical point of view, we are interested in the following kinds of questions: how do modeling methods in the social sciences differ from those used in the physical or the biological sciences? Are there unifying methods that can be used across the different social sciences? How much subject matter-specific knowledge is required to construct and to use the models? Can the models be constructed from empirical data alone or is substantive theoretical input required? Just how successful can mathematical modeling in the social sciences be? This chapter will address most of these questions but it must be emphasized that there are no quick and easy answers. Indeed, sweeping generalizations in this area are premature at best, and usually seriously misleading. Progress in understanding the circumstances in which mathematical modeling is helpful, where its limits lie, and how new methods of modeling are changing the way in which social science can be conducted, will be achieved only by close attention to the potential and deficiencies of specific methods.

The general theme that will be developed is this: the degree of disunity and context specificity involved in social science modeling is more than is involved in the physical sciences, but is not so great as to preclude interesting generalizations about modeling methods. One thing that models show us is that identical representations are possible across disciplinary boundaries and that much can be learned by attention to methods used in other disciplines. For example, game-theoretic models can be applied both to biological systems and to economic systems, models of disease epidemics can be used to treat the diffusion of ideas, and there are intriguing similarities between predator–prey models in ecology and models of arms build-up in peacetime.[1] This raises interesting questions about disciplinary organization – should the sciences be grouped according to subject matter, as has been traditional, or should they instead be divided up by which modeling methods are successful? Ontologically, we have a natural preference for the former mode of organization, but methodologically the latter, newer, mode has greater promise.

Why Use Mathematical Models?

Many social scientists are resistant to the use of mathematical models. Some of these objections are reasonable, many are not. An important line divides those who treat models as merely the basis of more or less successful predictions, and for whom the correspondence with reality of these models is of little concern, and those who view models as exposing the underlying structure of the system being modeled. A well-known polemic in favor of the first approach is Milton Friedman's (1953) and some of the Cowles Commission work in the 1950s in econometrics seems to have been conducted under this approach (see, e.g., Koopmans 1950, Hood and Koopmans 1953). In contrast, many sociological models of status structures, psycholinguistic models of embedded phrase structure processing, and anthropological models of kinship relations and gift economies, to cite just a few examples, are explicitly intended to model reality. For the time being, we can set aside these differences in motivation and look at the common features possessed by such models.

Frequently, the objections to modeling social phenomena arise from modeling activities that have not been carried out in an intelligent and cautious manner. Mathematical models can appear sophisticated when they are merely sophistical; they can produce the illusion of knowledge in situations where none is to be had; they can produce gilded absurdities; they possess the ever-present danger that mechanical computations will replace intelligent inference; and many of the assumptions behind the models either fail to be satisfied in practice, or only the flimsiest of justifications can be given for them Yet, against the background of these cautions, some other reasons that have been given for being wary of modeling are less than persuasive.

For example, a common reaction to modeling is to insist that using mathematical representations omits something that is essential to the social or human phenomenon under consideration. This outlook motivates the use of *Verstehen* (empathic understanding) in sociology and anthropology; it underlies the use of simulation theory in psychology, where projecting oneself into the subject's role replaces a theoretical analysis of the activity; and it supports appeals to the intrinsic quality of experience – the subjective "feel" of experiences – as a reason to consider any objective study of humans to be essentially incomplete. In the *Verstehen* approach, a proper understanding of certain anthropological and sociological phenomena is said to be achievable only by embedding oneself in the society and experiencing the phenomena from the perspective of the society at hand. In the simulation approach, it is said that a sympathetic projection of oneself into the mental status of the individuals whose behavior needs to be understood is the preferred basis from which a proper insight into their state of mind can be had. In the last, subjectivist, approach formal representations are considered to omit the qualitative content of human experience without which one cannot fully appreciate, or appreciate at all, the essential quality of our inner

lives. No mathematical theory of humor, it is claimed, could ever convey the experience of a good guffaw.

There is no doubt that each of these nonformal approaches provides us with information about human experiences. But this is no reason to deny oneself the different kind of information that can come from mathematical models. Models of the transmission and development of protolanguages can show us connections between languages that have hitherto been considered distinct, without the need for us to speak those languages or to have been a part of the historical societies that did. It is becoming possible to understand how schizophrenia is inherited and how the genetic causes interact with developmental factors (and, perhaps, infectious agents) without fully being able to project oneself into, let alone experience first-hand, the mental states of someone suffering from the disorder. Although it is true that ethnic segregation frequently involves deep emotions, there are also powerful models that explain how such segregation can arise given only simple interactions between individuals, and these models do not require the user to either profess or to experience ethnic hostilities in order for them to be effectively used.

Abstractions are sometimes necessary because moving away from the concrete is required to capture changes that have taken place in society. The concept of money in economics, for example, has become very abstract indeed, and any medium of exchange, be it metallic, electronic, or paper can be considered to fall under that general, functionally defined, concept.[2] A different consideration in favor of abstraction is that it is the very abstractness of mathematical models, their immediate lack of content, that makes them so useful. It is precisely because models of dynamic equilibrium can be applied to economic systems as well as to populations of predators and prey that insights from one area of science, and mathematical techniques developed to refine those insights, can be transferred to areas of the social sciences that are apparently remote from the original applications. This ability to see mathematical identities between different subject matters in virtue of structural similarities at a more abstract level is an indispensable modeling skill and the scarcity of social scientists who have been trained in other fields is a serious barrier to the development of these skills.

A second common objection to mathematical modeling rests on the grounds that the phenomena to be modeled are too complex to be adequately represented, that they are subject to too many perturbing influences to be systematically treated, and that they are too transient to bear the weight of serious theory. Of these considerations, the second and the third have the greater importance. Complexity by itself is not a problem, for the physical sciences have successfully modeled enormously complex phenomena using abstraction and simplification to reduce the number of variables to a manageable number. The already sparse nature of some of those physical systems, the presence of dominant influences swamping smaller effects, and the separability of various influences, has resulted in quite accurate physical models being developed on the basic of a small set of mechanisms. Sparse systems with dominant, separable influences are, unfortunately, uncommon in the social sciences outside the laboratory.

Where available, laboratory results are important – there should be more of them – and interesting results exist in experimental economics, in small group theory in sociology, in perception, in the area of cognitive heuristics and fallacies, to name only a few that are experimentally based. Such experimental contexts are amenable to modeling because dominance, sparseness, and separability have been artificially imposed. But with experimental results the burden is always to demonstrate that what holds in the laboratory holds in the wider world and it is in the transition from the controlled and known environment of the laboratory to the uncontrolled and often unknown environments of the everyday social world that the assumptions behind the initial models are often violated or simply not known to hold.[3] Modeling and experimentation are thus similar in that they both restrict themselves to simplified versions of the everyday world and models can be accurate representations of experimental set-ups even when they are defective as models of naturally occurring social systems. What is crucial in social modeling (and, ordinarily, in experimentation) is, first, to choose the variables appropriately so that the dominant influences are included in the model and, secondly, to ensure that those influences are robust in the sense that they are invariant across many different contexts. These choices require considerable skill and insight into the specific subject matter. To have both desiderata hold is quite rare in the social sciences, and often simply capturing the gross qualitative structure of a phenomenon can count as a success.

Certain generic features of social science systems tend to influence the form taken by mathematical models in this domain and these features make effective modeling more difficult than in the natural sciences. Most social systems are open rather than closed. That is, they are subject to influences from outside the system, these influences being either not included in the model or, if included, unpredictable. Familiar examples of such influences are the so-called "error terms" of causal models, usually representing the aggregate effect of all causal influences on the system that have not been explicitly included, and random shock models of economic cycles.[4]

When dealing with open systems, the chief danger is confounding, where a confounding variable is a variable that is associated with the explicitly considered variables in such a way that associations are mistaken for causal connections. For example, a variable that is not included in the model because we are unaware of its existence, or have wrongly concluded that it is irrelevant, can be the common cause of two variables that are included in the model, thus producing an association between them. Without the omitted common cause, this association can easily be taken to show a direct causal connection between the two included variables.[5]

Prediction, even probabilistic prediction, over the long run in open systems is difficult unless the system structure is sufficiently stable to ground a fixed functional form relating inputs to outputs. Many social systems are subject to the effects of a very large number of relevant variables, most of which are unknown or unmeasured, unlike the textbook examples of physics, within which the state

variables are few, known, and measurable. A system's qualities of being open or closed, on the one hand, and being multivariate or not on the other hand, are logically independent, in that there are both closed and open systems with multiple relevant variables, and both open and closed systems with few variables. Nevertheless, within the social sciences, having an open system usually means dealing with large numbers of variables, even if most of them are lumped into a generic error term. In addition, social science systems are rarely deterministic; the concepts that are used to represent the system are either not sharply defined or involve abstract properties that do not correspond in any obvious way to familiar features of known systems; the models can rarely be tested with empirical data coming from systems that are known to satisfy the model's assumptions; and the data that are used to evaluate the models are often irreproducible, of dubious quality, and far too few to produce much detail.

A lack of determinism in models can arise from dealing with fundamentally indeterministic phenomena – those where there is an irreducible element of chance involved – or it can be the result of underlying deterministic phenomena being subject to stochastic inputs and in consequence being treated as if they were indeterministic. The former case should not be taken too seriously at present in the social realm, if only because the current state of knowledge in this area cannot support the kinds of arguments for irreducible indeterminism that have been constructed for certain quantum phenomena. Stochastic models are the vehicle of choice for the latter case, with chaotic models showing some promise for selected phenomena.

Yet all of these difficulties have beset modeling projects in the natural sciences and they have not prevented highly successful mathematical models from being developed. What is often required to circumvent the difficulties is to reconcile ourselves to a fact that many find profoundly unappealing: the fact that almost any kind of modeling requires us to simplify the real system, usually severely, in order to represent it. It is this aversion to simplification and abstraction that lies behind most criticisms of social modeling. Because humans are familiar with our own, often complex, motives, many of us reject the very idea that our actions can result from quite simple mechanisms or interactions or that important features of social structure can be modeled by such mechanisms. Yet the behavior of soccer thugs, of status unequals participating in juries, of audience members joining a standing ovation, of participants in Yankee auctions, of subjects perceiving regularities in patterned wallpaper, and of many other actors, can be described and sometimes predicted quite well with surprisingly simple mathematical models.

To address some other concerns: there is no doubt that the frequent use of radical simplifications such as the linearity assumptions that are often used in social models is almost entirely due to mathematical convenience. In addition, many social variables have interactive effects and causal influences are often not simply additive. Yet nonlinearities and interactive effects in the models can now be addressed through computational methods rather than by using traditional analytic techniques, and with the widespread availability of inexpensive computer

power, purely computational models of social phenomena are emerging as a powerful supplement to more traditional modeling methods. The extent to which this development will transform the mathematical modeling of all the sciences cannot be overestimated. It is as least as great a methodological advance as was the introduction of the calculus and the development of statistical methods.

Models must conform to the available data. The problem with many areas of social science is that there are both too many and too few data. Too many – because the number of data points that can be, and sometimes are, collected about an economic system during the census, for example, is enormous; too few – because the available data for any specific system are generally of a limited type and either are not gathered with a particular research goal in mind or are very expensive to obtain. Yet by itself, the quantity of data available is not the problem. Some of the physical sciences can construct statistical models about populations of a size vastly greater than any in the domain of the social sciences. Even if data were gathered on every individual in the United States, the number of humans involved would still be of the order of 10^{-15} fewer than the number of molecules in a gas. The problem is that social science data are frequently contaminated: respondents are not always truthful, subjects drop out of experiments, and surveys are either incomplete or oversampled – not all selected participants could be contacted, they refused to participate either wholly or in part, or they submitted more than one response. It is also an unavoidable fact that human populations can be affected by outside influences in a way that is far more complex than a gas in thermal disequilibrium with its environment.[6] Human populations are rarely homogeneous in the way that gases are, thus making generalizations hazardous, and social regularities frequently do not persist for extended periods of time.

Cumulatively, this would all seem to suggest that a great deal of caution is needed in using and interpreting the results of modeling in the social sciences and that is indeed one of the main morals of this chapter. But there have been successes too. We can divide modeling approaches into three primary categories: theory-based models, data-based models, and computational models. The last category partially overlaps each of the first two but it has importantly different methods to contribute in some cases.

Theory-based Models

The process of constructing theory-based models can be illustrated by appeal to an example.[7] This example, although neither excessively simple nor particularly sophisticated, requires consideration of many of the special issues faced by modelers in the social sciences. Consider the problem of recidivism among criminals. We want to know the probability that an individual, convicted of at least one crime, will commit another. The first task is to stipulatively fix what constitutes recidivism. Because different jurisdictions define recidivism differently, there is no commonly

agreed upon definition for the term. Correctional systems are not interested in those who, although arrested and convicted, are not sent to jail and subsequently commit further crimes, whereas the police, naturally, are concerned with such people. The general public, in turn, tends primarily to be concerned with repeated crimes, irrespective of whether the perpetrator is arrested or imprisoned.[8] This is the first decision point for modelers, determining appropriate definitions and measurement criteria for the variables used in the model. One common definition is to consider as a recidivist any individual who, once having been incarcerated after committing a crime, is arrested again after release. This gives a definition that includes some, but not all, of the interests mentioned above. Modelers have to accept that such stipulative definitions will often be controversial: many definitions are forced upon us by operational considerations such as the availability of systematic data on a class of individuals. Whatever the reason, it is better to be clear and contentious than to equivocate. There is nothing inappropriate about giving a stipulative definition of recidivism, but such definitions are unlikely to be universally adopted; most areas of the social sciences, with the exception of psychiatry, have wisely refrained from setting up National Bureaus of Standards.

Whereas such disagreements over definitions tend to be obvious in the social sciences, the difficulty of agreeing upon the appropriate definition of core concepts is often hidden by a longer history and conventional agreement in the natural sciences. For example, it might seem that temperature is a natural variable that can be measured with sufficient ease that no controversy could arise. Yet there was for decades a vigorous controversy about which theoretical definition of temperature to adopt, and once that was settled there remained in use two quite distinct concepts of temperature – the theoretical thermodynamical concept based upon a reversible Carnot cycle on the one hand and a multitude of practical temperature scales on the other, each practical scale being based upon a different material substance, and each differing subtly in its value from the theoretical concept.[9]

Returning to the model, let the population consist of criminals, individuals who have committed at least one crime. We suppose that there are four possible states such an individual can be in: s_1: never commits another crime and is not further involved with the justice system; s_2: has committed a crime in the given time step; s_3: has been arrested in the given time step; s_4: is incarcerated in the given time step. Transitions from one time step to another can occur between any states, including from a state to itself, with these exceptions: once an individual is in s_1, he or she remains there; no individual can go straight from s_2 to s_4, nor straight from s_4 to either s_3 or s_4; and to model the situation that there cannot be back-to-back arrests without an intervening crime, an individual cannot go straight from s_3 to s_3, an assumption that excludes false arrests. Some decision must be made about the appropriate length of the time step; the choice here is to set it so that consecutive arrests cannot occur within a given time step.

With these states it is possible to model recidivism by a Markov process with one absorbing end-state. The state s_1 is considered as absorbing because, once

entered, it is never left. A very strong assumption that the probability of transition from state s_i to state s_j (where i may equal j) is independent of the previous history of the individual gives us the Markov property, and we also assume that these transition probabilities are constant over the career of the criminal. It is evident that neither of these assumptions is plausible in general, and consequently the resulting model should eventually be refined to include non-Markovian and nonstationary transition probabilities. In allowing us to see explicitly that these assumptions are implausible, the construction process has the invaluable virtue of forcing such assumptions into the open and it is clear in exactly what way the model needs to be refined. Of course, making the process non-Markovian will greatly complicate the mathematics. Here we see the common trade-off between accuracy and mathematical tractability in models. All models simplify or distort reality but one of the primary advantages of theory-based modeling is that these simplifications and distortions are, if the modeler is careful, made explicit and it is often clear what modifications will later be needed to improve the model.

Let p_R be the probability of ever committing another crime, given previous occupancy of state s_2, or of state s_3, or of state s_4. That is, the probability of committing another crime, given a previous crime, is taken to have the value of the probability of committing another crime given a previous arrest, and also to the probability of another crime given a previous incarceration. These probabilities are identified with the probability of recidivism, which is now taken in a more general fashion than in some of the views discussed earlier. p_R is an unknown parameter of the model, assumed to be constant across time and individuals. It is thus assumed to be independent of the individual's arrest and conviction history. None of these assumptions is particularly plausible, but their adoption greatly simplifies the model.

Let $p_A = P$ (arrested/crime committed). This is also assumed to be a constant and hence unaffected by periodic law enforcement initiatives. The value of p_A can be estimated from police data. The probability of at least one new crime being committed in the next time step is $(1 - p_A)p_R$.

Let $p_I = P$ (incarcerated/arrested). This is assumed to be a constant and can also be empirically estimated. The probability of at least one new crime being committed, given an arrest, is $(1 - p_I)p_R$. From these probabilities a transition probability matrix can be constructed[10] from which it is possible to predict such phenomena as the probabilities of re-arrest and of re-incarceration, as well as the average number of crimes committed in a criminal career and the effects on that average of altering the probability of recidivism. On the basis of this transparent model, some unexpected predictions can be made, for example that a 0.1 decrease in p_R can result in a 50 percent decrease in the average number of crimes committed.[11] It is here that the power of mathematical models is most clearly exhibited in allowing us to escape the falsehoods and commonplaces of common sense. The difficult task, and one where modeling can play a significant role, is in seeing whether these surprising consequences hold when the artificial assumptions noted earlier are made more realistic.

One of the traditional disputes in the philosophy of the social sciences has concerned the existence of laws. Some, such as Donald Black (1997) have argued that not only does sociology have autonomous laws, but that it is only with such laws that sociology can resist reduction to other sciences. Others, such as John Searle (1984), have suggested that because of the multiple realizability of social properties, no nontrivial generalizations can be found and *a fortiori* there are no social laws. This desire for laws is understandable, but if we switch our attention to models, the need for laws to underpin understanding lessens. What is required instead is a good grip on the mechanisms involved in producing behavior of various kinds and the assumptions that went into the construction of the models. Although traditionally model building in the natural sciences has presupposed that laws lie behind models, there is in fact no need for such law-like generality, for the essence of model building is tailoring the model to the specific system being modeled. The difficulty is to do the tailoring without falling into ad hoc "curve fitting."

I have, somewhat simplistically, entitled this section "theory-based models." To say that a model is theory-based is not to require that it be based on a grand, universal, theory of the kind employed by rational choice theory or generative grammar. Most models, such as the one just developed, are composed of specific, system-dependent, assumptions, each of which is separately justified. Sometimes there is an explicit theory behind the justifications but this is not necessary. Informed "common sense," a healthy sense of the absurd, and practitioner's knowledge acquired through exposure to a wide variety of applications, are often used to construct models and will help protect many models against disaster. Moreover, even grand theories require supplementation by system-specific specifications of parameter values, boundary conditions, and the justification of idealizations and approximations.

What these theory-based models give us are *computational templates*, model schemas that have crucial components such as preference orderings, status rankings, degrees of alienation, and probabilities of re-arrest that are left unspecified. These must be filled in by system-dependent specifications, sometimes on the basis of more specialized theory, sometimes by direct measurement, and sometimes by indirect inference.

It is tempting to construe such computational templates as representing the abstract structure of social systems, rather than capturing the intrinsic content of specific systems. This is because the very same template can be used to model a wide variety of systems, the intrinsic nature of which is very different. For example, the Markov chain models just described can be used to model the social mobility of generations across classes, occupational mobility, demographic changes, stock price movements, market share, and many other applications. While this is of course true, the situation is not straightforward. Most textbook mathematical models are merely general strategies for approaching the system; they must be supplemented with subject-specific stratagems to model the details of the system at hand. That is, the computational templates are brought into contact with real systems by employing system-specific knowledge to justify the modification of

assumptions, the relaxation of idealizations, the estimation of parameter values, and so on. It is this attention to capturing the fine structure of a system where the art of modeling is most needed but is least studied. Most mathematical models in the social sciences are of the "off-the-shelf" type, stock representations that are applied to systems the structure of which is familiar enough that the model can be straightforwardly applied without modification or much thought. Yet thinking of these stock models, which figure large in textbook examples and much philosophical discussion, as simply pieces of formalism can be misleading. For all models, even these stock models, are constructed, and the construction process plays an important role in justifying the use and modification of that model. The formalism comes in each case with an interpretation and justification embedded in it and any modification of the template requires us to explicitly address that interpretation (e.g., via the definition of recidivism we adopted) and the justifications that were used to initially construct the model.

When it comes to testing models such as the one described above, one is faced with a problem common to many social contexts – incomplete and erroneous data. For example, if a criminal is arrested in a jurisdiction other than the one being studied the arrest often will not appear in the data. Other difficulties include false arrests, plea bargains, cases dropped because of insufficient evidence, and decisions not to prosecute certain crimes. Furthermore, although erroneous and missing data are not unknown in the natural sciences, in any circumstance where data are collected in a nonexperimental context, there is no possibility of replicating the data unless one has a guarantee that the system structure has remained unchanged, a guarantee that is hard to acquire in most social contexts.

The feelings of inadequacy that sometimes accompany a comparison between models in the social sciences and those employed elsewhere should thus be lessened by a healthy sense of what are the appropriate goals for modeling in a given area. The goal for open, multivariate, stochastic systems ought not to be one analogous to computing energy levels in atoms to six significant figures, nor to piloting a lunar explorer to within a few feet of a predetermined landing site. It can be a significant accomplishment to have predicted or to have explained a few salient but central structural features of a system and this can be achieved for open, multivariate, stochastic physical systems. For example, high-rise buildings can be demolished so that their collapse is highly predictable and can be confined to a tightly demarcated area – this is done not by calculating the minutiae of every brick, but by relying on simple laws about detonating charges, structural stability, and gravity. The need to begin with lower expectation has been voiced by some authors in the field:

> If a model can be constructed which reproduces the main qualitative features of the real system, the social scientist may be well satisfied . . . The underlying aim [of stochastic modeling] is to render the complex patterns exhibited by real social systems intelligible by showing how far they are explicable in terms of a few simple postulates about individual behavior. (Bartholomew 1982:2)

> There may be many micro specifications that will do as well – the mapping from microrules to macrostructure could be *many to one*. In the social sciences, that would be an embarrassment of riches; in many areas, *any to one* would be an advance. (Epstein and Axtell 1996:20)

But a wider recognition of the limits of modeling would be welcome.

Data-based Modeling

Data-based models rely on a "bottom up" approach, usually constructing relations between the variables of interest by using a variety of statistical or computational methods on data collected in either experimental or nonexperimental contexts. Most of the emphasis has been on methods that are useful in nonexperimental contexts where randomized trials are not possible for ethical or practical reasons, for these contexts provide the most challenging difficulties of inferring stable relationships between variables. To illustrate the techniques and difficulties, we can describe a set of widely used techniques in the social sciences, with applications ranging from economics to sociology, that falls under the generic heading of "causal models." In comparison with the models we have already discussed, these have a strange form, for whatever subject-specific content there may be in any given application of these techniques is hidden well below the surface of the directed graphs that form the standard representational apparatus, as they purport to provide a general, subject-independent, set of methods.

The early history of investigation into these causal models is typified by the work of Sewall Wright (1960), Herbert Simon (1954), and Hubert Blalock (1971). Almost all of these methods are designed to infer causal relationships from data gathered in nonexperimental contexts without the benefit of a randomized design. The principal goal of these methods is to replace simple regression techniques that attempt to represent causal relationships between a single dependent variable and a number of independent variables with more sophisticated techniques that can represent causal relations between variables that are embedded in a network of causal relationships.[12] The methods go under various titles, such as structural equation models and path analysis, but I shall here ignore the relatively minor differences between these techniques. The results from these methods are usually represented by graphs in which the nodes represent the variables of interest and the lines between the nodes represent relationships, usually linear, of strengths that are represented by the coefficients attached to the link.

Although these methods can be helpful in exploring the data, the hope sometimes expressed that they can be used with minimal appeal to theory or – which is not the same – without using substantive, subject-specific knowledge, is unrealistic. Why is this? Because the often very strong assumptions behind the

statistical methods need to be justified; because decisions must be made about the appropriate level of conceptualization to be used; because the vast number of logically possible relations between variables must be pruned before data analysis can begin; and because inferences made from simulated data need to be checked for plausibility against existing knowledge. None of these tasks can sensibly be addressed without employing substantive background information about the subject area in question. Although it has been claimed that these causal models can be evaluated on the basis of empirical data alone, there must ordinarily be some theory behind the model that is being investigated. In addition to providing a causal ordering between the variables, background theory can help to eliminate scientifically dubious relations and to serve as a check on the validity of conclusions drawn by the models. For example, it is almost always assumed that for any dependent variable the various causal influences on it that have been omitted from the model can be represented by an "error term," and these error terms are usually assumed either to be both statistically independent of one another and of the remaining independent variables occurring in the relevant equation, or to be uncorrelated. Such strong assumptions can rarely be justified in the absence of a randomized design without appeal to substantive scientific knowledge. Moreover, theory, or at least thought, should enter into the choice of variables. An inappropriate choice of variables can lead to aggregation errors, as in the ecological fallacy,[13] or an attribution of causation to generic variables, such as social class, that are simply aggregated indicators of multiple underlying traits that constitute the true causal variables.

Some of the issues involving causal models are similar to those we have discussed in connection with theory-based models. For example, one of the key considerations with these models is the idea of identifiability. For a parameter in a system of equations to be identifiable, there must be sufficient data to estimate that parameter uniquely. With insufficient data, there will be more than one model that is compatible with the data, and choosing the wrong model will result in incorrect causal relationships being inferred. Other assumptions that frequently must be made in these models are linear relationships between variables and perfect measurement of the variables, neither of which is ordinarily realistic. Often, appeals to either background knowledge or time-ordering are needed to ground the asymmetries between variables needed for recursive models (see note 12).

A second major question is how to interpret the causal claims made by these models. The models themselves postulate deterministic relationships between the variables included in the model, with the error terms introducing stochastic inputs. This requires that we attribute some form of probabilistic causality to the system, rather than the more traditional accounts in terms of either necessary or sufficient conditions. A systematic treatment of the connections between philosophical theories of probabilistic causality and these causal models can be found in Humphreys (1989). A related causal interpretation of these models involves the idea that the coefficients of the models (the "structural coefficients") indicate

how much a dependent variable will change when its cause is changed by one unit. The fact that there is a relationship between two variables X and Y which is such that, in the circumstances generating the data, a unit level of X is sufficient to sustain a value of bX in the second variable Y, is insufficient to support the inference that when an intervention is made on the system to alter the value of the X by ΔX, that Y will change by bΔX units. This difference is important for policy decisions because the intervention may upset the equilibrium that led to the original relationship between X and Y or induce effects on Y by other causal routes. There is a still emerging literature on the role of counterfactual reasoning in such models. Classic sources are Rubin (1974) and Holland (1988), although the idea goes back at least to Neyman (1923). Unfortunately, the highly stylized possible worlds approach to counterfactuals, such as is found in Lewis (1986), valuable though it might be for the formal semantics of causal talk, is of little practical use in these circumstances.

A more recent development has been the invention of computer-assisted methods for testing databases for dependency relations. Among the best-known of these are LISREL and the work of Judea Pearl (e.g., Pearl 1988, 2000). Although some of these new approaches are interesting, inflated claims have been made for what automated search procedures can achieve, especially the degree to which the search procedures can be successful without appealing to background theory. The sense in which the models are supposed to be causal is also often less than transparent. An extensive discussion of the merits and deficiencies of such approaches can be found in McKim and Turner (1997); see also Humphreys and Freedman (1996), Robins and Wasserman (1999), Freedman and Humphreys (2000).

Computational Approaches

One of the most serious problems facing mathematical modelers in the social sciences – the mathematical complexity of the models – can now be at least partially addressed through the use of computer-assisted modeling and simulation. Before modeling social systems computationally, a decision must be made, whether explicitly or implicitly, about whether to adopt methodological individualism or to reject it. Although what counts as methodological individualism is notoriously vague, it standardly involves the position that all social phenomena can be accounted for in terms of the intrinsic properties of individuals. In particular, social relations between individuals must either be reinterpreted in terms of intrinsic properties of those individuals, together with permissible physical relations such as spatial relationships or, in more recent treatments, they must be shown to supervene upon intrinsic properties of individuals. To claim that relations supervene upon intrinsic properties of individuals is to say that once the properties of the individuals have been fixed, the relations between them are

automatically fixed as well. To put it another way, it is impossible to have two identical groups of N individuals with different N-ary social relations holding between them.[14] Hence, the existence of autonomous social facts is denied in such individualistic approaches. As a result, there is no causal influence from the group level to the individual level. In contrast, models that allow for real social relations – or as I prefer, models within which emergent phenomena appear – usually include some kind of downwards causation from a social structure to the individuals operating within that structure.

An increasingly important type of contemporary modeling process is explicitly built upon individualism – what are usually called agent-based models. Such models in the social sciences are descendants of the cellular automata models developed by von Neumann and Ulam[15] and are usually considered to be a special case of complex adaptive systems. Models developed along these lines can be found in Thomas Schelling's early work on segregation (Schelling 1971) and in the more recent treatments of prisoner's dilemma situations by Robert Axelrod (Axelrod 1997), the Swarm modeling programs,[16] and in Kohler and Gumerman (1999). It is a central feature of such models that only local influences on individuals are allowed, these influences being governed by simple rules. Agent-based models can overcome one of the primary difficulties that we saw affected traditional theory-based modeling methods – the fact that the agents are operating within an environment that is constantly changing, and that an agent's actions are reciprocally affected by the choices made by other agents. In addition, in complex adaptive systems, agents are subject to learning or mutation and selection. One key advantage of agent-based models is that they can iteratively compute the state of the system at each time step in the computation and accommodate such dynamic changes, and this can challenge fundamental assumptions of a given field. For example, in Epstein and Axtell's (1996) Sugarscape models, it is possible to model changing preference orderings for the agents, and this leads to a significant degree of disequilibrium in the system as compared with the equilibria that generally result from the fixed preferences of traditional economic models. Most traditional models consider the individuals to be homogeneous; each individual in the population is considered to have the same basic properties as any other. This constraint need not be imposed upon the more flexible, individualistic, agent-based models. Within modern societies, agents are often quite diverse and the constraints and influences on the social systems are changing quite rapidly. An example occurs using business models where computer companies are the agents. The business environment in such an industry is fluid and highly dynamic, with changing expectations on the part of consumers, evolving standards, the constant appearance and disappearance of competitors, internal changes in the companies themselves, and so on. To effectively model such systems requires an essentially dynamic approach.

This use of computers in modeling social systems is very different from their use in traditional computer simulations. In those, an explicit mathematical model, such as the Markov model for recidivism we considered earlier, is constructed

before the numerical simulation begins and the computational power is brought to bear on producing solutions from the equations that are part of the model. Simulations of that kind are common in economics and allow predictions to be drawn out from complex models in a way that would be impossible without the computer. Within agent-based models, in contrast, there is no overarching model of the social system – phenomena often emerge at the macro level that are the result of multiple interactions at the micro level, these macrophenomena being unforeseen when the individual level analysis is begun. This type of approach is, however, not new. Almost all statistical or probabilistic models produce aggregate behavior at the population level that is distinctively different from what happens at the individual level. We need look no further than results such as the laws of large numbers in probability theory to see that stable and predictable phenomena can result at the population level from heterogeneous and unpredictable behavior at the individual level. What is new is the use of computational experiments to overcome the inability to explicitly deduce many of the consequences of the model. That is, rather than analytically deriving predictions from the mathematical model, it is only by representing the elements of the model in computer code and running the programs on a real machine that certain consequences can be known. This is a genuinely new method, although not one peculiar to agent-based modeling or to complexity theory, for such approaches have been used in cellular automata and solid state physics for many years.

It is the appearance of these higher level phenomena that makes methodological individualism a relatively uninteresting position. Ontologically appealing as it may be, it points us in the wrong direction regarding what kinds of information we can extract from the model. For it is often in virtue of examining a system at a level above that of the individual that emergent properties of the system become apparent that are not perceptible at the level of individual analysis.

In the Sugarscape simulations just mentioned, one of the more important questions about this type of modeling is the degree of understanding that is produced by the models. It is often claimed that rather than giving a realistic account of social phenomena, what these agent-based models can do is to provide us with clarification of, and insight into, the social phenomena they model, and that the primary goal is to reproduce broad qualitative features of the system from a few simple assumptions about the agents. This view is not much different from what many traditional modeling procedures have been doing and which we earlier urged as a modest goal for those older approaches.

Modesty notwithstanding, claims about insight and understanding must, once again, be taken with a fair degree of caution. A famous example of reproducing complex macrolevel phenomena using simple underlying rules was the system of epicycles and deferents in ancient Greek astronomy. This employed basic assumptions about the motions of individual planets (that their apparent motions were the result of compounded uniform circular motions), from which "emergent" behavior, such as the retrograde motions of the superior planets, arose from combinations ("interactions") of those more basic behaviors. It is clear that any

insights into the real structure of the planetary system the Ptolemaic system generated were completely illusory, and the example illustrates the dangers of confusing an efficient computational model with one that has genuine explanatory power. As Epstein and Axtell put it: "As social scientists we are presented with 'already emerged' collective phenomena, and we seek micro rules that can generate them" (1996:20). In fact, because the goal of such modeling procedures is to find a set of conditions that is sufficient to reproduce the observed data, rather than to isolate conditions that are necessary to achieve that result, a misplaced sense of understanding is always a danger. Agent-based models are a powerful addition to the armory of social scientists, but as with any black box computational procedures, the illusion of understanding is all too easy to generate.

The second caveat to keep in mind about such models is that they leave no room for an intelligent member of the population to recognize the emergent social features and, through a strategic intervention, to impose that structure from above. This inability to include such downwards causation is a direct consequence of the "bottom up" procedures used in agent-based models, but it also results from the desire to keep the modeling apparatus general enough to model biological as well as social processes, and no such "manager" would be present in the biological case. Perhaps it is also because the primary motivation is to show how order can emerge in the absence of some central organizing tendency. Whatever the case may be, an individualistic approach that omits the possibility of an astute leader engineering a transient structural advantage into a quasi-permanent one is leaving out something that is important to many social processes.[17] The moral here is that the employment of simple mechanisms is acceptable if there is good reason to hold that these "mechanisms" correspond to genuine features of the systems under investigation. Certainly, quite sophisticated physical models employ radically simplified physical mechanisms to construct descriptively accurate computational models. One example is the frequent use of Ising models in physics to describe ferromagnetism. Here a common simplification is that the interactions between particles on a lattice are restricted to nearest neighbor interactions, and the continuously variable orientation of the spins of those particles are reduced to binary "up" and "down" values. Despite these severe simplifications, such models have been successful at reproducing major features of ferromagnetism and other phenomena in solid state physics.

Conclusions

We have described and assessed in some detail three different approaches to mathematical modeling. These methods stand in sharp contrast to other methods that have been employed in the social sciences, such as narrative description and the use of grand theory. Although mathematical modeling will not give a

complete account of social phenomena, it is now an indispensable part of scientific method. Some general conclusions that we can draw from our assessments are the following. The traditional division between theoretically oriented approaches and empirically based approaches is not particularly illuminating in the context of modeling. Modeling involves a complex set of interactions between theory, background knowledge, idealizations, approximations, simplifications, and data. This is especially true in data-based approaches, where claims to provide models solely on the basis of empirical data are unsupported. Many of the modeling methods used in the social sciences are not peculiar to the area but provide formal unity across a number of disciplines. Thus in some cases there is nothing peculiar about the subject matter of the human sciences which requires modeling methods unique to the area. Yet subject-specific knowledge is ordinarily required in the construction of the models to justify their adoption. There is no incompatibility between this formal unity and the subject-specificity of justification.

The construction techniques used in modeling give both a built-in justification and interpretation to the models that cannot be removed without losing the rationale for adopting the models. So an excessively formalist approach to models, or a testing outlook that views them as akin to hypotheses to simply be rejected when at variance with data, or both incorrect. What the construction techniques give us is a guide as to how to modify the model when it, not unexpectedly, goes wrong. Modern computational methods provide access to emergent features of social systems; a dogged insistence on pure individualism is misguided. Social phenomena can be modeled using individuals as the basic units, but stable structures will often emerge at a higher level from the interactions between those individuals. Modeling the broad qualitative features of social systems often counts as a significant success – precise numerical predictions are a bonus.

Modeling in the social sciences requires modest goals and recognition of the special features of its subject matter. Nevertheless, much can be gained by intelligently borrowing existing formal models from other areas and reconstructing them so that they correctly apply to the social domain. Although the ratio of successful to unsuccessful models is currently quite low, the addition of computationally based models to the stock of methods holds out some promise of effectively dealing with the complexity of many social systems, and counts as one of the most promising additions to social science modeling in many years.

Notes

1   For details of the last, see Epstein (1997).
2   Within modern societies, this holds irrespective of which measure of money is adopted, be it M1, M2, or some other quantity.
3   This task is sometimes called establishing "external validity."
4   See Morgan (1990:ch. 3) for a description of the latter.

5   An excellent systematic and largely nonmathematical discussion of confounding can be found in Freedman et al. (1998:ch. 2).

6   The move from closed to open systems is the basis of the so-called "frame problem" in artificial intelligence – the problem being to incorporate knowledge about all contingencies into the data base and to provide efficient retrieval of the relevant knowledge when needed. The generic problem of *ceteris paribus* conditions is closely connected with open systems and with the frame problem.

7   This example is based on one presented in Beltrami (1993).

8   For details see Maltz (1984:ch. 6).

9   For details see Quinn (1983).

10 See Beltrami (1993:sec.1.4).

11 For details, see Blumstein and Larson (1971).

12 Usually the emphasis within causal models is on recursive structures; that is, models within which there are no causal loops. When the system of equations is recursive, then ordinary least squares techniques can be used to estimate the parameters of the model.

13 This occurs when an analysis of data at the individual level produces associations that have different signs, or values, than is given by an analyis at the group level. For a discussion of this see Robinson (1950).

14 In contrast, the spatial relations between identical pairs of individuals can obviously be different. In this sense, spatial relations are external. There is a close connection between denying social facts and asserting that all social relations are internal.

15 See Casti (1992). John Horton Conway's game of "Life" is also a direct descendent of cellular automata. models.

16 For references see http://www.swarm.org or http://www.santafe.edu

17 I am indebted to Tiha von Ghyzy for pointing out to me this aspect of agent-based modeling.

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The Practical Turn[1]

David G. Stern

What is Practice Theory? What is a Practice?

What is "practice theory"? The best short answer is that it is any theory that treats practice as a fundamental category, or takes practices as its point of departure. Naturally, this answer leads to further questions. What is meant by "practices" here? What is involved in taking practices as a point of departure or a fundamental category, and what does that commitment amount to? And what is the point of the contrast between a practice-based theory and one that starts elsewhere?

Perhaps the most significant point of agreement among those who have taken the practical turn is that it offers a way out of Procustean yet seemingly inescapable categories, such as subject and object, representation and represented, conceptual scheme and content, belief and desire, structure and action, rules and their application, micro and macro, individual and totality. Instead, practice theorists propose that we start with practices and rethink our theories from the ground up. Bourdieu, for instance, insists that only a theory of practice can open up a way forward:

> Objective analysis of practical apprehension of the familiar world . . . teaches us that we shall escape from the ritual either/or choice between objectivism and subjectivism in which the social sciences have so far allowed themselves to be trapped only if we are prepared to inquire into the mode of production and functioning of the practical mastery which makes possible both an objectively intelligible practice and also an objectively enchanted experience of that practice. (Bourdieu 1977:2–3)

Two of the most important characteristics of the practical turn are a holism about meaning – a holism that serves to undermine traditional distinctions – and an emphasis on the importance of close attention to particular practices and the context within which they are located. But there is much less agreement as to how practices are to be understood, and precisely how practices provide the basis

for overcoming such entrenched distinctions. It would be only too easy for an unsympathetic critic to argue that practice theorists face a dilemma: either they remain tacitly committed to traditional categories, or they give them up at the price of failing to adopt any coherent position at all. Certainly these dangers do arise for the practice theorist who wishes to redraw familiar maps, or those who take the practical turn to lead us to give up maps in favor of investigating the landscape. But they are no reason to think that such a project cannot succeed.

What is a practice? No short answer will do here. At the very least, a practice is something people do, not just once, but on a regular basis. But it is more than just a disposition to behave in a certain way: the identity of a practice depends not only on what people do, but also on the significance of those actions and the surroundings in which they occur. This is only to begin to answer the question how we are to understand "what people do" when they are engaged in a practice, or just what a practice amounts to. For there are enormous differences among practice theorists on just this point, and the differences are far-reaching. Discussions of practice make use of several overlapping clusters of loosely connected and ambiguous terms, terms that suggest connections that lead in a number of different directions. These include: activity, praxis, performance, use, language-game, customs, habit, skill, know-how, equipment, habitus, tacit knowledge, presupposition, rule, norm, institution, paradigm, framework, tradition, conceptual scheme, worldview, background, and world-picture. One way of classifying practice theories is by looking at which terms are central to competing conceptions of practice. For instance, one could contrast individualistic with social conceptions, local with global, normative with descriptive, or implicit with explicit. But this would be doubly problematic. It would reintroduce at the very beginning just those dichotomies that practice theory problematizes, and it would not do justice to the fact that many of the terms in question are just as disputed as practice itself.

"Practice theory" can be an elusive expression to pin down. Taking practices as a point of departure does not require a commitment to any particular method, or any specific destination. As a result, "practice theorists" are an unusually diverse group. Talk of practices has become widespread, not only in the philosophy of social science, but throughout philosophy, the humanities, and the social sciences. Nearly 20 years ago, in a review essay on "Theory in anthropology since the sixties," Ortner observed that "For the past several years, there has been growing interest in analysis focussed through one or another of a bundle of interrelated terms: practice, praxis, action, interaction, activity, experience, performance," and identified it as one of the most promising and interesting recent trends in her field. Referring to connected work in linguistics, sociology, history, and literary studies, Ortner (1984:44–5) observed that "the present movement appears much broader than the field of anthropology alone." Yet while the interest in practice has only continued to grow, relatively few writers explicitly identify themselves as "practice theorists," and among those who do, there is considerable disagreement as to who is a practice theorist and how practice theory is to be understood. Practice theorists have often presented their work in a rhetoric of revolution and

radical change, on which almost all past work is condemned as part of a mono-lithic "tradition." They have rarely acknowledged in any detail the points on which they agree with anyone other than their immediate allies. It is only recently that the work of carefully comparing and contrasting different practice approaches and examining their relationship to previous work has begun; *The Practice Turn in Contemporary Theory* (Schatzki et al. 2001), the first volume to bring together philosophers, sociologists, and scholars of science to explore the significance of practices in human life, was still in press when this chapter was written.

