ROGER F. GIBSON JR.

Willard Van Orman Quine

Willard Van Orman Quine was born in Akron, Ohio, on June 25, 1908. His father, Cloyd Robert Quine, was an Akron businessman with a machine shop background. In 1917, Cloyd Quine founded the Akron Equipment Company, whose business was the manufacture of tire molds. The business flourished, what with Akron being then the rubber tire capital of the world. Willard's mother, Harriet Ellis Van Orman, was a housewife and public school teacher who taught at a local elementary school for ten years. In his autobiography Quine fondly recalls his mother's culinary skills:

My mother baked bread and rolls in my early years and the smell beckoned. She was also good at pies, cakes, and strawberry shortcake. She made jelly from the fruit of our little quince tree, and she made cherry sunshine by the heat of the sun. (TL 12)

Harriet Quine considered herself to be deeply religious, and in her later life she became a deaconess in the Congregational Church. The religious training of Willard and his only sibling, Robert Cloyd Quine, a year and a half his senior, consisted of their being "sent to Sunday school about half the time, and seldom sent to church" (TL 14). However, the more Willard was exposed to the Word, the more skeptical he became:

I may have been nine when I began to worry bout the absurdity of heaven and eternal life, and about the jeopardy that I was incurring by those evil doubts. Presently I recognized that the jeopardy was illusory if the doubts were right. My somber conclusion was nonetheless disappointing, but I rested with it. I said nothing of this to my parents, but I did harangue one or another of my little friends, and I vaguely remember a parental repercussion. Such, then,

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was the dim beginning of my philosophical concern. Perhaps the same is true of the majority of philosophers. (TL 14)

Young Willard seems to have enjoyed a pleasant middle-class up-bringing in Akron, with plenty of playmates and frequent interaction with his extended family living in and around Akron. It was also during these formative years that he developed a lifelong passion for world geography and maps and a seemingly insatiable yearning to travel. (In 1968 he would publish a review of *The Times Atlas of the World* in the *Times* of London.)

Quine earned his diploma from Akron's West High School in January 1926 at the age of seventeen. In the fall of 1926 he entered Ohio's Oberlin College. During his freshman year he learned from a fellow student of the existence of a British philosopher by the name of Bertrand Russell who had a "mathematical philosophy." Quine was intrigued: "Mathematics was a dry subject, and stopped short of most that mattered, but the link to philosophy promised wider possibilities" (TL 51). Thus Quine chose to major in mathematics, with honors reading in mathematical philosophy, that is, in mathematical logic.

Much contentment with my mathematics major came in my Junior year, with my honors reading. Nobody at Oberlin knew modern logic; however, the chairman of the mathematics department, William D. Cairns, made inquiries and got me books. They were Venn's *Symbolic Logic*, Peano's *Formulaire de mathématiques*, Couturat's *Algebra of Logic*, Whitehead's *Introduction to Mathematics*, Keyser's *Mathematical Philosophy*, Russell's *Principles of Mathematics*, and the crowning glory, Whitehead and Russell's *Principia Mathematica*. (TL 59)

Quine graduated summa cum laude from Oberlin in 1930. However, his exposure to Russell, especially to the Russell of "On Denoting" and *Principia Mathematica*, made a lifelong impession on Quine. So did Quine's exposure to John B. Watson's behaviorism, which he studied in a psychology course at Oberlin. Years later Quine wrote the following about Harvard's great behaviorist B. F. Skinner (who was a junior fellow with Quine in Harvard's Society of Fellows from 1933 to 1936):

Fred and I were congenial, sharing an interest in language and behavioristic bias in psychology. It has been wrongly assumed that I imbibed my

behaviorism from Fred; I lately learned from his autobiography that in fact my exposure to John B. Watson antedated his. It was particularly in language theory, rather, that Fred opened doors for me . . . ; he put me onto Bloomfield and Jespersen and gave me a first American edition of John Horne Tooke. (TL 110)

The rest is history, as the saying goes. (See Chapter 7 for more on Quine's behaviorism.)

In part because Whitehead was a faculty member of Harvard's philosophy department, Quine applied for admission to Harvard's graduate program in philosophy, beginning in the fall of 1930. His application was successful, so in the late summer of 1930 Quine and his soon-to-be wife, Naomi Clayton, hitchhiked from Ohio to Boston.

Our last ride was on a fish truck, from which we dropped into Scollay Square. I took a room in Allston Street, between the statehouse and the courthouse, and Naomi stayed with a cousin in Brookline. My scholarship would have been voided by marriage, but I applied to the department chairman, James Houghton Woods, and got a waiver. We were married in Marblehead by a justice of the peace. (TL 75)

Now a married couple, the Quines

moved into a furnished room and kitchen in Mrs. Sheehan's house at 13 Howland Street, Cambridge, close to Somerville. Learning that we we were from Ohio, she told us that she had a brother in Idaho and that the lady across the street was from "Motano." It's a small world. (TL 75)

In a somewhat Herculean effort, largely induced by the hard economic times of the Great Depression, Quine managed to complete his Ph.D. in just two years. His dissertation, *The Logic of Sequences: A Generalization of Principia Mathematica*, was (nominally) directed by Whitehead. Some fifty years later Quine reminisced, "Long sleepless and with a week's beard, I took the dissertation to Whitehead's in the evening of April 1, 1932, with three hours to spare" (TL 86). Quine was but twenty-three when awarded his two-year Ph.D.

For the next four years Quine enjoyed fellowships. First was a Sheldon Traveling Fellowship (1932–33), followed by three consecutive years as a junior fellow in Harvard's brand-new Society of Fellows. During his Sheldon year Quine visited Vienna, where he attended Moritz Schlick's lectures given at the University of Vienna

and also went to the weekly meetings of the Vienna Circle. At those meetings Quine met Kurt Gödel, Friedrich Waismann, and A. J. Ayer, among other notables of the Vienna Circle. Rudolf Carnap had moved from Vienna to Prague, but he and Quine first met when Carnap visited Vienna in late 1932 (possibly December): "Carnap contracted a fever on arriving in Vienna. I met him in the hospital and we settled on March 1 for the move to Prague" (TL 95). Quine also visited Warsaw, where he met Stanislaw Leśniewski, Jan Łukasiewicz, and Alfred Tarski, among other prominent logicians. In a letter Quine sent from Vienna to his parents in Akron, he wrote,

I have written a note to the great Wittgenstein. He now teaches in Cambridge, England, but . . . probably spends his vacations here in Vienna. I want an audience with the prophet. It remains to be seen whether he . . . will act on my request (for he doesn't know how nice I am). (TL 88), (italics in original)

Unfortunately for posterity: "Of course he did not answer. . . . I have never seen Wittgenstein" (TL 88).

Nevertheless, his Sheldon year proved to be a watershed for Quine, especially the weeks he spent in Prague with Carnap. In late January 1933, Quine and Naomi joined up with Carnap and his wife Ina in Prague. Quine warmly recalled,

We were overwhelmed by the kindness of the Carnaps. He had written me twice with information and sent a map. I attended his lecture the day after our arrival, and he invited us to their house. Meanwhile his Viennese wife Ina, hearing from him of our lodging problem, tramped the streets with us for three hours, talking in broken Czechish with the landladies. (TL 97)

The Quines and the Carnaps saw a lot of each other over the next two months, February and March 1933:

I eagerly attended Carnap's lectures. He was expounding his *Logische Syntax der Sprache*, which Ina was typing. Carnap lent me the typescript sheaf by sheaf. Days when he was not lecturing, Naomi and I would go to their flat. . . . He and I would discuss his work. . . . But it was made clear that after supper there could be only small talk, no "science," or Carnap would have a sleepless night. He was a big man, mild and genial, with a stern regimen. No alcohol, no tobacco, no coffee. (TL 98)

During his stay in Prague, Quine was an impressionable young man of 23; Carnap was 41. Quine describes Carnap's lasting influence

on him as follows:

Carnap was my greatest teacher. I got to him in Prague ... just a few months after I had finished my formal studies and received my Ph.D. I was very much his disciple for six years. In later years his views went on evolving and so did mine, divergent ways. But even where we disagreed he was still setting the theme; the line of my thought was largely determined by problems that I felt his position presented. (HRC 41)

The Quines departed Prague in April 1933. Three years later, the Carnaps emigrated to the United States.

Carnap died in 1970. At a memorial meeting held in Boston in October 1970, under the auspices of the Philosophy of Science Association, Quine presented "Homage to Rudolf Carnap," in which he wrote,

Carnap is a towering figure. I see him as the dominant figure in philosophy from the 1930s onward, as Russell had been in the decades before. Russell's well-earned glory went on mounting afterward, as the evidence of his historical importance continued to pile up; but the leader of the continuing developments was Carnap. Some philosophers would assign this role rather to Wittgenstein; but many see the scene as I do. (HRC 40)

Upon Quine's return to the United States, he began the first of his three years as a junior fellow. In November 1934, Quine gave three largely sympathetic lectures at Harvard on Carnap, in effect introducing Carnap to an American audience. (See Chapter 9 for an examination of the extent to which Quine was influenced by logical positivism.)

In 1936, at the conclusion of his three years as a junior fellow, Quine was appointed to the Harvard philosophy faculty. In 1942 he joined the Navy, rising to the rank of lieutenant commander before the war's end in 1945. Quine resumed his teaching duties at Harvard in 1946. In 1947 he and Naomi divorced. The following year he was made a senior fellow in the Society of Fellows, the same year he married Marjorie Boynton. Quine had two daughters with Naomi and a son and a daughter with Marjorie.

Quine continued to teach at Harvard until 1978, when he reached the mandatory retirement age of seventy. However, he continued to give lectures around the world, and to publish, until 1998, when he was ninety. He died on Christmas Day, 2000 at the age of 92.

During his stellar sixty-five-year-long career he published twenty-some books and scores of articles, and he lectured in six languages on six continents. He made major contributions to a large number of fields within philosophy, including epistemology, metaphysics, metaethics, logic, set theory, philosophy of logic, philosophy of language, philosophy of science, and philosophy of mind. In recognition of his many contributions, Quine was awarded eighteen honorary degrees and numerous other honors, prizes, and medals. Without doubt, Quine was one of the most gifted and influential analytic philosophers of the twentieth century and belongs squarely in the ranks of Carnap, Russell, and Wittgenstein. (See the website maintained by Quine's son Dr. Douglas Quine: http://www.wvquine.org.)

In spite of the diversity of Quine's contributions to philosophy, they form a systematic unity. Quine once remarked that the bulk of his philosophy consists of corollaries to his commitments to *naturalism* and *extensionalism*. In a word, Quine was a systematic philosopher.

As a naturalist, Quine accepts the following two claims: First, there is no successful first philosophy – that is, no experiential or a priori ground outside of science upon which science can be justified or rationally reconstructed. Second, it is up to science to tell us what there is and how we know what there is – that is, science is the measure of what there is (ontology) and of how we come to know what there is (epistemology). Furthermore, according to Quine, the currently best science advocates a physicalist ontology and an empiricist epistemology. So Quine the naturalist is also Quine the physicalist and Quine the empiricist.

To say that Quine is a *physicalist* can be interpreted in at least three ways, depending on the context. When the context is philosophy of language, the term 'physicalism' signals his rejection of mentalistic semantics; when the context is philosophy of mind, the term signals his rejection of mind-body dualism; when the context is general ontology, the term signals his acceptance of the doctrine that "nothing happens in the world, not the flutter of an eyelid, not the flicker of a thought, without some redistribution of microphysical states" (GWW 98). However, Quine's ontological physicalism includes more than microphysical states (i.e., physical objects); it also includes the abstract objects of mathematics, such as numbers or sets. Quine is obligated to admit these abstract objects into his

physicalist ontology because science would be impossible without them. Accordingly, Quine represses his nominalistic predilections and somewhat grudgingly embraces a bifurcated ontology (physical objects and sets). Bifurcated, yes, but singularly extensional, for all its objects are suitable to be values of the bound variables of some formalized version of the best scientific theory we can muster at the time. Let's unpack the previous sentence; what is extensionalism?

A context is *extensional* if its truth value cannot be changed by supplanting a component sentence by another of the same truth value, nor by supplanting a component predicate by another with all the same denota, nor by supplanting a singular term by another with the same designatum. Succinctly, the three requirements are substitutivity of *covalence*, of *coextensiveness*, and of *identity, salva veritate*. A context is *intensional* if it is not extensional. (FSS 90)

So, for example, the context of 'Cicero' in 'Cicero was a Roman' is extensional since a codesignatum of 'Cicero', say 'Tully', can be substituted in the context to produce a sentence ('Tully was a Roman') having the same truth value as 'Cicero was a Roman'. However, the context of 'Cicero' in 'Tom believes Cicero was a Roman' is intensional since a codesignatum of 'Cicero', say 'Tully', can be substituted in the context to produce a sentence ('Tom believes Tully was a Roman') having a different truth value from 'Tom believes Cicero was a Roman'. (For example, it may be true that Tom believes Cicero was a Roman but false that Tom believes Tully was a Roman, for Tom may not know that Cicero and Tully are one and the same person.) Now we may characterize Quine's extensionalism as the doctrine that extensionality is necessary, though not sufficient, for a full understanding of a theory (see FSS 91–2). (See Chapter 8 for an account of the evolution of Quine's argument against quantified modal logic.)

An extensional language par excellence is elementary logic (i.e., first-order predicate logic with relations and identity) augmented by the epsilon of set theory. (See Chapter 10 for more on Quine's philosophy of logic.) Quine maintains that, given such a language, one can determine the ontological commitments of a theory by translating the theory into the canonical idiom and noting the range of its bound variables: *To be is to be the value of a bound variable*. By this criterion, if a scientific theory quantifies over both physical objects

and sets, then the theory is committed to physical objects and sets. Notice that the criterion does not determine what exists, it determines merely what a theory says exists; the criterion is trivial. Moreover, for an entity to be the value of a bound variable, it must have identity criteria: *No entity without identity*. For example, physical objects are identical if and only if they occupy the same region(s) of space-time, while sets are identical if and only if they have the same members. So, to say that Quine's ontological physicalism countenances a bifurcated but extensional ontology is to say that when the best scientific theory we have is translated into the canonical idiom, we find it irreducibly quantifying over both concrete and abstract objects, namely, physical objects and sets. (See Chapter 5 for further discussion of first-order logic, reference, and ontological commitment.)

Returning to the discussion of Quine's naturalism, we should note that as an *empiricist* Quine accepts the following two cardinal tenets of empiricism: "Whatever evidence there *is* for science *is* sensory evidence . . . [and] all inculcation of meanings of words must rest ultimately on sensory evidence" (EN 75). Consistent with his naturalism, Quine cites science as the source of these two tenets of empiricism:

Science itself teaches that there is no clairvoyance, that the only information that can reach our sensory surfaces from external objects must be limited to two-dimensional optical projections and various impacts of air waves on the eardrums and some gaseous reactions in the nasal passages and a few kindred odds and ends. (RR 2)

As we have just seen, Quine's acceptance of a physicalist ontology and an empiricist epistemology is based on scientific findings. Not that the naturalistic philosopher must slavishly defer to the scientist in these matters, nor must the naturalistic philosopher become a scientist. The home domains of the scientist and of the philosopher are distinct but overlapping. In *Word and Object* Quine put the point as follows:

Given physical objects in general, the natural scientist is the man to decide about wombats and unicorns. Given classes, or whatever other broad realm of objects the mathematician needs, it is for the mathematician to say whether in particular there are even prime numbers or any cubic numbers that are sums of pairs of cubic numbers. On the other hand it is scrutiny of this uncritical acceptance of the realm of physical objects, or of classes, etc.,

that devolves upon ontology. Here is the task of making explicit what had been tacit, and precise what had been vague, of exposing and resolving paradoxes, smoothing kinks, lopping off vestigial growths, clearing ontological slums.

The philosopher's task differs from others', then, in detail; but in no such drastic way as those suppose who imagine for the philosopher a vantage point outside the conceptual scheme that he takes in charge. There is no such cosmic exile. He cannot study and revise the fundamental conceptual scheme of science and common sense without having some conceptual scheme, the same or another no less in need of philosophical scrutiny, in which to work. (WO 275–6)

Thus, Quine's naturalistic philosopher operates in a conceptual space between the uncritical acceptance of objects by the scientist (in the broadest sense), on the one hand, and the feigned cosmic exile of the philosopher, on the other.

Finally, we must note that Quine is a *fallibilist*. He recognizes that science changes over time and that someday science could conceivably withdraw its support for physicalism and/or empiricism. Thus Quine's commitments to physicalism and empiricism are firm but tentative.

As previously mentioned, Quine repudiates first philosophy, that is, traditional epistemology. However, he does not repudiate epistemology altogether. There remains what he calls *naturalized epistemology*: the scientific study of man's acquisition of science.

A far cry, this, from old epistemology. Yet it is no gratuitous change of subject matter, but an enlightened persistence rather in the original epistemological problem. It is enlightened in recognizing that the skeptical challenge springs from science itself, and that in coping with it we are free to use scientific knowledge. The old epistemologist failed to recognize the strength of his position. (RR 3)

Some philosophers have claimed that Quine's naturalized epistemology is not epistemology at all, for epistemology is normative whereas so-called naturalized epistemology (the scientific study of man's acquisition of science) drops the normative in favor of the descriptive. However, as Quine explains,

The normative is naturalized, not dropped. The crowning normative principle of naturalized epistemology is nothing less than empiricism itself; for empiricism is both a rule of scientific method and a scientific discovery. It

is natural science that tells us that our information about the world comes only through impacts on our sensory surfaces. And it is conspicuously normative, counselling us to mistrust soothsayers and telepathists.

For normative content of a more technical kind we may look to mathematical statistics. These norms, again, are at the level of science itself. Normative epistemology, under naturalism, is simply the technology of science, the technology of predicting sensory stimulation. It is scientific method. (CL 229)

It is clear from these remarks that Quine regards naturalized epistemology to be normative as well as descriptive. However, it is also clear that Quine regards naturalized epistemology to be a far cry from old epistemology, that is, a far cry from the tradition connecting Descartes' rationalism with Carnap's empiricism. Indeed, in so far as epistemology is taken to be a quest for a theory of knowledge, Quine's naturalized epistemology would not count as epistemology. Quine explains:

I think that for scientific or philosophical purposes the best we can do is give up the notion of knowledge as a bad job and make do rather with its separate ingredients. We can still speak of a belief as true, and of one belief as firmer or more certain, to the believer's mind, than another. There is also the element of justification.... These reflections perhaps belong in their rudimentary way to the branch of philosophy known as epistemology, the theory of knowledge. Rejection of the very concept of knowledge is oddly ironic. (Q 109)

Epistemology or not, it is important in understanding Quine to appreciate that he takes naturalism very seriously. Thus consider the following three versions of the same theme of naturalism: (1) For Quine, science and epistemology contain one another, though in different senses of 'contain'. There being no first philosophy, science contains epistemology in the sense that engaging in epistemology presupposes an accepted scientific framework as background; epistemology contains science insofar as science is constrained by the findings of epistemology. (2) Quine endorses Otto Neurath's likening "science to a boat which, if we are to rebuild it, we must rebuild plank by plank, while staying afloat in it. The philosopher and the scientist are in the same boat" (WO 3). (3) Concerning the positing of objects, Quine writes,

To call a posit a posit is not to patronize it. A posit can be unavoidable except at the cost of other no less artificial expedients. Everything to which

we concede existence is a posit from the standpoint of a description of the theory-building process, and simultaneously real from the standpoint of the theory that is being built. Nor let us look down on the standpoint of the theory as make-believe; for we can never do better than occupy the standpoint of some theory, the best we can muster at the time. (WO 22)

Thus for Quine neither epistemologizing, nor revising one's conceptual scheme, nor speculating on the positing of bodies takes place on a vacuum. There is always some background theory that is accepted (even if only temporarily) at face value. In sum, as Quine states,

[My] position is a naturalistic one; I see philosophy not as an *a priori* propaedeutic or groundwork for science, but as continuous with science. I see philosophy and science in the same boat – a boat which, to revert to Neurath's figure as I so often do, we can rebuild only at sea while saying float in it. There is no external vantage point, no first philosophy. (NK 126–7)

Robert Fogelin, in Chapter 1, explains key aspects of Quine's naturalized epistemology. Along the way, Fogelin draws some interesting parallels between David Hume and Quine. Surprisingly, Fogelin claims that some of Quine's views on language, meaning, and reference are decidedly not naturalistic. Fogelin concludes (laments?) that Quine is not as thoroughgoing a naturalist as he professes to be.

I have alluded to Quine's remark that the bulk of his philosophy consists of corollaries to his naturalism and extensionalism. And now that we have some appreciation of those commitments, we can now inquire into some of those corollaries.

ANALYTICITY

Beginning with their discussions in Prague in 1933 and episodically during the 1940s and 1950s, Quine and Carnap expressed their disagreement over the question of the intelligibility of the so-called analytic-synthetic distinction. Analytic statements (so called) are those deemed true (or false) *solely* in virtue of their meanings (e.g., 'All triangles have three sides'). Synthetic statements (so called) are those deemed true (or false) in virtue of their meanings *and* how the world is (e.g., 'There have been black dogs'). Carnap accepted this distinction; Quine rejected it.

In his most famous article, "Two Dogmas of Empiricism" (1951), Quine attempts to show that the analytic-synthetic distinction is a dogma of empiricism, a metaphysical article of faith. More

particularly, consistent with his commitment to extensionalism, Quine rejected analyticity because it relies on an unempirical notion of meaning (e.g., true solely in virtue of meanings).

But why did Carnap and Quine regard the question of analyticity to be so philosophically important? One answer is that as empiricists they regarded all knowledge of the world to be a posteriori, and contingent yet logic and mathematics appear to be a priori and necessary. How can empiricists account for this appearance? Three general approaches to the problem come to mind.

First, there is the approach taken by John Stuart Mill, who argues that the truths of logic and mathematics have empirical content and are therefore not necessary. They are, in fact, empirical generalizations based on induction. As such, they are contingent; their apparent necessity is nothing more than the product of habituation. Mill's approach is truly heroic but highly implausible.

Second, there is the approach taken by Carnap, who argues that the truths of logic and mathematics lack empirical content and are necessary. However, according to Carnap such statements pose no threat to empiricism since their lack of content and their necessity follow directly from their analyticity: The statements of logic and mathematics are true (or false) solely in virtue of their meanings. In a word, they are tautologies. Quine conjectures that

Carnap's tenacity to analyticity was due largely to his philosophy of mathematics. One problem for him was the lack of empirical content: how could an empiricist accept mathematics as meaningful. Another problem was the *necessity* of mathematical truth. Analyticity was his answer to both. (TDR 269)

Third, there is the approach taken by Quine: "I answer both [problems] with my moderate holism. Take the first problem: lack of content. Insofar as mathematics gets applied in natural sciences, I see it as sharing empirical content" (TDR 269).

However, it should be noted that Quine eventually came to share Carnap's view that mathematics lacks content:

[Roger] Gibson has found, to my chagrin but gratitude, a disagreement between my consecutive little books *Pursuit of Truth* [1990] and *From Stimulus to Science* [1995] regarding empirical content of mathematics. I rest with the later position, namely, that mathematics lacks empirical content. The point is that no set of mathematical truths implies any synthetic observation categoricals. (RGQ 685)

What now of the second problem, the apparent necessity of mathematics?

This again is nicely cleared up by moderate holism, without the help of analyticity. For let us recall that when a cluster of sentences with critical semantic mass is refuted by an experiment, the crisis can be resolved by revoking one *or* another sentence of the cluster. We hope to choose in such a way as to optimize future progress. If one of the sentences is purely mathematical, we will not choose to revoke it; such a move would reverberate excessively through the rest of science. We are restrained by a maxim of minimum mutilation. It is simply in this, I hold, that the necessity of mathematics lies: our determination to make revisions elsewhere instead. I make no deeper sense of necessity anywhere. Metaphysical necessity has no place in my naturalistic view of things, and analyticity hasn't much. (TDR 269–70)

In *Roots of Reference* (1974), Quine made a positive effort to see just what empirical sense, if any, could be made of analyticity in terms of language learning:

Carnap maintained, and Frege before him, that the laws of logic held by virtue purely of language: by virtue of the meanings of the logical words. In a word, they are analytic. I have protested more than once that no empirical meaning has been given to the notion of meaning, nor consequently, to this linguistic theory of logic. But now in the terms of the learning process can we perhaps find some sense for the doctrine? (RR 78)

Quine goes on to explain that a standing sentence (i.e., a sentence that does not require the presentation of a nonverbal stimulus each time the sentence is queried for assent or dissent, such as 'The *Times* has arrived') is analytic "if *everybody* learns that it is true by learning its words" (RR 79). For example, if everybody in the speech community learns 'bachelor' by discovering that those speakers from whom they are learning their language are disposed to assent to it in just those circumstances where they would assent to 'unmarried man', then, in virtue of that discovery, everybody in the speech community has learned the truth of the standing sentence 'A bachelor is an unmarried man'. Such a sentence approximates analyticity.

Even so, we have here no such radical cleavage between analytic and synthetic sentences as was called for by Carnap and other epistemologists. In learning our language each of us learns to count certain sentences, outright, as true; there are sentences whose truth is learned in that way by many of us, and there are sentences whose truth is learned in that way by few or none of us. The former sentences are more *nearly* analytic than the latter. The

analytic sentences are the ones learned in that way by all of us; and these extreme cases do not differ notably from their neighbors, nor can we always say which ones they are. (RR 80)

In Chapter 2, Richard Creath explains and critiques the Carnap-Quine debate over analyticity. In fashioning a defense of Carnap's position, Creath makes novel use of Quine's attempt to explicate analyticity in terms of language learning, explained earlier. Ultimately, Creath declares the debate a draw, and in a true Carnapian spirit of toleration he urges that much is to be learned by pursuing both Carnap's and Quine's approaches to analyticity. (See Chapter 5 for a discussion of Carnap's principle of toleration.)

HOLISM

As we have seen, one of the dogmas that Quine repudiates in "Two Dogmas of Empiricism" is analyticity; the other dogma is reductionism, that is, the view that each sentence of a scientific theory admits, individually, of confirmation or infirmation. Quine's holistic "countersuggestion . . . is that our statements about the external world face the tribunal of sense experience not individually but only as a corporate body" (TDEb 41). But, more precisely, what is holism? "It is holism that has rightly been called the Duhem thesis and also, rather generously, the Duhem-Quine thesis. It says that scientific statements are not separately vulnerable to adverse observations, because it is only jointly as a theory that they imply their observable consequences" (EES 313).

The holism espoused by Quine in "Two Dogmas" is extreme because he intended the expression 'corporate body' therein to include all of science. However, nearly a decade later in *Word and Object* (1960) and in some of his subsequent writings he moderated his holism. He acknowledged that it is more accurate to think of significant stretches of science, rather than the whole of science, as having observable consequences:

[W]e can appreciate . . . how unrealistic it would be to extend a Duhemian holism to the whole of science, taking all of science as the unit that is responsible to observation. Science is neither discontinuous nor monolithic. It is variously jointed, and loose at the joints in various degrees. In the face of a recalcitrant observation we are free to choose what statements to revise and

what ones to hold fast, and these alternatives will disrupt various stretches of scientific theory in various ways, varying in severity. Little is gained by saying that the unit is in principle the whole of science, however defensible this claim may be in a legalistic way. (EES 314–5)

Thus, moderate holism is an important part of Quine's philosophy of science. (See Chapter 3 for a discussion of Quine's holism.)

UNDERDETERMINATION

It is obvious that scientific theory (what Quine calls physical theory) deductively implies various statements descriptive of observable circumstances, and it is equally obvious that those same statements do not deductively imply the theory. In Quine's terminology, any theory manifesting such empirical slack is said to be underdetermined by experience. Quine articulates three main varieties of underdetermination. First, theories are underdetermined by past observation because some future observation might conflict with them. Second, theories are underdetermined by both past and future observations because some conflicting observation may go unnoticed. Third, theories are underdetermined by all possible observations because the observational criteria of theoretical terms are so flexible and fragmentary. It is this third variety of underdetermination that Quine and his commentators have focused on, for it suggests the philosophically intriguing prospect of there being alternative theories that are empirically equivalent and yet logically incompatible with one another. Such is Quine's thesis of underdetermination of physical theory, which in Chapter 4 Lars Bergström seeks to elucidate and to criticize. Bergström skillfully teases apart the various strands constitutive of the thesis of underdetermination: theory, theory formulation, empirical content, empirical equivalence, observation sentence, logical incompatibility, and so on. Along the way, he argues that acceptance of the underdetermination thesis leads to skepticism and relativism.

RADICAL TRANSLATION

Are there such entities as propositions? A necessary condition for something to be an entity is that it must possess identity conditions;

for Quine there can be no entity without identity. If propositions are entities, then they must possess identity conditions that determine when we have a single proposition and when we have different propositions or the same proposition. How might we tell, for example, whether the utterance of 'Carnap taught Quine' expresses a single proposition and whether the utterance of 'Carnap taught Quine' and the utterance of 'Quine was taught by Carnap' express different propositions or the same proposition? One not very informative answer to the former question is that a proposition is a single proposition just in case it does not contain another proposition as a constituent. An equally unsatisfactory answer to the latter question is that a proposition is what utterances of a declarative sentence and its translations have in common. And what they have in common is sentence meanings – objectively valid translation relations. On this approach, one might say that if the utterance of 'Carnap taught Quine' and the utterance of 'Quine was taught by Carnap' are translations of one another, they are so because the two express the same meaning (or proposition). However, Quine's position is just the reverse: if utterances of the two sentences in question are said to express the same meaning (or proposition), they do so because they are translations of one another. For Quine, translation (synonymy) is where the philosophical action is, meanings (or propositions) are by the by. In his famous thought experiment of radical translation, he is out to show that whatever propositions might be, they are not sentence meanings.

Radical translation is an idealized context in which a field linguist sets about translating a hitherto unknown language that has no historical or cultural connections with any known language. Nor does the linguist have recourse to bilinguals. Presumably, then, the total empirical data available to the linguist consist of the observable behavior of native speakers amid publicly observable circumstances. Moreover, none of the empirical data is hidden from the linguist. Even so, the linguist's completed manual for translating the foreign language (Jungle) into the linguist's home language (English) is underdetermined by all of the possible empirical data. In particular, the translation of the foreign language's terms and the meanings of its theoretical sentences are underdetermined.

So much is relatively uncontroversial, but Quine concludes from this thought experiment that the translation of theoretical sentences

is not merely underdetermined but *indeterminate*. (See Chapter 5 for a discussion of Quine's thesis of indeterminacy of *terms*.) In what sense is the translation of theoretical sentences indeterminate? In the sense that the same foreign sentence can be translated equally well by two (or more) different home language sentences.[1] This is the core idea of Quine's famous thesis of indeterminacy of translation.

But if indeterminacy is accepted, then sentence meanings do not have identity conditions and therefore cannot serve as propositions or as objectively valid translation relations, for there is no entity without identity. Quine's argument assumes, reasonably enough, that a necessary condition for the identity of propositions is as follows: If P_1, P_2, and P_3 are propositions, then if $P_1 = P_2$ and $P_1 = P_3$, then $P_2 = P_3$. But this is just the identity condition that indeterminacy of translation shows that sentence meanings lack. Consider: if S_1, S_2, and S_3 are sentence meanings, then if $S_1 = S_2$ and $S_1 = S_3$, it does not follow that $S_2 = S_3$. In Quine's own words, "What the indeterminacy of translation shows is the notion of propositions as sentence meanings is untenable" (PTb 102). Finally, note that the indeterminacy of translation is not a problem confronting translation; in particular, it is not the claim that some sentences are untranslatable. On the contrary, it is the claim that some sentences have more than one acceptable translation. Thus indeterminacy is good news, not bad news. (See Chapter 6 for an in-depth analysis and evaluation of Quine's indeterminacy thesis, and see Chapter 1 for the claim that Quine's thought experiment of radical translation is inconsistent with his professed naturalism.)

SUMMARY AND CONCLUSION

As we have seen, Quine regarded himself to be a systematic thinker insofar as the bulk of his philosophy, to include his repudiation of the two dogmas of empiricism (viz., the analytic-synthetic distinction, and reductionism), and his advocacy of moderate holism, underdetermination of physical theory, and indeterminacy of translation, as well as his advocacy of physicalism and empiricism, consists in corollaries of his naturalism and extensionalism. This systematic philosophy established Quine as the most influential philosopher of the latter half of the twentieth century: his philosophical interests

and problems became the philosophical community's interests and problems.

A tempting, but perhaps impossible, question is: What might be Quine's enduring legacy for the twenty-first century? Only time can tell, but I suggest that his revival of naturalism (the "naturalistic turn") will survive well into the new century: Philosophy is continuous with science; there is no first philosophy, no external vantage point (see NK 125–26).

NOTE

1. How are we to take 'equally well' and 'different' as they occur in this sentence? Following Quine, we can say that the two (or more) home language sentences serve equally well as translations of some foreign sentence just in case those home language sentences facilitate communication to the same degree. "Success in communication is judged by smoothness of conversation, by frequent predictability of verbal and nonverbal reactions, and by coherence and plausibility of native testimony" (PTb 43). Again following Quine, we can say two sentences of the home language are different just in case they are not interchangeable in home contexts (see PTb 48).

ROBERT J. FOGELIN

1   Aspects of Quine's Naturalized Epistemology

Though there are clear anticipations in Quine's earlier writings of his commitment to a naturalized epistemology, its first full-dress presentation appears in his essay "Epistemology Naturalized." I will use this carefully plotted essay as the central guide to Quine's conception of naturalized epistemology, making excursions into earlier and later works where this proves useful.

Quine begins this essay declaring that "epistemology is concerned with the foundations of science" (EN 69). Oddly, this opening claim naturally suggests a project quite the opposite of the one he is about to endorse. To speak of the foundations of science suggests an attempt to find some way of validating science as a whole – that is, an attempt to find some way of basing science on something more primitive and more secure than science. This, however, gets Quine's conception of epistemology pretty much backwards. For Quine, epistemology does not provide an independent standpoint for validating empirical science; instead, empirical science provides the framework for understanding empirical knowledge, including the empirical knowledge provided by empirical science. This reversal represents the revolutionary core of Quine's conception of naturalized epistemology.

In order to explain why he adopts this revolutionary standpoint, Quine presents an elaborate comparison between his naturalistic approach to epistemology and what he takes to be the correct way of viewing the outcome of twentieth-century research in the foundations of mathematics:

Studies in the foundations of mathematics divide symmetrically into two sorts, conceptual and doctrinal. The conceptual studies are concerned with meaning, the doctrinal with truth. The conceptual studies are concerned

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with clarifying concepts by defining them, some in terms of others. The doctrinal studies are concerned with establishing laws by proving them, some on the basis of others. Ideally the obscurer concepts would be defined in terms of the clearer ones so as to maximize clarity, and the less obvious laws would be proved from the more obvious ones so as to maximize certainty. Ideally the definitions would generate all the concepts from clear and distinct ideas, and the proofs would generate all the theorems from self-evident truths. (EN 69–70)

Specifically, the logicist program was an attempt, on the conceptual side, to reduce the concepts of mathematics to concepts of logic, and then, on the doctrinal side, to exhibit all the truths of mathematics as truths of logic. Such a reduction, if carried through, would be a triumph for epistemology, for the truths of logic seem epistemically secure and the reduction of mathematics to logic would make the edifice of mathematics epistemically secure as well. Unfortunately, as Quine tells us, this goal has not been fully attained. He explains why in these words:

This particular outcome is in fact denied us, however, since mathematics reduces only to set theory and not to logic proper. Such reduction still enhances clarity, but only because of the interrelations that emerge and not because the end terms of the analysis are clearer than others. As for the end truths, the axioms of set theory, these have less obviousness and certainty to recommend them than do most of the mathematical theorems that we would derive from them....Reduction in the foundations of mathematics remains mathematically and philosophically fascinating, but it does not do what the epistemologist would like of it: it does not reveal the ground of mathematical knowledge, it does not show how mathematical certainty is possible. (EN 70)

The moral to be drawn seems clear: Though progress has been made on the conceptual side, advances in the foundations of mathematics have left what Quine calls the doctrinal task essentially unfulfilled. Quine expresses no hope that, with time, we might do better. Another, more interesting, moral seems to lie in back of this: In order to make progress on the conceptual issues, it may sometimes be necessary to abandon the doctrinal goal, or at least to make the doctrinal goal much more modest.

   Quine holds that a striking parallel obtains between the epistemology of mathematics and what he calls the "epistemology of natural

knowledge":

The parallel is as follows. Just as mathematics is to be reduced to logic, or logic and set theory, so natural knowledge is to be based somehow on sense experience. This means explaining the notion of body in sensory terms; here is the conceptual side. And it means justifying our knowledge of truths of nature in sensory terms; here is the doctrinal side of the bifurcation. (EN 71)

Quine cites David Hume as an example of a philosopher who addressed both issues: "[Hume's] handling of the conceptual side of the problem, the explanation of body in sensory terms, was bold and simple: he identified bodies outright with the sense impressions" (EN 71). It is a central part of Quine's project to replace this aspect of Hume's empiricism with a version of empiricism grounded in science. We will come back to this. First, it is important to note Quine's positive attitude to Hume's treatment of the doctrinal problem of empirical knowledge:

What then of the doctrinal side, the justification of our knowledge of truths about nature? Here, Hume despaired. By his identification of bodies with impressions he did succeed in construing some singular statements about bodies as indubitable truths, yes; as truths about impressions, directly known. But general statements, also singular statements about the future, gained no increment of certainty by being construed as about impressions. (EN 72)

I do not think Hume ever put things quite as Quine here states them, though in essence he has Hume right. Quine gets closer to Hume's actual worries in a number of other places, for example, in a later work, where he remarks,

The happy circumstance that nature has apparently persisted pretty well in her old ways right down to the present day...accounts for the continuing success by and large of induction....But all this is compatible with a major change, right now, in the course of nature, so I see no entitlement. Such a change would be contrary to our firmest scientific laws, but to argue thus is to argue inductively, begging the question. (RH 503)

Here is how Hume makes this point in the *Enquiry concerning Human Understanding*:

[A]ll inferences from experience suppose as their foundation, that the future will resemble the past....If there be any suspicion that the course of nature may change, and that the past may be no rule for the future, all experience

becomes useless, and can give rise to no inference or conclusion. It is impossible, therefore, that any arguments from experience can prove this resemblance of the past to the future; since all these arguments are founded on the supposition of that resemblance.[1]

Quine both notes Hume's despair at solving the problem of induction and clearly endorses it: "On the doctrinal side, I do not see that we are farther along today than where Hume left us. The Humean predicament is the human predicament" (EN 72).

This commitment to Humean skepticism with regard to induction is, I believe, a fundamental aspect of Quine's position. Moreover, it is more than bare acknowledgment of a puzzle that he finds impossible to solve. Not only does Quine accept Hume's skeptical argument, his response to it – his way of dealing with it – is strikingly Humean as well. This important connection is evident in Quine's notes for lectures he gave in 1946 on the history of philosophy. Michael Pakaluk was given access to these notes and published selections from them, with commentary, in a fine piece entitled "Quine's 1946 Lectures on Hume." These notes show that Quine had a subtle (and to my mind correct) understanding of the role that inductive skepticism played in Hume's own attempt to naturalize philosophy, or as Hume described his project in the subtitle to the *Treatise of Human Nature,* his *Attempt to Introduce the Experimental Method of Reasoning into Moral Subjects.*

Quine saw, for example, that Hume's skepticism concerning induction was independent of Hume's specific account of experience. Pakaluk remarks that "Quine is of course in great sympathy with Hume's conclusion that there can be no justification for our inductive practices." In fact, the passage he cites to support this says more: "Hume's negative doctrine is inevitable, I think, in any thoroughgoing empiricism; and it does not depend on the extreme or questionable feature of his particular underlying system of elements and psychology."[2] Pakaluk also cites a passage indicating that Quine recognized the full force of Hume's inductive skepticism: "The consequences [of Hume's inductive skepticism are] that there is no rational basis for prediction, even probable prediction; no rational basis for scientific law, even probable law."[3] As Quine understands Hume's skeptical argument, it was intended to establish more than mere probabilism or fallibilism with respect to causal inferences – a reading, with Quine, I take to be correct.

On a more subtle matter, Pakaluk notes that Quine recognized and applauded Hume's idea that induction, while incapable of general validation, can still be used to support particular inductive procedures. As Pakaluk puts it, "Experimental activity takes the form of applying general rules of scientific method – rules that are themselves established. So, then, sophisticated experimental reasoning arises from and can be resolved into many instances of simple conditioning."[4] In support of this, Pakaluk cites the following, somewhat telegraphic, notes:

Hume does not say why these [rules] are right; *impossible to defend*. But he says why *believed*. . . .

   And the rules are believed *by induction* because they work, or because derived from principles (uniformity of nature, etc.) which induction establishes – ultimately by sheer undeliberated conditioning.[5]

The notes Pakaluk cites show that Quine had no difficulty with an aspect of Hume's philosophy that many philosophers have found problematic. For some, there seems to be an inconsistency between Hume's unrestricted inductive skepticism and his tendency, throughout his writings, to treat certain causal claims as better founded than others. It seems hard to understand how some causal claims can get higher epistemic marks than others when they are all equally groundless. Seemingly worse, the *Treatise of Human Nature* actually contains a section entitled "Rules by Which to Judge of Causes and Effects."[6] Again, this seems inconsistent with Hume's inductive skepticism. To avoid this supposed inconsistency, some commentators have thought that the presence of these rules shows that Hume was not a skeptic concerning induction after all. Here Quine, perhaps because his instincts were deeply Humean, got things right. Although our inductive procedures are always under a catastrophic threat that cannot be removed, we can still, counting our blessings and hoping for the best, apply inductive procedures to, among other things, perfecting inductive procedures themselves.

   Pakaluk's essay ends by citing the following marvelous passage that gives Quine's overall assessment of the significance of Humean skepticism:

While it is a skepticism, it is not a doctrine of despair and inactivity. The same old drive to science and induction exists, and is applauded; but it is a natural drive, its methodology is ultimate and irreducible to deductive logic,

and the effort to find a foundation below and beyond science itself is vain
and doomed to failure. Skepticism as a counsel of despair and inaction exists
in Hume only with regard to this latter point.

Indeed, Hume pointed out quite eloquently that skepticism in this
sense, far from being antithetical to science, is decidedly in the scien-
tific spirit.[7] On the doctrinal side, Quine was simply an unapologetic
Humean.

   Rejecting the doctrinal demand that epistemology provide a justi-
fication or validation of empirical knowledge carries with it a num-
ber of advantages that Quine (and Hume before him) recognized and
exploited. For example, if we hold that it is the task of science to
provide a validation for empirical science, then it seems question-
begging (or improperly circular) to cite the results of empirical sci-
ence in doing so. This is one reason for thinking that appeals to
psychology are wholly out of place in doing epistemology. Under
the doctrinal demand for validation, the theory of natural knowl-
edge must perforce be purely conceptual. The situation is radically
changed once this doctrinal demand is dropped. Quine makes the
point this way:

[A] surrender of the epistemological burden to psychology is a move that was
disallowed in earlier times as circular reasoning. If the epistemologist's goal
is validation of the grounds of empirical science, he defeats his purpose by
using psychology or other empirical science in the validation. However, such
scruples against circularity have little point once we have stopped dreaming
of deducing science from observations. If we are out simply to understand
the link between observations and science, we are well advised to use any
available information, including that provided by the very science whose
link with observation we are seeking to understand. (EN 75–6)

Later Quine explains his understanding of the relationship between
epistemology and the natural sciences as follows:

There is thus a reciprocal containment, though containment in different
senses: epistemology in natural science and natural science in epistemology.
   This interplay is reminiscent again of the old threat of circularity, but it is
all right now that we have stopped dreaming of deducing science from sense
data. We are after an understanding of science as an institution or process
in the world, and we do not intend that understanding to be any better than
the science which is its object. (EN 83–4)

Yet even if the doctrinal project of trying to validate empirical knowledge is abandoned because of Humean inductive skepticism, as Quine notes, a motive might still remain for attempting to "*translate* science into logic and observation terms and set theory" (EN 76). Again, Quine thinks that a comparison with work in the foundations of mathematics is instructive. Though Quine does not actually say this, he could have put things the following way: Since sets lie in the domain of mathematics rather than logic, it would be *question-begging* to include them in a theory intended to establish the epistemic foundations of mathematics. On the other hand, if we abandon the quest for such epistemological foundations, then there is no reason why we cannot introduce sets into our theories and exploit them for whatever conceptual insight they might bring. Why not seek a similar advantage in epistemology, for if we could translate science into logic, observation terms, and set theory, "this would be a great epistemological achievement, for it would show all the rest of the concepts of science to be theoretically superfluous . . . [thus establishing] the essential innocence of physical concepts, by showing them to be theoretically dispensable" (EN 76). Here then, is another reason – this time a conceptual rather than a doctrinal reason – for pursuing a reductionist program with respect to science.

Quine acknowledges that such a program, if it could be carried through, would be of enormous significance. He also concedes that psychology would not be serviceable for carrying out such a program: "If psychology itself could deliver a truly translational reduction . . . , we should welcome it; but certainly it cannot, for certainly we did not grow up learning definitions of physicalistic language in terms of a prior language of set theory, logic and observation" (EN 76). Notice that Quine does not rule out the use of psychological terms in such reductive definitions on purely conceptual grounds, as others might. For Quine, a psychological account of language concerns the actual way in which human beings acquire certain concepts. He takes it to be a fact altogether obvious that human beings do not acquire physical concepts via translations from a language of a kind found, say, in Carnap's *Der logische Aufbau der Welt*. So, as Quine sees it, to the extent that reductionist accounts of empirical knowledge are still in, the use of psychology in epistemology is still out. What Quine needs, then, are good reasons for rejecting reductionism even in addressing *conceptual* issues. He does, after all, concede, or at least

seems to concede, that the reduction of mathematics to set theory and logic is to some extent conceptually illuminating. Why cannot a parallel reduction illuminate the nature of empirical knowledge? What is the difference between the two cases?