One reason for this state of affairs is that most practice theorists are opposed to the very idea of a *theory* of practice, if one considers a "theory" to be a formal system of hypotheses that generate explanations and predictions. However, there is also a much more open-ended sense in which the term "theory" is used for any general or systematic way of approaching a given subject matter, a usage which includes such activities as providing models, offering exemplary studies of particu-lar cases, developing conceptual frameworks or categories, or providing a genealogy, and it is in this sense in which "practice theory" is a theory. But even so, it would be a misnomer to speak of all work on practices as "practice *theory*," as one motiva-tion for attending closely to practice, particularly among those most influenced by Wittgenstein, is a thoroughgoing opposition to theorizing about practice. On this view, it is precisely those aspects of our practical abilities that cannot be captured by a systematic or formal account that are the point of attending to practice. For this reason, it can be helpful to talk more inclusively of a practical turn, and to regard practice theory as one part of it. One of the ironies of the relationship between these intertwined yet opposing approaches is that practice theorists such as Bourdieu or Bloor employ Wittgensteinian arguments for the irreducibility of practice to theory in the service of a systematic theory of practice.

Within the field of social theory, practice talk has attracted those who see it as a way of moving beyond traditional debates about methodology and ontology, over whether social theory should commit itself to giving a fundamental place to social wholes – overarching categories such as nation, community, class, or race – or whether it must begin with individuals, such as rational actors, human beings, utility maximizers, or biological organisms. Thus Schatzki takes "practice theory" to cover

> a collection of accounts that promote practices as the fundamental social phenom-enon. Such theorists as Pierre Bourdieu, Anthony Giddens, Jean-François Lyotard, Charles Taylor, and to some extent Ernesto Laclau and Chantal Mouffe, agree that practices are not only pivotal objects of analysis in an account of contemporary Western society, but also the central phenomenon by reference to which other social entities such as actions, institutions, and structures are to be understood. (Schatzki 1996:11)

Practice theory also offers a new approach to seemingly irresoluble problems about rule-following. Preda has proposed that practice theory is a response to

problems that social theorists have faced in making sense of the assumption that "rules of conduct" must play a foundational role in any account of human activity. How are such rules to be reconstructed from concrete examples of their application? What does it mean to say that someone is following a rule? How is social order achieved and reproduced? In view of the difficulties that these questions have raised for theorists who have treated rule-following as foundational, practice theorists have claimed that "rethinking social rules leads to a reconceptualization of social order and, therefore, of the sociological enterprise" (Preda 2000:270) Preda focuses on three constellations of practice theorists: Bourdieu and his collaborators; Harold Garfinkel's ethnomethodology, and the work of ethnomethodologists such as Michael Lynch, Jeff Coulter and David Bogen; and Michel Callon and Bruno Latour's actor-network theory.

While the term "practice theory" is most commonly applied to the relatively small groups of social theorists discussed by Schatzki and Preda, they are part of a much more widespread "practical turn," and it is that turn which is my principal topic. A survey of the philosophical antecedents from Aristotle to Marx and American pragmatism is not possible here. For our purposes, and for many participants in the current practical turn, practice theory has its roots in the work of Martin Heidegger and Ludwig Wittgenstein, and principally Heidegger's *Being and Time* ([1927] 1962) and Wittgenstein's *Philosophical Investigations* ([1953] 1958).

Being-in-the-World and Practical Holism

Basic disagreements about theory and practice not only divide practice theorists, but lead to systematic and far-reaching misunderstandings. This problem is particularly acute in the case of Heidegger and Wittgenstein, where sympathetic exposition often takes the form of uncritical paraphrase, and unsympathetic criticism usually turns their writings into trivial falsehoods. It is for this reason that I take as my point of departure Hubert Dreyfus's lucid and provocative Wittgensteinian interpretation of Heidegger on the primacy of practice. Dreyfus draws a helpful distinction between two kinds of holism about meaning and interpretation. Theoretical holism holds that all understanding is a matter of interpreting, in the sense of applying a familiar theory, a "home language," to an unfamiliar one, the "target language." On this Quinean model, we always have to start from our understanding of our own language, an understanding that consists in a system of rules and representations. Practical holism is the view that while understanding "involves explicit beliefs and hypotheses, these can only be meaningful in specific contexts and against a background of shared practices" (Dreyfus 1980:7). The practical holist agrees with the theoretical holist that we are always already within the "hermeneutic circle" – we have no alternative to starting with our current understanding – but argues that theoretical holism

mistakenly conceives of understanding a language on the model of formulating a theory, or mapping an unfamiliar landscape. This leaves out the background practices, equipment, locations, and broader horizons that are not specific presuppositions or assumptions, yet are part and parcel of our ability to engage in conversation or find our way about.

> So long as he remains unaware of the limits inherent in his point of view on the object, the anthropologist is condemned to adopt unwittingly for his own use the representation of action which is forced on agents or groups when they lack practical mastery of a highly valued competence and have to provide themselves with an explicit and at least semi-formalized substitute for it in the form of a *repertoire of rules*. . . . It is significant that "culture" is sometimes described as a *map*; it is the analogy which occurs to an outsider who has to find his way around in a foreign landscape and who compensates for his lack of practical mastery, the prerogative of the native, by the use of a model of all possible routes. (Bourdieu 1977:2)

Considered in abstraction from its context, a rule, like an ostensive definition, can be made to conform with every course of action. At first sight, stating a familiar rule, or pointing to an object in plain view, are acts that seem entirely straightforward and unproblematic. But with a little ingenuity, we can easily imagine circumstances in which the rule, or the act of pointing, do not have their usual significance. For instance, a word used in stating the rule may have been given an unusual meaning, or the object pointed to is found by following a line from finger to elbow. Furthermore, we can just as easily imagine that these clarificatory statements are themselves open to reinterpretation. In such a case, "we give one interpretation after another; as if each one contented us at least for a moment, until we thought of yet another lying behind it" (Wittgenstein [1953] 1958:§201.) It is only when we return to the "rough ground" (§107) and consider the background of practices to which a rule belongs that the rule takes on a determinate form. The theoretical holist will reply that if such a background is necessary, it must be analyzable in terms of further rules, intentions, or a tacit belief system. In turn, the practical holist will respond that it is a mistake to postulate tacit belief whenever explicit beliefs cannot be found, and to fail to do justice to the contextual, embodied, and improvisational character of practice. Rules are not self-interpreting, and their application depends on skill: "rules leave loop-holes open, and the practice has to speak for itself" (Wittgenstein 1969:§139).

In *Being-in-the-World*, Dreyfus argues that for both Wittgenstein and Heidegger, conformity to publicly established norms is woven into the fabric of our lives, that "the source of the intelligibility of the world is the average public practices through which alone there can be any understanding at all" (Dreyfus 1991:155). The norms that are constitutive of these practices should not be understood in terms of sharing explicitly stated or statable beliefs or values, or in terms of conscious intentions – although these will certainly play a part from time to

time – but rather as a matter of unreflectively acting in the same way as others, of doing what "one" does. Good examples are the way in which one typically conforms to local patterns of pronunciation and comportment:

> If I pronounce a word or name incorrectly others will pronounce the word correctly with a subtle stress on what I have mispronounced, and often I shape up without even noticing. (We certainly do not notice how we are shaped into standing the distance from others one is supposed to stand.) (Dreyfus 1991:152)

The "averageness" of these practices is not primarily statistical or causal: it is the result of the way conformity shapes what we do and what we are. Dreyfus reads Heidegger and Wittgenstein as replacing a view on which communication is made possible by our knowledge of objects by a view on which knowledge of objects is made possible by a shared language and background practices: "We have the *same thing* in view, because it is in *the same* averageness that we have a common understanding of what is said" (Heidegger [1927] 1962:168). Another way of putting this point is to say that "our social practices embody an ontology" (Dreyfus 1991:16).

In defending Heidegger's thesis that conformity is the source of intelligibility, Dreyfus cites a much-quoted passage from Wittgenstein's *Investigations* ([1953] 1958:§241) and provides a parenthetical translation:

> Wittgenstein answers an objector's question just as Heidegger would:
> "So you are saying that human agreement decides what is true and what is false?" –
> It is what human beings *say* that is true and false; and they agree in the *language* they use. That is not agreement in opinions [intentional states] but in form of life [background practices]. (Dreyfus 1991:155)

We can sum up this practical holist reading as follows: unless we shared a language, where a language is understood to include background practices, we could not say anything, true or false. Dreyfus has recently restated these claims in closely related terms:

> In my Commentary, I spelled out Heidegger's basic theses that (1) people have skills for coping with equipment, other people, and themselves; (2) their shared everyday coping practices conform to norms; (3) the interrelated totality of equipment, norms and social roles form a whole which Heidegger calls "significance"; (4) significance is the basis of average intelligibility; (5) this average intelligibility can be further articulated in language. . . . I concluded that, for both Heidegger and Wittgenstein, the source of the intelligibility of the world and of Dasein is the average public practices articulated in ordinary language. (Dreyfus 2000b:156)

Practice enters this account in two different but related ways. First, there are the familiar, if ordinarily unnoticed, "coping skills," that are taken for granted in our everyday activity, which Dreyfus refers to in (1). Favored examples include

the skills involved in pronouncing words and distance-standing, but such skills are omnipresent in our dealings with our surroundings, others, and even ourselves. One of the main contentions of Division I of *Being and Time* (Heidegger 1962) is that these practices cannot be understood piecemeal. Instead they hang together to form a whole, the phenomenon of "being-in-the-world," which is prior to both self and world as those terms are ordinarily understood. This leads us from average public practices, the particular skills that are the topic of thesis (1), to "significance," the interrelated totality that is the topic of thesis (3). But this brings us to the second level at which practice enters the account: background coping. Significance, understood as background coping, is not something radically different from ordinary coping, "rather, it is *the same sort of coping* functioning as the holistic background for all purposive comportment. . . . Being-in-the-world is just more skilled activity" (Dreyfus 1991:107). More recently, Dreyfus has been led to qualify this identification, acknowledging that background practices are more than the sum of our coping skills, connecting them with Heidegger's conception of "disclosure," as what makes coping skills possible (see Wrathall 2000, Dreyfus 2000a:338–9.) As Rouse (2000) has observed, over-emphasizing the contrasts between "coping skills" and explicit rule-following or action can lead us to overlook how much they have in common. Interpreting background practices as disclosure should not lead to an approach on which practical activity is contrasted with the nonpractical, but rather one on which every aspect of our lives is always already within a practical horizon: "practices are not just agents' activities but also the configuration of the world within which these activities are significant" (Rouse 1996:133). Just what this horizon amounts to is the principal topic of Division I of *Being and Time*. Without delving further into Heideggerese and Heidegger exegesis, one can say that it includes an orientation toward others, familiar things, locations, mood, past circumstances and future projects.

Critics of practical holism usually take talk of practices to be another way of talking about a more familiar category, such as the causes of behavior, observable regularities in behavior, or systems of belief. But practices are neither simply intentional states nor behavior, and theories of practice that attempt to account for practices in those terms alone fail to do them justice. Readers of Heidegger and Wittgenstein who are looking for a positive theory often take their talk of practices or "forms of life" as a starting point for a theory of practice. The point of the positive views would be to justify our talk of meaning and understanding, by locating it either within the space of causes or the space of reasons – what Bourdieu calls the "dilemma of mechanism or finalism" (Bourdieu 1977:22). "Mechanists" offer a nonintentional, or nonnormative, theory of practice, by placing it in a broader context of human behavior which can be described in naturalistic and causal terms (see, e.g., Bloor 1983, 1997, 2001)."Finalists" give an intentional, or normative theory of practice, by placing it in a broader context of human behavior which can best be described in terms of justification or reason giving (see, e.g., Brandom 1994).

However, both the normative and causal approaches only get at part of what practices are, and neither delivers on the promise of finding a way out of the standard and misleading philosophical dichotomies. They are two sides of the same coin. Bourdieu, like the Heidegger of *Being-in-the-World*, believes that practical holism offers the correct way out of this dilemma – a theory of practice that neither reduces practices to a system of rules nor to a causal theory:

> The place which a notion as visibly ambiguous as that of the *rule* occupies in anthropological or linguistic theory cannot be fully understood unless it is seen that this notion provides a solution to the contradictions and difficulties to which the researcher is condemned by an inadequate or – which amounts to the same thing – an implicit theory of practice. (Bourdieu 1977:22)

Bourdieu believes that the notion of "the *rule*" provides the solution to the problems posed by finalist and mechanist theories of practice, because it provides the basis for an explicit theory of what he calls the "habitus." Roughly speaking, habitus is Bourdieu's expression for what other practice theorists speak of as "know-how" or "practical understanding": those skills that make explicit rule-following possible, and which are the principal concern of his theory of practice. This theory aims to overcome the dilemma of mechanism or finalism within a broader framework that makes clear why each of these competitors offers a misleading and partial description of a more complex phenomenon.

Two Philosophers and an Antiphilosophy: Kripkenstein, Winchgenstein, and Therapeutic Quietism

Wittgenstein's *Philosophical Investigations*, a book that maintains that "we may not advance any kind of theory. . . . We must do away with all *explanation*, and description alone must take its place" (Wittgenstein [1953] 1958:§109) is at first sight an odd place to look for a practice *theory*, even on the loosest understanding of "theory." But there is actually a close connection between Wittgenstein's deep-rooted mistrust of philosophical theorizing and the theories his readers have found in the *Philosophical Investigations*. While the idea that Wittgenstein's skeptical questions about how rule-following is possible led him to a theory of practice can already be found in Winch (1958) and Fogelin ([1976] 1987), Kripke's *Wittgenstein on Rules and Private Language* (1982) set the terms of the current debate. Kripke reads Wittgenstein as raising and responding to a skeptical argument about rule-following, that is, as replying to someone who holds that we cannot provide a satisfactory answer to the question about what it is to follow a rule. The book sets out an argument that supposedly shows there is never any fact of the matter about whether one has followed a rule correctly, because one can always come up with a reading of the rule on which another action is the

right one. Because Kripke explicitly avoids committing himself to the view that the argument is Wittgenstein's, or endorsing it himself, it is convenient to put questions of authorship to one side by speaking of the skeptical view as "Kripkenstein's."

While Kripkensteinian skepticism about rule-following has few supporters, it has become the point of departure and disagreement for the standard approaches to rule-following. Most readers agree with Kripke that Wittgenstein is replying to skepticism about rule-following, but dispute the right answer. The two main camps are known as "individualists" and "communitarians." "Individualists," such as McGinn (1984) and Blackburn (1984) maintain that a single individual can provide the resources for a solution. In other words, the practices involved in following a rule may be the practices of an isolated individual. "Communitarians" such as Winch (1958) or Bloor (1983, 1997), hold that answering the skeptical problem is only possible if one is a member of a community – a group of a certain kind – and so the practices in question must be social, if not community-wide.

A further alternative is to hold that the debates between skeptics and antiskeptics, individualists and communitarians, miss Wittgenstein's point, which is that there is no philosophical problem about rule-following, no "gap" between rules and their application of the kind that concerns both skeptics and antiskeptics. Kripkensteinian theories of rule-following are based on the mistaken expectation that we need a theory of language and practice to justify our talk about rules, meaning, and understanding. On this quietist reading, Wittgenstein's practical turn is not the beginning of a positive theory of practice, or a pragmatist theory of meaning, but rather is meant as therapy, to help his readers get over their addiction to theorizing about mind and world, language and reality. Diamond (1991) and McDowell (1981, 1993) are prominent advocates; see also Crary and Read (2000).

The relatively small number of passages from the *Investigations* that are ritually cited as a statement of quietism are greatly outnumbered by those passages in which skeptical and antiskeptical theories are debated and defended. Nothing is easier than to read Wittgenstein as either a skeptic or an antiskeptic, theoretic holist, practical holist, or therapeutic quietist, depending on which of those passages one plays up and which one plays down. Not enough attention has been given to the fact that the places where Wittgenstein comes closest to endorsing practical holism, such as *Investigations* §241 or §§198–202 are responses to aggressive questions, not dogmatic theses. The text of the *Investigations* is best read as a dialogue that includes the voices of both practical holism and therapeutic quietism, rather than as unequivocally endorsing either (see Stern 1995:ch. 1). This is why the book provides some support for both a communitarian practice theory of some kind – where that term is understood broadly enough to include the theories constructed by Winch and Bourdieu, Bloor and Dreyfus – and for a quietist turn away from practice theory. Nevertheless, many practice theorists do rely on ideas they find in the *Investigations*, and the disputes about how to understand practice theory, including disagreements about method and theorizing,

have much in common with broader disputes about how to read the *Investigations* as a whole (see Stern forthcoming). For this reason, it will be helpful to approach the variety of conceptions of practice by looking at the fate of Winch's interpretation.

Winch's exposition of Wittgenstein's ideas about language and practice is particularly important because this is how Wittgenstein entered the "rationality debates" of the 1960s and 1970s: as a relativistic challenge to universal standards of rationality in philosophy and social science (see Wilson 1970, Dallmayr and McCarthy 1977, Hollis and Lukes 1982, and Hiley et al. 1991). What will concern us here is not what Winch or Wittgenstein intended, but the reading of Wittgenstein that most of Winch's readers took away from their reading of *The Idea of a Social Science* and "Understanding a primitive society" (Winch 1958, 1964). As this reading turns some of Wittgenstein's and Winch's most interesting ideas into a very bad theory, more Frankenstein than Wittgenstein, it is tempting simply to ignore it. Nevertheless, it is worth discussing, not only because so many philosophers still take this undead theory for granted, but because it is a good example not only of how not to do practice theory, but how practice theory has been systematically misunderstood. In order to put to one side questions about whether this is a fair reading of Winch, or Wittgenstein, I will speak of the holder of this view, whoever it may be, as "Winchgenstein."

Winchgensteinian Practice Theory

Winch's *The Idea of a Social Science and its Relation to Philosophy* defends an interpretive approach to social science that starts with what its subjects take for granted:

> I do not wish to maintain that we must stop at the unreflective kind of understanding. . . . But I do want to say that any more reflective understanding must necessarily presuppose, if it is to count as genuine understanding at all, the participant's unreflective understanding. (Winch [1958] 1990:89)

But this unreflective understanding cannot be understood in isolation from their broader practical and cultural context, the "forms of life" of the people in question. Because of the way in which what we say and do is embedded within this broader context, language and world are inextricably intertwined.

Winch's principal argument for these far-reaching conclusions is contained in his exposition of Wittgenstein on rule-following ([1958] 1990:24–39). Winch begins by pointing out that words do not have meaning in isolation from other words. We may explain what a word means by giving a definition, but then one still has to explain what is involved in following a definition, in using the word in the same way as that laid down in the definition. For in different contexts, "the

same" may be understood in different ways: "It is only in terms of a given *rule* that we can attach a specific sense to the words 'the same'" ([1958] 1990:27). But of course the same question can be raised about a rule, too: how are we to know what is to count as following the rule in the same way? Given sufficient ingenuity, it is always possible to think up new and unexpected ways of applying a rule. However, in practice we all do, for the most part, conform: "given a certain sort of training everybody does, as a matter of course, continue to use these words in the same way as would everybody else. It is this that makes it possible for us to attach a sense to the expression 'the same' in a given context" (Winch [1958] 1990:31).

An essential part of the concept of following a rule, Winch contends, is the notion of making a mistake, for if someone is really following a rule, rather than simply acting on whim, for instance, we must be able to distinguish between getting it right and getting it wrong. Making a mistake is to go against something that "is *established* as correct; as such, it must be *recognizable* as such a contravention. . . . Establishing a standard is not an activity which it makes sense to ascribe to any individual in complete isolation from other individuals" ([1958] 1990:32). Rule-following presupposes standards, and standards presuppose a community of rule-followers.

The Winchgensteinian approach gives center stage to everyday action, understood on the model of following rules. It takes for granted that those rules are usually implicit but can, if the need arises, be stated explicitly, either by the rule-users themselves, or by a sympathetic investigator such as a philosopher or an anthropologist. However, it is crucial to this approach that those rules only make the sense they do within a given form of life which, in turn, consists of certain shared practices. Social science then is the study of these shared practices. But how are the practices in question to be understood? One possibility is that they are patterns of activity, patterns that include action, equipment, sites of activity, patterns that are never precisely and finally demarcated. This, I believe, is what Wittgenstein proposed when he introduced the term "language-game" in the opening sections of the *Investigations*. In part, the term is introduced by describing some simple practices: Wittgenstein's "builders," the imaginary people who follow a limited repertoire of simple orders, children's games with words, such as ring-a-ring-a-roses, and the ways children learn words. But the term is also applied to any practice in which language is involved in some way, any interweaving of human life and language: "I shall also call the whole: the language and the activities into which it is woven, the 'language-game'" (Wittgenstein [1953] 1958:§7).

> But how many kinds of sentences are there? Say assertion, question, and command? There are *countless* kinds: countless different kinds of use of what we call "symbols," "words," "sentences." And this multiplicity is not something fixed, given once for all; but new types of language, new language-games, as we may say, come into existence, and others become obsolete and get forgotten. (We can get a rough picture of this from changes in mathematics.)

Here the term "language-*game*" is meant to bring into prominence the fact that the *speaking* of language is part of an activity, or a form of life.

Review the multiplicity of language-games in the following examples, and in others:

Giving orders, and obeying them –

Describing the appearance of an object, or giving its measurements –

Constructing an object from a description (a drawing) –

Speculating about an event –

Forming and testing a hypothesis –

Presenting the results of an experiment in tables and diagrams –

Making up a story; and reading it –

Play-acting –

Singing catches –

Guessing riddles –

Making a joke; telling it –

Solving a problem in practical arithmetic –

Translating from one language into another –

Asking, thanking, cursing, greeting, praying. (Wittgenstein [1953] 1958:§23)

A second possibility is that the appropriate notion of practice is rather that of what must be in place for the language game to go on. This complementary conception of practices is as "background": as whatever must be in place for the rules to operate.

These two interpretations of Wittgenstein – practices as language-games and practices as background – are closely related to Dreyfus's distinction between coping skills and background practice. The first incorporates aspects of both the "coping skills" and "background coping" conceptions, minus the transcendental aspect of the latter. However, it is considerably broader, as it extends beyond the bodily dispositions and cognitive abilities of the people involved, to include the equipment they use and the environment within which the activities in question take place.

Winchgensteinians usually speak of a "form of life" here, but there are barely a handful of uses of this term in the *Investigations* and it has been understood in the most diverse ways: transcendentally (e.g., as a necessary condition for the possibility of communication); biologically (e.g., an evolutionary account of how practice is possible); culturally (e.g., a sociological or anthropological account of what members of a particular social group have in common). However, in the 1950s and 1960s, practices were usually understood in terms of a set of rules, rules that govern use of the language in question, tacitly accepted by participants but only codified by researchers. The activities included under this conception of rule-governed language use were extremely diverse. At one end of the spectrum, there were particular, ordinarily small-scale, patterns of action, such as cooking a meal, making a promise, playing a game, praying, or carrying out an experiment. At the other end, there were patterns of patterns of action, which might include such matters as a regional cuisine, a legal system, the Olympic tradition, religion, or Newtonian physics.

While Wittgenstein and Winch stress the ways in which "the common beha-viour of mankind" (Wittgenstein [1953] 1958:§206) enable us to make sense of strangers and foreigners, most Winchgensteinians have primarily conceived of these taken-for-granted ways of behaving as specific to a given community. Even though he later came to regret it, Winch did provide a clear and controver-sial statement of how a relativism of standards can arise out of differences in background:

> Criteria of logic are not a direct gift of God, but arise out of, and are only intelli-gible in the context of, ways of living or modes of life. It follows that one cannot apply criteria of logic to modes of social life as such. For instance, science is one such mode and religion is another; and each has criteria of intelligibility peculiar to itself. So within science or religion actions can be logical or illogical . . . But we cannot sensibly say that either the practice of science or that of religion is either illogical or logical; both are non-logical. (Winch [1958] 1990:100–1)

Because the practice turn provides a way of conceiving of scientific theorizing as a social product, the most heated controversy has been around the application of the practice turn to knowledge, and especially scientific knowledge. Initially, Winchgensteinian ideas received most attention in the philosophy of anthropo-logy, thanks to "Understanding a primitive society" (Winch 1964). But they soon found a particularly fertile home in postpositivist philosophy of science and the sociology of scientific knowledge (SSK), a constructionist sociology of science that studied the content of scientific knowledge by means of sociological methods. Shapin (1982) is an early and influential manifesto for SSK; Golinski (1998) provides a somewhat uncritical overview of its reception in the history of science and science studies. For critical philosophical discussion, see Fine (1996), Friedman (1998), Rouse (1996), Roth (1998), and Stern (2001).

Winch's respect for the particularity of other cultures, and the need to under-stand them from within, was enormously attractive to those who wished to approach scientific cultures along comparable lines, by combining Thomas Kuhn's notion of a paradigm with Winch's account of understanding another culture. The crucial move here was to conceive of the culture of a particular group of scientists – one of the senses of Kuhn's famously slippery term, "paradigm" – along lines suggested, if not required, by Winch's discussion of forms of life. David Bloor and Harry Collins understand "forms of life" to refer to specific cultural or social groups, social entities comparable to the "primitive societies" discussed by Winch, or Kuhn's scientific research cultures.

> What Wittgenstein called a "pattern of life" or a "form of life" can be thought of as a pattern of socially sustained boundaries. (Bloor 1983:140)

> To use Kuhn's ([1962] 1996) idiom, the members of different cultures share differ-ent "paradigms," or in Wittgensteinian terms, they live within different forms of life. (Collins [1985] 1991:15)

The most important idea drawn from the philosophy of social science was that actors are to be understood as acting within a "form of life" (Winch [1958] 1990; Wittgenstein [1953] 1958). The idea was to have its counterpart, in the history of science, in the notion of "paradigm" (Kuhn [1962] 1996). (Collins [1985] 1991:171; cf. n.3)

While Bloor provided a more thorough and systematic interpretation of Wittgenstein as the principal philosophical antecedent of SSK, Collins's distinctive contribution was to provide detailed examples of how to do fieldwork on a practice-based approach. The principal methods he pioneered were sociological observation in the scientific laboratory and what became known as "controversy studies," which involved looking at every side of a disputed knowledge claim (Collins [1985] 1991, Collins and Pinch 1993, 1998). Collins and Bloor both insist that their respective approaches are scientific, but Bloor has no qualms about conceiving of philosophy and social science as modeled on the natural sciences while Collins, drawing on Winch and Berger, is in the *Verstehen* tradition.

Scientific Knowledge, an SSK textbook, states that the best way of "presenting the individual as an active agent in the context of the sociology of science" is "to characterize him or her as a participant in a form of life." It explains the term this way:

> The term is Wittgenstein's, and its use here is testimony to the relevance of Wittgenstein's work, directly or indirectly, to the work of many sociologists. Those who have taken up the work of Thomas Kuhn have thereby linked themselves to Wittgenstein; so have those who have extended ethnomethods into sociology of science. Harry Collins, who makes the most frequent explicit references to forms of life in science, has used the work of the philosopher Peter Winch as a line of access to Wittgenstein's ideas. [Bloor's] finitist account of the use of scientific knowledge in this book is another version of the same position. (Barnes et al. 1996:116)

Indeed, talk of "form of life" and "language-games" has become so widespread in SSK, as in many areas of the social sciences and humanities, that they are commonly used without any explicit reference to Wittgenstein. A good example of the wider use of "forms of life" in SSK can be found in Steven Shapin and Simon Schaffer's much-discussed *Leviathan and the Air Pump,* where the term is explicitly connected with Wittgenstein, but is used to refer to social interests:

> We intend to display scientific method as crystallizing forms of social organization and as a means of regulating social interaction within the scientific community. To this end we will make liberal, but informal, use of Wittgenstein's notions of a "language-game" and a "form of life." We mean to approach scientific method as integrated into *patterns of activity. . . .* We shall suggest that solutions to the problems of knowledge are embedded within practical solutions to the problem of

social order, and that differential practical solutions to the problem of social order encapsulate contrasting practical solutions to the problem of knowledge. (Shapin and Schaffer 1985:14–15)

But such "practical solutions" conceal a great deal of theoretical baggage, and critics have replied that the appeal to practice is no solution at all. In the next section, I outline some of the main criticisms that have been directed at Winchgensteinian practice theory.

From Winchgenstein to Frankenstein

For over 40 years, Ernest Gellner was one of the leading critics of Wittgenstein and Wittgensteinian "ordinary language" philosophy. His first book, *Words and Things* (1959), anticipates current objections to practice theory; *Language and Solitude: Wittgenstein, Malinowski and the Habsburg Dilemma* (1998) distills his critique into a concise condemnation. According to Gellner, the early Wittgenstein, the author of the *Tractatus*, had based his philosophy on two ingredients: abstract universal reason and sensory experience. Gellner sees this as a skeptical prison: starting with logic and an individual's experience, there is no way of escaping that isolated predicament. Gellner explains Wittgenstein's move from the "early" to the "late" philosophy by the "Habsburg dilemma": the conviction that "*there are two, and two only, options available to human thought*" (Gellner 1998:72): individualism or communitarian views of knowledge. On the first, knowledge is a relationship between an individual and nature. The alternative view is that knowledge is essentially social. Gellner attributes this communitarian view to Wittgenstein's later philosophy:

> The second option views human thought and language as embodied in systems of social custom, each tied to the community which employs it, and each logically ultimate, self-validating, and beyond any other possible validation. The custom of a community, expressed in speech, is the only law mankind can ever know or live by. (Gellner 1998:72)

Political conservatives used practice both to underwrite the customs and tradition of their country, region, or race, and to attack political reformers. Gellner's Wittgenstein is a philosophical conservative who used practice as a way of dismissing sceptical doubts and metaphysical systems: our ordinary ways of going on are in order, just as they are. They neither need any justification, nor can they be given one. This was "a philosophy which asserts that no rational, intellectual reason can possibly be found for any human practice, but that its justification can only be found by its place in a 'form of life', in other words, a culture" (Gellner 1998:105). But it is Gellner who is trapped by the "Habsburg dilemma," and

seeing no way out of it, he can only make sense of Wittgenstein as an enemy of the Enlightenment who retreated to an ontology of cultural traditions, rephrasing conservative dogmas as linguistic philosophy:

> Wittgenstein's theory of language, central to his philosophy, is but a coded theory of society: mankind lives in cultural communities or, in his words, "forms of life," which are self-sustaining, self-legitimating, logically and normatively final. . . . The real point is that the issue concerning the validation of linguistic habits is in effect the question concerning the validity and justification of social practices and customs, and the two issues become fused. (Gellner 1998:14)

Gellner gets around the problem that there is no direct textual support for this "coded theory of society" in the *Investigations* by saying that while Wittgenstein himself was primarily interested in applying this theory to traditional philosophical problems, it was Winchgensteinians who turned it into a cultural legitimation:

> Wittgenstein preached relativistic populism *in the abstract*, in general, without favouring any particular cultural cocoon. By contrast, his followers *did*. In practice, the deployment of the technique aimed not merely at obviating alleged pseudo-problems, . . . but also at positively vindicating their own "common sense". (Gellner 1998:161)

Gellner's critique is both epistemic and political. The rejection of culturally transcendent criteria is a bad theory of knowledge, because it leads to an irrationalist relativism on which every culture is internally self-validating, and immune to external criticism. Equally, it is bad political theory, because it discards the Enlightenment project of rational debate for a hazy and treacherous notion of tradition, locking each culture into a self-contained prison. Gellner characterizes this rejection of culture-transcendent rationality as relativism. But it also has a foundationalist aspect: because practices provide the ultimate basis for a culture's standards, they take on a foundational role for the culture, albeit an "internal" one. Thus, Steve Fuller classifies Kuhn and Wittgenstein as foundationalists, "only with the ONTOLOGY of the foundations shifting from propositions to practices" (Fuller June 17, 2000, email to HOPOS-L@listserv.nd.edu; see also Fuller 1992, 1997, and the 1997 *Human Studies* articles by Lynch, Pickering, and Turner).

Turner's objections to practice talk in social theory (1994, 1997), provide the fullest statement of the problem of the ontological status of practices, and particularly on their role in the transmission of culture. Turner contends that if practices are to do the work they are assigned by practice theory, then they are something that each member of a community has, and can pass on to others. But what can this thing be, if it is to ensure the maintenance of a culture? Turner argues that ultimately it is just a loose way of talking that has misled social theorists into believing that something substantial lies behind our actions. But

this notion of a practice as a "shared possession" is incoherent, "causally ludicrous." For a shared practice must "be transmitted from person to person. But no account of the acquisition of practices that makes sense causally supports the idea that the same internal thing, the same practice, is reproduced in another person" (Turner 1994:13). Turner is surely right that if we conceive of practices as akin to tacit beliefs – hidden, inner objects, that are causally responsible for our behavior – then the very notion of a social theory of practice is ludicrous, and there are insuperable difficulties in understanding how that thing can be transmitted. Turner advises us to replace practice talk with Humean "habits," observable patterns of behavior that do not presuppose a concealed cause.

If practice theory leaves us with a choice between occult objects and observable habits, or empty self-justification and democratic debate, then there is little doubt which choice is preferable. Certainly, these are forceful objections to some formulations of practice theory, but they do not show that the practical turn must lead to a dead end. One can read Heidegger, Wittgenstein, Kuhn, and their followers with Turner as relying on a shadowy, all-powerful and hidden notion of practice that, like the Wizard of Oz, is a deceptive subterfuge. But Wittgenstein is one of the greatest critics of the myth that the phenomena of our everyday lives must be explained by something invisible that lies behind them, and much of what Heidegger and Kuhn have to say about practice can be read in this way. While Wittgenstein and Heidegger have certainly inspired theories of practice that substitute magic and just-so stories for hard work, their work is also a resource for those who wish to investigate practices without being burdened by a theory of practices as hidden forces (cf. Rorty 1993, Stern 1997, 2000).

Investigating Practices

Practice theorists are drawn toward the idea that it is possible to navigate between the opposing dangers of subjectivism and objectivism, between description of our experience of agency and a deterministic social theory. Yet every attempt at such a theory has immediately found detractors who argue that it fails to transcend the dilemmas of subjectivism and objectivism. Here, we seem to be very close to what Kant calls an "antinomy" in the *Critique of Pure Reason*: a dispute in which two opposing positions appear to exhaust the range of possible views, yet each of which has telling objections to the other. Like Kant, practice theorists have attempted to resolve the dilemma by providing a broader perspective from which each opponent can be seen to be partially right, and partially wrong. Yet these attempts have always met with the criticism that they are only new versions of familiar, and unsuccesful, answers to the problems at hand. Bourdieu's theory of habitus, the key to his own reply to the antinomy of finalism and materialism, has repeatedly been charged with this very failing. The

standard objection to the notion of habitus in the secondary literature is that it "slips back into exactly the kind of objectivism Bourdieu refutes" (King 2000:418). For Boudieu holds that the perceptual structure and embodied dispositions that comprise the habitus are directly derived from the individuals' socioeconomic or structural positions. "Each agent, wittingly or unwittingly, willy nilly, is a reproducer of objective meaning. . . . his actions and words are the product of a *modus operandi* of which he is not the producer and has no conscious mastery" (Bourdieu 1977:79). However, King goes on to point out that while there is ample evidence one can cite for this reading, one can also find a second, incompatible, strand of thought within Bourdieu's writing that points toward a "non-dualistic social theory founded on intersubjective, meaningful practice" (Bourdieu 1977:79).

The central idea that motivates this second strand is a close description of virtuosos, experts at work, of cases of intimate understanding revealed in exceptionally skillful practical proficiency. The aim is to characterize a practical flexibility that outruns any mechanical application of finally stated principles. Faced with an unanticipated challenge, the virtuoso draws on his or her grasp of the entire situation to come up with a creative response that goes beyond precedent yet can, retrospectively, be recognized as a masterful response to the problem in hand. (Cf. Dreyfus 2000b for an Aristotelian reading of Heidegger along these lines.) This second strand of Bourdieu's work conceives of social life as a "mutually negotiated network of interactions and practices between individuals which is always necessarily open to strategic transformation" (King 2000:431). King insists that this does not "involve a retreat into subjectivism," but the critic of practice theory can respond that celebrating the virtuoso's "strategic transformation" of established precedents does not resolve practice theory's basic dilemma. If the transformation in question is ultimately a product of the virtuoso's socioeconomic position, then we are back with the first, objectivist, strand of thought. If it is not susceptible to objectivist explanation, then either we are back with a subjectivist celebration of individual creativity, or we are still unstably moving between objectivist and subjectivist approaches.

While Dreyfus stresses the parallels between his reading of Heidegger and the later Wittgenstein's insistence on the primacy of practice, he does not lose sight of the principal disanalogy between the early Heidegger and the later Wittgenstein. Heidegger's "existential analytic," his elaborate account of the structure of the background of everyday activity, is a systematic theory of practice, while Wittgenstein "is convinced that the practices that make up the human form of life are a hopeless tangle . . . and warns against any attempt to systematize this hurly-burly" (Dreyfus 1991:7). But Wittgenstein's description of this "hurly-burly" is only a "hopeless tangle" from the perspective of an inveterate systematizer. For those looking for an approach to practice that starts from particular cases, for a way of investigating practices without doing practice theory, Wittgenstein's unsystematic approach holds out the hope of doing justice to the indefinite and multicolored filigree of everyday life:

We judge an action according to its background within human life, and this background is not monochrome, but we might picture it as a very complicated filigree pattern, which, to be sure, we can't copy, but which we can recognize from the general impression it makes. The background is the bustle of life. . . . How could human behaviour be described? Surely only by showing the actions of a variety of humans, as they are all mixed together. Not what *one* man is doing *now*, but the whole hurly-burly, is the background against which we see an action, and it determines our judgment, our concepts, and our reactions. (Wittgenstein 1980:§624–5, 629)

In this passage, Wittgenstein provides a particularly clear statement of an approach to practice that insists on staying on the surface, by attending to the detail and complexity of the complicated patterns that make up our lives. However, to anyone attracted to the idea that the social scientist must go beyond simply describing the detail of our everyday lives, such an approach is akin to a naive empiricism or extreme subjectivism, a misguided attempt to give up all theorizing in favor of a first-person perspective on social life. To such a critic, Wittgensteinian description is so atheoretical that it no longer holds out the hope of a practice theory: by discarding the goals of system and rigor, it avoids the problems involved in trying to formulate a theory of practice, but no longer has the explanatory power of the original, admittedly problematic, notion of a theory of practice. On the other hand, to a Wittgensteinian, a more ambitious approach that aims to discern a systematic pattern behind the phenomena, such as Bourdieu's theory of practice, goes too far in the opposite direction, substituting a theory of fictitious forces for close observation of what actually goes on in our lives. Perhaps it is the protean character of practice theory, the way in which it holds out the promise of accommodating both the aim of a rigorous theory of society, and the desire for a close description of particulars, that has made it both so attractive and so hard to pin down. It remains an open question whether it is possible to produce a practice theory that provides a consistent resolution of this conflict.

Note

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Science & Technology Studies and the Philosophy of Social Sciences

Steve Fuller

The growth of the field of Science & Technology Studies ("STS") is usually regarded as a triumph for a relativist philosophical sensibility that has turned itself into an empirical research program. For those who think of intellectual matters in political terms, this is seen as a "progressive" moment, but in reality the situation is much more ambiguous. In the first section, I note the historic connection between relativism and a defense of scientific autonomy modeled on the politics of containment familiar from late British imperialism and United States' Cold War science policy. Here the analogy between scientists and natives, much exploited in STS rhetoric, needs to be taken more literally. In the following section, I argue that relativism is itself only one pole of the antirealist dialectic that defines STS's philosophical horizons. The other pole is constructivism. Both poles presume that scientific realism is false, but the diagnosis of its falseness diverges significantly, indeed in ways that reproduce the foundational dispute in sociology. STS masks its own origins and development by a self-conscious adoption of Thomas Kuhn's theory of scientific change. Kuhn's legacy is the topic of the third section. The syncretism of Kuhn's model, which serves to obscure the radical sociological changes that science has undergone in the twentieth century, begins to explain why STS is not as explicitly critical of the social function of science as one might expect. The fourth section discusses this point in relation to Ian Hacking's recent popular apologia for STS. Like most STS practitioners, Hacking is blind to the critical and transformative intent of the original sociology of knowledge, which STS has largely abandoned. Consequently, as I argue in the final section, STS has produced a sociology of science that is no longer a sociology of knowledge.

Relativism and the Illusion of Autonomy in Science

Those who follow the developments in Science & Technology Studies from a high altitude can easily reduce the field's philosophical contribution to a colorful extension of the basic relativist strategy that was initiated by Peter Winch in *The Idea of a Social Science* (Winch 1958). After all, the various schools of empirical STS – be it located in Edinburgh or Paris – follow in many of Winch's Wittgensteinian footsteps. They share a suspicion of distinctly "philosophical" explanations that override accounts explicitly grounded in native practices. The goal, then, is to delegitimate these explanations, and in some sense let the phenomena speak for themselves. Here "philosophical" is synonymous with "metaphysical" in the objectionable sense that originally united Wittgenstein and the logical positivists in common cause: namely, a source of misunderstandings, false expectations, and potentially disastrous interactions that are the result of our letting what we say get in the way of what we see. However, the range of actual practices deemed "philosophical" in this objectionable sense extends beyond metaphysics proper to much of what passes for explanatory theory in social science research and even the natives' ordinary self-understandings, insofar as these are in the grip of a theory that does not adequately capture native practices.

Recalling these features of the Winchian project helps account for many of the distinctive features of STS. Consider the opening move of the field's landmark monograph, Latour and Woolgar's *Laboratory Life*, in which the authors present the ideal observer of the scientific laboratory as someone ignorant of the science under investigation (Latour and Woolgar 1979). This then licenses their suspension of belief in the categories invoked by the scientists they interact with. Indeed, only those social scientific methods that remain "close" to the phenomena are officially allowed. In practice, this means that one strives to model one's research on ethnography and present one's theory as "grounded" in the natives' experience (Glaser and Strauss 1967). Steven Shapin's phrase "virtual witnessing," though originally meant to apply to scientific writing, aptly captures the narrative perspective that he as an STS-oriented historian adopts in order to stay "grounded." Thus, STS is just as dismissive (at least presumptively) of social scientific as of natural scientific explanations of the scientific enterprise (Fuller 2000b:322–4).