Quine's rejection of reductionism goes back to his famous early essay "Two Dogmas of Empiricism." There reductionism was anathematized as the second dogma of empiricism. In "Two Dogmas," the attack on reductionism was interlocked with the attack on the first dogma under consideration: the tenability of the analytic-synthetic distinction. Since other contributors to this volume examine "Two Dogmas" in detail, I will say little about this complex essay and the ideas that grew out of it. I will simply note that in this early essay Quine argued that, in general, theories are underdetermined by empirical data. That is, available evidence is always compatible with a plurality of competing theories. In a formula: Underdetermination yields indeterminacy. Over time, Quine's commitment to indeterminacy expanded to include indeterminacy of translation, indeterminacy of reference (ontological relativity), and indeterminacy of fact (infacticity). For Quine, it is clearly impossible to float a reductionist program on this sea of indeterminacy. Though it is clear that these distinctively Quinean ideas are constantly at work in "Epistemology Naturalized," he is content to point out that the strong reductionist program attempted by Carnap in the *Aufbau* has, in fact, failed – something Carnap acknowledged. In the end, Carnap had to renounce eliminative reductions in favor of weaker forms of reduction. With this, Quine claims, the final reason for excluding psychology from epistemology became moot:

To relax the demand for definition, and settle for a kind of reduction that does not eliminate, is to renounce the last remaining advantage that we supposed rational reconstruction to have over straight psychology; namely, the advantage of translational reduction. If all we hope for is a reconstruction that links science to experience in explicit ways short of translation, then it would seem more sensible to settle for psychology. Better to discover how science is in fact developed and learned than to fabricate a fictitious structure to a similar effect. (EN 78)

Here, then, in summary, is Quine's motivation for adopting a naturalized standpoint in epistemology. As long as we link the conceptual problem of understanding how we, as human beings, come to

have a reasonably adequate comprehension of the world around us with the doctrinal task of supplying a secure foundation for empirical knowledge, the prospect for progress on the conceptual front will be hopeless. There are two reasons for this. First, to Quine's satisfaction, Hume's skepticism with regard to induction shows that the doctrinal problem does not admit of a solution and thus will drag down anything linked to it. Second, as long as the doctrinal demand is in place, we are cut off, at the pain of begging the question, from exploiting a realm of highly reliable knowledge, namely, knowledge drawn from the empirical sciences. If we sever the doctrinal issue from the conceptual and just set it aside, then both hindrances are removed and a fair path opens before us. Nor, according to Quine, is there any hope that eliminative definitions can further a conceptual (as opposed to a doctrinal) program. First, as is generally acknowledged, the strict reductionist program has proven to be a failure. Beyond this, the existence of a wide range of deep indeterminacies dooms such an enterprise from the start. Given all this, a program that prohibits appeals to scientific considerations – in particular, psychological considerations – is no longer on the scene to concern us.

The opening pages of "Epistemology Naturalized" present a *general* defense of naturalized epistemology. They provide a justification for employing *any* information drawn from the natural sciences that can further our understanding of how human beings can form a reasonably accurate picture of the world they inhabit. For example, in various places Quine calls evolutionary theory to his aid, notably to offer an account – in outline at least – of why human beings and other animals naturally treat certain similarities as being more important than others. Among other things, seemingly shared and innate similarity standards seem to be needed for language acquisition and, indeed, for the application of all our everyday inductive procedures. Where do these similarity standards come from? What justifies their use? Quine responds to both questions by invoking Darwin. In "Natural Kinds," he puts things this way:

If people's innate spacing of qualities is a gene-linked trait, then the spacing that has made for the most successful inductions will have tended to predominate through natural selection. Creatures inveterately wrong in their inductions have a pathetic but praiseworthy tendency to die before reproducing their kind. (NK 126)

Quine realizes that this move is bound to raise the old charge of circularity and responds to it directly:

At this point let me say that I shall not be impressed by protests that I am using inductive generalizations, Darwin's and others, to justify induction, and thus reasoning in a circle. The reason I shall not be impressed by this is that my position is a naturalistic one; I see philosophy not as an *a priori* propaedeutic or groundwork for science, but as continuous with science. I see philosophy and science in the same boat – a boat which, to revert to Neurath's figure as I so often do, we can rebuild only at sea while staying afloat in it. There is no external vantage point, no first philosophy. All scientific findings, all scientific conjectures that are at present plausible, are therefore in my view as welcome for use in philosophy as elsewhere. (NK 126–7)

In an important respect, then, Quine's naturalized epistemology has an evolutionary component. That, however, is not the primary focus of his naturalized epistemology. It lies instead in his claim that "the stimulation of his sensory receptors is all the evidence anyone has to go on, ultimately, in arriving at his picture of the world" (EN 75). To mark this emphasis, I think, it is useful to draw a distinction between Quine's broad commitment to a naturalized epistemology, which can draw on any branch of science useful for his purposes, and his more narrow commitment to the project of showing how human beings, starting from (meager) sensory stimulation, can construct a reasonably good picture of the world around them. Since historically this research project has its roots in classical empiricism – primarily that of Hume – Quine's version of naturalized epistemology could reasonably be called naturalized empiricism.

But taking the stimulation of sensory receptors (or sensory surfaces) as the starting point admits of a number of developments. We can imagine a naturalized epistemologist basing his theory on the actual workings of perceptual mechanisms. In that case, epistemology would become a branch of physiological psychology. Quine sometimes writes in a way that suggests that the new naturalized epistemology should be pursued at just this level:

It studies a natural phenomenon, viz., a physical human subject. This human subject is accorded a certain experimentally controlled input – certain patterns of irradiation in assorted frequencies, for instance – and in the fullness of time the subject delivers as output a description of the three-dimensional

external world and its history. The relation between the meager input and torrential output is a relation that we are prompted to study for somewhat the same reasons that always prompted epistemology; namely, in order to see how evidence relates to theory. . . . (EN 82–3)

Passages of this sort suggest that Quine might be interested in going behind sensory surfaces in order to understand the "inner loop" of brain structures that link hits on sensory surfaces with linguistic expressions emitted, for example, by mouths. That would be epistemology naturalized with a vengeance. But despite his general commitment to physicalism, Quine does not opt for physiological psychology as his methodological standpoint. His writings contain no serious considerations of such things as "assorted frequencies," rods and cones, brain regions, or articulatory mechanisms. Instead, he adopts a behaviorist approach that explores the "outer loop" between publicly observable stimulatory *situations* and the publicly observable linguistic utterances that occur in such situations. The structures under consideration are not brain structures but sociolinguistic structures. As a product of his century, Quine replaces the way of ideas with the way of words; the way of things, though officially endorsed, is enshrined as a far-off ideal.

Just as a naturalized epistemology can take various forms depending on the empirical disciplines on which it relies, a naturalized empiricism can itself be developed in a variety of ways. Put perhaps too simply, Quine's version of naturalized empiricism, though deeply indebted to Hume, arose primarily from his critical reflections on the work of the logical empiricists, most notably Rudolph Carnap. Quine's most famous assault on logical empiricism is found in his essay "Two Dogmas of Empiricism." Despite what the title may seem to suggest, the aim of this essay is not to reject empiricism but instead to argue that the logical empiricists were not themselves sufficiently empirical in their outlook. Though it would scan less well, "Two Dogmas of Empiricism" would have been more accurately titled "Two Nonempiricist Dogmas Surviving in Logical Empiricism."

Broadly speaking, what Quine rejected as being antiempirical were the a priori conceptual methods embodied in the use of the analytic-synthetic distinction and, as noted earlier, the attempt to give reductive (or even quasi-reductive) analyses of empirical concepts. Quine, we might say, rejected the lingering apriorism of logical empiricism.

Yet even if he attacked logical empiricism in fundamental ways, he still accepted (or fell in with) some of its most important features. The underlying shared thought is that empirical science is a linguistic structure. Thus for Quine, as for the logical empiricists, the philosophy of empirical science is the study of the language of empirical science. Not only does this assumption underlie Quine's practice in dealing with empirical science, it is also a doctrine that he states explicitly in a number of places. For example, in "The Nature of Natural Knowledge," Quine remarks, "Science is a ponderous linguistic structure, fabricated of theoretical terms linked by fabricated hypotheses, and keyed to observable events here and there" (NNK 71). The central point of this passage is that science is, except for its observational content, a human *fabrication*. We will return to this point shortly. It is, however, worth pausing over the claim that science *is* a linguistic structure. This seems to be stronger than the claim that science *employs* linguistic structures. Bowling, for example, employs linguistic structures – score sheets – yet no one would say that bowling is a linguistic structure. In saying that science is a linguistic structure, Quine seems to be identifying science with its theories.

Treating science (or a scientific theory) as a linguistic structure is common ground between Quine and the logical empiricists. Among other things, this explains why the methods of logic are important to both and why Quine's constructive work often shadows Carnap's. Their differences have already been spelled out. For Quine, a naturalized empiricism was made possible by a set of interlocking considerations. By abandoning the project of validating science, Quine evades the charge of circularity. By rejecting the analytic-synthetic distinction, he undercuts the purely conceptual programs it made possible. By arguing for a wide range of indeterminacies, he deprives the logical empiricists of their chief employment: the production of reductive analyses. For Quine, these changes yield a form of empiricism more empirical than logical empiricism itself.

In summary, depending on the context, Quine's naturalism with respect to epistemology operates on at least three levels:

- *Naturalized epistemology*. This is broad view that epistemological questions are factual questions to be addressed using the results and methods of empirical science.

- *Naturalized empiricism*. The traditional empiricist view that all knowledge of the world around us is derived from information provided by the senses is transformed into a claim about hits on sensory surfaces. (Here we could also speak of empiricism *externalized*.)
- *Naturalized logical empiricism*. This is the logical empiricist's view that the philosophy of empirical science takes as its subject matter the language of empirical science, with the difference that this enterprise is taken to be itself empirical rather than merely conceptual and is pursued in a holist rather that in a reductionist manner.

Returning to "Epistemology Naturalized," we might note that the transition from a general defense of naturalized epistemology to a defense of his own naturalized version of logical empiricism occurs in a rather subtle fashion. Having recorded the failure of reductionist programs in both their strong and weak forms, Quine pauses to reflect on the reasons for this failure. One possibility, he tells us, is that "the implications of a typical statement about bodies are too complex for finite axiomatization, however lengthy" (EN 79). This is the view that reductionism, though theoretically sound, is in fact impossible to carry through to completion. Rejecting this, Quine offers his own diagnosis of the failure of reductionist programs of the kind put forward by logical empiricists:

I have a different explanation. It is that the typical statement about bodies has no fund of experiential implications it can call its own. A substantial mass of theory, taken together, will commonly have experiential implications; this is how we make verifiable predictions. We may not be able to explain why we arrive at theories which make successful predictions, but we do arrive at such theories. (EN 79)

In this passage, Quine returns to one of the fundamental themes of "Two Dogmas," but now expressed with more modesty. In "Two Dogmas," Quine famously declared,

The unit of empirical significance is the whole of science....Any statement can be held true come what may, if we make drastic enough adjustments....Conversely...no statement is immune to revision. (TDEb 42–3)

Reflecting on this claim forty years after it was published, Quine tells us that he regrets what he calls his "needlessly strong statement of holism" (TDR 268). In "Five Milestones of Empiricism," he makes the point more strongly:

It is an uninteresting legalism...to think of our scientific system of the world as involved e*n bloc* in every prediction. More modest chunks suffice, and so may be ascribed their independent empirical meaning, nearly enough, since some vagueness in meaning must be allowed for in any event. (FME 71)

In fact, not only does Quine's extreme holism become muted in his later writings, the radical revisability thesis associated with it has become muted as well. In a video discussion concerning Quine's naturalized epistemology, I had the opportunity to suggest to Quine that this strong version of revisability is rather hard to take, especially when applied to laws of logic. Quine responded as follows: "Well, I think I rather agree. I think nowadays it seems to me at best an uninteresting legalism."[8] The expression "uninteresting legalism" is Quine's marker for earlier views that he has come to view as – if not altogether wrong, and perhaps even in some Pickwickian sense correct – needlessly extreme. Elsewhere[9] I have suggested that this moderation in what were originally signature features of Quine's position reflects his growing commitment to a naturalistic standpoint. Naturalized epistemology makes one more modest – or at least it ought to. But even if Quine's growing commitment to naturalism forced revisions in some of his earlier, more exuberant views, in "Epistemology Naturalized" many of the Quinean leading characters are still on stage, including indeterminacy of translation, indeterminacy of reference (or ontological relativity), and observation sentences. I will not attempt to explain these features of Quine's positions in detail. Others in this volume do so. Noting only their general features, I wish to examine how they relate to Quine's naturalistic commitments.

   Let us start with indeterminacy of translation. Unlike some of the technical discussions elsewhere, in "Epistemology Naturalized" the exposition and defense of the doctrine of the indeterminacy of translation is quite straightforward. It turns on combining a simple point of logic with the notion of multiple forms of revision. The point of logic is this: Given a valid argument with a false conclusion, we may

conclude that at least one of its premises is false, but on the basis of this information, we are not, in general, able to determine *which* premise is false. The possibility of multiple possible revisions arises for the following reason. If a valid argument has a false conclusion, then at least one of its premises stands in need of revision, but given multiple premises, this generates the possibility of multiple possible revisions. The next step is to extend these reflections to theories – evidently accepting something like the hypothetico-*deductive* conception of science in doing so.

Sometimes . . . an experience implied by a theory fails to come off; and then, ideally, we declare the theory false. But the failure falsifies only a block of theory as a whole, a conjunction of many statements. The failure shows that one or more of those statements is false, but it does not show which. The predicted experiences, true and false, are not implied by any one of the component statements of the theory rather than another. The component statements simply do not have empirical meaning . . . but a sufficiently inclusive portion of the theory does. (EN 79)

We then reach the doctrine of the indeterminacy of translation in a somewhat curious way. Here Quine does not begin by speaking generally about translating one language into another; instead, he considers the special case of translating a *theory* from one language into another.

[I]t is to be expected that many different ways of translating the component sentences, essentially different individually, would deliver the same empirical implications for the theory as a whole; deviations in the translation of one component sentence could be compensated for in the translation of another component sentence. Insofar, there can be no ground for saying which of two glaringly unlike translations of individual sentences is right. (EN 80)

As stated, this passage only concerns the translation of theories from one natural language to another and thus does not seem to apply broadly to language. The transition to a more encompassing doctrine of the indeterminacy of translation occurs in these sentences:

[I]f we recognize with Duhem that theoretical sentences have their evidence not as single sentences but only as larger blocks of theory, then the indeterminacy of translation of theoretical sentences is the natural conclusion. *And most sentences, apart from observation sentences, are theoretical.* (EN 80–1, emphasis added)

Setting aside observation sentences, which we will discuss later, language itself is presented as a theoretical structure, or perhaps better, given Quine's retreat from extreme holism, as a set of theoretical structures. With this, we arrive at the strong version of the indeterminacy of translation associated with Quine.

When we stand back and examine these passages from a naturalistic perspective, it is hard not to be struck by their a priori character. What, for example, are we to make of the claim that "it is to be expected that many different ways of translating the component sentences, essentially different individually, would deliver the same empirical implications for the theory as a whole"? What precisely would lead us to expect this? Well, if linguists routinely returned from their fieldwork with *radically* different manuals translating the same native language, then the matter of radical translation would arise in the context of empirical inquiry. Yet nothing as radical as this actually happens. There are, admittedly, disagreements among linguists concerning proper translations of a target language, but on nothing like the scale one would expect given Quine's doctrine of the indeterminacy of translation. The facts go against Quine, and that, from a naturalistic standpoint, is something that should matter.

From a naturalistic standpoint, a more interesting question is this: Given that endlessly many equally adequate but incompatible translations are abstractly possible, how are we to explain the broad convergence in translation? Quine, in fact, has interesting and important things to say on this matter, for example, in his essay "Natural Kinds." There he notes that our inductive procedures, both common and theoretical, depend on treating certain similarities as salient while ignoring others. As noted earlier, Quine invokes Darwinian evolution as an account of the origin of these similarity factors. From a naturalistic standpoint, this is the way to go. Why then does Quine stress the doctrine of the indeterminacy of translation instead of dismissing it as no more than an "uninteresting legalism"? Quine could, after all, have argued in the following way: Given their own commitments – and taking them seriously – the logical empiricists should have recognized that theory is always underdetermined by evidence. If the logical empiricists had simply thought this through, they would have come to recognize that their position implied a wide range of indeterminacies, including indeterminacy of translation. Quine does, in fact, argue in just this way. What is peculiar from

a naturalistic standpoint is that Quine actually embraces indeterminacy of translation as a substantive doctrine of his own.

A similar concern arises with respect to the doctrine perhaps most dear to Quine's heart, which he variously calls ontological relativity, inscrutability of reference, and indeterminacy of reference. Since I am primarily interested in the broad question of the relationship between Quine's views concerning ontology and his naturalistic commitments, I will sketch his position concerning ontology in broad strokes. In "On What There Is," Quine asked, "What is there?" and gave a startlingly simple answer: "Everything" (WTI 1). Quine, of course, was not saying that everything that could exist does exist. He was offering the empty answer "There is what there is" as preface to raising a different question: "[How can we determine] what the ontological commitments of a theory are?"

Roughly, when we talk about things, we seem to commit ourselves to the existence of those things we are talking about. The most transparent way of doing this is to make an explicit claim that something exists – for example, that condors still exist in coastal California. More formally, we can put this remark about condors this way: There is at least one *x* such that *x* is a condor and *x* is in coastal California. This phrasing makes it transparent that employing this sentence commits one to the existence of condors and of coastal California. To reveal the ontological commitments of sentences that do not speak explicitly of something existing, we try to find a way to translate them into a form that does. Thus, 'Some condors are female' becomes 'There exists an *x* such that *x* is a condor and *x* is a female', thus showing that the original sentence involved a commitment to the existence of both condors and females. Reflections along these lines, developed, needless to say, with more rigor and subtlety, led Quine to formulate his criterion of ontological commitment as follows:

To be assumed as an entity is, purely and simply, to be reckoned as the value of a variable. (WTI 13)

It seems, however, that there is a more obvious – more direct – way of referring to something in the world and thereby getting committed to its existence: the use of a proper name. The standard way of talking about Nixon is to use his name, to wit, 'Nixon'. Why not also take the use of proper names as indicators of ontological commitment? Quine's answer is that doing so raises difficult problems

with fictitious entities. What, for example, are we to make of talk about Pegasus – in particular, what are the ontological commitments involved in the claim that Pegasus is not actual? If we take the use of a proper name as a mark of ontological commitment, then in saying that Pegasus does not exist we commit ourselves to the existence of the very thing whose existence we are attempting to deny. A bad result. One way out of this difficulty, according to Quine, is to find a way of translating 'Pegasus is not actual' into the canonical notion of quantifier logic in a way that avoids commitment to an entity named 'Pegasus'. There are a variety of ways of doing this, but the shortest, most direct method is to treat 'Pegasus is not actual' as equivalent to 'Nothing pegasizes' (see WTI 7 ff.). If that or something like it is right, then it has been shown that the use of a proper name does not *eo ipso* commit one to the existence of an entity corresponding to it. This is shown by the fact that there is a way of saying the same thing that carries no such commitment. As Quine puts it, "We need no longer labor under the delusion that the meaningfulness of a statement containing a singular term presupposes an entity named by the term. A singular term need not name to be significant" (WTI 8–9). Quine's treatment of proper names is, of course, reminiscent of Russell's analysis of definite descriptions. Where Quine worried about the reference of such fictitious entities as Pegasus, Russell was concerned about the reference of such empty descriptions as 'the present King of France'. In each case, a contextual definition is presented in which a term that seems to refer to a problematic entity is made to disappear, and with this an ontological bother is defused.

The project of defusing ontology, initiated in "On What There Is," was given a radical development twenty years later in Quine's essay "Ontological Relativity." In *Word and Object*, Quine put forward arguments in behalf of the indeterminacy of *translation*; in "Ontological Relativity," he pressed things further by arguing for the indeterminacy of reference as well. Invoking gavagai the rabbit, gavagai the undetached rabbit part, etc., used in *Word and Object*, Quine tells us,

It is philosophically interesting...that what is indeterminate in this... example is not just meaning, but extension; reference. My remarks on indeterminacy began as a challenge to likeness of meaning. . . . Reference, extension, has been the firm thing; meaning, intension, the infirm. The indeterminacy of translation now confronting us, however, cuts across

extension and intension alike. The terms "rabbit," "undetached rabbit part," and "rabbit stage" differ not only in meaning; they are true of different things. Reference itself proves behaviorally inscrutable. (OR 34–5)

In effect, Quine has moved from the ontological pluralism of "On What There Is" to embrace something much stronger: ontological inscrutability, or ontological indeterminacy.

In "Ontological Relativity," Quine subsequently presents a further argument for ontological indeterminacy that reappears, in various forms, throughout his later writings. This argument invokes what Quine calls proxy functions. Unfortunately, the discussion of proxy functions in "Ontological Relativity" relies on technical matters not accessible to the nonexpert. In his later writings, however, he introduced examples of proxy functions that are more easily grasped. In his last book, *From Stimulus to Science*, Quine defines proxy functions as functions that are "one-to-one reinterpretations of objective reference. They leave the truth values of the sentences undisturbed" (FSS 72). To explain this, he offers an elegant example: a proxy function that he calls a cosmic complement. The cosmic complement of Quine, for example, is the whole universe except for a hole where Quine is.[10] Using his favorite objects of reference, rabbits, Quine asks us to imagine a world where our referring terms would shift their standard reference to the reference of their cosmic complements.

The word 'rabbit' would now denote not each rabbit, but the cosmic complement of each, and the predicate 'furry' would now denote not each furry thing but the cosmic complement of each. Saying that rabbits are furry would thus be reinterpreted as saying that complements-of-rabbits are complements of furry things, with 'complements-of-rabbits' and 'complements-of-furry' seen as atomic predicates. The two sentences are obviously equivalent. (FSS 71)

Given the simple logical result that the two sentences are equivalent, it follows that anything that counts as confirming or disconfirming evidence of the one will equally count as confirming or disconfirming evidence of the other. Evidence, then, will be completely neutral with respect to which ontology is the correct one. From this example and others like it, in *From Stimulus to Science* Quine proceeds to draw a very strong conclusion:

I conclude from [the indeterminacy of reference as shown by proxy functions] that what matters for any object, concrete or abstract, is not what they are

but what they contribute to our overall theory of the world as neutral modes in its logical structure. (FSS 74–5)

In a section of *Pursuit of Truth* entitled "Ontology Defused," Quine makes the same point this way: "A lesson of proxy functions is that our ontology, like grammar, is part of our own conceptual contribution to our theory of the world" (PTb 36).

These passages – and there are many more like them throughout Quine's writings – capture one of his central ideas: Ontological commitments are not only posits but, as we might put it, *insubstantial* posits. They are nothing more than neutral nodes in the logic of a theoretical structure. In various places – for example, in the opening pages of "Ontological Relativity" – Quine suggests that this view of ontology is forced on us when we adopt a naturalistic standpoint. It is, however, far from obvious why this is so. For instance, consider the following two theses:

1. RP: Independent of us, the world contains all sorts of objects; which ones we take seriously is a function of our theoretical concerns.
2. ARP: Objects are posits (reifications, fictions) that we introduce as part of our theoretical activities.

Both views are pluralistic in acknowledging the legitimacy of using a wide range of referring terms as part of our theoretical apparatus. The first, however, expresses a realist position (RP), the second an antirealist position (ARP). Though Quine has no qualms about countenancing peculiar objects, such as large discontinuous particulars as the referents of mass nouns, deep down, throughout his career, he was committed to some form of the second, antirealist, thesis.

The first point to make is that the possibility of constructing proxy functions (e.g., cosmic complements) does not determine a choice between RP and ARP. The realist will say that Quine's cosmic complement (unlike Pegasus) is an object that really does exist – it is simply not a particularly interesting object from a scientific standpoint. Second, and more importantly, there seems to be no way of choosing between RP and ARP on the basis of empirical content. If that is so, then the thoroughgoing naturalist in philosophy should set aside the debate between realism and antirealism as unresolvable

and turn his attention to issues where empirical evidence makes a difference.

Quine's commitment to a version of antirealism is connected with another central feature of his position: his distinctive account of observation sentences. At first, observation sentences were primarily intended as a replacement for sense-data statements (and the like) favored by the logical empiricists. In "Epistemology Naturalized," Quine defines an observation sentence as "one on which all speakers of the language give the same verdict when given the same concurrent stimulation" (EN 86–7). He then goes on to say,

There is generally no subjectivity in the phrasing of observation sentences, as we are now conceiving them; they will usually be about bodies. Since the distinguishing trait of an observation sentence is intersubjective agreement under agreeing stimulation, a corporeal subject matter is likelier than not. (EN 87)

A bit later he says, "The observation sentence, situated at the sensory periphery of the body scientific, is the minimal verifiable aggregate; it has an empirical content all its own and wears it on its sleeve" (EN 89). Treating observation sentences as publicly observable entities generated under publicly observable conditions is an understandable line to take for one committed to naturalizing epistemology.

As regards ontology, there seems to be nothing in this account of observation sentences that concerns ontological relativity one way or another. Both RP and ARP can equally incorporate this notion of an observation sentence as their starting point. Later, however, Quine added, or at least made explicit, a further aspect to his account of observation sentences that did have strong ontological implications: Conceptually, observation sentences must be taken as unsegmented wholes. They must be taken, as Quine puts it, holophrastically. *Pursuit of Truth* provides a good guide to these matters. In the opening chapter of this work, Quine is concerned with a traditional problem for empiricism, namely, that observation sentences seem themselves to be theory-laden and thus cannot serve as an independent basis for theory evaluation. Quine's ingenious response is that observation sentences are not theory-laden because they contain no internal conceptual structure at all. They are, in their way, brute responses to the world a person encounters, responses later shaped by social reinforcement yielding the command of a highly structured

language. For parallel reasons, observation sentences also serve as the ultimate checkpoints for science. Impressively, they do both of these jobs without containing any theoretical content of their own.

How do these reflections bear upon ontology? For Quine, one advantage claimed for the holophrastic treatment of observation sentences is that "we can then study the acquisition and use of observation sentences without prejudging what objects, if any, the component words are meant to refer to. We are thus freed to speculate on the nature of reification and its utility for scientific theory" (PTb 8). Very roughly, the reason for this is as follows: Without internal structure, a sentence cannot have a subject-predicate structure; without a subject, there is no place for pronominalization (i.e., no open place to quantify over); and without that, there is no variable to bind and therefore no way of producing an ontological commitment. If all this is correct, then ontological commitments do not have their source in observation sentences but must occur someplace further down the linguistic stream.

I think that we are now in a position to see why Quine thought that his holophrastic account of observation sentences yields the doctrine of ontological indeterminacy. If we assume, as Quine did, that observation sentences are the sole source of content, and we further assume, along with Quine, that the content of an observation sentence is wholly nontheoretical, then we arrive at the result that everything theoretical, including ontological commitments, is introduced by us. It is in this way that objects become treated as posits, as reifications, and sometimes as fictions. Ontological indeterminacy emerges because a variety of different systems of theoretical posits will always be possible relative to a set of observation sentences, however large.

In a number of places, Quine draws this conclusion explicitly. For example, in his "Reply to Stroud," Quine invokes the notion of reinterpreting a theory by replacing functions with truth-preserving proxies. He then draws the following moral:

The structure of our theory of the world will remain unchanged. *Even its links to observational evidence will remain undisturbed, for the observation sentences are conditioned holophrastically to stimulations, irrespective of any shuffling of objective reference. Nothing detectable has happened*. Save the structure and you save all.

Russell urged the all-importance of structure in his *Analysis of Matter*, and Ramsey made the point more rigorously with his Ramsey sentences. Russell and Ramsey were urging the indifference only of theoretical objects, as against observational ones. *Once we take observation sentences holophrastically, however, reference and objects generally go theoretical. The indifference or inscrutability of ontology comes to apply across the board.* (RS 473, emphasis added)

An even more striking passage to the same effect occurs in the abstract of "Naturalism; Or, Living within One's Means":

[O]bservation sentences themselves, like ape cries and bird calls, are in holophrastic association with ranges of neural intake. Denotation of determinate objects figures neither in this association nor in deducing the categorical from the scientific hypotheses. Hence the indeterminacy of reference; ontology is purely auxiliary to the structure of theory. Truth, however, is seen still as transcendent at least in this sense: we say of a superseded scientific theory not that it ceased to be true, but that it is found to have been false. (NLWM 251)

In order to see the force of these remarks, it is essential to take Quine's reference to "ape cries and bird calls" quite literally. Apes perhaps and birds more certainly do not advance beyond these primitive vocalizations to develop a complex articulated linguistic structure of the kind that human beings possess, but if Quine is right, our starting point is precisely the same as theirs.

Quine's holophrastic interpretation of observation sentences is an innovation with enormous systematic power, for if it can be made good, we would then have the resources to resolve a wide range of difficulties that have previously plagued empiricism. But is this conception of observation sentences itself tenable? Proving that it is would require showing how, starting from unarticulated observation sentences, a route can be established leading to highly articulated theoretical sentences. It is, however, far from clear that Quine made the case that this project can, in principle, be carried out successfully. A difficulty seems to arise in the very first step in the road from observation sentences to theoretical sentences. Quine's first move from the homogeneity of observation sentences to the heterogeneity of theoretical sentences involves the seemingly unproblematic move of forming a conjunction of observation sentences. But does it even make sense to speak of conjoining observation sentences where

this would be on a par with conjoining bird calls. If conjoining just means concatenating, then it would be possible to conjoin two bird calls, getting a longer bird call or a repetition of a bird call. But an 'and' signifying concatenation is not the kind of 'and' that is needed, and it is unclear how, at this level, anything more than this could be supplied. Although the words that occur in observation sentences holophrastically conceived may reappear later in sentences that are genuine truth-bearers, it does not seem that observation sentences are truth-bearers at all. If they are not, then they simply are not the right sorts of things to serve as an epistemological starting point.[11]

Given Quine's avowed commitment to a naturalistic standpoint, another perplexing feature of his position is this: His attempt to trace a pathway from stimulation to observation sentences and ultimately to theoretical sentences was carried out with virtually no concern for the psychological reality of the process he claimed to be examining. The reason for this, I suggest, is that Quine's reflections were not driven or constrained by the exigencies of empirical research in psychology; his primary concerns were internal to a particular philosophical tradition, specifically, logical empiricism. This becomes clear in his essay "In Praise of Observation Sentences," where Quine enumerated the advantages of observation sentences holophrastically understood:

1. They provide an observational starting point that is not theory-laden (POS 110).
2. They are the "infant's entry into language" (POS 110).
3. They are the "radical translator's way into language" (POS 110).
4. They are "vehicles of evidence for our knowledge of the external world" (POS 110).
5. They provide a way of dealing with the supposed incommensurability of competing theories (POS 111).
6. They are primitive sources of idioms of belief (POS 112).
7. They play a central role in diffusing ontology (POS 112).

If we judge the character of a theory by the problems it is intended to solve, then, with the possible exception of (2) and (3), none of the items on this list is closely associated with experimental activities. Even with respect to (2) and (3), Quine showed little interest in pertinent experimental results. Quine's goal in naturalizing epistemology

was to make empiricism itself more empirical by defending the legitimacy of treating it as a branch of empirical science. He did not, however, pay much attention to the experimental aspects of the program that he championed. Instead, his reflections are pitched at a very high speculative level. He defended the place of speculation in "The Nature of Natural Knowledge" in these words:

[A] speculative approach . . . seems required to begin with, in order to isolate just the factual questions that bear on our purposes. For our objective here is still philosophical – a better understanding of the relations between evidence and scientific theory. Moreover, the way to this objective requires consideration of linguistics and logic along with psychology. This is why the speculative phase has to precede, for the most part, the formulation of relevant questions to be posed to the experimental psychologist. (NNK 78)

This may seem innocent enough, but someone with strong naturalistic commitments might balk at the thought that relevant questions can be antecedently formulated and then handed over to the experimentalist for resolution. Opposed to this is the doctrine that genuine questions arise within the context of ongoing inquiry in reaction to the vicissitudes it encounters – a standpoint championed by C. S. Peirce and later by John Dewey. If this is correct, then it is contrary to the naturalistic spirit to defer reference to experimental data in the way that the passage just cited suggests. In becoming naturalized, epistemology has to get its hands dirty sooner that Quine seemed to acknowledge.

   Interestingly, Quine at times did adopt something like a Peircean theory of inquiry – particularly as grounds for dismissing traditional epistemological concerns.[12] This emerged in a video panel discussion with Quine in which I participated (along with Martin Davies, Paul Horwich, and Rudolph Fara). Davies asked Quine how he would deal with the Cartesian challenge that he (Quine) might, at the very moment, be asleep and dreaming. Quine responded as follows:

I am ruling the dream hypothesis out in the sense that I dismiss it as very unlikely. And I think that this is the mood in which we do our thinking generally, that there is plenty to worry about in the way of things that could interfere with our hypotheses, and show that we are wrong in them. And we worry about the likeliest ones [that is, those most likely to give us trouble].[13]

I got into the conversation, and the exchange continued as follows:

Fogelin: Is it fair to say that your answer is grounded in a kind of theory of
         inquiry . . . [a theory of] what honest inquiry looks like?
Quine: Very good, yes.
Fogelin: And these [Cartesian] doubts simply don't have a place in honest
         inquiry?
Quine: Yes.[14]

Quine's refusal to be concerned with problems that are not empiri-
cally motivated emerged again with his quick dismissal of the brain-
in-the-vat problem.

My attitude toward [this] is the same as my attitude toward more common-
place situations where the thing is not quite so unthinkable from a naïve
point of view. Namely, I would think in terms of naturalistic plausibility.
What we know, or what we firmly believe, . . . is that it would really be an
implausible achievement, at this stage anyway, to rig up such a brain. And
so I don't think I am one [i.e., a brain in a vat].[15]

The clear implication of this passage is that naturalized epistemol-
ogists, like scientists generally, will only concern themselves with
matters having a suitably high degree of "naturalistic plausibility."
It is, of course, entirely possible that with the advance of technology
brains could be rigged up in the way envisaged in skeptical scenarios.
They might be featured in science museums. In that world, Quine's
response would be inadequate. And there is some chance that we are
in such a world. Quine would not have denied this. Furthermore, he
did not, as others have, attempt to find some purely conceptual argu-
ment (or transcendental argument) intended to rule this possibility
out. Such arguments have no place in the naturalized epistemologi-
cal arsenal.

    But, alas, Quine was not always as fully committed to a natural-
istic standpoint as he might have been. This comes out in two ways.
First, in the development of his own views he tended to move at
a very high level of theoretical generality, rarely touching down at
empirical checkpoints. He often seemed more concerned with the re-
lationship between his philosophical position and the philosophical
positions of others than with the relationship between his position
and the data needed to support it. As a result, contrary to Quine's
stated intentions, his theory sometimes looks more like an "a priori

propaedeutic or groundwork for science" (NK 126) than like science itself. Second, though he criticized others for pressing possibilities having little naturalistic plausibility, in doing ontology, at least, he seemed to traffic in them himself. For example, Quine drew very strong conclusions from the bare possibility of radically different manuals of translation and the bare possibility of radically different ontologies. Such possibilities will not (indeed, should not) concern the linguist working in the field. Acknowledging this, Quine remarked, "But I am making a philosophical point" (OR 34). From a naturalistic standpoint, such philosophical points seem to be a relapse into a previous, prenaturalistic way of doing philosophy.

These remarks are not intended as a criticism of the program of naturalized epistemology – they are only aimed at Quine's execution of this program, which to my mind was not thoroughgoing enough. They leave Quine's fundamental idea untouched, namely, that it is not the business of epistemology to validate science and that therefore there is nothing question-begging in approaching epistemology from a scientific standpoint. It simply remains to be seen what success such an enterprise will have when confronted with the data it is intended to accommodate.[16]

NOTES

1. D. Hume, *Enquiries concerning the Human Understanding and concerning the Principles of Morals*, 3rd ed. (Oxford: Clarendon Press, 1975), 37–8.
2. M. Pakaluk, "Quine's 1946 Lectures on Hume," *Journal of the History of Philosophy* 27 (1989): 453.
3. Ibid., 457.
4. Ibid., 455.
5. Ibid.
6. D. Hume, *A Treatise of Human Nature*, 3rd ed. (Oxford: Oxford University Press, 1978), 173 ff.
7. See Pakaluk, "Quine's 1946 Lecture on Hume," 459.
8. R. Fara, *In Conversation with V. W. Quine: The Fogelin Panel* (London: Philosophical International, 1994).
9. See R. Fogelin, "Quine's Limited Naturalism," *Journal of Philosophy* 11 (1997): 543–63.
10. This is a very strange object indeed. For example, though we might be able to say where Quine is, about the only thing we can say about

      Quine's cosmic complement is that it is the volume of the entire universe minus Quine's volume, and so on. It is hard to think of a scientific use for such an entity, but still it is well defined.

11. Expressions that are not truth-bearers are sometimes conjoined – for example, questions – but these at least have a propositional content, which presumably is wholly lacking in observation sentences.

12. See Fogelin, "Quine's Limited Naturalism."

13. Fara, *In Conversation with W. V. Quine*.

14. Ibid.

15. Ibid.

16. Because of the limitation of space, I have not commented on the secondary literature concerning Quine. I have, however, profited from it, particularly from the writings of Roger F. Gibson and Christopher Hookway.

2   Quine on the Intelligibility and Relevance of Analyticity

W. V. O. Quine 's "Two Dogmas of Empiricism" (TDEa 20–43) is perhaps the most famous paper in twentieth-century philosophy. Certainly, it is the most widely reproduced of Quine's works. Even if it had been ignored, it would still hold a special place for Quine, for to a large extent Quine has defined himself and his philosophy in opposition to Rudolf Carnap's separation of our scientific claims into the analytic and the synthetic as well as in opposition to any theory of knowledge, such as Carnap's, in which the analytic-synthetic distinction figures so prominently. "Two Dogmas" is central to Quine's work if only because it contains his first sustained public attack on analyticity. Moreover, the paper's last section is the first, and one of the most systematic, of his sketches of an alternative epistemology.

Given the amount of attention that "Two Dogmas" has had and the variety of its readers, it is hardly surprising that its arguments have been variously understood. It has been called an attack on empiricism or on reductionism. It has been said to embrace a behaviorism of an antitheoretical sort. The fault that it finds in analyticity is sometimes said to lie in the circularity of the definitions for it. None of this seems to me to be very likely. Sometimes the paper is said to say exactly what Quine was saying nearly fifty years later. This also seems unlikely, for Quine continued to develop and modify his arguments and to reassess their relative importance. Nor is it surprising that he would have. Indeed, the rich body of Quine's later writings and discussions can be a hindrance as well as a help in understanding what some earlier passage may have meant, either to Quine or to his readers. Such evidence should be used, of course, but with caution. It is not my intention here to focus exclusively on "Two Dogmas." Rather, I shall step back a bit in order to come

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fully to grips with the central thrust of Quine's argument against analyticity. This has a number of distinct parts. We must identify that central argument and the demands Quine places on any satisfactory scientific concept, evaluate the legitimacy of these demands, and assay what it would take to meet the reasonable part thereof. In addition, I shall try to determine to what extent the situation regarding analyticity may have changed in the last fifty years – either in Quine's arguments, in his assessments of their relative merits, or in the responses that can be made to them. This will allow us to reflect on the prospects both for an analytic-synthetic distinction and for epistemologies defined by it or in opposition to it.

1. INTELLIGIBILITY

Quine begins his attack on analyticity by distinguishing two classes of analytic claims. The first comprises the logical truths, that is, those that remain true under all reinterpretations of their nonlogical terms. For example, 'All white horses are white' is a logical truth since every reinterpretation of its nonlogical terms, 'white' and 'horses', yields another truth such as 'All black ravens are black'. The second class of analytic claims comprises such truths as 'No bachelor is married'. These, which Quine would later call truths of essential predication (see CLT 402 ff), become logical truths when synonyms are substituted for synonyms. For example, assuming 'unmarried man' and 'bachelor' are synonymous, substituting the former for the latter in 'No bachelor is married' yields the logical truth 'No unmarried man is married'. This reduction of the class of essential predications to the class of logical truths would do as a general characterization of this second class of analytic sentences but for the fact that synonymy is itself, according to Quine, just as obscure and in the same way as analyticity. We might search in turn for a general criterion of synonymy in terms of definition, meaning, necessity, and the like. Quine finds these, too, unilluminating and suggests that they can be understood only by appeal to analyticity.

This would obviously be circular, which suggests that what Quine objects to about this whole family of terms is the circularity of such attempts at clarification. This suggestion is further reinforced by the fact that Quine explicitly makes the charge of circularity when he summarizes a reprise of this argument a few years later (see CLT 403).

But all demands for further clarification, if pushed far enough, have nowhere to turn but to terms earlier in the series; the complaint of circularity could be lodged everywhere. So Quine's worry must lie, not with circularity, but with some defect of each of the terms in this sequence. Quine does not say so explicitly here, but his basic demand is for behavioral criteria.[1] This is a demand about which we need to ask several questions: What precisely is the demand? Is it legitimate, and if so, to what extent? What would be required to satisfy the legitimate portion of the demand? And can this be done? Quine's demand for behavioral criteria is best understood as an instance of the many empiricist criteria of significance. Empiricists in the Viennese tradition had long sought to rule out metaphysics. Their strategy was to presume an observational basis and require some connection (in the form of observational criteria, correspondence rules, and the like) between that observational basis and the claims in question. Claims lacking the appropriate connection were said to be without empirical content. This is just what Quine says about the division of truths into the analytic and synthetic: it lacks empirical content and is, hence, a metaphysical article of faith. Since his favored observational basis is observable behavior, this is certainly methodological behaviorism, but by itself it need not rule out theoretical episodes of a fully mental sort. Quine does not spell out in any great detail what sort of connection he would approve between behavior and admissible concepts. Given the parallel with the demands of Carnap, however, we are left to surmise that the tie would permit theoretical concepts that are not fully expressible in the observational/behavioral framework.

There is an irony in all this, besides the obvious one that Quine is pushing against Carnap the very demands that Carnap had pushed against the metaphysicians. This second irony is that many philosophers such as Hempel came to reject analyticity because they thought it could not meet Quine's demands and at the very same time came to reject the whole idea of empiricist criteria of significance.[2] On this issue, however, Quine never wavered. Carnap's demand for empirical significance is among the features of Carnap's philosophy that Quine most enthusiastically and enduringly approves.

But is Quine's demand legitimate, however much it may be modeled on Carnap's? Quine thinks that it is because he thinks that the theory of language must be an empirical theory. Putting the matter this way, though, begs the question against Carnap. Carnap would

and did quickly agree that his own talk of analyticity had no empirical content. But Carnap distinguished the pure from the descriptive study of language. The latter is empirical linguistics, and here criteria of empirical significance are perfectly appropriate. The former Carnap thinks of as philosophic and hence as analytic. Since the pure study of language is not construed as empirical, it neither needs empirical content nor can have any. Quine, of course, makes no such differentiation between kinds of linguistic study because he has already abandoned the analytic-synthetic distinction. Whether Quine is ultimately right or not, he can hardly use his conclusion as a premise against Carnap.

Thus, Carnap rejects Quine's demand for behavioral criteria, making instead what I will call his proposal gambit: Carnap claims not to be describing English or any other natural language. He claims rather to be making a proposal, that is, to be articulating an artificial language system for the purpose of explicating or even replacing a natural language in a given context. He claims further that the analytic-synthetic distinction is drawn clearly only for such artificial systems. Because he is proposing rather than describing, demands for empirical content are misplaced.

There is some force in this proposal gambit, but it is open to a devastating reply (and one which does not beg any questions against Carnap). Quine can say that Carnap is not even making a proposal unless it is at least possible in principle to determine whether such a proposal has been adopted. Without behavioral criteria, there is no such possibility. This can be illustrated in an example that Carnap himself had discussed. Carnap had argued that the notion of an entelechy was without empirical content. Assuming that to be the case, one cannot intelligibly propose to get one's entelechy adjusted, and one cannot defend the attempt by saying that it was only a proposal and not an empirical claim. A further analogy may be helpful. Carnap distinguished between pure or mathematical geometry and physical geometry; the former was to be analytic and hence in no need of correspondence rules, such as may be provided by measurement or surveying techniques. The corresponding Quinean point can be put by noting that without such measurement techniques at least waiting in the wings the purely mathematical work forfeits the right to the name 'geometry', for there would be no reason to suppose that it is in any way about points, lines, or space. The system would,

if consistent, have a model in any denumerable domain. Similarly, Carnap's artificial systems forfeit the claim to be about language unless there are behavioral criteria at least waiting in the wings. This is what Quine means in saying that Carnap's artificial languages could clarify analyticity but only if "the mental or behavioral or cultural factors relevant to analyticity – whatever they may be – were somehow sketched into the simplified model" (TDEa 34).

This Quinean response seems to me to be utterly convincing. If the analytic-synthetic distinction is to be viable at all, there must be somehow some behavioral criteria for analyticity. Yet, although we will concede this point on Carnap's behalf (and, it would seem, against his protests), the question of whether Quine has an argument against the intelligibility of analyticity that ought to be convincing is not thereby settled. We need first to achieve a better understanding of the demands for behavioral criteria by determining what is required for (and what would count as) satisfying them. In short, Quine has raised a good and legitimate issue, but it still needs to be clarified. Even then the fate of analyticity would not be settled until we come to a conclusion about the prospects for meeting those demands once they are clarified. Only if we could reasonably conclude (and the argument need not be airtight) that there were no such prospects should we take Hume's inflammatory advice given at the end of the *Enquiry*.[3]

Quine's demand for behavioral criteria derives both its legitimacy and its content from the empiricist tradition of formulating criteria of empirical significance. One is forced to the idea that the content comes from that tradition because Quine himself is very inexplicit about the details of his own demand. Presumably, the demand is of the sort that would be reasonable for any theoretical term in empirical science. This already tells us a fair bit. For one thing, the criteria need not amount to a full definition of analyticity. Theoretical terms are, in general, not definable in the observational framework. The desired connection is a looser one, allowing what is observable to count as evidence for or against the theoretical claim. Second, the behavioral criteria need not be given for analyticity directly. Often the various terms of a theoretical framework are so tightly bound up with one another that supplying observational criteria for one term serves to provide adequately for all. The theory of meaning is no exception. Quine is content to say that the various terms from

the theory of meaning – for example, 'analyticity', 'synonymy', 'intension', and 'meaning' – are fully interdefinable. Thus, behavioral criteria for synonymy, say, would suffice for all the rest.

It is important to note that the issue with behavioral criteria is intelligibility, not truth. This is what makes the Quinean response to the proposal gambit extremely powerful. So construed, however, it cannot be further demanded, if and when behavioral criteria are provided, that they must also turn Carnap's claims as to what is or is not analytic into a true theory of English. (Note: And so it is with geometry as well. Correspondence rules [observational criteria] were needed to make the axioms deserve the name 'geometry' at all. There can be no further requirement that they turn, say, Euclidian geometry into a true description of physical space.) Carnap says that logic and mathematics are to be counted among the analytic truths and that quantum mechanics, taken as a whole, is not. This need not be taken as a claim about English, and Carnap himself is explicit that what he says is to be taken as a proposal. Granted, Carnap undoubtedly thought that logic and mathematics were in fact analytic in English. But it is irrelevant to the merits of Carnap's proposal *as a proposal* whether English already embodies it. Now it would be a serious objection to a set of criteria if nothing or everything *always* turned out to be analytic under the criteria. More specifically, it would be equally objectionable if there were no *possible* language in which logic and mathematics turn out to be analytic while quantum mechanics and the general theory of relativity (taken as wholes) turn out to be synthetic. These objections would be grave, but they would also be very difficult objections to sustain, even if one wanted to talk about all possible languages, which Quine certainly does not.