Here it may be useful to relativize the Winchian relativist strategy common to STS, since it did not itself arise in a sociological vacuum. Significantly, Winch and his followers take their principal empirical inspiration from anthropology – specifically, the field work of Edward Evans-Pritchard, whose most distinguished disciple, Mary Douglas, was an early resource for much STS theorizing (for example, Bloor 1983). Philosophers know of Evans-Pritchard largely through his studies in the 1930s and 1940s of two Sudanese tribes, the Nuer and the Azande, which have provided "hard cases" for cross-cultural judgments of rationality. However, in the annals of anthropology, Evans-Pritchard is known as one of the

first to insist that anthropologists eschew interpreters and master native languages for themselves for purposes of penetrating the natives' worldview (Evans-Pritchard 1964:79–80). This signature relativist move was developed just as the sun began to set on the British Empire, when the Crown was mainly concerned with retaining the good will of natives whose loyalties were likely to be tested by the Nazis as the theater of war shifted from Europe to Africa in a second Great War (Goody 1995:63–6).

It is small wonder that the main contributors to the universalism–relativism debate that invented the "philosophy of the social sciences" in the anglophone world were more likely to have been educated in the United Kingdom than the United States. The two sides of this debate reflect consecutive moments in the history of British imperial rule. The first moment corresponds to the original subordination of native peoples to the Crown, the second to the maintenance of their loyalty in the face of threat from other imperial powers. Here the relativist offers the prospect of external rule without either force or condescension, since (say) the Sudanese are allowed to conduct themselves as they see fit, as long as they do not try to undermine Britain's struggle with Germany. In short, the British and the Sudanese agree not to interfere in each other's practices as long as these do not interfere in their own. Yet, despite its official concern for the integrity of native practices, from the standpoint of power politics, relativism merely reinforced the existing terms of colonial domination (Fuller 2000b:18–21, 146–9).

Indeed, the success of this strategy suggests the Machiavellian lesson that treating people as "ends-in-themselves" may be one of the best means of treating them as means to one's own ends. The lesson acquires a special poignancy when applied to the institutional history of modern science. Two sorts of reversing means and ends have occurred. First, the original relativist strategy for appeasing the natives has been disembedded from its original context and become an end in itself, as arguments for and against relativism are endlessly refined and debated. Here STS is just as complicit as philosophers (Hollis and Lukes 1982, Pickering 1992). But more importantly, the political success of relativism lay in the natives' allowing themselves to function as reliable players in larger power struggles. We shall now apply these two types of reversals to science, with scientists functioning as natives. In the following subsections I examine (1) how *means turn into ends*, and then (2) how *ends turn into means*. The former accounts for how we come to have science as distinct from politics, the latter for how science comes to have political import. I shall then suggest a moral based on the reversible means–ends reversal that results from combining (1) and (2).

Means turn into ends

According to most nineteenth- and twentieth-century evolutionary models of social change, an innovative social practice usually emerges (unintentionally)

as a means to achieving some standing ends, which then, by virtue of its success in this role, becomes (intentionally) an end in its own right. Wilhelm Wundt originally colonized this insight for social psychology by speaking, in Kantian terms, of the evolution from the "heteronomy" to the "autonomy" of human ends as a by-product of the division of labor in complex societies. Wundt held that heteronomy was bred by the need to resolve, say, the competing demands of spiritual and material well-being in the same institution, such as a church, whereas now these are handled more rationally in two separate institutions, each exclusively devoted to only one form of welfare. At the same time, Wundt carried over Kant's original connotation of autonomous pursuits as ethically purer than heteronomous ones, which helped to square the moral circle of scientific fields whose increasing technicality removed them from immediate relevance to standing social problems. Ferdinand Tönnies applied Wundt's insight to explain the differentiation of social science from social policy at the inaugural meeting of the German Society for Sociology in 1910 (Proctor 1991:91–3).

In 1925, at the dawn of logical positivism, Moritz Schlick ([1925] 1974:94–101) argued for the superiority of deontological ("Kantian") over consequentialist ("Benthamite") ethics on grounds similar to Wundt's. Schlick added the Aristotelian insight that society had to be sufficiently leisured to allow certain activities to be pursued beyond the point where they provided a clear benefit to those pursuing them. Invoking a sociological distinction popular at the time, Schlick saw "culture" and "civilization" as corresponding, respectively, to the sphere of the consequentialist and the deontologist. The former was explicitly concerned with promoting a particular people, the latter with a more diffuse and potentially limitless agenda. Schlick's formulation intuitively resonated in the imperial era, since residents of the "mother country" generally bore the brunt of the "white man's burden," as their own standard of living levelled off and even dropped with the spread of "civilization" overseas. As we shall see in the next section, the culture–civilization distinction has been raised to a still more abstract level, in terms of the difference between relativism and constructivism.

Aided by evolutionary theory and heroic generalization, Karl Popper (1972) extended Schlick's original line of thought to explain the origins of objective knowledge, or "World Three." In Popper's telling, efforts to improve the instruments of survival breed a class of specialists who study the instruments' properties abstracted from any context of application. These specialists usually begin as troubleshooters who, as we now say, "reverse engineer" the instruments to discover the limits on their performance. Thus, practices of measuring and calculating yielded knowledge of geometry and arithmetic. In contemporary economics, this is portrayed as a self-organizing process for producing what Michael Perelman (1991) has called "metapublic goods." Accordingly, a science is seeded once "free riders" are encouraged to use a technology in exchange for communicating their experience to other users who lack the relevant experience.

Ends turn into means

In the Western legal tradition, guilds provide the surest guide to groups that have constituted themselves in terms of commonly shared ends but then ended up becoming manipulated by a larger agent (Krause 1996). Guilds officially enjoy an effective monopoly over the transmission of certain skills and products by virtue of their ability to maintain a consistently high level of quality. Historically, guilds acquired the conservative disposition of insurance bodies, censoring deviant practices that do not meet with the governing board's approval. As Bismarck well knew, the guild right of "academic freedom" made German academics more manageable: the state need not intervene to stop the spread of politically subversive positions, if the academics themselves already find it in their collective interest to do so. Thus, the mutual criticism of the peer-review process simultaneously launders out more radical positions and insures that what remains is of sufficiently high quality to be appropriated for orthodox political purposes (Hofstadter and Metzger 1955).

The constitution of science as a self-organizing and self-directing "community," though presaged in the writings of philosophers ranging from Thomas More to Charles Sanders Peirce, only became second nature to *natural* scientists in the 1950s, largely as an ideological defense against unwanted public scrutiny (Hollinger 1990). Unlike humanists and social scientists, natural scientists have never been as heavily concentrated in universities. The location of natural scientists in virtually every class position across the entire economy meant that they failed to share a common relationship to the means of production, a necessary condition for the emergence of Marxist class consciousness. The notably sustained attempt by J. D. Bernal to align natural scientists with the industrial proletariat from the 1930s to the 1960s foundered precisely on these grounds. Indeed, Joseph Rotblat's continuing failure to persuade fellow scientists to adopt a code of professional ethics modeled on the Hippocratic Oath should be seen in a similar light: it is far from obvious that natural scientists all belong to the same profession.

However, none of this has made natural scientists any less manipulable. The original proposal generated by the United States Congress to support a "National Science Foundation" (NSF) shortly after World War II would have rendered scientific research a means to larger societal ends, subject to the approval of social-scientifically led oversight panels (Fuller 2000a:117–31). The considered response of American politicians to the success of the atomic bomb project was that academic scientists had to be kept on a short leash to ensure that they continue to perform for the public good. They held that science serves society best when it serves society directly. In retrospect, this attitude turned out to be the last gasp of Franklin Roosevelt's New Deal. The peer-review-based NSF that ultimately triumphed reflected the anti-New Deal Republican majority that was elected to Congress in 1946 (Kevles 1977). The leading ideologues for this policy shift were such card-carrying "Yankee Republicans" as James Bryant Conant

and Vannevar Bush, who promoted the old Bismarck idea that the spontaneously self-policing functions of academic research scientists ensured the quality of the published findings. These could then be confidently appropriated by policy makers without the scientists themselves becoming involved in the applications.

In the emerging Cold War context, this proposal was read as calling for a collectivization of scientific effort in the name of national security. The lesson that Conant and Bush helped Congress to draw from the success of the United States' atomic bomb project was that academic scientists could be confidently left to their own devices to get the job done. After all, the brains behind the project included several pioneers of subatomic physics. In this way, and for the first time, the government became the majority shareholder in American science, the management of which was turned over to largely discipline-based peer review panels that were assisted by such newly developed "metascientific" policy instruments as the *Science Citation Index* that were used to nurture research as one might incubate an organism. The master thinkers of this strangely autonomous yet manipulable "scientific community" were Thomas Kuhn and Derek de Solla Price, who theorized the qualitative and quantitative sides of this image of science.

Most scientists flourished under this regime, comfortably serving Caesar and God at once. For example, decision theory – a field that changed the face of philosophy, psychology, economics, and computer science – was the spawn of Cold War science policy, a point easily inferred from the acknowledgments sections of the field's seminal articles. While these publications were often subject to military clearance, they usually passed scrutiny because the abstract and specialized nature of academic interest in decision theory posed little threat to national security. Indeed, here is a clear case of the relativist moral paradise previously illustrated by the British imperial policy toward the Sudanese tribes, whereby each side achieves its own ends by enabling the other side to achieve its ends. However, problems arise once one side decides to take an interest in the other side's ends, perhaps because it comes to believe that they all live in the same moral universe. One prominent example was the scandal associated with *The Pentagon Papers*, classified documents about the Vietnam War that were passed to *The New York Times* in 1971 by the distinguished decision theorist, Daniel Ellsberg.

The moral of this reversible means–ends reversal

To appreciate the full implications of this reversibility, we must begin by taking literally that science is "autonomous" in the sense that the self is in modern ethics. In that case, science is a special case of the following generalization: not only may autonomy be a long-term emergent property of instrumental success, but instrumentality may emerge as an unintended consequence of the autonomization of certain social practices from the contexts that originally gave them shape and direction. The moral of this story is ironic, to say the least. Put in interpersonal terms, the more consistently you are constituted as a Kantian ego, the more

efficiently I can render you a potential means to my own ends. Of course, I cannot make you do exactly what I want, but your consistency enables me to adjust my own behavior so I can get the most out of you. Thus, the principled agent turns out to be the "second-best" solution to the problem of strategic rationality, where the best solution is someone who does only what I want, and the worst solution is someone who does what anyone (including my opponents) wants.

However, the end of the Cold War has so subverted this second-best solution in science policy that the "serving two masters" scenario described above may well come to be regarded as a "golden age" in the history of free inquiry, especially if the United States remains the dominant power in a world possessed by free market ideology. From that standpoint, the legacy of the Cold War would be its unprecedented period of consolidated growth for universities and other nation-based epistemic institutions. In contrast, our brave new world of neo-liberal inquiry has "secularized" science, rendering it increasingly demand-driven, problem-oriented, and structurally permeable (Fuller 2000a:99–116). In European science policy circles, it is now fashionable to speak of this transition as between "Mode 1" and "Mode 2" knowledge production (Gibbons et al. 1994). Moreover, as we shall now see, this development has been shadowed by relativism and constructivism as successive phases in the philosophical history of STS.

STS's Janus-faced Antirealism: Relativism versus Constructivism

So far I have followed the standard philosophical practice of referring to STS's underlying philosophy as relativism. Another term – constructivism – is increasingly used in the same context. However, relativism and constructivism are not the same position. Nevertheless, they are often used interchangeably because they have a common enemy, scientific realism, against which much of the STS research agenda has been defined. Below is a definition of scientific realism, along with the distinct challenges that relativism and constructivism pose to it:

Scientific realism involves two distinct claims, each of which can be denied separately:

1. A scientific account is universally valid. Therefore, if scientific theory, T, is true, it is true everywhere and always. The denial of this claim is *relativism*. It implies that reality may vary across space at any given time.
2. A scientific account is valid independently of what people think and do. Therefore if T is true, it is true even if nobody believes it. The denial of this claim is *constructivism*. It implies that, for a given place, reality may change over time.

Relativism and constructivism thus pose alternative challenges to realism. The particularist orientation of relativism opposes realism's claim to universality, whereas

constructivism's reliance on the contingent actions of knowers undermines realism's claim to necessary truth. Thus, philosophical criticism targeted at constructivism may miss its mark by taking issue with relativism. For example, Boghossian (2001) glosses the constructivist slogan, "the rational itself is constitutively social" as "a relativization of good reasons to variable social circumstance." However, the constructivist slogan is meant to deny any clear distinction between what is rationally and socially acceptable. This view is compatible with *either* a relativist or a universalist epistemology. All that it implies is that rationality is to be explained sociologically. In principle, the relevant sense of "social" may be common to all societies. It certainly need not be limited to the relativist's clearly bounded, self-contained social worlds that are grist for paradoxes about the impossibility of standing both "inside" and "outside" one's world at the same time. Indeed, constructivists do not accept the idea that worlds (social or otherwise) have such clear "insides" and "outsides," as these boundaries are themselves the product of social construction, not the cause of them.

It follows that relativism and constructivism are compatible only under certain conditions but not others. However, from Parmenides onward, the Western philosophical tradition has tended to obscure this point, as necessity and universality, on the one hand, and contingency and particularity, on the other, have been associated together. Good examples include the long-standing distinction between *a priori* and *a posteriori* knowledge, or the kind of knowledge one can acquire from mathematical reasoning versus sense experience. To be sure, there have been attempts – such as Kant's synthetic *a priori* or Hegel's concrete universal – to forge intermediate forms, but they have been generally regarded with suspicion. The presumption has been that difference – be it defined in terms of sheer variety (à la relativism) or restless change (à la constructivism) – is always a deviation from a fixed norm that needs to be disciplined by either explanatory subsumption (that is, theoretically) or social control (that is, practically).

In contrast, had the West taken its marching orders from Heraclitus, we might now be operating in a philosophical universe where relativism and constructivism are clearly distinguished, but scientific realism exists only as an unstable hybrid. Thus, we would be puzzled about the idea of truths that remain invariant across all possible worlds without first having broken down the boundaries separating those worlds. (In other words: can universalism be anything other than imperialism?) For, in the Heraclitean universe, knowledge claims would be either contingently universal or necessarily particular. The presumption here would be that the maintenance of a consistent identity is an ongoing, and only locally successful, struggle in a world engulfed in endless flux.

As it turns out, the Heraclitean starting point is the one adopted by STS, which, as we shall see shortly, helps explain the distinctive character of its internal philosophical disputes. However, STS is not alone in deferring to Heraclitus. The literature surrounding "globalization" routinely supposes that we are living through a struggle between what Benjamin Barber (1995) has called "Jihad vs. McWorld"

or, in the titles of the first two volumes of Manuel Castells's magnum opus, *The Information Age* (Castells 1996–8), "The Network Society" and "The Power of Identity." In each binary, the former term refers to an unbounded constructivism and the latter to a resistant relativism. But we can reach back further to one of sociology's founding dualisms, what Ferdinand Tönnies called *Gemeinschaft* and *Gesellschaft*. To be sure, the classical sociologists regarded the flow of history rather differently from today's globalizationists. They saw *Gesellschaft* as a rationalization of *Gemeinschaft*, usually through the mediation of the state, whereas globalization theorists would have *Gemeinschaft* emerge as a spontaneous reaction against *gesellschaftlich* practices that are no longer under state control.

The clearest case of nonrelativist constructivism is free market capitalism, in which the value of goods is determined entirely by negotiated exchanges among interested parties. No preferences or beliefs are so fundamental as to be exempt from such negotiations, to which there is no "natural" outcome. Indeed, it is this inherent volatility that unites relativists and realists against constructivists. Indeed, the prototype of the modern relativist position – the "culture" that affixes a worldview to a particular group – was introduced in Germany to stave off the universalizing ambitions of the commercial ethic emanating from Britain in the early nineteenth century (Fuller 2000c). Moreover, before the advent of postmodernism, most anthropologists were probably relativists but *not* constructivists. To believe that truth is culturally relative has usually implied that there are facts of the matter (about tribal history, geography, and perhaps even biology) about which knowledge claims are true for which cultures. Anthropologists like Evans-Pritchard did not suppose that the natives negotiated their epistemic practices as they went along. That would have rendered a science of anthropology virtually impossible, a point postmodernists gladly admit. Instead, anthropologists regarded native practices as *ceteris paribus* instances of normal behavior in the societies where they occurred. The difficult task was establishing the scope of these practices: when and where did the natives tend to behave this way? But, in principle, this task was no different from establishing the boundary conditions under which an empirical regularity applied in the physical world.

It is often forgotten that the radical "otherness" with which anthropologists classically regarded the natives contributed to the idea that native cultures enjoyed a kind of epistemic independence from the anthropologist. Thus, to a constructivist, anthropology's relativism amounted to a realism about multiple social worlds. (Kuhn's incommensurability thesis is also such a realism; cf. Fuller 1988:85–9.) However, anthropology has suffered from its original nonconstructivist relativism, typically by underestimating interaction effects – that is, the ease with which both native practices can be altered by alien intrusion and aliens can be fooled by native irony and deceit. Unsurprisingly, then, the turn to constructivism in anthropology began with a reflexive realization that anthropologists were *in* the very worlds they were trying to write *about* (Marcus and Fischer 1986). From the standpoint of constructivism, the "otherness" of relativism turned

Table 9.1   *The Heraclitean dialectic in sociology*

Species Of Antirealism	Relativism	Constructivism
Fundamental process	Identification	Differentiation
Image of the social	Bounded groups	Interactive networks
Scope of the social	Finite and invariant	Infinite and variable
Source of value	Intrinsic to society	Defined in exchange
Principle of Social change	Reproduction at the macro level	Redistribution at the micro level
Classical sociological image	Customs (*Gemeinschaft*)	Contracts (*Gesellschaft*)
Defining social practice	War	Commerce
Political extreme	Totalitarianism	Imperialism
Biological version	Racialism	Adaptationism
STS version	Group-grid theory	Actor–network theory
"Return of the repressed"	"Revolution"	"Ecology"

out to be a form of self-deception whereby anthropologists were discouraged from observing their own participation in the imperialist project. In that sense, a reflexive relativism *is* constructivism (Fuller 1993:ch. 9).

To a large extent, and in a much shorter time, STS has repeated anthropology's sequence of relativist consolidation and constructivist dispersion, as recently epitomized by, respectively, David Bloor (1999a, 1999b) and Bruno Latour (1999). Together they illustrate an instance of what I call in table 9.1 *The Heraclitean Dialectic of Sociology.* As suggested by the row on "principle of social change," it is possible to regard relativism and constructivism as complementary positions, for example, if social functions are reproduced across generations by redistributing properties previously possessed by one group of individuals to another group. However, this neat macro–micro picture starts to crack once the redistributive process is seen as a source of emergent properties that alter the character of the reproductive process, such as when women come to fill roles previously filled by men. At this point, constructivism breaks free from relativism.

In the English intellectual tradition from Hobbes to Spencer, "war" and "commerce" often appeared as alternating historical phases. War unifies a people against a common foe, whereas commerce encourages people to forge networks that extend beyond tribal borders. Indeed, this attitude permeated science policy thinking well into the twentieth century, given the cycles of mobilization and redeployment of scientific effort that have marked the periods before and after wars (Reingold 1994). According to this model, the threat of war stabilizes society, which over time may intensify into a totalitarian mindset, whereby individual differences are completely submerged into a single cultural identity. The

removal of external threat opens the door to a more outward policy that by acquiring cumulative advantage over many exchanges may evolve into imperialism. No imperialist pretends that, say, Christian values are "natural" to non-Western cultures, only that these cultures are likely to be benefit from adopting them. Whether this is because Christianity satisfies a standing need or induces such a need by virtue of its associated consequences is a matter of indifference to the imperialist.

In the STS literature, actor–network theory, most closely linked with Latour, canonizes the imperialist's indifference as constructivist research methodology. Thus, constructivists tend to be insensitive to pre-existent ("structural" or "historical") power relations between the parties to an exchange that may overdetermine the outcome of the ensuing negotiations, as in British imperial encounters with African natives in the 1930s and 1940s. In contrast, group-grid theory, as adapted by Bloor from Douglas, is explicitly concerned with the conditions under which social identity is stabilized. Consequently, "outsiders" only figure as candidate insiders, not as potential subsumers of the entire social order. Here, then, are the complementary weaknesses of constructivism and relativism that appear as the "repressed" sides of their positions. Constructivists are haunted by the idea of a social and/or material limit to "free exchange," which nowadays is often expressed as the need to incorporate "ecology" into social theory (Murdoch 2001). For their part, relativists face the prospect of their normative orders imploding as anomalies accumulate without formal resolution. This situation is captured by the Kuhnian concept of "revolution" in science, which, as we shall see in the next section, raises additional worries about the philosophical and historical basis for STS.

Why is STS so Antiphilosophical and Ahistorical? The Kuhnian Legacy to STS

So far, we have set down two major pieces of the philosophical puzzle that is STS. The first is STS's failure to appreciate the paradoxical consequences of the reversibility of the means–ends reversal of science's place in modern society. The second is the field's guiding philosophical dialectic, which is not between realism and antirealism, but between relativist and constructivist versions of antirealism. These two pieces are combined with some intellectually insidious consequences in the field's selective use of Thomas Kuhn's legacy. In sum, STS's sociological sensibility is the result of Kuhn's historical sensibility minus his philosophical sensibility, or as I see it, what is bad about *The Structure of Scientific Revolutions* ([1962] 1996) without what is good about it – and certainly what Kuhn himself thought had justified his account of science. Since I shall be dwelling on the bad features of Kuhn's account, let me begin by epitomizing what was good about it, though admittedly Kuhn himself was not its most ardent publicist.

The legacy neglected: Science is only sometimes pure inquiry

Kuhn had a clear sense of the different social functions that science might perform, but he chose to dwell on only one of them – its function as organized inquiry. His justification is worth recalling, especially since it was made in response to the question why he had not altered his account, given twentieth-century developments in science:

> I see no reason to suppose that the things I think I have learned about the nature of *knowledge* are going to be disturbed by the need to change the theory of *science*. I could be all wrong with respect both to science and to the nature of knowledge, but I would make this separation to explain why I'm less concerned about the question, "Is science changing?" than I might be if studying the nature of science weren't in the first instance simply a way of looking at the picture of knowledge. (Kuhn in Sigurdsson 1990:24)

The suggestion here is that at some point in its history, the social function of science may turn (or have turned) out to be a factor of production or an instrument of governance, rather than a search for knowledge. In that case, science drops out of the normative horizons of Kuhn's model. While science might continue to produce truths on a reliable basis, these truths would be produced under social conditions that preclude science from constituting autonomous inquiry. Fields as otherwise opposed to each other as analytic epistemology and STS are united in their failure to appreciate this subtle point.

Contemporary analytic epistemology, partly under the influence of W. V. O. Quine, is in a self-styled "naturalistic" phase that is preoccupied with distinguishing forms of inquiry that on a regular basis are "truth-oriented" and "nontruth-oriented." Only the former are deemed "reliable" (Goldman 1999). This project aims to prove two things at once: first, that we are "always already" seeking the truth whenever we are engaged in a putatively rational activity; second, that we can (at least in principle) determine just how truth-oriented such an activity is. Given the above quote from Kuhn, it would seem that this preoccupation with reliability may allow science to win all the epistemological battles, yet lose the larger axiological war, if it turns out that science's reliably produced truths are done in the aid of interests that pervert the overall course of inquiry. One example that would have been vivid for Kuhn was the capture of physics by a military-industrial complex keen on perfecting its understanding of the payload delivery of top secret weapons.

Thus, we need to distinguish between an interest in pursuing knowledge as *an end in itself* and *a prerequisite for pursuing other ends*. The former captures Kuhn's sensibility, the latter the naturalized epistemologist's. To be sure, this is a delicate distinction to maintain in practice, as the naturalist can easily be read as legitimating the "serving two masters" scenario of Cold War science policy. Moreover, the situation has not been helped by STS's constructivist gloss, which

denies a clear *a priori* distinction between satisfying the needs of knowledge and other needs. Such a distinction is simply constructed *ex post facto,* which may prove especially convenient if one does not need or want to take responsibility for all of the consequences of one's actions. Indeed, scientists have been generally quick to take credit for the pathologies they help cure but not the problems they help cause.

The legacy uncritically adopted: Kuhn's syncretism

Kuhn's historical sensibility is best described as *syncretistic*: He combines features from different periods in the history of science as if they had been always present together (Fuller 2000b:195ff.). Thus, it is easy to find historical examples that fit elements of his account of scientific practice and the stages through which it allegedly passes, but impossible to find a historically extended episode that exemplifies his entire account. Not surprisingly, Kuhn's historical examples in *Structure* are not much more elaborated than those of other historically minded philosophers of science from his time, such as Imre Lakatos or Larry Laudan. For all of them, history provides snapshots of a process that is presumed to be explained by the theory on behalf of which the examples have been mobilized. Thus, Kuhn tells us that the accumulation of unsolved anomalies eventuates in a crisis for a paradigm, but we are never presented with the social processes by which members of an actual scientific community came to recognize this transition.

To be sure, some STS practitioners have tried to provide flesh for these Kuhnian bones, but significantly the scale of the scientific communities they have studied is much reduced from the original sociological pretensions of "paradigm," which was supposed to refer to an entire discipline or subdiscipline, not a mere research team (e.g., Pickering 1984). And while few STS practitioners openly endorse the model of scientific change presented in *Structure*, little effort has been made to find a replacement. Moreover, there is no reason to think that an aggregation of "micro-Kuhnian" stories of the sort told in Pickering (1984) will add to Kuhn's own grand narrative. On the contrary, it may simply render science more susceptible to the "ends-turned-into-means" reversal discussed above.

Syncretism in historical writing is associated with what rhetoricians call *chiasmus*, an exchange of properties that serves to blur the difference between the entities from which the properties are drawn. In *Thomas Kuhn: A Philosophical History for Our Times* (Fuller 2000b), I discussed this syncretistic strategy in the context of Kuhn's translation of the philosophical problem of "rationality" in science. In a nutshell, the history of modern science has thrown up two ways of conceptualizing the difference between the "rational" and "irrational," each associated with a philosophical standard-bearer, which I called *Enlightenment* and *Positivist* to mark their origins in eighteenth and nineteenth century thought, respectively (Fuller 2000b:289–94). Kuhn's innovation was to recombine these two distinctions into

Table 9.2   *Philosophy of science before Kuhn*

Philosophical conceptions of rationality	Image of the Rational	Image of the Irrational
Enlightenment	Critique	Tradition
Positivist	Method	Disorder

Table 9.3   *Philosophy of science after Kuhn*

Phases of Science	Normal Science (a.k.a "rational")	Revolutionary Science (a.k.a "irrational")
Research practice	Method	Critique
Social backdrop	Tradition	Disorder

alternating phases of his model of scientific change: normal and revolutionary. The result neutralized the normatively charged language in which both the Enlightenment and the positivist accounts of rationality had been cast. Thus, the philosophy of science lost its scary "demarcationist" image, which seemed ever on the lookout for charlatans. Indeed, Kuhn's model was sufficiently user-friendly to be adopted by social scientists and other inveterate "pseudoscientists" as a recipe for enhancing their epistemic status.

Comparing tables 9.2 and 9.3, we see that each Kuhnian phase contains elements of both historical models of scientific change. Specifically, Kuhn combined the tradition–method diagonal into the concept of normal science and the critique–disorder diagonal into the concept of revolutionary science. Moreover, since Kuhn made it clear that normal science was typical and revolutionary science aberrant, he was widely read as promoting normal science as a new model of rationality, with revolutionary science implicitly standing for irrationality. A sign of this shift was that the most prominent post-Kuhnian spokesperson for "science criticism" came to be the self-styled epistemological anarchist, Paul Feyerabend, whereas it had been previously that Enlightenment stalwart, Karl Popper. The legacy of the Kuhnian chiasmus continues in the tendency (e.g., Habermas, Mary Hesse) to assimilate all social epistemologies of science to the conformism of the "consensus theory of truth," including those (e.g., C. S. Peirce's, Donald Campbell's, my own) that clearly place disputation and competition at the heart of the scientific enterprise.

Charitable readers may wish to interpret Kuhn's model of scientific change as a Weberian "ideal type" that strategically highlights certain features that a wide range of cases exhibit to varying degrees but none exhibits completely. However,

this charitable gesture is problematic because, as we have seen, Kuhn's model is primarily normative in intent. Thus, philosophers have generally rejected the model, not for its lack of empirical adequacy or historical verisimilitude, but for its supposed failure to give rationalism and realism their due as metanorms of scientific inquiry. Similarly, social scientists have embraced the model, not because it captured the actual histories of their disciplines, but because it seemed to offer a prescription for converting their disciplines into proper sciences. In this respect, Kuhn's model has been received in much the same way as social scientists have received *homo economicus*. Most sociologists continue to reject *homo economicus* less for its failure to predict social behavior than for the "asocial" image of people it implies. In contrast, economists treat the model as a standard against which actual economies may be judged and, in the right policy context, improved. It was precisely these normatively inspired appeals to *homo economicus* that Weber's conception of ideal type was meant to *oppose*.

But more importantly, it is problematic to treat Kuhn's model of scientific change as an ideal type because *Structure*'s syncretism combines aspects of different periods in the history of science that have sociologically excluded each other, thereby rendering his model empirically incoherent. Kuhn assumes that changes in the size and shape of the social structure of science have not altered the cognitive motivation for doing science. Moreover, Kuhn's presumption comes with a twist: The likes of Newton and Maxwell are assigned today's motivation rather than today's scientists being assigned their motivation. Thus, virtually all of Kuhn's historical examples are taken from about 300 years in the history of the physical sciences in Europe, roughly bounded by 1620 and 1920, yet the terms he uses to discuss them – the language of "paradigms" – starts to have currency in the period immediately following that. At stake here are the background social conditions under which science is regarded as a self-organizing community of inquirers with sufficient control over the means of knowledge production to enjoy sovereignty over who counts as a scientist, what counts as a valid knowledge claim and an appropriate research direction.

Tables 9.4 and 9.5 present the resulting syncretism, whereby features of two successive stages in the institutional history of modern science, "little" and "big" science, have been recombined to produce the alternating paradigmatic phases of normal and revolutionary science, driven by "internal" and "external" factors, respectively. As we shall see, it would be a mistake to see the impact of Kuhn's syncretism as confined to philosophical conceptions of rationality. If anything, it left a stronger impression on sociological conceptions of scientific autonomy.

In the period from which Kuhn's examples are drawn, the economic demands and political ambitions of science were relatively modest. For example, the Charter of the Royal Society of London granted an exclusive license to practice science in exchange for loyalty to the Crown. The Royal Society did not depend on the Crown for funding its activities, nor did the Crown expect that the Royal Society would solve all of Britain's political and economic problems. Of course, government expectations (and hence funding) were raised from time to time, but in the

Table 9.4   *Sociology of science before Kuhn*

Sociological conditions for autonomy	"Little science" (before the military-industrial complex)	"Big science" (after the military-industrial complex)
Micro foundations	Individual curiosity	Technical problems
Macro foundations	Self-organized scientific societies	State-sponsored research and training programs

Table 9.5   *Sociology of science after Kuhn*

Paradigmatic phases of science	Normal science (a.k.a. "internal")	Revolutionary science (a.k.a. "external")
Micro foundations	Technical problems	Individual curiosity
Macro foundations	Self-organized scientific societies	State-sponsored research and training programs

period from which Kuhn's model is drawn, scientific activity was organized and funded largely by private means and, in that sense, autonomous from the larger political and economic forces in society.

Moreover, the experimentalists and naturalists associated with the original scientific institutions tended to be independent spirits who pursued a wide variety of research agendas and metaphysical orientations. What united them was agreement on the means of peer appraisal. This view of scientific inquiry as a couplet of diverse inspirations and common evaluations was eventually canonized in the philosophical literature as the distinction between the contexts of "discovery" and "justification." In telling contrast, Kuhn disavowed the distinction, especially its implication that the impulse to discover comes from *outside* a duly constituted scientific paradigm. For their part, it is unlikely that the early members of the Royal Society would have regarded their efforts as "puzzle-solving" contributions to Kuhnian "normal science."

Here it is worth stressing that Kuhn's vision is equally opposed to orientations to science as otherwise different as Michael Polanyi's (1958) "mutual trust" and Percy Bridgman's "operationalism." Nowadays, Polanyi's sense of trust is interpreted as based on the professional competence of fellow scientists, such that colleagues "trust" each other's research accounts unless there is reason to suspect error or fraud. However, Polanyi was ultimately harking back to a monastic ideal that implied the ultimate irrationality (or unrationalizability) of the exact course that the sincere inquirer would need to take in order to arrive at God. A

self-recognized community of the faithful would nevertheless be able to determine whether inquirers had reached their destination. Bridgman's operationalism updated this perspective for an age that had begun to transfer its faith in human judgment to mechanical measures. Yet, here too the source of the concepts that were in need of operationalization was that mysterious prepositivist world of "metaphysics," the tangible fruits of which operationalism would ultimately reveal.

Remnants of Polanyi's monastic ideal can be found today in popular science writing that motivates the path of inquiry by appealing to "curiosity" as a secular version of spiritual longing. Yet, curiosity fails to capture the microfoundations for the autonomy found in Kuhn's conception of normal science. *Structure* presents a microlevel account much closer to twentieth-century trends in the specialization and massification of scientific inquiry. Here scientific autonomy is underwritten by a social contract between science and the state that began with the German strategy in the Franco-Prussian War of 1870 and came to fruition in United States science policy in the Cold War. This period corresponds to the emergence of the military-industrial complex that marks the transition from little to big science in table 9.4. Thus, peer review processes have been extended from the validation of research results to the very entitlement to conduct research.

Scientists now depend on peers not only for final publication but also prior accreditation and funding. They are no longer curious "all-rounders" but tunnel-visioned technicians focused on problems of little relevance outside a widely shared paradigm. For this reason, followers of Karl Popper came to regard the workings of a Kuhnian paradigm as an expression of the herd mentality. Yet, no one seems to have noticed that all of Kuhn's examples come from *before* the onset of this objectionable microsociology. This oversight reflects the relative continuity of the language of scientific justification across major historic changes in science's scale and scope (Fuller 2000a:28–45). In other words, even though scientists are now doing radically different things in radically different contexts from three centuries ago, they nevertheless draw on largely the same linguistic resources to justify their knowledge claims. Not surprisingly, then, philosophers continue to rely on seventeenth-century science to illustrate basic points about the scientific method. And even the famous four sociological norms of science proposed by Robert Merton – universalism, communalism, disinterestedness, organized skepticism – were plucked from the pronouncements of philosophers and scientists over several centuries, without reference to particular scientific practices.

In comparative morphology, a distinction is drawn between *homologues* and *analogues* that may prove useful in this context (Runciman 1998:8). Biologists have observed, on the one hand, similar-looking physical structures that perform different functions in different species (homologues); on the other, recurrent organic functions that are performed by different structures in different species (analogues). The fact that all homologues are not analogues helped to undermine the idea of a universal blueprint for life that might have been used by a divine creator. We have yet to reach a comparable state of awareness in our understanding

of science, whereby the multiple research contexts in which "the scientific method" has been invoked would serve to undermine the method's univocality. To some extent, STS has been sensitive to this point, especially by drawing attention to the discrepancy between what scientists do and what they say they do (Knorr-Cetina 1981, Gilbert and Mulkay 1984). However, syncretism sets in, once STSers follow Kuhn's example, fortified by that protean creature, the "situated reasoner" of microsociology. The result is the rather mysterious image of scientists organized into discrete communities (a.k.a. "incommensurable paradigms") spread over time and space that nevertheless manage to interpret the same norms in ways that have radically different consequences for their communities and the knowledge they produce (Doppelt 1978). In what sense, then, can these norms remain "the same"?

Why is STS so Controversial? The (Pseudo-)Legacy of the Sociology of Knowledge

The "sociology of knowledge" has been a lightning rod for controversy ever since the field started to travel under that name in the interwar years of the twentieth century (Frisby 1992). Intellectuals who welcomed social scientific explanations, evaluations, and improvements of social life in general have typically balked at the consequences of those approaches applied to themselves. Observant of this history, Ian Hacking has established himself as the most prominent philosopher to assume the role of "honest broker" in what has come to be known as the Science Wars, an ongoing transatlantic battle to define the social function of science in the post-Cold War era that has updated the "two cultures" problem for our time (Gross and Levitt 1994, Ross 1996, Fuller 2000b:354–65). However, the issue is not as neatly reducible as Hacking thinks to scientists' resistance to the application of scientific scrutiny to their own practices. On the contrary, I shall argue that STS probably suffers from a greater lack of reflexivity than its scientific opponents.

Despite considerable ambiguity in presentation, Hacking (1999) suggests that social constructivism has succeeded in unmasking the pretensions of certain elite scientists who would like science to do more political work than is strictly warranted. At the same time, he also believes that the political and philosophical functions of social constructivism need to be kept separate: The former he calls "unmasking" (or "demystifying") and the latter "refuting." (Hacking believes that a few exemplary works, such as MacKenzie 1990, combine both.) Thus, the philosophical currency in which the Science Wars is traded – debates over contingency vs. necessity, nominalism vs. realism, externalism vs. internalism – is likely to remain forever inconclusive. But that does not deny that social constructivism has drawn attention to oppressive features of science's grip on society. Hacking's message then is that science's negative social functions should be addressed even

if social constructivism cannot provide a knock-down epistemological critique of science.

There is much to be said for Hacking's even-handedness. He shows that scientists mistakenly presume that a sociological unmasking of a scientific practice is tantamount to a refutation of the practice's underlying knowledge claims. Yet, Hacking's account also implies an unmasking of the unmaskers, since it would appear that social constructivism's radical philosophical packaging is a diversion from its straightforward empirical claim that certain scientifically inspired ideas have justified oppressive social practices. But this immediately raises the question of why abstract philosophical distinctions should have defined the terms of the Science Wars in the first place. On Hacking's telling, they should not have. Indeed, Hacking himself tends to treat the philosophical crossfire as little more than sophisticated name-calling.

Here lies the blindspot in Hacking's account. It is traceable to his generally antitheoretical approach to the philosophy of science (Hacking 1983). More like social constructivists than their philosophical and scientific antagonists, Hacking fixates on the status of the laboratory sciences as "stable material practices" to such an extent that he ignores the rhetorical significance of these practices commanding the relevant philosophical labels of "rational," "real," "objective," and so forth. Yet, command over this rhetoric remains the primary mode of social legitimation in *all* the sciences. Otherwise, why *should* intelligent lay people see a connection between the stability of laboratory phenomena and the stability of everyday social life (Fuller 1997:40–79)? How many people are familiar with the stages by which a laboratory finding is stabilized into a nugget of knowledge or a reliable technique? Were the numbers large, then Latour (1987) would not have caused such a stir. In this respect, natural scientists concerned about the consequences of their fields being seen as "constructed" have had a better grasp of the sociology of knowledge informing their situation than their sociological opponents. Scientists are not just personally offended by social constructivism: They are justifiably worried about the fate of their endeavors.

My perspective in the Science Wars can be captured precisely in the following paradox: I agree with the scientists' sociology of knowledge and the sociologists' philosophy of science. In other words, social constructivist accounts of science are largely correct, but science's high epistemic status in contemporary society depends on the principled rejection of such accounts. Thus, the social epistemology of science must ask how science is to be legitimated once social constructivist accounts are widely *accepted* (Fuller 1992). Can science, like religion, survive in a demystified form? The Kuhnian legacy ill equips us to address this question because practitioners of a scientific paradigm are licensed to discuss the overall ends of their inquiries only once they have started to fail on their own terms, and hence enter a "crisis."

This point applies with a vengeance to STS's own desire for academic respectability. It has led the field to embrace the Kuhnian image of normal science, which has in turn disabled it from seeing the reflexive consequences of its own

practices on the larger society. When Karl Mannheim (1940) originally proposed that a "free-floating intelligentsia" would be the ideal unmaskers, the last thing he imagined was that they would sell their critical independence for the protective coloration afforded by a paradigm, as arguably STS has done (Fuller 2000b:354–65).

Hacking draws the distinction between "unmasking" and "refuting" from Mannheim's original demarcation of the sociology of knowledge from epistemology:

> [The unmasking] turn of mind . . . does not seek to refute, negate, or call in doubt certain ideas, but rather to *disintegrate* them, and that in such a way that the whole world outlook of a social stratum becomes disintegrated at the same time. We must pay attention, at this point, to the phenomenological distinction between "denying the truth" of an idea, and "determining the function" it exercises. In denying the truth of an idea, I still presuppose it as a "thesis" and thus put myself upon the theoretical (and nothing but theoretical) basis as the one on which the idea is constituted. In casting doubt upon the "idea," I still think within the same categorical pattern as the one in which it has its being. But when I do not even raise the question (or at least when I do not make this question the burden of my argument) whether what the idea asserts is true, but consider it merely in terms of the *extra-theoretical function* it serves, then, and only then, do I achieve an "unmasking" which in fact represents no theoretical refutation but the destruction of the practical effectiveness of these ideas. (Mannheim [1925] 1952:140)

Hacking follows most commentators in interpreting Mannheim as juxtaposing two equally valid methods for the evaluation of knowledge claims, with "unmasking" and "refuting" corresponding to an "external" and "internal" approach. The question for Hacking, then, is whether the former requires the latter to complete its task. (Hacking says no.) However, Mannheim himself ([1929] 1936) did not regard the two methods as equally valid, especially in an age of ideological conflict. Refuting the knowledge claims of an ideology that is not one's own is both too easy and too difficult. For example, it is relatively easy to refute Marxist claims about capitalist exploitation if one does not presuppose that the capitalist extracts "surplus value" from the worker; but once the presupposition is granted, it is virtually impossible to refute the Marxist. Under the circumstances, the usual epistemological tools will not work. Consequently, the sociology of knowledge is needed to reveal the background conditions that have maintained the set of presuppositions that constitute an ideological framework.