Moreover, Quine attempts no direct argument that suitable behavioral criteria cannot be provided. Instead, what he does in "Two Dogmas" is to survey various attempts to clarify the theory of meaning. Does Quine look in all the right places? For the most part, yes, but given the conspicuously epistemic role of analyticity (it was, after all, a replacement for the apriority), the best place to look for meanings for a natural language ought to be in the community's epistemic activity. Thus, when the issue is translation, what the natives say about rabbits should matter less than what they say, in justification or criticism, about what they say. The questions to ask include

these: What claims need no justification at all? What inferences need no further justification? What inference preserves the degree of justification of any set of premises? Quine does eventually begin to address such questions, though in a less overtly epistemic and normative form. He does not do so, however, either in "Two Dogmas" or in the second chapter of *Word and Object*, which deals with translation and meaning.

Insofar as Quine's survey of ways to clarify concepts of meaning includes those attempts that would have been most naturally offered (and I think that, with the caveat of the previous paragraph, it does so) and insofar as the attempts fail (and they do), Quine can claim to have the basis of an inductive argument that the requisite behavioral criteria will not be forthcoming. So Quine does have an argument against the intelligibility of analyticity. It was never intended to be airtight. But its structure is fairly clear, its basis is a reasonable and minimal demand for behavioral criteria, and what it would take to defeat the argument is also clear. This is just what one would want from an argument in science.

2. RELEVANCE

I shall postpone the question of whether Quine's challenge to provide behavioral criteria for analyticity can be met and shall turn instead to the so-called second dogma of empiricism. The opening paragraph of "Two Dogmas" identifies the second dogma as reductionism, and §5, where the second dogma is discussed, links it to the verification theory of meaning. This challenge to reductionism and the verification theory is slightly unexpected because, while both terms have many senses, Quine is in some sense both a reductionist and a verificationist. Quine generally favors whatever reductions can be achieved, whether in ontology or in ideology, and the whole demand for behavioral criteria has its roots in verificationist accounts of significance. In any case, Quine's motivation for taking them up here is that the verification theory (of meaning rather than significance) purports to be an account of synonymy. If it is acceptable in this capacity, then analyticity can be saved after all. So analyticity is still the topic. Indeed, Quine says that the two dogmas are at root identical (see TDEa 38). Even so, something has profoundly changed in Quine's discussion in §§5–6 beyond his consideration of a subsidiary thesis about

analyticity. He is concerned no longer with behavioral criteria or even with the intelligibility of analyticity. Rather he is now concerned with epistemic matters, with the shape and structure of an account of confirmation. And the thrust of Quine's discussion is that analyticity (along with other concepts from the theory of meaning) has no place in the account he offers. Therefore, if that account is accepted, analyticity is epistemically irrelevant. This is a dispensability claim, much as one might reject as irrelevant the very idea of phlogiston because it simply has no place in modern chemistry.

The view about confirmation that Quine wants to reject we shall call sententialism, namely, the idea that sentences taken individually are susceptible of empirical confirmation or disconfirmation. What he wants to replace this with is the idea that "[t]he unit of empirical significance is the whole of science" (TDEa 39). Presumably, here, 'unit of empirical significance' is to be equated to 'unit that can be confirmed or disconfirmed'. Unfortunately, the structure of Quine's argument is extremely unclear. He offers no argument directly against sententialism. What he does argue against no one holds, which he virtually admits. And when he presents his positive view, the reader is left to guess what virtues Quine supposes that view to possess. The interpretive problem here is not so much to discern his views as it is to make a modicum of sense out of the way that Quine presents them. I think that that can be done, though at a half-century's remove there is probably no telling whether Quine's original intention is thereby recovered.

The discussion begins by considering the verification theory of meaning, formulated first as "the meaning of a statement is the method of empirically confirming or infirming it" and then as "statements are synonymous if and only if they are alike in point of empirical confirmation or infirmation" (TDEa 35). This implicitly ties the verification theory to sententialism, and Quine was to do so explicitly a few pages later. While these formulations treat only statements or sentences, other expressions can be accommodated easily. As Quine says, "[w]e could explain any two forms as synonymous when the putting of the one form for an occurrence of the other in any statement (apart from occurrences within 'words') yields a synonymous statement" (TDEa 35). Synonymy thus generally defined, analyticity could then also be defined via the devices canvassed earlier in his essay. Without refuting or even rejecting the verification

theory of meaning, Quine turns the discussion toward methods of confirmation.

Quine certainly means to give the impression that the verification theory is closely, even logically, tied to sententialism. This is not the case. The unit of confirmation could be a quite large chunk of theory. Then, as in Quine's own discussion, two expressions are synonymous just in case if whenever one is substituted for any occurrence of the other in any suitable (dis)confirmable chunk, its (dis)confirmation conditions are left undisturbed. This yields synonymy conditions for both sentences and terms. As in Quine's formulation, this will need a restriction against substitution within words. We shall return to this version of the verification theory later on.

When Quine turns to the methods of confirmation, he reformulates the question thus: "What . . . is the nature of the relation between a statement and the experiences which contribute or detract from its confirmation?" (TDEa 36). And the answer he considers first is radical reductionism. This is the view that every meaningful statement is translatable into a statement about immediate experience. The best version of this is that the translations are to proceed sentence by sentence rather than term by term. The best example of the best version is Carnap's *Aufbau*, but this fails in principle. It requires a second primitive, 'is at', which is not about immediate experience. Only general (holistic) directions are given for the use of this second primitive.

Because the best example of the best version fails, against radical reductionism there is a genuine argument. But, as Quine concedes, "Reductionism in its radical form has long since ceased to figure in Carnap's philosophy" (TDEa 38). And one would be hard pressed to name anyone at all who held the radical view in 1950. Of course, there are other forms of reductionism. As Quine says,

The dogma of reductionism survives in the supposition that each statement, taken in isolation from its fellows, can admit of confirmation or infirmation at all. My countersuggestion, issuing essentially from Carnap's doctrine of the physical world in the *Aufbau*, is that our statements about the external world face the tribunal of experience not individually but only as a corporate body. (TDEa 38)

This last comment at least helps to account for why Quine discussed the *Aufbau* at all; such reflections are intended to tar radical

reductionism, of course, but also to inspire and legitimate the holistic countersuggestion of modest reductionism.

In any case, it is unclear whether we are to suppose that Carnap was a modest reductionist in 1950. In fact, Carnap was some sort of holist as early as the 1930s,[4] and he used notions from the theory of meaning to identify the wholes that could be confirmed, to determine which observations would be germane to those wholes, and to specify the degree of support or the kind of revisions required. Analyticity, therefore, is highly relevant to Carnap's holistic epistemology even if not to Quine's. Sentialism, that is, modest reductionism, can be considered quite apart from the question of whether Carnap endorsed it, and that seems to be what the issues of §5 resolve to. Unfortunately, Quine did not show such a view to be a dogma of any kind. In fact, the countersuggestion, amplified in the final section, "Empiricism without the Dogmas," is all we ever get. Surely, Quine could not have intended to indict modest reductionism on the grounds of its parentage; who of us and what form of empiricism would survive so stern a judge? Not only is there no explicit argument against a form of reductionism that someone might currently hold, there is no explicit argument for the countersuggestion either. We are left to conclude that the elegance and coherence of his positive views were intended by Quine to *be* the argument, both for those views and against modest reductionism.

Unfortunately, even in amplified form those positive views are presented in such sketchy terms that it is difficult or impossible to assess their merit. Now sketchiness is not always a vice. Sometimes, especially in the early stages of a project, it is actually better to lay out the general structure of a view without filling in details that have not yet emerged. But the details must eventually be forthcoming. As Quine himself said of another example,

Russell had talked of deriving the world from experience by logical construction, but his constructions were sketchy and slight. Carnap, in *Der logische Aufbau der Welt* (1928), set himself to the task in earnest.... If the book did not achieve its exalted purpose, it did achieve a great deal. It afforded for the first time an example of what a scientific philosopher might aspire to in the way of rigor and explicitness.[5]

It is fair to say that Quine's own argument against the definability of physical concepts in experiential terminology was possible only

given Carnap's explicitness; that is, it would not have been possible given only Russell's sketchiness. In Quine's case, some progress was made beyond "Two Dogmas," but full accounts of simplicity and conservatism are still urgently needed. We do not know whether they can bear the weight that his sketch puts on them, and we do not know whether they can be clarified without appeal to the theory of meaning or at all.

Moreover, there is an issue of the extent to which it is legitimate to use one's own standards in the defense of those standards or in the criticism of others. Presumably, the former is fine. It should be perfectly reasonable to *defend* one's own position using one's own epistemic standards and to accept criticism only on the basis thereof. What other standards could one use? Thus, it is reasonable for Quine to prefer his holistic epistemology precisely on the grounds that it seems to avoid notions that he considers suspect, such as those from the theory of meaning. If it does avoid unclear notions without introducing others, it is to that extent clearer. If it employs fewer primitive notions and if that is part of what makes for simplicity, then it is to that extent simpler. Of course, there are important assumptions here, but the general strategy of appealing to one's own epistemology in its own defense is not viciously circular. Nor can Quine be faulted for not meeting Carnap's standards of clarity; Quine can hardly be required to say which of his claims are true in virtue of meaning.

What is fair for Quine must be fair for others as well. So it would seem impermissible, on pain of begging the issues at hand, for Quine to presuppose in his criticism of Carnap that such practical considerations as simplicity, elegance, and convenience, for example, are grounds for choosing among empirical theories. Carnap accepts these practical considerations in choosing among linguistic frameworks but not as bases for choosing among theories. Quine does make such a presupposition in "Carnap and Logical Truth."[6] Of course, even Carnap, early in his career and before the principle of tolerance, could also be accused of using his own science-oriented standards to discredit Heidigger, where Heidigger would have rejected the Carnapian standards that were being presupposed. In any case, Quine is not guilty of violating this rule at this point in "Two Dogmas," but only because he offers no explicit argument against the modest form of reductionism, that is, sententialism, at all. What then of the purported epistemic irrelevance of analyticity? That hinges entirely

on the fate of Quine's alternative epistemology, and that issue is as yet unresolved.

Whatever its fate, the epistemology espoused by Quine in "Two Dogmas" is a great and enduring contribution to the field. Both those who are inspired by it and those who reject it utterly have learned much from it and will continue to learn more. Carnap would have viewed it as a proposal that as such deserves to be carefully articulated and studied and whose consequences deserve to be made known. Surely we can honor it no less.

3. FURTHER DEVELOPMENTS

Quine's philosophy did not freeze with "Two Dogmas," and his epistemology continued to develop in ways that could hardly have been foreseen. One of the most striking features of "Two Dogmas" is that it treated simplicity of the overall system as a consideration on a par with evidence for choosing among alternative theories. This was one of the chief factors pushing him toward radical holism. After 1960 simplicity was viewed in a very different way:

Perhaps our vaunted sense of simplicity, or of likeliest explanation, is in many cases just a feeling of conviction attaching to the blind resultant of the interplay of chain stimulations in their various strengths. . . .

Simplicity is not a desideratum on a par with conformity to observation. Observation serves to test hypotheses after adoption; simplicity prompts their adoption for testing. (WO 19)

Such a stance avoids awkward questions about why a simpler theory would be more likely to be true, other things being equal. But it also tends to undermine both the mathematical and physical realism that Quine continued to defend through appeals to simplicity and also the radical holism that taking simplicity seriously seemed to require. In fact, radical holism did give way to a more modest version according to which it was large chunks of theory, which Quine called critical semantic masses, that were the units that could meet experience and hence be confirmed or disconfirmed. One of the salient features of the "Two Dogmas" picture is that, while there may be differences of degree, logic, mathematics, quantum theory, and the claim that there are brick houses on Elm Street are all in the same epistemic boat; none are immune to disconfirmation. But consider this passage

from *Pursuit of Truth*:

[w]e may picture the accommodation of a failed observation categorical as follows. We have before us some set *S* of purported truths that was found jointly to imply the false categorical.... Now some one or more of the sentences in *S* are going to have to be rescinded. We exempt some members of *S* from this threat on determining that the fateful implication still holds without their help. Any purely logical truth is thus exempted, since it adds nothing to what *S* would logically imply anyway.... (PTb 14)

It would certainly seem that now Quine too has a category of claims – in his case, consisting solely of the truths of elementary logic – that are immune to ordinary disconfirmation after all. Of course, they can still be revised, or better yet abandoned, but of course that is just what Carnap would have said of the analytic claims. However, such a revision for Quine is a revision of theory, while for Carnap it is a revision of language. These changes are certainly important, and taken together they undermine the conviction, so clear in "Two Dogmas," that if Quine's positive views were accepted, then analyticity had no epistemic role to play and was therefore irrelevant. Other changes in his philosophy tend to undermine another conviction that "Two Dogmas" may have induced, that analyticity is unintelligible for lack of behavioral criteria. There is no need to review all of these changes or do more than note their general character.

   One of the more striking developments of Quine's later philosophy occurs in *Roots of Reference* (1974), where Quine gives his own conception of analyticity (see RR 78–80). He repeats this characterization virtually unchanged up through *Pursuit of Truth* (1992), so it is no mere slip of the typewriter. In these publications he says that a sentence is analytic just in case everyone in the language community learns the truth of the sentence in the course of coming to understand the words therein. This, of course, requires criteria for understanding, and Quine supplies them. This conception, in its way, does make analyticity truth in virtue of language. And it would render truths of essential predication (e.g., 'No bachelor is married') as genuinely analytic. It would do the same for most or perhaps even all of elementary logic. Still, Quine insists, his notion will not do the job that Carnap requires, chiefly because it is vague or lacks the required extension. In particular, he thinks, mathematics would not come out as analytic, and so Carnap's general epistemology would

fall. In fact, Quine's conception could be remodeled without violating its spirit or jeopardizing the crucial behavioral criteria. But the important point to note is that such improvements are unnecessary. By providing the demanded criteria, it gives us *an* analytic-synthetic distinction, which is all that intelligibility requires. At that point Carnap's proposal gambit can work; Carnap is free to intelligibly propose that we draw the distinction just where he wants and with whatever level of precision anyone could desire. As we saw earlier, the very feature of Quine's demand for criteria that makes it so powerful against the proposal gambit precludes the further demand that those criteria, when supplied, turn the proposals into true descriptions of English. Thanks to Quine, some set of behavioral criteria is now at hand, and that is enough.

In "Carnap and Logical Truth," Quine remarked that elementary logic was obvious, or potentially so. That is, its various truths can be gotten from obvious truths by obvious inferences. In 1986, Quine called obviousness "blatantly behavioral" (RGH 206). Again Quine would complain that the limits of obviousness are not at all the intended limits of analyticity. True, but again, so what? Once we have behavioral criteria, Carnap can propose what he likes.

As noted earlier in this essay, Quine moved from a radical holism, in which only the totality of belief is confirmed or disconfirmed, to a more modest one, in which critical semantic masses (suitably large chunks of theorizing) can be tested. It was also noted above that the so-called verification theory of meaning ought to be compatible with such a modest holism. He uses other words, but the same idea occurred to Quine as well. In *Pursuit of Truth*, he says,

> One is tempted to suppose that we might define meanings for sentences of less than critical semantic mass, and even for terms, by substitutivity. If we can interchange two expressions without disturbing the empirical content of any testable context, are they not alike in meaning? (PTa 53)

And in the second edition, he adds in the next paragraph, "Sentences are *cognitively equivalent*, we might say, if putting one for the other does not affect the empirical content of any set of sentences. This sounds right in principle" (PTb 53). Indeed, it is right. Quine objects, of course, and properly so, that the imagined substitutions will work at best within a single language and that hence the plan

does not immediately give us a notion of synonymy that will do for translation. But even as it stands, with just an intralinguistic notion of synonymy, it is sufficient to define 'analytic' as suggested in "Two Dogmas" for any language, including English, for which the logical truths can be identified. For that matter, it is premature to give up on translation. In *The Logical Syntax of Language*, Carnap had defined a translation relation that presupposed only an intralinguistic notion of implication (definable on the basis of an intralinguistic notion of synonymy).[7] The fundamental idea was that a translation is a mapping of expressions of one language into those of another so as to preserve intralinguistic implication. This is a *very* strong requirement and is likely to result in there being no satisfactory translations rather than there being several. In this, Carnap's definition accords better with standard expectations than does Quine's discussion of the indeterminacy of translation. In any case, translation is beside our present point; what Quine says is enough to save analyticity.

These are not the only changes in Quine's views nor even necessarily the most important ones. It could be argued that his preoccupation with reference and ontology generally gave way to a greater concern for issues of an epistemic sort, and what he said about these may ultimately be more momentous than the changes we have discussed. Because of the importance and originality of his holistic picture, one can only applaud the increased attention to epistemology. But analyticity is important too, and regarding it the situation seems to have changed dramatically following "Two Dogmas," no doubt more fundamentally than Quine admitted. In that earlier paper, the demand for behavioral criteria had raised a deep and potentially fatal question about analyticity. If the demand could not be met, both the concept of analyticity and the epistemology that rested on it would have to be rejected as unintelligible. The evidence from Quine's later writing, however, is that the crucial demand can be met to the full extent that it is legitimate. Moreover, "Two Dogmas" introduced a sketch for a novel approach to science. This would render analyticity irrelevant if each of two conditions were to be met. First, enough details would have to be provided to assure us that that new approach is what it seems in avoiding concepts from the theory of meaning without generating other difficulties. Second, a non-question-begging argument

would have to be given for the preferability of that approach over any other that does employ such notions as analyticity. So far, these two conditions have not been met. At this juncture, about fifty years after "Two Dogmas," it would seem that analyticity is indeed intelligible and highly relevant, at least to Carnap's own epistemic view.

It does not, however, settle the deepest issues between Quine and Carnap. Nor is it clear what would be required to do so. But there is a useful suggestion in the historical record. In 1951, Quine gave a department colloquium at the University of Chicago, and the second half of his talk was a précis of "Two Dogmas." Carnap was in the audience, as was Howard Stein, who later reported as follows:

Carnap's summary of the issue between Quine and himself was on the following lines: "Quine," he said (I am not quoting verbatim, but giving the gist as I remember it), "and I really differ, not concerning any matter of fact, nor any question with cognitive content, but rather in our respective estimates of the most fruitful course for science to follow. Quine is impressed by the continuity between scientific thought and that of daily life, between scientific language and the language of ordinary discourse – and sees no philosophical gain, no gain either in clarity or in fruitfulness, in the construction of distinct formalized languages for science. I concede the continuity, but, on the contrary, believe that very important gains in clarity and fruitfulness are to be had from the introduction of such formally constructed languages. This is a difference of opinion which, despite the fact that it does not concern (in my own terms) a matter with cognitive content, is nonetheless susceptible of a kind of rational resolution. In my view, both programs – mine of formalized languages, Quine's of a more free-flowing and casual use of language – ought to be pursued; and I think that if Quine and I could live, say, for two hundred years, it would be possible at the end of that time for us to agree on which of the two programs had proved more successful." . . . [A]s I recall, Quine happily assented to Carnap's diagnosis.[8]

None of us, presumably, has two hundred years to wait. But the moral for the short run is clear: Pursue both programs. Whether or not Quine is ultimately vindicated, we have much to learn from studying his suggestions, from articulating them in ways that may be helpful, from discovering their consequences, and from making comparisons with those from other programs. Pursuit of another theorist's research program is the highest honor that any profession has to bestow.

NOTES

1. That Quine is inexplicit in "Two Dogmas" may be part of the reason that Carnap seems not to have understood, at least initially, what Quine was after. But it can be only part of the reason. In writings both before and after "Two Dogmas," Quine's demand is more obvious. In "Truth by Convention" he had spoken approvingly of these topics when "[v]iewed behavioristically" (TBC 119). In "Notes on Existence and Necessity," he said, "The relation of synonymity, in turn, calls for a definition or a criterion in psychological and linguistic terms" (NEN 120). In a letter to Carnap written in January 1943, Quine expressed this as follows: "The definition of this relation of synonymity, within pragmatics, would make reference to criteria of behavioristic psychology and empirical linguistics" (Richard Creath, ed., *Dear Carnap, Dear Van: The Quine-Carnap Correspondence and Related Work* [Los Angeles: University of California Press, 1990], 298). This letter is many ways an early draft of the first sections of "Two Dogmas." In 1954, Quine wrote and Carnap read "Carnap and Logical Truth." It contains this sentence: "Conceivably the mechanism of such [synonymy] recognition, when better understood, might be made the basis of a definition of synonymy and analyticity in terms of linguistic behavior" (CLT 403). Lastly, as if further proof were needed that what Quine ultimately requires is behavioral criteria, Quine wrote in *Word and Object*, "Also we find it argued that the standard of clarity that I demand for synonymy and analyticity is unreasonably high; yet I ask for no more, after all, than a rough characterization in terms of dispositions to verbal behavior" (WO 207).

2. See especially Carl G. Hempel, "Problems and Changes in the Empiricist Criterion of Meaning," *Revue Internationale de Philosophie* 11 (1950): 41–63.

3. "When we run over libraries, persuaded of these principles, what havoc must we make? If we take in our hand any volume; of divinity or school metaphysics, for instance; let us ask, *Does it contain any abstract reasoning concerning quantity or number*? No. *Does it contain and experimental reasoning concerning matter of fact and existence*? No. Commit it then to the flames: for it can contain nothing but sophistry and illusion" (David Hume, *Enquiry concerning Human Understanding*, in *Enquiries concerning the Human Understanding and concerning the Principles of Morals*, ed. L. A. Selby-Bigge, 2nd ed. [Oxford: Clarendon Press, 1902], 165).

4. See, for example, *The Logical Syntax of Language*, trans. Amethe Smeaton (London: Kegan Paul Trench, Trubner & Co., 1937), 318, where

Carnap says,

Further, it is, in general, impossible to test even a singular hypothetical sentence. In the case of a single sentence of this kind, there are in general no suitable L-consequences of the form of protocol-sentences; hence for the deduction of sentences having the form of protocol-sentences the remaining hypotheses must be used. *Thus the test applies, at bottom, not to a single hypothesis but to the whole system of physics as a system of hypotheses* (Duhem, Poincaré).

5. Creath, *Dear Carnap, Dear Van*, 456.
6. Ibid., 396.
7. Ibid., 224 ff.
8. Howard Stein, "Was Carnap Entirely Wrong after All?" *Synthese* 93 (1992): 278–9.

3   Quine's Meaning Holisms

> Quine's [historic] importance does . . . depend upon his be-
> ing right in one central claim, a claim which he expressed
> by saying that there is no sensible distinction between an-
> alytic and synthetic truths but which he should have ex-
> pressed by saying that there is no sensible distinction be-
> tween *a priori* and *a posteriori* truths.
>
> <div align="right">Putnam 1983</div>

> Erasing the line between the analytic and the synthetic
> saved philosophy of language as a serious subject by show-
> ing how it could be pursued without what there cannot be:
> determinate meanings.
>
> <div align="right">Davidson 1986</div>

Quine's writings are the point of departure for the familiar doctrine
that goes by the name 'meaning holism'.[1] This doctrine contrasts
with meaning atomism, according to which a linguistic expression
e in a language L has its meaning 'Auf Eigene Faust' (viz., in and by
itself) by virtue of a symbol-world relation independent of, and (meta-
physically) prior to, whatever role *e* has in L. For meaning atomism,
reference (however specified), then, is primitive, and the role of *e* in
L is determined by, and derivative from, the meaning *e* acquires in
virtue of that relation.[2] In opposition, according to meaning holism,
a linguistic expression *e* in a language L has its meaning in virtue of
its (however specified) relations with other expressions in L; that is,
in virtue of its role in L. For meaning holism, since the role of *e* in
L constitutes *e*'s meaning, reference becomes derivative from, and
(metaphysically) posterior to, the role *e* plays in L.[3]

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The aims of this chapter are four. Section 1 sketches Quine's argument for meaning holism. Section 2 places this argument within the context of Quine's naturalism and verificationism about meaning. To do so vis-à-vis his naturalism permits us to illustrate the all too often neglected relation between Quine's argument for meaning holism as presented in "Two Dogmas of Empiricism"[4] and his speculation on radical translation. Retracing that connection is necessary in a discussion about Quine's holism, since he himself refers to his meaning holism as the thesis of the indeterminacy of translation. Section 3 examines in more detail Quine's argument for holism as presented in "Two Dogmas" and addresses whether Quine's claim that the analytic-synthetic distinction is unintelligible requires a moderate or radical holism. Section 4 concludes by offering a sample of the debate "Two Dogmas" has generated and of the questions that remain open and worth pursuing as a result of Quine's significant achievements.

1. QUINE'S ARGUMENT FOR MEANING HOLISM

Quine derives meaning holism from confirmation holism and verificationism about meaning. His confirmation holism is generally identified with Duhem's thesis: "[I]t is only the theory as a whole and not any one of the hypotheses that admits of evidence and counter-evidence in observation and experiment" (PL 5) – that is, *empirical content* does not belong to any individual statement of the theory in isolation from the other statements of the theory. Quine's verificationism is the thesis that "the meaning of a statement is the method of empirically confirming or infirming it" (TDEb 37) – that is, its empirical content. Following the empiricist and positivist tradition, Quine identifies the concepts of *meaning* and *evidence*, so that to know the meaning of a sentence is to know how it could be recognized as true.

Quine's intention to derive meaning holism from confirmation holism and verificationism is more or less explicit in many writings. Here are representative passages:

If we recognize with Peirce that the meaning of a sentence turns purely on what would count as evidence for its truth, and if we recognize with Duhem that theoretical sentences have their evidence not as single sentences but only as larger blocks of theory, then the indeterminacy of translation of

theoretical sentences [or, *mutatis mutandis*, meaning holism] is the natural conclusion. (EN 80–1)

The verification theory of meaning, which has been conspicuous in the literature from Peirce onward, is that the meaning of a statement is the method of empirically confirming or infirming it. . . . My countersuggestion [to the dogma of reductionism] . . . is that our statements about the external world face the tribunal of sense experience not individually but only as a corporate body [Duhem's thesis]. . . . What I am urging is that even in taking the statement as unit we have drawn our grid too finely. The unit of empirical significance is the whole of science [or language]. (TDEb 41–2)

[T]he indeterminacy of translation [meaning holism] follows from [confirmation] holism and the verification theory of meaning. (RG 155; see also RR 38)

It is, moreover, a common view in the critical tradition that, in "Two Dogmas" and elsewhere, Quine holds that verificationism together with confirmation holism (sometimes called the Quine-Duhem thesis) *entails* meaning holism.[5]

    To summarize, Quine's writings, and a conspicuous consensus in the critical tradition, suggest that his argument for meaning holism is as follows:

> (P1) The meaning of a sentence consists in its (dis)confirming experiences (or empirical content) – that is, in what counts as evidence for its truth (verificationism about meaning).
>
> (P2) Sentences of *a scientific theory* do not have their range of (dis)confirming experiences individually but have them only as a corporate body – that is, they lack empirical content in isolation from the other sentences of the theory (Duhem's thesis).
>
> ∴ (C) The sentences of a *language* do not have meaning individually but have it as a corporate body – that is, they lack meaning in isolation from the other sentences of the language (meaning holism).

If we assume with Quine that the meaning of a sentence consists in *its method of empirical (dis)confirmation* and that "the two roles of observations, their role in the support of theory and in the learning of language, are inseparable" (RR 38), the inference from (P1) and (P2) to (C) looks prima facie valid.[6]

Although "Two Dogmas" is generally considered to be Quine's manifesto on meaning holism, and his meaning holism is, more or less, identified with that essay's rejection of the analytic-synthetic distinction. Quine, as indicated by two of the passages just quoted, often refers to his meaning holism as 'the indeterminacy of translation'. So, before discussing Quine's holistic claims in detail, we present a brief discussion of the indeterminacy of translation and how it connects to his argument for meaning holism in "Two Dogmas."[7]

2. QUINE'S NATURALISM AS KEY TO HIS MEANING HOLISM

Quine is well known for his naturalism; he holds with Dewey that "knowledge, mind, and *meaning* are part of the same world they have to do with, and that they are to be studied in the same *empirical spirit* that animates natural science" (OR 26, emphasis added). For meaning to be investigated empirically, it must be made public. Meaning becomes a property of behavior, and "language is a social art which we all acquire on the evidence solely of other people's overt behavior under publicly recognizable circumstances" (OR 26). On Quine's view (as, barring differences, on Wittgenstein's and Dummett's),[8] the requirement that meaning is essentially public and social in nature relates to the identification of meaning with evidence (or use), that is, to some sort of verificationism, which in Quine's case explicitly takes the form of behaviorism. In short, *naturalism*, *verificationism*, and *behaviorism* are deeply interrelated in Quine's philosophy. It is within this framework that Quine's meaning holism, in the shape of his thesis of the indeterminacy of translation, is best understood.

Given these assumptions about the nature of meaning, it is no surprise that Quine's speculation on meaning takes the form of a theory of *radical* translation. Since meaning must be publicly determinable, all there is to know about meaning is what a radical translator can learn about it. To illustrate how Quine's meaning holism is related to his theory of radical translation, we follow the vicissitudes of a field linguist trying to translate an entirely alien language into (say) English.[9]

Since, by hypothesis, the language is alien, "[A]ll the objective data ... [the radical translator] has to go on are the forces that

he sees impinging on the native's surfaces and the observable behavior . . . of the speaker" (WO 28). Such data manifest native "meanings" only of the most objectively empirical or stimulus-linked kind, and yet, Quine continues, "the linguist . . . ends up with native 'meanings' in some quite unrestricted sense; purported translations . . . of all possible native sentences" (WO 28). Precisely in accounting for how a linguist comes to translate all possible native sentences on the basis of the only evidence available (i.e., meanings of the most objectively empirical and stimulus-linked kind), Quine concludes that translation (and hence meaning) is indeterminate. So how does a field linguist arrive at a translation manual for a foreign language?

Given the linguist's epistemic position, the native utterances "first and most surely translated" are utterances capable of being learned ostensively, that is, utterances "keyed to present events that are conspicuous to the linguist and his informant" (WO 29). For example, a rabbit scurries by, a native says, 'Gavagai', and the linguist writes down, as a tentative translation of 'Gavagai' (i.e., subject to further testing), the sentence 'Rabbit'.[10] How can the linguist further test his translation? Once he identifies native expressions for assent and dissent, he can ask 'Gavagai?' in each of various stimulatory situations and note whether the native assents, dissents, or neither. In other words, once native expressions for assent and dissent are available, the linguist is positioned to accumulate inductive evidence for translating 'Gavagai' as 'Rabbit'. As Quine puts it, "[T]he general law for which . . . [the linguist] is assembling instances is roughly that the native will assent to 'Gavagai' under just those stimulations under which we, if asked, would assent to 'Rabbit'" (WO 30); that is, working inductively, the linguist concludes that 'Gavagai' and 'Rabbit' have the same *stimulus meaning*.

A few words about stimulus meaning are in order, since, looking ahead, the question of whether observation sentences can indeed possess an *independent stimulus meaning* will be central to our discussion of whether Quine's holism is extreme or moderate. The stimulus meaning (or empirical meaning [see WO 32]) of a sentence for a subject "sums up his disposition to assent to or dissent from the sentence in response to present stimulation" (WO 34). And it is for observation sentences like 'Gavagai' – that is, sentences such that just about everyone in the speech community would assent to,

or dissent from, under appropriate circumstances[11] – that stimulus meaning constitutes a reasonable notion of meaning. As we saw, approximate sameness of stimulus of meaning, or stimulus synonymy, accounts for the translation of sentences like 'Gavagai' as 'Rabbit'. So at the level of observation sentences, stimulus meaning offers the linguist an objective criterion of translation into English.[12]

However, the linguist's task does not end here. In order to pass the bounds of observation sentences and stimulus meaning, the linguist segments heard utterances into short recurrent parts and thus (by essentially abstracting from the systematic role "words" play in learned sentences) compiles a list of native words. This too is no mean feat: The stimulus synonymy of the one-word sentences 'Gavagai' and 'Rabbit' does not guarantee that the words 'gavagai' and 'rabbit' are coextensive, because stimulus meaning is insufficient to decide among the possible translations of 'gavagai' as 'rabbit', 'rabbit stage', 'rabbithood', and so on (see WO 51–61). Supplementary pointing alone is no help, since when one points to a rabbit, one points to a stage of a rabbit, to an integral part of a rabbit, and to where rabbithood is manifested. Only after developing a system of *analytical hypotheses* as to how to translate our domestic apparatus of objective reference[13] into the native language can the linguist translate 'gavagai' as either 'rabbit', 'rabbit stage', or 'rabbithood'.

Crucial here is how the linguist arrives at his system of analytical hypotheses. Quine's answer is that he does so by abstraction and hypothesis.[14] But, he insists, it is only by outright projection of prior linguistic habits that the linguist can find general terms in the native language at all – or, having found them, match them with terms in his own, language – since stimulus meaning is insufficient to determine "even what words are terms, if any, much less what terms are coextensive" (WO 70). In projecting those habits, the linguist imposes his own language and conceptual scheme upon the native language: "Terms and references are local to our conceptual scheme" (WO 53). In other words, all observable data (i.e., the native's disposition to speech behavior) *underdetermine* the linguist's analytical hypotheses, and on this basis translation of all further sentences depends.

Here enters the *indeterminacy of translation*, for, although the formulation of a system of analytical hypotheses permits the linguist to assign 'gavagai' a determinate meaning relative to that system, his method of analytical hypothesis does not in principle settle

indeterminacy among 'rabbit', 'undetached rabbit part', and 'rabbit-hood'. In Quine's own words,

[I]f one workable overall system of analytical hypotheses provides for translating a given native expression into 'is the same as,' perhaps another equally workable but systematically different system would translate that native expression rather into something like 'belongs with.' Then when in the native language we try to ask 'Is this *gavagai* the same as that?' we could as well be asking 'Does this *gavagai* belong with that?' Insofar, the native assent is no objective evidence for translating 'gavagai' as 'rabbit' rather than 'undetached rabbit part'. (OR 33)

In short, since different systems of analytical hypotheses, by compensatorily juggling the translation of the apparatus of individuation, can specify mutually incompatible translation manuals still compatible with all possible evidence (the native's disposition to speech behavior), indeterminacy of translation or meaning follows.[15]

The indeterminacy of translation or meaning is a consequence of Quine's naturalism. Since, for a naturalist, meanings are grist for the behaviorist mill, 'we give up any assurance of determinacy' (OR 28):

For naturalism the question whether two expressions are alike or unlike in meaning has no determinate answer, known or unknown, except insofar as the answer is settled in principle by people's speech dispositions, known or unknown. *If by these standards there are no determinate cases*, so much the worse for the terminology of meaning and likeness of meaning. (OR 29, emphasis added)

Since the *Gedankenexperiment* of radical translation establishes[16] indeterminate cases, traditional notions of meaning and meaning identity are, according to Quine, in trouble – which, in effect, amounts to saying that meaning holism follows.

So far, we have illustrated how Quine's meaning holism, in the form of the indeterminacy of translation, is a consequence of the backbone of his philosophy, namely, his naturalism. What needs to be explained next is how all this relates to his argument for meaning holism as presented in "Two Dogmas" (outlined in §1 of this chapter). The relation, though clear, is obfuscated by a common practice of discussing Quine's holism and his theory of radical translation separately. First, the moral of Quine's speculation on radical translation, according to which experience is relevant to sentences in

indirect ways through the mediation of associated sentences (WO 64), coincides with and/or finds support in Duhem's confirmation holism, according to which "no particular experiences are linked with any particular statements in the interior of the . . . [theory], except indirectly through considerations of equilibrium affecting the . . . [theory] as a whole" (TDEb 43). Second, Quine's argument for his indeterminacy thesis (i.e., that mutually incompatible translation manuals can be made equally compatible with all the possible evidence by compensatorily juggling the translation of the apparatus of individuation) echoes his argument in "Two Dogmas" for meaning holism, on the basis of Duhem's confirmation holism (and the related rejection of the analytic-synthetic distinction).[17]

In "Two Dogmas," the argument for meaning holism goes as follows:

> (P1) "Our statements about the external world face the tribunal of experience not individually but only as a corporate body" (TDEb 41) (Duhem's confirmation holism).
> ∴ The empiricist dogma of reductionism (i.e., the view that each synthetic statement "taken in isolation from [the other statements of the theory] can admit of confirmation or infirmation" [TDEb 41]) must be abandoned.
> (P2) The dogma of reductionism and the analytic-synthetic distinction are "at root identical" (TDEb 41).[18]
> ∴ (C1) The analytic-synthetic distinction must also be abandoned.
> ∴ (C2) Meaning holism is true.

That is, *if confirmation holism is true*,

it is misleading to speak of the empirical content of an individual statement – especially if it is a statement at all remote from the experiential periphery of the field. Furthermore, it becomes folly to seek a boundary between synthetic statements, which hold contingently on experience, and analytic statements, which hold come what may. *Any* statement can be held true come what may, *if we make drastic enough adjustments elsewhere in the system*. Even a statement very close to the periphery can be held true in the face of recalcitrant experience by pleading hallucination or by amending certain statements of the kind called logical laws. (TDEb 43, emphasis added)

So if sentences lack meaning individually, the translation of any sentence, even an observation sentence, can, at least in principle, be held fast "come what may" by compensatorily juggling the translation of the apparatus of individuation – which is the moral of the indeterminacy of translation.[19]

In conclusion, we have illustrated, how the indeterminacy of translation is equivalent to meaning holism in "Two Dogmas," on the grounds of Duhem's confirmation holism and the rejection of the analytic-synthetic distinction.[20] Hence Quine's naturalism is the key to meaning holism. Here is an enriched version of the argument for meaning holism presented in §1 of this chapter:

> (P1) Confirmation holism is true, as Duhem has argued and as Quine's own theory of language learning and radical translation suggests.[21]
> (P2) The meaning of a sentence is its method of (dis)confirmation (Quine's verificationism about meaning, which is a corollary to his naturalism).
> ∴ (C) Any sentence of a language lacks meaning in isolation from the other sentences of the language (meaning holism).

In short, Duhem's confirmation holism and Quine's speculation on radical translation establish the demise of the dogma of reductionism and hence the demise of the analytic-synthetic distinction and consequently establish the truth of meaning holism.

3. QUINE'S MEANING HOLISM: STRONG OR MODERATE?

We now move on to illustrate how Quine can be interpreted as defending a *strong* or a *moderate* holism depending on whether, and in what sense, he is interpreted as holding that the analytic-synthetic distinction is unintelligible. We argue that understanding both what kind of analytic-synthetic distinction Quine deems unintelligible in "Two Dogmas" and what kind of analytic-synthetic distinction he can reintroduce (compatible with the arguments of "Two Dogmas") in order to moderate his holism is essential for establishing what kind of meaning holism he endorses. We conclude by raising difficulties for his defense of a moderate holism.[22] We first define 'strong holism' and 'moderate holism' in relation to the analytic-synthetic

distinction and then show that both versions can be located in Quine's writings.

Those who concur with Quine that, as a consequence of confirmation holism, no sentences are true in virtue of the meaning of their component words alone and none have their truth grounded in experience in isolation from the other sentences of the language (i.e., there is no analytic-synthetic distinction) are inclined to interpret him as inferring a strong holism from verificationism and confirmation holism, in particular:

> *Strong holism*: No sentence of the language has its meaning in isolation from every other; that is, the meaning of any sentence of the language is determined by its (evidential/inferential) relations to every other. The unit of meaning is the *whole* language.

Strong holism finds support in "Two Dogmas," for Quine claims there that *any* statement, even *an observation sentence*, "can be held true come what may in the face of recalcitrant experience by pleading hallucination or by amending certain statements of the kind called logical laws" (i.e., there are no synthetic statements). In consequence, he continues, nor are there analytic statements: "No statements are immune to revision" (TDEb 43). These claims together require that no sentence has meaning in isolation from the other sentences of the language (strong holism).

However, those who take Quine to continue to adhere to some sort of analytic-synthetic distinction (i.e., that there are both a class of sentences with individual empirical content independent of the rest of the language and a class of analytic statements) are inclined to read him as endorsing a moderate holism of this form:

> *Moderate holism*: The meaning of any sentence of a language is determined by its (evidential/inferential) relations to many other sentences. The units of meaning are *fragments* of the language.

In more recent writings, Quine defends moderate holism over strong holism; in *Word and Object*, for example, he claims that observation sentences are an exception to the interconnectedness of sentences of the language because they carry meaning (stimulus meaning) independently of the rest of the language.[23] In "Epistemology

Naturalized," to mention one other of many examples, he writes that observation sentences are "the repository of evidence for scientific hypotheses" (EN 88) (on pain of purchasing epistemological nihilism) and are "the cornerstone of semantics" (EN 89) since they are essential to the language-learning process. Whereas theoretical sentences confront the tribunal of experience in more or less inclusive aggregates, an observation sentence has "an empirical content all its own and wears it on its sleeve" (EN 89).[24]

Michael Dummett insists on an internal tension within the corpus of Quine's writings between strong and moderate holism. He observes that the very metaphor of a language as an articulated structure whose sentences lie at differing depths from the periphery (offered at the end of "Two Dogmas" [TDEb §6]) suggests a moderate holism[25] at odds with the strong holism implied by what is explicitly claimed there. The metaphor of language as an articulated structure making contact with reality only at its periphery (and whose peripheral sentences are observation sentences primarily verified or falsified by experience and whose theoretical sentences can be confirmed only as a result of inference via links with other sentences in that structure) "in no way represents an essentially holistic view of language [read: strong holism and indeed accords rather badly with such a view" (Dummett 1993b, 33). Rather, Dummett (1991) urges, this picture contains "the apparatus for quite precise definitions of 'analytic' and 'synthetic'" (p. 242).[26]

However, Dummett (1973) notices that Quine, in "Two Dogmas," advances two further theses whose consequences "lead to the destruction of the image of the language he presented" (p. 593). These theses are that even an observation sentence can be held true in the face of recalcitrant experience and that no statement, not even a logical law, is immune to revision (i.e., Quine's claim that there is no principled analytic-synthetic distinction). A consequence of the first thesis, Dummett observes, is "to subvert the metaphor of periphery and interior altogether." If alternative revisions are always possible, in particular, revisions that leave the periphery intact, then the metaphor of a multilayered structure that impinges on experience only at the periphery no longer makes sense. Since language confronts experience as a monolithic whole, as a single-storied complex, "there is no periphery and no interior" (p. 594). A consequence of the second thesis, Dummett notices, is to dissolve the internal structure

of the language/theory, consisting of the interconnections of sentences with one another (p. 597). To make sense of the distinction between sentences whose meanings are determined by their direct relation with experience (i.e., the sentences at the periphery) and sentences whose meanings are determined by inferential connections to other sentences, "we have to have some way of understanding in what the inferential connections between sentences consist" (p. 596). But since rules of inference (as well as logical laws) are not immune to revision, according to Quine's claim that there are no analytic statements, "there is nothing for the inferential links to consist in."

As a result, Dummett concludes, "Quine's original model of language is transformed into a theory quite rightly characterized as holism [read: strong holism]."[27] However, we mustn't forget that "Two Dogmas" was published in 1951, and though Quine never recanted the arguments for meaning holism he offered there, his later writings[28] present a holism he himself described as *moderate*, and he referred to the strong holism suggested by "Two Dogmas" as "pure legalism."

In "Reply to Jules Vuillemin," Quine defends a moderate holism by weakening the two theses of "Two Dogmas" that commit him to strong holism: "The holism for which I declared in broad lines [in "Two Dogmas"] exceeded what was needed in controversion [of the two dogmas of empiricism]" (RJV 619). The thesis that *no* single statement (not even an observation sentence) has its separable empirical meaning far exceeded what was needed to reject the empiricist dogma of reductionism. All that needed to be claimed (against reductionism) was that "*many* scientific sentences inseparably share empirical content"(RJV 619). The version of strong holism that holds that language as a whole confronts experience is defensible, Quine claims, only in legalistic terms. In practice, more modest chunks of the language "can be ascribed their independent empirical meaning" (FME 71); that is, in practice, moderate holism holds, not strong holism.

Moderate holism, as anticipated above, leaves room for a class of sentences (viz., observation sentences) with their "own separate empirical content" (RJV 620). Rather interestingly, in many places[29] Quine ascribes to observation sentences a peculiar double status: They are both theory-free (and hence have independent empirical content) and theory-laden (and hence have theory-relative or

immanent empirical content). "Seen holophrastically," Quine explains, "as conditioned to stimulatory situations, the [observation] sentence is theory-free; seen analytically, word by word, it is theory-laden" (PTb 7) since it shares much of the vocabulary of theoretical statements. This is why, according to Quine, Duhem's thesis both does and does not hold for observation sentences. It does not, because, holophrastically, observation sentences have independent, theory-transcendent empirical meaning. But since observation sentences are also theory-laden, "the Duhem thesis still holds in a somewhat *literalistic* way, even . . . [for them]. For the scientist does occasionally revoke even an observation statement, when it conflicts with a well-attested body of theory" (EES 314). The thesis holds, however, only legalistically, because, in practice, recanting an observation sentence in light of the rest of the theory "is an extreme case and happily not characteristic" (FME 71).

Quine makes similar remarks about his claim in "Two Dogmas" that no sentences are immune to revision – or, *mutatis mutandis*, that no statements are analytic. "Even a truth of logic or mathematics *could* be abandoned in order to hold fast some causal statement of ephemeral fact," observes Quine, "but would [it] be? . . . In principle . . . vulnerability is universal; . . . in practice, it comes in degrees. It is at a minimum in logic and mathematics because disruptions here would reverberate so widely through science" (RJV 619–20). He repeats the point often: In *Pursuit of Truth*, he writes that, in accommodating a failed observation categorical[30] deduced by a given hypothesis, a scientist heeds the *maxim of minimum mutilation* in choosing which of the sentences (composing the fragment of scientific theory that, together with the hypothesis, implies the observation categorical) to rescind. "The maxim constrains us . . . to safeguard any purely mathematical truth; for mathematics infiltrates all branches of our system of the world, and its disruption would reverberate intolerably" (PTa 15). The *necessity* (and analyticity) of mathematics is then explained "by our unstated policy of shielding mathematics by exercising our freedom to reject other beliefs instead" (PTa 15).[31]

In *Roots of Reference*, Quine offers an account of analyticity in the light of how speakers learn their native tongue. According to this characterization of analyticity, a sentence is analytic for a native speaker if he learns its truth by learning the use of one or more of

its words. This would render 'No bachelor is married' analytic, since "it seems that we all learned 'bachelor' uniformly, by learning that our elders are disposed to assent to it in just the circumstances where they will assent to 'unmarried man'" (RR 80). Accordingly, elementary logical truths are analytic, since "anyone who goes counter to *modus ponens*, or who affirms a conjunction and denies one of its components, is simply flouting what he learned in learning to use 'if' and 'and'" (TDR 270).[32]

To sum up, Quine defends moderate holism against strong holism as follows: *In principle*, being vulnerable is a universal test of observation; *in practice*, it comes in degrees. Being vulnerable to the test of observation is at a minimum in mathematics and logic (hence their analytic status); vulnerability to the test of observation is maximal with observation sentences (hence their synthetic status). But what explains the difference in degree of vulnerability between analytic and synthetic statements? Or, *mutatis mutandis*, what kind of analytic-synthetic distinction is Quine reintroducing? Quine replies that, although he showed in "Two Dogmas" that there is no principled (or language-transcendent) analytic-synthetic distinction, an arbitrary (or language-immanent) analytic-synthetic distinction is still available to temper strong holism. To evaluate Quine's claim, we must reconsider what sort of analytic-synthetic distinction did he show unintelligible in "Two Dogmas."