Like a wide range of thinkers of the interwar period concerned with "frameworks" in the broadest sense – ranging from Rudolf Carnap to Robin Collingwood – Mannheim assumed that an ideology's presuppositions are logically independent of each other but combined together for historically contingent reasons. The sociologist thus unmasks the ideology by identifying the relevant background reasons, thereby providing grounds for "disintegrating" the ideology's particular combination of presuppositions. This unmasking is often a diagnostic reading of the ideology, which may focus on stock examples ("paradigms" in that sense)

used to motivate the ideology's arguments. For example, the *prima facie* plausibility of the Marxist ideological framework trades on our tendency to imagine "labor" in terms of the nineteenth-century factory worker, say, drawn from the pages of Charles Dickens. In a sense, Marxism's presuppositions are meant to address different aspects of this striking image. Perhaps the relevant mental associations were grounded in the reality experienced by Karl Marx and his original followers, but it is no longer clear that it captures the experience of labor today. (Attentive readers will note that my earlier attempt to disentangle Kuhn's syncretistic account of the history of science follows in a similar mold.)

When Mannheim boldly suggested that deeply held knowledge claims cannot be evaluated simply at their level of expression, Karl Popper notoriously demonized the sociology of knowledge alongside psychoanalysis and, indeed, Marxism, as part of the ongoing authoritarian subversion of rational thought (Popper [1945] 1966:ch. 23). However, for Popper, rationality was a rather modest thing – no more than the product of an irrational choice to be sensitive to error and to eliminate it from future action. Popper's objection to Mannheim was not that he openly promoted irrationality, but that he pretended to a higher form of rationality that only served to undermine Popper's own modest version, as Mannheim restricted the capacity for criticism to those adept at the relevant unmasking techniques. While Popper scored a lasting rhetorical victory over Mannheim, Popper himself held that if ideologues are not willing to submit their knowledge expressed claims to possible refutation, then little could be done about it – since rationality is, in the final analysis, an irrational choice.

Abstracted from the Weimar context in which Mannheim and Popper first locked horns, we can see that the unmasker (Mannheim) engages more of human psychology in his sociologically comprehensive conception of rationality than the refuter (Popper) in his narrowly philosophical conception. In particular, the unmasker presumes that the mind is a house divided against itself, so that to address knowledge claims simply as expressed is to ignore the conflicting motives that remain unexpressed in the claims. Moreover, the unmasker presumes that the psychic balance of power can be redistributed under the right environmental conditions. For example, Mannheim seemed to anticipate the idea of the "consensus conference," which aims to resolve the ideological differences of the participants by having them focus on a common task, the design of policy guidelines that would be binding on all concerned (Fuller 2000a:11–19, cf. Hacohen 2000:542–3). If someone like Popper interprets this as manipulation, it may be because he presumes that people should alter their beliefs only when they are explicitly challenged, not when the conditions that first made the beliefs plausible have changed.

Mannheim was alive to what Jon Elster (1999:341) has called the "civilizing force of hypocrisy," whereby people mask their beliefs in contexts where the expression of those beliefs is immaterial and perhaps even detrimental to their interests. Moreover, if these contexts are institutionalized and people's interests have been regularly served by them, they may come to adopt the mask as their

true self, and thereby learn to want different things. Indeed, the deliberative character of civic republican democracies aims to facilitate this psychic transformation by providing incentives for people to think of their own personal interests as best served by promoting the interests of others (Pettit 1997, cf. Fuller 2000a).

In this respect, Mannheim wished to accomplish by design what the capitalist marketplace had clearly failed to achieve by studied indirection, namely, the conversion of private vices to public virtue. In more strictly social psychological terms, Mannheim supposed that in a specially constructed decision-making environment, people could be made to adapt their preferences so as to overcome the divisive anchoring effects of their disparate histories. Popper believed that the implementation of Mannheim's proposal would do more harm than good, whereas more recent defenders of deliberative democracy, such as Habermas, often write as if it were possible to achieve Mannheim's end within already existing political structures. For its part, STS has largely failed to take up normative reconstruction as the positive side of the project that begins by unmasking ideologies that cannot be directly refuted.

The Punchline: STS has Produced a Sociology of Science that is No Longer a Sociology of Knowledge

The sociology of knowledge is largely a tale of two traditions, based on proximity of knowers in *space* and *time*. The former tradition studies how people of different origins who are concentrated in one space acquire a common mindset over time, whereas the latter studies how people dispersed over a wide space retain a common mindset by virtue of having been born at roughly the same time. The first strategy is associated with the French tradition, exemplified by Lucien Lévy-Bruhl and Émile Durkheim, while the second strategy is associated with the German tradition, exemplified by Wilhelm Dilthey and Karl Mannheim. An assumption common to both traditions is that collective patterns of thought are constituted as collective acts of resistance to the environment. The exact nature of the resistance is explainable by the spatiotemporal arrangement of the people concerned. Religious movements and political parties are thus obvious targets for the sociologist of knowledge. A Durkheimian might show how religious rituals enable the faithful to escape the limitations of their material conditions and stand up to potential oppressors, while a Mannheimian might show how a persistent ideology enables the experience of a particular generation to define the parameters of policy for the entire society. In both cases, the sociology of knowledge is meant to complement, not replace, the psychology of normal thought processes through which individuals adapt to a world that is largely not of their own making.

The sociology of knowledge finds itself in a peculiar normative position. Is its object of inquiry – organized resistance to reality – to be valorized or pathologized?

Are religions and ideologies instances of thought operating at a standard, as it were, "above" or "below" that of everyday reasoning? Are they vocations or manias? The two sociology of knowledge traditions are themselves ambivalent on this point, and the addition of science as a potential object of inquiry has only complicated the matter. To be sure, some classical sociologists, notably Vilfredo Pareto, were very clear about the "nonlogical" status of the forms of knowledge eligible for sociological scrutiny. Unsurprisingly, philosophers of science from Hans Reichenbach to Larry Laudan have followed Pareto's lead by dividing the labor between the epistemology and the sociology of knowledge along the border of the "rational" and the "irrational." Interestingly, with the onset of World War II, Mannheim himself shifted from this view to one closer to American pragmatism, which would have judgments about the rationality of science turn – as they would of any belief system – on the consequences of actions taken in its name. This shift seeded C. Wright Mills' political critique of science as a military-industrial complex during the Cold War (Nelson 1995).

However, science is not so easily assimilated to the pragmatist model, since it is not obvious that science has had markedly rational consequences outside the controlled mental and physical spaces – say, seminars and laboratories – in which its distinctive forms of knowledge have been produced. After all, if judgments about the rationality of religion and politics were similarly contained to, say, theological disputation and parliamentary debate, would judgments of their rationality suffer significantly by comparison with science? Here the extended legacy of the Vienna Circle – including Carnap, Popper, and to a large extent Kuhn – provides an instructive point of reference. In most general terms, the Vienna Circle regarded conceptual frameworks in a generally Kantian fashion. Thus, the "external" question of which framework should be selected was treated as "practical," whereas the "internal" question of what follows from a selected framework was treated as "theoretical." This led to what we now recognize as an "asymmetrical" treatment of framework origins and consequences. The former represented an existential choice, the latter was determined by logic.

Mannheim departed from this legacy by interpreting the "consequences" of such a framework in existential, not logical, terms. This led him before 1940 to exclude the natural sciences and mathematics from the purview of the sociology of knowledge. But after 1940, he began to include these disciplines – at the cost of keeping their rationality an empirically open question. After all, the tradition-ally "purest" of the sciences – mathematics and physics – were integral to what were shaping up to be some of the most oppressive and destructive episodes in human history. In other words, the original turn to "symmetry" in the sociology of knowledge, otherwise so closely associated with the studied neutrality of STS research, came less from an open-mindedness to alternative research trajectories than an openly critical attitude toward the dominant trajectory.

However, the overall history of STS has consisted of the gradual removal of the normative sting of Mannheim's later sociology of knowledge, largely by attenuating science's status as a form of organized inquiry that offers collective resistance

to the world in which it finds itself. Thus, we find three successive solutions to the "problem of scientific rationality": (1) *relativize* it, (2) *bracket* it, and (3) *eliminate* it:

1. Science must be seen as organized resistance, much like a religious or political movement, and hence its rationality seen exclusively in terms of its internal workings. Ludwick Fleck's (1979) pioneering application of the French sociology of knowledge tradition to biomedical "thought collectives," and Kuhn's generalized model of scientific change, are cases in point.

2. The entire question of scientific rationality needs to be bracketed, specifically by presuming that apparent differences in the rationality of, say, science and religion or politics – or even true science and false science – is an artifact of whether one has chosen an evaluative framework internal or external to the practice in question. Otherwise, there is no empirical reason for treating one set of practices as more rational than another set. This is the once infamous but now widely accepted symmetry principle of the "Strong Program" of the Edinburgh School of STS (Bloor 1976).

3. Most recently has come the challenge of the Paris School to the idea that science can be clearly separated from the rest of society, especially once one applies an "external" sense of consequences. If it cannot, then epistemic rationality collapses into a special case of political rationality in an undifferentiated world of "techno-science." Those who maintain the longest networks for the longest time simply come to be defined as both the most knowledgeable *and* most powerful, with the former predicate used to explain the latter by obscuring the local struggles faced along the way (Latour 1987).

By the time we reach (3), it may be legitimate to ask whether STS's sociology of science is any longer a sociology of *knowledge*. Kuhn would certainly not have thought so, since STS does not recognize that knowledge has a distinct normative character that could channel the development of scientific institutions. To appreciate what the sociology of science looks like once it has lost its moorings in the sociology of knowledge, one need only turn to the emergence of "knowledge management" as a growth area in business schools (Fuller 2001). Knowledge managers perversely combine the classical philosophical idea that knowledge is given to "multiple instantiations" and the neoclassical economic idea that rational agents are "constrained optimizers." They conclude that knowledge is valuable only on a "need to know" basis by whatever organizational arrangement happens to be most efficient to the task at hand. Of course, philosophers and economists themselves have tended to regard knowledge as a rather special good – called "public," "collective," or "ethereal" – that resists such a reductive move. Nevertheless, this traditional normative barrier has begun to come down, especially as increasing numbers of STS researchers find themselves on short-term academic contracts that make them beholden to nonacademic "clients" for continued support. Thus, in the knowledge management literature, a university is the paradigm

case of a "dumb organization" that is so high on "intellectual capital" but low on "structural capital" that managers cannot fully capture the fruits of their employees' labors (Stewart 1997). Here the "smart organization" is exemplified by a fast food franchise whose low intellectual capital is complemented by the high structural capital supplied by scientific managers.

This is hardly the ideal social setting for reviving the positive side of Mannheim's project, which would develop institutions for incorporating an ever larger proportion of the populace in collective deliberation over the ends of knowledge. I have developed this possibility in terms of a civic republican approach to knowledge policy (Fuller 2000a, 2001:ch. 4). At the moment, however, social forces tend to be going in reverse, namely, toward accommodating what the populace already believes into already existing institutions of knowledge production. Only when STS formally recognizes the difference between these alternative trajectories, will it become a genuinely progressive agent in the ongoing social transformation of science and technology.

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Part III

Problematics

"See Also Literary Criticism": Social Science Between Fact and Figures

Hans Kellner

The (Anti)Social Sciences

A recent visit to a large national bookstore helped place for me the current situation of the social sciences. On the shelf marked "Sociology" was taped a small sign, "See Also Literary Criticism." This juxtaposition undid a basic proposition of nineteenth-century thought, the splitting of a formal knowledge of human affairs away from the literary and the rhetorical. At the same time it reinforced in an informal but highly pragmatic way the dilemma faced by the social sciences: insofar as they aspire to the status of sciences, the social sciences must work with tools that disavow their conventionality as tools. In so doing, these sciences cease to be social and become incoherent. To be a science for the last four centuries has been to separate one's practice from the give-and-take of social action and historical change. The scientific method, however described, is a challenge to those forms of knowledge (which some philosophers do not see as knowledge at all) that are the result of the conflict and temporary consensus of a society of persons with competing interests.

   Of course, the sociality of the social sciences has been repressed ever since they took an academic shape a century ago. The goal was a science of, in the sense of "about," the social, not a science known through its own social forms. At each of the points along the spectrum of the social sciences, the presumed purpose of the endeavor was an improved knowledge of an object in the world; these were discursive objects such as the events of the past, social formations, economic transactions, political forces. No one has ever seen any one of these "things" in the absence of the description that names it within a stipulated frame of meaning: no happening can be, for example, a historical event, or an event at all, in the absence of a complex and tacit social agreement about what is historical and what is to be accepted as the structure of a historical event, its inside and outside, so to

speak. Yet these phenomena survive as objects of study because of an implicit agreement without which the enterprises known as the social sciences in their present forms would collapse.

A part of the agreement that made social science possible was to keep tacit and untheorized the meanings that escaped the limits of methodology. For to theorize the grounds of possibility for a social science would surely have the effect of shifting attention away from reality – and the social sciences in their existing forms have little claim to attention or to resources if they lose their "realism," in the sense of addressing something that is neither the science itself nor derived from terms implicit in the tools which it uses, by acknowledging the depth of their debt to the forms of representation of reality, forms that always threaten to reveal themselves as *more* than representations, indeed as the actually constituting forces behind any claim to comprehension of the real. Access to the real, then, has depended upon suppression of the tools that seemed to provide that access, above all, of rhetoric and the poetic creation of meaning. To apprehend, for example, the Holocaust as a sociologist would is undoubtedly different from apprehending it as a political scientist, historian, or anthropologist would. But all of these social sciences are in accord in resisting the suggestion that language and the shifting world of human opinion create the "real" that those disciplines hope to confront and account for. The goal of each has been to produce a version of an event like the Holocaust that will stand as true, independent of the tides of opinion. They must, in short, produce a compelling argument without admitting that it is an argument, a persuasive action aimed at a particular audience for a particular purpose.

The so-called modernist understanding of the world that came into existence in the seventeenth century, and crystallized as a way of understanding human events in the nineteenth century, is a stable three-part structure comprising the observer, the thing observed, and the tool of observation. Each part of this triple structure is presumed to be an actual thing, separable from the others and possessing its own identity. The world and the things in the world are fixed things, and not dependent on either the tools used to observe and represent them or on the observer. The world exists apart from any perception of it. As with the telescope (eye–instrument–world) or the sea journey (port of departure–ship–destination), the model conjoins observant reason with the existent world via a mediating signifying system that does not interfere with either reason or the world. It is essentially identical with them both because it works the way they do (Reiss 1982:31). In other words, *cogito ergo sum*. The signifying system, language, will present the world to us in increasing detail, and will interpret it properly for us through its very form (Reiss 1982:41).

If this is taken to be the structure of the world, the structure of the real, the discreteness and individuality of the mind of the observer is generated by the structure; the observer (or the subject) is what experiences the world, and thus knows it. From this, of course, follows the ethics that places the individual at the center of a system of rights and responsibilities that define humanity itself. The

thing observed is the world "out there," defined by its thinghood. The existence of things is no more problematic than the existence of individual minds that experience them. In this scheme, the world has joints at which it may be cut in pieces, just as the human individual exists as a namable, ethical, and experiential entity.

There is nothing particularly modern about the conjoining of the observer and the thing observed. What is new in this structure is the central position of something that is neither observer nor observed, namely the tool of observation itself. The epitome of the structure is the image of Galileo gazing at the heavens through his telescope. It is the telescope that is new, modern, but also modern is the trust that is placed in what the telescope reveals. Metaphorically, the telescope stands here for the word, particularly the conceptual word, which may also be viewed as a modern invention. For the older notion of the word, an arbitrary sign equivalent to a thing, has given way to the modern word, a telescope that will afford a vision of the object. The modern world is not a manmade world, but it is knowable (Reiss 1982:54).

The social sciences took their present shape at the end of the nineteenth century when the fascination with history and a relativism bred by that fascination brought about the division into a naturalistic historicism, where laws ground human events, and a metaphysical historicism, where a rational plan governs the whole. The former view leads to a sociology, the latter to philosophies like Marxism. A third attitude toward human events, however, recognized both of these – indeed, any approach to the thing itself, actual human experience – as ideological. This third approach was an aesthetic social vision, and it took as its focus of study not the events themselves but rather their representations. Aesthetic historicism believes, for example, that history is a function of historiography.

The aesthetic standpoint calls into question the purpose of history, and of the human sciences generally, because it maintains that they cannot deliver on their promises. The truth of human affairs is what human beings have made. It is thus a created, never a found, thing, fashioned from the changing materials of life. Vico ([1725] 1984) had presented a version of this sensibility in opposition to the positivism of Enlightenment thought. His idea was that human creations must serve human needs, and these included representations of human affairs. If a version of human actions was oppressive, frustrating to hopes and initiative, it must be overcome. This overcoming will always be in the form of an alternative, a rival version. In a sense, then, the goal of the science of human affairs will be to provide a wide variety of plausible images of human life for the service of life itself. Humanity ultimately will choose.

From an aesthetic perspective, all representations that aspire to membership in the social sciences must do two things. First, they must be formally – as made objects – recognizable as plausible and responsible renderings of reality; this is the work of a poetic. Second, they must argue for their superiority over other, differing, renderings of things; this is the work of rhetoric. Any attempt to hide the "made" and the "argued" basis of our knowledge has lost its claim to cognitive

responsibility, in spite of the fact that exactly such an attempt is the ideology of the disciplinary social sciences in the Euro-American tradition. Disciplinarity involves the imaginary apportionment of human experience through methodological screens. Any event may be said to have a sociological, political, economic, or historical meaning, although no one can point to any part of the event that embodies that meaning; indeed, to point to any part of an event would simply raise the issue again by creating another event whose meanings could be further distributed. Disciplines express aspects of social desire (that is, that the past has a meaning, that humanity may be grouped and classified coherently, that power has definable forms, and that scarcity and finitude can be conceptualized), which they reinforce by propagating and enforcing a particular set of anxieties, known as method.

The creation of the discipline means framing the conceptual scope of a range of questions that will count as proper, and the marginalization of those that will not. It might appear that this framing would apply to the topics deemed appropriate for study, but in fact the limitation that produces a social science is formal, and applies rather to the levels of reflection. In history, for example, what is excluded from "proper" practice as spelled out in the later nineteenth century are questions about the lowest and highest (most concrete and most abstract) ways of looking at things. To probe the lowest, the lexical, level where the basic components of history (individuals, states, wars, etc.) are named, is to imagine a disturbingly incoherent vision, with an unending chaos of historical objects and points of view confronting us once we cease to accept the given lexical field as necessary and exhaustive.

The discipline of history, despite its recent flexibility in broadening the notion of what may be considered to have a history, could not accept such a fluid basis. Its foundation, like that of the other discourses in formation a century ago, would be the repression of any potentially sublime issues of naming the field, issues that might make impossible the point of the exercise, finding meaning or meanings in reality. To see the Holocaust, for example, as an event with meanings that go beyond nations, groups, politics, ideas, technologies, or other forms of proper historical study, might be deemed religious or mystical if it aimed at the highest forms of reflection on meaning; it might be deemed merely physical if its focus was upon lower matters, such as environmental impact or the poisonous effects of chemicals. The former case is the semantic search for a final meaning; the latter is a choice of elements for the historical lexicon that are not traditionally historical. On the other hand, the discourse of meaning, the highest, semantic, level where the multivarious forms of events take on a clear and inevitable shape, posed an equal threat to the possibility of a plausible discipline. For in the explicit expression of a governing system capable of producing a clear shape for the social past and a clear path for the social future lay an ideological force that could be accepted only at the cost of disciplinary credibility. Thus, Christian providentialism, Comtean positivism, Hegelian dialectic, and Marxian materialism all fall away from a disciplined social science. An explicit look at the lexical level made meaning

impossible; equally unacceptable was a semantic clarity that provides too much meaning.

Disciplinarity, then, will be confined to the two middle levels of representation, the grammatical and the syntactical. It is there that rules and usage are enshrined and explicated, and this was the task that the disciplines of the social sciences took on. Method involved the ever more complex techniques of counting, arranging, and resorting predesignated items. The meaning of these operations would, ideally, spring from the process itself, without an application of some external interpretive system whose intrusion would be reductive.

The "Science Effect" and the Modern Fact

The unquestioned lexical item of most social scientific work is the fact, the thing that is certain, demonstrated, and known. Yet factuality has a history, and as Mary Poovey has suggested, the modern fact (as opposed, say, to the ancient fact, or the postmodern fact) is to be distinguished from an earlier sort which was a reflection of metaphysical essences. Modern facts are empirical and recorded in the "transparent" language of numbers, although the telling of the relationship of fact and number is involved and equivocal. Double entry bookkeeping, itself related to sixteenth-century rhetorical practice, created a numbering of things that would change both politics and knowledge. Numerical expertise *per se* replaced the mercantile concreteness of the accountants; as this happened, the particulars of the accounting books came to be seen as "natural matters of fact" (Poovey 1998:29–91).

By the 1830s the debate over numbering, or rather over induction from numbered things, saw a successful appeal to statistics, "a dull, dry parade of stupid figures," against the "pathetic tales" of a literary sort; the value of the figures was precisely in their dullness, "a guarantee against the undue embellishment associated with fiction, hyperbole, and rhetoric" (Poovey 1998:313). The insoluble methodological problems of induction – how to move from number-facts to general principles – were obscured by this triumph over fiction and rhetoric. At the same time, poetry had a solution of its own to the problem of induction. In Shelley's view, poetry embodies knowledge itself at all levels, comprehending all science, and its reference point. The key to the concordance of the generality of science with the particularity of poetic experience (the counterpart of the number-fact) was metaphor, which transformed the particular into meaning (Poovey 1998:326). Poovey notes, but does not embrace, Shelley's figural solution to the problem of the particular and the general, part and whole. She finds the problem unsolved throughout the long era of modernity. Only if we renounce the desire for systematic knowledge for doubt, skepticism, uncertainty, can we escape this dilemma. Be that as it may, her discussion of the modern fact, the deracinated particular, which set aside and ignored the basic

assumptions of ancient philosophy, points to a postmodern fact that in a similar way sets aside and ignores the problems and debate surrounding modern factuality (Poovey 1998:327–8).

Historicizing the fact – indeed, distinguishing among the ancient, modern, and postmodern versions – is troubling because it opens up for attention the constitution of the lexical level of conceptuality. In many languages, the lexicon determines what combination of meaningless elements (such as letters) will count as words. In a social science, some human affairs – say, public statements by leaders – may count as proper parts of the discourse. Other things, although human, like the daily digestive process of a person, do not. If the modern fact is seen as the product of a passing moment of time, like sartorial fashions or gender roles, the effect it creates of a social science that transcends poetry and ideology is hard to maintain. It calls attention to the passing historicality of concepts like society, power, value, or event. Because the disciplinary social sciences exist within the middle levels of conceptualization, the grammar and syntax of a system whose basic elements must be granted in order to play the game, attention to the constitution of basic terms would pose problems, even if their origins were not related to rhetorical and poetic discussions.

The "Science Effect" and the *APA Publication Manual*

At the opposite level of conceptual representation, the open semantic world, where the "meaning of it all" is asserted, other problems arise when historical questions are asked. Because the social sciences were established a century ago as nonideological expressions of (modern) fact, dependent neither on Comtean or Marxist laws of historical process nor on the romantic tales of good and evil found in literary or religious accounts of human affairs, in theory at least, the only meaning that could be expressed by a responsible social science, that is, any meaning that is not dependent on ultimately ideological terms outside the grammatical/syntactic processes of the discipline, is found in the notion, "This is science," in the broad sense of *Wissenschaft*, a disciplined study without value-laden presuppositions. In order to achieve this semantic closure, every exercise must demonstrate the "science effect" in one way or another.

The "science effect" is a rhetorical device intended to instill a proper attitude in the reader, to establish the ethos of the social scientist. The science effect comes from the form of presentation, not from the thing presented. A look at the development of the *American Psychological Association Publication Manual*, long the authoritative guide to the preparation of research across the social sciences, demonstrates how formal constraints that seem neutral and objective create a tight version of what may and may not appear. Charles Bazerman (1987) has studied the changes in the first three editions of this work and concludes that they demonstrate a distinct turn away from argument; recasting of the roles of researcher,

subject, and audience; and a change in the function of scholarship itself. The upshot of these changes was to put creation of what I have called the "science effect" above other semantic issues, such as solving problems or offering general conclusions.

The development of references from the footnote system (1927) to the cross-reference system (1944) to the author–date system (1967) marks the retreat from argument, which confronts the footnoted source as a fellow conversationalist with whom one may agree, disagree, or reason, toward an unargued experimentalism, in which sources are an intrinsic part of the structure. Conflict and open questions rarely emerge; all references have the same form and status, for example, "Smith, 1998; Jones, 1974," just as their articles will have the same form. The argument has been subsumed to the article style itself: it is always "this is science; what isn't this is not science." The hardening of form, which Bazerman places in the late 1920s, with its objectification of subject, author, audience, and literature, marks a decision to frame narrowly the presentation of social scientific work. The framing will dictate the nature of the work. Gone is the author (as opposed to the experimenter), discussing ideas with other authors in the field before an audience presumed to be both knowledgeable and open to the theoretical speculations that might ensue. Gone as well is argument for a theoretical position with persuasive moves that lead to conclusions. In this bygone form the success of the exercise will be the adherence of an audience of interested reasoners.

Instead, we find a modernist form emerging where conclusions do not exist, there being no step-by-step reasoning in the article. The "candidate statement" is placed at the beginning; its testing according to precise rules make up the rest of the piece. As testing replaces reasoning, technical language increases, and statistics become increasingly important. The audience is thus narrowed as much as possible to those with whom one need not reason because it has acceded to the one correct frame and to the questions it authorizes. Articles decrease in scope and length; multiauthored work increases. Both changes serve a profession where numbers of separate publications are a primary factor in advancement. Microincrementalism in topic selection, increased technicality, use of acronyms and abbreviations and statistical apparatus create a new rhetoric. The purpose is not so much to persuade anyone of much of anything, except that the experimenter is competent and following the rules. The greatest danger is not being published. To abandon the rules of the *APA Publication Manual* does indeed disqualify work for many important journals in the social sciences where the science effect is obligatory. In his treatment of the manual and its force, Bazerman notes that much work in recent decades goes beyond the limited frame of the manual, but the manual has not responded much.

An index of Bazerman's conclusions is the vexed question of the first person in the body of a scholarly essay. The "I" signals the presence of embodied arguer, working at a time and a place; its repression turns this real person into a function of something beyond any particular moment or situation. I can affirm that debates in the academy over the use of the first person in scholarly work such as

doctoral dissertations remain alive, as fields where rhetoric is an acknowledged factor assert that no realistic presentation can ignore the situation of enunciation and the subjectivity of any author.

What we find in the changes marked by the evolution of the *APA Publication Manual* is the replacement of one rhetoric by another. In the new rhetoric, science consists of carefulness rather than good reasoning. References are defensive guarantors of thoroughness, rather than encounters with other reasoners. Philosophic problems disappear before the small, incremental bits of knowledge. Looking for faults in other work becomes a major genre. Above all, perhaps, the fixed sections with their standard subtitles eliminate the transitional reasoning that characterize argument. Here, the form is the argument because it both derives from – and enforces – the tacit agreement of the community. Professionalization through graduate training means above all learning to work and think and write within the form of discourse required to publish. An economist has written of the *APA Publication Manual*, "It is clearly meant to be a joke. A jolly good one it is, too, giving for instance page upon page of advice on style couched in the most miserable style the editors could manage, and fully ten hilarious pages on the official formula for a scientific paper" (McCloskey 1985:175).

The *APA Publication Manual*, in effect, offers rules on how to remain within the middle levels of conceptualization, eschewing semantic reasoning and any questioning of the status of the fact itself. Although it is a guide to only the more formalized of the social sciences, it embodies the rhetoric of antirhetoric that marks scholarly ethos across the board. Poovey and Bazerman each describe how poetics invades this realm: Poovey by citing Shelley's metaphoric solution to the problem of particulars and meaning; Bazerman by detailing how the narrator, narratee, and structure of emplotment become increasingly frozen as the genre of the article slowly shifts from argumentative inquiry to experimental report.

If the "science effect" continues to hold sway in certain social sciences, responses have not been lacking. A revived sense of a new science of aesthetics in social science has emerged, a science in the Vichian manner that examines human knowledge and truths as the inevitable errors of our finite position in the world, working with tools we have ourselves fashioned. In the rest of this chapter, I shall discuss work in political science, sociology, economics, and history, with an eye toward moving the conversation toward its logical conclusion by taking up, one by one, increasingly radical presentations of the case.

Society as Text

Richard Harvey Brown has spent two decades developing a vision of sociology as a form of poetic discourse, caught between two opposing conceptions, positivism and romanticism. By the protean term positivism, Brown means an attitude to representation (or rather, statements) that demands a one-to-one correspondence

between the "voice of science" and its objects. What must be excluded in this voice is connotation, the resonance of language that brings an uncontrolled personal voice into the conversation (Brown 1977:27). That resonance is consigned to poetry and its "ingenious nonsense," as Newton put it. Romanticism championed the higher truths of intuition and art, but in doing so took an inward turn that all but surrendered the field of nature and the outer world to science. So Brown can summarize the institutional forms of positivism and romanticism thus:

Science	Art
truth	beauty
reality	symbols
things and events	feelings and meanings
"out-there"	"in-here"
objective	subjective
explanation	interpretation
proof	insight
determinism	freedom

Brown proposes a "cognitive aesthetics" that will overcome the partiality of the scientific construction of society through "a dialectical hermeneutic that recaptures the fuller meaning of a thesis by transcoding it and thereby transcending its partiality . . ." (Brown 1977:48). The optimism of this formulation does not remove us from what Brown refers to as the "tragedy of culture," the recognition that our world is an alienation of reality, but it places at the forefront a metaphor of wholeness which will serve Brown as a goal through later books.

It is through irony that Brown seeks to overcome the partiality inherent in social science. Irony creates a tension and "dramatic richness" by revealing contradictions between intentions of actors and the outcomes of these intentions. It emancipates by showing the audience not only the mismatches between actions and results, but also casts the observer as yet another ironic position in the process of comprehension. The observer becomes an actor, with all of the uncertainties that this entails. Disillusioned in advanced, ironic social scientists will look back upon themselves as (interpretive) actors in the social process, and see the role played by poetics and figural language, for example, the theatrical metaphors that have dominated this paragraph (Brown 1989:117–88).

In his description of "dialectical irony" in *Society as Text*, Brown takes a Hegelian stance. When faced with two plausible alternatives, the social scientist must affirm them both "on a more sophisticated and reflective level" (Brown 1987:142). The silence between opposing positions will figure forth a deeper meaning that will free us from moralisms of all sorts. Put poetically, a sensitivity to the misrepresentation inherent in all realistic presentations will heighten our ethical intuitions that moral norms are also figural errors; this is the truth as error that Vico had suggested was our destiny (Brown 1987:173, 182, 187).

The relativism fostered by rhetorical criticism is the challenge Brown offers to the absolutists. To ponder alternative constructions is to render their effects visible and make them more open to improvement. Rhetoric here serves as an ethical touchstone, humanizing and making whole what was once a fractured and distorted image of the social (Brown 1995:9). Brown is careful to distinguish between both the "mere rhetoric" scorned by positivists, the rhetoric that constitutes our sense of "facts, logic, and truth" and the "*critical* rhetoric" that announces its own partisanship (Brown 1989:171 n.1). In the same vein, he separates "epistemic" from "judgmental" relativism (Brown 1995:10). Irony, another of Brown's key terms, also has two forms – a conservative one that serves as a safety valve for social pressures, and a free or mastered form that liberates (Brown 1987:187). Perhaps a telling example of this irony would be the episode in Art Spiegelman's *Maus* (1986) in which Art, the cartoonist-author recording the experiences of his father in Auschwitz, argues with the old man about the existence of an orchestra in the camp. Vladek insists that there was no music; Art argues that the historical record proves there was. Who is the authority, the camp survivor or the son who relies on books and photographs? The ironic dilemmas of experience versus discourse should be acknowledged and made an opportunity for reflection (Spiegelman 1986:54).

If a cognitive aesthetics, in effect, an ironized poetics, can relativize sociology so as to increase its sensitivity to a variety of moral norms and to undermine positivist absolutism, one might wonder how such a change is to come about. To turn a discipline, even toward sensitivity, is an aggressive act, a political act with consequences for careers and the training of fledgling scholars. Where, in what context, could the change take place? How can a discipline be redirected? The answer to this question is political, and it is in political science that a response takes shape.

Epistemics are Rhetorics are Politics

Ideas take form in academia by becoming official subfields. So the attention to the constitution of the social sciences in discourse has come to be called the "Rhetoric of Inquiry." Beginning at the University of Iowa in the 1980s, the Rhetoric of Inquiry movement has promoted a wide variety of projects that borrow from literary and rhetorical theory. John Nelson, the political scientist who coined the term rhetoric of inquiry, epitomizes its positions (Nelson 1987a).

The opponent is foundationalism. By choosing as his foe foundationalism, a philosophical notion that denotes a reliance on some basic premises at the end of any chain of reasoning, Nelson has turned his attention to the area of epistemics, the ways in which knowledge is produced and validated. Philosophical foundationalism depends upon key documents and "Founding Fathers" just as political

systems do, and both of them – indeed, all foundationalisms – invoke universal, context-independent standards. Here, Nelson recognizes the intrinsic contradiction within the expression "political science." If there is to be a science of politics, it must rest on some view of the political that does not derive from any particular example, but from a sense of politics as abstract and universal as the concept physicists have of matter. If, on the other hand, we take politics as it is actually practiced, then our concepts of a "political science" will need a different way of accounting for itself.

Citing Vico's dictum that "doctrines must take their beginning from that of the matters of which they treat," Nelson in effect replaces rhetoric with politics at the same time he equates the philosophical with the political: "Epistemics are rhetorics are politics" (Nelson 1987a:268). This is a remarkable and plausible undoing of the sixteenth-century work of Petrus Ramus ([1549] 1986), who split the concept of rhetoric that had prevailed in the West for almost two thousand years, the concept of a knowledge-generating force that answers to the entire human soul, into a mental system of philosophy and a physical system of decoration known as rhetoric. Thought first, then expression – with philosophy the master discourse. Nelson understands rhetoric in its traditional sense, with invention the source of ideas (epistemics), and the appeal to the emotions the source of power (politics). He takes seriously the metaphoric nature of all disciplinary language.

And everybody knows that our academic discourse about grounds, foundations, fields, roots, branches, spaces, places, claims, warrants, backings, constructions, deconstructions, questions, issues, positions, links, turns, and literally all other terms of argumentation is a magician's trunk of tropes. The puns, images, symbols, associations, inversions, and myriad other operations seem never to cease. Until we speak. Or write. And then we forget. Even the scholars. That's how it goes (Nelson 1998:135). Forgetting language is the problem, and in political science it leads to political problems.

Ironically, criticizing the foundationalism that represses the awareness of language and figurality can lead to a paralyzing openness, in which self-awareness and reflexivity leave the community unable to act. This Nelson rejects:

> Philosophical critics of foundationalism might seem to exempt communities or conversations from the proscription of ends, but they tend to celebrate unbounded communities and conversations without end. By contrast, rhetoricians must recognize that human finitude enjoins choices because it precludes omnicompetent attention and participation. Without substantive ends, we achieve only vague cases for "the open society" and "continuing the conversation." (Nelson 1987a:266)

Put otherwise, the reality that we cannot know all we need to know about the consequences of our political actions in the world makes it all the more necessary for us to envision specific goals and work to attain them. Finitude is an enabling concept.

Nelson believes that foundations, political and epistemological, are principles that create myths. The ongoing practice of a political science, then, would be mythic as long as it claimed to be working within the conventions of the discipline. But these conventions, Nelson argues, are political in every sense, created by political rhetoric. Consequently, the antifoundational practitioner of a rhetoric of inquiry should direct attention to the kinds of narratives, gossip, and lore that reflect the actual practices of the field (Nelson 1987b:199–200). There is an ethical concern in this debate, and Nelson must confront the consequences of undermining political foundations in the eyes of a national community that sees itself as morally bound both to and by those foundational concepts. He acknowledges that people hate to hear their foundations, political or academic, called rhetorical, let alone mythic. It questions their identity, and brings on charges of nihilism, barbarisms, selling out civilization (Nelson 1987a:281). In the face of this, Nelson suggests a cautious awareness of the social need for foundations, while attempting to enlist the study of politics (as argument) as the basis of epistemology.

Models are Stories

Economics is traditionally the most formal and abstract of the discourses about human interactions. It stands at the edge of the social sciences, pretending to belong elsewhere. The economist, according to Deirdre McCloskey, aspires to the condition of pure science, a world of models that can be calculated perfectly, where neither argument nor tale-telling obstruct the proofs. McCloskey calls the practice of economists modernism.

McCloskey relates modernism in economics to both the aesthetics and the psychology of a "dissociated sensibility," which encounters a language without speakers, artistic forms that do not represent reality, and economic models that have no contact with the world. Such models, however, are metaphors, and economists are the poets who make them in McCloskey's telling. Metaphor and story-telling are the economist's unacknowledged tools, but their failure to examine the sources of their product causes economists to tell bad stories. "The uncriticized story is not worth living" (McCloskey 1985:vi). The trouble with modernism is its partiality; it refuses to notice most of the human mind, while examining one small part of it in great detail. Here McCloskey is using a rhetorical view of the mind, a view derived from Aristotle's notion that persuasion has three methods – logos, ethos, and pathos (Aristotle 1991:para 1356a). The ethos of economics is that of the scientist, who, like the modernist poet, is well aware that obscurity in the argument pays (McCloskey 1990:57). It is the extreme focus on logos in the guise of statistical significance that renders much of modernist economics unethical, failing to take note of ethos and character (McCloskey 1996:59). The significance of any counting must come from outside the system

itself, but as the cost of computation has dropped, people, as good economic subjects, bought more of it, leading to an overreliance on simulation and modeling, and a pompous overreaching in policy prescriptions.

Although physicists gave up stories in favor of models in the seventeenth century, narratives remain the crucial, if unrecognized, force behind modernist reasoning. Economists love "unforeseen consequences, trick endings, a pleasure shared with other social scientists" (McCloskey 1990:15). The stories economists tell are structured in the same sense as the Russian folk tales studied by Vladimir Propp in the 1920s ([1928] 1968). Propp demonstrated that the great variety of folk tales were constructed from a rather small set of functions. The functions in folk tales, of course, are simpler than those in an economic argument. Whereas Proppian morphology has functions like interdiction, abstentions, deliverance, economists describe things like entering a market, exit, price setting, purchase, sale, valuation (McCloskey 1990:24). They like to appeal to a magical force "underneath it all." As stories, economic treatises respond to the same array of critical strategies as any literary narrative; McCloskey reads economists like Paul Samuelson or Alexander Gershenkron as a literary critic might read Henry James (McCloskey 1985:69–72, 1996:ch. 3). The point is that stories and their structures are everywhere – in the lecture hall as well as around the campfire.

McCloskey wants to create a redefinition of economics to bring it firmly back to the social sciences by emphasizing that the social is the dominant force. "Science is rhetoric, human argument, all the way down . . ." (McCloskey 1990:8). She confronts the question "so what?", and from her answer generates a story different from the one where expertise of one sort or another brings happiness and a better life. In doing this, she joins Brown and Nelson in predicting clear benefits from an increased sensitivity to the rhetorical and poetic aspects of social science. Indeed, McCloskey adds a gender dimension to her analyses by referring to male economists as "boys in the sandbox," playing with their models. Confronting the arrogant certainties of blackboard economics with womanly sarcasm, McCloskey emphasizes repeatedly that "profitable prediction is impossible" – another version of what she calls the American question, "If you're so smart, why aren't you rich?" (McCloskey 1996:102). The payoff then is a retreat from the superiority of the antirhetorical stance, a renewed sense of modesty (of human finitude, as John Nelson, McCloskey's one-time colleague at Iowa, put it). "See also literary criticism" is a warning to social science and its pretensions.

The Figurality of it All

The most fully developed picture of the form and the consequences of the aesthetic dimensions of the social sciences describes the most realistic and least schematic of representations of human events and structures – history. As indicated above, the aesthetic aspect of historical representation was a casualty of the

move for cognitive accountability and disciplinary status in the late nineteenth century. Historical writing in the tradition of Vico or Michelet gave way to a professionalized version of Ranke's method. The poetics of history became a challenge to its capacity to present truth. At the same time, innovation in the literary form of historical discourse froze at the moment of professionalization into the realistic presuppositions of the reigning fictional genre of that moment, the so-called realist novel (White 1978:43). Later literary developments, which challenge and foreground the means of representation and which problematize memory and consciousness (e.g., the work of modernist authors like Woolf and Proust), have found no imitators among academic historians, who are more likely to profess horror at such devices as Edmund Morris's recent creation of a fictionalized narrative character in a presidential biography, although the narrator is always a fictional character in modern narratological terms.

Hayden White, who has waged a sustained campaign to move history back into the realm of the aesthetic and moral sciences, suggested in "The burden of history" (1966) that formal innovation in how the past may be configured, how evidence is construed, and on what basis data should be eliminated from consideration has been undertaken primarily by writers outside of academic history departments, figures like the psychiatrically trained Foucault, the art historian E. H. Gombrich, the psychoanalytic critic Norman O. Brown, or the historian of science Thomas Kuhn. The resistance of historians to creative forms of narrative was the counterpart of attempts to downgrade narrative in history, and move it closer to the more mathematical social sciences. White maintained that the sort of history being produced by academics was increasingly burdensome, and that some revival could only come from outside the field. First, however, a formal description of the historical work was needed.

In *Metahistory: The Historical Imagination in Nineteenth-Century Europe* (1973), White undertook such a description. White's poetics of the historical work was schematic and lent itself to being laid out in a table. He noted four modes of Emplotment (romance, tragedy, comedy, satire) derived from Northrop Frye, four modes of Argument (formist, mechanist, organicist, contextualist) derived from Stephen Pepper, four modes of ideology (anarchist, radical, conservative, liberal) derived from Karl Mannheim, and four ways of organizing parts and wholes by the rhetorical tropes (metaphor, metonymy, synecdoche, and irony) derived from Vico. It was the last scheme, the theory of tropes, that attracted the most interest and hostility. White referred to the tropes as the "deep structures" of thought, using Chomsky's term, and undertook to establish them as the basis for most of the features of discourse. The tropes were depicted as the possible ways in which consciousness can sort out complex kinds of experience that are difficult to grasp, and in doing so, render them capable of being analyzed and explained (White 1973:36). The tropes, in other words, are prefigurations of thought, the framework within which thought is possible.

Metaphor equates whole with whole, allowing one form to represent another in a world that is construed as a vast array of correspondences. Michelet and

Carlyle embody this essentially romantic apprehension of the world, which finds in Formism its appropriate philosophical counterpart. Metonymy reduces wholes to a group of parts dominated by a single one, which then stands for them all in a mechanical way, in the sense, say, that mass and force are the terms of the Newtonian universe. This trope has been the favored scientific mode of explanation, allowing thinkers like Marx or Buckle who aspired to the status of science to appeal to laws of historical process. Synecdoche, the third trope, grasps the essence of a given whole by positing an object that unifies the whole by embodying its intrinsic spirit, creating an organic sense of life. Friedrich Ranke, who used the nation as a figure that pulled together all the threads of human activity in a drama of living forces, is a synecdochic historian, to be differentiated from, say, Marx, not on ideological grounds alone but also on tropological ones. The economic structures to which Marx appeals metonymically produce laws, not dramatic plots. But the Marxist sense of the course of things has a tragic quality of inevitability as exterior forces make things happen. Ranke, on the other hand, conceives of nations rather fulfilling their internal destinies, in a teleology that is ultimately comic.

The three tropes described above – metaphor, metonymy, synecdoche – have one thing in common: each functions in the confident certainty that the world can be understood via the strategies it offers. Adopting one trope or another means getting things right. However, the fourth trope – irony – manifests an enduring doubt about the possibility of ever getting things right. It leads, on the one hand, to an intense self-consciousness about the ways in which all representations are misrepresentation. As Vico might have said, we know the secular world precisely by getting it wrong (and should remain clear on that point). But irony also leads to a sort of cognitive despair and a rejection of any affirmative vision that calls for action. It is this ironic despair that White found in much academic history, poised between its rejection of the literary-imaginative modes of conceiving the past and a corresponding rejection of scientific-ideological uses of the past as directions for political action. Because it recognizes all language as a problem, irony can be used tactically by any particular position to discredit opponents.

But, as the basis of a worldview, irony tends to dissolve all belief in the possibility of positive political actions. In its apprehension of the essential folly or absurdity of the human condition, it tends to engender belief in the "madness" of civilization itself and to inspire a Mandarin-like disdain for those seeking to grasp the nature of social reality in either science or art (White 1973:8).

White's hope in *Metahistory* was to show the "sciences of man," as the French call them, a way beyond the methodological frigidity of irony toward a renewed attention to those aspects of human existence realized by the arts. If irony was a symptom of the problem, it was also a part of the solution, for White construed irony as above all the dialectical trope. And dialectic, the explicit frame of mind of the great theorists of history like Hegel, Marx, Nietzsche, and Croce, is simply the formal awareness of the tropical nature of discourse, and of its propensity to

move back and forth between alternate ways of encoding reality (White 1973:428, 1978:4).

Irony, which I have elsewhere described as the "trope of tropology" (the figure that describes how figural thought changes and unsettles what it contemplates), is what allows us to get a dynamic view of whatever we are studying (Kellner 1989:193–251). It forces us to consider the conflict of viewpoints as a part of reality equivalent to the object of these viewpoints; the change that these conflicts bring about becomes part of what caused it all in the first place. No single vision or method is adequate, in the ironist's opinion (Rorty 1989:Part II.). We can only be certain that a future vision will be different. It may be disturbing to suggest that the Holocaust be apprehended ironically after half a century, but the proliferation of uses to which it has been put, and the many plausible constructions and explanations that continue to reshape it, demand an ironic look at the phenomenon as a dynamic, ongoing event (Novick 1999).

Narrative is the form in which the meaningfulness of human affairs can be established through emplotment, the crucial way of encoding reality. It is also, however, impossible to reconcile with the desire realistically to represent real events because real events never present themselves to us in the form of stories, in White's view (White 1987:4). The appeal of historical discourse is that it offers up the real as an object of desire by supplying it with a meaningfulness and closure added to it by the forms of emplotment available to a culture at any moment in the accepted understandings of what comprises a plausible story. Because this meaningfulness and formal clarity is unavailable to us in our lived experiences, the plotted nature of historical narrative must be conceived as "found" in the events themselves, although any notion of what the events themselves consist of must come from requirements of the plot (White 1973:20–1). If the plot – that is, the meaning – of a historical account were perceived to be not found, but invented according to currently acceptable forms, the claims and purpose of historical discourse would be rather different. The truthfulness of a historical account is thus only relevant to the chronicle of facts that lie behind the discourse. This chronicle, according to White, will be bereft of any real meaning because only emplotment can supply the culturally supported forms that connote meanings for any community (White 1973:43). Historical truthfulness, as opposed to the truth of chronicle, is cut from the same cloth as the truthfulness we ascribe to literature or myth. It is the truthfulness of the logic of figuration, tropology.

White has been above all a reader of history, far more interested in analyzing historical discourse than in prescribing any particular course for the historian. He has shown a particular interest in what he has called "mastertexts," those historical works that offer such a fully developed and compelling picture of things that disconfirmation of factual details cannot shake their hold. Mastertexts will be the work that must be addressed until another work presents a more compelling vision to the community whose past is at stake. Consequently, White has avoided analysis of routine historical texts of the monographic sort, while attending closely to a broad range of figures from Vico to Foucault. What progress can be claimed

in historical discourse comes from the production of such classics as these. In this sense, history is like literature in that alternative visions of things can coexist without coinciding, just as both a Balzac novel and one by Virginia Woolf can serve as valid pictures of the world. The purpose of history is to embody, challenge, and extend the capacity of our cultural means of figuring reality through fictions. White applauds the new form, the experimental work that tests the possibilities rather than repeating the patterns of historical discourse.

The relationships among phenomena that the tropes name and describe appear to be part of reality, but are rather the forms by which mind and the experienced world come to terms with the perceived need to find meaning in human affairs (White 1978:72). White's theory of tropes is based on Renaissance models taken as deep structures of thought. Unlike Chomsky, though, White does not maintain that these structures are "wired in," as it were, fundamental and universal brain patterns. Rather, he sees them in historicist terms, as simply conventional Western forms of thought.

Although White calls himself a formalist, and has followed a very formal method of analysis for much of his career, there has been a crucial and consistent exception throughout his work. The alternative visions of history that are generated by the formal conventions at any given time do not provide a way of choosing among them, although choice is necessary for a responsible social existence. White refuses to claim, for example, that Tocqueville was a more "scientific" historian than Michelet, or that Marx was more "realistic" than Hegel, because "in order to render such a judgment I would have to ignore the fact that *on historical grounds alone* I have no basis for preferring one conception of the 'science' of history over the other" (White 1973:432, emphasis in original). Thus history, like the other social sciences, teaches many lessons, but does not teach how to decide on the right one. Such decisions will always depend on *moral* or *aesthetic* grounds, which exist in an interpretive community apart from any expressions of scholarly expertise. If the community is committed to a particular way of conceiving things, any historical work that adopts a different approach will fail (White 1973:430).

White has suggested that the suppression of rhetoric in the nineteenth century was political. Insofar as rhetoric illuminated the relationship between language and power and at the same time taught the politics of language use, it could no longer serve the purposes of those who wanted the emergent masses to be literate as receivers of messages, but not (rhetorically adept) readers of them. At the same time, rhetoric could claim a far wider reach than simply the path to political power. As the self-conscious analytical system that demonstrated that all of discourse was figural at every level from the most elementary sound distinctions to the largest structures of discourse, rhetoric threatened to bring all of knowledge into a condition of misknowing that would challenge the footing of any social science by relativizing it at every turn (White 1997).

It is worth adding to White's analysis that the class politics he describes, in which the nullification of rhetorical awareness has the effect of producing

consumers of discourse lacking the tools to take hold of language possibilities and recast the world presented to them, is also the strategy by which a social science enforces its control through method. The methodology course that is the universal entry to a social science exists to instill anxieties in the student which will forge his or her identity as a professional practitioner: the sociologist must see the world of human phenomena differently from a historian, the historian differently from a political scientist, and all of them differently from a theorist of language.

So What?

We must certainly beware of the sort of story that McCloskey warns us against, the tale in which the expert brings us information which, if we heed it, will keep us warm and happy. And yet it is difficult to imagine the plot of any academic endeavor that aims at turning the profession in a different direction without such a promise of payoff. The antagonistic responses to the positions I have described above take predictable forms: "killing the past," nihilism, relativism. Others wonder whether the new attention to presentation allows for "an authentic experience of the past in which the past can still assert its independence from historical writing" (Ankersmit 1994:194). The suggestion on the bookstore shelf, "See Also Literary Criticism," seems threatening, and the counterarguments often wind up as moral ones several thousand years old. If the kind of realities addressed by the social sciences are constituted at virtually every point by the workings of language and discourse, on what basis, it is asked, can we hold to any principle or commit ourselves to any cause? All these responses presume some place outside the social community from which beliefs can be justified in a way that is not relative to the argumentative norms of any particular community, even the community of scholars. Such a view of social science requires that it *not* be social, and presumes that we hold opinions and engage in public activity because we have been convinced by philosophic or empirical truths, and not because we have been socialized within a certain group. As Richard Rorty points out, however, once convinced of the linguistic turn we will make moral decisions on the same basis we have always had, because we cannot not be what we already are (Rorty 1987:48–50).

A more interesting response to the enterprise described in this chapter asks precisely what the goal of the enterprise would be. If you got rid of modernist, positivist, foundational social science, or made it sensitive and modest, would you be reforming it, or would you be replacing it with something else? And from what position could one do this? All of the theorists discussed in this chapter came from within the fields they study. It is not a question of an invasion by narratologists and rhetorical sophists, but what has been the appeal of the analytical tools borrowed from them? After all, as Stanley Fish has noted:

> Literary critics do not traffic in wisdom, but in metrics, narrative structures, double, triple, and quadruple meanings, recondite allusions, unity in the midst of apparent fragmentation, fragmentation despite surface unity, reversals, convergences, mirror images, hidden arguments, climaxes, denouements, stylistic registers, personae. The list goes on and on, but does not include arms control or city management or bridge-building or judicial expertise or a thousand other things, even though many of those things find their way into the texts critics study as "topics" or "themes." (Fish 1995:90)

Given this, why have literary and rhetorical studies come to the fore?

When it comes to demonstrating that historical texts are tropologically pre-figured or that econometricians tell stories, the appropriation of the discourse spawned in literary and rhetorical circles raises the question: how are people to be convinced? After all, as Deirdre McCloskey writes, "The typical number of mind-changes in most fields through a scholarly career is zero" (McCloskey 1990:50). There are several possible answers to the question, among them what we might call the Kuhnian and the Freudian responses. Kuhn suggests that challenges to scientific paradigms typically come from outsiders and the young; he apparently agrees with McCloskey's comments on scholarly mind-changes. This theory, though, cannot account for McCloskey herself, or White, Brown, Nelson, and many others. We should consider rather the possibility that something repressed in the human sciences has re-emerged. It is not entirely clear what this "return of the repressed," as Freud once put it, will become. Certainly, Richard Lanham's notion that we are simply returning to the normal Western scheme of things, the rhetorical ideal of life that was dominant until "the Newtonian interlude," describes a *ricorso* that is hard to imagine, however suggestive it may be (Lanham 1993:195). In any case, if it happens, it will be part of a cultural shift far broader than the disciplinary interests described here.

This chapter will not conclude with claims that a new social science is at hand, nor that the work of the old disciplines – positivistic, foundational, modernist – has been or ought to be superseded by a countermethodology of postmodernity. I do not believe that. What has happened, I think, and will continue to develop, is a more complex set of arguments within the disciplines. Criteria of truth will not be lost, but will be perceived differently in different contexts, including an aesthetic context. It will be impossible to claim that there exists any supercontext (Fish 1989:480). "Changing the subject," as Rorty puts it, may occur so gradu-ally it seems quite normal to be interested in other aspects of the discourse of academic social science. Research projects that might have looked quite marginal a few years ago will seem normal, as they build on similar projects. Finally, it will be more difficult for any discipline to claim the sort of prestige that has derived from being on the tough side of the romantic division of the world of prose and practical reality from the world of poetry and imaginative fancy. It is, I think, already more difficult to make such claims bring the automatic respect that expert-ise and the science effect have brought in the past. People are skeptical, and need

to be persuaded, reasoned with. Questions about the point of it all, the use and the impact of any science of the social, will have to consider the imaginative, the figural, and the persuasive force of its production. The meaning, however, the force of these arguments, will ultimately come from outside the disciplines, from the public sphere of persuasion, from the social.

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The Descent of Evolutionary Explanations: Darwinian Vestiges in the Social Sciences[1]

Lynn Hankinson Nelson

Evolutionary adaptation is a special and onerous concept

(Williams 1966:vii)

Methodological Particulars

The argument of my larger discussion is that the purportedly evolutionary explanations being advanced in psychology and the social sciences, for example, in "evolutionary psychology" and "cognitive anthropology," are not the genuine article. The roots of the failure to generate evolutionary explanations lie in the methods social scientists employ. I emphasize two failures that trace their roots to the methods social scientists employ. First, those who use "reverse engineering" rather than historical methods to derive "evolutionary" explanations of the features of human psychology they propose, consistently fail to rule out alternative explanations. Thus, they fail to make a plausible case that these features are *adaptations.* Second, a common line of reasoning is that the discovery of an alleged "universal" in human behavior constitutes evidence of some innate psychological mechanism, and innateness constitutes a *prima facie* case for adaptation. This methodological assumption suffers from some well-known problems, as I later note. But a minimum requirement for making the case for adaptation on its basis is demonstrating that the psychological feature posited – the cognitive "mechanism" or "predisposition" – *guides* rather than merely *fits* an aspect of behavior. Proponents, I contend, consistently fail to establish this stronger claim.

In the end, cognitive psychology contributes little other than the notions of "cognitive modules" and "predispositions" to the research here considered, evolutionary biology little other than the notions of natural selection and adaptation.

Several qualifications are in order. First, few of the arguments I advance are original. Many figure in critiques of evolutionary psychology, the most visible and influential of the relevant research programs, and were prominent features of the criticisms leveled at human sociobiology more than two decades ago. Regrettably, recurrent themes in the current literature demonstrate that these arguments need to be rehearsed again.

Second, although I arrive at skeptical conclusions concerning the present and future prospects of these research programs, I am a Darwinian, to paraphrase W. V. Quine in another context, to the extent that anyone in their right mind would be. But one can grant that aspects of human behavior and psychology stem from evolved capacities, and maintain that social scientists have not *succeeded* in their efforts to construct evolutionary explanations of them – and are unlikely to do so given the methods they employ (cf. Dupre 1998, Richardson 2001). Alternatively put, my critique concerns the *evidential warrant* of the methods employed and hypotheses advanced in these research programs. It is not motivated, as advocates of these programs often suggest, by naive views about humans, or fear that "hard-headed" investigations into human nature will uncover some nasty truths about it (at the risk of being overly facetious, there is little to fear in this regard), or an opposition to any and all forms of reductionism.[2]

Commonly, these programs assume a specific model of conceptual integration, according to which biology explains psychology and psychology explains culture. More specifically, we are told that evolutionary psychology supplies "the middle causal theory" linking biology and the social sciences: "the psychological mechanisms that come between theories of selection pressures on the one hand and fully realized sociocultural behavior on the other" (Cosmides et al. 1992:6). But, of course, conceptual integration so construed is not the only way to conceptualize the relationship between the social/behavioral sciences and biological/natural sciences. We can grant that the objects studied by the various sciences differ in their compoundedness and think of them as being arranged on something akin to a ladder. And we can grant that, assuming physics is at the "bottom" of this ladder, there are constraints that flow upward: that is, that economic cycles and social institutions are subject to the laws of physics, that human biology dictates that we cannot fly even if a desire to do so should turn out to be an innate feature of human psychology, and that evolutionary processes contributed to aspects of human psychology and behavior, as well as to our biology. We can also grant that the assumption that there is a "lower level" description available for objects that have dubious identity criteria (e.g., the assumption that there is a physical-state description of "ideas" or "beliefs") finances or underwrites our present commitment to such objects. We can even maintain that such assumptions are not just presuppositions of science as we know it, but *results* of science to date. All of this is compatible with there being unique causal relationships at each level that a full

account of what goes on would include (Nelson and Nelson 2000b, cf. Dawkins 1989, Dennett 1995, Dupre 1998). Moreover, on the arguments canvassed here, no genuine integration of evolutionary biology is being achieved in the research under consideration.

The criticisms I offer are also not motivated by "political" considerations of the simple-minded or vulgar sort evolutionary psychologists often attribute to their critics.[3] But they do challenge the "value-freedom" and apolitical status evolutionary psychologists claim for their hypotheses, and for those of evolutionary biology and human sociobiology they seek to appropriate. I also develop an argument Philip Kitcher advanced some years ago[4] to maintain that we are warranted in bringing the most stringent standards of evidence to bear on hypotheses positing "innate" features of human psychology precisely *because* these claims typically carry significant social and/or moral implications.[5]

Stringent standards, but defensible ones, and the acceptance of a "new" research program or theory may be justified on grounds other than empirical results.[6] Such grounds obtain when rival research programs or theories have been shown to be bankrupt or there is a perceived need, again based on developments in the sciences, for a new discipline. But even in such cases (and it is not clear that either situation presently obtains),[7] we need to ask whether the new program is *promising*. At a minimum, this calls for an evaluation of its core methodological assumptions, research questions, and hypotheses in light of closely related and successful theories. The research programs under consideration claim to *use* evolutionary biology and, by so doing, to provide *evolutionary* explanations of human psychology and behavior. Thus, it is appropriate to assess their methods and explanations using core tenets and accepted methods of evolutionary biology. It is the argument of the next several sections that when we do so, these programs are less than promising.

Finally, I will contend that, in the case of evolutionary psychology, those of us who argue against the viability of this research program are not aptly described as "cutting off a new research program at the knees."[8] As will become clear, many hypotheses being advanced in this research program are well-worn hypotheses of human sociobiology and, in some cases, Social Darwinism before that, to which evolutionary psychologists have simply grafted technical notions from cognitive science.[9]

The next two sections outline some significant difficulties attendant to making a plausible case, let alone demonstrating, that a trait or capacity is an adaptation, and some formidable obstacles to using historical methods to make the case that features of human psychology are adaptations. In the fourth section these arguments are among those brought to bear on the methods employed in evolutionary psychology to demonstrate the failure to make the case for adaptation. The fifth section is devoted to an analysis of parental investment theory, a mainstay of evolutionary psychologists' explanations of alleged evolved predispositions toward mating and parenting. An analysis of an argument Richard Dawkins has offered for asymmetrical sex roles makes it clear that even quite sophisticated theorizing

in evolutionary biology, indeed theorizing not thought to apply to human behavior, can be deeply informed by sociocultural and value-laden assumptions. In the sixth section I use a hypothesis from cognitive anthropology to explore what is needed to make the case that a specific cognitive mechanism or predisposition guides human behavior, and to show that, in terms of this hypothesis and others, there is a failure to make this case. In my conclusion, I maintain that there is a special demand to be made of scientists advancing hypotheses that carry social implications that in no way violates the boundary traditionally (if problematically) assumed between science and politics. In such cases, we should expect scientists to uphold and meet the highest epistemic standards.

Adaptation

Explanations in evolutionary theorizing take a number of forms, in part because we know that a number of processes and forces impact on evolution's directions and results.[10] Natural selection, the differential survival of organisms with some advantage over conspecifics relative to environmental and/or reproductive problems, is a (or the) mechanism of evolution, a process that "selects for" traits (and, some would have it, genes).[11] Some but by no means all evolutionary theorists, recognize sexual selection as a distinct mechanism that "selects for" traits conducive to success in mating and/or parenting, rather than survival. And although not selection mechanisms, forces such as genetic drift, mutation, and migration, as well as developmental constraints, help to determine available genotypes and phenotypes. Disagreements remain concerning the relative significance of these various processes, including the singular importance traditionally attributed to natural selection and to adaptations in the sense of its products. But for reasons explored in some detail below, the most defensible view is that not all traits are selected for – that is, some are neutral in adaptive significance, some "free riders," and some the result of processes other than natural or sexual selection. Only traits that are selected for *are* adaptations.[12]

Adaptation so understood is, as G. C. Williams makes the point, "a special and onerous concept" (Williams 1966:vii). Williams was in part responding to misapplications of the notion to traits only shown to be conducive to fitness (Williams 1966:vii). Arguments insisting on this distinction are common and originate with Darwin. Elliot Sober's claim that a trait is an adaptation if and only if is "selected for" is representative (Sober 1993), as is Richard Burian's criticism of the assumption among some population geneticists that a correlation with increased fitness is sufficient to demonstrate that a trait is an adaptation (Burian 1992).[13]

This points to a second reason why adaptation is a difficult notion: *making the case that a given trait or capacity is an adaptation requires ruling out alternative explanations that are equally viable.* As earlier noted, a trait or capacity may be the

result of biological processes other than selection and/or neutral in adaptive significance. In addition, and again as Darwin recognized, current function may reflect conversion and cannot itself establish original function or causal history (Darwin 1859). Finally, as Daniel Dennett makes the point, when the "trait" in question is an adaptive behavior or capacity exhibited by a species displaying some degree of behavioral flexibility, *equally* viable explanations include the discovery of "a forced move" or "good trick" – and at least in the human case, cultural descent, codiscovery, and information transmission (Dennett 1995).[14]

Reflecting these issues, it is a common argument that to make a plausible case, let alone demonstrate, that a given trait or capacity is an adaptation requires knowledge and use of history. Sober emphasizes, for example, that "adaptation is a historical concept" (1993:84). Burian and Ernst Mayr argue that it is because correlation with increased fitness does not indicate the historical origin of a trait that it is insufficient to demonstrate adaptation (Burian 1992:11, Mayr 1983, cf. Brandon 1978). This yields a third difficulty. For reasons later explored in some detail below, the kinds of historical information required to rule out alternative explanations or to identify the causal process(es) that resulted in a given trait or capacity *are often difficult to come by* – sometimes simply because selection tends "to cover its own tracks . . . to destroy the variation on which it acts" (Sober 1993:69).

We will have reason to return to most of the arguments so far summarized. Here I note their general implication: *generating an adaptation explanation is hard work.*

The Role of History in Adaptation Explanations

In outlining approaches to adaptation explanations that enjoy acceptance among evolutionary theorists, I rely on Robert Richardson's explication in "Evolution without History: Critical reflections on evolutionary psychology" (2001). As his title suggests, Richardson maintains that the lack of requisite historical information about the conditions in which human intelligence and language emerged undermines not only the specific explanations of them offered by evolutionary psychologists, but the *raison d'être* of the discipline. His arguments are persuasive and have obvious applicability to the purported evolutionary explanations other social scientists are advancing. I use them, however, to more modest ends. They illustrate the difficulties noted in the previous section, particularly those attendant to evolutionary explanations of uniquely human characteristics. I take them also to reveal that, their claims to the contrary notwithstanding, evolutionary psychologists are not using historical methods to derive "evolutionary" explanations of features of human psychology.[15]

The most direct historical approach to adaptation explanations makes use of population genetics. Citing a similar list proposed by Robert Brandon, Richardson

lays out the criteria that "an ideally complete" adaptation explanation of this type would meet (cf. Brandon 1990). Such an explanation would include information from population genetics about the strength of selection and about variation in the trait in ancestral forms; evidence that differences in the trait among conspecifics were heritable; and information about factors that impact on the rate of evolution (e.g., population size and structure, mutation rates, and gene flow) as well as information concerning whether the trait is primitive or derived. It would also include knowledge of one or more factors in the historical environment (social, physical, etc.) that would render the trait adaptively significant (Richardson 2001, 334–6).

Richardson describes these criteria as those met by an ideally complete adaptation explanation, not criteria we can demand all such explanations to meet. For one thing, explanations making use of the resources of population genetics are more feasible in cases involving microevolution than in cases of macroevolution. Even in the former, however, they are difficult to achieve. Consider, for example, cases involving simple molecules. As Richardson notes, "it is notoriously difficult to distinguish the effects of selection from drift and in the case of complex molecules, it is even more difficult" (2001, 336, cf. Lewontin 1974). In the case of human intelligence and language, Richardson argues, the historical information required for an adaptation explanation using population genetics is not only unavailable but unlikely to become so.

> We have no information concerning the strength of selection, or the nature of variation. We have at best a meager grasp of the ecological and social conditions that are relevant. Heritability rates are unknown. The ancestral structure is something we do not know, and the relevant ancestral traits are also unknown. This is exactly the sort of information that is necessary to show that a given evolutionary change is a consequence of natural selection rather than some other process, using the resources of population genetics. (Richardson 2001, 336, cf. Richardson 1996)

One need not share Richardson's pessimism about the likelihood that the requisite information will become available to grant that it is not now available. Paraphrasing his conclusion, "we must look elsewhere to defend" the purportedly evolutionary explanations social scientists are advancing (2001).

The "comparative method" is a less-direct historical approach to adaptation explanation. It involves comparing a trait to those of phylogenetically related species and in relation to relevant ancestral conditions to determine if the trait conveyed an advantage and originated in a lineage for which its present function is relevant. Richardson notes that using the comparative method to generate an adaptation explanation also requires specific kinds of historical information.

> The method imposes severe requirements: we need comparative data on related taxa, developmental information, information concerning the character of the environment, the trait family under consideration, and the relative adaptiveness of the traits characteristic of the several taxa. (Richardson 2001, 345)

Richardson cites formidable problems confronting any effort to use the comparative method to generate adaptation explanations of human intelligence or language. These include that: the relationships between three of the four hominid species remain unclear and more than one phylogeny enjoys a degree of acceptance; it is not known whether language use is unique to humans or general to the hominid lineage; it appears that increased brain size was a tendency throughout the lineage and the fossil record provides little information about brain differences in structure and function implicated in human intelligence; and the ecological settings of the several hominid species were sufficiently varied to preclude conclusions about which was linked to increased cranial capacity (Richardson 2001, 357–9). Thus, Richardson concludes, "this is hardly an ideal case for comparative analysis" (2001, 359). The traits under consideration might be special to humans or ancestral, and their relationship to historical ecological conditions is unclear.

So we need to take a look to see what methods social scientists are using in their efforts to construct evolutionary explanations. I begin with evolutionary psychology.

Reverse Engineering in Evolutionary Psychology

> Evolutionary theory is to social scientists as statues are to birds: a convenient platform on which to deposit badly digested ideas. (Jones 1999/2000:xxvii)

Evolutionary psychologists, as much if not more than other social scientists, are clear about basic evolutionary notions. They claim to demonstrate that the features of human psychology they posit are adaptations and distinguish this from claims that they are presently adaptive. They also recognize that making a plausible case for, let alone demonstrating, adaptation requires history. Finally, they claim to have and use the requisite historical information. The Introduction to *The Adapted Mind* (Cosmides et al. 1992), a collection whose editors and contributors include prominent members of the discipline, cites each claim as a central premise of the research program.[16]

In this section, I postpone the question of whether evolutionary psychologists make even a minimally plausible case for the specific "evolved psychological mechanisms" they posit. I focus here on the nature of the reasoning that yields "the history" to which they appeal, the reasoning they use to identify specific "adaptive problems" facing ancestral populations, and the reasoning they use to predict specific "psychological mechanisms" selected for in light of "known adaptive problems." I contend that, in each case, reverse engineering is the primary method employed. As the use of this method to derive adaptation explanations has its champions as well as its critics, I begin by summarizing arguments on each side. I then explore cases that demonstrate that evolutionary psychologists fail to

meet the standards outlined by supporters of this method and graphically illustrate the pitfalls its critics cite.

By its nature, reverse engineering is an ahistorical method of explanation in which, beginning from the assumption that a phenomenon is the result of some process of building or "design" (that is, is an artifact in the most general sense), one infers "why" it is the way it is or "why" it came to be. Its use in evolutionary theorizing is uncontroversial when history is irrelevant – for example, when it is used to derive an explanation of a trait's present adaptiveness. But when used to generate adaptation explanations – that is, when present traits or capacities are used to infer historical conditions and causal processes – it is at least controversial.[17]

Some, including Dennett, maintain that reverse engineering is not only appropriate but indispensable to adaptation explanations, on the grounds that biology just is engineering and the work of natural selection just is R & D. Thus, Dennett argues, "the engineering perspective on biology is not merely occasionally useful, not merely a valuable option, but the obligatory organizer of all Darwinian thinking, and the primary source of its power" (Dennett 1985:185, cf. Pinker 1994, Pinker and Bloom 1992). Critics of the method, according to Dennett, were motivated by "a misplaced fear about what it might entail" (1985:185). Dennett's reference here to "a misplaced fear" is misleading, for critics of the use of this method express more than one concern. But he does defend the method against one of them: the charge that beginning from the assumption that a given trait is an adaptation encourages what Steven Jay Gould and Richard Lewontin call "Just So Stories" (Gould and Lewontin 1979). Critics of the method, Dennett argues, "were reacting against a certain sort of laziness: the adaptationist who hits upon a truly nifty explanation for why a particular circumstance should prevail, and then never bothers to test it – because it is too good a story, presumably, not to be true" (1995:242).

> [But] there are good rules of thumb to be followed by the prospective reverse engineer . . . (1) Don't invoke adaptation when other, lower-level explanations are available (such as physics). . . . (2) Don't invoke adaptation when a feature is the outcome of some general developmental requirement. . . . (3) Don't invoke adaptation when a feature is a by-product of another adaptation. (Dennett 1995:247)

Recall that Dennett insists that there are additional requirements when those things to be explained are behaviors or capacities exhibited by "nonstupid species," in particular the need to rule out equally viable explanations such as the discovery of "a good trick." Moreover, in the human case, Dennett concurs with Gould and Richard Dawkins (who don't themselves agree on much) that further requirements need to be met. As Dennett makes this argument, "the very considerations that in other parts of the biosphere count *for* an explanation in terms of natural selection – manifest utility, obvious value, undeniable reasonableness of design – count *against* the need for any such explanation in the case of human

behavior" (1995:487–8, emphasis in original, cf. Gould 1980, Dawkins 1989). To make a plausible case that a trait or capacity enjoying these virtues is an adaptation requires ruling out codiscovery of a good trick, information transmission, and the like.

Critics of reverse engineering point to other problems Dennett does not address (or address adequately). These include: that assuming the ubiquitousness of adaptations threatens to render the hypothesis of natural selection circular; that explanations that assume a trait is an adaptation *cannot* rule out alternative explanations; and that specifying *a priori* or *a posteriori* the conditions that render a trait "optimal" or adaptive is largely "a test of the ingenuity of theorists . . ." (Lewontin 1984:242; cf. Maynard Smith 1984).[18] It is the second and third criticisms that are of most relevance here. That an adaptation explanation is tested, as Dennett calls for, is not sufficient to overcome them. As his own arguments suggest, the evidential warrant of a hypothesis is in part a function of the status of rival hypotheses, not just its testability or confirmation.

Let me clarify the position on reverse engineering that informs the balance of my discussion. If we understand natural selection as an algorithmic process that builds as well as sorts and winnows – and I think Dawkins and Dennett are right in characterizing it this way – then although the explanations that result are not as solid as those arrived at by historical methods, reverse engineering is an appropriate method for generating adaptation explanations. But all such explanations are subject to the norm of explanatory power, with the objects being explained in part determining the kinds of alternative explanation that need to be ruled out. In the case of physical traits and capacities, these include conversion of function, by-products of adaptations, mutation, migration, and so forth. In the case of features of human psychology, the list of viable alternatives is much larger.

It is time to bring these several lines of argument to bear on evolutionary psychology. I begin with what evolutionary psychologists claim to know about "the Pleistocene way of life" in which, they contend, a host of cognitive mechanisms and predispositions were selected for. A close look reveals that the general outline of this "history" is a reconstruction of the ancestral conditions and selection pressures that led to human intelligence and language first proposed by "Man, the Hunter" theorists of the 1950s and 1960s, and subsequently defended and expanded upon by human sociobiologists.[19] According to this theory, the relevant ancestral populations were "Pleistocene hunter-gatherers" and "the hunting adaptation" provided the selection pressures propelling the emergence of a large and complex brain, language, and social organization.

It is not the emphasis on hunting that is of interest here (for despite valiant efforts to resuscitate it, it remains controversial if not largely discredited) but the accounts of "Pleistocene life" that anthropologists proposed and evolutionary psychologists continue to assume and develop. In interpreting fossils and artifacts, and in drawing inferences about ancestral behavior and forms of social organization, architects of the theory relied in no small measure on then-current ethnographic studies of contemporary hunter-gatherers and then-current accounts of social

behavior exhibited by some nonhuman primates. They relied, in short, on reverse engineering – using accounts of behavior in these present groups to draw inferences about the relevant ancestral population.

As previously noted, we are not in a position to determine "the" ancestral population, "its" ecological setting, and so forth, in which uniquely human traits and capacities emerged. Indeed, although evolutionary psychologists' explanations assume a relatively sudden emergence of human cognitive capacities, there is not consensus within evolutionary theorizing as to whether human cognitive capacities emerged gradually or suddenly, or emerged earlier than artifacts would suggest and remained latent until provoked by a change in some aspect of the environment. Even setting these considerations aside (and although one would not know it reading most accounts evolutionary psychologists provide of "the Pleistocene way of life"[20]) the history that resulted has been and remains controversial. Its central features were subjected to substantial critique in the 1970s and 1980s, and on precisely the grounds critics of reverse engineering cite. Anthropologists and biologists demonstrated that there was insufficient evidence to warrant the emphasis on hunting by identifying alternative selection pressures as likely to have been implicated in the emergence of distinctively human characteristics. They also demonstrated that there was insufficient evidence to support the reconstructions of social organization among Pleistocene hunter-gatherers, including assumptions about stereotypical divisions of labor by gender and dominance hierarchies, by identifying alternative models equally commensurate with available evidence by way of artifacts and fossils. Together with primatologists critical of the theory, they also pointed out that neither contemporary hunter-gatherers nor nonhuman primates constitute our ancestral population.[21]

Other criticisms of this reconstruction included charges that the ethnographic accounts of behavior in contemporary hunter-gatherer groups were ethnocentric and androcentric, and that androcentrism characterized the models of primate behavior appealed to. Likewise, it is worth noting the emergence in the 1990s of critiques in archaeology and paleobiology that challenged traditional interpretations of fossils and artifacts, and traditional hypotheses about prehistoric social life, as unwarranted by available evidence and often androcentric.[22] For one need not take a side in any of these debates to acknowledge that the history to which evolutionary psychologists appeal is controversial, and far more so than they typically acknowledge.

I next turn to specific hypotheses advanced in evolutionary psychology that are representative in their reliance on reverse engineering. The first hypothesis, advanced by David M. Buss and others, proposes that women are endowed with an evolved psychological mechanism to prefer men with good economic prospects (Buss 1999, cf. Symons 1979). As I later explore, evolutionary psychologists often appeal to "parental investment theory" to predict sex-differentiated predispositions towards mating and parenting. They also predict sex-differentiated cognitive mechanisms based on what they claim to know was an adaptive problem our ancestors faced, knowledge in turn partly based on studies of contemporary

hunter/gatherer groups. This method is one of two the editors of *The Adapted Mind* advocate.[23]

> By combining data from paleontology and hunter-gatherer studies with principles drawn from evolutionary biology, one can develop a task analysis that defines the nature of the adaptive information-processing problem to be solved. . . . Once one understands the nature of the problem, one can then generate very specific, empirically testable hypotheses about the structure of the information-processing mechanisms that evolved to solve it. (Cosmides et al. 1992:11)

Cosmides et al. maintain that because this method generates testable hypotheses, "it is immune to the usual (but often vacuous) accusation of post hoc story-telling" (1992:11). That is, the abilities to *predict* a cognitive mechanism and to *test* that prediction are supposed to insure that the hypothesis in question is not a "Just So Story." But neither insures that the hypothesis in question meets the norm of "explanatory power," that is, that it is more viable than rival explanations, or that it is warranted by available evidence. Finally, the researcher who uses this method to identify an adaptive problem is again relying on a "history," including paleontological and archaeological hypotheses concerning fossils and artifacts, that is itself largely the product of reverse engineering.

One argument Buss advances for the hypothesis outlined above concerning women's mating preferences is representative in these several respects, and instructive in others.[24] It is representative in that Buss maintains that the prediction of this preference is derived from our knowledge of an adaptive problem Pleistocene females faced. They needed to identify a male able to invest in them and their offspring, given that males would have varied in terms of their access to resources and that females would have been burdened with frequent pregnancies, breast feeding, and child rearing, and thus could not sustain themselves and offspring by gathering (1999:77). This "adaptive problem" is identified using ethnographic studies of contemporary hunter/gatherer groups (1999:79–80, 110–11). Finally, Buss reports that numerous cross-cultural studies confirm that contemporary women are "the descendants" of female ancestors endowed with an evolved psychological mechanism to prefer males with resources, a mechanism they in turn have inherited.

Buss's argument is instructive because, of course, the "adaptive problems" surrounding mating and parenting constitute only a subset of those impacting on inclusive fitness.[25] But evolutionary psychologists, like human sociobiologists before them, devote a great deal of attention to identifying and/or explaining alleged sex-differentiated predispositions to mating and parenting strategies.[26] In addition, many hypotheses they advance in this arena, including that on which we are focusing, were mainstays of human sociobiology. What's "new" are the notions of "evolved psychological mechanisms" (and predispositions) that evolutionary psychologists borrow from cognitive science and graft onto the hypotheses

in question. Finally, as I next explore, in no other domain is evolutionary psychologists' reliance on reverse engineering or the problems that result more obvious.

For the sake of argument, let us grant the account of present hunter/gatherers Buss offers. Let us also set aside one obvious way in which his reasoning is circular: his use of *contemporary* hunter/gatherers to identify *ancestral* problems on the basis of which he allegedly *predicts* that *contemporary* women will have inherited specific psychological mechanisms. Setting these things aside will allow us to focus on a far more serious and general problem with adaptation explanations arrived at using reverse engineering. What is the nature of the *evidence* for the hypothesis that identifying a mate with resources was an adaptive problem facing our female ancestors? It cannot be the cross-cultural surveys of preferences among contemporary women Buss undertakes and cites. These are described as *testing a prediction derived from the prior identification of the historical adaptive problem*. Can the adaptive problem be derived from behaviors attributed to contemporary hunter/gatherers? Again, although it requires that we assume that observing these groups is akin to time travel, let us grant that these groups afford insights into our evolutionary history and, although controversial, let us also grant that they exhibit the behaviors Buss claims. Let us further grant that the case has been made for the historical adaptive problem earlier outlined. Then we need to ask what grounds there are for believing that an *adaptation* in the form of a psychological mechanism was its *solution*. After all, there is an obvious rational explanation for this preference: given that men control the vast majority of resources in most if not all cultures, the preference is highly rational if not what Dennett calls "a forced move." Indeed, freshmen in an introductory course on Darwin identified this alternative in about a minute. All of this reveals that if the reasoning here isn't outright question begging, it is something close to it. To accept this explanation, we must *begin* by assuming that an adaptation *was* the solution, an assumption that is not warranted and, indeed, was that which was to be tested and confirmed.

A more defensible hypothesis from the perspective of evolutionary theory is Leda Cosmides and John Tooby's hypothesis positing that humans are endowed with an innate cognitive mechanism for detecting cheaters (Cosmides and Tooby 1992).[27] Cosmides and Tooby propose the mechanism to explain an apparently common ability by test subjects to solve a logic problem couched in terms that involve detecting a cheater, and an equally common inability to solve the same puzzle presented in abstract mathematical terms. Such a mechanism, they maintain, would have been selected for because of its obvious benefits in maintaining social contracts. Their reliance on reverse engineering is obvious; the psychological mechanism is inferred from inconsistencies in logical reasoning, and the "adaptive problem" it is taken to have solved is inferred from reconstructions of Pleistocene life earlier discussed. This methodological approach is the second the editors of *The Adapted Mind* describe as central to evolutionary psychology.

. . . researchers can start with a known psychological phenomenon, and begin to investigate its adaptive function, if any, by placing it in the context of hunter-gatherer life and known selection pressures . . . to try to understand what its adaptive function was – why that design was selected for rather than alternative ones. (Cosmides et al. 1992:10)

For reasons to be explored in the next section, it is not the commonness of the inconsistency in logical reasoning that warrants the consideration of an evolutionary explanation. It is because this phenomenon might be characterized as "irrational" that it can plausibly be considered to fall into the category Francis Crick called "frozen accidents" and Dennett, alluding to the arrangement of keys on a keyboard, calls "QWERTY" phenomena – phenomena whose present *mal*adaptive nature suggests historical, and in this case evolutionary, origin. There are all too many viable explanations for "rational" aspects of psychology and behavior.

Nevertheless, Cosmides and Tooby's hypothesis suffers from the general problem to which adaptation explanations arrived at by reverse engineering are prone. There are equally plausible alternative explanations for the phenomenon in question. These include: that humans are generally less adept at solving logic puzzles presented in mathematical terms than they are at those presented in applied contexts, that they are less adept at solving logic puzzles couched in unfamiliar terms than familiar, that they often suffer from math phobia, and so forth. Indeed there is a deeper problem. Unless we assume that human cognitive capacities are the product of a process capable of producing "perfect" artifacts (and natural selection is no such thing), the fallibility involved and typically overcome in a basic logic course, in no way calls for a specific adaptation explanation. Finally, as any student in a basic course in evolution knows, two requirements for natural selection are variation and heritability. No case has been made for either.

Case Study: Parental Investment Theory

Deductive arguments are notoriously treacherous; what seems to "stand to reason" can be betrayed by an overlooked detail. (Dennett 1995:49)

The status of sexual selection as an evolutionary mechanism remains a matter of ongoing investigation and debate in evolutionary biology. Some view sexual selection, as Darwin viewed it, as an evolutionary mechanism distinct from natural selection (e.g., Dawkins 1989); others as a useful way to think about processes that are, in the end, subcategories of natural selection (e.g., Bateman 1948); and some maintain that if it is a distinct mechanism, its role is "very meager" (Mayr 1972).

Nor do the disagreements end here. One of Alfred Russell Wallace's objections to sexual selection was the notion of "female choice," which together with

intrasexual selection constituted sexual selection as Darwin defined it. As noted in a recent survey of the relevant literature, disagreements remain about intersexual selection (Spencer and Master 1992). Some concern the methodological difficulties in demonstrating mate choice. Critics maintain that many experiments and field studies claiming to demonstrate female choice do not succeed in eliminating alternative explanations (e.g., Lambert et al. 1982). To cite just one example, it was long assumed that females of lek species tended to mate with males at the center of the lek because males so located were "superior." Partridge and Halliday (1984) among others suggest as an alternative explanation that the central area of the lek provides more protection from predators during mating. The general criticism of explanations appealing to intrasexual selection is familiar; too often, critics argue, alternative explanations are not considered or ruled out.