Quine, in "Two Dogmas," regarded as untenable the distinction between analytic, or a priori, truths, and synthetic, or a posteriori, truths. If analytic truths exist, we do not know them a priori.[33] What Duhem's thesis establishes, Quine claims, is that, contrary to the traditional verificationist assumption that analyticity and confirmation are transcendent (or language-independent) notions, they are immanent (or language-dependent) notions. Analyticity is only *domestically* definable,[34] and theory-independent facts do not determine whether an observation confirms a statement.[35]

The analytic-synthetic distinction reintroduced to moderate his holism is no longer epistemologically significant (but rather arbitrary and epistemologically insignificant).[36] Even with this present account of analyticity,

we have no such *radical* [emphasis added; read: absolute or principled] cleavage between analytic and synthetic sentences as was called for by Carnap and

other epistemologists. In learning our language each of us learns to count certain sentences, outright, as true; there are sentences whose truth is learned in that way by many of us, and there are sentences whose truth is learned in that way by few or none of us. The former sentences are more *nearly* analytic than the latter. The *analytic* sentences are the ones whose truth is learned in that way by all of us; and *these extreme cases do not differ notably from their neighbors, nor can we always say which ones they are.* (RR 80, emphasis added)

In "Two Dogmas in Retrospect," Quine writes,

[M]y reservations over analyticity are the same as ever, and they concern the tracing of any demarcation . . . across the domain of sentences *in general*. The crude criterion in *Roots of Reference*, based on word learning, is no help; we don't in *general* know how we learned a word. . . . In short, I recognize the notion of analyticity in its obvious and useful but *epistemologically insignificant* applications. (TDR 271, emphasis added)

We conclude this section by assessing Quine's effort to temper the strong holism of "Two Dogmas" by reintroducing both a class of sentences – namely, observation sentences – that have independent empirical content and an epistemologically insignificant (or arbitrary) notion of analyticity. The issue is whether his new analytic-synthetic distinction permits Quine to cleave to moderate holism instead of strong holism.

We begin by discussing the first horn of Quine's strategy, that is, whether observation sentences have independent empirical content. At least two problems confront Quine's account of observation sentences. The first concerns their double life: being the repository of the empirical evidence of the theory (viz., they afford the theory empirical content) and being in a (dis)confirmation relation with the theory. On the one hand, if observation sentences are the independent empirical content of the theory (or, *mutatis mutandis*, the given that grounds the theory and anchors it to the world), then they cannot be theory relative (i.e., they must be theory independent or we will be driven toward epistemological nihilism or coherentism).[37] On the other hand, if observation sentences must enter in confirmation relations with other statements of the theory (and thus discharge the purpose for which they were introduced, to provide empirical evidence for the theory), then they enter into a confirmation relation with *all* statements of the theory, since (as Quine claimed in "Two

Dogmas" and never later denied) there is no principled way to distinguish confirmation relations that depend on the meaning of the words from those that depend on what the world is like. And if an observation sentence is confirmationally related to the *whole* theory, its empirical content cannot be theory-independent (i.e., observation sentences lack independent empirical content). Such was the conclusion of "Two Dogmas": Confirmation is immanent (not transcendent). Further, since Quine concedes that observation sentences are in a confirmation relation with the rest of the theory,[38] they cannot have independent empirical content.[39] So, we face the following dilemma: Either observation sentences are theory-free and hence have independent empirical content but confirm nothing except themselves or they are theory-laden and hence (dis)confirm the theory but lack independent empirical content.

Quine might try to resolve this dilemma by appeal to the double status of observation sentences: They are both theory-laden (taken analytically) and theory-free (taken holophrastically). This double status affords them a privileged position as a link between the *outside* and the *inside* of the theory. However, this dilemma is not so easily resolved. First, how can observation sentences both be the empirical content (and hence lack the rational relation with the rest of the theory) and at the same time be what (dis)confirms the theory (and hence be in a rational relation with the rest of the theory)? How does *the stimulation of sensory surfaces* to which observation sentences (holophrastically taken) are conditioned get translated into the *rational* evidence that (analytically taken) observation sentences express? Do these double-faced sentences have an interface?[40]

Second, even if we can make sense of the dual status of observation sentences, how is Quine's revised position essentially different from his position in "Two Dogmas"? There Quine claimed that, since no principled or "epistemologically significant" analytic-synthetic distinction is available, the possibility of revoking an observation sentence always exists. Quine did not disown this possibility in his later writings. Since he never recanted the unintelligibility of a confirmation-based analytic-synthetic distinction, and since he always admitted that, although only legalistically, Duhem's thesis holds for observation statements, dual status for observation sentences does not prevent the in-principle possibility of revoking an observation statement when it conflicts with a well-attested body

of theory. That in practice we do not usually revoke observation statements is insufficient to prevent the possibility of our doing so in principle. Admitting a legalistic possibility of revising an observation sentence is all one needs to deny that observation sentences have their content independently of the theory and hence to infer strong holism.

We move now to the second horn of Quine's strategy: introducing an arbitrary, or immanent, notion of analyticity. How does this position differ essentially from the one in "Two Dogmas"? There the denial of a principled criterion for analyticity in terms of apriority led Quine to impugn analytic truths (or statements immune to revision). However, reintroducing an arbitrary and domestic notion of analyticity (which, by Quine's own admission, does not reintroduce a principled, or "radical," analytic-synthetic distinction) does not prevent the in-principle revisability of even analytic statements. And in-principle revisability of analytic statements is all that one needs to infer strong holism.

In conclusion, introducing an *arbitrary* and *immanent* analytic-synthetic distinction does not moderate the strong holism of "Two Dogmas" because the claim in that essay that a principled or epistemologically significant analytic-synthetic distinction is unintelligible amounts to, or at least is compatible with, Quine's concession that there could be an immanent and arbitrary analytic-synthetic distinction. If Quine's strong holism in "Two Dogmas" is compatible with (and indeed equivalent to) the claim that confirmation and analyticity can only be immanent or arbitrary notions, appeal to them cannot do the work they were introduced for, namely, to moderate strong holism: *Only* an epistemological significant (or absolute) analytic-synthetic distinction can achieve this end.

Put differently, it is debatable whether Quine could have moderated his holism by introducing an arbitrary or immanent analytic-synthetic distinction. The main problem is his verificationism, that is, his view that what there is to meaning must be reconstructed in terms of verification or confirmation conditions. The strong holism implied by "Two Dogmas" results from Quine's argument that the analytic-synthetic distinction is untenable by verificationist standards – it lacks clarity according to a verificationist or confirmationist criterion. However, Quine *did* hold a verificationist view of meaning and never recanted the unintelligibility of a confirmation-based

analytic-synthetic distinction. It follows that if Quine had wanted to hold fast to his verificationism, he could have blocked the inference from verificationism to strong holism only by reintroducing an epistemologically significant, or confirmation-based, analytic-synthetic distinction. In other words, a domestic, language-immanent, analytic-synthetic distinction could not have done the work.

The purpose of these comments is to sensitize the reader to concerns about Quine's effort to temper the strong holism of "Two Dogmas." Although the literature tends to just take Quine's word for it and attribute a moderate holism to him, his move to moderate holism is not unproblematic.[41]

4. QUINE'S GAMBIT

The last section concluded that if Quine had wanted to hold fast to verificationism,[42] it is debatable whether he could have blocked the inference from confirmation holism to strong holism, despite what he claimed. Gibson (1988) once remarked that the only hope of overturning the position in "Two Dogmas" "must reside with the hope of overturning his behavioristic orientation towards language" (p. 101). This is also definitively a tendency in the literature.

Fodor and Lepore (1992) argue that one quick way to overturn Quine's strong holism in "Two Dogmas" is to reject his verificatonist or behavioristic views on meaning (p. 43). Moreover, they argue that, since Quine, in "Two Dogmas," rejects only an *epistemologically* based analytic-synthetic distinction, he rejects in that essay only the conjunction of the claims that some inferential relations are constitutive of semantic relations and that what they are can be determined by applying an epistemic criterion, such as apriority. However, meaning holism does not follow from the negation of this conjunction, unless one is verificationist about meaning (Fodor and Lepore 1992, 55–8). Quine only showed that meaning cannot be reduced to the inferences one is prepared to accept, and hence he leaves open the possibility of accounting for semantic facts, synonymy, and analyticity *nonepistemically*.[43]

Putnam (1986) endorses Quine's meaning holism (at the level of meaning that "is in the head") insofar as the character of our decision

of synonymy is "informal (and unformalizable)" (Putnam 1986, 424) – or insofar as "the sameness of meaning is the *reasonableness* of ignoring the difference in the psychological processes" (p. 419, emphasis added). However, because of his aversion to verificationism, Putnam does not infer meaning holism *tout court* from this meaning holism.[44] Putnam writes that "when two words have exactly the same extension we'll treat them as synonyms" (p. 419).

This antiverificationist wave, along with the search for a nonepistemic criterion of meaning identity, is motivated by the view that Quine has shown it is impossible to provide a criterion of meaning identity if meaning is epistemic (e.g., if meaning is identified with verification conditions) – or, *mutatis mutandis*, by the view that Quine is endorsing a strong holism. But this is not the sole reaction Quine's arguments have generated. Philosophers who are sympathetic to some kind of verificationism (or who identify meaning with use)[45] accept Quine's claim that the analytic-synthetic distinction is untenable but argue, in different ways, that, although there is no *principled* analytic-synthetic distinction, a *denatured* distinction, one somehow graded or contextually relativized, is not precluded by Quine's arguments.[46] The strategy is to block an inference from an epistemic conception of meaning (e.g., meaning as conceptual role) to strong holism by appeal to this denatured analytic-synthetic distinction. Problems remain, however, in articulating an analytic-synthetic distinction of this type.[47]

Our aim in this section has been minimal: namely, to provide a rough idea of the kind of discussion Quine's work has generated in the contemporary literature in semantics, though we have not provided anything like an exhaustive catalogue of Quine's influence in so many areas and among so many philosophers. We agree with Putnam that Quine was a philosopher of historic importance and that his work will continue to be the source of inspiration for generations of philosophers to come.

NOTES

1. See, e.g., Loar 1982 and Putnam 1986.
2. Meaning atomism has a long history. Exponents of some version or another range from Plato and Aristotle to the British empiricists and the

positivists up to contemporary informational semanticists like Fodor. What unites exponents is the common view that, so to speak, we must start with the simple and build up, that is, we must begin with a non-linguistic characterization of reference and then explain the meaning of more complex expressions. What differentiates exponents is how they specify the reference relation.

3. In defining 'meaning holism', we are deliberately unspecific. Holistic claims about meaning vary in kind (depending on the theorist's conception of meaning) and/or degree of strength (depending on the theorist's degree of commitment to and interpretation of the demise of the analytic-synthetic distinction). In particular, in between meaning atomism and strong versions of meaning holism lie moderate positions. We touch on the complications of this disjunctive picture – meaning atomism or meaning holism – and clarify what we mean by 'strong holism' as opposed to 'moderate holism' in §3, where we address the question whether Quine is to be interpreted as a moderate or strong meaning holist.

4. This text is considered Quine's manifesto of meaning holism.

5. See, e.g., Gibson 1982, Loar 1982, and Putnam 1986. The exposition of Quine's philosophy in Gibson 1982 is enthusiastically endorsed by Quine in its foreword.

6. As we mention in §4, this inference has been contested by Fodor and Lepore 1992.

7. Introducing Quine's holism from the more comprehensive context of his speculation on meaning and translation is good preparation for the later discussion of whether his holism should be interpreted as extreme or moderate.

8. See Wittgenstein's notion of outward criteria (Wittgenstein 1968, 580) and Dummett's manifestation principle (Dummett 1973, 1993b, and 1993c).

9. This is the setting for "radical translation," which is "the task of the linguist who, unaided by an interpreter, is out to penetrate and translate a language hitherto unknown" (WO 28).

10. In Quine's jargon, the native expression 'Gavagai' is first interpreted holophrastically (as a one-word sentence) and then analytically (word by word).

11. This defines Quine's notion of *observation sentence*. It is a social notion (see WO 45); i.e., the degree of observationality of a sentence is, in turn, defined in terms of agreement or disagreement on the part of well-placed observers. What renders an (occasion) sentence high in degree of observationality (i.e., what makes it an *observation* sentence) is the similarity in stimulus meaning (i.e., similarity in assent or dissent on

the part of well-placed observers); what makes it low in observationality is "wide intersubjective variability of stimulus meaning" (WO 45).

12. In *Word and Object*, Quine writes,

> We were impressed . . . with the interdependence of sentences. We may well have begun to wonder whether meanings even of whole sentences . . . could reasonably be talked of at all, except relative to the other sentences of an inclusive theory. Such relativity would be awkward, since, conversely, the individual component sentences offer the only way into the theory. Now the notion of stimulus meaning partially resolves the predicament. *It isolates a sort of net empirical import* of each of various single sentences *without regard to the containing theory, even though without loss of what the sentence owes to that containing theory*. It is a device . . . for exploring the fabric of interlocking sentences, a sentence at a time. (WO 34–5, emphasis added)

On p. 72, he refers to observation sentences as "independently translatable sentences," i.e., as sentences translatable by *independent* evidence of stimulatory occasions. So Quine, in *Word and Object* claims that observation sentences have theory-independent (or language-independent) empirical content. As we will see later in the chapter, this claim has its problems and conflicts with Quine's claims in "Two Dogmas."

13. That is, our devices of individuation, such as plural endings, pronouns, numerals, the 'is' of identity, and its adaptations 'same' and 'other' (see WO 52–3 and OR 32–3).

14. See "Ontological Relativity": "[The radical translator abstracts] native particles and constructions from observed native sentences and tries associating these variously with English particles and constructions" (OR 33). See also WO 70.

15. As is well known, Quine holds that radical translation shows that meaning is indeterminate, not only at the intensional level but at the extensional level as well. Indeterminacy of meaning (in any intuitive sense of 'meaning' different from reference) is on a par with the inscrutability of reference (or ontological relativity). As Quine likes to put it, the indeterminacy of translation "cuts across extension and intension alike. . . . Reference itself proves behaviorally inscrutable" (OR 35), since the words 'rabbit' and 'undetached rabbit part' not only differ in meaning but are also true of different things in the world.

16. It is worth reminding the reader that according to Quine the 'gavagai' example, although artificial, illustrates real linguistic phenomena (e.g., the case of the Japanese classifiers; see OR 36). Moreover, according to Quine, radical translation begins at home (OR 47–8).

17. For an example of how Quine's speculation on radical translation comes together with his rejection of the analytic-synthetic distinction in "Two Dogmas" (Quine's manifesto on meaning holism), see pp. 61–8 of *Word*

and Object. There Quine ties together his speculations and conclusions on stimulus synonymy with those of analytic sentences and the analytic-synthetic distinction in "Two Dogmas."

18. The dogma of the analytic-synthetic distinction is that there is a fundamental distinction between analytic sentences (statements true in virtue of the meanings of their component words alone, i.e., independently of experience) and synthetic statements (statements whose truth is grounded in experience). According to Quine, the two dogmas are at root one because as long as it is taken to be significant to speak of the confirmation and infirmation of one statement in isolation from all the others, it seems significant to speak also of a limiting kind of statement confirmed come what may.

19. We are aware that one may disagree with both our points – that the moral of Quine's speculation on radical translation coincides with, or finds support in, Duhem's confirmation holism and that Quine's writings on translation *echo* "Two Dogmas" – because Quine, in *Word and Object*, grants observation sentences a special status that is denied them in "Two Dogmas," which suggests a more moderate confirmation holism than Duhem's. However, as we argue in §3, since we are doubtful that observation sentences can be consistently given such a special status, we don't see any essential difference between "Two Dogmas" and Quine's writings on translation.

20. Although indeterminacy of meaning, meaning holism, and rejection of the analytic-synthetic distinction are essentially correlated in Quine's view, they need not be correlated. Their interdependence seems to be inevitable for philosophers who, like Quine, cleave to an epistemic conception of meaning, but a realist about meaning can accept Quine's rejection of the analytic-synthetic distinction without ipso facto buying into the indeterminacy of meaning or into meaning holism.

21. Gibson (1982) notes, correctly in our view, that if pressed to support confirmation holism "beyond merely appealing to Duhem . . . , Quine would call attention to the way (theoretical) language is learned" (p. 106) and that Quine views the situation of a radical translator learning the native's (theoretical) language as the same (from an epistemic point of view) as that of a child learning the theoretical language.

22. Since accepting meaning holism is often seen to follow from agreeing with Quine about the analytic-synthetic distinction, assessing what brand of holism his arguments support is important not only as a matter of exegesis but also for assessing the fortune of the doctrine itself. If, on the one hand, we take seriously Quine's claim that there is no principled analytic-synthetic distinction, strong holism seems to follow, and with it the deleterious consequences it implies. (For such consequences,

see Fodor and Lepore 1992 and Dummett 1991, 1993b, and 1993c). If, on the other hand, we interpret Quine as still committed to *some kind* of analytic-synthetic distinction, a moderate holism follows that can circumvent difficulties raised by a strong version of holism.

23. "The relativity of observation sentences to the rest of the language would be odd indeed" since the empirical meaning of observation sentences is a "device . . . for exploring the fabric of interlocking sentences" (WO 35).

24. We presently discuss in greater detail Quine's defense of moderate holism.

25. A moderate holism much along the lines of Dummett's own molecularism. See Dummett 1993c, 44–5.

26. Dummett explains as follows: "[A]n analytic sentence will be one the assignment to which of the value true will be untouched by any admissible revision made in response to a recalcitrant experience; a synthetic sentence will be one the assignment to which of the value true will be overturned by any admissible revision made in response to certain possible experiences" (Dummett 1993c, 44–5). See Dummett 1973, 592; 1993c, 71.

27. This theory "quite rightly characterized as holism" is the "Global Holism" Dummett identifies with the Quine-Duhem thesis and harshly criticizes in Chapter 10 of *The Logical Basis of Metaphysics* (Dummett 1991). For a considerate account of Dummett's interpretation of Quine's holism see Shieh 1997.

28. See, e.g., EES, FME, RHB, RGH, RRN, RHP, RJV, PTa, and TDR.

29. See, e.g., RHP, PTa, and POS.

30. By 'observation categorical' Quine means a generalization of the form 'Whenever this, that', where 'this' and 'that' are observation sentences. See PTa 9–11.

31. See, also, TDR on this.

32. See, also, RR 78, RHB 94–5, and RGH.

33. Putnam (1983) makes this point. See also Fodor and Lepore 1992, Gibson 1988, and Lepore 1995. See, also, RGH, where Quine writes that the dogma of reductionism "creates a need for analyticity as a key notion of *epistemology*, and that the need lapses when we heed Duhem" (p. 207).

34. That is, 'is analytic' has a clear use only as 'analytic in English' or 'analytic in Italian' but not across languages. See TDR and Lepore 1995. This clarifies why many criticisms of Quine's attack on analyticity miss his point in "Two Dogmas." Searle 1969, Grice and Strawson 1971, and Putnam 1975 all argue against Quine that 'analyticity' is clear because users agree on its *application*. But Quine never denied that 'analyticity' is intelligible in its domestic/immanent/arbitrary sense. For a criticism

of Grice and Strawson 1971, see Gibson 1988, 86–93, and for a criticism of Grice and Strawson 1971, Putnam 1975, and Searle 1969, see Lepore 1995, 473.

35. To the objection that Quine believes observation sentences, taken holophrastically, *transcend* theory-relative confirmation, we note the following: (1) Quine introduced holophrastically construed observation sentences that wear their empirical content on their sleeve *after* "Two Dogmas" in order to moderate the strong holism suggested by "Two Dogmas," and (2) it is not clear whether observation sentences holophrastically construed can *really* do what they are introduced to do: that is, moderate strong holism by introducing a language-independent notion of empirical content by which language is anchored to the world. We discuss (2) presently.

36. The epistemological insignificance of the analytic-synthetic distinction explains why this move is compatible with Quine's claims in "Two Dogmas" and hence why he never recanted that essay's arguments for meaning holism.

37. That Quine distinguishes his view from epistemological nihilism is clear in "Epistemology Naturalized," and he distances himself from coherentism in "Reply to Herbert G. Bohnert."

38. As he puts it, analytically taken, observation sentences are theory-laden.

39. Quine implicitly admits this by saying that "Duhem's thesis still holds [for observation sentences] in a somewhat literalistic way" (EES 314). See, also, *Word and Object*: "[The notion of stimulus meaning] isolates a sort of net empirical import of each of various single sentences without regard of the containing theory *even though without loss of what the sentence owes to that containing theory*" (WO 34–5, emphasis added).

40. The problem of how Quine can account for a concept of experience (or empirical content) that rationally justifies a system of beliefs while remaining outside the order of justification has been emphasized by McDowell 1994 and is already implicit in Davidson's 1984 and 1986 criticisms of Quine's commitment to a third dogma of empiricism.

41. Quine's interpreters disagree on whether Quine held a strong holism or a moderate holism. Putnam 1986, Fodor and Lepore 1992, and Lepore 1995, for example, characterize Quine's holism as strong. Dummett, as we have seen, acknowledges the tension within the corpus of Quine's writings between strong and moderate holism. However, Dummett (1991) attributes to Quine the profession of an "inextricability thesis," which has nothing to do with (strong) holism (pp. 242–4). Davidson's attribution of moderate holism to Quine (in Davidson 1984 and 1986) is implicit in his regarding Quine as committed to the third dogma of

empiricism (the dualism of language and empirical content). Note, however, that the incoherence of Quine's moderate holism (and of any endorsement of a dualism that separates language and empirical content) is pointed out by Davidson in both Davidson 1984 and 1986.

42. And he *did*, since his verificationism is directly related to his naturalism. See §2.

43. A third line of attack on Quine's strong holism does not have to do with the assumption of verificationism. For the sake of argument, Fodor and Lepore 1992 grant the truth of verificationism and attack instead the *validity* of Quine's argument for meaning holism as presented in "Two Dogmas." In particular, they claim that "even the conjunction of confirmation holism and verificationism is compatible with the denial of [meaning] holism" (p. 43) since meaning holism does not follow from the truth of the premises owing to an unavoidable equivocation in the word 'statement' in the premises (see pp. 41–54). For a response to the Fodor and Lepore 1992 criticism that Quine's defense of meaning holism in "Two Dogmas" is fallacious, see Harrell 1996 and Okasha 1999.

44. Putnam departs from Quine by not buying into the inscrutability of reference thesis.

45. We have in mind, in particular, conceptual role semanticists.

46. See, e.g., Block 1994, 95: "Without an analytic-synthetic distinction, we would have to move to a scientific conception of meaning that does away with the crude dichotomy of same/different meaning in favor of a multidimensional gradient of similarity of meaning."

47. See Fodor and Lepore 1992, chaps. 1 and 6.

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LARS BERGSTRÖM

4 Underdetermination of Physical Theory

INTRODUCTION

Our theories of the world are related in various ways to experience. We construct theories partly in order to account for what we have observed and partly in order to systematize and support our expectations for future experience. But what we experience is not sufficient to determine our theories. Different theories may account for our observations equally well. This, roughly speaking, is the thesis that physical theory is underdetermined. W. V. Quine has formulated the idea in different ways in different contexts.

However, before we consider Quine's formulations, let us look at the following passage from an address delivered by Albert Einstein on the occasion of Max Planck's sixtieth birthday in 1918:

The supreme task of the physicist is to arrive at those universal elementary laws from which the cosmos can be built up by pure deduction. There is no logical path to these laws; only intuition, resting on sympathetic understanding of experience, can reach them. In this methodological uncertainty, one might suppose that there were any number of possible systems of theoretical physics all equally well justified; and this opinion is no doubt correct, theoretically. But the development of physics has shown that at any given moment, out of all conceivable constructions, a single one has always proved itself decidedly superior to all the rest. Nobody who has gone deeply into the matter will deny that in practice the world of phenomena uniquely determines the theoretical system, in spite of the fact that there is no logical bridge between phenomena and their theoretical principals.[1]

In the first half of this passage, Einstein seems to say that physical theory is underdetermined by experience or observational evidence; in the second half, he seems to say that physical theory is

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nevertheless uniquely determined by "the world of phenomena." Presumably, the term 'phenomena' stands for what we directly observe or experience. Hence, there appears to be a tension between the first and the second parts of Einstein's pronouncement.

It is hard to see how it can be shown in practice that a given theoretical system is superior to every *conceivable* rival, even relative to a given moment. This may be doubted even if there is always one system that is clearly better than its *actual* rivals. Neither can we assume that the tension in Einstein's pronouncement disappears if we distinguish between actual and possible experience. At any given moment, our data consist of (a subset of) the actual observations of mankind up to this moment. Einstein's point is not that these actual data are insufficient – while all possible data might be sufficient – to determine the theoretical system. On the contrary, he seems to be saying that actual data are sufficient, at least in practice.

Quine suggests, on the other hand, that not even all possible data are sufficient. His claim is that "scientific theory is underdetermined by all possible data; in other words, that different theories can be empirically equivalent" (RA 294). A somewhat earlier formulation is this:

[P]hysical theory is underdetermined even by all *possible* observations.... Physical theories can be at odds with each other and yet compatible with all possible data even in the broadest sense. In a word, they can be logically incompatible and empirically equivalent. (RIT 178–9)

One puzzling feature of these formulations is that they seem to equate empirical equivalence and compatibility with all possible data. Surely, empirically equivalent theories may be incompatible with certain data. They need not be empirically viable. In a later context, Quine said that "it is a poor idea to assume compatibility with all possible data.... What matters is that the theories be empirically equivalent" (CB 53).

One might suppose that different theories are empirically equivalent if they have the same empirical content. If so, one version of the underdetermination thesis would be that theories are underdetermined by their empirical content. One and the same empirical content can be embedded within different theoretical superstructures. In Quine's words, "there are alternative hypothetical substructures that would surface in the same observable ways" (EES 313).

The formulations presented so far may give a sense of what the underdetermination thesis is about. But more needs to be said. For example, more needs to be said about the interpretation of key terms such as 'theory', 'empirical content', and 'empirical equivalence'.

THEORIES

When Quine speaks of 'theories', what he has in mind are usually theory formulations (see, e.g., EC 24 and PTb 96). A theory formulation "is simply a sentence – typically a conjunctive sentence comprising the so-called axioms of the theory" (EES 318). Obviously, different theory formulations may be logically equivalent, and it should come as no surprise that logically equivalent theories are also empirically equivalent. Similarly with different theory formulations where one is merely a translation of the other. The underdetermination thesis goes beyond this. The relevant kind of difference between the theory formulations in question is that we cannot convert one into a formulation logically equivalent to the other by reinterpretation sentence by sentence (EES 320, PTb 97). Let us say that theories that are different in this sense are (mutually) *irreconcilable*. Theories that are irreconcilable cannot be regarded as mere notational variants of one and the same theory.

Quine sometimes says that physical theory is underdetermined, but in other places he says that physical theories – in the plural – can be underdetermined. He is sometimes taken to hold that *all* theories, or all physical theories, are underdetermined, but this interpretation should be rejected. Thus, in a reply to a paper by W. H. Newton-Smith, Quine writes the following:

He [Newton-Smith] begins with a reference to "Quine's notorious claim that . . . *all* theories are underdetermined. . . . " I conjectured that physical theory, the global system of the world, is underdetermined but not that every subordinate system was underdetermined. (CNS 66)

This indicates that Quine takes the underdetermination thesis to apply primarily to our global system of the world. One gets the same impression from several other passages. Sometimes he suggests that a "global theory" or "system of the world" is a theory that can account for all observable events (see, e.g., EES 313 and 327). However, if the thesis is restricted in this way, it is perhaps less interesting, since we

might never come across a theory that is global in this sense. Rather I think we should take the underdetermination thesis to apply to theories that are global in the sense that they formulate the totality of someone's (explicit or implicit) beliefs at some time about the world. Such theories also include large parts of mathematics, "for mathematics infiltrates all branches of our system of the world" (PTb 15).

At one point, Quine asks us to imagine "an exhaustive encyclopedic formulation of our total scientific theory of the world" (EC 28). Such a formulation would be a global theory in the relevant sense. This is what Quine refers to by the phrase 'physical theory'; he is not merely concerned with theories of phenomena that are professionally studied by physicists. The underdetermination theory says that global theories are underdetermined. However, there is no need to exclude theories that can account for all observable events but are never formulated or believed. Let us also call these 'global'. We may recall that Einstein was concerned with possible "systems of theoretical physics," systems that contain those "universal elementary laws from which the cosmos can be built up by pure deduction." If the cosmos contains everything that exists or happens in the world, such systems should also be regarded as global in what I take to be Quine's sense. But maybe Einstein was thinking of physical theories in a narrower sense.

However, I suggest that the underdetermination thesis should also be taken to concern less global theories. I think this is in accordance with Quine's intentions. He sometimes gives examples of underdetermined theories – such as "Riemannian and Euclidean geometry as applied to the surface of a sphere" (PTb 96) – that are certainly not global. I believe his claim is that such "subordinate" theories can also be underdetermined. As indicated by several of the quotations given above, I believe his underdetermination thesis says two things: that our global theory of the world *is* underdetermined and that other, more subordinate theories *can* be underdetermined.

EMPIRICAL CONTENT

Intuitively, the empirical content of a theory is what the theory says or implies about observable features of the world. Observable features of the world are described by means of observation sentences.

According to Quine, an observation sentence for a given speech community is an occasion sentence that is directly and firmly associated with sensory stimulations for every member of the community and on which all members give the same verdict when witnessing the same situation (PTb 3). (Clearly, this is an idealization. People are sometimes mistaken about the truth-value of observation sentences. Possibly, no sentence satisfies Quine's definition, strictly speaking.)[2] Examples of observation sentences are 'It's cold', 'That is a dog', 'This is a flower'. Most observation sentences report physical things and events, but some – for example, 'Tom perceives a dog' – are mentalistic (PTb 62).

Observation sentences are occasion sentences; that is, they are true on some occasions and false on others. Therefore, they cannot be implied by scientific theories, which are either true or false once and for all. However, two observation sentences can be combined into a general sentence of the form 'Whenever this, that'. An example would be 'Whenever there is a raven, it is black', or simply 'All ravens are black'. Such sentences are called *observation categoricals*; they are true or false once and for all, and they can be implied by scientific theories. An observation categorical is synthetic for a given speaker if the stimulations associated with the antecedent are not completely included among the stimulations associated with the consequent. Synthetic observation categoricals can be tested in experiments. Two observation categoricals are synonymous for a speaker if their respective components are associated with the same stimulations. The empirical content of a theory for a given speaker consists of the set of synthetic observation categoricals implied by it, plus all synonymous ones. Moreover, Quine says that two theories are empirically equivalent for a given community if they have the same empirical content for each member (PTb 16–17). Presumably, the empirical content that is common to the two theories can be different for different speakers. Therefore, it is unclear what would be the empirical content of a theory for a given community. Maybe we could say that it is the union – or the intersection? – of the empirical contents for all members.

Notice that, on this account, the empirical content of a theory is relative to speakers of communities. It seems that it is also relative to times, since the stimulations associated with an observation sentence for a speaker can be expected to vary from time to time.

Quine claims to have defined empirical content only for testable theories, that is, for theories that imply some synthetic observation categoricals (PTb 16, 95). But he would probably allow for the possibility that some untestable theories have empirical content. He simply says that he has no definition of empirical content to offer for such theories (PTb 95). Maybe the idea is that an untestable theory can somehow *contribute* to the empirical content of some more comprehensive theory of which it is a part and that this gives an empirical content even though it does not by itself imply any observation categoricals.[3] But if theories that do not imply any synthetic observation categoricals can have empirical content, it seems rather strange to say that the empirical content of a testable theory is exhausted by the set of synthetic observation categoricals implied by it (plus all synonymous ones). If untestable theories can get empirical content in some other way, it seems reasonable to assume that the same is true for testable theories. But then the empirical content of a testable theory should not be defined in the way Quine suggests.

EMPIRICAL EQUIVALENCE

Quine sometimes seems to identify empirical equivalence and sameness of empirical content. However, he notes that "much solid experimental science fails of testability in the defined sense" (PTb 95). Therefore, he also has a more general definition of empirical equivalence, according to which two theories are empirically equivalent when "whatever observation would be counted for or against the one theory counts equally for or against the other" (PTb 96). As far as I can see, there is no guarantee that testable theories that have the same empirical content are also empirically equivalent in this general sense.

Let me try to illustrate this by means of an example. Let R be the general theory of relativity. I take it that R is not by itself testable in Quine's sense. A lot of other theories and auxiliary hypotheses, including mathematical ones, are needed to derive synthetic observation categoricals. If A is the conjunction of the auxiliary theories, the compound theory RA is testable. (RA is the conjunction of R and A.) Now let C be Gödel's axiom of constructibility, '$V = L$', which says that all sets are constructable. I assume that C is not implied by RA; it is not needed to derive observation categoricals from general

relativity and it is not part of ordinary set theory. If C is added to RA we get RAC. Presumably, RA and RAC have the same empirical content (for the scientific community at a given time). C does not add anything to the empirical content of RA. But we would probably not say that whatever observation would be counted for or against RA would count equally for or against RAC. Presumably, RA and RAC are empirically equivalent in one of Quine's senses but not in the other.

Let me explain. I assume that RA and RAC are empirically equivalent in the sense that they imply the same synthetic observation categoricals. Then there are three different possibilities: (1) Either RA is a better theory than RAC by our scientific standards. Maybe it is better because it is "simpler." If so, we may say that RA is a better explanation of available data and that, as far as we can see, whatever observation would be counted for RA counts much less or not at all for RAC. (2) Or else RAC is a better theory than RA, perhaps because there are mathematical or philosophical reasons for adding Gödel's axiom to the mathematical parts of A.[4] If so, we might say that RAC is a better explanation and that whatever observation counts for RA counts more for RAC. In cases (1) and (2), RA and RAC are not empirically equivalent in Quine's more general sense. But there is also a third possibility. (3) RA and RAC are equally good theories according to our scientific standards. If so, we have a case of underdetermination. We have two irreconcilable theories that are empirically equivalent – not merely in the sense that they imply the same synthetic observation categoricals but in the general sense that "whatever observation would be counted for or against the one theory counts equally for or against the other." In Einstein's words, the two theories are "equally well justified."

I do not think that (3) is a very realistic alternative. I believe that (1) is the actual case. It also seems to me that (1) is not a very interesting case of underdetermination. After all, in case (1), RA is a better explanation than RAC of the available data. Consequently, sameness of empirical content is not the most interesting kind of empirical equivalence. Quine's general sense of 'empirical equivalence' is more relevant in the present context. Another illustration of this point is where two theories imply the same synthetic observation categoricals but one of them fits much better than the other into a larger system of theories for which there is independent empirical support.

That such cases may occur is one reason Quine often limits his discussion to global theories, "so that there is no question of fitting the rival theories into a broader context" (PTb 98). He seems to agree that such cases do not exemplify underdetermination in his sense.

Now, since the relevant sense of 'empirical equivalence' makes empirical equivalence depend on the specific scientific virtues that make some theories better than others, something needs to be said about these and about the notion of scientific value.

SCIENTIFIC VALUE

Quine says that his general sense of 'empirical equivalence' is "ill-defined" (PTb 96). The reason, presumably, is that we have no precise standards of empirical justification or scientific value. In one place, Quine says that scientific method "is a matter of being guided by sensory stimuli, a taste for simplicity in some sense, and a taste for old things" (WO 23). This is rather vague. In another context, he lists the following virtues that scientific hypothesis can have in varying degrees: conservatism, modesty, simplicity, generality, refutability, and precision (WB 66–79, 98). Other philosophers have mentioned the same or similar scientific virtues. For example, Thomas Kuhn says that

five characteristics – accuracy, consistency, scope, simplicity, and fruitfulness – are all standard criteria for evaluating the adequacy of a theory.... Together with others of much the same sort, they provide *the* shared basis for theory choice.[5]

Quine seems to regard his virtues as means to predictive efficacy (see WB 135), but he also says that prediction is not the main purpose of science. "One major purpose is understanding. Another is control and modification of the environment" (PTb 2). Kuhn, on the other hand, regards his five characteristics more as constitutive of the goal or purpose of science.[6]

It would be generally admitted, I believe, that there is no algorithm for determining the scientific value of a theory – that is, the degree to which the theory has the good-making characteristics of a good theory.[7] But most philosophers of science would agree that the scientific value of a theory depends on features of the kind mentioned

by Quine and Kuhn. The terms used to indicate such features are vague and ambiguous. Moreover, competent scientists may disagree about the relative importance of the various features and about the way they should be aggregated in particular cases. In practice, different opinions within the scientific community may usually tend to converge, in due course, toward a common verdict. But this does not show that the common verdict is correct in an objective sense. Presumably, in many cases there is no fact of the matter independent of the very verdict arrived at by scientists. Rather, a consensus within the scientific community that one theory is better, or simpler, than another can be explained (within limits) by reference to ordinary group-psychological mechanisms. As long as a group of people believe that there is a correct answer to a question, they tend to work their way to a consensus, even if there is no sufficient evidence one way or the other, and even if many different answers are in fact equally possible. Consider, for example, the classic experiments with the so-called autokinetic effect. When a spot of light is projected in a totally darkened room, it will appear to move, but different individuals will perceive very different movements. However, when they are asked to describe the movement of the spot to the other members of a group, their judgments soon tend to converge toward a group norm. It seems that a consensus concerning the overall scientific value of a theory may arise in a similar way.

   With reference to considerations of this kind, I suggest that there is often no fact of the matter as to whether two theories are "equally good" or "equally justified." Consequently, there is often no fact of the matter as to whether two theories are empirically equivalent in Quine's general sense either. This in turn leaves room for another psychological mechanism.[8] Scientists can be expected to dislike situations in which two irreconcilable theories are equally good. Such a situation would make scientific life more difficult. Therefore, whenever a situation of this kind appears to obtain, especially if the two theories also appear to be logically incompatible, scientists can be expected to make it their business to show that one of them is better after all. And they will not give up until they succeed (by their own lights). Indeed, this may explain Einstein's observation that "out of all conceivable constructions, a single one has always proved itself decidedly superior to all the rest."

But if this is right, it undermines Quine's underdetermination thesis. The relevant kind of empirical equivalence can hardly be expected to obtain. Quine says that

we have no reason to suppose that man's surface irritations even unto eternity admit any one systematization that is scientifically better or simpler than all possible others. It seems likelier, if only on account of symmetries and dualities, that countless alternative theories would be tied for first place. (WO 23)

As far as I can see, this is not likelier. Rather, it seems very unlikely. Empirical and methodological considerations can never force the scientific community to conclude that two theories are tied for first place, and psychological mechanisms can be expected to work against such a conclusion. Moreover, there is no fact of the matter about which the scientific community would then be mistaken.

WEAK UNDERDETERMINATION

However, at this point we may return to Quine's other notion of empirical equivalence: sameness of empirical content. In order to avoid the problem that 'empirical content' is only defined for testable theories, let us restrict ourselves to such theories. In particular, let us focus on global theories. Let us also presuppose a certain community and time in order to bypass the relativity of empirical content.

The underdetermination thesis might now be taken to say that irreconcilable global systems of the world may have the same empirical content. However, this is not a very interesting claim. Thus, for example, if G is our global system of the world (at a given time) and C is Gödel's axiom of constructibility, then presumably G and GC have the same empirical content and are irreconcilable (I assume that C is not implied by G). Yet they are not different in a very interesting way. They have too much in common theoretically.

One might require that the theories in question are *tight* in the sense that they are not the result of adding to another theory some theoretical sentence that does not affect the empirical content or the explanatory power of the original theory.[9] Thus, for example, it might be argued that G and GC are not serious rivals in the sense of the underdetermination thesis since GC is not a tight theory.

However, I suggest that a better alternative is to say that under-determination is a matter of *degree*. Even in the case of *G* and *GC* there is a certain small amount of underdetermination, but this is almost negligible. More interesting cases of underdetermination are those in which theories with the same empirical content not only are irreconcilable – in the sense that we cannot convert one into the other by reinterpretation sentence by sentence, which is the sense Quine has in mind – but also have theoretical contents that are "very different." This is certainly vague, but that should not be a big problem if underdetermination is a matter of degree. In general, we may say that the degree to which a given theory is underdetermined is the degree to which it is theoretically different from irreconcilable theories with the same empirical content.[10] It may not be an easy task to define a plausible and precise scale on which the theoretical difference between theories can be measured, but I think the intuitive idea is clear enough.

Now, if our global system of the world has the same empirical content as some (possibly unknown) system that is theoretically very different from ours, this fact would certainly be interesting. It would be interesting even if one of the systems – ours, for example – seems to us to be scientifically better or simpler than the other. Let us call this *weak underdetermination*. This is probably not the kind of underdetermination Quine has in mind most of the time, but it is perhaps just as interesting. If I am right, it may even be more interesting because it is more realistic. Also, the higher the degree to which a given theory is underdetermined, the more interesting is the fact that it is underdetermined. Or so it seems.

LOGICAL INCOMPATIBILITY

Is it possible that irreconcilable theories with the same empirical content are logically incompatible? We have seen that Quine holds that physical theories "can be logically incompatible and empirically equivalent" (RIT 179). On the other hand, Michael Dummett has argued that this claim "is absurd, because there could be nothing to prevent our attributing the apparent incompatibility to equivocation."[11] In 1975, Quine "took up the question of equivocation" (see CNS 67), but even then he seems to have believed that some incompatibility

may remain when the idea of equivocation has been fully exploited (EES 326–7). However, in later works, under the influence of Donald Davidson, he seems to have come closer to Dummett's position. The argument runs as follows. If two incompatible theory formulations have the same empirical content, the incompatibility can only concern purely theoretical sentences. For example, suppose that one theory says that neutrinos have mass and the other theory says that neutrinos do not have mass.[12] This looks like a contradiction, but the incompatibility can easily be avoided if we assume that the word 'neutrino' does not have the same meaning in the two theories; to mark the difference we may even change the spelling to 'neuttrino' in one of the theories (EC 29, PTb 97–8). Of course, we may also assume that the word 'mass' has different meanings in the two theories. Other apparent incompatibilities are treated in the same way.

So far, so good. But can we also rule out the possibility that the words 'neutrino' and 'mass' do have the same meanings in the two theories so that there is a real incompatibility? Is it "absurd" to assume that the two theories are logically incompatible, as Dummett says? It seems that Quine would not go this far. In 1990 he gave an example of empirically equivalent theories that are incompatible:

An example is Poincaré's, is which he contrasts our common-sense infinite space and familiar rigid bodies with a finite space in which those bodies shrink as they move away from center. The two theories are clearly empirically equivalent but logically incompatible.[13] (CB 53)

He went on to say that "in case of incompatibility we can shift to a theory formulation that operates in the same way and is compatible." Still, it seems that, in Quine's view, empirically equivalent theories can be logically incompatible. Another example of this might be GC and $G \neg C$, that is, our global system of the world in conjunction with the constructibility axiom and with its negation, respectively. Surely, it is very natural to think of GC and $G \neg C$ as incompatible theories.

However, from a Quinean point of view, there appears to be another argument that might be used to show that GC and $G \neg C$ are *not* incompatible. Of course, they are syntactically incompatible, but they are not incompatible in the sense that both cannot be true. The reason is that the meaning of theoretical sentences is immanent or relative to a theory. Quine writes, "Unless pretty firmly and directly

conditioned to sensory stimulation, a sentence S is meaningless except relative to its own theory; meaningless intertheoretically" (WO 24). Therefore, one might conclude, the sentence C does not have the same meaning in GC and in $G \neg C$, and similarly, for that matter, with G. Consequently, the two theories are not logically incompatible (in the sense that both cannot be true). In Dummett's words, the claim that they are empirically equivalent and logically incompatible "is absurd."

This argument is too legalistic. In some sense, and to some extent, a sentence may be meaningless except relative to its own theory, but this need not make it unintelligible from the point of view of other theories. Suppose I accept GC and you accept $G \neg C$. If we want to understand one another, each of us must devise a translation manual from the other's idiolect into his own in order to understand the other's theory. Surely, it would be very natural for me to use a manual that would translate $G \neg C$ in your idiolect as $G \neg C$ in my idiolect. In other words, I would assume that we use language in the same way. If this translation manual works – if it leads to "fluency and effectiveness of dialogue" (PTb 59) – then it is okay. Nothing more can be asked for, and relative to this translation manual the two theories are indeed logically incompatible. In fact, even if the manual does not work, by applying it I have formulated a theory in my own language that is incompatible with my theory. Surely, there is nothing absurd about this.

In principle, however, there may be other translation manuals from your idiolect to mine. For some such manual, $\neg C$ in your idiolect is compatible with C in mine. For example, according to such a manual, the word 'set' in your idiolect does not mean the same as 'set' in mine. Consequently, neither does G mean the same for you and me. Presumably, the word 'set' is connected to a lot of mathematical and set-theoretic sentences in G. But if these sentences do not mean the same for you and me, and if my translation of $\neg C$ in your idiolect is compatible with C in mine, presumably our dialogue is *not* fluent and effective. The reason is that my translations of simple sentences in your idiolect would quite often be very complicated. I might simplify such translations by defining new terms in my idiolect, but this would complicate my vocabulary. Alternatively, I might coin new undefined words in my idiolect as translations of some of your words – for example, I might translate your word 'set' as 'sett', which

is a new word in my idiolect – but it seems to me that this is really no translation, since I do not use the word 'sett' except as a translation of your 'set'. So maybe this kind of translation manual is not acceptable. If so, Quine's way of getting rid of incompatibilities is perhaps not so unproblematic after all.

A more interesting case is where we come across a global system that is *very* different from ours and where, according to our translation manual, the two systems have the same empirical content and are irreconcilable. In such a case, it would be tempting to question the assumption that shared terms mean the same in the two systems. The reason is that a theoretical term gets at least some of its meaning holistically from its use in the theory to which it belongs. In other words, we may assume that shared terms are ambiguous in the way suggested by Quine. If there are any syntactic incompatibilities, they should not be taken seriously. Perhaps it would even be absurd to stick to a translation manual that makes the other theory incompatible with but empirically equivalent to ours. Besides, the so-called principle of charity (WO 59, n. 2) might be taken to count against such a manual.