Against this background, it might seem curious to undertake an extensive analysis of parental investment theory, a cluster of hypotheses its proponents claim to derive from sexual selection. But parental investment theory figures prominently in evolutionary psychology, and both it and the way it is used in the discipline illustrate all of the problems so far identified. Arrived at by reverse engineering, circular reasoning marks the arguments for the theory and the uses made of it. Although it has been subjected to devastating critique in the last three decades, evolutionary psychologists present it as a "mainstream" theory of evolutionary biology that enjoys extensive confirmation. Finally, even its most sophisticated formulation, offered by Dawkins and considered below, is deeply informed by sociocultural assumptions about gender. I begin by briefly summarizing its history.

Parental investment theory traces its origins to some 64 experiments on fruit flies undertaken by geneticist Angus John Bateman in 1948. Bateman took what he found to be a much greater variance in male reproductive success to be significant and to entail "a nearly universal dichotomy in the sexual nature of the male and female . . . a combination of an undiscriminating eagerness in the males and a discriminating passivity in the females," including human males and females (1948:365).

In an article that became the second most-cited article in human sociobiology, R. L. Trivers (1972) introduced what is now called "parental investment theory." Describing Bateman's paper as his "key reference," Trivers proposed that the notion of "parental investment" explains Bateman's results. Gametic dimorphism alone, he argued, dictates that sperm are "cheaper" than eggs, and because female investment in offspring typically extends to gestation or egg-sitting, nursing in mammals, and so forth, it is significantly higher than male investment. This differential investment explains the asymmetry in breeding potential Bateman found and predicts the differences in reproductive strategies (the "undiscriminating" males and "coy" females) traditionally "observed."

As evolutionary psychologists like to point out, parental investment theory is predictive and thus, they claim, avoids the charge of *a priori* reasoning or ad hoc theorizing. I turn shortly to the question of its predictive success. But it should

be clear that the arguments just summarized rely on reverse engineering – Bateman's from experimental results on fruit flies, Trivers' from those results supplemented by the notion of "parental investment."

Evolutionary psychologists rely heavily on parental investment theory to derive hypotheses predicting sex-differentiated adaptive problems and predispositions. The hypothesis earlier considered, that women have an evolved predisposition to prefer mates likely to be good providers, is but one of a number of hypotheses claimed to follow from parental investment theory.[28] Men are predicted to be predisposed to engage in behaviors (including coercive behaviors) to insure paternity, to be predisposed to value fertility in prospective mates more than women do, and to be endowed with cognitive mechanisms that allow them to recognize fertility, to name just a few.[29] These predictions do follow from Trivers' hypotheses when these are supplemented with the notions of "predispositions" and "evolved psychological mechanisms." But like the reconstruction of Pleistocene life that evolutionary psychologists assume, Trivers' hypotheses have been subjected to substantive critiques, most leveled by other biologists. Even a brief summary of these arguments suffices to show that the theory lacks the status evolutionary psychologists claim.

Trivers' core assumption about the relative cheapness of sperm has been challenged on several grounds. These include that males produce millions of sperm for each egg a female produces and, in many species, high sperm production is required for successful insemination; that in some species, high sperm production is correlated with shorter life-span; and that males of a number of species produce costly accessory secretions.[30] Hence, critics argue, a more accurate comparison of parental investment would invoke the minimum number of ejaculations required for fertilization and the cost of a single egg (e.g., Lanier et al. 1975, Dewsbury 1982, Tang-Martinez 2000).

Nor does field research bear out the theory's predictions. Again the literature documenting counterexamples is wide-ranging. An extensive survey article published in 1986 by Sandra Hrdy detailed numerous counter-examples to the "coy female" hypothesis that demonstrated that females of a number of species seek to mate more than once or twice, including when they are not ovulating and/or are pregnant (Hrdy 1986).[31] Field studies involving birds, marine organisms, leopards, and some nonhuman primates report females to be less than monogamous, and an extensive survey published in 1986 detailed numerous counterexamples to the coy female hypothesis (Hrdy 1986). Indeed, at that time Sandra Hrdy reported that "no fewer than six different models to explain how females might benefit from mating with different males" had been proposed to explain reports dating back to 1975 of polyandrous mating (1986:127). These models were proposed in light of the numerous reports of polyandrous mating practices dating as early as 1975. In short, there have been reports of abundant counterexamples to the "coy female" hypothesis predicted by parental investment theory. Finally, this hypothesis seems straightforwardly incompatible with the arguments evolutionary psychologists and others advance for "sperm competition," a third type of

sexual selection purported to select traits to ensure paternity *in light of* poly-androus behavior.

Trivers and other advocates of the theory also take differential investment in offspring to entail that the interests of the sexes are inherently conflicting (glee-fully described as "the battle of the sexes" in publications intended for a lay public): that males are inherently interested in acquiring as many mates as pos-sible, females inherently interested in acquiring a mate who will provide consistent support for them and their offspring. But, critics argue, given that sexual repro-duction is the primary function of courtship in sexually reproducing species, an inherent conflict between the sexes is highly suspect (e.g., Tang-Martinez 2000). As Spencer and Masters (1992) make the argument, "the male–female communication system is subject to strong stabilizing selection." Thus, "unusual or fussy individuals" (an allusion to the "coy females" predicted) would be at a distinct disadvantage, either because they would be more likely to be rejected or more likely to reject suitable mates (1992:301).

Finally, critics point to obvious cases of circular reasoning in applications of the theory. In one well-known example, Trivers notes that in dung flies, "the male who first leaps on top of a newly arrived female copulates with her." But the lack of female choice in this case does not necessarily challenge parental investment theory, Trivers maintains, for it may "result from the *prima facie* case the first male establishes for his sound reproductive abilities" (Trivers 1972:170). As Spencer and Masters note, in this passage "the theory is quite obviously untestable; even when female choice is entirely absent, it is possible to explain the phenomenon in terms of female choice" (1992:296). Reflecting these and the other lines of argument just summarized, they conclude that

> Uncritical acceptance of [sexual selection's] ubiquitous occurrence (for example, in Trivers 1972) does no service to evolutionary theory . . . *heritable variance in repro-ductive success, based on characters that are not favored by natural selection, must be demonstrable before sexual selection may be invoked unequivocally.* (Spencer and Masters 1992:301, emphasis added)

Here Spencer and Masters allude to Darwin's argument for sexual selection and it is instructive to return to his reasoning.

Recall that Darwin proposed the mechanism of sexual selection to explain phenomena that he thought natural selection could *not* explain: traits that seemed to provide no advantage in terms of survival and might work against it. Darwin did not assume that every trait was selected for; but he did view the preponder-ance of such traits as calling for some sort of explanation (if for no other reason than that they might constitute counterexamples to the hypothesis of evolution by natural selection). In other words, the traits to be explained by sexual selection were those that, if one only assumed natural selection, seemed irrational.

Add the components of sexual selection, however, and they no longer seem so. But that does not mean that in order to use sexual selection to explain a behavior,

all we need to establish is its utility. It has precisely the opposite effect. Sexual selection sets severe limits on the traits to be explained on its basis: to *exclude* behaviors or capacities that might be "good tricks" discovered by a nonstupid species and traits natural selection can explain, and to include *only* those for which there is evidence of heritable variation in reproductive success.

There are, then, a number of serious challenges to Trivers' theory. Indeed, no less a champion of it than Dawkins in the first edition of *The Selfish Gene* acknowledges in the second edition that the assumption that sperm are cheap relative to eggs is deeply problematic. But Dawkins remains convinced that there is "a fundamental asymmetry in sex roles" and uses an extensive endnote annotating the original text to propose an alternative explanation (Dawkins 1989:300–1).

It is worth considering. For one thing, Dawkins maintains that the separation of sperm and egg is itself the product of a more fundamental asymmetry in the sexes that closely parallels that advocated by evolutionary psychologists. (So although their appeals to parental investment theory are dated, Dawkins' explanation might provide the alternative evolutionary psychologists need.) In addition, Dawkins' explanation is arrived at by reverse engineering and suffers from the problems to which such explanations are prone. Finally, although more sophisticated than some hypotheses we have encountered, Dawkins' explanation is deeply informed by gender stereotypes.

I quote his explanation (1989:300–1) in its entirety, interspersing comments that point to what I take to be significant problems. Dawkins begins by asking us to "Suppose that we start with two sexes that have none of the particular attributes of males and females. Call them by the neutral names *A* and *B*." Assuming that the *A*s and *B*s have none of the attributes of males and females – and we do need to assume this given that Dawkins is offering an explanation of them – they do not have sperm or eggs. In what sense, then, are these organisms *sexes*? In the body of this chapter, Dawkins maintains the standard definition of the sexes, noting that "one fundamental feature . . . used to label males as males, and females as females, throughout animals and plants" is that male gametes are smaller and more numerous than female gametes (1989:141). So, at this point the *A*s and *B*s are at most *proto* sexes.

Dawkins continues:

> Now, any animal, whether an *A* or a *B*, faces a trade-off. Time and effort devoted to fighting with rivals cannot be spent on rearing existing offspring, and vice versa. Any animal can be expected to balance its effort between these rival claims.

Perhaps. But other rival claims are equally plausible. "Sleep now or eat?" "Look for a mate or groom?" "Look for prey or fight with conspecifics over food or territory?" Or, if the protosexed organisms (which have somehow graduated to *animals*) were, as is likely, too simple to have such options, then "Drift here or there?" Of course if they were this simple, than fighting rivals and parenting were not likely among their behavioral options.

Dawkins next introduces the possibility that the groups "may" have come to "differ."

> The point I am about to come to is that the *A*s may settle at a different balance from the *B*s and that, once they do, there is likely to be an escalating disparity between them.
>
> To see this, suppose that the two sexes, the *A*s and the *B*s, differ from one another, right from the start, in whether they can most influence their success by investing in children or by investing in fighting. (I'll use "fighting" to stand for all kinds of direct competition within one sex.)

Again, why suppose that the differences that "may" obtain between groups of organisms fall on these axes as opposed to others? And why at this point is "fighting" restricted to intersexual competition – or, if we grant the reasonableness of so restricting it, why assume it wouldn't have been in the best interest of all of these organisms?

> Initially the difference between the sexes can be very slight, since my point will be that there is an inherent tendency for it to grow.
>
> Say the *A*s start out with fighting making a greater contribution to their reproductive success than parental behavior does; the *B*s on the other hand start out with parental behavior contributing slightly more than fighting to variation in their reproductive success. This means, for example, that although an *A* of course benefits from parental care, the difference between a successful carer and an unsuccessful carer among the *A*s is smaller than the difference between a successful fighter and an unsuccessful fighter among the *A*s. Among the *B*s, just the reverse is true. So, for a given amount of effort, an *A* can do itself good by fighting, whereas a *B* is more likely to do itself good by shifting its effort away from fighting and towards parental care.

I don't know whether to attach any significance to it, but there are subtle differences in Dawkins' wording here. For *A*s, fighting "makes a greater contribution" to their reproductive success than parental behavior; for *B*s, parental behavior "contributes slightly more" than fighting to such success. An *A* "*can* do itself good" by fighting; a *B* is "*more likely*" to do itself good. Perhaps Dawkins shares my suspicion that in terms of simple and as yet unsexed organisms, it is less plausible to attribute "parenting" than it is "competing." The latter, after all, might be accomplished by drifting faster or sideways.

In any event, Dawkins maintains that if we grant his "starting" assumptions, we are well on our way to an explanation of both a fundamental asymmetry in sex roles and gametic dimorphism.

> In subsequent generations, therefore, the *A*s will fight a bit more than their parents, the *B*s will fight a bit less and care a bit more than their parents. Now the difference between the best *A* and the worst *A* with respect to fighting will be even greater,

the difference between the best *A* and the worst *A* with respect to caring will be even less. Therefore an *A* has even more to gain by putting its effort into fighting, even less to gain by putting its effort into caring. Exactly the opposite will be true of the *B*s as the generations go by.

   The key idea here is that a small initial difference between the sexes [sic] can be self-enhancing: selection can start with an initial, slight difference and make it grow larger and larger, until the *A*s become what we now call males, the *B*s what we now call females.

Dawkins concludes by noting that his explanation of asymmetrical sex roles reverses the common trajectory of such explanations. The separation of sperm and egg does not explain all the "characteristics" of males and females. Rather, an initial division between organisms that gained from parenting and those that gained from competing explains both it and such characteristics.

   There is nothing wrong with the logic of Dawkins' argument. But I suggest that his explanation is either circular or a "Just So Story," the central assumptions of which we have no reason to accept. Questions and comments interjected along the way reflect both worries. Although Dawkins does not assume gametic dimorphism, he does assume that for some protosexed organisms, the available and good reproductive strategies were fighting rivals and parenting. In the initial stages of the argument, he is careful to couch what he says about "initial" and "slight" differences in terms of possibilities. But differences in reproductive strategies, let alone of these kinds, too closely mirror what is to be explained.

   Dawkins also maintains that, in relation to one another, each of these reproductive strategies would have undergone runaway selection. This may hold in the cases in which all the *B*s and all the *A*s settled, respectively, on parenting and fighting. But as Dawkins notes in the same work, field research reveals four combinations of reproductive strategies, and computer simulations indicate that each can attain stability: males desert (fight rather than parent), females care (parent); males care, females desert; males care, females care; and males desert, females desert. Stability can also be reached in populations that display either the pair "male deserts/female parents" and "female deserts/male parents," or the pair "both desert" and "both parent" (Dawkins 1989:302). But given *these* results, the situation that we are asked to suppose started the ball rolling toward sex and "asymmetrical sex roles" is again indistinguishable from what Dawkins claims to explain. It is just as plausible that all the organisms with which the story begins would have settled on "parent."

   Consider the alternative: that Dawkins' explanation does not presuppose what it was meant to explain. Then we have been given no reason to assume important features, however abstract, of the hypothetical situation he describes – including an initial difference between two groups of unsexed organisms – and that these involved tendencies toward "competing" and "parenting." The concluding sentences of Dawkins' argument are these: "The initial difference can be small enough

to arise at random. After all, the starting conditions of the two sexes are unlikely to be exactly identical" (1989:301). They reflect, respectively, the kind of "Just So Story" whose details aren't argued for, and the circularity, I have suggested.

I have argued that Dawkins' explanation of asymmetrical sex roles presupposes rather than explains them, an all too common problem in the domain of sex differences.[32] But there is an irony worth noting before concluding this section. Although Dawkins notes that the stereotypes of coy females and promiscuous males may "strike a chord" when we think of human behavior, he also maintains that in this and other cases there are significant difficulties in applying the principles involved to humans (Dawkins 1989:164). It is in this work that he maintains that "we must begin by throwing out the gene as the sole basis of our ideas on evolution" for understanding human evolution and modern humans, and that he introduces the notion of "memes" as units of "cultural transmission." The notion of memes, he contends, is a more appropriate explanatory notion than genes in the study of human behavior (Dawkins 1989:189–201). Publications in evolutionary psychology are replete with appeals to sociobiology, including to Dawkins' work, but I have yet to find this argument positively cited.

I have argued in this and the previous section that although reverse engineering is an appropriate method for deriving adaptation explanations, the explanations making use of this method that are being advanced in evolutionary psychology and human sociobiology fail to meet the standards advocates of the method call for, and illustrate the problems its critics cite. I have not yet considered whether social scientists and psychologists make even a minimally plausible case for the cognitive mechanisms they propose and/or the "respectability" of these purported mechanisms as "scientific objects." I now turn to these questions.

Rules that "Fit" vs. Rules that "Guide" Behavior

> In all this talk [of an innate depth grammar] there is no folly, I feel sure, that conscientious reflection on method and evidence cannot cure; but the cure is apt to take time. (Quine 1972:447)

Another line of reasoning common in the relevant literatures proceeds from an alleged "universal" in human behavior, to an argument that the universality constitutes evidence of an innate cognitive structure or predisposition, to an explanation of this cognitive feature as an adaptation. Critics of human sociobiology and evolutionary psychology repeatedly point out that universality does not entail "innateness," and that "innateness" is insufficient to demonstrate adaptation. A third line of critique challenges the "universals" themselves, charging that the degree of vagueness required to make the case for them (even when the claim is limited to human behavior) results in "objects" whose status as scientific objects is at least dubious (e.g., Dennett 1995). And a fourth challenges arguments for

adaptations that appeal to alleged universals across species, insisting on the distinction between homologous traits and analogous traits (e.g., Burian 1978, Gould 1977).

I emphasize a problem less often noted but equally significant. The lynchpin of adaptation explanations using this line of reasoning is the assumption that identifying a cognitive algorithm or predisposition that *fits* some aspect of human behavior is sufficient to demonstrating that the feature of psychology posited *guides* this behavior. The explanations advanced in evolutionary psychology and earlier considered are representative in this regard, as is the hypothesis here considered.[33]

Cognitive anthropologist Scott Atran proposes that universal features of folk taxonomies for living things, features found across cultures and throughout history, suggest that humans are endowed with a cognitive module aiding in the classification of, and thus inductive reasoning about, plants and animals (Atran 1995). Atran's first line of argument is from universality. He appeals to anthropological research that suggests several relevant universals: first, that "virtually all humans in all cultures" divide living things using the categories "plants" and "non-human animals"; second, that whatever the particular constitution of a culture's life form groups or taxa, "the life form level or *rank*" divides living things in hierarchical and mutually exclusive partitions; and, finally, that plants and animals are perceived to be "natural kinds," whose "essence" determines their morphology and behavioral characteristics (1995:206–8). To further the case for an innate basis for these phenomena, Atran also cites a psychological study reporting that infants pay more attention to animate than inanimate objects and can in their first year "distinguish plastic representations of animals from plastic representations of all other things" (1995:206). In a third line of argument, Atran appeals to a study comparing the folk taxonomy of the Itza of Guatemala with that of university students raised in rural Michigan. This found cases in which the two folk taxonomies would group animals (e.g., felines and canines), or classify individual species, in much the same way *and* not in keeping with scientific classification. Atran takes the study to suggest that "there are at least some universal cognitive factors at work in folk-biological classification that are mitigated or ignored by science" (1995:212).

On the basis of these arguments, Atran proposes "an innate living-kind module": an innately driven "research program" that compels people "to deepen and extend the domain of information relevant to living kinds into an all-embracing taxonomy" (Altran 1995:220). As the tendency to classify objects as "natural kinds" does not extend to inanimate or inert objects, Atran contends that it is plausibly explained as an adaptation, noting the evolutionary advantage to "acquiring 'automatic' competency" in understanding living species and being able to reason inductively about them (1995:222).

Presumably, Atran views the research devoted to infants' attention spans as ruling out information transmission, mimicry, and the like, as plausible explanations of the purported universals in folk taxonomies (although it seems quite a

leap to infer an innate *research program* on its basis). Likewise, Atran may take the assumption of "natural kinds" characteristic of folk taxonomies to be just the kind of irrational phenomenon lending itself to an adaptation explanation. Finally, the innate research program he proposes would certainly be advantageous in the ways he proposes. But although Atran may have succeeded in making a case that the innate research program he posits fits aspects of infant and adult behavior, he has not made the case that any such program guides it.[34]

Insofar as it is possible to identify an alternative research program that equally well fits the behavior in question, no case has been made that the program proposed guides it.[35] We can use examples from Quine's argument for the indeterminacy (or inscrutability) of reference to illustrate the problem. An innate research program that takes "kinds of event" rather than "kinds of living thing" as its basic ontological category is extensionally equivalent to that which Atran proposes, as is a research program whose basic ontological categories are Platonic forms and instantiations of them. Thus, there is no evidence to warrant the conclusion that one of these "unconscious programs" is guiding the behavior in question. Moreover, if Quine is correct about the indeterminacy of reference, arguments for cross-cultural universals in taxonomies are nonstarters. There is no "fact of the matter" in terms of the ontologies we or anyone else is committed to (Quine 1969).

To return to the general argument of this section, the need to demonstrate that a proposed cognitive algorithm or predisposition does more than fit some aspect of human behavior carries serious implications for evolutionary psychology. A core methodological assumption of this research program is that the universal human nature it posits exists at the level of "evolved psychological mechanisms," *not* of (expressed) cultural behaviors (Cosmides et al. 1992:5). Of course it would be remiss not to note that, as we saw in the previous section, evolutionary psychologists *do* appeal to "expressed behaviors" in their attempts to devise evolutionary explanations.[36] But given their claims to identify a human nature existing at the level of psychology, the *rationale* of this discipline depends on the ability to demonstrate guidance, not mere compatibility. The warrant for any posited cognitive mechanism consists, in part, of there being no *alternative* algorithm or predisposition that equally fits the behavior in question.[37] This is a high bar. Reverse engineering cannot catapult evolutionary psychology over the explanatory hurdles with allusions to the technical notions of "cognitive modules" and "adaptation."

Responsible Science

Evolutionary psychology provides some of the most important tools for unlocking the mysteries of . . . the mechanisms of the mind that define what it means to be human. (Buss 1999:411)

Advocates of the turn to evolutionary biology commonly advance a particular line of argument for the "vertical integration" they envision between the social/behavioral and biological/natural sciences. It starts from the premise that some science is value-laden, of which "old" social science paradigms such as "behaviorism" and "cultural relativism" are argued to be prime examples, and some science is value-free, of which evolutionary biology is put forward as an exemplar. Thus, the argument goes, using evolutionary biology to explain human psychology and behavior is a way for the social sciences to achieve the objectivity (in the sense of "value-neutrality") the natural and biological sciences enjoy (e.g., Brown 1991, Cosmides et al. 1992).

There are two appropriate rejoinders to this line of argument. The most obvious is that compelling evidence has emerged in the last five decades that challenges the "value-freedom" traditionally attributed to the sciences. Reflecting this, there is important work under way, much of it supported by organizations such as the National Science Foundation and involving collaborations between scientists and science scholars, to investigate the ways in which noncognitive values function in scientific practice – often *positively*. In addition, the debates over the value-ladenness of core tenets of Darwinian theory, including but not limited to its obvious parallels with economic theory, are well known, and those over human sociobiology notorious.

Closer to home, we have explored a hypothesis about women's evolved mating preferences that is representative of others advanced in evolutionary psychology in purporting to explain what Kitcher (1985) dubs barroom stereotypes about gender. Finally, our exploration of parental investment theory demonstrates that even the most sophisticated theorizing, theorizing not argued to have implications for humans, can be deeply informed by sociocultural assumptions about gender.

The second response was perhaps best articulated by Kitcher in *Vaulting Ambition*, his critique of human sociobiology. As he noted there, "*The dispute about human sociobiology is a dispute about evidence*" (1985:8, emphasis in original). But although all sides in this dispute might agree that the social and political implications of sociobiology's explanations should be assessed separately from their evidential warrant, Kitcher argued, things are not so simple. "*Given sufficient evidence*," we should of course accept a hypothesis about human nature – regardless of its political implications (1985:9, emphasis in original). But the question of how much evidence *is* sufficient to warrant the adoption of such hypotheses is not independent of their social, political, and/or ethical, implications.

> If a single scientist, or even the whole community of scientists, comes to adopt an incorrect view of the origins of a distant galaxy, an inadequate model of foraging behavior in ants, or a crazy explanation of the extinction of the dinosaurs, then the mistake will not prove tragic. (Kitcher 1985:9)

In contrast, Kitcher notes, there may be grave consequences if we adopt an incorrect hypothesis about human nature that carries social and/or political

implications. In such cases, it is not only reasonable but responsible to ask for more evidence than we might otherwise (1985:8–10).

Note that this is not an argument calling on scientists to abandon a research agenda or a hypothesis on political and/or moral grounds (although this is a brush with which evolutionary psychologists like to tar their critics). It is an argument that calls on scientists to uphold stringent epistemic standards in assessing the evidential warrant of hypotheses when there are social and/or ethical consequences to being right or wrong. And although this argument does recognize and indeed emphasize the social and ethical implications of scientific hypotheses, it does not call upon scientists to wade into the social or ethical issues at stake. (Indeed, there is no reason to think they are well equipped to do so). Is an argument that the cognitive authority scientists are granted, claim, and exercise carries moral responsibilities. To meet them, scientists need not do any more or less than what they were trained to do: good science.

Kitcher's argument is appropriately brought to bear on the research here considered. The central question to ask about these research programs concerns their evidential warrant. Accordingly, my larger discussion has been devoted to demonstrating that judged in light of basic tenets of evolutionary biology, and the norms of explanatory power and empirical success, the evidential warrant for the methods being used and the hypotheses being advanced is sorely lacking.

It is also true that many of the hypotheses evolutionary psychologists advance carry significant social and moral implications. These include purportedly evolutionary explanations of rape (Thornhill and Thornhill 1987); the positing of a predisposition to "sexual jealousy" in men to explain domestic violence and other coercive behaviors (Thornhill and Thornhill 1992, Wilson and Daly 1992); and purportedly evolutionary explanations of divisions in labor and innate temperament by sex (Buss 1999). Caveats to the effect that to claim that evolution endowed us with one or another of these "evolved mechanism" is not to claim that the mechanism is *presently* adaptive, go no way at all toward mitigating such implications. Having posited these features of human psychology and often describing them as "confirmed," evolutionary psychologists and others who advance them are in no position to control how their explanations are used (nor, of course, would we want them to be). One must also scrutinize claims that evolutionary psychology does not assume genetic determinism, also designed to deflect responsibility for their potential consequences and the kind of argument I am advancing. Decoupling evolutionary psychology from any form of biological determinism undermines whatever rationale there might be for this discipline. Finally, Kitcher's critique of human sociobiology demonstrated that sociobiology's explanations of aggression, of "sex roles," and the like were in not, as sociobiologists claimed, "new" hypotheses. They represented efforts to graft notions from contemporary evolutionary theorizing on hypotheses first advanced by Social Darwinists. The well-known poverty of Social Darwinism, and the failure by sociobiologists to provide any new evidence to support their own versions of these hypotheses, undermine arguments that seek to defend human sociobiology

as "a new and promising research program," and to absolve human sociobiologists of responsibility for the social or ethical consequences of their claims. If I am correct that many of the hypotheses evolutionary psychologists advance are versions of those advanced by human sociobiology to which notions from cognitive science have been grafted, such arguments in defense of this research program are also undermined.

Citing human sociobiology's empirical failings and significant social implications, Kitcher quotes the narrator at the conclusion of *The Great Gatsby* as noting that "carelessness that results in the destruction or diminution of human life is unforgivable" (1985:10). There is no excuse for failing to meet standard norms such as explanatory power, for ignoring relevant and obvious counterevidence, and/or for misrepresenting the status of the hypotheses to which one appeals. Science so characterized just is bad science. When the hypotheses generated carry significant implications for social policy and/or human self-understanding and aspirations, as do many of those advanced in evolutionary psychology, such research is appallingly irresponsible.

Notes

1 Jack Nelson and Paul Roth provided extensive feedback on earlier versions of this paper, and Robert Richardson also provided helpful feedback. Earlier versions were presented to the STA Colloquium at Washington University and the Philosophy Colloquium at the University of Washington. I am grateful to members of these audiences for helpful comments and criticisms, particularly Garland Allen, Ken Clatterbaugh, Arthur Fine, Andrea Woody, and Alison Wylie. Parts of sections four and six were written jointly with Jack Nelson and presented at the PNP Colloquium at Washington University (Nelson and Nelson 2000a). Again, thanks to members of the audience for their comments and criticisms.

2 Contributors to Barkow et al. (1992), Buss (1999), and Schubert (1989), offer arguments charging critics with one or more of these motivations.

3 For example, the editors of *Sex, Power, Conflict: Evolutionary and Feminist Perspectives*, who are evolutionary psychologists, insist there is a clear distinction between the goals, rationale, and methods of evolutionary psychology, on the one hand, and those of feminism (more correctly, what they characterize as feminism) on the other. Predictably, they claim that evolutionary psychology is concerned with "how things are," and "feminism" with "how things should be" (Buss and Malamuth 1996:3–8). They fail to acknowledge that many feminist critiques of evolutionary psychology are offered by *scientists*, indeed biologists, and emphasize lack of *evidential warrant* for hypotheses being advanced in the discipline.

4 Kitcher's argument, outlined in my concluding section, occurs in his critique of human sociobiology in *Vaulting Ambition* (Kitcher 1985).

5 I assume a holistic account of evidence, according to which the evidence for any specific hypothesis or claim includes *both* the observation consequences of that hypothesis or claim (together with the larger theory or theories within which it is embedded)

and the relationship of the hypothesis or claim in question to other accepted theories (Nelson 1996).

6 I use scare quotes for "new" because I will later contend that in the case of evolutionary psychology's approach to mating and parenting strategies, the hypotheses advanced are well-worn hypotheses of human sociobiology to which evolutionary psychologists simply add technical notions they borrow from cognitive science.

7 I leave it to those better versed in recent social science literature to respond to claims that older social science paradigms are dead and/or that integrating evolutionary biology into their explanations is the only way for the social sciences to achieve respectability (as argued for in Brown 1991, Cosmides et al. 1992, Rosenberg 1980, and Schubert 1989, among other places). Sometimes the accounts given by advocates of these programs of the "alternatives" available to social scientists are unduly limited, as is Cosmides et al.'s description of "The Standard Social Science Model" in chapter 1 of *The Adapted Mind* (1992). And sometimes, the accounts of available alternatives are ridiculous. For example, Buss (1999) cites "seeding theory" and "creationism" as the only alternatives to an evolutionary approach to human psychology and behavior.

8 I am grateful to Ken Clatterbaugh, Arthur Fine, and Andrea Woody whose comments on an earlier version of this paper made clear the need to make this argument explicit.

9 A number of research programs in the social sciences seek or claim to incorporate evolutionary theory in their explanations of human psychology and culture, and there are differences in the nature of the relationship they envision with evolutionary biology. (Compare, e.g., Brown 1991 with Schubert 1989.) I emphasize evolutionary psychology for several reasons. It is the most visible of these programs, outside as well as within academia and I confess to having no patience with the practice of writing off publications aimed at the lay public as "howlers" that are not worthy of serious attention. Such was the response two decades ago to the critiques feminist scientists leveled at human sociobiology; feminists, critics charged, were focusing on an obvious case of bad science that no one took seriously. But its closest descendant, evolutionary psychology, propounds many of the same hypotheses – albeit dressed up in the garb of technical notions from cognitive science. As discussed in my concluding section, many of the hypotheses advanced to a lay audience carry significant social implications and thus do warrant serious attention by philosophers of science. As much to the point, the goals, research questions, and methodological/metaphysical assumptions of evolutionary psychology are influential in other social sciences. This is reflected in the use of "evolutionary psychology" by other social scientists to describe their research, and in their appeals to notions such as "cognitive modules" and "evolved psychological mechanisms." Used here, the phrase denotes the narrower research program but many of the arguments I advance apply more broadly.

10 As explored below, the methods used to arrive at evolutionary explanations also differ because some are more feasible in explaining cases involving microevolution than they are in cases of macroevolution, and because they make use of different kinds of historical information.

11 There are of course disagreements about the "units of selection," and a number of arguments that natural selection only works at the level of organisms, not that of genes (see, e.g., Dawkins 1979, Mayr 1988:ch. 8; Ruse 1988:ch. 4 provides a good overview of these disagreements). I emphasize its role in selecting for traits because this is the emphasis of the research I consider.

12 Arguments representative of the views outlined in this paragraph include Brandon (1984), Burian (1983, 1992), Gould and Lewontin (1979), Maynard Smith (1984), Mayr (1983, 1988), Richardson (2001), and West-Ebelard (1992).

13 Cf. Brandon (1984), Mayr (1988), and West-Ebelard (1992). Ernst Mayr notes that the majority of evolutionary biologists now recognize that many phenotypic traits are by-products of evolutionary processes and are "quite cautious" in what they attribute to natural selection (Mayr 1988:136).

14 Dennett uses "a good trick" to denote "a behavioral talent that protects [an organism] or enhances its chances dramatically" (1995:77–8); and, borrowing from chess, "a forced move" to denote an adjustment in behavior to achieve "the one and only one solution to staving off disaster" (1995:128).

15 Here I diverge from Richardson's approach, which takes evolutionary psychology as using historical methods but not successfully.

16 Cosmides et al. note that:

> the central premise of *The Adapted Mind* is that there is a universal human nature . . . that exists primarily at the level of evolved psychological mechanisms, not of expressed cultural behaviors. . . . A second premise is that these evolved psychological mechanisms are adaptations, constructed by natural selection over evolutionary time. . . . A third assumption . . . is that the evolved structure of the human mind is adapted to the way of life of Pleistocene hunter-gatherers. . . . (Cosmides et al. 1992:5)

17 Good discussions of the issues involved include Dennett (1995), Lewontin (1977, 1978), Maynard Smith (1984), and Richardson (2001).

18 Cf. Dupre (1998), Gould and Lewontin (1979), and Richardson (2001).

19 For example, Washburn (1961), Lee and DeVore (1968), Wilson (1978), Tooby and DeVore (1987).

20 In this sense, Buss (1999) is an exception for he does note some of the debates I next mention. But he also contends that the hunting adaptation and "Man, the Hunter" theory provide the most viable explanations of the evolution of language and social organization, and what evolutionary psychologists and human sociobiologists describe as the sexual division of labor.

21 The literature mentioned in this paragraph is extensive. Representative critiques can be found in Bleier (1984), Haraway (1978a, 1978b), and Tanner and Zihlman (1976).

22 See the works cited in n.21 above, Conkey and Spector (1984), and Wylie (1997), for overviews of the critiques cited in this paragraph.

23 In other words, technical terms from cognitive science are taken to yield a "new" causal theory that links evolutionary biology and the social sciences.

24 Buss also offers an argument based on parental investment theory for this hypothesis. I discuss parental investment theory in the next section.

25 As Sarah Hrdy puts this point,

> [In the case of debates about sexual selection theory], as always in such debates, reality exists in a plane distinct from that predefined by the debate. In this case, reality is hours and hours, sometimes months and months, of existence where sexual behavior is not even an issue, hours where animals are walking, feeding, resting, grooming. (Hrdy 1986:124)

26 For example, David Barash noted that sociobiology "relies heavily on male-female differences" (Barash 1977:283); similarly Buss maintains that "if selection has not sculpted psychological mechanisms designed to solve adaptive problems posed by sex and mating, then evolutionary psychology would be 'out of business' before it even got off the ground" (Buss 1999:97).

27 I explore below why this hypothesis is more defensible from the perspective of evolutionary theorizing, but there are other compelling criticisms of this hypothesis. See, for example, Fodor (2000).

28 For example, in Buss (1999), Daly and Wilson (1981, 1983, 1990), Symons (1979, 1992). In *The Adapted Mind* alone (Barkow et al. 1992), 11 of 18 articles draw on one or more aspects of Trivers' theory of parental investment to derive hypotheses positing mating and/or parenting strategies.

29 See the works by Buss, Daly and Wilson, Wilson and Daly, Symons, and Thornhill and Thornhill in the reference list.

30 Current hypotheses as to why there are so many more sperm relative to eggs do nothing to further the case that male investment is minimal. They include that sperm "compete" with each other; that the relatively large number of sperm reflects the "hostility" of the female reproductive tract; and that females in some species are able to defer fertilization and "select" sperm after numerous copulations. Tang-Martinez (2000) provides an extensive survey of the biological literature challenging parental investment theory.

31 See, for example, Eaton (1978), Hrdy (1986), Gladstone (1979), Hunter et al. (1993), Lanier et al. (1975), Smith (1988), Tang-Martinez (2000), and Van Voorhies (1992).

32 For example, Buss (1996) advocates a claim he attributes to Symons (1992): "To an evolutionary psychologist, the likelihood that the sexes are psychological identical in domains in which they have recurrently confronted different adaptive problems over the long expanse of human history is essentially zero" (1996:301). But of course *that* the sexes have consistently confronted different adaptive problems is not yielded by historical research. It is either taken to be entailed by parental investment theory or simply assumed and thus circular. Finally, although Buss attributes this claim (which he cites without quotes) to Symons (1992), it is not so stated in this article although it can be taken as an implication of Symons' arguments.

33 This section builds on an argument advanced in Nelson and Nelson (2000a).

34 Since writing this section, I have read recent publications by Atran that include more nuanced and technical arguments, and back off some of his earlier conclusions about the exact nature of the cognitive mechanism underlying folk taxonomies. But the point of the present discussion – that demonstrating that a proposed cognitive mechanism *fits* an aspect of human behavior is not sufficient to demonstrate that it *guides* it – applies to this recent work as well.

35 A classic argument for the difficulties in establishing the stronger claim was made by Quine in his critique of Noam Chomsky's claim that an innate language structure facilitates language acquisition and underlies verbal behavior (Quine 1972). It is one thing, Quine pointed out, to take language at face value and speak of every known language being translatable into a language structured by referential position, bound variables, logical operators, and the like. It is another to claim that this is the *only* way to translate or parse the language (it is not), and yet a further step to suggest there is

evidence that infants *use* or *prefer* one set of unconscious rules to another, extensionally equivalent set in understanding and generating linguistic output (Nelson and Nelson 2000a). "What is more than trivial in the new doctrine," Quine argued, is that "it imputes to the natives an unconscious preference for one system of rules over another, equally unconscious, which is extensionally equivalent to it" (1972:443).

36 As we saw in the last section, evolutionary psychologists use studies of contemporary hunter-gatherer groups to identify ancestral adaptive problems; they appeal to cross-cultural surveys to support hypotheses positing evolved psychological mechanisms; and they build from findings in psychological research to derive hypotheses positing cognitive mechanisms (e.g., the algorithm for detecting cheaters earlier discussed). Perhaps Cosmides et al. (1992) emphasize that the human nature they posit is at the level of psychological mechanisms to forestall the kinds of counter-examples in "expressed cultural behaviors" that bedeviled human sociobiologists. Evolutionary psychologists also emphasize their focus on psychological mechanisms in arguments that theirs is a "genuinely new discipline," not (as it might appear) a version of human sociobiology (e.g., Buss 1999:ch. 1).

37 John Dupre offers a similar argument:

> With arguments for the existence of [evolved mental modules] almost exclusively a prioristic, and evidence for the behavior they are supposed to generate equivocal at the very best, we should treat this school of genetic determinism [evolutionary psychology] with all the respect that thinking people have come to accord sociobiology. (Dupre 1998:162)

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How Standpoint Methodology Informs Philosophy of Social Science

Sandra Harding

Standpoint theory re-emerged in the 1970s and 1980s as a feminist critical theory of the relations between knowledge and power, and also as a guide to improving actual research projects – as a methodology. Standpoint theory authors argued that existing relations between politics and the production of knowledge were far from the scientifically and socially progressive ones claimed by economic, governmental and legal, medical, health, education, welfare, scientific and other dominant institutions and the research disciplines that serviced them. When it came to understanding and accounting for gender relations, the prevailing philosophies of science and the research projects they legitimated were politically regressive, philosophically weak, and scientifically less than maximally effective.

Evidence for such claims appeared in the increasing documentation of sexist and androcentric results of research in biology and the social sciences. Standpoint theorists analyzed causes of the gaps between actual and ideal relations between knowledge and power, and reflected on causes of the successes of feminist research in the social sciences and biology. Such work led to prescriptions in some of these writings for how to produce empirically and theoretically more successful research. Even though such research was guided by feminist political goals, such epistemic and scientific successes were possible because certain kinds of "good politics" in themselves had the potential to advance the growth of scientific knowledge: some kinds of politics could be productive of knowledge.[1]

Such arguments insured that the subsequent history of standpoint theory would be a tumultuous one. In the fast-moving world of feminist theory, where few analyses have been unchallenged within the field, let alone outside it, one observer notes that standpoint theory has nevertheless managed to achieve a certain distinction:

> Standpoint theory may rank as one of the most contentious theories to have been proposed and debated in the 25–30 year history of second wave feminist thinking about knowledge and science. Its advocates as much as its critics disagree vehemently about its parentage, its status as a theory and, crucially, its relevance to current feminist thinking about knowledge. (Wylie forthcoming)

Some reasons for its contentiousness are immediately apparent: it challenges or, worse, undermines still widely held dogmas of rationalism, empiricism, and positivism. Some of these, such as the commitment to the exclusion of politics from and selection of value-neutral methods of research, have been retained in some forms of postpositivism.[2] Moreover, critics persist in attributing to standpoint writings three positions that standpoint theorists mostly never held or perpetrated, and have subsequently again and again denied and countered. These are essentialist assumptions about women (and men), assumptions that claims to knowledge by the oppressed are always automatically privileged, and commitment to or unintended perpetration of a damaging epistemological relativism.[3]

Yet other sources of its contentiousness may be less obvious. For example, the histories of standpoint theory's independent development and subsequent debate within several distinct disciplinary contexts, with their different histories and preoccupations, have produced emphases on different aspects of it. This is clear if one examines the concerns of theorists and researchers working in the sociology of knowledge, political philosophy, and the philosophy of science and epistemology, to mention three central sites of standpoint development. Moreover, the theory has served as a site for debate about more general intriguing or troubling contemporary intellectual and political issues that are only partially about rethinking the positivist legacy. Is it postmodernist or antipostmodernist, stuck in modernity or creatively updating modernity's problematics? Is its goal to recover the (epistemically privileged) "ways of knowing" of the (essentialized) oppressed, to produce "situated knowledge," "ethnoscience," or "identity epistemologies," or instead to engage in ideology critique? Is it a kind of ethnography or a critical theory? Is it an epistemology, a sociology, a methodology, or a social theory? What are its uses for anti-Eurocentric and postcolonial projects? What is its use for the natural sciences and post-Kuhnian science and technology studies?

After all this controversy, what is surprising is that standpoint theory appears to have survived and even to be flourishing anew after more than two decades of contention. In the last few years a flurry of reappraisals has begun to chart its recent history and its still promising potential (Campbell and Manicom 1995, Garcia Selgas forthcoming, Harding forthcoming b, Hekman 1997,[4] Kenney and Kinsella 1997, Pels 1997, Wylie forthcoming). Moreover, other authors continue to do it the honor of claiming it to be a serious threat to feminism, science, and civilization more generally (Gross and Levitt 1994, Walby 2001[5]). Disturbing though virtually everyone may find some or other of its claims and projects, standpoint theory apparently is destined to persist at least for a while as a seductively volatile site for reflection and debate about persistent contemporary dilemmas.