Nevertheless, such a translation manual might exist, and it is not obvious that it must violate the requirement of fluent and effective dialogue. I suggest that it is still an open question whether there could be interesting cases of alternative global systems that have the same empirical content and are logically incompatible. However, cases involving incompatibilities like that between GC and $G \neg C$ are not interesting enough.

SCEPTICISM

Quine says that cases of incompatible systems can always be "reduced" to cases of compatible systems by the method mentioned earlier (PTb 97). Maybe so. But if the systems in question are incompatible, they must already be formulated in one and the same language.[14] The "reduction" consists in an extension of the vocabulary of the language and the creation of a new theory formulated in the extended language. However, the reduced theory is not reduced away. The original systems are still incompatible.

If the two original systems are also empirically equivalent in the sense that "whatever observation would be counted for or against

the one theory counts equally for or against the other," it seems that everything that counts for one of the systems counts equally *against* the very same system – since it also counts for an incompatible system. In this way, underdetermination seems to lead to scepticism.

Quine attempts to avoid scepticism by insisting that truth is immanent to our system of the world. Given the situation in which some system of the world is empirically equivalent but incompatible with ours, Quine asks, "Can we say that one [of the systems], perhaps, is true, and the other therefore false, but that it is impossible in principle to know which?" His answer is that this would be a mistake; rather "it would be our place to insist on the truth of our laws and the falsity of the other theory where it conflicts." The reason is that, on his naturalistic view, "there is no extra-theoretic truth, no higher truth than the truth we are claiming or aspiring to as we continue to tinker with our system of the world from within" (EES 327).

But it seems to me that there are really two different questions here and that Quine does not keep them clearly apart. One question is whether we should call our system (or the other system) true. The other is whether we can know that our system is true. Quine answers the first question thus: Our system is the system we believe is true. As long as we believe that it is true, we should call it true. But if we also believe that all our evidence for this system is equally good evidence *against* it, perhaps we should conclude that we do not *know* that our system is true. If underdetermination is unavoidable, we might also conclude that it is impossible to know which system is true.

However, this sceptical conclusion presupposes that the incompatible systems in question are empirically equivalent in Quine's general sense. As I have argued above, this presupposition is unwarranted.

Besides, even if our system of the world is incompatible with some empirically equivalent system, the two systems may have a lot in common. If so, there is no need to adopt a sceptical attitude toward the common part.[15] For example, if the systems are our old friends GC and $G \neg C$, the incompatibility argument should not make us skeptical of G. If both systems are equally coherent, we may have good reasons to accept G but no good reasons to either accept or reject C. To the extent that there are more interesting cases of incompatible and empirically equivalent systems, we may also be entitled to more

interesting degrees of scepticism. But as I argued earlier, the existence of such cases is dubious.

ECUMENISM AND SECTARIANISM

On the other hand, it seems entirely plausible to assume that logically compatible but quite different global systems might have the same empirical content (relative to us now) as our own global system of the world. If so, one might find it reasonable to regard all such systems as equally true. In Quine's words,

> If we subscribe to one of them as true, we can call them all true and view them as different descriptions of one and the same world. We are no strangers, after all, to strange languages. If this be relativism, make the most of it. (RA 295)

This is an expression of Quine's *ecumenic* stance (RG 156). In other passages, as we saw earlier, he adopts instead a *sectarian* position, according to which we should regard our own system as true and the other systems as false or meaningless. For example,

> we should indeed recognize [all the systems] as equally well *warranted*. We might even oscillate between them, for the sake of a richer perspective on nature. But we should still limit the ascription of truth to whichever theory formulation we are entertaining at the time, for there is no wider frame of reference. (EC 29)

Quine is both an empiricist and a naturalist. He claims that his empiricism motivates the ecumenic position whereas naturalism motivates sectarianism (RG 156, PTb 99). In one place, he also says that the question of which position to take is "a question of words" (PTb 101). What are we to make of this?

As far as I can see, it is not a question of words. Rather, it is a fundamental question of ontology. It has to do with what to count as real. Such questions have to be dealt with from the point of view of what we believe, that is, from the point of view of our global system of the world. This means that Quine should accept the sectarian position.

The ecumenic position can only be acceptable for someone who accepts an empiricist theory of truth. According to an empiricist

theory of truth, roughly speaking, a sentence is true if it is entailed by an empirically adequate theory. But Quine does not accept an empiricist theory of truth.[16] The empiricism he accepts is a theory of evidence and of meaning (see, e.g., EN 75).

We may also argue against ecumenism as follows. Suppose that two systems *A* and *B* are irreconcilable and have the same empirical content. Suppose also that we believe, ecumenically, that they are both true. Surely, we should then believe that the conjunction *AB* is true. Presumably, these three theories – the conjunction and its two conjuncts – are empirically equivalent in the sense that they have the same empirical content. But they are not empirically equivalent in the sense that "whatever observation would be counted for or against the one theory counts equally for or against the other." The conjunction *AB* is less simple than its conjuncts, and it may also be less coherent. Quine says that *AB* should not be accepted under such conditions. He rejects what he calls the "tandem" solution (PTb 99). Now, it seems to me that if we should not accept the conjunction, neither should we accept both conjuncts. This undermines the ecumenical position. Strangely enough, Quine seems to think that it is all right for us to accept both *A* and *B* ("we account both theories separately true") and at the same time not accept the tandem theory *AB* (PTb 99–101). In my opinion, this is not an attractive position. I find it unintelligible.

RELATIVISM

So, from a Quinean point of view, sectarianism is right – as Quine himself appears to have thought most of the time. But sectarianism seems to be a form of relativism. It is a form of relativism in that it makes truth in some way relative to theory. Quine sometimes expressed this by saying that truth is *immanent*. For example, he said that

it is a confusion to suppose that we can stand aloof and recognize all the alternative ontologies as true in their several ways, all the envisaged worlds as real. It is a confusion of truth with evidential support. Truth is immanent, and there is no higher. We must speak from within a theory, albeit any of various. (TPT 21–2)

In this passage, Quine rejects ecumenism and accepts sectarianism. Much the same thought seems to be expressed in the following remark:

It is . . . when we turn back into the midst of an actually present theory . . . that we can and do speak sensibly of this or that sentence as true. Where it makes sense to apply 'true' is to a sentence couched in the terms of a given theory and seen from within the theory. (WO 24)

This passage is cited by Davidson as seeming to point in the direction of relativism. However, in the same context, Davidson writes,

I had worried that when he [Quine] wrote that truth is 'immanent' he was expressing the idea that truth is relative not only to a language, but also in some further way. He assures me that no other relativism is implied beyond the familiar, and unavoidable, relativization to a language.[17]

But Davison also points to the fact that 'theory' and 'language' are not to be clearly distinguished in Quine's writings, so there may still be some kind of relativization of truth to theory. However, in his response to Davidson, Quine seems to agree that relativization to language is all that is involved, and he refers to "our common foe who would relativize truth to theory." The role of theory "was not in legislating truth, but in clarifying the theoretical sentence" (RD 498).

This is not easy to understand. For example, what is meant by the statement that truth is relative to language? One might suppose that a given sentence can belong to more that one language and that it can have different meanings in different languages; hence it may be true in one language and false in another. Maybe this is what Davidson has in mind. But Quine seems to hold that a given theoretical sentence (or utterance) belongs to just one language or one theory. Thus, in his response to Davidson, he quotes his own statement that "a sentence is meaningless except relative to its own theory" (RD 498, WO 24). This indicates that each (theoretical) sentence has a theory it can call its own.[18] If so, there is no point in saying that a sentence can have different meanings in different theories or in different languages. A sentence is intelligible, and hence disquotable, only as seen from within its own theory or system of the world. "Disquotation explains truth only insofar as the disquoted sentence is intelligible," as Quine puts it (RD 498).

So truth is not relative in the sense that one and the same sentence can be true relative to one theory and false relative to another. In this sense, at least, Quine is not a relativist. However, in Quine's view, truth still seems to be relative to theory in the sense that the truth of a sentence presupposes a whole theory or system of the world to which the sentence belongs and from which it gets all the meaning it has. From this point of view, a sentence of another culture, one in which a different system of the world is accepted, is meaningless and therefore neither true nor false. Of course, it can be *given* a meaning in the sense that we may devise a successful translation manual from the alien language into ours, but according to Quine's thesis of the indeterminacy of translation, many different manuals may be equally successful and none of them is objectively the correct one. We may also say that the alien sentence is true relative to a given translation manual, but what is relative here is meaning rather that truth. Truth is still somehow related to our own theory. Disquotation can only be applied within one's own language. This is also why ecumenism has to be rejected.

Quine's idea that truth is immanent can hardly be taken as a purely epistemological claim. The point is not merely that in order to *find out* whether a given sentence is true we have to take much of our own system of the world for granted; we have to relate the sentence to other sentences that we accept as true. This is unavoidable, but I take it that Quine's claim is also, and primarily, ontological. Of course, the claim is that a sentence is true if and only if it is implied by our system of the world. Parts of our system may be false. Rather, I believe that the claim can be roughly stated as follows: A necessary condition for a sentence of our language to be true is that most of the sentences in our system of the world are true. And similarly for other cultures. In this sense, I suggest, truth is relative to theory.[19]

It might be objected that Quine's naturalism is violated if we allow the addition 'similarly for other cultures'. By saying this, we may seem to transcend our own system of the world in an illegitimate way. But I think this would have been acceptable to Quine. What we say about other cultures is still said from within our own system of the world. We are not violating Quine's thesis that "it is a confusion to suppose that we can stand aloof and recognize all the alternative ontologies as true in their several ways, all the envisaged worlds as

real" (TPT 21). We are not saying that all the systems are true. We are not in this sense adopting a transcendent position outside all systems, including our own.

What we do is, I believe, similar to what Quine himself did when he said that the sectarian "is as free as the ecumenist to oscillate between the two theories [two empirically equivalent systems of the world] for the sake of added perspective from which to triangulate on problems" (PTb 100). Suppose the sectarian says, from within our system of the world, that a certain sentence S is true. According to Quine, the sectarian also recognizes that he may "oscillate" to some alien system that is just as warranted as ours but from within which he would no longer regard S as true. I think we may legitimately express this by saying that truth is relative to theory. (Personally, however, I am not so sure that it would really be possible to oscillate in this way.)

As far as I can see, then, the relativism I attribute to Quine is consistent with his naturalism. However, Quine has explicitly rejected relativism on the ground that it is paradoxical:

Truth, says the cultural relativist, is culture-bound. But if it were, then he, within his own culture, ought to see his own culture-bound truth as absolute. He cannot proclaim cultural relativism without rising above it, and he cannot rise above it without giving it up. (EES 327–8)

But it is not clear to me that this is really paradoxical. To say that truth is culture-bound is, in this context, much the same as to say that it is immanent, and Quine does not seem to find this paradoxical. The relativist says that truth is relative to a culture or a system of the world (culture-bound). Does he need to see this very thesis itself as "absolute"? Yes and no. He cannot make a statement and relativize it at the same time. This would be much the same as making the statement and taking it back in the same breath. Nothing would be accomplished. However, he may make second-order statements about his own statements. Such a second-order statement might be as follows: 'While relativism is absolutely true, someone from another culture with a very different system of the world may say that the relativism I proclaim is meaningless or merely true relative to my system, just as I say that his utterances can be true relative to his system even thought they are in fact meaningless or at most true relative to some translation manual I might devise.'

A slightly different view, which can also be described as a form of "cultural relativism," is expressed in the following passage:

Might another culture, another species, take a radically different line of scientific development, guided by norms that differ sharply from ours but that are justified by their scientific findings as ours are by ours? And might these people predict as successfully and thrive as well as we? Yes, I think that we must admit this as a possibility in principle; that we must admit it even from the point of view of our own science, which is the only point of view I can offer. I should be surprised to see this possibility realized, but I cannot picture a disproof. (R 181)

Surely, this has a relativistic ring. We may note that this passage describes a different kind of underdetermination from those identified above. In this case, we are asked to imagine two systems of the world that are equally *successful* in a certain way. As far as I can see, this does not imply that they have the same empirical content, nor that they are empirically equivalent in Quine's more general sense. Again, however, it seems that we can "proclaim cultural relativism without rising above it."

NOTES

  1. Albert Einstein, *Ideas and Opinions* (New York: Crown, 1954), 226. Incidentally, we may note that Einstein seems to recommend something like the so-called method of *Verstehen* for use in the *Naturwissenschaften.*

  2. In one place, Quine says that "even a common observation term such as 'blue' has its penumbra of vagueness, where witnesses may disagree in their verdicts. The really distinctive trait of observation terms and sentences is to be sought not in concurrence of witnesses but in ways of learning. Observational expressions are expressions that can be learned ostensively" (EES 316).

  3. However, as Quine points out, we cannot assume that an untestable sentence has empirical content if it implies synthetic observation categoricals in conjunction with some other untestable sentence, for every sentence satisfies this requirement. For example, let Q be Russell's nonsense sentence 'Quadruplicity drinks procrastination' and

let *O* be any synthetic observation categorical. Now, neither *Q* nor $(Q \supset O)$ is testable, but their conjunction is testable (see FSS 48).

4. Quine says that considerations such as "simplicity, economy, and naturalness...contribute to the molding of scientific theories generally" and that they support Gödel's axiom of constructibility (PTb 95). So maybe our global system of the world should contain Gödel's axiom. Notice, that our system of the world may contain a lot of sentences that have very little to do with empirical content. Quine writes, "Much that is accepted as true or plausible even in the hard sciences, I expect, is accepted without thought of its joining forces with other plausible hypotheses to form a testable set. Such acceptations may be prompted by symmetries and analogies, or as welcome unifying links in the structure of the theory....Having reasonable grounds is one thing, and implying an observation categorical is another" (FSS 49).

5. T. S. Kuhn, "Objectivity, Value Judgment, and Theory Choice," in *The Essential Tension* (Chicago: University of Chicago Press, 1977), 322.

6. For arguments and further clarification concerning this claim and other points made in this section, see my paper "Scientific Value," *International Studies in the Philosophy of Science* 10 (1996): 189–202.

7. For example, Quine writes, "No general calibration of either conservation or simplicity is known, much less any comparative scale of the one against the other" (FSS 49).

8. See also my paper "Quine, Underdetermination, and Skepticism," *Journal of Philosophy* 90 (1993): 339.

9. The notion of tightness is employed in my paper "Underdetermination and Realism," *Erkenntnis* 21 (1984): 351. It is based on a rather similar idea of Quine's (EES 323), that the addition to a theory of "some gratuitous further sentences that had no effect on its empirical content" is not sufficient for underdetermination. Notice that if theories *A* and *B* are irreconcilable and have the same empirical content, each has the same empirical content as the disjunctive theory $A \lor B$, which is logically weaker. Are *A* and *B* "gratuitous further sentences," in Quine's sense, in relation to $A \lor B$? Suppose further that the empirical content is a finite set of synthetic observation categoricals. Does it follow that *A*, *B*, and $A \lor B$ are all gratuitous further sentences in relation to the observation categoricals? In one place, Quine says that "gratuitous branching of theories" is of "no interest to the thesis of under-determination" (EES 323), but it is not so clear what a "gratuitous branching" is. We may suppose that a branching is gratuitous if it is scientifically worthless, but that does not help much.

10. It may be tempting to add that *GC* is more underdetermined that is *G* because it is logically stronger but has the same empirical content. However, I shall disregard this idea.

11. Michael Dummett, *Frege: Philosophy of Language* (London: Duckworth, 1973), note on p. 617.

12. The example of neutrinos and mass is discussed by Quine in WO 16 and FSS 70.

13. Note that this particular example may not be quite convincing. At roughly the same time, or slightly later, Quine said that the two theories in Poincaré's example are irreconcilable but logically compatible (PTb 97).

14. This is pointed out by Davidson. He writes, "Quine's two theories can belong to, and be stated in, the same language; indeed, they must be if we are to understand the claim that the theories conflict" ("The Structure and Content of Truth," *Journal of Philosophy* 87 [1990]: 306).

15. Hence, the scepticism I have discussed here should be described as partial. In fact, there is some reason to think that Quine would have accepted a partial scepticism of this kind. In his reply to Gibson, Quine wrote, "What can be known of the world is the common denominator of all the world systems, logically reconciled, that conform to all possible observation" (RG 156). It seems to follow that, if there is no common denominator, nothing can be known of the world. In the case of empirically equivalent systems, there must be a common denominator. This includes, at the very least, the common empirical content of the different systems. Perhaps the systems may overlap in other ways too; the underdetermination thesis does not say anything in particular about the extent of such overlap.

16. Compare VITD 39, RB, and also Quine's comments on my paper "Quine, Empiricism, and Truth," in *Knowledge, Language and Logic: Questions for Quine*, ed. Alex Orenstein and Petr Kotátko (Dordrecht: Kluwer, 2000).

17. "What Is Quine's View of Truth?" *Inquiry* 37 (1994): 437.

18. Presumably, a theoretical sentence in one theory (language) may have the same syntactic form as a sentence in another, but, if so, they are still to be reckoned as two distinct sentences. Notice, also, that there is still a sense in which "truth is relative to language," namely, that an explicit *definition* of truth, in nonsemantic terms, can only be constructed *for a particular language* (in accordance with the method invented by Tarski). But this shows only that definitions of truth are less than general, not that truth is relative.

19. In his recent paper "Quine's Philosophy: A Brief Sketch," in *The Philosophy of W. V. Quine*, ed. L. E. Hahn and P. A. Schilpp, expanded ed. (Chicago and La Salle, Ill.: Open Court, 1998), Roger Gibson said that Quine "believes truth is an immanent notion, relativized to a theory (or language)" (p. 680). In his reply to Gibson, Quine accepted relativization to language but not to theory; he said, "I grant language but balk

at theory" (RGQ 685). However, his reason seems to be that "[a] theory that I hold may turn out false." This is uncontroversial but, I think, irrelevant. I suspect that Quine would not have allowed that *most* of the sentences in our system of the world may turn out to be false. The truth or falsity of a theory or sentence in our system of the world may still presuppose the truth of most of the system.

5   Quine on Reference and Ontology

Issues of reference and ontology occupy a considerable portion of Quine's work. In the Preface to *Word and Object*, Quine indicates that the bulk of that book is the product of his reflecting on "the development and structure of our own referential apparatus" (WO ix). His revival of the word 'ontology' in a nonpejorative sense marks, in precise fashion, a central disagreement that he has with the work of Carnap, who was his greatest teacher. In spite of their centrality to his thought as a whole, however, Quine's views on these topics are not well understood. Nor, indeed, are they straightforward. The aim of this chapter is to set out those views as clearly as may be and to indicate points of remaining unclarity.

I

Let us begin with the views of Russell, which form a sharp and useful contrast with those of Quine on these topics.[1] Russell postulated a direct and immediate relation between the mind and entities outside the mind, a relation he called acquaintance; this relation he held to lie at the base of all knowledge.[2] His insistence on the directness and immediacy of the relation is to be explained in terms of his opposition to idealism. The idealists held that our knowledge is always mediated by a complex structure of which we can have a priori knowledge; this also gives us knowledge of the world, at least as far as it is knowable. It was in reaction to this that Russell, along with G. E. Moore, had postulated the notion of acquaintance. It was to be a cognitive relation, holding between the mind and objects, that relied on no kind of structure or theory: an *im*mediate relation rather than a mediated relation. The mind is, as it were, in direct contact with

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objects outside it. The directness of the contact is held to exclude mistakes, so the existence of illusion and error gives us reason to think that we cannot have this kind of immediate cognitive relation to ordinary physical objects. Russell came to think that the objects with which we are acquainted are abstract objects and sense-data (as well the contents of our minds, and past objects of acquaintance). Sense-data, in Russell's thought, are the appropriate relata of the relation of acquaintance.[3]

The relation of acquaintance, for Russell, provides an answer to the question, How do our thoughts and our language succeed in being about the world outside our minds? That relation is the point at which this contact is made. While we appear to know about many things that are not objects of acquaintance, all such knowledge must ultimately be reducible to knowledge of objects that are. Thus in *Problems of Philosophy* Russell says, "*Every proposition which we can understand must be composed wholly of constituents with which we are acquainted*" (p. 58; emphasis in the original). This demand gives rise to an extremely far reaching program of philosophical analysis: to show that sentences that appear to express propositions that violate this demand in fact express propositions that accord with it. In a fully analyzed sentence – one that reveals in fully explicit fashion the structure of the proposition it expresses – every word will refer to an object of acquaintance.

Two features of this view are salient. First, acquaintance is entirely presuppositionless and wholly independent of beliefs and concepts. It is, we might say, pretheoretical; this is why it can serve as the foundation for all theory. Second, it is a relation between a person and an *object*, not between a person and a fact or a proposition or a sentence. For Russell, then, reference, a version of the relation between a name and the named object, is a presuppositionless relation that is at the foundation of all knowledge.

Quine, as we shall see, rejects the Russellian view utterly, in every aspect. It is, however, worth noting that he does not wholly reject the sort of question to which this view is an answer. We might put the question like this: How do we come to have knowledge at all? Or how can our thoughts be *about* the world? Or how does the mind come to be in contact with things other than itself? Quine in fact has an answer to questions of this sort, questions that he construes along naturalistic lines – roughly, as scientific questions. Taken as

scientific questions, they are answered in terms of the effects that the world has on us and of our responses to those effects. Various forms of energy, most obviously light, sound, and heat, impinge on the surfaces of our body; in some cases, a person responds differentially to such impingements. Thus we may respond in one way when a certain pattern of light is impinging on our eyes and in another way when it is not. The body contains sensory surfaces that are stimulated by impingement of the relevant forms of energy; these stimulations affect behavior.

It is a long way from forms of energy impinging on the body to our discourse and our thought being about the world. We need to see, in broad outline, how Quine thinks we get from one to the other. This discussion will take us, for a couple of pages, completely away from the subject of reference; this fact, that we can discuss Quine's views on this subject without talking about reference, is of great significance.

Our present concern, then, is Quine's account of the transition from the impingement of energy on our sensory surfaces to the empirical content of our theories. The key here is Quine's notion of an *observation sentence*. An observation sentence satisfies three conditions. First, it is a complete utterance whose truth-value varies with the occasion of utterance ('There's milk in the glass' rather than 'Milk is good for young children'). Second, for each individual, it is directly tied to stimulation of the sensory surfaces: The individual's willingness to make the utterance, or to agree with it when another makes it, depends only on which of that individual's sensory surfaces are being stimulated at that time. Third, from the point of view of the community of language speakers, the observation sentences are utterances about which there will be general agreement in any given circumstances. If I am in a situation that stimulates my sensory surfaces in such a way as to lead me to agree or disagree with a particular observation sentence, then any other speaker of the same language in the same situation would have his or her sensory surfaces stimulated in a way that results in the same verdict. Quine speaks of "projecting ourselves into the witness's position" (PTb 43; there is some vagueness in this idea, as Quine himself points out).

Observation sentences will thus be uncontroversial, to the point of banality. 'It's raining', 'It's red' (or just 'Red'), and 'There's a horse' might count as examples. Not by chance, these are the sorts of

sentences that an infant learning the language acquires first; they have no presuppositions, and their mastery does not at all depend on a mastery of other aspects of the language. Within the small community of language speakers where the infant lives, other sentences will count as observational: 'Mama', for example, or 'Rover', said of the family dog.[4] In spite of their triviality, observation sentences are, in Quine's view, at the foundation of all knowledge. Everything we know about the world is due to the impingement of energy on our sensory surfaces; this for Quine is a scientifically established fact. And observation sentences encapsulate those impingements insofar as they are relevant to our knowledge.

All of our knowledge, however theoretical, is thus ultimately answerable to observation sentences. Our theories are empirically testable because they imply what Quine calls observation categoricals. These are, roughly, sentences saying that whenever one observation sentence holds, another will also hold. An empirical test, in extreme cases, will consist in finding some observation categorical that, according to the theory, should be true and then seeing whether falsifying instances can be found.[5] It is just the uncontroversial nature of observation sentences that fits them to play this role. By the same token, Quine defines the empirical content of a set of sentences as the observation categorical it implies (see FSS, chap. 4).

Russell asked how our words and thoughts come to be about the world. Quine's answer to that question, as he construes it, is via the impingement of energy on the sensory surfaces of our bodies as this is encapsulated in observation sentences and in the observation categoricals formed from them. Now this answer, unlike Russell's, has nothing to do with *reference*, in the sense of the relation between a name and its bearer. The willingness to utter, or to assent to, an observation sentence is brought about by the occurrence of the relevant stimulation of one's sensory surfaces. But the observation sentence does not refer to such stimulation, or to the energy impinging on these surfaces. Obviously, the child happily uttering 'Mama' in the presence of its mother is not in any sense talking *about* the light reaching its retinas or the stimulations of the retinal nerves. To talk of these takes a good deal of linguistic and theoretical sophistication, whereas a crucial fact about observation sentences is that one can master them while completely devoid of all such sophistication. It is for this reason that they are the child's "entering wedge"

into language, as Quine says more than once (see, e.g., PTb 5 and FSS 22). It is for this reason also that they can play the role of ultimate evidence, for while we may disagree about all the sophistication, we cannot disagree about observation sentences. This last point is not simply a matter of the definition of observation sentences as those that all speakers of the language would give the same verdict on in the same circumstances. It is also that if we come to disagree about a significant number of (what were formerly) observation sentences, then communication between us will have broken down.

Observation sentences, then, do not refer to stimulations of our sensory surfaces, or anything of that kind. Do they refer to other objects? This question is more complicated, though it may appear to be simple enough. It looks as if the utterance 'Mama' by a particular child refers (let us say) to that child's mother; indeed, it may seem simply to be a name for that person. Equally, it looks as if the sentence 'There's a horse' contains the term 'horse', which (of course) refers to horses. If we think of an observation sentence as it might occur in the discourse of an adult (someone who has mastered the language and possesses a range of knowledge about various matters), then these appearances are correct. Thought of in that way, an observation sentence will typically contain, or simply be, a referring term. Quine claims, however, this is true only because the adult has mastered not only the use of the observation sentence but also the use of more sophisticated parts of the language. To refer to a horse, he argues, it is not sufficient to know that it is appropriate to say 'Horse' or 'There's a horse' when one is in the presence of a horse. One needs also to have some idea of the answers to questions such as these: What counts as one horse and what counts as two? Under what circumstances do we have the same horse over again and when do we have a new horse? The beginner, who simply makes the noise 'Horse' when receiving (say) the sort of visual stimulations that one typically receives when looking at a horse (or, more realistically, at the picture of a horse), cannot even formulate such questions, much less start to answer them. Yet it is, Quine insists, only in the mouth of one who has these capacities that such words can be said to *refer* at all: There is more to reference than merely making a sound in response to patterns of stimulation.

Another way to make this point is to say that observation sentences are indeed *sentences*. Even a one-word observation sentence,

such as 'Mama' or 'Red' or 'Horse', must be counted in context – uttered all by itself in response to present stimulation – as a sentence and not as a referring term. Clearly the criterion of sentencehood here is not narrowly syntactical. What does it mean to say that observation sentences are indeed *sentences* and not referring terms? In part it means that an observation sentence is complete: All by itself, as it stands, it is to be assessed as correct or incorrect, true or false. A referring term, by contrast, picks out an object or a kind of object in order to say something about it. In part it also means that no question of reference arises until we analyze observation sentences – break them down into significant parts. This analysis enables us to see them as made up of parts that may refer. The analysis, however, draws on more sophisticated parts of the language, which are not required simply for an initial mastery of the observation sentence. (All of this applies equally to one-word observation sentences, though in such cases the only part is orthographically identical to the whole.) The upshot of this discussion is that the means by which language comes into contact with the world, in Quine's view, is *not* referential. It is, rather, in the relation between a sentence and the circumstances that make the utterance of that sentence correct or incorrect in the eyes of the community of language speakers as a whole. This is not to say that Quine denies that our language contains expressions that refer to the world; as we shall see, he insists on the referential aspect of language. What he does deny is that reference is fundamental (in the sense in which it is fundamental for Russell). Reference, for Quine, is a derivative notion; the fundamental notion is that of the relation between a complete utterance and the circumstances that make it true. If reference is not the foundation of language use, how can language be referential at all? This is a question that Quine takes seriously as a scientific question and to which he has suggested an answer.[6] What is important, for his philosophical view as a whole, is not so much the details of the answer but that there be *some* answer of a purely naturalistic sort compatible with the basic picture; for this reason, his answer is avowedly tentative and speculative.[7] On the account that he suggests, the first steps of reification take place very early and at the observational level. In some situations, the observation sentences 'Blue' and 'Pebble' are both appropriately asserted, but not all of those situations make it appropriate to utter

'Blue pebble'. Someone who does utter these words can already, in retrospect, be seen as postulating an object – a pebble – and saying something about it.

It is only with hindsight, however, that this distinction at the observational level should be seen as genuine reification, as introducing objects in the familiar sense. Objects such as pebbles have identity conditions that provide for their reidentification from time to time. This is a matter that cannot be dealt with at the purely observational level, for it involves rudimentary physical theory: "[R]eification of bodies across time is beyond the reach of observation sentences and categoricals. Substantial reification is theoretical" (PTb 25). Reification also requires the ability to use words such as 'same', 'if another', and 'an', means of forming plurals, and so on. These expressions and transformations, Quine suggests, are acquired contextually, together and little by little.[8] Merely standing in front of something making a noise is not yet naming an object, not even if one consistently makes the same noise when in front of the same object. As another author put it, "[A] great deal of stage-setting in the language is presupposed if the mere act of naming is to make sense."[9] For the adult language user, the stage is set; hence we are likely to see the child's first recognizable noises as already names in the full-fledged sense. But the capacity to name things, in our sense, is acquired little by little. There is, of course, far more to be said about Quine's account of the acquisition of fully referential language – and far more than Quine says about the process itself. But it is not our purpose here to explore these matters in detail. What is important for us is that reference, for Quine, is not the fundamental relation between language and the world; it is not the means by which language acquires its empirical content and comes to be about the world. That relation is, rather, the relation between an observation sentence and the situations that typically lead to the sorts of stimulations under which the sentence is uttered or commands assent when uttered by another. Reference, on Quine's account, *is* a relation between language, or some linguistic expressions, and the world, just not one that is fundamental in the sense we have indicated. The capacity to refer is a language-using capacity that is more sophisticated than the most elementary, purely observational sort. A child acquires this capacity, little by little, as it grows up.

II

Reference, for Quine, is thus not the fundamental relation that language has to the world. It is, however, a notion of great importance. Our cognitive discourse is, or aims to be, *about* the world; a particular utterance is, or aims to be, about some portion of the world, typically about some object or objects. That reference is not fundamental means that there is an explanation, in other terms, of how it comes about; it does not mean that reference is unimportant. And for Quine an understanding of the referential capacities of our cognitive language is the crucial step in the clarification of that language. We have already seen Quine's emphasis, in the preface to *Word and Object*, on "the development and structure of our own referential apparatus" (WO ix). Later in the book he speaks of understanding the referential work of language and clarifying our conceptual scheme. These for Quine are not separate endeavors: Understanding reference *is* an important step toward conceptual clarification.

Taking our language as a going concern, then, how are we to understand its referential function – our ability to speak *about* objects? What is the *aboutness* relation here? The fact that reference is, in the sense we have discussed, not fundamental shows itself in the fact that we must begin with a set of true sentences, a body of theory that is true, or at any rate accepted as true. Only when the truths are in place can we raise the question of existence. In this sense, acceptance of sentences is prior to reference, and truth is prior to existence. Those objects that a given body of theory is about are presumably the ones that must exist if that body of theory is to be true. They are, in Quine's words, the *ontological commitment* of that body of theory. How are we to understand this idea? For Quine, the answer is quantification theory, which has first-order logic at its heart.[10]

I shall give a very brief explanation of the relevant aspect of logical notation. An *open sentence* is obtained from a sentence containing names by replacing one or more of those names by a variable, that is, a lowercase italicized letter from near the end of the alphabet. Thus we obtain 'x is human' from 'Socrates is human', 'If x is human, then x is mortal' from 'If Socrates is human, then Socrates is mortal', and so on. A name need not be replaced at every occurrence, nor need every name be replaced. An open sentence contains one or more variables and may also contain names. An open sentence,

clearly, is not true or false as it stands; it is, however, true or false *of* each object (or of each pair of objects, if it contains two distinct variables, and so on). Thus our first example is true of each human being, false of everything else; our second example is true of every object (for it is true of everything, whether human or not, that *if* it is human, then it is mortal). Now we introduce the quantifiers. The *existential quantifier*, usually signified by a backwards E, is prefixed to a (one-variable) open sentence to yield a sentence that is true just in case the open sentence is true of at least one object. Thus '($\exists x$) x is mortal' is a true sentence (not an open sentence). There are two complications here. First, a variable goes with the quantifier, for some open sentences contain more than one variable, and we need to keep track of which one is quantified, or bound, by the quantifier. Second, if an open sentence contains two distinct variables, then prefixing it by one quantifier will still leave an open sentence. Thus '($\exists x$) x loves y' is true of just those objects that love something or other. Affixing the universal quantifier to an open sentence (usually signified by an upside-down A or just by the variable alone in parentheses) yields a sentence that is true just in case the open sentence is true of *every* object, as, for example, '($\forall x$) If x is human, then x is mortal'.

Now when we have a body of theory cast in the notation of first-order logic, its ontological commitments, Quine claims, are apparent. For every existentially quantified sentence that the theory contains or implies, there must be an object of which the corresponding open sentence is true; such an object must exist if the theory is to be true. (The "corresponding open sentence" here we would obtain by simply deleting the quantifier.) This much seems to be implied by the explanation of quantification just presented.[11]

Let us, then, accept that if a body of theory contains or implies an existentially quantified sentence, then the theory cannot be true unless an object exists of which the corresponding open sentence is true. (Note that a universally quantified open sentence implies the existential quantification of the same open sentence; our focus on existential quantification here is thus only for the purposes of clarity.) This, however, gives us only a sufficient condition of ontological commitment, not a full criterion. Under the given circumstances, a theory is committed to there being an object of the given sort. Quine holds something more, however: that this is the *only* way in which a theory can be so committed.[12] This is the view that has been summed

up in the slogan "To be is to be the value of a variable" (see, e.g., WTI 15). More accurately, "a theory is committed to those and only those entities to which the bound variables of the theory must be capable of referring in order that the affirmations made in the theory be true" (WTI 14–15). Quine also accepts idioms equivalent to quantification theory as indicating the same commitment, even if those idioms do not use variables (see, e.g., his discussion of predicate functors in FSS, chaps. 3 and 4 and the appendix). It is only relative to some such idiom, however, that the ontological question as we understand it can be raised (see PTb 35–6).

On the face of it, it might seem as if Quine's condition is a sufficient but not a necessary condition for ontological commitment. It will be useful to see how Quine responds to a couple of reasons for thinking this. Perhaps the most obvious reason for thinking that Quine's criterion is incomplete is the existence of *names*. Surely, one might think, the use of a name in a body of theory commits that theory to there being an object named by the name (a bearer of the name). If the sentence 'Socrates is human' is among the things that I sincerely and reflectively believe, then surely I must also accept that Socrates – the bearer of the name 'Socrates' – *exists*. ('Exists' here is used timelessly, as is 'is' in the original sentence that I believe. We need not worry here about the legitimacy of this, for we can use the past tense without altering the point: If I believe that Socrates was human, then I must believe that Socrates exist*ed*.) Here, one might think, we have a source of ontological commitment that is wholly independent of complicated considerations about quantifiers, variables, open sentences, and so on. (One might, indeed, be tempted to think of names as the essential source of all ontological commitment, but the existence of unnamed objects should be a deterrent here; see n. 11.)

We need to understand, then, why Quine does not see names as a source of ontological commitment on a par with quantified variables. In any language containing names, the point might seem undeniable, and in a sense Quine does not deny it. His claim is that when we are concerned with exposing the ontological commitments of a given language, we should reformulate it so that the names are all eliminated. The easiest way to see why we should do this is to consider the fact that there are names that do not actually name any object. Myths and works of fiction are obvious sources of such names, but

these names are perhaps subject to special treatment. Other names, more clearly to the point, are simply introduced by mistake, or as a hoax. The name 'Vulcan', for example, was introduced to name a tenth planet whose existence was postulated – wrongly as we now know – to explain certain astronomical phenomena; in fact there is no such planet. Now someone who asserts, say, 'Vulcan is a small planet' *is* thereby committed to the existence of Vulcan. No such object actually exists: Any criterion of ontological commitment will tell us what a given body of theory is committed to but will not tell us what there really is unless we add the claim that the body of theory is true. But what if someone asserts (correctly), 'Vulcan does not exist'? That belief clearly does not commit the speaker to the existence of Vulcan; quite the opposite. So the mere presence of a name in a body of theory does not show that the theory is committed to the existence of a bearer of that name. Nor can we simply fence off sentences of the form '...does not exist', for a body of theory might contain the sentence 'Either Vulcan is a medium-size planet or Jupiter is a very large planet' without thereby being committed to the existence of Vulcan; examples might be multiplied at will.

Difficulties of this sort convinced Quine that the ontological commitments of a language are most clearly displayed when the names of the language have been eliminated, along the lines suggested by Russell's application of his theory of descriptions.[13] Suppose we have a name, such as 'Socrates', and a sentence in which a predicate, 'is human', is ascribed to Socrates. We introduce a predicate 'S' that we take to apply to Socrates and to no one else (if need be, we do this quite artificially, simply by stipulation). Then we can say, there is an object that is S, only one object is S, and that object is human. In partial logical notation,

$$(\exists x)[Sx. \ (\forall y)(\text{if } Sy \text{ then } x = y). \ x \text{ is human}].$$

Here there is nothing that purports to be a name for Socrates; the burden of reference is borne by the existentially quantified variable. On this sort of reformulation, the sentence 'Socrates does not exist' is represented

$$\text{It is not the case that } (\exists x)[Sx \ (\forall y)(\text{if } Sy \text{ then } x = y)].$$

There is, I hope, no temptation at all to think that the acceptance of this sentence might commit one to the existence of Socrates.

Quine's criterion of ontological commitment is directly applicable to what we ordinarily say only after that has been reformulated in first-order logic. The moral of the last few paragraphs is that for Quine this reformulation will include the elimination of names, as indicated. This elimination avoids difficulties connected with empty names and recommends itself to Quine also on the grounds of clarity and economy; it restores the idea that it is only through variables and quantifiers that a body of theory has ontological commitments.

There is also a second objection to the idea that Quine's account is the *only* way in which a body of theory comes to be committed to the existence of entities. The idea here is that the predicate of a true sentence must correspond to some entity – a property, or a "universal," as it is often called. Thus it is held that one who asserts that the rose is red is committed not only to the existence of the rose but also to there being a property, redness. The word 'existence' is not always used here; some hold that universals have a different sort of ontological status from that of objects and mark it with a different word, such as 'being' or 'subsistence'.

Quine rejects all of this completely. For him there is a single and unequivocal question: What exists, or what is there? Unlike most philosophers over the centuries, he acknowledges no distinctions, no "orders of being" or modes of existence, but a single status. And he denies that properties or universals must have this status for sentences using the corresponding predicates to be true.[14] It is tempting to see in this rejection simply the flat insistence that existence is captured by the use of variables and quantifiers and that the predicate position of a simple sentence is not accessible to quantification. The clarity and simplicity that Quine finds in first-order logic have great appeal to him, but there is more to his position than this flat insistence. Postulating universals, he holds, simply does not explain anything; any appearance to the contrary is an illusion:

That the houses and roses and sunsets are all of them red may be taken as ultimate and irreducible, and it may be held that McX [an imaginary opponent] is no better off, in point of real explanatory power, for all the occult entities which he posits under such names as 'redness'. (WTI 10)

The notion of explanation that Quine deploys here is naturalistic. His point is that, in a naturalistic explanation of how we use and understand language, appeal to properties will play no role. In §I, we

saw, in barest outline, how such an explanation might go, at least for the simplest kind of sentences. It is very hard to see how any naturalistic explanation – an explanation along causal-scientific lines – could make an appeal to properties, entities that are, supposedly, not in space and time and not capable of causally interacting with us.[15]

There is, of course, room for other sorts of questions to be raised about Quine's criterion of ontological commitment. The two we have discussed, however, at least indicate what that doctrine comes to and why Quine holds it. One immediate consequence of the doctrine is that talk of the ontological commitment of any body of informal theory draws on the idea of a *regimentation* of that body of theory, that is, a reformulation of it, actual or envisaged, into the language of first-order logic. This does not mean that we cannot talk about, say, the ontological commitments of Aristotelian physics: With some intellectual imagination and sympathy, we can consider what reformulation Aristotle himself might have accepted for his view if he had had first-order logic at his disposal. It does, however, mean that an author who rejects the idea of such a reformulation entirely is simply refusing to answer the ontological question as we understand it (or as Quine thinks we do, or should). Of course, we can imagine what a reformulation might look like, in spite of the objections of the author, but the imagined author himself is, on Quine's view of the matter, simply rejecting the question.

Another consequence of adopting Quine's criterion is, of course, that the ontological commitments of any body of discourse are dependent on the way in which it is regimented (i.e., cast into the notation of first-order logic). Informal discourse, taken as such, has no definite ontology implicit in it, for there are various ways in which it can be regimented. Let us see what Quine says on this matter[16]:

The common man's ontology . . . is vague in its scope; we cannot tell in general which . . . things to ascribe to a man's ontology at all, which things to count him as assuming. Should we regard grammar as decisive? Does every noun demand some array of denotata? Surely not: the nominalizing of verbs is often a mere stylistic variation. But where can we draw the line?

It is a wrong question; there is no line to draw. Bodies are assumed. . . . Beyond them there is a succession of dwindling analogies.

. . . [A] fenced ontology is just not implicit in ordinary language. The idea of a boundary between being and nonbeing is a philosophical idea, an idea of technical science in a broad sense. . . . Ontological concern is not a correction

of a lay thought and practice; it is foreign to the lay culture, though an outgrowth of it. (TPT 9)

To those who complain that Quine's ideas about ontology distort common sense, his answer is that any attempt at ontology is bound to do so, for common sense does not contain an answer to the ontological question, not even implicitly. To those who complain of artificiality, the answer is that the very question is a product of artifice, as are all advances in our knowledge.

III

Ontology, on Quine's account, is thus an artificial matter. In particular, it is dependent on regimentation and thus on a regimented language. Ontological commitments cannot simply be read off from a body of informal and loosely stated knowledge; their discovery requires a more or less artificial language. That it does may suggest an element of relativity, for various artificial languages are available, and different choices may lead to different outcomes. Now this matter is complicated, for in one way Quine denies the relativity of ontology while in another way he asserts it.

Very roughly, we might characterize the difference like this. In the sense in which Quine denies the relativity, it arises already at the level of sentences and thus of truth. Here the idea is that we are free to choose any one of various languages for science (in the broad sense)[17] and that this choice is not a matter of right and wrong. What sentences count as true will then depend on the choice of language: Truth itself becomes a relative notion. Since ontology, in Quine's view, is derivative from truth, the relativity of ontology would follow as a matter of course. This sort of relativity is associated with Carnap, but Quine denies it. It will be our subject in this section. The second sort of relativity is quite different and does not go along with relativity about truth. Suppose we have a body of truths formulated in some regimented language. Even so, Quine argues, there will be more than one way to interpret the ontological commitments of this body of truths: A different interpretation of the predicates will go along with a different interpretation of the ontology, leaving the net empirical significance of the body of truths unchanged. This is the view that Quine called the 'inscrutability of reference' or 'ontological

relativity'.[18] (This latter name might also have been applied to the sort of relativity that Quine denies, but we shall stick to the established usage.)

In this section, then, we shall contrast Quine's views on ontology with those of Carnap.[19] We need first to have a view of the similarities, which are considerable. For both, existence is secondary to truth. We need first to have a body of knowledge formulated in a suitably regimented language; only then can questions of existence be raised. Both Carnap and Quine disagree with Russell here. Moreover, they both hold that there is no requirement that our regimentation somehow follow ordinary language or common sense. On the contrary, each would hold that a language suitable for science may, and perhaps must, deviate from ordinary usage at various points. For each, the procedure is to a greater or lesser extent artificial, but none the worse for that.

The contrast between Carnap and Quine comes over the issue of choice of language. For Carnap, this cannot be a matter of right or wrong, or correct or incorrect. Those evaluative terms, in his view, have a grip only when we have rules of evidence in place, rules telling us what experience bears on what claims. But such rules are, in his view, part and parcel of a language; they are what is being chosen when we choose a language. In choosing a language, then, we cannot rely on such rules, for they are not yet in place. Hence we have no basis for applying the evaluative terms to choice of language. Once the language is chosen, we make assertions within it (internal assertions, as Carnap calls them), and these assertions are the proper subject of evaluation: Some are correct, justified by the evidence, and so on; others are not. Within a language are rules that give substance to these evaluations. When it is the choice of a language that is in question, however, we have no such rules and no basis for evaluation. Therefore, the only reasonable attitude is what Carnap calls the *principle of tolerance*, or, as he also puts it, "the principle of the conventionality of language-forms."[20] The point here is simply that the choice of a language is distinct from the "choice" of which theory to accept once the language is in place. The latter sort of decision is constrained by rules given by the language and hence is either correct or incorrect. The former choice, however, is unconstrained and is indeed a choice. With no basis for dismissing any such choice as incorrect, tolerance is the reasonable attitude.

Now what has this to do with ontology? The answer is that, on this view, ontology becomes *language-relative*. Ontology will vary from language to language: This language might take sets as basic and define expressions for numbers, whereas this other language might have no set theory and presuppose the existence of the numbers outright. More drastically, we might choose to speak a language of sense-data – one in which the basic terms refer to immediate experience – rather than the language of physical objects that we in fact speak. So an ontological question, say, 'Are there sets?' has no flat-out answer. The proper initial response is, it depends on what language you choose to speak. Independent of the choice of language, there simply is no answer to the question – indeed, Carnap holds that if we attempt to ask the question absolutely rather than relative to some particular language, then we are crossing the bounds of sense: There simply is no absolute question to be asked.

The result of this is that the ontological question vanishes, along with other metaphysical questions. The ontological question was precisely the absolute question, and Carnap denies it any meaning. Thus we may, on Carnap's account, speak a language that quantifies over sets, say. But this does not commit us to saying that there really are sets, in some language-independent sense. There is no language-independent sense in which we can say what there is or isn't. Of course, while we are speaking that language, we will assert that there are sets (or at least that sets of this or that kind exist). But our speaking that language implies nothing more than that for certain purposes we find it convenient and useful; our saying things in it implies only that *given the language* those sentences are correct. For Carnap, there really is no ontological commitment at all.

This defusing of ontology depends on the idea that the adoption of a language is, in principle at least, separable from the adoption of a theory within a language. Carnap's position here requires that the two have different epistemological bases: Within a language there are rules that determine which theory is correct; the lack of corresponding rules governing choice of language makes it, by contrast, a fit area for tolerance.

This nexus of ideas is equivalent to an epistemologically significant distinction between the analytic and the synthetic. If we could clearly distinguish changes of language from changes of theory within a language, then we could clearly distinguish analytic

sentences from synthetic sentences (the analytic sentences would be those about which we cannot change our minds without change of language; the synthetic would be those about which a change of mind did not involve change of language). And if the two sorts of changes are justified in quite different ways, as they must be on Carnap's picture, then the analytic-synthetic distinction marks an important epistemological gulf.

Quine's objections to that sort of distinction between the analytic and the synthetic have been widely discussed, and I shall not rehearse them here. (See chapter 2, including the references.) It is worth emphasizing, however, that Carnap's way of disposing of ontology requires more than that we can draw *some* distinction or other between the analytic and the synthetic. It requires also that this distinction be of epistemological significance. Quine himself said, almost from the start but later with increasing clarity, that we may well be able to define some distinction, based perhaps on the way in which language is learnt, but this, he held, will lack any particular epistemological significance.[21] For Carnap's defusing of ontology to succeed, however, it is not only a distinction that we need. We need also an argument that the distinction we give is of epistemological significance; constructing such an argument is a much harder task for Carnap's defenders.[22]

Quine, then, rejects the analytic-synthetic distinction, at least in Carnap's epistemologically loaded version. In his view, we have no reason to accept that there are changes of two epistemologically distinct kinds. We have, correspondingly, no basis for thinking that some changes (Carnap's internal changes) are rule governed in a clear sense in which others (external changes) are not. Hence we have no reason to apply the principle of tolerance to changes of the latter sort.

We can reach the same conclusion by a more circuitous but more informative route. For Quine, all of our knowledge has the same aim: obtaining the best theory for predicting and understanding the course of events in the world. The idea of the "best" theory here has to do with simplicity as well as with conformity to observation. Carnap's external changes may contribute to this goal just as much as his internal changes: The change from the language of Newtonian mechanics to the language of relativistic mechanics made possible a simpler theory of the world. Even mathematics contributes to this goal, by the role that it plays in our scientific theories. So all changes

are made and justified on what is, seen very abstractly, the same sort of basis. Even if we can distinguish changes of language from changes of theory within a language, there is no reason to say that we have a notion of correctness for changes of the one kind but not for changes of the other. Changes of language would, at least in principle, be as much matters of correctness as changes of theory within a language, for all changes would be justified by appeal to the overarching aim of achieving a simple and empirically correct theory of the world. Hence there is no reason to invoke the principle of tolerance.

The rejection of the principle of tolerance makes all the difference to the status of ontology. For Carnap, the question whether we should use Newtonian or relativistic mechanics was a question of choice of language and therefore a matter for free choice rather than a right-or-wrong issue. For Quine, it is also perhaps largely a question of language but *not* therefore a matter for free choice. The language of relativistic mechanics makes possible a preferable theory and is therefore a better choice. Language relativity, after all, does not arise merely from the existence of a multiplicity of distinct languages, any more than the (supposed) relativity of ethics arises merely from the existence of a multiplicity of opinions on that subject. What is required is, in addition, the idea that there is no basis for choosing one as better than another and that therefore a relativized answer is the best we can achieve. In rejecting the principle of tolerance, Quine rejects just this sort of relativism about language choice.

The upshot of this is a general rejection of Carnap's relativism, most clearly seen, perhaps, in the case of truth. We no longer say, with Carnap, that so-and-so is true *relative to the choice of such-and-such language*. It is because of his doubts about the distinction between language and beliefs accepted within a language that Quine generally speaks simply of "theory," meaning all the sentences that we accept. He recognizes, of course, that various theories are possible, but he denies that this makes for relativity. If a sentence is part of the theory that we hold, then we accept that sentence as true – not true in some relativized sense but flat-out. (This does not show, of course, that we will not later change our minds. Nothing, in Quine's view, can rule this out; he is a fallibilist through and through.) That is simply what it is to hold a theory, and since we cannot get by without some theory or other to guide us through the world, we must hold the best we can. Having said that we can only make sense of the idea

of truth from within some theory of the world, and having admitted the existence of a multiplicity of such theories, Quine then asks,

Have we now so far lowered our sights as to settle for a relativistic doctrine of truth – rating the statements of each theory as true for that theory, and brooking no higher criticism? Not so. The saving consideration is that we continue to take seriously our own particular aggregate science, our own particular world-theory or loose total fabric of quasi-theories, whatever it may be. Unlike Descartes, we own and use our beliefs of the moment, even in the midst of philosophizing. (WO 24–5)

Quine thus rejects relativism about truth: We should simply accept the best theory we have (until, of course, a better one becomes available); in accepting it, we count it as true, true flat-out, in an unrelativized sense. And in accepting a theory, we also adopt a language: Choice of language is no more relative than is choice of theory within a language. For Quine, there are not two separate stages here, two separate issues to be settled on quite different sorts of grounds. There is only the single issue of finding the best theory (language-own-theory, from a Carnapian point of view) for coping with the world. So Quine also rejects Carnapian relativism about language choice.

This rejection of relativism about language choice is also a rejection of the principle of tolerance. If we have accepted one theory as true, then we have no need to hold that any other language is just as good. On the contrary, from the point of view of the theory that we accept – which is our point of view – any language that is not a more or less minor variant of our own will distort matters and so is to be rejected.[23]

The other side of Quine's rejection of relativism is that we take our own theory (language-own-theory) seriously, as telling us the truth about the world. For the only sense we can make of the idea of "the truth about the world" is in terms of our own theoretical understanding. Hence we take the ontological claims of our own theory seriously. If it is part of our theory of the world that there are mountains, stars, electrons, and sets, then we are committed to the idea that these things really exist. The ontological question, which Carnap had attempted to nullify, survives in Quine's work. In one sense, this is a metaphysical question – a question about what really exists. Quine is a realist and takes the objects presupposed by our

best theory of the world to be real, in the only sense that word has. In another sense, it is not metaphysical, for it is not to be settled by anything resembling *a priori* speculation. It is settled, rather, by the ordinary processes by which our theories of the world are constructed. For Quine, his own reflections on ontological commitment and his use of regimentation and of reduction (e.g., of ordered pairs to sets) are part of that process of theory construction – the part that is particularly concerned with the clarity of the theory and with the avoidance of useless questions.

Quine is thus a realist about ontology, not a relativist, at least not in the way in which Carnap was. Carnap's relativism was a relativism of language based on the principle of tolerance; Quine's rejection of an epistemologically grounded distinction between the analytic and the synthetic undermines that principle entirely. This is not to say that for Quine questions of ontology have nothing to do with language. On the contrary, many ontological questions can be conveniently phrased as questions about language, such as the choice between the language of Newtonian mechanics and the language of relativistic mechanics. The contrast with Carnap is that for Quine there *is* a correct answer to the question of language choice. If one theory enables us to predict and deal with events better than another, then the language of the first is the one we should accept. And in accepting it, we no doubt accept a certain range of entities as existing – we accept an ontology.

IV

In one way, then, Quine is not a relativist about ontology; in another way, however, he is. Indeed, the Quinean doctrine known as ontological relativity or the inscrutability of reference has become famous, even notorious. This form of relativism about ontology is not derivative from relativism about truth: On the contrary, it occurs even if we suppose all problems about truth to be settled – even if we suppose that we possess a theory of the world whose complete truth is not in doubt. Such a completely true theory is, after all, a body of true sentences. The referential burden of language, however, is not simply a matter of which sentences are true but also a matter of how we see those sentences as making ontological claims. It is thus a matter of how the whole sentences are analyzed into parts.

Here, in the gap between the significance of the sentence and the significance of its parts, Quine sees empirical slack, which is manifest in ontological relativity.

We have already seen a clear example of the gap between the significance of the whole sentence and the significance of its constituent parts. In the case of observation sentences, we saw that, taken as wholes, they get their meaning simply from their relation to sensory stimulation. If we think of them in that way, as responses to stimulation, we have no reason to view them as making any ontological claims. It becomes possible to see them as making such claims only when we take them to be part of a more sophisticated theory, which includes the notion of identity, plurals, and so on. To repeat the point of §I, the relation between language and the world by which our language comes to have empirical meaning and empirical content is not a relation between names and objects but rather a relation between sentences and sensory stimulations.

This idea is fundamental for all of Quine's views about language, and it underlies ontological relativity. For the idea applies not only to observation sentences but also to all sentences – to language as a whole. It is most clearly seen in the case of an observation sentence. One's propensity to accept or reject such a sentence depends only on one's current sensory stimulations. Other sentences are responsive not only to current stimulations but also to the other beliefs that one has. If two scientists, say, agree about what they are now seeing but disagree about whether it is evidence for a given claim, this will surely be because of some other disagreements. The claim is thus answerable not only to what they are then observing but also to the other things about which they disagree. The empirical meaning of such a sentence is not exhausted by its links to stimulations; it also consists of its links to other sentences. These sentence-to-sentence links, and the dependencies of meaning they set up, are far too complex and many-sided for us be able to reconstruct them in detail.

Let us quote a passage from *Word and Object* on this point. Quine is considering the inference that a chemist may make from seeing a greenish tint in a test tube to thinking that the substance contains copper. Such an inference – from the observation sentence 'It's greenish', let's say, to 'There was copper in it' – obviously draws on the general background knowledge that our chemist possesses. Quine

comments as follows:

The intervening theory is composed of sentences associated with one an-
other in multifarious ways not easily reconstructed even in conjecture.
There are so-called logical connections, and there are so-called causal ones;
but any such interconnections of sentences must finally be due to the con-
ditioning of sentences as responses to sentences as stimuli. If some of the
connections count more particularly as logical or causal, they do so only
by reference to so-called logical or causal laws, which in turn are sentences
within the theory. The theory as a whole – a chapter of chemistry, in this
case, plus relevant adjuncts from logic and elsewhere – is a fabric of sen-
tences variously associated to one another and to non-verbal stimuli by the
mechanism of conditioned response. (WO 11)

The meaning of 'There is copper in it' is thus by no means exhausted
by its direct links to stimulation – by an account of which stimula-
tions would prompt one to agree with it and which to disagree with
it. The sentence also plays a role in our theory and is thus linked to
countless other sentences of that theory.

   For our immediate purposes, the thing to emphasize about this
account of the significance of language is that it all takes place at
the level of sentences. It is observation *sentences* that are linked to
stimulations; it is links between one *sentence* and others that play a
role in the meaning of nonobservational sentences. We might put the
point like this: An observation sentence is directly linked to stimula-
tions, and those links determine its correct use and thus its meaning.
For a nonobservation sentence, links to stimulations are equally im-
portant, but in this case those links are partly or wholly *indirect*. The
sentence is linked to other sentences that are in turn linked to other
sentences and so on, terminating in observation sentences. This ter-
minus provides the empirical meaning for any sentence, however
indirect and complex the connections may be.[24]

   What we said about observation sentences may thus be general-
ized to all sentences, though with an important qualification. For any
sentence, there is a sense in which what one needs to know to be a
competent user of that sentence does not depend on the structure of
the sentence – on its analysis into constituent parts. If one uses the
sentence as a whole correctly, both in relation to stimulations and in
relation to other sentences, then one is a competent user of it; that is
all that can be required. In particular, it is not required that one see

the sentence as made up of parts that are themselves significant. The qualification is that it will in fact be impossible for us to use theoretical sentences correctly without an analysis of those sentences into significant constituent parts. Take the most obvious case, that of the "so-called logical connections" between sentences. A logical connection may be thought of as summing up infinitely many facts of the following form: Whenever it is appropriate to assert *that* sentence, it is also appropriate to assert *this* one. But we could not learn all of these facts one by one. What we learn – implicitly at first, explicitly from logic books – is that whenever it is appropriate to assert a sentence of *this form*, it is appropriate also to assert a corresponding sentence of *that form*. But this requires that we attribute forms to sentences, that is, see them as made up of constituent parts and patterns that are significant and recur in other sentences.

Let us see where we are in our argument. Reference is a relation between words – significant constituent parts of sentences – and objects. The fundamental relation of language to the world, the relation that guarantees empirical significance to our language, however, does not take place at that level but rather at the level of the relation between sentences and stimulations. We are bound to attribute significance to constituent parts of sentences because it is only in that way that we can see the patterns and analogies in language that make it possible for us to use it. This situation leaves it open that there might in fact be more than one way to attribute structure to our sentences, more than one way to attribute significance to their constituent parts – more than one way to analyze them so as to make perspicuous sense of our language. And note that if there were, then there would be no way to bring evidence to bear on the question which of the methods of analysis is correct. *Evidence*, as Quine conceives it, bears on whole sentences, and the imagined dispute is between two ways of breaking sentences down into constituent parts, ways that will make no difference to which sentences are to be accepted.

Here we have, in outline, Quine's doctrine of ontological relativity, or at least the necessary background to that doctrine. Let us see how Quine encapsulates some of the points we have been making:

Reference and ontology recede thus to the status of mere auxiliaries. True sentences, observational and theoretical, are the alpha and omega of the

scientific enterprise. They are related by structure, and objects figure as mere nodes of the structure. What particular objects there may be is indifferent to the truth of observation sentences, indifferent to the support they lend to theoretical sentences, indifferent to the success of the theory in its predictions. (PTb 31)

Objects, along with reference to objects, are central to our theory of the world, for we cannot get by without analyzing our sentences so as to reveal structure: Otherwise we could never master the sentence-to-sentence links that are essential to language beyond the observational level. For all that, however, the role of objects is secondary; the truth of sentences and their links to one another are primary. Hence the idea that we could analyze sentences in more than one way while leaving untouched both their truth-values and all the infinitely complex links among them.

So far we have spoken of this idea as no more than an abstract possibility. Quine, however, is in no doubt at all that there are such alternative methods of analysis. He says, indeed, that the matter "admits of trivial proof" (RJW 728). There are in fact a number of ways in which Quine thinks the point can be demonstrated. Perhaps the simplest, and certainly the one to which he most often appeals in recent writings, employs the idea of a *proxy function*. The idea here is that we take any one-to-one function *f* defined over the objects to which our beliefs apparently commit us. (An example for spatiotemporal objects, trivial but no worse for that, is *spatiotemporal complement of*. This function maps any given object onto the rest of space-time, i.e., to all of space-time excluding the given object.) Now we reinterpret each sentence that appears to be about an object *x* as being instead about *f(x)*. And we also reconstrue each predicate so that it holds of *f(x)* (the spatiotemporal complement of *x*, in our example) just in case the original predicate held of *x*.

In one sense the reconstruals – of objects and of predicates – cancel out, in trivial fashion, to leave the sentence unaffected. A reconstrued sentence will be true under any particular circumstances just in case the original version was also true under the same circumstances. Since truth-values are unchanged, so also are inferential relations among sentences and all of the sentence-to-sentence connections. So at the level of sentences and truth, it seems as if talking in the proxy-function language instead of our own would simply make no difference at all; hence it might seem as if these are not two separate

languages but just two ways of describing the same language. At the level of the constituent parts of sentences, however, there is a difference. Does 'Rover' refer to the family pet or to all of space-time other than the family pet? (It makes no difference here whether we leave the name unanalyzed or subject it to Russell's theory of descriptions.) We thought we knew the answer, but Quine's proxy functions may seem to call it into question.

How are we to think of this argument and its significance? One way in which the point is often put is in terms of translation. Suppose Martians were to land (or merely to observe us with their superfine instruments), and suppose too that for them the object language (in which names refer to objects) and the complement language (in which they refer spatiotemporal complements of objects) were about equally natural or unnatural. Then two different Martians might come up with wholly different accounts of our ontology, two accounts that attributed the same net import to each human sentence.[25] And, crucially, there would then be nothing to which either of the Martians could appeal to show that the one account was correct and the other incorrect. The way in which evidence relates to a given sentence would be equally accounted for by the two translations. A gesture such as pointing would on the one account indicate an object in the direction of the pointing finger; on the other account, however, pointing would simply indicate the complement of something in the direction of the pointing finger. *Asking* the person pointing which object he intended would do no good: He might say 'Rover, of course', but the issue is precisely whether Rover is the one object or its complement. Nor would more subtle questions help, for there is no agreement about the construal of the language in which the questions themselves would be asked and answered.

In short, there will simply be no settling the question which of the translations is correct; the case has been set up in such a way that this unsettleability is an intrinsic feature.[26] And for Quine this means that there is no "correct" answer here. What the case shows has nothing to do with the Martians' ignorance or their inability to get at some fact. Its significance, rather, is that there is no fact to be gotten at, because ontology simply is relative – relative, in particular, to the choice of some general system for translation.[27]

We introduced the point about translation by talking about Martians, but this was of course unnecessary. The same point applies if two of us each undertake to translate a third person's talk. We

might come up with translations that agreed about the net import of each sentence but still differed in the ontology they ascribed to the person being translated. I might translate you as speaking the proxy-function language, and my translation would have just as much claim to correctness as the translations of your friends and neighbors, who translate you as speaking our normal language. There would be no fact of the matter as to which translation was correct and thus no fact of the matter as to which language you were really speaking or which ontology you really accepted.

At this point one may think that the whole idea of ontological relativity collapses into incoherence.[28] For of course my words too are subject to various translations equally correct. Suppose I speak (what appears to be) the object language but translate you as speaking the complement language. Someone else can with equal justice claim that I am in fact speaking the complement language so that when I translate you I am actually attributing to you the object ontology, not the complement ontology. We seem, in short, to be in danger of a regress. I translate your use of the word 'Rover' as referring to the space-time complement of the family dog, but if my words themselves are subject to various translations, what claim do they actually make? And if someone were to answer this question, still *her* words would be susceptible of various translations, and so on. The idea of translation seems to be undermined here by the lack of a stable language into which to translate – a language that simply says what it says.

It is tempting to put an end to this difficulty by positing for each person a language that is in this sense stable. Or never mind the others: It is tempting for *me* to suppose that *I* have a language in which I can simply mean what I mean – objects and not their complements. If my words are always subject to reconstrual, then this simply shows that they do not fully capture what I *mean*: My meaning at least must be fully determinate. This line of thought is, as I say, tempting. And it goes along with the idea that understanding someone always involves translating them. For if I understand you by translating you, then I must have some language into which I translate. And if we are to avoid a regress, I cannot understand that language by translating it into yet another language. On the contrary, at some point I have to have a language that I understand in some altogether more immediate fashion; this would be the language in

which I can determinately say what I mean. This language would be private to me alone, since anything I can utter, anything I can make public, is, we are supposing, subject to reconstrual along the lines indicated, whereas my language is not.

This picture is tempting, and it is to some extent reinforced by remarks of Quine in which he suggests that to understand someone's words is to translate them.[29] But the tempting picture cannot be a correct account of Quine's views. The idea of a language that is in this sense private, in which my real (determinate) meaning never gets expressed – this idea is completely foreign to the man who began the preface to *Word and Object* with the sentence "Language is a social art." It is completely foreign to his whole naturalistic, antimentalistic outlook. Apart from the sorts of remarks just mentioned, nothing in Quine's work even suggests such a view of language.[30] Also, as we shall see, he does suggest other ways of stopping the regress that the private language was invoked to stop.

If not by a private language, then how is the regress of languages to be stopped? If I do not understand your words by translating them into my own language, how are we to think of understanding? Take this second question first. I may occasionally do something like translating you if you use words that I do not immediately understand or if you make a remark that strikes me as excessively odd when taken in the sense that first comes to mind. But normally I do not translate your words. Normally I simply respond to what you say – by uttering some remark or by modifying my actions in some way (at the least, my dispositions to act change). To understand someone's utterances is not, in the usual case, to translate them into some other language; it is simply to be disposed to respond to them in appropriate ways, linguistic and nonlinguistic, immediate and long term. And this provides the clue that we need to attempt to answer the first question. When I say of you that your word 'Rover' refers to the family dog and not to its space-time complement, what gives my words meaning is simply that they are part of our familiar shared language, which, for the time being at least, is unquestioned. It is not a place at which we find *real reference* – in the sense of reference that is in some more or less mysterious fashion not susceptible to the inscrutability argument. But it is a language that we can and do for the most part simply use, without attempting to consider it from the vantage point of some other language. As Quine says, "[I]n practice

we end the regress of background languages by acquiescing in our mother tongue and taking its words at face value" (OR 49).

It is crucial here that what give our words their meaning is our use of them, not our translations of them. For it is with translation, but only with translation, that the issue of inscrutability arises; while we are simply using our language, there is no such issue. Let us look at two longer comments of Quine's on this subject:

To say what objects someone is talking about is to say no more than how we propose to translate his terms into ours....

The point is not that we ourselves are casting about in vain for a mooring. Staying aboard our own language and not rocking the boat, we are borne smoothly along on it and all is well; 'rabbit' denotes rabbits, and there is no sense in asking, 'Rabbits in what sense of "rabbit"?' Reference goes inscrutable if, rocking the boat, we contemplate a permuational mapping of our language on itself, or if we undertake translation. (TPT 20)

And again:

Within the home language, reference is best seen (I now hold) as unproblematic but trivial, on a par with Tarski's truth paradigm. Thus 'London' denotes London (whatever *that* is) and 'rabbit' denotes rabbits (whatever *they* are). Inscrutability of reference emerges only in translation. (RPR 460)

The point here, I think, is that while we are speaking our familiar language (or a language whose translation into it is well established), the inscrutability of reference simply gets no grip. It does not in anyway interfere with language use or force us to modify our account of that use. And this is true also of that special case of language use in which we use some words to say what other (or indeed non-other) words refer to. There is, of course, no saying what objects a word refers to except by using, and thereby taking for granted, some language. But this should not seem threatening or paradoxical: There is no saying anything except by using and taking for granted some language and indeed, on Quine's account, some substantive theory of the world. If this continues to seem paradoxical, it is perhaps because the point of §I is hard to absorb. We tend to treat the notion of an object as fundamental rather than seeing objects merely as "neutral nodes [in] the structure of [our] theory" (PTb 33). We tend, therefore, to think that there should be something that an object really and truly *is*, is in itself – something that outruns the role it plays in our

theory of the world. The lesson of inscrutability, by contrast, is that there is no more to an object than its role in theory.[31]

V

Finally, I want to raise, and very briefly to address, a difficult question on which Quine's own writings give us comparatively little guidance. To what extent does the relativity of ontology, as we discussed it in the previous section, undermine the importance that Quine attributed to ontology and to reference, at least in earlier decades? There was certainly a shift of emphasis in his work as he came to formulate the inscrutability of reference and then to lay greater stress on it. The present question is whether this was merely a shift of emphasis or did it also have repercussions on the central Quinean tenets relating to reference?

For Quine, as we saw in §II, studying the referential structure of our language is a central way of gaining insight into its functioning; clarifying that structure – replacing it with a better – is a central way in which we may clarify and improve our language and our conceptual scheme. These ideas seem to me quite untouched by an acceptance of ontological relativity. The insight into our language that studies of reference afford is precisely an insight into its referential *structure*, and this is not affected by ontological relativity. That idea suggests that it is indifferent whether we take ourselves to be referring to one set of objects or to another set of objects that play the exact same roles in our theory. And it is the roles that are at stake in Quinean studies of reference.

When Quine suggests ontological reform, the sort of clarification he offers also survives, I think, an appreciation of ontological relativity. Thus he offers a definition of ordered pairs, for example, and a way of reconstruing talk of states of mind as talk of states of body.[32] The ontological reforms Quine favors can perfectly well be understood in terms of an object's theoretical role (i.e., its place in the structure of our theory). Let us consider minds and bodies. Quine's reasons for not wanting to take talk of states of mind at face value, such as their lack of clear identity criteria, will apply equally to anything playing the same theoretical role – that is, to any proxy for states of mind. And by the same token, any proxy for states of body will have the advantages that lead Quine to prefer them to states of

mind as entities to take seriously in our system of the world. If we invoke any proxy function and use it to translate everything he says on this subject, all of his reasons will remain intact.

The sort of concern with reference that came to the fore in §II thus seems to me unaffected by ontological relativity. Nor should this surprise us, for Quine was already aware of that doctrine – though not yet of its distinctness from indeterminacy of translation – when he wrote *Word and Object*. Thus in the preface to that book he alludes to ontological relativity and then says,

Studies of the semantics of reference consequently turn out to make sense only when directed upon substantially our language, from within. But we do remain free to reflect, thus parochially, on the development and structure of our own referential apparatus. (WO ix)

The relation of ontological relativity to the issues discussed in §III may seem more dubious. The emphasis there was on Quine's realism: his insistence that the objects that our best theory presupposes really do exist, that our commitment to the existence of such objects cannot be waived by invoking language relativity. But what is left of our insistence that the objects we refer to are real if we acknowledge that it is indeterminate just which objects those are? 'Rover' must be taken as referring to a real object, we insist; we then add, however, that this object may be the family dog, or its space-time complement, or a set of canine time-slices, or a set of real numbers corresponding to the space-time points occupied by the dog, or who knows what else? What is the force of the original insistence in the face of these additions?

It is hard to dismiss these questions entirely. For Quine, however, I think their force is not so much to undermine realism about objects as to show us what it can coherently come to. The clue here is provided by the idea emphasized at the end of §IV: that there is no more to an object than its role in the theory. Quine, taking the logician's ingenuity perhaps to the point of perversity, adds to this idea the claim that we can systematically switch objects from role to role.

The result is ontological relativity. By reconstruing predicates, we can, for example, switch the roles of Rover and his space-time complement, and so on for all other physical objects. The possibility of this sort of switching, if granted, seems to undermine realism by indicating that we do not really know what we are being realistic about;

we feel as if we are in the odd position of insisting that something must exist but having to acknowledge that we cannot say what. But if all that there is to an object is the role that it plays in theory, then what is it that we do not know? According to the view considered in §IV, the correct way to think of an object is simply as marking a certain role – a "neutral node" – in the structure of our theory. So it would seem that all that being a realist about an object can come to is, so to speak, taking that role, that node, seriously. But this is simply to take seriously the theory of which it is an aspect. So we should perhaps take ontological relativity to show that there is no issue concerning realism about objects separate from the issue of realism about the theory that mentions them: To repeat, ontology is derivative from truth.

This conclusion may seem to be too anodyne to be the moral of the seemingly bizarre idea of ontological relativity, for it simply repeats the moral of §I, which seemed moderate enough. I am inclined to think, however, that the ideas of §I are less moderate, less in conformity with unreconstructed common sense, than may have appeared and that the apparent oddness of ontological relativity was lurking from the start. We are, as Quine says, "body-minded" (RR 54); it comes entirely naturally to us to think in terms of bodies, and of objects more generally, and to take them as fundamental to our knowledge. Russell's way of articulating this nexus of ideas, when dissected and exposed to the light of day, may easily come to seem offensive to common sense, but it is an articulation of ideas that are, I think, very natural. The apparently paradoxical character of ontological relativity arises in part because Quine's ingenuity enables him to draw conclusions that most of us would never have dreamed of. But it also, and more significantly, arises because the notion of an object and the nexus of ideas associated with it go very deep in our conception of the world. Ontological relativity dramatizes the fact that Quine's work repudiates those ideas completely.

NOTES

For comments on an earlier draft, I am indebted to Roger Gibson, Dorothy Grover, and Bill Hart.

1. Russell was an important influence on Quine, second only to Carnap. In a tribute to the latter, Quine says, "I see him [Carnap] as the dominant

figure from the 1930s onwards, as Russell had been in the decades be-fore" (HRC 40). I shall speak here of Russell's views in the first two decades of this century and shall not be concerned with subsequent shifts of doctrine.

2. Thus, in *Problems of Philosophy* (Oxford: Oxford University Press, 1952; first published in 1912), Russell says, "The faculty of being ac-quainted with things other than itself is the main characteristic of a mind. Acquaintance with objects essentially consists in a relation be-tween the mind and something other than the mind; it is this that con-stitutes the mind's power of knowing things" (p. 42).

3. For an elaboration of this very hasty summary, see my book *Russell, Idealism, and the Emergence of Analytic Philosophy* (Oxford: Oxford University Press, 1990).

4. Implicit here is a point that would require elaboration in a more detailed treatment; what counts as an observation sentence depends on the size of the community of language speakers. For some purposes it is relevant to think of all normally functioning speakers of the language, but for the purposes of explaining a child's acquisition of language, a smaller community – perhaps a very small one – may be appropriate. Quine's views on observationality have shifted over time. For a recent statement, see PTF 159–63.

5. Because observation sentences are, in given circumstances, quite uncon-troversial, it will also be uncontroversial whether given circumstances falsify an observation categorical. But since an observation categorical is a generalization, its truth will not vary from occasion to occasion and may be quite controversial.

   Note also that in practice a test of a theory will seldom need to go all the way down to the level of observation sentences and the correspond-ing observational categoricals. Almost always there will be higher level sentences that are still agreed upon by the various parties to a dispute; perhaps we should think of these sentences as observational for the lin-guistic community concerned (advanced organic chemists, or whatever the relevant group may be). But still in such cases there will be a more fully observational sentence to which appeal could be made if there was no agreement at higher levels, and Quine's idealization of the process seizes upon this fact.

6. Most obviously in *Roots of Reference* and in parts of *Word and Object*.

7. Thus Quine says, "In *Roots of Reference* I was posing a Kantian sort of question: how is reification possible?" (CP 291). Again, in *Pursuit of Truth* he says that one of the advantages of beginning an account of language acquisition with observation sentences is that we "are freed

to *speculate* on the nature of reification and its utility for scientific theory" (PTb 8, emphasis added).

8. See WO 93. In a striking image, Quine there compares such acquisition to a climbers ascent of a "chimney," or narrow space between two rock faces: "The contextual learning of these various particles goes on simultaneously, we may suppose, so that they are gradually adjusted to one another and a coherent pattern of usage is evolved matching that of society. The child scrambles up an intellectual chimney, supporting himself against each side by pressure against the others."

9. Ludwig Wittgenstein, *Philosophical Investigations*, trans. G. E. M. Anscombe (New York: Macmillan, 1953), §257.

10. Very roughly, first-order quantification theory is a logic that enables us to generalize over all objects – and hence to speak of all objects being thus-and-so, or no objects, or at least one object. Quine generally takes logic to include the notion of identity and rules governing its use (see PL, chap. 6). By contrast with first-order logic, second-order logic would add the capacity to generalize about properties or qualities of objects. Quine does not favor second-order logic, both because its truths cannot be captured by any precisely formulated system of axioms or rules and because he finds the idea of a property or quality to be unclear. He holds that it is both philosophically less misleading and technically more advantageous to use instead a combination of first-order logic and set theory.

11. Thus Quine speaks of the "triviality" of the connection between ontology and quantification, saying that the connection "is trivially assured by the very explanation of referential quantification" (see RFK 174–5). By "referential quantification," Quine here means the sort of qualification just explained, which he, along with most logicians, generally takes as standard. It is opposed to "substitutional quantification," in which we think not of open sentences being true of this or that object but rather as yielding truths when a given name replaces the variable. The difference shows up if we admit objects not all of which have names; in the case of real numbers, we cannot avoid doing so, since there are more of them than there can be names.

12. Quine is perhaps influenced here by his early work in set theory, where the question of the entities in the range of the quantifiers of a given theory is a very natural measure of the strength of that theory and where the strength of the theory is in turn crucial to its vulnerability to paradox. I am indebted here to Stephen Menn.

13. The theory is first set forth in Russell's "On Denoting" (*Mind* 14 [1905]: 479–93; very widely reprinted) but is perhaps to be seen more clearly in

Chapter 16 of his *Introduction to Mathematical Philosophy* (London: George Allen and Unwin, 1919). Note that Quine uses it to show the possibility of eliminating *all* names. Russell's concern was with the elimination only of those names that name objects with which we are not acquainted. In the end, however, Russell comes to hold that we are acquainted with almost none of what we ordinarily think of as objects; from some points of view, the net result is not so far from Quine, though there are great differences along the way.

14. We can, of course, form sentences such as '(∃x)(∃y)(x is an object. y is a property. x has y)'; but in this sentence the predicative work is done by 'has'. The issue here, in any case, is whether *every* sentence using a predicate is thereby committed to the existence of properties. Whether there might be some limited class of sentences of which this is true is a different question; Quine's answer is the same, though his reasons are not.

15. This is not to say that Quine rejects all abstract entities. On the contrary, he accepts the existence of classes. But this is because any convenient statement of our knowledge will include sentences that explicitly quantify over classes. It is not because a naturalistic account of how we use and understand such sentences postulates causal contact between us and classes: It does not.

16. This passage is from TPT 9; it overlaps, to a large extent, a passage from FM 160.

17. Quine says explicitly, "I use 'science' broadly," and he includes psychology, economics, sociology, and history among the "softer sciences" (FSS 49). I follow this usage.

18. Some critics have tried to make out a difference in Quine's use of these two phrases, but Quine himself intends no such difference. See RPR 459.

19. I hope it is unnecessary to stress that the treatment of Carnap given here is very cursory and, inevitably, does not do justice to the force of his position.

20. See *The Logical Syntax of Language* (London: Routledge & Kegan Paul, 1937; first published in German, 1934), esp. 51–2; see also Carnap's "Intellectual Autobiography," in *The Philosophy of Rudolf Carnap*, ed. Paul Arthur Schilpp (La Salle, Ill.: Open Court, 1963), 54–5.

21. See, e.g., CLT 396, RR 78–80, PTb 55–6, and esp. RGH 207.

22. In "Analyticity and the Indeterminacy of Translation" (*Synthese* [52]: 1982, 167–84), I argue that Quine's arguments against the epistemological significance of the distinction are to a large extent separable from his claims that there is no distinction.

23. It is clear at this point that talk of "language" here is not exactly talk of languages in the ordinary sense. Modern scientific French, German,

Russian, and Chinese would presumably, from this point of view, count as minor variants on modern scientific English – although from the point of view of the poor language-learner they are very diverse languages indeed.

24. This account perhaps plays down Quine's *holism* – the view that it is in general not individual sentences in isolation that have links to stimulations but only more or less inclusive classes of sentences. But there is no contradiction between the holistic point and the way I am putting the matter here. If a given sentence has links to stimulations only as part of a more inclusive class of sentences, then it is, we might say, linked to the other sentences that make up that class. Such links – and hence the more inclusive class – would have to be taken into account in considering the meaning of the individual sentence. Part of the complexity here can be seen from the fact that many sentences will occur in indefinitely many such classes.

25. Quine has also argued that it is conceivable, at least, that two Martians might come up with equally correct translations that did *not* attribute the same net import to each human sentence but diverged in exactly this particular. That is the doctrine known as "the indeterminacy of translation," discussed in Chapter 6 of this volume. The two doctrines – indeterminacy of translation and ontological relativity – were presented at a single point in *Word and Object* (chap. 2), and Quine was not immediately clear on the differences between them. More recently he has emphasized their differences. In particular, as we saw, he thinks that the latter can be proved whereas he has spoken of the former as a "conjecture" (RJW 728).

26. This is, of course, an extremely controversial claim. There is no room here to go into the controversy; I am simply trying to represent the matter as I think Quine sees it.

27. Compare Quine: "Ontological relativity is the relativity of ontological ascriptions to a translation manual" (RPR 460).

    Note that once the general scheme of translation is in place, there is room for factual dispute. Two Martians who have both adopted the complement translation may argue about whether 'Rover' refers to the complement of the family dog or to the complement of the family cat. In this case, one of them is right and one wrong, and the matter is settled in exactly the same way as the analogous dispute between two adherents of the other general scheme of translation.

28. I have attempted a more discursive treatment of the issues discussed over the next few pages in my essay "Translation, Meaning, and Self – Knowledge," *Proceedings of the Aristotelian Society* 91, pt. 3 (1990–1): 269–90.

29. See, e.g., OR 46. I do not think that Quine's remarks there must be read as implying the view that I think cannot be his, but I shall not argue this point here.

30. The opening pages of "Ontological Relativity" (pp. 26–7) also explicitly reject the idea of a private language – and do so in a way that makes it seem as if Quine simply never took the idea seriously at all. There are innumerable other passages in which Quine assumes, and occasionally insists on, the public nature of language.

31. In "Ontological Relativity," Quine said, "What makes sense is to say not what the objects of a theory are, absolutely speaking, but how one theory of objects is interpretable or reinterpretable in another" (OR 50). I think that later he might have been inclined to say that the idea of speaking *absolutely* in this sense is a mere chimera from the start and not a reasonable idea to be ruled out – not, so to speak, something that we might be able to do but cannot but rather an incoherent idea that dissolves under examination. See the essay referred to in n. 28.

32. See WO §53 and §54. For the latter, more controversial, kind of case, see also TPT 18–19.

ROBERT KIRK

6   Indeterminacy of Translation

1. INTRODUCTION

Quine's doctrine of the indeterminacy of translation has been described as "the most fascinating and the most discussed philosophical argument since Kant's Transcendental Deduction of the Categories."[1] Yet it has proved extraordinarily hard to state clearly without trivializing it. An illustration will give a preliminary idea of what it is about.

Suppose a German physicist remarks, 'Das Neutrino hat keine Masse'. Then any English-speaking physicist with a knowledge of German will translate that sentence by 'Neutrinos have no mass'. That meshes perfectly with the going scheme for translating between the two languages and raises no problems at all. However, if Quine is right, it would be possible to devise an alternative scheme for translating between German and English that fitted all the relevant objective facts yet offered as its own version of 'Das Neutrino hat keine Masse' an English sentence that we should all agree was not even loosely equivalent to 'Neutrinos have no mass'. I cannot say what such an alternative translation would be like. The trouble, according to Quine, is that to produce a complete alternative scheme for translating between a given pair of languages would require too much time and effort to be seriously considered. (The project seems unlikely to attract a grant.) Attempts have been made to construct simple examples, but they are not compelling (see §11). Still, that illustration will serve to convey the general idea – except that without further explanation it is likely to create misunderstandings. I will try to forestall the commonest ones straight away.

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2. WHAT THE DOCTRINE IS NOT

One mistaken view is that Quine's doctrine is just an instance of the truism that finite data do not fix a unique rule covering cases not actually encountered. If we had only the inscriptions on the Rosetta stone to go on, for example, we could devise infinitely many incompatible schemes for translating between the three languages represented on it. Quine's point, in contrast, is that it would be possible to devise a rival to the usual scheme for translating sentences between, say, German and English that would fit not just the data we have about the actual behavior of German and English speakers but the *totality* of relevant objective facts about the two languages. I will discuss later the vital question of what the relevant facts may be.

Another common misunderstanding is that Quine is overdramatizing the familiar point that translation requires the exercise of judgment because there will always be a range of alternative yet inequivalent versions in one language of sentences from a language with different concepts and syntax. It would be silly to expect that a poem, for example, must have a uniquely correct translation. Now Quine's thesis does not depend on subtle nuances, so we can forget the special case of poetry. It may still seem that he is only emphasizing a simple point: In translating between relatively remote languages and cultures, inequivalent sentences of one language will often do equally well as rough translations of a single sentence of another. Color words are a good example: Different languages often draw the boundaries of their color concepts at different places. For two important reasons, that cannot be what he is getting at. The first is that one of his main purposes in pressing the indeterminacy doctrine is to undermine the assumption that what we mean by what we say is a *question of fact*. He appeals to the indeterminacy to back up the claim (to be discussed shortly) that the whole idea of meaning and sameness of meaning is little more than a convenient way of talking, without solid empirical foundation. The second reason is even more decisive: He maintains that the doctrine holds even within a single language, where it would be nonsense to say there was conceptual incongruity.

You might protest that bilinguals – if necessary a committee – could settle any potential disagreements. According to Quine, they are no better placed than anyone else when it comes to adjudicating

between entire rival schemes of translation: Their schemes are still subject to the indeterminacy. If they agree among themselves, that is not surprising, since they probably acquired their languages in the context of a generally accepted system for translating between them.

Finally, the point is metaphysical rather than epistemological. You may have been suspecting that Quine is just a skeptic, refusing to accept ordinary evidence for what people mean by what they say. But ordinary skeptics do not deny that there *are* facts about meaning, synonymy, and so on, they just maintain that the evidence always falls short of justifying our beliefs about what those facts are. Quine's position, in contrast, is that there are no such facts. He often puts his point in those terms: "[T]here is not even . . . an objective matter of fact to be right or wrong about" (WO 73). He is not so much a skeptic about sameness of meaning as a nihilist.

By now you may be wondering what on earth is going on. Can that be Quine's position, or is it just a caricature? Bewilderment is a common symptom when people first learn about the indeterminacy doctrine: If you don't find it puzzling, you haven't understood it. At any rate, that is so if you are not content to represent it as trivially true or trivially false, as some interpreters do – unlike Quine himself, for whom it is a "serious and controversial thesis" (PTb 50). Here, for reference, is his own first statement of it:

[M]anuals for translating one language into another can be set up in divergent ways, all compatible with the totality of speech dispositions, yet incompatible with one another.[2] (WO 27)

3. BACKGROUND

A look at the background to this thesis will be useful. Quine's doubts about the soundness of common assumptions about meaning and related notions showed up in his assault in "Two Dogmas of Empiricism" on traditional ideas about the analytic-synthetic distinction. The logical positivists had assumed that that distinction was well founded and tended also to assume that the meaning of a sentence was associated or even identical with a set of experiences: the experiences that would verify it. On Quine's holistic approach, the grounds for accepting or rejecting individual statements cannot be so simple: Our beliefs have to be assessed as parts of our total theory of the

world. Experiences can suggest that our theory needs revision, yet which revisions we should make depend not just on the parts of it most directly associated with the experiences but on the system as a whole. Seeing drops of water on the window would normally lead me to believe it was raining, but if I happened to know there were builders on the roof using a hose, I might not acquire that belief. In that and similar ways, "Our statements about the external world face the tribunal of sense experience not individually but only as a corporate body" (TDEb 41). So the meanings of individual statements cannot be paired off with sets of experiences. For the same holistic reason, they cannot be paired off with patterns of behavior either. That starts to make the whole idea of meaning seem less firm and objective than we tend to assume. True, meaning and sameness of meaning (synonymy) can be defined quite neatly if we may rely on such notions as those of *analyticity*, *proposition*, and *necessity*. But those notions themselves are all hard to define in reasonably clear terms, and Quine's view is that they are in the same leaky boat together.

Quine tends to associate his rejection of the objectivity of meaning with rejection of the conception of meanings as mental entities – the myth of the mind as a museum where "the exhibits are meanings and the words are labels" (OR 27). However, those are distinct issues. It seems possible to join with him (and Dewey and Wittgenstein) in rejecting that primitive conception while still insisting that it is a matter of fact whether two sentences mean the same. (It is not the notion of meanings as entities that Quine objects to. He points out that if synonymy were a matter of fact, meanings could be defined as classes of synonyms. His objection is to the assumption that meaning and sameness of meaning are objective.)[3]

He concedes that the situation would be different if the notion of meaning, even if not definable in reasonably clear terms, had a useful role to play in explaining behavior. We tend to *assume* it plays such a role. Why did that German say, 'Das Neutrino hat keine Masse'? Because she believes photons have no mass, and that is what the sentence *means*. Such remarks appear to be explanatory. However, Quine has called that sort of thing "spurious explanation, mentalistic explanation at its worst" (MVD 87). Of three levels of explanation of behavior, the mental is "the most superficial" and scarcely deserves

the "name of explanation," the physiological is "deepest and most ambitious," but for practical purposes we must rest content with the third variety: explanations in term of behavioral dispositions. That is "what we must settle for in our descriptions of language, in our formulations of language rules, and in our explications of semantical terms" (MVD 88).

The indeterminacy thesis appears to give powerful support to Quine's views on the shortcomings of the notion of meaning. If two schemes of translation can both fit all the relevant objective facts yet still be in substantial conflict with one another, sameness of meaning itself cannot be a matter of objective fact. For Quine, that whole scheme of description and explanation is misconceived. One way in which he has expressed this point is in terms of Brentano's view that "intentional" notions cannot be defined in other terms.

One may accept the Brentano thesis either as showing the indispensability of intentional idioms and the importance of an autonomous science of intention, or as showing the baselessness of intentional idioms and the emptiness of a science of intention. My attitude, unlike Brentano's, is the second.[4] (WO 221)

4. THE DOMESTIC CASE

If Quine is right, relations of synonymy are not matters of fact even when they are supposed to hold between sentences of one and the same language. I tend to presuppose that, for each sentence of our shared language, what you mean by it is also what I mean by it (usually, of course, there may be exceptional circumstances). I "translate" each sentence of your language by that same sentence. Quine thinks that if I were perverse and ingenious, I could "scorn" that homophonic scheme of translation and devise an alternative that would attribute to you "unimagined views" while still fitting all the relevant objective facts, including facts about your verbal and other behavior (see WO 78).

But it is still not clear what he is driving at, since there is room for different interpretations of 'fitting the facts'. Two questions have become urgent: What are the relevant objective facts, and what is it for a translation manual to fit them?

5. THE FACTS

If the facts are defined too narrowly, the doctrine is trivialized. As we have noticed, Quine often writes in terms of behavioral dispositions, but these may be construed more or less broadly. It might be useful now to get acquainted with an idea he uses in *Word and Object*. At any moment each of us is undergoing a certain pattern of sensory stimulation, which may influence our dispositions to say 'Yes', 'No', or 'Maybe' when a sentence is pronounced with a rising (querying) intonation. Faced here and now with the queried sentence 'The sun is shining', for example, I am disposed to indicate assent. To 'There's a rabbit on the desk' I will indicate dissent. To 'That man is a father' (queried on spotting a stranger) I will withhold a verdict. These dispositions to assent, dissent, or withhold a verdict when hearing a given sentence uttered queryingly while at the same time being exposed to a particular brief pattern of sensory stimulation provide the basis for a very narrow conception of behavioral dispositions. Suppose we say it is exclusively those narrowly conceived verbal dispositions that are to be *the facts* that schemes of translation must fit. And suppose we add that for a scheme of translation to fit those facts is simply for it to pair off sentences in such a way that for each possible pattern of stimulation (within some brief time interval) mature speakers of the two languages share the same dispositions to assent, dissent, or withhold a verdict when the sentence is queried. (That would be to match up "stimulus meanings"; see §7.) Notice now that for all practical – and Quinean – purposes, we are disposed to assent to familiar mathematical truths under all patterns of stimulation. That being so, the indeterminacy doctrine, when fitting the facts is construed as above, can be quickly seen to be true even for the domestic case. We could render '$2 + 2 = 4$' by 'There is a prime greater than 2' and vice versa, mapping every other sentence onto itself, and our narrowly construed verbal dispositions would remain unchanged. (We could extend the idea so as to match up infinitely many such pairs.) Since no one without a heavy philosophical axe to grind would ordinarily regard those two sentences as even loosely equivalent, that perverse scheme of translation would be a genuine rival to the homophonic scheme, yet it would still fit the facts in the narrow sense defined. Nor need the example be confined to mathematical truths. We could carry the transposition over

into compound statements, rendering 'It's raining and $2 + 2 = 4$' by 'It's raining and there is a prime greater than 2', which again would not ordinarily be counted as meaning the same. (Notice, by the way, that it would not help to apply to the question of conflict between schemes of translation the same very loose conception of equivalence implied by mere agreement in dispositions to respond. By that standard, the above pairs of sentences would count as equivalent – in which case the two schemes of translation would not be in conflict, and the corresponding indeterminacy thesis would lose its support.)