This chapter focuses on just one part of these debates and discussions: the methodology issues and the challenges they raise to conventional epistemology and philosophy of natural and social sciences. Philosophers of science and political philosophers have tended to focus on standpoint theory as an epistemology. Yet Dorothy Smith, Patricia Hill Collins, and other empirical social science researchers have always understood it as a prescription for research practices. Some explicitly see it as a particular kind of participatory action research. Standpoint approaches have been widely taught and used nationally and globally as a research methodology in social science departments, professional schools, and independent research institutes and projects. They have been used as a theory of how to improve the scientific quality and political effects of research projects. In such contexts, standpoint theory enters debates about how to counter the inherently "colonial" politics and scientific/epistemic challenges of social research projects, and thereby highlights one kind of threat standpoint theory raises for even postpositivist epistemologies and philosophies of science.[6] Thus methodological considerations provide resources for epistemological and philosophy of social science arguments. And they also suggest one important reason why this theory can persist in spite of widespread unease about it: it does tend to produce, as well as to "rationally reconstruct," empirically and theoretically more robust research results.

The next section briefly outlines the origins of standpoint theory and some major themes that have appeared in the diverse histories of its development. These origins and themes shape central strengths of its development as a research methodology, which are identified in the second section.

Standpoint Logic: Origins

Frederic Jameson points out that it was not until the work of feminist theorists of the 1970s and 1980s that Lukacs's abandoned arguments about the logic of the Marxian "standpoint of the proletariat" were again pursued (Jameson 1988, Lukacs [1923] 1971).[7] Yet this "logic of inquiry" has other sources in which such a legacy is not directly visible. Moreover, echoes of its themes appear in other writings. The sociologists also cite powerful themes in Mannheim ([1929] 1936), Merton (1972), and Simmel (1921) about the resources provided by the "stranger's" social position. Collins develops this into the "outsider within" position of trained researchers from marginalized social groups – a position that is capable of detecting aspects of social relations not accessible by those who are only outsiders or only insiders (Collins 1986, 1991).[8] The writings of Paulo Freire (1970) and the participatory action researchers also echo standpoint themes (McTaggart 1997, Maguire 1987, Petras and Porpora 1993).[9]

Other feminist writers at least partially developed standpoint arguments independently. For example, Catharine MacKinnon's (1983) influential essay on

jurisprudence advanced standpoint arguments on how we can see that "the state is male" in its insistence on standards for the purportedly objective understanding of violence against women that coincide with the way men but not women understand and evaluate the existence and nature of such violence.[10] From a perspective in post-Kuhnian science and technology studies and the history of primatology, Donna Haraway (1981, 1991) engaged with standpoint authors on their ambivalence toward their own insistence on the situatedness of knowledge – a notion which she influentially articulated. Kuhn's project "to display the historical integrity of . . . science in its own time," as Kuhn famously put the point ([1962] 1970:1) opened the way for thinking about the "historical integrity" of social and natural science conceptual frameworks, methods and practices with the gender relations of their day.[11] Meanwhile, a standpoint logic can now be seen to structure much recent work in the multicultural and postcolonial science and technology studies movements: Western natural sciences and technologies, no less than those from other cultures, are "local knowledge systems." Western sciences have a "historical integrity" with the global social relations of their eras that they in turn helped to constitute and maintain.[12]

What was this abandoned "logic of inquiry" that the feminist standpoint theorists took up? In the Marxian accounts, bourgeois understandings of class relations were articulated not only in specific claims about such matters as the inferiority of workers and superiority of educated classes, but also in the abstract conceptual frameworks within which research was organized and conducted on such topics as human evolution, reproductive practices, the distinguishing features and distribution of intelligence, social deviance, the appropriate organization of work and of the economy, and the thought and behavior of "masses" (Sohn-Rethel 1978). Feminist theorists made a similar claim about the way sexist and androcentric understandings of gender relations appeared in the most abstract conceptual practices. They noted, as had Marx himself, that the social relations between women and men in significant respects resembled those between workers and their bosses: gender and class relations had parallel social structures. Men, in their double capacity as the largely exclusive designers and managers of social institutions and as heads of households, intentionally or not exploited women's reproductive and productive labor. They benefited as men, as well as in the capacity of some of them as managers, administrators, and capitalists, from male control of women's bodies, their unpaid domestic labor, and discrimination against and exploitation of their work in wage labor. The exploitation of women in the spheres of domestic and wage-labor were causally linked, for each undermined women's power in the other domain.[13]

Feminist sociologists, political scientists, legal theorists, anthropologists, psychologists, economists, and biologists, often actively participatory in the public politics of the then emerging women's movements, began to provide accounts of how gender relations were articulated not just in explicit claims about women and men and their appropriate social roles, but also in the apparently value-neutral abstract conceptual frameworks of the dominant institutions and the disciplines

that serviced them. They began to look at the "conceptual practices of power," in Dorothy Smith's phrase (Smith 1990a). They did so, the theorists argued, by starting off research from women's concerns and practices in everyday life rather than from the concerns of those institutions and disciplines. For example, Carol Gilligan's analyses (1982) started off thinking about the gap between on the one hand, the distinctive kinds of moral decisions women faced as mothers and care-takers, and on the other hand, moral theory. The latter elevated to the highest ethical categories only the kinds of decisions that tended to arise in the kinds of social relations outside the household where men made moral decisions as managers, administrators, lawyers, and the like – decision-making from which women had long been excluded. Why was it that the most influential authors on morality and moral development (such as Kant, Freud, Piaget, Rawls, and Kohlberg) could not perceive women's moral decisions as exemplifying the highest categories of moral thought? Similarly, MacKinnon identified how what counted as rape and what counted as objectivity had a distressingly close fit with only men's conceptions of such matters – conceptions that reasonably arose from their distinctive kinds of social experiences with women and in courts of law.[14]

Natural and social science disciplines lacked both the will and effective mechanisms to examine critically how their own conceptual frameworks served hierarchical power relations in the larger society. Traditional philosophy of science held that the "context of discovery" should be left free of methodological controls. Yet this stance blocked from critical scrutiny a major route for the entrance of social values and interests into parts of the research process that were thus relatively immune to the kinds of controls of which even the most rigorous of scientific methods were capable. Scientific method could come into play only in the "context of justification," after researchers (and their funders) had selected the social or natural phenomena to be examined, what they identified as problematic about them, the hypotheses and concepts they favored to examine such problems, and had designed a research process. It was only in the research design that the methods of research were specified; obviously these could not exercise any control over the processes that led up to their very designation.[15]

In starting off inquiry from women's lives, feminist research projects appeared to violate the norms of good research in the disciplines in several ways. They failed to respect the autonomy of the "context of discovery" from methodological controls. They were perceived to be importing into research feminist political agendas. Since most of these researchers were women, they were said thereby to fail to respect the importance of impartiality, separation, and distance in the researcher's relation to the researched. Moreover, they proposed something that appeared outrageous to conventional philosophies of science, namely that the purportedly culturally neutral conceptual frameworks of research disciplines, including standards for objectivity and good method, were not in fact culturally neutral. Yet it was hard to deny that substantive feminist research in the social sciences and biology that did start off from women's lives often produced empirically more accurate and theoretically more comprehensive accounts of nature and

social life. Indeed, the achievements of such substantive research tended to be systematically ignored by critics of standpoint epistemology and philosophy of science.

How was this kind of apparently illicit feminist research practice to be understood? The abandoned standpoint epistemology seemed to provide resources that traditional epistemologies lacked for responding thoughtfully to such a question. Several central themes in the standpoint accounts provide a fuller picture of such resources.[16]

First, how societies are structured has epistemological consequences. Knowledge and power are internally linked; they coconstitute and comaintain each other. What people do – what kinds of interactions they have in social relations and relations to the natural world – both enables and limits what they can know.[17] Yet what people typically can "do" depends in part upon their locations in social structures – whether or not they are assigned the work of taking care of children, and of people's bodies and the spaces they inhabit, or of administering large agencies, corporations, or research institutes. Material life both enables and limits what people can come to know about themselves and the worlds around them. So the social structures of societies provide a kind of laboratory within which we can explore how different kinds of assigned or chosen activities enable some insights and block others.

Second, when material life is hierarchically organized, as in societies structured by class, gender, or race, or ethnic, religious, or other forms of oppression and discrimination, the understandings of such hierarchical relations that are available to "rulers" and "ruled" will tend to be diametrically opposed in certain respects, and the understandings available to the dominant group tend to be perverse (Hartsock 1983:287). The slave-owner can see his slaves' actions only as (unwilled) "behavior" caused by slaves' inferior nature or obedience to the master's will: he commands and they obey. Slaves don't appear to be fully human to their masters. However, following around the slaves in their everyday life, one could see their purportedly natural laziness as the only kind of political protest they reasonably think that they can get away with, or their smiling at the master as a subterfuge to obscure the fact that they are secretly planning to run away or perhaps even to kill him. One can see them struggling to make their own human history in conditions not of their choosing. Similarly, the women's movement of the 1970s revealed how women's work was both socially necessary and exploited labor, not just an expression of their natural inclinations or only a "labor of love," as men and public institutions saw it. Feminists revealed many more inversions and perverse understandings of social relations in the conceptual frameworks of dominant institutions. To take another example, women never "asked for" or "deserved" rape or physical violence, contrary to the view of their abusers and the legal system; rather "the state is male" in its insistence on regarding as objective and rational a perception of violence against women that could look reasonable only from the perspective of men's position in hierarchical gender structures (MacKinnon 1982).

Thus, third, the oppressors' false and perverse perceptions are nevertheless made "real" and operative, for all are forced to live in social structures and institutions designed to serve the oppressors' understandings of self and society. These hierarchical structures and institutions engage in conceptual practices, in ideologies, that solidify and disseminate as natural, inevitable, and desirable their continued power. Social and natural sciences play an important role in developing and maintaining such ideologies, involuntarily or not.[18]

Fourth, consequently, it takes both science and politics to see the world "behind," "beneath," or "from outside" the oppressors' institutionalized vision.[19] Thus a standpoint is an achievement, not an ascription. It must be struggled for against the apparent realities made "natural" and "obvious" by dominant institutions, and against the ongoing political disempowerment of oppressed groups. Dominant groups do not want revealed either the falsity or the unjust political consequences of their material and conceptual practices. They usually do not know that their assumptions are false (that slaves are fully human, that men are not the only model of the ideal human), and do not want to confront the claim that unjust political conditions are the consequence of their views. It takes "strong objectivity" methods to locate the practices of power that appear only in the apparently abstract, value-neutral conceptual frameworks favored by dominant social institutions and the disciplines that service them (Harding 1992a, 1998). Importantly, the standpoint claim is that these political struggles that are necessary to reveal such institutional and disciplinary practices are themselves systematically knowledge producing.[20]

Thus such liberatory research "starts off" from the everyday lives of oppressed groups, rather than from the conceptual frameworks of the dominant social institutions and the disciplines that provide them with the resources they need for administration and management of the oppressed. However, such research doesn't stop there, in the lives of oppressed groups, as do conventional hermeneutic and ethnographic approaches. Rather it "studies up" to identify the "conceptual practices of power," in the words of Dorothy Smith (1990a). Standpoint theory is part of post-Marxian critical theories that regard ideology critique as crucial to the growth of knowledge and to liberation. The causes of the conditions of the lives of the oppressed cannot be detected by only observing those lives. Instead, one must critically examine how the Supreme Court, Pentagon, transnational corporations, and welfare, health, and educational systems "think" in order to understand why women, racial minorities, and the poor in the USA have only the limited life choices that are available to them. Because the maintenance and legitimacy of these institutions depend on the services of research disciplines, one must critically examine the conceptual frameworks and philosophies of sociology, economics, and other social (and natural) sciences to understand the thinking of dominant institutions.

Fifth, the achievement of a standpoint brings the possibility of liberation.[21] An oppressed group must become a group "for itself," not just "in itself," in order for it to see the importance of engaging in political and scientific struggles to see

the world from the perspective of its own lives. Women have always been an identifiable category for social thought – an object conceptualized from outside the group. But it took women's movements for women to recognize their shared interests and transform themselves into groups "for women" – defining themselves, their lives, their needs and desires for themselves. They learned together to recognize that it was not just "their man" (father, husband, boss) who was mean or misbehaving; rather cultural meanings and institutional practices encouraged and legitimated men's treatment of women in such ways at home, in workplaces, and in public life. Women's movements created a group consciousness in those that participated in them (and many who only watched) that enabled feminist struggles and then further feminist perceptions. Similarly, it took civil rights struggles and black nationalist movements of the 1960s to mobilize African Americans into collective political actions that could, it was hoped, end racial inequities. The Chicano/a movement developed to mobilize Mexican Americans to a group consciousness capable of advancing an end to the injustices visited upon them. The Lesbian and Gay Pride movement had a similar goal and effect. New group consciousnesses were created through these processes, consciousnesses that could produce new understandings of social relations, past and present.

It should be noted that while standpoint theorists were originally concerned to distance their projects from positivist ones, they have recently had to articulate their distance also from relativist excesses of postmodernisms or poststructuralisms and of some ethnographic and cultural studies approaches to research. Early articulations of standpoint approaches, from Marx through feminist writings of the 1970s and early 1980s, retained without critical examination problematic Enlightenment concepts and goals that have for the most part now been jettisoned or revised by the original feminist standpoint theorists and by later thinkers. Thus, some standpoint theorists explicitly have brought poststructuralist insights to bear on scientific epistemologies.[22] Others *de facto* do so. After all, Marxism provides a powerful theory of how knowledge is socially situated; this insight was not invented only recently. Rather than completely reject the Enlightenment legacy, one can find in these writings, as well as in the work of others working in postcolonial and science studies especially, new and often stronger standards for and recodings of objectivity, rationality, good method, "real science" and other such central notions for philosophies of the natural and social sciences.[23]

It is not difficult to see how these themes embed methodological prescriptions that violate the conventional norms of good research and its "objectivism." Such prescriptions thus bring into even sharper focus important breaks between standard epistemologies and philosophies of science, including self-proclaimed postpositivist ones, and those of standpoint theory. Standpoint theory provides both epistemological and philosophy of natural and social science insights and methodological directives.[24]

Moreover, three sources of criticism of standpoint approaches as epistemologies and philosophies of science are more easily countered if one looks at the recommended research practices of standpoint approaches. As indicated in the

opening section, these are the charges that standpoint theories either embrace or inevitably commit essentialism, relativism, and the assumption of automatic epistemic privilege for the understandings of oppressed groups. While such charges have repeatedly been countered by standpoint theorists, their persistence is likely since the successful avoidance of such positions, without the crutch of positivist assumptions, deeply threatens prevailing epistemic and philosophic assumptions, even among those who otherwise distance themselves from positivist excesses.[25] Perhaps approaching such anxieties from the perspective of standpoint-recommended research practices can give courage to a least a few skeptics of standpoint possibilities.

A Philosophy of Method

Let us enter this topic by way of criticisms of the colonial structure of the conventional model of good social research (Blauner and Wellman 1973, L. T. Smith 1999, Wolf 1996). Feminists are certainly not the only critics to argue that the conventionally recommended relation between the researcher and the researched is intrinsically politically unequal, even a "colonial" relation. Conventionally it is researchers, influenced by the assumptions of their discipline and their culture – not to mention of their potential funders – who decide on what social conditions, peoples, events, or processes the research project will focus, how it will be organized, conducted, interpreted, and, to a large extent, disseminated. They decide which social situations to study; what is problematic about them; which hypothesis to pursue; upon which concepts and background literatures to rely; what constitutes an appropriate research design including the choice of methods; how to interpret, sort, analyze, and write up data into evidence; and how and to whom the results of research will be disseminated. Conventionally, the researched are allotted little say in this process. Through the self-discipline of rigorous attention to disciplinary methodological rules, researchers are to secure their own disinterest, impartiality, and distance from the concerns of those they study – control of the research process is to belong entirely to them, the researchers. If this were not the case, according to conventional thinking the project would not be sufficiently objective and scientific.

Yet emancipatory movements have two kinds of reasons to criticize this level of researchers' control of research. One is political. The researched have usually belonged to social groups already less powerful than the researchers and their sponsors. It is the behaviors of the less powerful – workers, union activists, foot soldiers, prisoners, students, potential consumers, women, welfare users, already economically and politically disadvantaged races and classes, plus actual or soon-to-be colonized groups – that the institutions funding social research have wanted to discover how better to manage. Yet the researched are disempowered – further disempowered in the case of already disadvantaged groups – by such research

processes. Such disempowerment also illuminates reasons for the resistance domin-
ant groups have to becoming the object of study of social scientists. "Studying
up" also is politically offensive to those studied.

However, the other reason is scientific: the disempowerment of the researched
in the research process (as well as outside it) tends to nourish distorted accounts
of their beliefs and behaviors. Left to their own devices, researchers, like the rest
of us, will tend to impose on what they observe and how they interpret it the
conceptual frameworks valued in their cultures and disciplines, which all too often
are those valued by the already powerful groups in the larger society. Moreover,
as is well-known, such a colonial situation simultaneously nourishes distorted
accounts of the researchers and the social groups to which they belong and that
their work services. The dominant groups' perverse understandings of themselves,
too, are reinforced by research that further disempowers the groups likely to be
most critical of their dominance.

This is not to imply that researchers do not seek to block such conceptual
imposition; they often do so for both reasons. The history of ethnography, soci-
ology, and other disciplines shows constant attempts to control the cultural
impulses of inquirers – attempts that have been successful in many respects. Nor
is it to imply that all such imposed conceptual frameworks are unreliable; many
are valuable since "the stranger" often can detect patterns and causes of behavior
that are difficult for "the natives" to see. Rather the issue is that even the most
well-intentioned researchers lack some of the resources that the researched pos-
sess – resources that can be used to critically evaluate researchers' own taken-for-
granted conceptual frameworks. This is the case regardless of the relative social
status of researcher and researched. However, the chances that the researchers'
conceptual frameworks are unreliable increase the greater the difference in social
power between the observers and the observed. So the disempowerment of
already politically disadvantaged research subjects not only tends to further
disempower them; it also tends to produce "bad science" (L. T. Smith 1999, Wolf
1996).

How can the kind of disempowering and distorting power of the researcher,
apparently inherent in the research process, be blocked to prevent such colonial-
ization of research? How can this be accomplished without losing the valuable
"powers of the stranger"? First of all, the futility of several widely practiced
strategies requires recognition. For example, it should be recognized that the
social statuses that the researcher and researched bring to research processes are
for the most part permanent. No amount of empathy, careful listening, or "going
native," valuable as such strategies may be for various reasons, will erase the fact
that the Western, white, usually male, university-educated, or international-agency-
funded researchers are going to leave the research process with (for the most
part) no less than the economic, political, and cultural resources with which they
arrived. And the researched will leave with largely whatever such resources they
brought to the research process. Of course research processes frequently do
enlarge the vision, invite self-reflection, and in other ways contribute to ongoing

personal growth in both researcher and researched. Perhaps each becomes inspired to experience more of the resources available in the other's lifeworld. Yet the fundamental economic, political, and social structural inequalities that positioned the researcher and the researched in their social relation initially will not be changed by the research process alone (Blauner and Wellman 1973).

Another inevitably unsuccessful strategy for equalizing the power between researcher and researched – a strategy that young (and, alas, not so young) researchers attempt – is for researchers to try to disempower themselves personally by "confessing" to the reader their particular social location, for example: "I, the author, am a woman of European descent, a middle-class academic, trained as a philosopher, who has lived all her life in the USA." Some such information can be useful to the reader, but for researchers to stop their analysis of their social location here, with just the confession, is to leave all the work up to the reader. It is the reader who must figure out just how such a location has shaped the disciplinary and other conceptual frameworks used, the questions asked, how they are pursued, and so forth. Moreover, such a strategy makes the familiar liberal assumption that individuals are capable of voluntarily identifying all of the relevant cultural assumptions that shape their research practices; Marx, Freud, and historians have taught us how self-deluding that assumption is.

Yet another futile strategy researchers attempt, or at least think their courses on research methods have directed them to pursue, is to try to forgo any theoretical or conceptual input into the research process itself. Researchers sometimes think the most useful procedure they can undertake is simply to "record the voices" of their subjects. Critics (and even misguided defenders) of standpoint approaches have often thought that this was the standpoint project. To be sure, there are good reasons to want to record the voices of all kinds of subjects. Moreover it is valuable to recommend that researchers try to set aside their own assumptions when approaching a research situation, whether or not it is familiar. Yet to restrict research in such ways would be to reduce the researcher to a kind of (inevitably inaccurate) transcription machine. This strategy has the effect of discarding some of the most valuable political and scientific resources of the researcher. These include precisely the often higher social status carried by the researcher that can, instead of enacting a colonial destiny, be deployed on behalf of the researched. It includes the expertise and resources to conceptualize and articulate social relations in, paradoxically, the kinds of disciplinary and institutional languages that can be heard by public policy makers and the disciplines and institutions upon which they depend.

So what contributions can standpoint approaches make to blocking the inherently colonial relations of social research? And how do they raise new philosophic and scientific questions that conventional philosophies of science ignored or disallowed? The research process can be divided into four sites where such "colonial" relations between the observer and the observed can flourish. The first is the selection of the research problematic and the design of the research project: the "context of discovery." The second is the conduct of the research – the field

or archive work. The third is the writing up of the research findings – the interpretation and theorization of the data. The last is the dissemination procedures, intended or not by the researcher. Standpoint methodology can be valuable in the last three parts of the process, yet it has unique value in the first stage.

Standpoint approaches innovatively recommend that the "context of discovery" be brought under methodological controls. The dominant group's values and interests perhaps powerfully shape research projects at this stage of inquiry in ways identified earlier. Yet it is only the "context of justification" that is regarded as legitimately controllable by method. The so-called "logic of scientific inquiry" does indeed begin with bold conjectures, yet the sciences have designed their methods to focus only on the process of seeking the severe refutations of primarily those hypotheses that manage to get thought up by people who can get research funded. Indeed, in fields where research is expensive, it is only such hypotheses that reach the starting line to face the trials of attempted refutation. Thus a new question arises: which are the politically and scientifically valuable and which the not-to-be-valued ways to bring the context of discovery under methodological controls? Pursuing such a question deploys a stronger kind of reflexivity: a robust attempt critically to evaluate the selection of research problems and their conceptual frameworks and methods.[26]

Second, contrary to conventional methodological prescriptions, standpoint methods are engaged. They are not dispassionate, disinterested, distanced, value-free. It takes politics as well as science to see beneath, behind, or through the institutional rules and practices that have been designed to serve primarily the already most economically and politically advantaged groups. Standpoint methods recognize that some kinds of passions, interests, values, and politics advance the growth of knowledge and that other kinds block or limit it. Politics can be productive of the growth of knowledge as well as an obstacle to it, as it often is. Here, too, a form of the first new question arises: which such political engagements promote, and which limit, the growth of knowledge? The hypothesis that standpoint analyses make plausible is that vigorous commitments to democratic inclusiveness, fairness, and accountability to the "worst off" can also advance the growth of knowledge. Such commitments do not automatically do so, but neither should they automatically be excluded from playing a possibly productive role in research processes.

Thus such considerations require re-evaluation of the conventional requirement of value-neutrality in order to maximize objectivity. The standpoint argument is that such a requirement blocks the deployment of politics that increase the inclusiveness, fairness, and accountability of research. If research is to be accountable only to disciplinary conceptual frameworks and methodological requirements that in fact often service ruling institutions but not the "ruled," more research will succeed in further entrenching such ruling conceptual frameworks and increasing the gap between the "haves" and the "have-nots." The solution here is not to abandon the project of maximizing objectivity, but rather to cease

to require the maximization of complete social neutrality in order to achieve it (Harding 1992a, 1998:ch. 8).

Finally, economic, social, psychological, and cultural heterogeneity is to be exploited rather than suppressed in standpoint methods. The dominant assumptions are abstractly encoded in the conceptual frameworks of a society's institutions and research disciplines. Yet these represent only one distinctive cultural point of view, and an especially and suspiciously unreliable one at that – the point of view of the ruling group. Bringing into focus accounts of nature and social relations as these emerge from the lives of many different subjugated groups creates a broader horizon of understanding of how nature and social relations work. It is not that these subjugated understandings are automatically the best ones on sound empirical and theoretical grounds, but rather that they can lead to the identification of additional problematic or just interesting natural and social phenomena, suggest different hypotheses and conceptual frameworks for investigating them, suggest different lines of evidence and challenges to favored evidence practices, uncover unnoticed cultural tendencies in the writing up of data, and make strong arguments for dissemination practices that differ from those favored by contemporary research property rights systems.

The ideal conditions for exploiting heterogeneity require genuinely democratic societies in which inequality has already disappeared and no prodemocratic group is, or can legitimately be, silenced through formal or informal means. All would be equally articulate in the selection of problems to research, the specification of what is problematic about them, the selection of a conceptual framework and methods of research and so on. Of course we do not have such a situation. My point – and the argument of standpoint theorists – is that standpoint epistemologies, philosophies of science, and methodologies can help move toward such a goal. There is no easy formula for insuring that subjugated groups will become empowered in research processes. Yet there is plenty of reflection on this topic now available (Collins 1986, McTaggart 1997, Maguire 1987, Petras and Porpora 1993, D. Smith 1987, 1990a, 1990b, 1999, L. T. Smith 1999, Wolf 1996).

I have hoped that an examination of the methodological prescriptions of standpoint theory can help to dispel widely articulated anxieties about some of the ways this philosophy of science and epistemology differs from its conventional predecessors. However, it is valuable also to focus on two facts that discourage such a hope. On the one hand, critics of standpoint approaches persist in charges of essentialism, relativism, and conferring automatic epistemological privilege on the assumptions and perceptions of the oppressed in the face of decades of argument against such positions by standpoint theorists themselves as well as by others. On the other hand, standpoint approaches continue to develop and proliferate in ever more research contexts here and around the world in the face of the persistence of such criticisms. These facts suggest that more is at stake in standpoint approaches than the direct discussions of these charges confront. Often it takes changes in social relations themselves for new ways of thinking about knowledge and its production to become widely plausible. We may just

have to live through the ongoing contentiousness of standpoint theory as some sort of such changes are under way around us. Articulation of just what sort of changes these are could make a future project.[27]

Notes

1 The earliest of such accounts were Dorothy Smith's, from the late 1970s and early 1980s, subsequently collected in her 1987 and 1990a works. (See also D. Smith 1990b and 1999.) Shortly thereafter appeared Hartsock (1983), Rose (1983), Jaggar (1983: ch. 11), Collins (1986, 1991) and Harding (1986).

2 Some of these projects are shared with feminist science studies more generally. See Rouse's (1996) account of the way feminist science studies, in contrast to contemporary (postpositivist) sociologies of science and traditional philosophies of science, take what Rouse calls a "postepistemological" stance toward the production of scientific knowledge.

3 All three of these attributed positions are excessive versions, or perhaps the only imaginable alternatives in the eyes of critics, of standpoint rejections of conventional positions. Thus critics assume that someone who claims that women are a political group, rather than only the individuals of liberal political philosophy, must have an essentialist understanding of women. They suppose that if a standpoint theorist thinks that women's claims about their own or others' lives have any authority at all, standpoint theory must be giving women automatic epistemic privilege. If someone claims that women or feminists can have a distinctively different epistemic position than that held by dominant institutions, standpoint theory must be committing epistemic relativism, they say. See Hartsock (1998) and Wylie (forthcoming) for two reviews of responses to such charges.

4 See also responses by Patricia Hill Collins, Sandra Harding, Nancy Hartsock, and Dorothy Smith, and Hekman's reply, in the same issue of *Signs*, 367–402.

5 See also responses by Joey Spague and Sandra Harding, and Walby's reply, in the same issue of *Signs*, 511–40.

6 Especially, but not exclusively, social science research. (See Harding 1986, 1991, 1998, Sismondo 1995, Weasel 2000 for discussions of its usefulness in the natural sciences.) As the post-Kuhnian science studies have again and again revealed, the objects of natural science scrutiny are always already objects-of-knowledge, identified and characterized by prior scientific discourses and contemporary social concerns. Hence starting off projects about nature's order from the lives of subjugated groups can reveal yet additional patterns and causal relations to those detected by pursuit of the concerns of dominant institutions. Postcolonial, multicultural, and feminist science studies have all made this point in their own ways. (See Harding 1998, Hess 1995 for overviews of postcolonial natural science issues.)

7 Hartsock (1983), Jaggar (1983:ch. 11), and Pels (1997) also review the Marxian history of this approach. The feminist theorists have generally avoided Lukacs's Hegelian machinery.

What caused standpoint theory to languish between Lukacs's writings and the second wave of the US/European women's movement? Of course Lukacs's work was

controversial within Marxism from the beginning. However, US Marxian social scientists in the 1960s, 1970s, and later tended to prefer the Popperian form of a positivist philosophy of science to justify their research strategies (Popper [1963] 1992). Given the prevailing political climate of McCarthyism and the Cold War, it is understandable that researchers and philosophers distanced their work from an epistemology/ methodology that wore both its political engagement and its specifically Marxian origins on its sleeve, so to speak.

8   Representations of similar researcher positions appear in Anzaldua's (1981) "borderlands" consciousness, hooks' (1983) "theory from margin to center," and D. Smith's (1987) "bifurcated consciousness" of the woman sociology graduate student. W. E. B. DuBois's "double consciousness" of African Americans is one precursor of this kind of representation.

9   As Alison Wylie (forthcoming) notes.

10 Hartsock (1998), Hirschmann (1997), and Pels (1997) review some of these analagous accounts.

11 In philosophy, W. V. O. Quine's work on how scientific "networks of belief" seamlessly link everyday and scientific thought, and other aspects of his criticisms of logical positivism, directly influenced at least some standpoint theorists (Quine 1953, Harding 1976).

12 For perspectives on different aspects of this literature see Harding (1998), Hess (1995), and Selin (1997). Figueroa and Harding (forthcoming) is part of the dissemination project of a National Science Foundation grant to the American Philosophical Association.

13 Such an analysis did not usually illuminate the lives of women domestic workers, as feminist scholars of color and standpoint theorists themselves subsequently noted (Collins 1991).

14 Note that standpoint approaches start off from the lives of the oppressed, but that they refuse to end there. Standpoint approaches are not ethnographies. Rather they are a form of critical theory that "studies up." This point is explored further below.

15 I have discussed this problem in a number of papers on "strong objectivity." See, for example, Harding (1992a, 1998:ch. 8).

16 As indicated earlier, such theories were developed within a number of different disciplines with diverse histories and preoccupations, and by theorists with commitments of varying strength to Marxian and to Enlightenment projects. Consequently, it is risky to try to summarize this approach in any way that attributes to it a unified set of claims. Nevertheless, theorists from these different disciplines do share important assumptions and projects that differ from conventional understandings of what makes good science, including, I propose, the following. (I articulate them in a form which stays close to Hartsock's 1983 account.) Of course not every theorist equally prioritizes or emphasizes each of these, since what is perceived to be important in the context of sociology may be less important to political philosophers or philosophers of science and vice versa. Nor are disciplinary concerns, themselves heterogeneous, the only ones that lead to divergence in how standpoint approaches have been developed.

17 Note that this theme echoes standard beliefs about the effectiveness of scientific methods: which interactions with, or kinds of observations of, natural and social worlds are pursued both enables and limits what one can know.

18 I use the term "ideology" here to mean systems of false interested beliefs, not just of any interested beliefs.

19 Of course one's understanding can never completely escape its historical moment – that was the positivist dream that standpoint approaches deny. All understanding is socially located or situated. The success of standpoint research requires only a degree of freedom from the dominant understanding, not complete freedom from it. It is the structural position of the oppressed that provides the possibility of often small but nevertheless important degrees of freedom from prevailing discourses, including institutions, their practices and cultures.

20 A motto of the early days of the women's movements of the 1970s said "The degree of his resistance is the measure of your oppression." If this point is lost, and even some standpoint defenders sometimes lose it, "standpoint" seems like just another term for a perspective or viewpoint. Yet the standpoint claim about the epistemic value of some kinds of political struggle – the epistemic value of the engagement of the researcher – is thereby made obscure when its technical use, which I retain here, is abandoned. This point is related to the disagreement over whether the theory is best articulated as about a feminist or a women's standpoint. Hartsock has opted for the former, and D. Smith for the latter, for reasons which I suspect have to do with concerns about their respective disciplines – a topic for another place.

21 I shall refer to standpoint approaches as inherently progressive since that is the way they have been understood today through the Marxian legacy inherited by leading movements for social justice. Yet it is useful to recall that Nazi ideology also (ambivalently) opposed modern science on standpoint grounds and, indeed, conceptualized its murderous program as one of advancing social justice (see Pels 1997). Religious fundamentalist movements, geographically based ethnic movements, and patriot or neo-Nazi social movements usually are not reasonably characterized as dominant groups. Nevertheless, they too are threatened by modernity's political values and interests. They often make something close to politically regressive standpoint arguments. So theories about which kinds of social movements are liberatory, and for whom, must be articulated to justify research projects in the natural and social sciences. See Castells (1997) for interesting discussion of the different political potentialities of various identity-based social movements around the world today, and Castells (2000) for an overview of the project within which this discussion is set. See also Harding (forthcoming a) for a discussion of epistemological issues in such a context.

   Of course there is nothing new about natural and social science research *assuming* such political theories; conventional philosophies of natural and social science always assumed – consciously or not – liberal political philosophies and their understandings of relations between knowledge, politics, and social emancipation. Sciences and their philosophies are always at least partially integrated into their larger economic, political, and social formation, to put Kuhn's point another way. Thus it is not standpoint theory that introduces the conjunction of social theory and epistemology, let alone their "integrity" with actual historical features of a society.

22 This is true of Haraway (1991) and Harding (1991, 1998). Garcia-Selgas (forthcoming) discusses standpoint theory's resources for resolving problems that postmodernist insights create for critical theory. See also Hirschmann (1997).

23 For a sampling of such epistemological recodings, see Galison and Stump (1996), Haraway (1991), Harding (1991, 1998, forthcoming a), Hess (1995), Jaggar (1989),

Latour (1988), Rouse (1987), Schuster and Yeo (1986), Selin (1997), Shapin (1994), Shapin and Shaffer (1985), D. Smith 1987, 1990a, 1990b, 1999.

24 As Joe Rouse (1996) pointed out, this is one of the places where feminist science studies parts ways with post-Kuhnian sociology of science. The sociologists, like conventional philosophers of science, left up to scientists the final authority about what should count as "good method" and good results of research. Standpoint and other feminist (and other social movement) approaches do not.

25 For example, see Harding's (1992b), Hartsock's (1998), and Wylie's (forthcoming) review of such responses by standpoint theorists.

26 Of course research proposals face peer review. But the problem standpoint approaches address is the common situation in which the entire "peer group" shares the widespread ethnocentric assumptions that the proposed researchers also make: assumptions that are androcentric, Eurocentric, white supremacist, class based, heterosexist, and so forth.

27 I thank Stephen Turner and Alison Wylie for helpful comments on an earlier draft of this chapter.

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Beyond Understanding: The Career of the Concept of Understanding in the Human Sciences

Paul A. Roth

Only [that which] spirit has created does it understand.

Vico ([1725] 1984)

A fundamental intuition underpins efforts to distinguish the human and the natural sciences: humans create and sustain the social but not the natural order. Comprehending what structures the social thus involves factors – human values and purposes – that do not belong to the natural order. Understanding situates the social (in all its forms) within a matrix of human concerns and purposes assumed to sustain it.

Understanders understand by apprehending what others do or value and why they do or value what they do. Explanation situates whatever wants explaining within the general causal structure of the world; explainers explain by identifying the general causal processes at work in particular cases. Explainers pose the study of humans *qua* social beings as continuous with the study of humans *qua* natural objects. Understanders conceive of the human sciences as *sui generis*, a realm of study of nonnatural objects constituted by values and interests. An ability to parse experience in terms of categories we create presumably divides us from the remainder of the natural world.

Yet despite many decades of debate, it remains unclear whether demands for "understanding" pose a genuinely contrasting or even an ultimately coherent alternative to whatever "explanation" requires. The issue need not concern the supposed "reduction" of one order of things to another. The more fundamental question is whether or not anything essentially differentiates the processes needed to account for human behaviors from those needed for other processes in nature.

Answering this question requires exploring how notions of understanding and an interrelated family of terms – "interpretation," "meaning," and "translation" – figure into philosophical debates. Does an examination of these terms and their uses suggest an epistemological license for a principled distinction between understanding and explanation?[1]

For the notions of understanding, meaning, and so forth to play the unique roles for which they typically are cast requires showing how they might work to systematically set the social apart from the general causal order. Making social factors part of a world humans share marks them as real; their role in structuring behaviors gives them claim to systematicity, and so objects of a science. Sustaining a principled explanation/understanding divide requires, in short, some story of how, for example, interests and values create orderings not ascertainable by methods for studying how the natural order orders.

Yet talk of nonnatural factors appears as wanton reification, a mere *façon de parler*, unless these claims to sharing and systematicity prove *necessary* to our ways of organizing and comprehending the social world. The assumption that understanding and its conceptual kin are nonnatural implies there can be a "fact of the matter" to meaning – a realism with regard to meaning. Such "objects of understanding" require a special science; that is, in order to make systematic sense of the observed we are required to add these nonnatural elements to our ontological inventory. Yet cutting the world up into two ontologically incommensurable chunks – nonnatural and natural, meaningful and nonmeaningful – calls for compelling justification.[2]

The first section surveys some of the underlying issues historically implicated in distinguishing between explanation and understanding. It builds the case for a distinct science of understanding by examining why nonnatural meaning escapes all accounting from within the natural realm and yet can be scientifically studied. The next section turns to an examination of an important debate invoking a historicist form of "meaning realism," and asks what empirical significance attaches to "real meaning." Does it abet the study of peoples who are not "culturally near"? The final section focuses on a dispute in Holocaust historiography between Christopher Browning and Daniel Jonah Goldhagen which may, at first blush, appear to pose a different challenge for understanding than that arising from the examination of exotic others. However, I maintain that, for such cases too, appeals to a special science of understanding add nothing. I conclude by suggesting reasons why a distinction between explanation and understanding is not one we need draw.

Real Understanding

My concern in this section and the next will be with a basic historicist rationale for an object of understanding – for a shared something for understanding to be

about, a something that eludes explanation. Friends of understanding need to say how the significance of the social escapes being accounted within the order of nature. Otherwise, there will be no principle by which to distinguish between explanation and understanding.

Sentient beings, the thought goes, are essentially unlike atoms in the void by virtue of having a perspective on the world. Possession of a perspective impacts behavior by allowing humans to formulate their own order of things. Such structures of understanding "overlie" the natural order and are distinct from it. Dilthey's dictum, "Nature we explain; psychic life we understand [*verstehen*]"[3] expresses an insisted-upon contrast between the invariant order of nature and the contingencies of human comprehension of the historical moment.

But to matter, there must be something shared, something that understanding is jointly an understanding of. For without a *shared* something, understanding offers no route to an account of the social. Yet the relation of people to their shared perspective must not be just that of actors to a shared script. For that type of sharing obviates any special place for understanding by depicting people as just "judgmental dopes" (in Harold Garfinkel's memorable phrase). A social script then goes proxy for laws of nature. Any need for a deep divide between explanation and understanding disappears.

In order to support a principled distinction between explanation and understanding then, whatever is shared in understanding must be "doubly contingent."[4] The first contingency is of time and place. Were circumstances different, the shared stuff would be other than it is. The task of understanding here is to recover the shared something of the cultural matrix.

The second contingency concerns variability in how "insiders" interpret social rules, and so forth. The task of understanding here is to provide an account that makes people into something more than social automatons. That is, to support a special role for understanding, the shared stuff must not only be historically contingent, but it also must allow of application not rigidly determined by circumstance. This second form of contingency requires variability *within* (and not just between) social orders. Unlike mindless nature, individuals *judge* what matters, and how it matters. This variability sets the social worlds that sentient beings create and inhabit somehow apart from the invariant laws patterning nature. Understanding consists, then, of a mental framework stable enough to be shared but dynamic enough to allow for individual improvisation.

The natural sciences cannot incorporate the world of social experience, the argument runs, because value-orientation defines that world. It is one dominated by actions influenced by and "directed toward" objects that are not things in the world – for example, religious beliefs, personal relationships, loyalties to groups and institutions. Weber speaks for the tradition here by identifying value-orientation as what "seals off" accounts of human action from explanatory approaches used by the natural sciences.

> The concept of culture is a *value-concept*. Empirical reality becomes "culture" to us because and insofar as we relate it to value ideas. . . . We cannot discover, however, what is meaningful to us by means of a "presuppositionless" investigation of empirical data. Rather perception of its meaningfulness to us is the presupposition of its becoming an *object* of investigation. (Weber [1904] 1949:76–7)

Weber decisively influences subsequent debate in at least two respects. The first is the Humean point that an inventory of the furniture of the universe does not contain value statements. The second is that "'culture' is that segment of nature on which *human beings* confer meaning and significance." "Cultural reality" is "knowledge from particular points of view" (Weber [1904] 1949:81).[5] Social reality only "shows up" from within a historically received and contingently constituted perspective.

Cultural meaning exemplifies how the mind structures experience and concomitantly frames the task of understanding. Gadamer nicely situates the issues in the following way. Just as the lawlike structure found in the natural sciences constitutes the explanatory frame imposed on experience of the physical world, so too do the values and suppositions unique to each society and age form the framework within which to comprehend the experiences of beings like us (Gadamer 1979:116). The sciences of nature explain (causally account for) the particular events by fitting them in with the general way the world works. In contrast, the human sciences want to understand how historically specific cultural things fit into historically specific lives. Where answers to the former require patterns of universal necessity, the latter call for patterns of temporal contingency.

But this privileging of understanding as an organon by which to comprehend the inner life of humans introduces in its wake problems of truth and objectivity peculiar to that inner realm. If there is to be a science to be had, there must be accepted evidence against which claims can be checked, and a reliable method of testing as well. For there to be a plausible parallel to the natural world, there must be an object of understanding as truth-maker and a method of understanding as its test.

Dilthey exercises a fateful influence here. Understanding, Dilthey held, cannot be mind-reading, for we have no direct access to other minds. Better to model understanding on analogy with a text that is mutually comprehensible – readable by all. The processes of translating, interpreting, and finally understanding a text – how issues of meaning are settled within a community – becomes the paradigm for the study of understanding.