The resulting thesis, though true, has no interesting philosophical implications. It construes 'fitting the facts' much too narrowly. However, the facts must not be allowed to include too much. To include beliefs, desires, and intentions among them, for example, would demolish the doctrine, for if such were assumed to be objective, they would fix meanings and block the indeterminacy. As Quine remarks in *Word and Object*, "[U]sing the intentional words 'believe' and 'ascribe', one could say that a speaker's term is to be construed as 'rabbit' if and only if the speaker is disposed to ascribe it to all and only the objects that he believes to be rabbits" (WO 220–1). He could hardly resist such further definitions as this: '*S* means that *p* (in language *L*) if and only if *S* would typically be used by *L*-speakers to express the belief that *p*.' So if we had some independent basis for maintaining the objective soundness of such intentional notions as those of belief and desire, that would be a quick way to refute Quine's position.

But as the quotation referring to Brentano's thesis showed, Quine thinks the indeterminacy reveals the "baselessness" of such notions. If it is a mistake to think that relations of synonymy and translation are matters of fact, then it is also a mistake to think that people's beliefs, desires, intentions, and the rest are matters of fact. The implications of the indeterminacy doctrine cannot be kept caged inside the philosophy of language. They strike at psychology and the philosophy of mind as well.

What *should* be counted as objective facts in this context? Critics have objected to Quine's sometimes seeming to restrict the objective facts to verbal dispositions, even to verbal dispositions construed in the narrow sense just discussed – "dispositions to respond verbally to current stimulation" (WO 28). One reason why the facts had better not be restricted so tightly is that a lot of the behavioral capacities that bear on our understanding and use of language cannot be

adequately summarized in terms of such dispositions. Such things as our abilities to follow directions in the street, look up words in dictionaries, and count the fish in a pond reflect our grasp of language without being exhaustively representable by dispositions to respond to queried sentences. As Quine himself remarked, "[I]t would be wrong to suppose that learning when to volunteer statements of fact *or* to assent to them is all or most of what goes into language learning. Learning to react in appropriate non-verbal ways to heard language is equally important" (RR 45–6).

Another important reason can be introduced by considering the problem of discovering which program is running on a computer on behavioral evidence alone. If we confined ourselves to considering its dispositions to respond to relatively short inputs, we should be denying ourselves vital evidence. A program, as well as equipping the machine with dispositions to respond in certain ways to short inputs, typically provides for those "first-order" dispositions to be modified on the basis of more or less extended *sequences* of inputs whose members each activate further operations by the program. Such changes are like learning. To have any serious chance of discovering what the program was, we should have to attempt to find out what effects whole ranges of different types of input sequences had on its "higher-order" dispositions to change its first-order dispositions – on its "learning capacity." Of course, there is no guarantee that we could succeed: The point is that we would pretty certainly fail if we limited ourselves to first-order dispositions. Comparably, we would be ignoring relevant facts about language if we confined ourselves to dispositions to assent or dissent to queried sentences: We would be ignoring the role of language in learning. It is relevant to our knowledge of our language that we are disposed to modify our existing dispositions to assent or dissent in certain ways. For example, we are apt to revise our attitudes, including our beliefs, as a result of reflection and deliberation. Quine often writes quite generally of "all dispositions to behavior" as embodying the facts that schemes of translation must fit, and that is surely a better way of putting his point.

Why stop at dispositions? What about events inside our heads? Here is what some regard as a genuine possibility. Behavioral dispositions alone do not determine a uniquely correct scheme of translation, but the churnings of the neurons together with relevant

relations between the individual and the rest of the world do. That seems to provide a basis for formulating two distinct indeterminacy theses, one saying only that a uniquely correct scheme of translation is not fixed by behavioral dispositions, the other saying that it is not fixed even by all the physical facts, including brain processes. Now, according to Quine, "[T]he behaviorist approach is mandatory" (ITA 5, PTb 37). He believes that all the facts that bear on meaning are in some way manifestable in behavioral dispositions. It is only because of this belief that he can maintain that the indeterminacy thesis entails that there are no nonbehavioral facts of translation to be right or wrong about. But if that belief is mistaken, and there are relevant facts not manifestable in the dispositions, obviously he would want those further facts to be taken into account. If translation was indeterminate when the objective facts were restricted to behavioral dispositions but determinate when brain processes were taken into account as well, then the weaker thesis would conspicuously fail to do the philosophical work Quine assigns to his indeterminacy thesis. It would fail to imply that translation relations, hence beliefs and desires, are not matters of fact. Indeed he has often insisted that he thinks translation is underdetermined not just by the facts about behavioral dispositions but by all the objective facts about the universe: "The point about indeterminacy of translation is that it withstands even . . . the whole truth about nature" (RWO 303) when this is supposed to be expressed in terms of physics.[5]

6. FITTING THE FACTS

What is it for a scheme of translation to *fit* the facts? We have seen that merely matching up first-order dispositions to respond has to be ruled out as trivializing the doctrine, in spite of its sometimes appearing to be Quine's own approach. Surely we must avoid a trivializing interpretation if there is an interesting one to be had – and there is, as we shall soon see.

One apparently promising approach would be to state constraints supposed to be individually necessary and jointly sufficient for a scheme of translation to be empirically adequate. But how do we decide just which constraints will do? Evidently that depends on what we are aiming at. Are we trying, for example, to systematize the actual practice of linguists? Or do we instead want a Quine-acceptable

substitute for that practice? The second suggestion can be ruled out very swiftly. One reason is that Quine himself despaired of finding any such substitute.[6] A more compelling reason is that if a cleaned-up notion were to be substituted for our ordinary notions of meaning and the rest, as these occur in statements of the indeterminacy doctrine, the doctrine would lack interesting implications for philosophy or psychology. If the substitute notion applied objectively, it would not give rise to indeterminacy of the kind in question. More to the point, it would have no repercussions on our ordinary notions: It would not prevent us from thinking of *belief*, *desire*, *intention*, and the rest as applying objectively.

That is important. As we have seen, Quine regards the indeterminacy doctrine as undermining the objectivity and general respectability of our *ordinary* notions of translation and meaning and of belief, desire, and other intentional notions as well. Its implications for those notions give the doctrine its chief philosophical interest and importance: It can have those implications only by somehow engaging with those very notions. So if the "constraints" approach to fact-fitting is to succeed, it cannot be via an attempt to find hygienic substitutes. If we stick with that approach, then, the constraints must somehow do justice to the actual practice of linguists who employ the ordinary notions. But that proposal also is problematic. Linguists impose constraints that Quine regards as merely practical, or at any rate theoretically unwarranted. For example, given a choice how to match up the sentence parts of one language with those of another, linguists tend to match up short expressions with short expressions, even though alternative schemes that matched up long ones with short ones might have worked too. In addition, they tend to look for manageably systematic ways of moving between the two languages in preference to complicated and arbitrary ones. According to Quine, such practices do not reflect a recognition of "substantive laws of speech behavior" (WO 75), yet together with other practices they tend to narrow down the range of alternative schemes of translation. Refusing to count a scheme of translation as fitting the facts unless it satisfies the actual practices of linguists would thus tend to defeat Quine's purposes. It would favor the existing, apparently uniquely correct schemes of translation.

An alternative to the constraints approach that avoids this undesirable result and also ensures that the indeterminacy doctrine

engages with our actual notions of translation and the rest is to rely on the dispositions of competent linguists, for example, as follows: By applying ordinary notions of sameness of meaning to a given pair of languages but disregarding the usual constraints of simplicity and practicality, rival manuals of sentence translation could be produced that linguists would judge to be substantially conflicting yet empirically adequate.

By invoking the judgment of linguists, we ensure that our ordinary notions of meaning and translation are brought to bear in a competent way. The resulting version of Quine's thesis is neither obviously false nor obviously true, and it has the implications for our ordinary notions that he supposes his thesis to have. From now on, that is what I shall take the indeterminacy thesis to be. (I distinguish this *thesis* of the indeterminacy of sentence translation from Quine's indeterminacy *doctrine* as a whole, which includes, notably, the thesis of the inscrutability of reference, to be noted below.)

7. QUINE'S REASONS

If that thesis is true, it has profound, not to say disturbing, implications for our ordinary conceptions of meaning and translation and indeed for all the interlocking intentional notions. Some philosophers have been so impressed that they have regarded it as justifying the view that those ordinary notions are serious candidates for elimination. However, unscientific sampling of colleagues' opinions suggests that a majority of philosophers remain unconvinced. Indeed, one reason for the continuing fascination of Quine's doctrine may be that neither the arguments for it, nor those against, have seemed compelling. (That suggests to me that, although the indeterminacy doctrine would, if true, support several other Quinean claims, its falsity would not do very much damage to his overall position. In spite of appearances, it is itself only loosely supported by his other views.)[7] Given a nontrivial interpretation, it is clearly very important, yet it remains tantalizingly hard both to defend and to attack. It is high time to consider Quine's own defense of it.

The classic text remains Chapter 2 of *Word and Object*, where Quine describes the situation of linguists engaged in the famous project of "radical translation." This is translation from scratch, unaided by interpreters, dictionaries, grammars, or any knowledge of

the "subjects" culture. The linguists have to produce a "manual of translation" between the jungle language of their subjects and their own language, English. The objective data, Quine remarks, "are the focus that [the radical translator] sees impinging on the native's surfaces and the observable behavior, vocal and otherwise, of the native" (WO 28). Toward the end of the chapter, he comments – optimistically, in light of the ensuing debate – "One has only to reflect on the nature of possible data and methods to appreciate the indeterminacy" (WO 72). Two main ideas are brought into play with a view to making this alleged indeterminacy manifest.

One is that of *stimulus meaning*. It exploits the idea of responding to queried sentences that we noted earlier. For any given sentence, there is a set of (relatively brief) stimulations that will prompt me to assent to it, a set that will prompt me to dissent from it, and a set that will leave me undecided. Together they make up that sentence's stimulus meaning. For some sentences, assent or dissent are forthcoming only after "an appropriate prompting stimulation." They are the *occasion* sentences, whose affirmative and negative stimulus meanings are nonempty sets. Examples are 'Red', 'It hurts', 'His face is dirty'. All other sentences are standing sentences, assent and dissent to which is typically not affected by current stimulation, although there is no sharp division between the two types. An important subset of occasion sentences consists of *observation* sentences, whose stimulus meanings for different speakers do not vary significantly under the influence of information not accessible to simple inspection ("collateral information"). Observation sentences are highly significant for Quine: They "afford our only entry to a language" (EN 89). Observationality is a matter of degree, but translation of observation sentences is supposedly pretty firm. These sentences, together with a few other types of sentences, are "translatable outright, translatable by independent evidence of stimulatory occasions"; however, they are "sparse and must woefully underdetermine the analytical hypotheses on which the translation of all further sentences depends" (WO 72).

These *analytical hypotheses* embody the other main idea Quine uses to press the case for indeterminacy. To produce a translation manual for a potential infinity of pairs of sentences, the linguists have among other things to correlate sentence parts with sentence parts – subject to any number of conditions. The linguists apparently

have vast freedom of choice in fixing these correlations and the conditions on them, and this seems to be one reason Quine thinks there is indeterminacy of translation. Although the analytical hypotheses are only part of a whole translation manual (which must also include information about syntax), they do the main work:

There can be no doubt that rival systems of analytical hypotheses can fit the totality of speech behavior to perfection, and can fit the totality of dispositions to speech behavior as well, and still specify mutually incompatible translations of countless sentences insusceptible of independent control. (WO 72)

However, this claim does not amount to an argument. As we shall see, opponents can maintain that the totality of behavioral dispositions exerts such powerful constraints on the choice of analytical hypotheses that there is no scope for substantial conflict between rival schemes.

A further consideration to which Quine appeals is what Dummett has nicknamed the 'inextricability thesis': the view that there is no such thing as pure knowledge of meanings uncontaminated by factual beliefs. This holistic view, set out so effectively in "Two Dogmas," comes into play throughout his discussion.

In *Word and Object* it is hard to be clear just which considerations are supposed to constitute his main reasons for the indeterminacy thesis and which he regards rather as corollaries of it. But in a later article, "On the Reasons for Indeterminacy of Translation" (1970), he distinguishes two main lines of argument: "pressing from below" and "pressing from above." Pressing from below is prominent in *Word and Object*. It consists of "pressing whatever arguments for indeterminacy of translation can be based on the inscrutability of terms" (RIT 183).

8. pressing from below

The radical translators notice that the jungle people tend to assent to 'Gavagai' in circumstances where English speakers would assent to the one-word sentence 'Rabbit'. Both sentences are highly observational, so the translators have pretty strong independent evidence for rendering 'Gavagai' by 'Rabbit'. But even given similarly strong reasons for the translation of every single observation sentence of each of

the two languages, the linguists' task has hardly begun. To translate the countless nonobservational sentences, they have to construct analytical hypotheses. 'Gavagai' itself is a one-word sentence. But the language also has what appears to be a *term*, 'gavagai'. The most natural analytical hypothesis would match it with the English term 'rabbit'. That would imply that they were coextensive terms, "true of the same things." But Quine invites us to consider that 'gavagai' might refer not to rabbits at all but to "mere stages, or brief temporal segments, of rabbits" (WO 52). That would be entirely consistent with 'Rabbit' and 'Gavagai' having the same stimulus meanings as whole sentences, for we are never stimulated by a rabbit without at the same time being stimulated by a rabbit phase, and vice versa. Alternatively, he suggests, 'gavagai' might be matched up with 'undetached rabbit part'. Or yet again, it might be equated with the "fusion" of all rabbits, "that single though discontinuous portion of the spatiotemporal world that consists of rabbits." Finally, it might be taken to be "a singular term naming a recurring universal, rabbithood."

We might suspect that a uniquely correct rendering of 'gavagai' could be established by means of "a little supplementary pointing and questioning." But Quine notes that in pointing to a rabbit you are at the same time pointing to "a stage of a rabbit, to an integral part of a rabbit, to the rabbit fusion, and to where rabbithood is manifested." Similarly, in pointing to any of the other things, you are pointing to each of the rest (see WO 52 ff). No doubt, we may think that some well-designed questions would surely settle the matter. The trouble is that to be in a position to assess the outcome of our questioning we need to know what the questions themselves mean. Suppose we imagine we are asking, 'Is this the same gavagai as that?' or 'Do we have here one gavagai or two?' How do we know such questions mean what we think they mean? Quine maintains that "[W]e could equate a native expression with any of the disparate English terms 'rabbit', 'rabbit stage', 'undetached rabbit part', etc., and still, by compensatorily juggling the translation of numerical identity and associated particles, preserve conformity to stimulus meanings of occasion sentences" (WO 54). If that argument works, he has established the thesis he calls the 'inscrutability of reference' (or 'of terms'). It is exactly the same as the thesis of indeterminacy of translation except that it applies to terms rather than whole sentences (substitute 'term' for 'sentence' in the statement of the thesis

toward the end of §7).[8] According to it, two schemes of reference for a pair of languages could both fit all the Quine-acceptable facts yet conflict in their pairings of referring expressions. It is a highly significant thesis in its own right, since it implies that there is no matter of fact as to what we are *referring* to (see Chapter 5). Quine also seems to suppose (at any rate in *Word and Object*, but see the end of this paragraph) that *if* there is inscrutability of terms, then there is also indeterminacy of translation of whole sentences. That seems right. If there is indeterminacy of translation at all, it holds in the "domestic" case. So if translation in the domestic case is not subject to the indeterminacy, statements such as '"Rabbit" refers to rabbits and not rabbit stages' are determinately translatable by other English speakers. In that case, we can hardly represent English speakers as believing that 'rabbit' refers to anything but rabbits or as intending to use that expression to apply to anything but rabbits. So if reference is after all inscrutable (in the strong sense so far considered), translation is subject to the indeterminacy. (It is true that in "On the Reasons for Indeterminacy of Translation" Quine stated that there is "little room for debate" over the inscrutability thesis and also that this thesis does not entail indeterminacy of sentence translations.[9] However, both those claims rest on an example of translation from Japanese into English that depends essentially on the two languages having different syntactic and semantic resources. Since the example admittedly fails to support the indeterminacy of translation, I will not set it out here.)

Does the 'gavagai' argument work? There has been a great deal of discussion in the literature, and some further considerations have been advanced, not all by Quine himself. But the argument stated above appears to be defective. Quine is right to say that pointing by itself cannot settle the matter – though it does seem to constrain the radical translators to connect 'gavagai' with rabbits in some way or other. Where the reasoning falls short is in its treatment of the prospects for settling the matter with the help of intelligent questioning. Certainly, *if* the translation of our questions is itself subject to indeterminacy, then questioning cannot establish determinacy of reference. And of course Quine's opponents must not assume that the questions are free from indeterminacy. But by the same token, Quine himself must not presuppose indeterminacy, since pressing from below is intended to establish it. It is true that isolated questions

of the kind exemplified by 'Do we have here one gavagai or two?',
or even systematic sequences of such questions, cannot establish
determinate reference. It does not follow that there is no determi-
nate reference to be established. After all, the interrogative sen-
tences are locked into a total theory or "quasi-theory" of inter-
connected sentences (see Chapter 3). The interconnections, for any
individual, are reflected in that individual's total system of behav-
ioral dispositions. Reflecting on this – and noting that the disposi-
tions are included among the Quine-acceptable facts – Quine's oppo-
nents can object that, for each speaker of a language, his dispositions
form a sufficiently coherent structure to fix references and rule out
inscrutability.[10]

These reflections expose a tension in Quine's discussions. On the
one hand, he emphasizes the search for the sort of "independent ev-
idence" that is exemplified by the stimulus meanings of occasion
sentences. That shows up in remarks like the one quoted earlier,
to the effect that when we are reconstruing the reference of 'gava-
gai', compensatory juggling with the translation of expressions for
identity and so on is aimed at preserving "conformity to stimulus
meanings of occasion sentences." On the other hand, there is his
holism, according to which what we tend to call 'meanings' depend
not on any limited set of experiences but on the interconnections
among words and sentences in our total theory of the world. Sup-
pose we accept that holism. Then why, when we are attempting to
put translation on an objective basis, should we disregard the holistic
interconnections?

The tension can be highlighted by a simple illustration. Assume
that Gavagese has linguistic resources similar to English and that the
three Gavagese expressions 'gavagai', 'gavagaiex', and 'gavagaiwy' on
the most straightforward construals are translatable by 'rabbit', 'rab-
bit stage', and 'integral rabbit part' – because 'gavagai' matches up
neatly with 'rabbit', 'ex' with 'stage', and 'wy' with 'integral . . . part'.
Each of these six expressions gives rise to a one-word observation
sentence, and if Quine is right, all have the same stimulus mean-
ing. If we were innocent of the indeterminacy doctrine, we should
unhesitatingly render 'Gavagai' by 'Rabbit', 'Gavagaiex' by 'Rabbit
stage', and 'Gavagaiwy' by 'Integral rabbit part'. The tendency of
Quine's argument is that there are no *objective* grounds for prefer-
ring those particular renderings over any of the alternatives – over,

for example, translating 'Gavagaiex' by 'Integral rabbit part' instead of 'Rabbit stage' in spite of the match between 'ex' and 'stage' – since all those renderings preserve conformity to stimulus meanings of occasion sentences.

The crucial question is this: How can Quine both insist on his holism and ignore holistic interconnections when it comes to translation? Perhaps he assumes that those interconnections themselves cannot be construed determinately – that it is not an objective matter of fact that Gavagese 'ex' matches up with 'stage' rather than 'integral . . . part'. The point now, though, is that even if that is so, he offers no argument for it. It is just an assumption – yet a vital one. And surely the interconnections must be taken into account. It cannot be right to consider *only* conformity to stimulus meanings of occasion sentences. Even highly observational sentences do not derive what we ordinarily think of as their meanings only from the links between patterns of sensory stimulation and our dispositions to assent to and dissent from them. Their links with other expressions in the language, hence with the speaker's theory of the world, are also relevant. It is question-begging to assume that the totality of behavioral dispositions falls short of fixing what those links are.

The waters hereabouts are muddied by Quine's appeal to the fact that the structure of the foreigners' language may be unlike our own. In particular, the foreigners may "achieve the same net effects" as

our own various auxiliaries to objective reference: our articles and pronouns, our singular and plural, our copula, our identity predicate . . . through linguistic structures so different that any eventual construing of our devices in the native language and vice versa can prove unnatural and largely arbitrary. (WO 53)

That is true, but can such differences legitimately be appealed to in support of the inscrutability thesis?

9. INSCRUTABILITY AND LINGUISTIC DIFFERENCES

If two languages differ in their syntactic and semantic resources, it is not surprising if there is no single correct scheme for translating or assigning references between them. That thought may predispose Quine's readers to go along with his reasoning when he applies it to the case of Gavagese and English. If his claims *depended* on that

thought, we could concede them immediately, noting only that in that case they would have slight philosophical interest. For they would give no support to the claim Quine has actually based on his indeterminacy doctrine, to the effect that relations of meaning and associated notions are not matters of fact. Even Plato, that archexponent of the myth of the meaning museum, could have conceded that different languages pick out different meanings (or Forms) while still insisting that it was a matter of hard objective fact which meaning each expression picked out.

In general, the effect of two languages having different resources will be that there is no *exact* translation between them, whether we are talking about the translation of whole sentences or of referring expressions. Granted, exactness is a matter of degree, and it is not news that rough translations and assignments of reference may be inequivalent. That has no tendency to support Quine's interesting claims. To clinch the point, notice that a single manual of translation could consistently offer a set of different *rough* translations in English for a given single sentence of the foreign language – without actually rejecting any of those rough translations. As Quine himself clearly recognizes, the only interesting sort of indeterminacy would be where one manual actually rejected the other's translations or assignments of reference. There is no interesting indeterminacy or inscrutability at all unless it holds also for the domestic case, which is free from syntactic and semantic differences.

Before leaving the inscrutability thesis, we should note that Quine offers further considerations in support of it in later writings, notably in *Ontological Relativity* (1969). Davidson and Putnam have also defended versions of the thesis.[11] (For more on this thesis, see Chapter 5.)

10. PRESSING FROM ABOVE

In *Word and Object*, the dominant argument for the indeterminacy of translation seems to be the inscrutability of reference: pressing from below. In "On the Reasons for Indeterminacy of Translation," Quine suggests there has been a misapprehension: "The real ground of the doctrine [of indeterminacy of translation] is very different" (RIT 178), namely, his thesis of the underdetermination of our theory of nature.

This thesis is independently interesting. The broad idea is that our theory of the world is underdetermined not just by the evidence we may happen to have encountered but by all possible evidence. (A thorough discussion of the thesis – out of place in this chapter – would need to consider just what is to be counted as evidence and also whether the thesis requires the possibility of competing theories that are not just different but mutually incompatible.)

The first point to notice is that the indeterminacy of translation is not to be regarded as just a special case of the underdetermination of theory. Some critics have been unimpressed by that distinction, but it seems reasonably clear. Granted that physics itself is underdetermined, the indeterminacy of translation is "additional" in the following sense. Suppose we regard current physics as fixed and take "the whole truth about nature" to be representable by a set of statements describing every detail of the universe, past, present, and future, in terms of that fixed physics. The claim is that this assumed determinate physical reality fails to fix translation. It still leaves room for the construction of mutually incompatible translation manuals that nevertheless fit the totality of those supposedly fixed physical facts.[12]

Pressing from above consists of an argument that attempts to exploit the underdetermination of physics itself. The argument does not purport to be completely general, it seeks only to establish the indeterminacy of the translation of as much of physics as is itself underdetermined. Consider the radical translation of a radically foreign physicist's theory. The translation starts by matching up observation sentences of the two languages "by an inductive equating of stimulus meanings." As usual, analytical hypotheses are required, and Quine claims that their "ultimate justification is substantially just that the implied observation sentences match up." But, he goes on,

the same old empirical slack, the old indeterminacy between physical theories [the underdetermination of theories], recurs in second intention. Insofar as the truth of a physical theory is underdetermined by observables, the translation of the foreigner's physical theory is underdetermined by translation of his observation sentences. If our physical theory can vary though all possible observations be fixed, then our translation of his physical theory can vary though our translations of all possible observation reports on his part be fixed. (RIT 179–80)

It seems that this argument depends on essentially the same
question-begging assumption as was made in defense of the in-
scrutability of reference in *Word and Object*. This is that the only
objective basis for analytical hypotheses is the matching up of ob-
servation sentences. As we noticed when discussing that earlier ar-
gument, the assumption is at best implausible. Its implausibility is
highlighted by the present argument. For this focuses on the foreign
physicist's *theory*. In order to tell what that theory is – even to tell
which sentences belong to it – it is necessary to take account of a
lot more than observation sentences. The dispositions of the foreign
physicist that are relevant are not merely ones to assent to sentences
belonging to the theory. There are higher-order dispositions to be
taken into account as well, for example, dispositions to revise the
first-order dispositions in various circumstances. If Quine is saying
only that all those dispositions are to be disregarded and that there is
"indeterminacy" if we do disregard them, then for reasons noted ear-
lier the indeterminacy doctrine is trivially true (see §5). To avoid trivi-
alizing it, therefore, all such facts must be allowed to be taken into ac-
count. Once that is seen, pressing from above appears to be a failure.
It provides no independent support for the indeterminacy thesis.[13]

11. ALLEGED INSTANCES

The weakness of other arguments for the indeterminacy thesis would
not matter if there were well-established instances of it. Quine him-
self has been wary of suggesting such instances,[14] but others have
offered to fill the gap. Here are some of them.

Hartry Field's example is that "we can translate certain out-
dated . . . physical theories into current theory in a variety of ways."[15]
For example, Newton's 'mass' can be translated either as 'relativistic
mass' or as 'rest mass'. Field claims there is no fact of the matter as
to which translation is correct. That is surely right. However, since
Newton's theory is significantly different from current theory, this
appears to be no more than an example of the familiar phenomenon
noted earlier: There are likely to be a number of different rough trans-
lations and assignments of reference from one theory or conceptual
scheme into another.

Other suggested instances have been taken from mathematics and
set theory. For example, it has been argued that "a shining example of

translational indeterminacy" is provided by the notion of "forcing" used by Paul Cohen in his independence proofs for set theory.[16] One thing that greatly weakens the appeal of this and similar examples is that only relatively loose constraints are imposed on the translations. In the present case, it is taken to be enough if theorems are matched up with theorems and if logical relations in one theory are matched up with parallel relations among their translational images in the other theory. Arguably, if we can tell the difference between the two theories, there are other factors, reflected in the totality of behavioral dispositions, that provide for those differences. So in such mathematical cases it is not easy to justify the claim that the alleged rival translation manuals are empirically adequate in the first place.

Gerald Massey has proposed an intriguing rival to the homophonic manual of translation.[17] If his proposal is sound, it would provide a dramatic illustration of Quine's thesis – at least for a substantial part of ordinary English – since his manual differs from the homophonic manual over the translation of every sentence, every translation is a contradictory of the homophonic manual's version, and it supports the inscrutability of reference as well as the indeterminacy of (sentence) translation. This manual exploits the logical principle of duality, rendering each expression by its dual (negation being its own dual). For example 'All rabbits are vegetarians' is rendered by 'Some rabbits are nonvegetarians'. Those two sentences are straightforward contradictories, as is the case for every pair of sentences translated by this dualizing manual, and each predicate is translated by its complement. To square these translations with people's dispositions to assent to and dissent from sentences, the dualizing manual construes 'Yes' as a sign of *dissent* and 'No' as a sign of *assent*. If assent and dissent were objective matters of fact, of course, this manual would fail. But in spite of Quine's willingness to treat them as if they were objective,[18] Massey maintains that they cannot properly be so regarded because they depend on analytical hypotheses.

Objectors will point out that if we tried to communicate with people by using, instead of the sentences we should otherwise have uttered, the ones correlated with them by the dualizing manual, we should quickly get into trouble. Uttering the duals of sentences you would otherwise have used is guaranteed to spoil any conversation – as Massey recognizes. He thinks translation manuals should ensure that the utterances they match up should convey the same

information (or belief). So he suggests that the way to use the dualizing manual is to apply it to the *negation* of the sentences you would otherwise have used; successful communication is then assured. However, the resulting sentences are actually equivalent to those yielded by the homophonic manual! Massey does not seem to have specified a manual that is in the same respects both a genuine rival to the homophonic one and empirically adequate.

12. ATTEMPTED REFUTATIONS

There have been many attempted refutations of Quine's indeterminacy doctrine: I will mention a few. The first has probably occurred to most readers of *Word and Object*. If we consider the structures of the languages we are translating between, the same factors that permit translation of a sentence S by a sentence T and also by a different sentence U must surely guarantee that T and U occupy similar, if not identical, structural roles within the same language. Our claims about sameness of meaning depend for their justification on relations among our uses of words and relations of those uses to extralinguistic reality. Suppose we follow Quine and think of all those relations as represented by behavioral dispositions. Then whatever facts about the two languages and their relations to extralinguistic reality may be reflected in behavioral dispositions, if those facts permit translation of S by T and also by U, then T and U must have exactly similar relations both to other bits of language and to extralinguistic reality. Matching up the structures of the two languages is necessary in order to provide translations in the first place, and since T and U must occupy exactly similar places in the structure of the second language, that structure must ensure that they mean the same. So S and T cannot permissibly be represented as inequivalent, as the indeterminacy thesis requires.[19] However, as it stands, this argument begs the question. In effect, Quine's claim is that although there does indeed have to be a certain structural symmetry in the way S and T are related by behavioral dispositions to the rest of the language and to extralinguistic reality, it is not enough to rule out genuine conflicts over translation such as he envisages.

Related to that "argument from structure" are appeals to the use of words. Meaning depends on use, and it is objected that the total pattern of uses of the words of a language ensures that if each of two different English sentences can be represented as meaning the same

as a given foreign one, that can only be because they both mean the same as each other. Underlying this approach is the idea that Quine ignores a fundamental feature of language, namely, that it is subject to rules or norm:

> From the point of view of a normative conception of meaning such as Wittgenstein defends, a behavioristic conception like Quine's is simply no conception of meaning at all, not even an ersatz one. Indeed it is no conception of *language*, for a language stripped of normativity is no more language than chess stripped of its rules is a game.[20]

One possible reply is that Quine is not denying that it is useful to talk of our using words in accordance with norms. What he denies is that there is a uniquely correct way to say what those norms and uses of words are. If the indeterminacy doctrine is right, the same linguistic population can be represented as adhering to significantly different norms. Appealing to uses, norms, or rules appears merely to beg the question.

Another popular line of attack has been to appeal to constraints on translation and interpretation. David Lewis has proposed that the constraints should be "the fundamental principles of our general theory of persons, [which] tell us how beliefs and desires and meanings are normally related to one another, to behavioral output, and to sensory input."[21] This theory, he says, "must amount to no more than a mass of platitudes of common sense." He remarks, "If ever you prove to me that all the constraints we have yet found could permit two perfect solutions, . . . then you will have proved that we have not yet found all the constraints."[22] But that is offered as a "credo," not an argument. Quine can insist that no justified set of constraints will ensure that even the totality of physical truths leaves room for only one correct scheme of interpretation.

A different line of attack has the piquant feature that it appeals to Quine's own views in the philosophy of science. Quine encourages us to take as true whatever theory of the world best fits the data and best satisfies our vague guiding principles of simplicity and conservatism.[23] So it seems we must count as facts whatever our evolving total theory provides for. The objection is that those very principles seem to rule out any significant indeterminacy. Quinean principles, it appears, would favor existing schemes of translation over whatever peculiar alternatives someone might manage to concoct. In radical translation from Gavagese to English, for example,

the foreign expression 'gavagai' must be matched with the English term 'rabbit' rather than with any of the exotic alternatives Quine offers, since that scheme is both simpler and easier to accommodate in our existing theory of the world. Similarly, within our own language any rivals to the homophonic scheme will inevitably be more complicated and harder to fit into the rest of our theory. Rorty, for example, has maintained that Quine faces a dilemma: "[H]e should either give up the notion of 'objective matter of fact' all along the line, or reinstate it in linguistics."[24]

This objection overlooks the question of how the statements of linguistics are related to statements in terms of physics. In spite of the admitted underdetermination of physical theory, Quine *does* regard statements in terms of physics as stating facts. Now, some nonphysical sentences are such that it is undeniable that their truth is fixed by the purely physical truths. Given the physical facts, the existence and natures of sticks, stones, and stars, for example, are thereby determined: God could not have set up those same physical truths without thereby creating sticks, stones, and the rest. Although it seems conceivable that the same should have been so for relations of synonymy – in which case they could safely have been counted among the objective facts, too – that is just what Quine denies. If he is right, statements about sameness of meaning float free of the physical facts so obstinately that the latter are consistent with statements of synonymy that actually contradict one another. It seems fully in accord with Quinean principles to refuse to count such statements as genuinely fact stating. Quine's philosophy of science does not compel him to accept determinacy of translation.

The "argument from structure" is inadequate by itself; perhaps it can be supplemented on the basis of reflections on how the structure of a language may be built up. It is common, and reasonable, to assume a language can be acquired by a succession of steps (parents tend to think of them as a matter of acquiring "words"). Imagine a pair of twins who acquire English by the same sequence of steps, and consider a very early stage – when they have only some hundred or so words and only simple constructions. Is there room for indeterminacy at that early stage? Most of the expressions of this fragmentary language will be highly observational, such as the one-word utterance 'Dog', but even those that are not so observational will be very simple, such as 'Dog nice'. True, if the question were how

to render that last utterance into *adult* English, there would be plenty of alternatives, such as 'There's a nice dog', 'All dogs are nice', 'Some dogs are nice'. But we have to consider only translation from and into that same fragment of English – baby English.

We saw that Quine's point would not be interesting if it were only that there are alternative and mutually incompatible renderings of utterances from one language into a conceptually or syntactically different language, where often the best translations will only be rough (§2 and §9). The interesting claim is that there is significant indeterminacy of translation within a single language: the domestic case. Once that is taken into account, it looks as if there will be no room for indeterminacy in translating the utterances of baby English into that same version of baby English. The children's behavioral dispositions seem to rule out alternatives.

Now, the argument I am sketching has the form of an inductive proof, the basis for which has just been indicated. The inductive step requires us to take any stage En in the twins' acquisition of English and consider whether its transformation into a very slightly bigger language En + 1 by the addition of a single word or other atomic increment could possibly introduce indeterminacy – on the assumption that there is no indeterminacy in the translation of sentences of En by sentences of En. There would be no problem if the new word could be actually defined in terms of En, but that will not generally be possible. However, it should be capable of being introduced on the basis of En, since that is what actually happens: We acquire new expressions on the basis of our already existing language. It is at any rate arguable that determinacy for the case of En guarantees determinacy for the case of En + 1. If so, translation of and by the sentences of grown-up English is determinate, in which case translation generally is not subject to Quinean indeterminacy. In that way the argument from structure can be reinforced by consideration of how the structure might be built up.[25]

13. ASSOCIATIONS

There is an unexpected footnote in *Word and Object*:

Perhaps the doctrine of indeterminacy of translation will have little air of paradox for readers familiar with Wittgenstein's latter-day remarks on meaning. (WO 77, n. 2)

Another philosopher influenced by Quine is Dennett. He appeals to Quine's reasoning in support of the view that interpretations of people's intentional states are subject to an indeterminacy.[31]

Although there is not such a torrent of publications on Quine's indeterminacy doctrine today as there was a couple of decades ago, that is not a sign that the issue has been decided. His suggestions continue to challenge and excite.

NOTES

1. H. Putnam, "The Refutation of Conventionalism," in *Mind, Language and Reality: Philosophical Papers*, vol. 2 (Cambridge: Cambridge University Press, 1975), 159.
2. Note that Quine prefers to represent the conflict between translation manuals as a matter of one excluding the other. Later, Quine writes of "translations each of which would be excluded by the other system" (WO 73 ff.); and still later, he writes, "[I]t is just that one translator would reject the other's translation" (RWO 297). There is a further modification in *Pursuit of Truth*, where he suggests the following statement of the thesis: "A manual of Jungle-to-English translation constitutes a recursive, or inductive, definition of a *translation relation* together with a claim that it correlates sentences compatibly with the behavior of all concerned. The thesis of indeterminacy of translation is that these claims on the part of the two manuals might both be true and yet the two translation relations might not be usable in alternation, from sentence to sentence, without issuing in incoherent sequences" (PTb 48). In spite of differences in emphasis, especially the rather indirect way in which conflict is represented, this appears to be essentially the same thesis.
3. "Hypostasis of meanings is a red herring. I keep urging that we could happily hypostatize meanings if we could admit synonymy. We could simply identify meanings with the classes of synonyms" (RWA 73).
4. For a late statement of Quine's view on the relations between the indeterminacy doctrine and "the old notion of separate and distinct meanings," see ITA.
5. Although he maintains that physics is itself underdetermined by all the possible evidence, he formats the indeterminacy doctrine on the basis of the assumption that physics is construed realistically: "[T]here is no fact of the matter even to *within* the acknowledged under-determination of a theory of nature (RWO 303). Now the relation between the interchangeable but incompatible manuals of translation that I have

postulated is that they accord with exactly the same states of human organisms, however minutely modulated; all the same hidden states of nerves. This is the sense in which I say there is no fact of the matter" (RWA 75).

6. In *Word and Object*, summarizing the outcome of his use of the notion of stimulus meaning, Quine states that if the radical translator goes bilingual, then (1) all occasion sentences can be translated, (2) truth functions can be translated, and (3) "stimulus analytic" and "stimulus contradictory" sentences can be recognized. Some writers have assumed that he regards these as objective constraints on translation. But that is a mistake: They simply represent the consequences of adopting the methods he describes.

7. See R. Kirk, *Translation Determined* (Oxford: Clarendon Press, 1986), 239–50.

8. So Quine later preferred to call the doctrine 'indeterminacy of reference' (see PTb 50).

9. See PTb 50 for emphasis on the contrast between the inscrutability of reference (construed weakly) and what he there calls the 'holophrastic thesis' of indeterminacy of translation of whole sentences.

10. For development of related points, see C. Hill's "'Gavagai'," *Analysis* 32 (1972): 68–75, and G. Evans, "Identity and Prediction," *Journal of Philosophy* 72 (1975): 346–63. Evans's suggestions have been particularly influential: For discussion, see C. Hookway, *Quine: Language, Experience and Reality* (Palo Alto, Calif.: Stanford University Press, 1988), 152–9, and C. Wright, "The Indeterminacy of Translation," in *A Companion to the Philosophy of Language*, ed. B. Hale and C. Wright (Oxford: Blackwell, 1997), 397–426.

11. Quine exploits the notion of proxy functions in his (OR); see also PTb 31 ff. Both Davidson and Putnam offer versions of an argument that uses the notion of a permutation of individuals in the universe. Putnam's version applies to possible worlds as well as the actual world; see D. Davidson, *Inquiries into Truth and Interpretation* (Oxford: University Press, 1984), and H. Putnam, *Reason, Truth and History* (Cambridge: Cambridge University Press, 1981).

12. See RWO 303, RIT 180, and RWA 75 (quoted in n. 5).

13. For discussion, see R. Kirk, *Translation Determined* (Oxford: Clarendon Press, 1986), 133–52, and C. Wright, "The Indeterminacy of Translation," in *A Companion to the Philosophy of Language*, ed. B. Hale and C. Wright (Oxford: Blackwell, 1997), 413–21.

14. So far as I know he has in print offered only two; see PTb 51. One is Gerald Massey's, mentioned later. The other is Edwin Levy's, which exploits the ideal that the geometry attributable to a space depends on

the method of measurement adopted; see E. Levy, "Competing Radical Translations: Examples, Limitations and Implications," *Boston Studies in the Philosophy of Science* 7 (1971): 590–604. While Levy himself argues that his example is far from establishing the indeterminacy thesis – indeed his article is an attack on it – Quine has said that in view of the example one can scarcely question the thesis. I cannot discuss the example here but would argue that only by begging the question can it be taken to support Quine's thesis.

15. H. Field, "Theory Change and the Indeterminacy of Reference," *Journal of Philosophy* 72 (1973): 462–81.

16. M. Levin, "Forcing and the Indeterminacy of Translation," *Erkenntnis* 14 (1979): 25–31.

17. See G. Massey, "The Indeterminacy of Translation: A Study in Philosophical Exegesis," *Philosophical Topics* 20 (1992): 317–45.

18. Quine says that he is "prepared to pass over whatever traces of underlying indeterminacy there may be in the signs for assent and dissent themselves" (RWO 317).

19. For one clear example of this argument from structure, see J. Bennett, *Linguistic Behavior* (Cambridge: Cambridge University Press, 1976), 262. There is a version in M. Dummett, "The Significance of Quine's Indeterminacy Thesis," in *Truth and Other Enigmas* (London: Duckworth, 1978), 372.

20. P. Hacker, "Wittgenstein and Quine: Proximity at Great Distance," in *Wittgenstein and Quine*, ed. R. Arrington and H.-J. Glock (New York: Routledge, 1996), 15–16.

21. D. Lewis, "Radical Interpretation," *Synthese* 27 (1974): 345.

22. Ibid., 343.

23. See WO 20–5, RWO 303, and RR 137 ff.

24. R. Rorty, "Indeterminacy of Translation and of Truth," *Synthese* 23 (1972): 443–62. Also see R. Rorty, *Philosophy and the Mirror of Nature* (Princeton, N.J.: Princeton University Press, 1980), 202 ff.

25. For detailed discussion, see R. Kirk, *Translation Determined* (Oxford: Clarendon Press, 1986), 215–36.

26. For the previous two quotations from Wittgenstein, see L. Wittgenstein, *Philosophical Investigations*, trans. G. E. M. Anscombe (Oxford: Blackwell, 1953) §199 and §206. For his ideas on interpretation, see §§143–206.

27. S. Kripke, *Wittgenstein on Rules and Private Language* (Oxford: Clarendon Press, 1982), 55–7.

28. For a Wittgensteinian perspective on the relations between Wittgenstein and Quine, see P. Hacker, "Wittgenstein and Quine: Proximity at Great Distance," in *Wittgenstein and Quine*, ed. R. Arrington and H.-J. Glock (New York: Routledge, 1996).

29. D. Davidson, "Radical Interpretation," in *Inquiries into Truth and Interpretation* (New York: Oxford University Press, 1984), 127. For more on his approach to these problems, see other essays in that collection.
30. D. Davidson, "Belief and the Basis of Meaning," in *Inquiries into Truth and Interpretation* (New York: Oxford University Press, 1984), 152.
31. See D. Dennett, *The Intentional Stance* (Cambridge, Mass.: M.I.T. Press, 1987), 13–42, and D. Dennett, "Real Patterns," *Journal of Philosophy* 88 (1991): 27–51.

7   Quine's Behaviorism cum Empiricism

> I do consider myself as behavioristic as anyone in his right mind could be.
>
> W. V. Quine, "Linguistics and Philosophy"

I. INTRODUCTION

Quine is an advocate of naturalism, a view comprising two theses, one negative, one positive. The negative thesis is that there is no adequate first philosophy – that is, there is no a priori or experiential ground outside of science upon which science can either be justified or rationally reconstructed, as was the wont of many traditional epistemologists. The positive thesis is that it is up to science to inform us about what exists and how we come to know what exists.

On the negative side, if there is no adequate first philosophy, then epistemologies as disparate as Descartes' and Carnap's fail of their purpose. While Descartes sought to deduce the truths of nature from a foundation of clear and distinct ideas, (early) Carnap sought to rationally reconstruct scientific discourse from a foundation of elementary experiences. Quine advances a series of philosophical arguments and considerations designed to establish the untenability of Descartes-like and Carnap-like epistemic projects. In short, Quine argues that Descartes-like efforts fail because not even the truths of arithmetic, let alone all the truths of nature, can be deduced from a (consistent) foundation of clear and distinct ideas, and he argues that Carnap-like efforts fail because a theory's theoretical terms cannot be defined, even contextually, in observation terms. Such reductionism is misdirected if, as Quine maintains, many or most individual sentences of scientific theories do not possess their own observable

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confirming and infirming conditions in terms of which such reductive definitions must be framed. The negative (naturalistic) conclusion Quine draws from all this is that traditional epistemology must be abandoned; the quest for a nonscientific ground for science is a will-o'-the-wisp.

On the positive side, all is not lost with the passing of first philosophy, for science remains, and, according to Quine, not only does science offer the currently best theory of what exists (ontology), it also offers the currently best theory of how we come to know what exists (epistemology). In particular, the currently best theory of what exists supports physicalism, while the currently best theory of how we come to know what exists supports empiricism.

Quine's advocacy of physicalism means different things in different contexts. In philosophy of language, it indicates his repudiation of mentalistic semantics; in philosophy of mind, it indicates his repudiation of mind-body dualism; in ontology, it indicates his acceptance of the doctrine that "nothing happens in the world, not the flutter of an eyelid, not the flicker of a thought, without some redistribution of microphysical states" (GWW 98). Still, Quine's ontological physicalism countenances more than physical states; it also countenances the abstract objects of applied mathematics, namely, classes. Quine's ground for admitting these abstract objects into his physicalist ontology is simply that science cannot proceed without them.

Quine's advocacy of empiricism endorses the following two tenets: "[W]hatever evidence there *is* for science *is* sensory evidence ... [and] all inculcation of meanings of words rests ultimately on sensory evidence" (EN 75). These two tenets of empiricism are, according to Quine, findings of science:

Science itself teaches there is no clairvoyance; that the only information that can reach our sensory surfaces from external objects must be limited to two-dimensional optical projections and various impacts of air waves on the eardrums and some gaseous reactions in the nasal passages and a few kindred odds and ends. (RR 2)

So, Quine's commitments to physicalism and to empiricism are based on current scientific findings. But Quine is also a fallibilist regarding science; he recognizes that science is changeable and therefore could conceivably (however unlikely) someday withdraw

its support for physicalism and/or empiricism. Thus, Quine's commitments to physicalism and to empiricism are firm but tentative.

Quine's naturalism, physicalism, empiricism, and fallibilism provide a framework for his scientific (empiricist) account of the relation of evidence to theory. It is in connection with this epistemological endeavor that Quine professes to be as behavioristic as anyone in his right mind could be. But what, precisely, does he mean by 'behaviorism'? As we shall see, he construes the term broadly; in particular, he does not define 'behaviorism' in terms of conditioned response:

When I dismiss a definition of behaviorism that limits it to conditioned response, am I simply extending the term to cover everyone? Well, I do think of it as covering all reasonable men. What matters, as I see it, is just the insistence upon couching all criteria in observation terms. By observation terms I mean terms that are or can be taught by ostension, and whose application in each particular case can therefore be checked intersubjectively. Not to cavil over the word 'behaviorism', perhaps current usage would be best suited by referring to this orientation to observation simply as empiricism; but it is empiricism in a distinctly modern sense, for it rejects the naive mentalism that typified the old empiricism. It does still condone the recourse to introspection that [Noam] Chomsky has spoken in favor of, but it condones it as a means of arriving at conjectures or conclusions only insofar as these can eventually be made sense of in terms of external observation. (LP 58)

And what is this distinctly modern sort of empiricism to which Quine alludes?