If, as Gadamer remarks, "Understanding is a participation in the common aim" (1979:147), what could be better evidence that one achieves that participation than "knowing how to go on" in "interpreting" a social text? By assimilating the notion of a science of understanding to the text metaphor, and so analogizing the processes of reading and meaning, one obtains the desired parallel with explanation. For this identifies understanding with known methods and discernible outcomes. Taking cultural artifacts as reifications of meaning, success in dealing with

such texts constitutes evidence for attributing truth to translations and interpretations, just as ongoing success in experimental encounters with nature seemingly licenses claims to representational truth.[6]

Three assumptions emerge here. First, cultural artifacts are evidence of meaning, of inner life reified. Second, meaning so represented may be translated, literally taken from the idiom of their creators and put into an idiom accessible to us. Third, translation – mapping of one idiom into another – may be appropriately interpreted, that is, put into a context that determines its meaning for us and others.[7]

Together, these assumptions – artifacts are evidence of meaning, meaning translates, and translation allows of rational disambiguation via further contextualization (interpretation) – mutually support the intuition that the senses provide evidence for something apart from the order of nature – meaning. The assumptions define as well what makes for objectivity in investigations of the social experience of others – successful translation or interpretation. The reading–meaning link implies the systematicity, intersubjectivity, and yet also the individuality (of interpretation) that understanding requires. Nonnatural states – how things stand in the minds of those studied – account for objects in the world – texts and other cultural artifacts. As in the natural sciences, the science of understanding infers from the seen to the unseen.

The concepts of understanding and meaning are thus linked insofar as a shared meaning is what humans add to experience seen "from within" a particular cultural perspective. Understanding constitutes a participation in or sharing of that perspective.[8]

The Experience Distant – Understanding Hawaiian-style

Does, in fact, a historicist perspective suffice to legitimate a robust notion of understanding? Do the assumptions identified in the previous section work as advertised, as a rationale for a nonnatural realm of meaning and a special science of understanding? Historicism as here imagined seeks to reconstruct the shared mental stuff answering to the "what is it like to be" for historically specific groups. To do the work intended, nonnatural meaning must be *necessary* to "participating" in the views of others. If nonnatural meaning does not constitute what one must apprehend in order to participate socially, then it has lost its *raison d'être*, at least for purposes of underwriting a special science of understanding. Without the assumption of a shared something linking those studied, there is no special mentality to reconstruct, nothing for historicism to be about.

In order to bring out lurking difficulties in the "shared stuff" assumption, I turn to a recent exemplification of an ongoing controversy centering on issues concerning how to "discover" by which standards people think, and so how to interpret their actions. This is the dispute between anthropologists Gananath

Obeyesekere and Marshall Sahlins.[9] Their interpretive disagreement concerns the eighteenth-century Hawaiians' response to Captain James Cook's landing in Hawaii and his subsequent death at the hands of the Hawaiians.

Indeed, what makes the death of Cook appear to be of singular significance is just that it manifests a point of access to the "inner workings" of the indigenous conceptual scheme. Ironically, both Obeyesekere and Sahlins claim to speak from "inside" a native perspective. Yet Obeyesekere insists on attributing to the "natives" a distinctly universal form of game-theoretic generic wisdom. Sahlins defends a strongly enculturated notion, one unique to that time and place.[10]

The facts not in dispute are that Captain James Cook landed on a beach on the island of Hawaii during the Makahiki festival sometime late in 1778 or early 1779. After a brief stay, he departed. Damage to one of his ships forced his unanticipated and unplanned return to the island shortly thereafter. His return occasioned serious dissension between the local chiefs and Cook. Cook soon became involved in a confrontation with the Hawaiians in the course of which they stabbed and clubbed him to death, carried away his body, and (apparently) dismembered it.

Why did this happen? Sahlins maintains that the manner and circumstance of Cook's arrival during the Makahiki festival established for Hawaiians that Cook was the god Lono.[11] What did not sit well, Sahlins suggests, was Cook's return. Having unwittingly established himself as Lono, his return to the islands did not fit into the cultural category into which he had been placed. Upon his return, Sahlins remarks, "Cook was now *hors catégorie*" (1981:22). The "explanation" of Cook's death, on this account, locates it as a consequence of Cook's "violation" of the part for which he was scripted. For the Makahiki is about, *inter alia*, challenge and renewal of basic political forms of Hawaiian cultural life. "The killing of Captain Cook was not premeditated by the Hawaiians. But neither was it an accident, structurally speaking. It was the Makahiki in an historical form" (Sahlins 1981:24). Having been granted the status of a god, Cook suffered the ritual fate.[12]

For Sahlins, the circumstances surrounding Cook's death constitute a case of "cultural improvisation" (1981:67). Sahlins terms such improvisations a "structure of the conjuncture," an effort to assimilate a dissonant experience (the unexpected return to Hawaii by Cook) given the available conceptual resources. Cook's initial conformity with, and then transgression of, Hawaiian categories provide, on Sahlins' view, a natural experiment in how categories re-form when experience diverges from what people anticipate.[13] Sahlins poses the question as one asked within a determinate conceptual framework, albeit a framework peculiar to people, time, and place. In this regard, the conception of rationality is "local."

Obeyesekere maintains at least two theses in opposing Sahlins. The first – Obeyesekere's negative or critical thesis – charges that Sahlins has written his own preconceptions and prejudices into the psyche of the Hawaiians. In particular,

here is yet another case of a Western anthropologist assuming that dark-skinned people are too witless to see the British as mere mortals like themselves. Sahlins postures the Hawaiians as so in the grips of their cultural lore as to be unable to distinguish between a light-skinned foreigner and a mythical god.

Logically independent of Obeyesekere's critique of Sahlins is a second thesis. This develops an interpretive account of the Hawaiians as endowed with "practical rationality." "In the West rational systematization of thought was articulated to a 'pragmatic rationality' where goals are achieved through technically efficient means, culminating in modern capitalism. . . . I take the position that 'practical rationality,' if not the systematization of conceptual thought, must exist in most, if not all, societies, admittedly in varying degrees of importance" (Obeyesekere [1992] 1997:263 n.48). But Obeyesekere expands the notion beyond utilitarian consideration so as to include "reflective decision making by a calculation or weight of the issues involved in any problematic situation" ([1992] 1997:20).[14] "Reflective" decisions "see past" culturally freighted coincidences (Cook's arriving during the Makahiki) to the actual state of affairs (Cook as a British "chief") ([1992] 1997:91).[15]

More importantly, this expansion of the concept implies that cultural actors can decide whether or not to apply their "normal" modes of understanding to particular situations. So, for example, Hawaiians presumably could choose to understand Cook's arrival on the shores of Hawaii either as the arrival of Lono, or as a coincidental happening. Indeed, their choice would be made in light of how it best advantaged them to accommodate the events in question.[16] Against the Sahlinsian "tyranny of culture," Obeyesekere invokes universal bases of rationality rooted in "the physical and neurological bases of cognition and perception" ([1992] 1997:60, see also 20–2).

Yet neither Obeyesekere nor Sahlins ever pause to disentangle the issues of defending their particular interpretation of events from the general methodological question of what marks "genuine" understanding. The dispute would have particular point if one grants the assumption each makes of a necessarily shared cultural truth by the Hawaiians. But no argument animates this assumption; it is idle except for purposes of fueling polemics. That is, although both Obeyesekere and Sahlins accuse one another of reading Western ways into Hawaiian mores, neither appears to doubt that a nonnatural meaning "stands behind" the texts they interpret. Each claims to specify what the Hawaiians share in the process of providing an understanding of the rationale for killing Cook. But neither ever begins to make the case that some such sharing constitutes a necessary condition for Hawaiian (or any other) culture.[17]

The debate proves "philosophical" insofar as it centers on issues impervious to empirical test, for example, which standards (Sahlins'? Obeyesekere's? some other?) are in use? For each, substantive assumptions about the basic nature of human cognition must first be made, and these assumptions drive subsequent interpretations of the evidence. The different assumptions result in logically

incompatible but empirically equivalent judgments regarding what or how natives think. It is not just that claims to understanding are underdetermined by the available evidence. The very claim that there is a special, determinate understanding remains unjustified, indeed without any supporting argument.

Steven Lukes, a principal in earlier debates regarding Peter Winch's reading of Evans-Pritchard on the Azande, provides an interesting perspective on the Sahlins–Obeyesekere exchange.[18] Lukes too sees the issues as continuous. Is rationality a local matter (as Sahlins maintains) or a transcultural norm (as Obeyesekere insists)?

Lukes, not unreasonably, offers to split the difference: others (or Others) must "minimally" share some sense of truth and reality with us and, as well, have reasons that are contextually determined (Lukes 2000:13). But, as already argued, this suggests no more than the convenience of presuming or postulating a "sharing" for purposes of "getting started" with translation – sharing as a necessary presumption for translation. As such, it fails to inform as to what, if anything, is or must actually be shared. Perhaps commonalties are just blindly imposed in order to "get on" with communication. *There is no distinguishing here between imputing our standards to others and "discovering" that, after all, they share that standard.* Either philosophical assumption – we've made them into us, or we've discovered that they are, in essentials, like us – accommodates the possible outcomes. No *evidence* could *possibly* decide between competing views about "how natives think" in this regard. Rather, what this debates reminds us is that assumptions to the effect that there exists just one "meaning in mind" function as unargued legitimation for what gets done in any case.[19]

Recall, in this regard, the three pillars of meaning realism identified in the previous section: social artifacts (including language) are evidence of meaning, meanings translate, and translations allow of further disambiguation through interpretation. Using these assumptions, historicist perspectivalism underwrites a notion of "real meaning" and a corresponding science of understanding. But note how debate about translation and interpretation undoes in practice this reified view of meaning. For actual debates reveal that translations and interpretations do *not* yield unique results. If the available evidence yields disparate results to interpreters, why not to the natives as well? *So claims that some one meaning, some fixed frame of mind, must stand behind meaning production itself require additional justification.*

"Meaning" may be, as the text metaphor implies, a process that communities investigate, but *nothing* so far mandates that there must be more to meaning beyond social mechanisms for the maintenance of apparent consensus. No argument yet shows that rational translators, working unceasingly on a text, must or will converge on a particular interpretation. Indeed, experience reveals just the opposite result in this regard. *Meaning is "read into" social artifacts on the yet to be justified assumption that a shared meaning must be a condition of their being "available" to others.* Yet nothing so far establishes that the social consumption of texts requires a prior shared meaning.

The Experience Near – Understanding Holocaust Perpetrators

Reconstructing a historically or culturally distant framework of understanding does not benefit by the postulate of an "object of understanding." Meaning can be "stabilized" in different and conflicting ways. Alternatively, emphasis may be placed on rationalizing behavior that is historically or culturally near. This highlights instead the second aspect of the double contingency of interpretation – variation of application of norms within a community. When the problems are set by concerns accounting for why apparently cultural kin behave in a certain way, do appeals to understanding help advance the search for answers? I turn to some recent debates in Holocaust historiography which turn on this issue of finding the determinants of extraordinary mass behavior.

Regarding late eighteenth-century Hawaiians prior to real contact with Europeans, the question concerns just how different could their perception of another human being be from ours? Regarding Germans in the third and fourth decades of the twentieth century with a long history of contact with fellow citizens of Jewish descent, the question too concerns just how different could their perception of another human being be from ours? In the first case, cultural distance animates the question; in the second case, cultural proximity generates the puzzle. How does each group reason from and about experience; what is it like to be one of them?[20]

What motivated Germans of the Nazi era to tolerate and participate in mass killings of Jews and others?[21] Such choices ultimately engender what Raul Hilberg (1985) aptly terms "the destruction of the European Jews" – the Holocaust.[22] Broadly speaking, competing lines of interpretation stress either structural-functional elements – the nature of totalitarian states, the dynamics of modernity, the banality of evil – or motivational/intentional factors – the anti-Semitism of Hitler and his functionaries, or the general climate of anti-Semitism in Germany. Neither alone seems sufficient. The former cannot explain the complicity, indeed enthusiasm, with which the extermination process was embraced. But the intentionalist thesis cannot explain the timing and answer the question of why the Holocaust took place in Germany (and not, for example, France or Russia).[23]

Why not simply combine the two? Because they are (or appear to be) logically incompatible. One explains by specifying a motivation to kill, the other explains why killing occurs in the absence of any clear or fixed plan specifying this outcome. For example, deportation to Madagascar would be a possible functionalist outcome to demands for a "final solution"; for intentionalists, the "final solution" entails genocide by whatever means possible. In this regard, the functionalist thesis does not supplement intentionalism, but replaces it.[24] Intentionalism and functionalism, in short, cannot cohabit the same explanatory framework because they ask substantially different explanatory questions.[25]

A particularly clear example of this debate is the dispute regarding "perpetrator history." The principals here are Daniel Jonah Goldhagen (1997, esp. chs. 6–9)

and Christopher Browning (1992a). The disagreement concerns how to explain the actions of Nazi death squads in Poland and other Nazi-occupied territories.[26] Upon examination, interpretive disagreement only reflects an even more fundamental underlying division on just what needs explaining.

Browning writes as a "modified functionalist" (1992b), Goldhagen as a strong (and broad) intentionalist. Both use basically the same archival evidence to account for the actions of Reserve Police Battalion 101. Both Browning and Goldhagen attempt to answer what I term the "choice problem": why did so many people with no prior history of brutalization or murder participate, at one level or another, in the killing operations? If the theological problem of evil is why a supposedly beneficent deity permits human suffering, the historians' problem of evil is set by the choice problem. As A. D. Moses remarks, "Here we are dealing with very basic, precritical orientations to the problem of evil, which historians bring to bear on the problems they study" (Moses 1998:199–200). Here one seeks a why.

For Goldhagen, the form and evolution of the manner of killing – shootings, gassings, death marches – is incidental to explaining why *Jews* were killed. On Goldhagen's account, all Germans wanted to kill all Jews. The killings took place as circumstances allowed. Bureaucratic structure forms no part of his explanation regarding why the Holocaust happened.

Browning, in contrast, sees the Holocaust as an evolutionary process, one in which lower level officials "felt their way" in response to various problems and pressures, including ambiguous directives, shortages of men and materiel, and "inefficiencies" involved in the task of killing. For Browning, expediency explains, to the extent anything does, the transitions from deportation to the East to shootings to factory-style killing. What emerges retrospectively as "the Holocaust" is real enough, but it represents, for Browning, no single or single-minded intention. Nonintentional factors generate and sustain the deadly dynamics.

Browning brought "perpetrator history" to scholarly center-stage with his 1992 work. He provides a microhistory, an examination of a single (but, as the figures show, typical) battalion of reservists (that is, Germans too old or too unfit for frontline duty) acting as military police in occupied Poland. These "ordinary men" became the instruments of mass executions, shooting to death an estimated 1.3 million civilians, including women, children, and the elderly. The estimated number of people involved in the killing operations is about 300,000. In addition, it is now widely recognized that the general populace knew the fate befalling Jews.

In the context of perpetrator history generally, and the case of Reserve Police Battalion 101 in particular, the apparent absence of exculpatory factors generates the need for explanation. For example, as both Browning and Goldhagen agree, people were not prosecuted for refusing to kill Jews. Soldiers assigned to execution squads could opt out of participation without apparent retaliation, and some did. Most, however, did not. In addition, for the reservists studied by Browning and Goldhagen, anti-Semitism was *not* an expressed motive. When interviewed by

prosecutors in the postwar era, none of the surviving members of the battalion cited hatred of Jews as a reason for their participation.

Further, the reservist battalion, unlike the notorious *SS Einsatzgruppen*, were not self-selected or screened prior to their assignment. To the contrary, the reservists in Battalion 101 came, as Browning notes, from areas of Germany known for low-levels of anti-Semitism and without prior histories of political or criminal involvement. Likewise, since these were reservists, their actions cannot be explained by the psychological effects of brutalization due to service in combat. These men saw no combat. Finally, the operations were carried out without any apparent concern for secrecy. Photos abound. Spouses were present, if not at the site of actions, then in the area. Letters to home communicate what was happening.[27] As scholars focus more on "perpetrator history," solving the choice problem becomes the test for a successful analysis.[28]

Just how "ordinary" are Browning's Germans? Browning, invoking important research in the postwar years by Stanley Milgram and later Philip Zimbardo, concludes (albeit regretfully) that "I must recognize that in such a situation I could have been either a killer or an evader – both were human – if I want to understand and explain the behavior of both as best I can" (Browning 1992c:36). "If the men of Reserve Police Battalion 101 could become killers under such circumstances, what group of men cannot?" (Browning 1992a:189).[29] Whether we are killers or not, in short, is a matter of moral luck.

In his review of Browning's book (published several years prior to his own, and clearly very much on his mind as he wrote), Goldhagen put the difference between his view and Browning's as follows. "The men of Reserve Police Battalion 101 were not ordinary 'men,' but ordinary members of an extraordinary political culture, the culture of Nazi Germany, which was possessed of a hallucinatory, lethal view of the Jews. That view was the mainspring of what was, in essence, voluntary barbarism" (Goldhagen 1992:52). Browning (1996:88–9) concurs with this diagnosis of the difference.

The operant term here is "voluntary." It marks for Goldhagen what separates his account from all others. For Goldhagen charges that a situational or functionalist account of the choice problem provides no answer at all.[30] Given the absence of other possible exculpatory factors noted above, Browning seeks to locate motivating factors, either within the immediate or near situation (peer pressure, role assignment, etc.) or in the background, as just pervasive and enduring parts of the culture in which the reservists operated. The results are not merely predictable but, as Browning states, ones to which any person could or would fall prey. But, Goldhagen protests, Browning's account makes Germans into judgmental dopes of an extraordinary sort. "One does not have to be a Kantian philosopher to recognize and then to say that the wholesale slaughter of unarmed, unresisting men, women, and children is wrong" (Goldhagen 1992:51).[31] Functionalist accounts fail, I take Goldhagen's suggestion to be, because he finds the motivation they ascribe far too weak to rationalize what was done to the Jews.

Goldhagen has been roundly excoriated on almost every aspect of his view, from his advocacy of a monocausal explanation of the Holocaust – the anti-Semites did it, and they were all anti-Semites – to his claims to originality.[32] What has not been appreciated by his critics, however, has been his novel and important complications of the choice problem, and how these complications lend some measure of credibility to his insistence on a monocausal explanation.

Goldhagen puts his critical challenge to functionalism in a chilling way: "Surely the obvious relish of these men [the reservists], the tone that it suggests existed for many in the battalion, casts doubt on the sense of reluctance and disapproval that pervades Browning's book" (Goldhagen 1992:51). Call this the "smile problem." The problem is just this: what, on the functionalist account, explains the exhibited pleasure and enthusiasm with which Jews were persecuted and killed? Peer pressure or situational factors seem explanatory of compliance, not enthusiasm. The pervasive, bloody, and personal forms of killing that precede the death camps seems unexplained by appeal to the "banality of evil."

Browning, to his credit, recognizes and acknowledges this important anomaly for the functionalist and "situationalist" account he otherwise endorses. Describing a series of "Jew hunts" – sweeps of areas supposedly already cleared of the Jewish population – Browning puzzles over how the reservists went "above and beyond" what was required for their grisly tasks. "But the 'Jew hunt' was not a brief episode. It was a tenacious, remorseless, ongoing campaign in which the 'hunters' tracked down and killed their 'prey' in direct and personal confrontation. It was not a passing phase but an existential condition of constant readiness and intention to kill every last Jew who could be found" (Browning 1992a:32). Called upon to account for sustained displays of enthusiasm and initiative, the functionalist has nothing to offer.

Indeed, what Goldhagen most strenuously and consistently maintains wants explaining is the particular viciousness and enthusiasm that the persecution and murder of Jews displays – the "smile problem."[33] "Because the killers . . . did not have to kill, any explanation which is incompatible with the killers' possibility of choice must, in light of this evidence, be ruled out. Germans could say 'no' to mass murder. They chose to say 'yes'" (1997:381, see also 487n.4). No one else has an explanation of the "smile problem"; Goldhagen does.

A. D. Moses' "Structure and agency in the Holocaust" (1998)[34] offers an appreciation of the role of the "smile problem" in lending significance to Goldhagen's claims. But Moses sees the explanatory conflict in terms of commitment to structures – general factors, of the sort Browning favors – and cultural particulars – the specifics of German history and culture that allowed the widespread participation needed to make the Holocaust possible *for Germans*. These, Moses maintains, are basic *metahistorical* "narrative strategies"; it is the basic form, Moses suggests, of historical underdetermination. Historians, that is, can always match the facts to one or the other of these explanatory lines. "The current debate is so polarized because Goldhagen and his critics are arguing about these contending narratives as much as they are disputing 'the facts'"

(Moses 1998:199). In consequence, "the questions he [Goldhagen] has posed for the study of the Holocaust are not the sort that can be dispensed with by reference to some protocol of facticity or professional orthodoxy" (Moses 1998:197). The only way to arbitrate such disputes, according to Moses, is to go to a "deeper level."

Unfortunately, the "deeper level" to which Moses recurs relies on Goldhagen's self-description of his key thesis, that is, that his account alone identifies and details the pervasive motivational factors driving the behavior of perpetrators.

> I acknowledge the humanity of the actors in a specific manner that others do not. . . . I recognize that the perpetrators were not automatons or puppets but individuals who had beliefs and values . . . which informed the choices that these individuals . . . made. My analysis is predicated upon the recognition that each individual made choices about how to treat Jews. It therefore restores the notion of individual responsibility. (Goldhagen 1996:38)[35]

His account of the motivational factors shows why German brutality regularly greatly exceeded what circumstances appear to require in their treatment of Jews. Only by understanding the special character of German anti-Semitism, his claim goes, do the important differences emerge between "ordinary" Germans and "ordinary" men.

Goldhagen makes a solution to the smile problem his litmus test for adequacy of explanation, secure in his knowledge that the smile problem is a singularly glaring and recognized anomaly for the functionalist account. "[M]y critics say that my explanation is wrong without providing any coherent alternative . . . . What critics do not say is that, far from being dismissive of them I demonstrate that the conventional explanations cannot account for the actions of the perpetrators and other central features of the Holocaust to which they pertain" (Goldhagen 1996:39). As far as he goes, Goldhagen is correct.

But the irony here is that Goldhagen's account fails completely to distinguish itself from those he opposes. For Goldhagen's Germans turn out to be as puppet-like, and as psychologically implausible, as the players imagined in the functionalist scenarios. For if the Germans as functionalists imagine them appear incomprehensibly morally numb, Goldhagen's willing executioners are as much social automatons as Browning's ordinary men, and for basically the same reason – neither can reasonably be expected to break the grip of the conditions in which they find themselves.

German anti-Semitism on Goldhagen's account constitutes a type of psychological reagent, an irresistible, coercive belief–desire combination.

> Explaining why the Holocaust occurred requires a radical revision of what has until now been written. This book is that revision.
>
> This revision calls for us to acknowledge what has for so long been generally denied or obscured by academic and non-academic interpreters alike: Germans' antisemitic beliefs about Jews were the *central causal agent* of the Holocaust. They

were the central causal agent not only of Hitler's decision to annihilate European Jewry . . . but also of the perpetrators' willingness to kill and to brutalize Jews. The conclusion of this book is that antisemitism *moved* many thousands of "ordinary" Germans – and would have moved millions more, had they been appropriately positioned – to slaughter Jews. Not economic hardship, not the coercive means of a totalitarian state, not social psychological pressure, not invariable psychological propensities, but ideas about Jews that were pervasive in Germany, and had been for decades, induced ordinary Germans to kill unarmed, defenseless Jewish men, women, and children by the thousands, systematically and without pity. (Goldhagen 1997:9, my italics)

Goldhagen insists upon a "thick description" that pictures people as literally incapable of acting against their beliefs, as locked in the iron grip of socially inculcated categories. "During the Nazi period, and even long before, most Germans could no more emerge with cognitive models foreign to their society . . . than they could speak fluent Romanian without ever having been exposed to it" (1997:34, see also 46). Even more than the Hawaiians as Sahlins portrays them, Goldhagen's Germans cannot think outside their particular cultural box.

Goldhagen's meditations on the iron grip of culture reaches full rhetorical flower when he characterizes the "autonomous power of the eliminationist antisemitism" as having "free rein to shape the Germans' actions to induce Germans voluntarily on their own initiative to act barbarously towards Jews . . ." (1997:449). How does one reconcile the paradoxical suggestion that beliefs have "autonomous power" and that people behave "voluntarily" and "on their own initiative"? Goldhagen's Germans, held in the almost literally hypnotic sway of beliefs, seem more like than different from Browning's "ordinary men." If Browning's soldiers display a puzzling moral numbness, Goldhagen's Germans appear rather too thoroughly culturally brainwashed. Both are moved to act by circumstances beyond their power to resist or control.

Goldhagen never recognizes, much less resolves, his own transformation of Germans into judgmental dopes.[36] But if Goldhagen's "thick description" is correct, his account becomes morally equivalent to functionalism. For functionalists, mass murders happen because, in the context of the system, they became the only practical option for a "final solution." For Goldhagen, genocide results because of a cultural outlook that was literally incapable of imagining Jews as deserving any fate except that which befell them. In both accounts, the people involved move blindly, mechanically, and most notably predictably in response to their environments.

A. D. Moses, as I noted above, nicely characterizes the theoretical and narrative strategies that suggest that the details of Holocaust historiography can be incorporated *ad infinitum* into empirically equivalent but logically incompatible strategies, one stressing structural-functional aspects, the other intentional. But, I have argued, Moses' analysis derails insofar as he accepts Goldhagen's formulation of the smile problem – "Not the method of killing, but the will to kill, is the key issue" (Moses 1998:213) – but fails to recognize that Goldhagen's account

offers no contrast to functionalist automatons (Moses 1998:217). Goldhagen portrays only determinism by other means. Moses, having stared the demon of cultural determinism in the face given the only two theoretical/narrative options, shrinks from drawing the requisite conclusion – Browning's social scientific approach and Goldhagen's ethnographic one simply do not differ on this point.[37]

The actual debate thus only concerns by what path people came to be automatons, not whether or not they were. Neither Browning nor Goldhagen preserve the need for an understanding of perpetrator's actions, if by "understanding" in these cases one means a need to comprehend how individuals rationalize their behavior. For each locates perpetrators in "causally coercive" situations. The overwhelming majority of reservists were fated to behave as they did. Browning relies on "thin description" (not much cultural/historical background needed for explanation), Goldhagen on "thick" (a great deal of cultural background provided for explanation), but to the same effect. We have only judgmental dopes, and the explanatory dispute reduces to the terms of the conditioning. As Thomas Nagel remarks, "The effect of concentrating on the influence of what is not under his control is to make this responsible self seem to disappear, swallowed up by the order of mere events" (Nagel 1987:440). Moral luck determines whether or not one becomes a willing executioner.[38]

In the previous section I argued that appeals to understanding offer no insight into the bases of intersubjectivity since multiple possibilities exist for "stabilizing" meaning, and nothing established that an "originary" meaning must play this role. Yet in the culturally nearer cases as well scouted in this section, no gain results by appeal to nonnatural meaning, albeit for somewhat different reasons. "Thinner" accounts of rationality need not invoke any "originary" or consciously deliberative element at all. Reconstructing behavior Browning's way engenders an account of that behavior as a product of its environment. But "thickening" the description does not enhance the case for nonnatural meaning either. In our example, there is only an "eliminationist anti-Semitism" so strong that people could not possibly think or choose other than as they did.[39] In neither case does an assumption regarding determinacy of meaning advance empirical work or eliminate the glaring (if different) psychological weaknesses of each account. Indeed, the "perpetrator" cases suggest that ceding a need for "special" understanding requires first a clear account of freedom of the will, for otherwise the situationalist explanations may plumb all the "depth" that there is to accounting for motivation.[40]

Conclusion

The philosophical moral I urge from the cases surveyed in the last two sections is this: there is nothing nonnatural needed for purposes of the human sciences. For a supposed problem in accounting for why people did what they did exists only

on the assumption that the agents possess some shared and prior complex of beliefs and motives. Reconstructing these becomes the task of a science of understanding. But whether people are culturally distant or culturally near does not effect the need to interpret or lessen the variability of possible accounts. Whatever we term "explanation" or "understanding" appeals in the end to our ongoing interactions with the world and each other.

In the end, I suggest, any controversy regarding how to parse the difference between explanation and understanding will go the way of the debate in biology on how to cut the difference between the living and nonliving. A strong intuition underwrote the thought that some essential biological difference must account for the difference between living matter and the rest, that such a difference is a difference in kind. But positing theoretical entities to account for this difference turned out to be a misdirected strategy. For as interesting as the difference between living and nonliving might be, making sense of it turned out not to require some essential differences in kind after all. Talk of entities and methods unique to the science of living matter yielded to talk of modes of organizing substances common to all matter. Such a unified scheme sufficed to do the job intended by a distinction in kind.

As for the biological sciences, so for the social sciences. For in the social sciences, what some kinds of reductionisms claim is that the issues of interest in the human sciences get answered (if they allow of scientific answer at all) within the panoply of the special sciences – it is all in our genes, it is all just behavior, and so on.[41] But whether such claims are correct remains an open and empirical question even after one naturalizes understanding – sees it as of a piece with other forms of investigating the natural world.[42]

My claim in this chapter is only that the enduring presumption that something must essentially separate the human from the natural sciences appears as theoretically groundless as the presumption that something essentially separates the living from the rest. At the very least, I have tried to show why those favoring a special science of understanding have yet to make a case for assuming an essential something, a "common meaning" or a "special perspective," for such a science to be about.[43]

Notes

1   Good historical backgrounds to the intellectual origins of these debates are available in Karl-Otto Apel's *Understanding and Explanation* (1984) and Jürgen Habermas's *On the Logic of the Social Sciences* ([1968] 1988) In the analytic tradition, see Peter Winch's *The Idea of a Social Science* (1958). Richard Bernstein provides an excellent survey of the debate Winch inspired in *The Restructuring of Social and Political Theory* (1976). An important collection addressing Winch's work is Brian Wilson's *Rationality* (1970). For a recent survey of how Winch stands on the current scene, see Brian Fay's "Winch's philosophical bearings" (2000), as well as other articles in that issue.

2 Without an argument for dividing the world in this special way – that which is the object of a science of understanding and that which is the object of natural science – meaning is indeterminate. There is no fact of the matter to meaning, no state of things requiring an ontology of meaning for purposes of explanation. As I have long argued, the need for a *prior justification* of a realm of meaning distinguishes the indeterminacy of meaning from garden variety theoretical underdetermination – the plethora of logically distinct but empirically equivalent explanatory theories. (See Roth 1987 and 2000.)

3 Dallmayr and McCarthy (1977:4). A clear and sophisticated introduction to historicism is David D. Roberts, *Nothing But History* (1995).

4 Jim Bohman tells me that this is a term Talcott Parsons uses and for a like purpose.

5 For a helpful and comprehensive introduction to the general intellectual background to Weber's views, see Georg G. Iggers (1997:chs. 1–3). Iggers also provides comprehensive references to relevant literature.

6 See, in particular, Gadamer's development of this point (1979:150f.).

7 "Translation" may be taken as the narrowest of the concepts concerning us here, for it involves the process by which one system of signs is converted into another. But translation is difficult to distinguish from interpretation, even if one seeks only analytic equivalences. For translation shades over into interpretation as one makes explicit justifications for how to map one idiom into another. I find no absolute distinction between what to count as translation and what as interpretation.

8 Gadamer anticipates and articulates what emerges as a fundamental objection to conceptions of historical or cultural realism. For, Gadamer notes, the realism becomes plausible only by turning what is a dynamic (historical) process into a static one. The natural sciences succeed on the basis that the processes governing change are static – that is what allows laws to be laws. But if what is taken to separate history and nature just is the former's lack of perduring general features, then champions of understanding cannot have it both ways. Understanding cannot be postured as apprehension of a dynamic process for purposes of distinguishing it in kind from the natural world but then be construed as a static process in order for the human sciences to have a reality to investigate. See, for example, Gadamer's remarks on "historical objectivism" (1979:158–9). The alleged "textuality" of the nature of the human sciences is, of course, a critical factor in debates regarding the "postmodern condition" in the human sciences. See Hans Kellner's chapter in this volume.

9 Key documents in this particular academic tempest are: Gananath Obeyesekere, *The Apotheosis of Captain Cook* ([1992] 1997) and Marshall Sahlins, *How "Natives" Think: About Captain Cook, For Example* (1995). For a "reader's guide to feuding anthropologists, " see Clifford Geertz, "Culture wars" (1995) and Robert Borofsky, "Cook, Lono, Obeyesekere, and Sahlins" (1997). Ian Hacking weighs in on the dispute in *The Social Construction of What?* (1999:ch. 8).

10 As Clifford Geertz remarks:

> What is at stake here is thus a question that has haunted anthropologists for over a hundred years, and haunts us even more now that we work in a decolonized world: What are we to make of cultural practices that seem to us odd and illogical? . . . In what precisely does reason lie? This is a question to be asked not about eighteenth-century Hawaiians. . . . It is to be asked as well about eighteenth-century Englishmen, sailors and navigators, wandering womanless about the oceans in search of discoveries . . . and of the

inquisitive, aggressive society, the knowledge-is-glory world that, hoping, ultimately, for a temporal salvation, sent the Englishmen there. (Geertz 1995:6)

11 See, for example, Marshall Sahlins (1981:20f.).

12 The Sahlins–Obeyesekere exchange invokes much of the rhetoric of debates regarding postmodernism, postcolonialism, etc. The intellectual/political issue is whether or not social science is just domination by other means. In this regard, the true *éminence gris* in this debate is Sir James Frazer and the tradition represented by *The Golden Bough* (1963). Ironically, where Winch imagines that defending the integrity of understanding "how natives think" requires disputing claims of a single standard of rationality, Obeyesekere seeks to preserve the integrity of native rationality by maintaining that it instantiates just such universal patterns. More on this below.

13 Cook's return was open to the reinterpretation that Lono had come back to challenge the chiefs and priests for power. They met the challenge, but Cook's death did not disprove the godly status which had been previously bestowed. The challenge and cultural response fit with prior understandings of the world.

14 Indeed, Obeyesekere imputes practical rationality to the Hawaiians but not to Cook – or, for that matter, to Sahlins. See Sahlins' remarks (1981:148).

15 See also Obeyesekere ([1992] 1997:60).

16 But then, Sahlins complains, what people share by way of reasoning is precisely what is "*in principle independent of any specific cultural or historical knowledge.*" Obeyesekere's Hawaiians are rational insofar as they cease thinking like Hawaiians (Sahlins 1995:150).

17 For an important critique of efforts to "map" the rules of social intercourse, see Stephen Turner (1994). This is not to say, of course, that one could not prefer one account to the other for good reasons, for example, one seems to accommodate the evidence more successfully than the other. As both Berel Dov Lerner and Karsten Stueber emphasized to me, rejecting meaning realism does not render the debate entirely pointless, or leave one only with the conclusion that any imputation of meaning is as good as any other.

18 Lukes has been remarkably consistent over three decades of discussing these issues. See his "Some problems about rationality" (1970), "Relativism in its place" (1982), and "Different cultures, different rationalities?" (2000).

19 This is the point of a key argument made in *Meaning and Method* (Roth 1987:ch. 9).

20 These are differences of degree, not of kind. Nothing turns on accepting my characterization of what is deviant or distant. The point I wish to emphasize is that problems of meaning do not depend on exotic cases. As Quine maintained, problems of translation begin at home.

21 A number of interesting interpretive debates flourish on various aspects of Holocaust historiography. One is the *Historikerstreit*, which concerns how to accommodate the Nazi period to the rest of modern German history. See, for example, Charles S. Maier (1997), Peter Baldwin (1990), but especially the exchange between Martin Broszat and Saul Friedlander and a special issue of the journal *History and Memory* (9, Fall 1997). A second concerns the latitude of interpretations available for the Holocaust, as represented by criticisms of the historical relativism imputed to Hayden White (see Friedlander 1992).

22 Hilberg's magisterial and monumental work (1985) remains the place where any serious scholarly interest in these events must begin.

23 The classic statement of the intentionalist vs. functionalist theses as dividing the field in Holocaust historiography is Tim Mason, "Intention and explanation: A current controversy about the interpretation of National Socialism" (1981). Two interesting and significant efforts to survey and thematically organize the field are the ones by Michael R. Marrus, *The Holocaust in History* (1987) and "Reflections on the historiography of the Holocaust" (1994). Note, however, that the 1994 article attempts to arrange the field differently.

24 The classic statements are Hannah Arendt, *Eichmann in Jerusalem* ([1965]1990) and Zygmunt Bauman, *Modernity and the Holocaust* (1989). A strong hint of this position is in Hilberg's work as well (1985). See, for example, Bauman (1989:105–6). Asked "Why the Holocaust?" the functionalist accords no necessary place to anti-Semitism or to Germany. For an intentionalist, both of these factors are necessary. There is no obvious or apparent way to "split the difference" between the two theses.

25 One might argue that functionalists and intentionalists seek to answer different explanatory why questions. The functionalist answers the question: why *kill* (as opposed to deport, resettle, jail) Jews? The answer charts the "twisted road to Auschwitz" (see Schleunes 1970). Intentionalists answer the question: why kill *Jews* (rather than the French, Swedes, etc)? Anti-Semitism is the answer. Functionalists deny that killing was the goal from the outset; it is the unintended but inevitable consequence of other policies. Intentionalists insist that killing was the goal from the outset; the only questions were ones of manpower and opportunity. Browning the functionalist finds in Reserve Police Battalion 101 just "ordinary men" down on their moral luck; Goldhagen the intentionalist portrays "Hitler's willing executioners."

26 For a good introduction to the issues and key literature in this acrimonious and vexed debate, see A. D. Moses (1998).

27 All this leads Michael Marrus, in his review of this aspect of the literature in 1994, to quote Walter Laqueur's sardonic comment that, "While many Germans thought that the Jews were no longer alive, they did not necessarily believe that they were dead" (Marrus 1994:110).

28 As Robert Braun remarks, the "banality of evil was Arendt's answer to the choice problem. The 'banality of evil' does not answer our questions about the substance of the human soul but shows us the potential of 'thoughtless' acts" (1994:185).

29 Regarding the "perpetrator mentality," see John Sabini and Maury Silver, "Destroying the innocent with a clear conscience: A sociopsychology of the Holocaust" (1980).

30 Accounts of the genocidal killings beggar the imagination. A singularly striking example of this problem occurs in postwar testimony of a reservist which Browning cites. "I made the effort, and it was possible for me, to shoot only children. It so happened that the mothers led the children by the hand. My neighbor then shot the mother and I shot the child that belonged to her, because I reasoned with myself that after all without its mother the child could not live any longer" (1992a:73). None of these men were ever prosecuted for war crimes.

31 But see below with regard to how this remark comes back to haunt Goldhagen.

32 There are by now several books devoted to Goldhagen's work. For a sense of initial negative scholarly responses, see, for example, Geoff Eley (1997) or Omer Bartov (1996). See also Robert R. Shandley (1998).

33 Indeed, Goldhagen's generally acknowledged substantive contribution to the debate is his account of the death marches in chapters 13 and 14 of *Hitler's Willing Executioners*

(1997). Goldhagen appears to be on solid ground when he complains, against his critics, that however weak they may regard his account, still his is the only game in town, the only proposal on the table that responds to the smile problem (see, e.g., Goldhagen 1996).

34   This essay references much of the secondary literature generated by this debate. But even a casual glance through recent issues of journals such as *History and Theory* or *History and Memory* testifies to the continued strong interest in the historiography of the Holocaust.

35   This point Goldhagen repeats numerous times. See, for example, *Hitler's Willing Executioners* (1997:389–99) for a particularly explicit statement of how Goldhagen situates his account relative to those he opposes.

36   Instead, he maintains, Germans "were not automatons, but were responsible actors, were capable of making choices, and were ultimately the authors of their own actions" (1997:482). At the 1998 NEH Summer Institute, "The Idea of a Social Science – 40 Years Later," Dominic LaCapra lectured on Holocaust historiography. In that context he remarked that someone possessed by the past may be incapable of ethically responsible behavior. For LaCapra's own very thoughtful and nuanced views on the topic of Holocaust historiography, see his *Representing the Holocaust* (1994) and *History and Memory after Auschwitz* (1998).

37   Moses seems to think that a discussion of ideology goes proxy for agency (see his "Conclusion" 1998:217–19). But, for all the reasons rehearsed above, there is just no reason to believe that one is any less "conditioned" to adopt an ideology than anything else.

38   Goldhagen's willingness early on to invoke Kant against his opponents proves deeply ironic. For a Kantian would expect people to be able to morally elevate above their culture by the force of reason alone and perceive moral truth by dint of reason alone. But Goldhagen so chains his Germans to their cultural beliefs that any such elevation becomes impossible.

39   The barbarism cannot be traced exclusively or even primarily to those with some prior history of or commitment to anti-Semitism. This is chillingly brought home in the way in which colleagues and neighbors turn on one another. See, for example, Victor Klemperer, *I Will Bear Witness: A Diary of the Nazi Years, 1933–1941* (1998).

40   Habermas's championing of Goldhagen is to be explained by his view that what Goldhagen provides can be used to make people reflect on the consequences of their "common sense" views about others. Here again we find a strong echo of issues involved in the Obeyesekere–Sahlins version.

   Cannot certain accounts be just ruled out, the contingencies of who wins wars or who controls presses notwithstanding? The literature here is vast. No attempt to assess specific historiographic controversies should ignore the following general background works: Gene Wise, *American Historical Explanations* (1980), Peter Novick, *That Noble Dream: The "Objectivity Question" and the American Historical Profession* (1988), Georg G. Iggers, *Historiography in the Twentieth Century* (1997), and Hayden White, *Metahistory: The Historical Imagination in Nineteenth-Century Europe* (1973). See also White's two influential collections of essays, *The Content of the Form* (1987), and *Tropics of Discourse* (1978). A good introduction to White's work is Wulf Kansteiner, "Hayden White's critique of the writing of history" (1993). See also Fay et al. *History and Theory: Contemporary Readings* (1998).

41   For more on these issues, see the essay by Lynn Hankinson Nelson in this volume.

42   For an interesting and illuminating account of the historical background to the debate in biology, see Ronald Munson, "Mechanism, vitalism, reductionism, and organismic biology" (1979). For an insightful account of how resistance to reductionism gets confused and mistakenly intertwined with a resistance to naturalism, see Clifford Geertz, "The strange estrangement: Taylor and the natural sciences" in *Available Light* (2000:83–95).

43   I wish to thank James Bohman, Larry Davis, Laura Howard, Berel Dov Lerner, Piers Rawling, Karsten Stueber, and Stephen Turner for help with an earlier draft of this paper.

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