Empiricism of this modern sort, or behaviorism broadly so called, comes of the old empiricism by a drastic externalization. The old empiricist looked inward upon his ideas; the new empiricist looks outward upon the social institution of language. Ideas dwindle to meanings, seen as adjuncts of words. The old inner-directed empiricists – Hobbes, Gassendi, Locke, and their followers – had perforce to formulate their empiricist standard by reference to ideas; and they did so by exalting sense impressions and scouting innate ideas. When empiricism is externalized, on the other hand, the idea itself passes under a cloud; talk of ideas comes to count as unsatisfactory except insofar as it can be paraphrased into terms of dispositions to observable behavior. (LP 58)

Quine's construal of behaviorism (broadly so called) as externalized empiricism is closely connected to his views regarding the nature of the explanation of human behavior. He distinguishes "three

levels of purported explanation, three degrees of depth: the mental, the behavioural, and the physiological" (MVD 87). Of these three, the mental is the most superficial, "scarcely deserving the name of explanation. The physiological is the deepest and most ambitious, and it is the place for causal explanations." But, for the present, it is the behavioral level that Quine thinks is the most useful in theorizing about language and mind:

Until we can aspire to actual physiological explanation of linguistic activity in physiological terms, the level at which to work is the middle one; that of dispositions to overt behaviour. Its virtue is not that it affords causal explanations but that it is less likely than the mentalistic level to engender an illusion of being more explanatory than it is. The easy familiarity of mentalistic talk is not to be trusted. (MVD 95)

As this quotation makes clear, Quine regards behavioral explanation as a temporary measure that might hasten the day when (if ever) actual physiological explanation becomes available.

2. BEHAVIORISM AND PHILOSOPHY OF LANGUAGE

Competent in six languages (English, French, German, Italian, Portuguese, and Spanish) and familiar with dozens more, Quine always has had a passion for languages and their study – antedating even his passion for philosophy. Nevertheless, Quine's writings on meaning, synonymy, analyticity, language learning, reference, and translation emanate more from his passion for epistemology than from any passion for languages and their study. Furthermore, his approach to these topics is consistently behavioristic; indeed, he maintains that one has no choice in the matter:

In psychology one may or may not be a behaviorist, but in linguistics one has no choice. Each of us learns his language by observing other people's verbal behavior and having his own faltering verbal behavior observed and reinforced or corrected by others. We depend strictly on overt behavior in observable situations. As long as our command of our language fits all external checkpoints, where our utterance or our reaction to someone's utterance can be appraised in the light of some shared situation, so long all is well. Our mental life between checkpoints is indifferent to our rating as a master of the language. There is nothing in linguistic meaning beyond what is to be gleaned from overt behavior in observable circumstances. (PTb 37–8)

The last sentence of this quotation from *Pursuit of Truth* expresses a behavioristic corollary of the equally behavioristic opening lines from the preface to *Word and Object*: "Language is a social art. In acquiring it we have to depend entirely on intersubjectively available cues as to what to say and when" (WO ix). In *Word and Object*, Quine set out to see just how much behavioristic sense could be made of meaning, synonymy, analyticity, language learning, reference, and translation. Thus, we can best glean a sense of the role that behaviorism plays in Quine's philosophy of language by surveying some of his findings.

A. Meaning, Synonymy, and Analyticity

Prior to the publication of *Word and Object*, Quine scrutinized various intensional accounts of meaning, synonymy, and analyticity in his celebrated "Two Dogmas of Empiricism" and found them to be unacceptable. Quine rejected meanings on the grounds that such putative posits lack identity criteria (no entity without identity), they are nonexplanatory, and they even can be obfuscating. He did not reject meanings because meanings were said to be abstract objects. He fully appreciated that if an acceptable account of synonymy were at hand, then meanings could be taken as abstract objects, namely, as sets of synonymous expressions. However, Quine rejected synonymy (sameness of meaning) and analyticity (truth in virtue of meaning) because of their lack of clarity. True, synonymy and analyticity can be defined in terms of each other, but neither can be defined in terms of dispositions to verbal behavior. When critics complained that Quine's standard of clarity for analyticity and synonymy were unreasonably high, he responded by saying that he asks "no more, after all, than a rough characterization in terms of dispositions to verbal behavior" (WO 207). Consistent with his brand of behaviorism, Quine insists merely on a rough characterization and not a full definition of analyticity or synonymy in terms of dispositions to verbal behavior.

Quine's treatment of semantics is not simply negative, it is also constructive. In *Word and Object* (and in several subsequent writings), Quine articulates a scientific (behavioristic) semantics (see TT 43–54). The cornerstone of this scientific semantics is the behavioristic method of querying sentences for a subject's assent and dissent.

"Without this device there would be no hope of handing language down the generations, nor any hope of breaking into newly discovered languages. It is primarily by querying sentences for assent and dissent that we tap the reservoirs of verbal disposition" (MVD 88).

Quine calls the class of patterns of a person's activated nerve endings that would prompt a person's assent to a queried sentence the *affirmative stimulus meaning* (for that sentence, person, and time). The class that would prompt dissent he calls the *negative stimulus meaning* (for that sentence, person, and time). The ordered pair of the affirmative and negative stimulus meanings for a sentence constitutes its *stimulus meaning simpliciter* (for a person at a time). Note also that the affirmative and negative stimulus meanings for a sentence do not determine one another; there will be stimulus patterns belonging to neither. In other words, the querying of some sentence under certain stimulus conditions would elicit neither assent nor dissent.

Quine divides, again along behavioristic lines, the class of English declarative sentences into standing sentences and occasion sentences. Roughly, *standing* sentences are those to which a subject would assent or dissent each time they are queried *without* each query being accompanied by prompting nonverbal stimuli. For example, most English speakers would assent to 'There have been black dogs?' without being prompted each time by some black dog–presenting pattern of stimulation. Not so for the *occasion* sentence 'This dog is black'. An occasion sentence requires that a new prompting nonverbal stimulus be given on each occasion the sentence is queried.

Among the class of standing sentences are those Quine calls *eternal* sentences. The defining characteristic of such sentences is that their truth-values remain permanently fixed: "An eternal sentence may be general in import, or it may report a specific local event. In the latter case it will gain its specificity through explicit use of names, dates, or addresses. The eternal sentences most characteristic of scientific theory are of course general" (RR 63).

Among the class of occasion sentences are those Quine calls *observation* sentences. Quine has offered different characterizations of observation sentences at different times, but in *Word and Object* he wrote, "[I]n behavioral terms, an occasion sentence may be said to be the more observational the more nearly its stimulus meanings for different speakers tend to coincide" (WO 43). (Quine eventually realized

that no theoretical sense could be made of this talk of different individuals' stimulus meanings tending to coincide; after all, only in the rarest of circumstances do two individuals share any nerve ending. See PTb 40–5.)

Given his behavioristic notion of stimulus meaning and his behavioristic classification of sentences, Quine proceeds to construct his scientific semantics. He fashions, so far as possible, behavioristic parodies of the repudiated intensional notions of meaning, synonymy, and analyticity. For example, Quine explains that two occasion sentences are cognitively synonymous for a person if whenever he would assent to or dissent from the one, he would do likewise with regard to the other (i.e., when the two sentences have the same stimulus meaning for that person). Two such sentences are cognitively synonymous for the entire linguistic community if found to be cognitively synonymous for each member of the community. Furthermore, "a sentence is analytic if *everybody* learns that it is true by learning its words" (RR 79). Nevertheless, Quine is the first person to admit that none of these behavioristic parodies will bear the philosophical weight that some traditional epistemologists wanted the parodied intensional forerunners to bear. For example, Quine's behavioristic notion of analyticity cannot explain the putative necessity of mathematics – as the intensional notion of analyticity was thought by some logical positivists to do.

As previously noted, Quine characterized observation sentences differently in different places. For example, in *Roots of Reference*, he wrote, "A sentence is observational insofar as its truth value, on any occasion, would be agreed to by just about any member of the speech community witnessing the occasion. . . . What is worth noticing is that we have here a behavioral criterion of what to count as an observation sentence" (RR 39).

In "On Empirically Equivalent Systems of the World," he wrote, "The really distinctive trait of observation terms and sentences is to be sought not in concurrence of witnesses, but in ways of learning. Observational expressions are expressions that can be learned ostensively" (EES 316).

In "Empirical Content," he wrote,

An observation sentence is an occasion sentence that the speaker will consistently assent to when his sensory receptors are stimulated in certain ways, and consistently dissent from when they are stimulated in certain other

ways. If querying the sentence elicits assent from the given speaker on one occasion, it will elicit assent likewise on any other occasion when the same total set of receptors is triggered; and similarly for dissent. This and this only this is what qualifies sentences as observation sentences for the speaker in question, and this is the sense in which they are the sentences most directly associated with sensory stimulation. (EC 25)

Critics have claimed that some of these characterizations of observation sentences are inconsistent with one another. For example, Lars Bergström points out that, according to Quine's 1981 characterization (see EC 25),

a sentence may be observational for every speaker in a community, even though the speakers disagree about its truth value on many occasions. For example, some people may assent to 'It's cold' and 'That's a rabbit' on occasions when others dissent from these sentences. (People are not equally sensitive to cold, and many of us might easily mistake a hare for a rabbit.) In earlier writings, Quine had a different conception of an observation sentence: he required precisely that 'its truth value, on any occasion, would be agreed to by just about any member of the speech community witnessing the occasion . . . ' [RR 39]. However, this requirement is hardly consistent with his examples, and he has since claimed that the 'really distinctive trait of observation terms and sentences is to be sought not in concurrence of witnesses but in ways of learning. Observational expressions are expressions that can be learned ostensively. . . . ' [EES 316][1]

Getting clear on the nature of observation sentences is an important matter for Quine, since they play crucial roles in his scientific semantics and his epistemology. In his semantics, they are the kind of sentences that can be learned in isolation and whose meanings are pretty well captured by stimulus meaning ("all inculcation of meanings of words must rest ultimately on sensory evidence" [EN 75]). In his epistemology, they are the kind of sentences that state the intersubjectively appreciable evidence for science ("whatever evidence there *is* for science *is* sensory evidence" [EN 75]).

Quine responded to Bergström in "Comment on Bergström" (see CB 53–4) and more fully in *Pursuit of Truth*, where he wrote,

As for the lacuna that Bergström noted, . . . I retain my 1981 definition of observation sentence for the single speaker, and then account a sentence observational for a group if it is observational for each member *and* if each would agree in assenting to it, or dissenting, on witnessing the occasion of

utterance. We judge what counts as witnessing the occasion . . . by projecting ourselves into the witness's position. (PTb 43)

This talk of projecting ourselves into the witness's position – that is, talk of empathy, or *Verstehen* – has long been a part of Quine's thinking about language learning and the propositional attitudes. Only in his last writings, however, did he emphasize the role that empathy plays in scientific semantics. This is a point to which we shall return in discussing language learning and translation.

B. Language Learning

According to Quine, "Language is a social art. In acquiring it, we have to depend entirely on intersubjectively available cues as to what to say and when" (WO ix). In *Word and Object*, Quine sketches a largely Skinnerian theory of language learning wherein babbling, mimicry, conditioning, innate quality spaces, and ostension all play a role. When exposed to a linguistic environment, a normal child begins learning his first language. (A normal child is one who is endowed with instincts for babbling and mimicry, has a set of innate quality spaces needed for detecting and systematizing salient features of his environment, and is motivated by stimulations encoded with pleasure and pain.)

The initial method of his learning is *ostension*. Through babbling and mimicry, the child utters a sound, say that of 'mama', when Mama is present. Mama rewards this chance occurrence with a coo, a smile, or even with a pat or a hug. Thus is the child reinforced to repeat the performance. Such learning requires observationality. For example, in learning (or in teaching) ostensively the one-word sentence 'Fido', both the teacher and the pupil must see Fido's ostended surface at the time, and at least one of them must also see that the other sees Fido's surface at the time. Meeting this last condition involves empathy, or *Verstehen*.

Before long, by observing his elders' overt behavior under intersubjectively appreciable cues, the child catches on to the unconscious trick of associating sentences (as unstructured wholes) with his own appropriate nonverbal stimulations. In short, the child learns, inductively, the range of stimulus conditions governing the correct use of particular (observation) sentences. The psychological mechanism

underlying the method of ostension approximates direct conditioning. However, the conditioning involved is not of the simplest kind; the child does not utter 'Mama' or 'Fido' whenever he sees, respectively, Mama or Fido. Nevertheless, having once learned 'Mama' and 'Fido', he would assent to them when queried in Mama's and Fido's respective salient presences. "Once the child reaches this stage, his further learning of language becomes independent of operant behavior . . . ; and then, with little or no deliberate encouragement on the part of his elders, he proceeds to amass language hand over fist" (WO 82).

Quine acknowledges that "Skinner, whose ideas the foregoing sketch is meant to follow in essential respects, is not without his critics. But at worst we may suppose that the description, besides being conveniently definite, is substantially true of a good part of what goes into the first learning of words. Room remains for further forces" (WO 82). These further forces allow for a second general method for learning language, what Quine calls *analogic synthesis*. Sentences learned by this method are built up from learned parts by analogy with the ways in which those parts have been previously noticed to occur in other sentences – sentences that themselves may or may not have been learned as wholes. However, unlike the case of ostension, virtually nothing is known about the further psychological mechanisms underlying analogic synthesis.

In 1968, in "Linguistics and Philosophy" (see LP 56–8), Quine emphasized various features of the behavioristic theory of language learning he sketched in *Word and Object* eight years earlier. First, he emphasized that behaviorism and nativism are not incompatible:

[T]he behaviorist is knowingly and cheerfully up to his neck in innate mechanisms of learning-readiness. The very reinforcement and extinction of responses, so central to behaviorism, depends on prior inequalities in the subject's qualitative spacing, so to speak, of stimulations. . . . Innate biases and dispositions are the cornerstone of behaviorism, and have been studied by behaviorists. (LP 57)

Second, Quine reiterated features of his 1960 theory, namely, that quality spaces are innate and that more innate structure is required to explain language leaning: "The qualitative spacing of stimulations is as readily verifiable in other animals, after all, as in man; so

the language-readiness of the human infant must depend on further endowments" (LP 57). These further "as yet unknown innate structures, additional to mere quality space, that are needed in language-learning, are needed specifically to get the child over the great hump that lies beyond ostension, or induction" (LP 58). And what, according to Quine, is the fate of empiricism should the processes involved turn out to be very unlike the classical process of conditioning? "This would be no refutation of behaviorism, in a philosophically significant sense of the term; for I see no interest in restricting the term 'behaviorism' to a specific psychological schematism of conditioned response" (LP 57).

Six years after "Linguistics and Philosophy," in *Roots of Reference* (1974), Quine further refined and extended his 1960 theory of language learning. Talk of patterns of activated nerve endings gives way to talk of global episodes of activated nerve endings and of the receptual similarity of episodes; talk of quality spaces gives way to talk of the perceptual similarity and the behavioral similarity of episodes. Moreover, Quine extended his 1960 theory by speculating on the psychological mechanisms underlying analogic synthesis, mechanisms by which a child could learn to refer to substances, bodies, physical objects, and eventually abstract objects. More precisely, Quine speculated on how a child, or the race, could acquire first-order predicate logic and set theory. First-order predicate logic encapsulates in pristine form the referential mechanisms of English, and sets are abstract objects (or universals) par excellence. In extending his theory in *Roots of Reference* in this way, Quine did not abandon his behavioristic scruples, but he acknowledged elsewhere that in the final – speculative – third of the book "the behaviorism dwindles" (CP 291). He added the following, however:

Where I have insisted on behaviorism is in linguistics, because of how language is learned. I would hope and expect that behavioristic rigor could also be brought in pretty much along the course of the story sketched in *Roots of Reference*, but I was struggling with what I felt were more significant problems. I expected also that some notions would resist full reduction to behavioral criteria. I would never, early or late, have aspired to the ascetic adherence to operational definitions that [P. W.] Bridgman envisaged. Science settles for partial criteria and for partial explanation in terms of other partially explained notions. (CP 291)

Thus, Quine's speculations in *Roots of Reference* about how referential language emerges (for the individual or the race) do not represent a real retreat from his previous commitment to behaviorism in the study of language learning.

C. Translation and Indeterminacy

In Chapter 2 of *Word and Object*, Quine articulates his famous thought experiment concerning radical translation. In this thought experiment, a linguist is confronted with the task of translating a totally alien human language into, say, English. Since the language to be translated bears no historical or cultural ties to any known language, then, supposing there are no bilinguals, all the linguist will have to go on in constructing his Jungle-to-English manual of translation is the overt behavior of native speakers in publicly observable circumstances. For example, a rabbit scurries by, apparently prompting the native to utter 'Gavagai'. Upon witnessing this event, the linguist forms the tentative inductive hypothesis that 'Gavagai' can be translated as 'Rabbit'. Once the linguist identifies the Jungle expressions for assent and dissent, the linguist can query 'Gavagai' of the native as appropriate occasions present themselves. If all goes well, the native's assent to (or dissent from) the queried sentence coincides with the linguist's assent to (or dissent from) the one-word sentence 'Rabbit'. But things might not go well, the native may assent to (dissent from) 'Gavagai' where the linguist might dissent from (assent to) 'Rabbit'.

   The linguist in our story, practical soul that he is, knows nothing of stimulus meaning, with its patterns of activated nerve endings. The linguist relies, rather, on folk psychology and empathy to determine whether in some context 'Gavagai' translates as 'Rabbit'. Concern with stimulus meaning enters at the theoretical level. It may seem that it makes sense to say that the linguist's stimulus meaning for 'Rabbit' is approximately the same as the native's stimulus meaning for 'Gavagai'. In *Roots of Reference*, Quine calls this the homology assumption. But there is a fly in the ointment.

   Let us assume that the translation of 'Gavagai' as 'Rabbit' has held up under testing. What theoretical sense can be made of the claim that the native's stimulus meaning for 'Gavagai' is approximately the same as the linguist's stimulus meaning for 'Rabbit'? Not much,

for stimulus meaning is defined relative to an individual's nerve nets, and no two people share nerve nets. In *Roots of Reference*, Quine deals with the homology problem as follows:

In practice, of course, psychologists find no difficulty in such intersubjective equating of stimulus situations; they simply see that there is no physical differences that are apt to matter. We shall do well to take the same line, having just noted in passing that there is more to equating stimulations than meets eye, or indeed perhaps rather less than seems to do so. (RR 24)

But as theorists, what are we to do apart from practice? There the homology question continued to rankle until Quine addressed the issue in "Three Indeterminacies" and, more fully, in *Pursuit of Truth*. In these writings, Quine modifies his thought experiment concerning radical translation so as to avoid raising the homology question. Thus, instead of saying that the native's stimulus meaning for 'Gavagai' is approximately the same as the linguist's for 'Rabbit', one can make do with talking solely of the linguist's stimulus meaning:

The observation sentence 'Rabbit' has its stimulus meaning for the linguist and 'Gavagai' has its for the native, but the affinity of the two sentences is to be sought in the externals of communication. The linguist notes the native's utterance of 'Gavagai' where he, in the native's position, might have said 'Rabbit'. So he tries bandying 'Gavagai' on occasions that would have prompted 'Rabbit', and looks to the natives for approval. Encouraged, he tentatively adopts 'Rabbit' as translation. (PTb 42)

In short, the native's stimulus meaning for 'Gavagai' is dropped, and the linguist's empathy with the native's perceptual situation is added. The homology question is rendered otiose. Moreover, this tact renders the linguist's imagined strategy in the thought experiment concerning radical translation both more realistic and more in tune with Quine's long-held view of the child's strategy in the normal language-learning context:

Empathy dominates the learning of language, both by child and by field linguist. In the child's case it is the parent's empathy. The parent assesses the appropriateness of the child's observation sentence by noting the child's orientation and how the scene would look from there. In the field linguist's case it is empathy on his own part when he makes his first conjecture about 'Gavagai' on the strength of the native's utterance and orientation, and again when he queries 'Gavagai' for the native's assent in a promising subsequent

situation. We all have an uncanny knack for empathizing another's perceptual situation, however ignorant of the physiological or optical mechanisms of his perception. (PTb 42)

Where does this new wrinkle, this talk of the role that empathy plays in radical translation, leave Quine's behaviorism? Recall that Quine steadfastly maintained that his form of behaviorism (externalized empiricism) condones introspection as a means of arriving at conjectures or conclusions only insofar as these eventually can be made sense of in terms of external observation. Are the linguist's reliance on empathy to translate native sentences and the parent's reliance on empathy to teach the child some sentence such forms of introspection? Can the linguist and the parent, even in principle, objectively test their conjectures? Each such "test" would seem always to rely on a further instance of empathy. However, this "hermeneutic circle" can be broken if, as Quine maintains, the test involves the externals of communication.

Be that as it may, soon in his task of constructing a Jungle-to-English translation manual, the linguist rises above inductive hypotheses regarding translations of native observation sentences such as 'Gavagai' and formulates analytical hypotheses that allow for translating words and theoretical sentences that are remote from stimulus meanings. Quine maintains that, unlike inductive hypotheses (*real* hypotheses), analytical hypotheses are nonfactual. Still, it is empathy again that guides the linguist in formulating his analytical hypotheses, "though there he is trying to project into the native's associations and grammatical trends rather than his perceptions. And much the same must be true of the growing child" (PTb 43).

With the advent of these nonfactual analytical hypotheses, translation of theoretical sentences goes indeterminate. Different linguists who formulate different sets of analytical hypotheses could construct different Jungle-to-English manuals of translations such that the "manuals might be indistinguishable in terms of any native behavior that they gave reason to expect, and yet each manual might prescribe some translations that the other translator would reject. Such is the indeterminacy of translation" (ITA 8).

As Quine makes explicit, there is a behavioristic source of the indeterminacy thesis:

Critics have said that the thesis is a consequence of my behaviorism. Some have said that it is a *reductio ad absurdum* of my behaviorism. I disagree with the second point, but I agree with the first. I hold further that the behaviorist approach is mandatory. In psychology one may or may not be a behaviorist, but in linguistics one has no choice. Each of us learns his language by observing other people's verbal behavior and having his own faltering verbal behavior observed and reinforced or corrected by others. We depend strictly on overt behavior in observable situations; . . .

There is nothing in linguistic meaning, then, beyond what is to be gleaned from overt behavior in observable circumstances. (ITA 5)

Since "the only facts of nature that bear on the correctness of translation are speech dispositions" (RHP 429), "even a full understanding of neurology would in no way resolve the indeterminacy of translation" (RRN 365).

And not only is there indeterminacy of theoretical sentences, there is also indeterminacy of reference (or inscrutability of reference). The point here is that stimulus meaning does not determine reference. Knowing that the native's occasion sentence 'Gavagai' is translatable into English as the occasion sentence 'Rabbit' does not settle the question whether 'gavagai' is a native term, or, if it is, what it refers to. The only way to settle these issues is against a background of some nonunique set of analytical hypotheses (which, as already noted, are by their very nature nonfactual). Thus, consistent with the speech dispositions of all concerned, one linguist might translate 'gavagai' as a concrete general term denoting rabbits whereas another linguist might translate it as an abstract singular term designating rabbithood. And just as with the indeterminacy of translation of theoretical sentences, both of these translations of 'gavagai' are fully correct. The question which (if either) of these translations captures what the native intended by 'gavagai' is spurious; there is simply no fact of the matter.

In sum, Quine's behaviorism permeates his philosophy of language. It shapes his treatment of meaning, synonymy, analyticity, language learning, reference, and translation. Moreover, it shapes his general epistemology. We have noted that he rejects foundationalist epistemology (first philosophy), but he remains interested in the empirical study of the epistemological relation of evidence to theory. Breaking with the empiricist tradition, though, Quine calls for externalizing that study – that is, for construing the relation of

evidence to theory as a relation between observation sentences and theoretical sentences. For the externalized epistemologist, the theory of language learning takes on added significance:

We see, then, a strategy for investigating the relation of evidential support, between observation and scientific theory. We can adopt a genetic approach, studying how theoretical language is learned. For the evidential relation is virtually enacted, it would seem, in the learning. This genetic strategy is attractive because the learning of language goes on in the world and is open to scientific study. It is a strategy for the scientific study of scientific method and evidence. We have here a good reason to regard the theory of language as vital to the theory of knowledge. (NNK 74–5)

Hence my earlier claim that Quine's writings on meaning, synonymy, analyticity, language leaning, reference, and translation emanate more from his passion for epistemology than from his passion for languages and linguistics.

D. Philosophy of Mind

If Quine's interest in epistemology shapes his philosophy of language, so does his philosophy of language shape his philosophy of mind. Quine says in "Mind and Verbal Dispositions" that he believes in the affinity of mind and language, though he wants to keep the relation right side up. However inadequate, J. B. Watson's theory of thought, which holds that most thought is incipient speech, has matters right side up: "A theory of mind can gain clarity and substance, I think, from a better understanding of the workings of language, whereas little understanding of the workings of language is to be hoped for in mentalistic terms" (MVD 84). And after surveying his theories of language leaning and linguistic meaning, Quine concludes "Mind and Verbal Disposition" with an endorsement of the identity theory:

[M]ind consists in dispositions to behaviour, and these are physiological states. We recall that John B. Watson did not claim that quite *all* thought was incipient speech; it was all incipient twitching of muscles, and *mostly* of speech muscles. Just so, I would not identify mind quite wholly with verbal disposition; with [Gilbert] Ryle and [Wilfrid] Sellars I would identify it with behavioural dispositions, and *mostly* verbal. And then, having construed

behavioural dispositions in turn as physiological states, I end up with the so-called identity theory of mind: mental states are states of the body. (MVD 94)

Quine's philosophy of mind can be briefly summarized as follows: Most everyday uses of mentalistic terms (e.g., 'belief', 'desire', and so on) have empirical content, though many uses of the same terms do not. We apply mentalistic terms having empirical content to persons other than ourselves largely (but not always) on the basis of those persons' behavioral symptoms. This follows from the way such terms are learned: "[S]uch terms are applied in the light of publicly observable symptoms: bodily symptoms strictly of bodily states. . . . Without the outward signs to begin with, mentalistic terms could not be learned at all" (SM 5–6). (Here is a clear example of Quine's philosophy of language shaping his philosophy of mind.)

However, such behavioral symptoms are neither necessary nor sufficient for ascribing mentalistic terms to other persons in particular instances since mental states do not manifest themselves in behavior and since mentalistic terms are vague. "Other grounds for ascribing beliefs [for example] may be sought unsystematically by probing the subject's past for probable causes of his present state of mind, or by seeing how he will defend his purported belief when challenged" (SM 7).

On the other hand, in ascribing menalistic terms to ourselves, we can rely on introspection: "[I]ntrospection may be seen as a witnessing to one's own bodily condition, as in introspecting an acid stomach, even though the introspector be vague on the medical details" (WO 264–5). (Thus does Quine's brand of behaviorism reserve a role for introspection in his philosophy of mind.)

When mentalistic ascriptions (grounded on either behavioral symptoms or introspection) have empirical content, they do *not* refer to behavior; rather, they refer to dispositions, mostly verbal ones. And since Quine construes such dispositions as physiological states of the organism, "it is these states that the [contentful] mental terms may be seen as denoting" (SM 6).

Quine has called his theory of mind the identity theory, but he has also called it the repudiation theory. What is the difference? None, according to Quine: "In either case the states of nerves are retained, mental states in any other sense are repudiated, and the mental terms

are thereupon appropriated to states of nerves" (SM 6). Nonetheless, Quine prefers the repudiation theory over the identity theory. He does so because the identity theory is so easily abused:

For, product though the identity theory is of hard-headed materialism, we must be aware of its sedative use to relieve intellectual discomfort. We can imagine someone appealing to the identity theory to excuse his own free and uncritical recourse to mentalistic semantics. We can imagine him pleading that it is after all just a matter of physiology, even if no one knows quite how. This would be a sad irony indeed, and the repudiation theory has the virtue, over the identity theory, of precluding it. (MVD 95)

However, whether the repudiation theory is adopted or not, there are some dispositions to behavior that are more explanatory than others. "The ones we should favour, in explanations, are the ones whose physiological mechanisms seem likeliest to be detected in the foreseeable future" (MVD 95).

Though Quine accepts the repudiation theory, in which contentful mental state terms refer to physiological states, he admits that "there is no presumption that the mentalistic idioms would in general be translatable into anatomical and biochemical terminology of neurology, even if all details of the neurological mechanisms were understood" (SM 6). The uniform structure of the idioms of the prepositional attitudes, for example, mask the great heterogeneity in empirical evidence and neural mechanisms. Thus Quine concludes that "even those of us who do not acquiesce in a metaphysical dualism of mind and body must take the best of what [Donald] Davidson has called anomalous monism" (SM 7).

3. CONCLUSION

Quine's brand of behaviorism is less rigorous than some. For example, he rejects any definition of behaviorism that limits it to conditioned response. Indeed, he is willing to give up the term as descriptive of his methodology in epistemology, philosophy of language, and philosophy of mind in favor of the term 'externalized empiricism'. Terminology aside, what matters to Quine is that theorists maintain empiricist discipline, and that means couching all criteria for ascribing mentalistic terms in observational terms (i.e., terms that can be taught by ostension). As a further departure

from stricter behaviorism, Quine's behaviorism reserves a role for introspection.

As Quine remarks, one may or may not choose to be a behaviorist in psychology, but one has no choice but to be a behaviorist in linguistics. Quine believes one has no choice based on the empirical claim that people learn their language by observing the behavior of other people amid intersubjectively appreciable circumstances. The corollary to this empirical claim about language learning is the semantical claim that there is nothing to linguistic meaning that cannot be manifested in behavior. But if this were so, then ascriptions of intensional terms generally can sometimes extend vacuously beyond all the behavioral facts. This overextendedness can occur in both theoretical and practical contexts. And when theories of language learning, or of semantics, or of mind routinely incorporate such lapses of empiricist discipline, Quine considers them to be unscientific. Indeed, this is perhaps the central philosophical point of Quine's right-minded behaviorism cum empiricism.

NOTES

This essay is an abridged and corrected version of my essay "Quine's Behaviorism," *The Philosophy of Psychology*, ed. William O'Donohue and Richard E. Kitchener (London: Sage Publications, 1996), 96–107. "Quine's Behaviorism" was reprinted in *Handbook of Behaviorism*, ed. William O'Donohue and Richard E. Kitchener (San Diego: Academic Press, 1999), 419–36. "Quine's Behaviorism cum Empiricism" appears here with the permission of these presses and editors.

1. Lars Bergström, "Quine on Underdetermination," in *Perspectives on Quine*, ed. R. Barrett and R. Gibson (Oxford: Blackwell, 1990), 39.

8   Quine on Modality

One main theme in Quine's philosophy, one that emerged in the very early article "Truth by Convention" (1936), was skepticism toward the notions of meaning and analyticity. These were key notions in the work of Carnap and other philosophers whom Quine regarded highly. His criticism soon spread to the notions of logical necessity and possibility, which, following Carnap and C. I. Lewis, he regarded as closely connected with the former notions. Carnap and Lewis subscribed to the so-called linguistic view on necessity, which Quine formulated this way: "[A] statement of the form 'Necessarily . . . ' is true if and only if the component statement which 'necessarily' governs is analytic, and a statement of the form 'Possibly . . . ' is false if and only if the negation of the component statement which 'possibly' governs is analytic" (RAM 143).

Quine saw two kinds of problems connected with the modal notions. First, like the notions of meaning and analyticity, they are unclear: It is hard to draw a line between what is necessary and what is merely accidental. This is the case with many other notions, too. Where does one draw the line between mountains and mere hills, and when does a man cease being thin haired and become bald? However, the obscurity affecting the modal notions and the notions of meaning and analyticity is of a different and more malignant kind. There is not just vagueness, a problem of difficult borderline cases; even in the seemingly most clear-cut cases, it is difficult to understand what distinguishes the necessary from that which is merely possible. One can, of course, "explain" necessity in terms of possibility: What is necessary is what cannot possibly be otherwise. However, unless we come up with an illuminating account of

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possibility that does not invoke necessity, we here move in a very small circle.

This is also one reason Quine found the so-called "possible world semantics" question-begging. Surely, such semantics are formulated in an extensional metalanguage, but since in this metalanguage one quantifies over possible worlds, the semantics do not bring us any further toward understanding the modal notions unless the notion of possible worlds is made clear. All the semantics tell us is how the key notions of the small circle are interconnected: Possibility may in most such semantics be defined in terms of necessity, thus: '$\diamondsuit$ p if and only if $\sim\Box\sim$ p'. In some semantics, what is not necessary is necessarily not necessary: 'If $\sim\Box$ p, then $\Box\sim\Box$ p', and so on.

Quine's second problem with the modalities is that they are ontologically obscure. Not only is the notion of a possible world murky, but also what objects we are speaking about when we use modal expressions is unclear. These ontological problems are much more conspicuous in the case of the modalities than they are in connection with meaning. For Quine, who regarded the modalities as intimately connected with the notion of meaning via the linguistic doctrine of necessity, the ontological obscurity of the modalities was therefore particularly well suited to bring out the murkiness of this whole cluster of interrelated notions.

SUBSTITUTIVITY OF IDENTITY

Already in his first criticism of the modalities, his contribution to the Whitehead volume in the *Library of Living Philosophers* in 1941, Quine therefore focused on the ontological issues. He observed that the principle of substitutivity of identity breaks down in modal contexts: If in '$\diamondsuit$ (the number of planets >7)', which is presumably true, we substitute for '9' 'the number of planets', we get '$\diamondsuit$ (9>7)', which is false. So what, then, is meant by the identity statement 'the number of planets = 9'? What is the object that is possibly larger than 7 but not possibly larger than 7?

In his review of this essay in *The Journal of Symbolic Logic* the following year, Alonzo Church pointed out that Quine's example involves the description or class abstract 'the number of planets', which both in *Principia Mathematica* and in Quine's own *Mathematical*

Logic would be construed contextually: "[A]ny formal deduction must refer to the unabbreviated forms of the sentences in question, and the unabbreviated form of the first sentence is found actually to contain no name of the number 9" (ML 101).

Church added that he would prefer a system in which class abstracts and descriptions are construed as names and hence are not contextually defined. In a system of this kind, Quine's argument shows that "a non-truth-functional operator, such as '◇', if it is admitted, must be prefixed to names of propositions rather than to sentences" (ML 101).

QUANTIFIED MODALITIES

That same year, 1941, in his review of Russell's *Inquiry into Meaning and Truth*, Quine called attention to the difficulties relating to quantification into belief contexts: "Moreover, he [Russell] never mentions the more difficult sort of contexts, wherein the matter following 'believes that' falls short of being a sentence because a variable in it is governed by a quantifier somewhere to the left of 'believes that'" (ROR 29–30).

This remark in Quine's review of Russell shows that already in 1941 Quine was aware that there were problems with quantification into modal contexts. This is what we should expect: If substitutivity of identity breaks down in modal contexts, it is unclear what the objects are over which we quantify. That is, it seems that we cannot quantify into modal contexts.

Quine's remark was, as far as I know, the first objection ever raised against quantification into modal contexts. Two years later, in "Notes on Existence and Necessity" (1943), Quine spelled out his objection in more detail. He noted that there is a curious air about, for example,

$$(\exists x) \, \Box \, (x > 7)$$

and he asked, "[W]ould 9, that is, the number of planets, be one of the numbers necessarily greater than 7?" (ROR 30). Quine pointed out that such an affirmation would be true in the form

$$\Box \, (9 > 7)$$

and false in the form

$$\Box(\text{the number of planets} > 7)$$

Nevertheless, modal logicians have tried to construct systems of quantified modal logic. And for good reasons. For as Carnap put it some years later, "Any system of modal logic without quantification is of interest only as a basis for a wider system including quantification. If such a wider system were found to be impossible, logicians would probably abandon modal logic entirely."[1]

INTENSIONAL ONTOLOGY

In the same paper ("Notes on Existence and Necessity"), Quine also pointed out that, as already remarked, substitutivity of identity breaks down in the context 'Necessarily . . . ', at least when 'necessarily' is taken in the "analytic" sense, and that this context, therefore, is similar to a quotation context, which does not admit pronouns that refer to quantifiers anterior to the context (NEN 123). Church, in his review of Quine's "Notes on Existence and Necessity" in *The Journal of Symbolic Logic*, agreed with Quine that modal contexts are opaque.[2] But Church argued that this does not prevent variables within the modal context from referring to a quantifier anterior to the context, *provided the quantifier has an intensional range* – a range, for instance, composed of attributes rather than classes.

In 1945–46, in letters to Carnap, quoted in Carnap's *Meaning and Necessity*, Quine agrees that "adherence to an intensional ontology, with extrusion of extensional entities altogether from the range of values of the variables, is indeed an effective way of reconciling quantification and modality."[3] But he points out that this is a more radical move than one might think, as becomes apparent when one tries to reformulate in intensional language these two statements:

> The number of planets is a power of three.
> The wives of two of the directors are deaf.

It can be done, but the examples "give some hint of the unusual character which a development of it [an intensional language] adequate to general purposes would have to assume."[4]

SUBSTITUTIONAL QUANTIFICATION

In 1947, in "The Problem of Interpreting Modal Logic," Quine again took up the problem of making sense of existential quantifications containing modal operators. Let us suppose, he argued, that we try to make sense of such quantifications by using, for example, this criterion:

> (SUBST) An existential quantification holds if there is a constant whose substitution for the variable of quantification would render the open sentence true.

This criterion is only a partial (sufficient) one, because of unnamed objects, hence the 'if'. But it allows Quine to show that a quantified modal logic would have queer ontological consequences:

It leads us to hold that there are no concrete objects (men, planets, etc.), but rather that there are only, corresponding to each supposed concrete object, a multitude of distinguishable entities (perhaps 'individual concepts', in Church's phrase). It leads us to hold, e.g., that there is no such ball of matter as the so-called planet Venus, but rather at least three distinct entities: Venus, Evening Star, and Morning Star. (PIML 47)

To show this, Quine uses

'*C*' for 'congruence' to express the relation that Venus, the Evening Star, and the Morning Star, e.g., bear to themselves and, according to empirical evidence, to one another. (It is the relation of *identity* according to materialistic astronomy, but let us not prejudge this.) (PIML 47)

Then

Morning Star *C* Evening Star. □ (Morning Star *C* Morning Star).

Hence, by (SUBST),

(1)   ($\exists$ *x*)(*x C* Evening Star . □ (*x C* Morning Star)).

But also

Evening Star *C* Evening Star . $\sim$ □ (Evening Star *C* Morning Star)

and

Evening Star *C* Evening Star. □ (Evening Star *C* Morning Star)

so that, by (SUBST),

(2) $(\exists\, x)(x\ C\ \text{Evening Star} .\sim \Box\ (x\ C\ \text{Morning Star}))$.

Since the open sentences quantified in (1) and (2) are mutual contraries, Quine concludes that there are at least two objects congruent to the Evening Star. Similarly, if the term 'Venus' is introduced, a third such object can be inferred.

Parallel arguments may be used to show that the contemplated version of quantified modal logic is committed to an ontology repudiating classes and admitting only attributes, Quine adds. But the argument concerning individuals just stated is, of course, more disturbing for modal logicians.

Later the same year, in his review of Ruth Barcan's "The Identity of Individuals in a Strict Functional Calculus of Second Order," Quine remarks that Barcan's system is "best understood by reconstructing the so-called individuals as 'individual concepts'" (RRB 96).

DIFFICULTIES THAT DO NOT DEPEND ON SINGULAR TERMS

The article "Reference and Modality" in Quine's *From a Logical Point of View* (1953) is a fusion of "Notes on Existence and Necessity" with "The Problem of Interpreting Modal Logic." But new arguments are added, notably arguments to the effect that we cannot properly quantify into a modal context.

First, lest the reader feel that the arguments against quantification into modal contexts always turn on an interplay between singular terms like 'Tully' and 'Cicero', '9' and 'the number of planets', 'Evening Star' and Morning Star', Quine reargues the meaninglessness of quantification into modal contexts *without reverting to singular terms*. He points out that one and the same number x is uniquely determined by these conditions:

(3) $x = \sqrt{x} + \sqrt{x} + \sqrt{x} \neq \sqrt{x}$

and

(4)   There are exactly x planets.

Nevertheless (3) has '$x > 7$' as a necessary consequence, while (4) has not. "*Necessary* greaterness than 7 makes no sense as applied to a

number x; necessity attaches only to the connection between '$x >$ 7' and the particular method (3), as opposed to (4), of specifying x" (RAM 149).

Similarly, Quine argues that

$$(\exists\, x)(\text{necessarily if there is life on} \\ \text{the Evening Star then there is life on } x)$$

is meaningless "because the sort of thing x which fulfills the condition

(5)   If there is life on the Evening Star then there is life on x,

namely, a physical object, can be uniquely determined by any of various conditions, not all of which have (5) as a necessary consequence. *Necessary* fulfillment of (5) makes no sense as applied to a physical object x; necessity attaches, at best, only to the connection between (5) and one or another means of specifying x" (RAM 149).

ANALYTICALLY EQUIVALENT CONDITIONS

A few pages further on in the article, Quine makes the following proposal[5]: Suppose that, in order to overcome this difficulty, one were to retain within one's universe of discourse only objects x such that *any two conditions uniquely determining* x *are analytically equivalent*, that is, such that

(EQUIV) $(y)(Fy \equiv .\, y = x)\,.\,(y)(Gy \equiv .\, y = x)\,.\supset \square\, (y)(Fy \equiv Gy).$

If we were to permit quantification into modal contexts, this can be simplified to

(EQUIV′) $(y)(Fy \equiv .\, y = x)\, \supset \square\, (y)(Fy \equiv .\, y = x).$

IDENTITIES ARE NECESSARY

Condition (EQUIV), or (EQUIV′), has, however, Quine points out, consequences that some modal logicians might be reluctant to accept, for example,

(6) $(x)(y)(x = y\,.\supset \square\, (y = x)),$

which is got by introducing the predicate '①$= y$' for 'F ①' and then simplifying and closing.

In "Three Grades of Modal Involvement" (1953, p. 80), Quine adduces a different argument to the same effect, viz., that

$$(x)(y)(x = y \, . \supset \Box \, (y = x)).$$

In any theory, whatever the shape of its symbols, an open sentence whose free variables are 'x' and 'y' is an expression of identity only in case it fulfills

(7) $(x)(y)(x = y \, . \supset . \; Fx \equiv y)$

in the role of '$x = y$'. But introducing the predicate '$\Box \, (x = ①)$' for 'F ①' in (7) (or rather in '$x = y \, . \supset . \; Fx \equiv Fy$' and then closing), as we can do if we admit quantification into modal contexts, we get, after simplification, (6) of the preceding argument over again:

$$(x)(y)(x = y \, . \supset \Box \, (y = x)).$$

INTERFERENCE IN THE CONTEXTUAL DEFINITION OF SINGULAR TERMS

Quine adds that we do not have to infer from this that

$$\Box \, (\text{the number of planets} = 9)$$

if we accept some interference in the contextual definition of singular terms even when their objects exist (e.g., a rule that we can't use them to instantiate universal quantification unless some special supporting lemma is at hand; see TGMI).

ARISTOTELIAN ESSENTIALISM

A third consequence of quantification into modal contexts, in addition to (6) and interference in the contextual definition of singular terms, is, according to Quine, Aristotelian essentialism, that is, the doctrine that some of the attributes of a thing are essential to it, necessary of the thing regardless of the way in which we refer to it, while other attributes are accidental to it. For example, a man is essentially rational, not merely *qua* man but *qua* himself. Quine even

points out that not only can one have open sentences 'Fx' and 'Gx' such that

$$(\exists\, x)(\Box\, Fx.Gx.\sim \Box\, Gx)$$

but one must require that there are open sentences fulfilling

$$(x)(\Box\, Fx.Gx.\sim \Box\, Gx).$$

"An appropriate choice of 'Fx' is easy: '$x = x$'. And an appropriate choice of 'Gx' is '$x = x.p$' where in place of 'p' any statement is chosen which is true but not necessarily true" (TGMI 81).

COLLAPSE OF MODAL DISTINCTIONS

In *Word and Object*, Quine draws a further disastrous consequence of the requirement on open sentences at which he arrived in *From a Logical Point of View* (requirement (EQUIV) above), viz., the consequence that every true sentence is necessarily true (WO 197–8).

Let 'p' stand for any true sentence, let x be any object in our purified universe of discourse, and let $w = x$. Then

(8) $$(y)(p.y = w.\equiv. y = x)$$

(9) $$(y)(y = w.\equiv. y = x)$$

Introducing '$p.\; ① = w$' for '$F\; ①$' and '$① = w$' for '$G①$' in (EQUIV), one gets

(10) $$(y)(p.y = w.\equiv. y = x).\; (y)(y = w.\equiv. y = x) \supset$$ $$\Box\,(y)\,(p.y = w.\equiv. y = w),$$

which together with (8) and (9) implies

(11) $$\Box\,(y)(p.y = w.\equiv. y = w).$$

But the quantification in (11) implies in particular '$p.w = w .\equiv. w = w$', which in turn implies 'p', so from (11) we conclude '$\Box\, p$'.

Since in this proof nothing is assumed about the objects over which we quantify, restricting the values to intensional objects does not prevent this collapse of modal distinctions. So, unless quantification into modal contexts can be interpreted without assuming (EQUIV), the prospects for a quantified modal logic are very gloomy indeed.

This argument from 1960 was meant by Quine to clinch his case against the modalities. After nineteen years of steadily stronger arguments, he finally had something that came close to a proof that if one quantifies into modal contexts, then the modal distinctions collapse, in which case the modalities would no longer have any point.

changes in quine's view on the modalities

In the spring of 1961, Quine came to acknowledge the following points:

1. There is something wrong with the argument from *Word and Object* that we just went through. It applies not just to necessity and possibility but to all operators that aim at singling out from the class of all true sentences a proper subclass. An argument parallel to that of Quine shows that the intended distinction will collapse: The subclass will coincide with the full class. Hence, for example, since 'knows that' is such an operator, everything that is true will be known. Many other notions also will collapse, such as probability, obligation, belief, and so on.

2. By formalizing the argument so as to make its various assumptions explicit, one finds that the argument makes no assumptions that were not universally accepted in 1960.

3. The assumption that can most plausibly be given up is the unified, or one-sorted, semantics that one finds in Frege, Carnap, and many others, that is, the view that singular terms, general terms, and sentences all have the same kind of semantics – they have a meaning that determines their reference.

4. If one assumes a two-sorted semantics, where general terms and sentences behave as in standard Fregean semantics while singular terms keep their reference "in all possible worlds" (if one likes to speak that way), one can avoid the collapse and make sense of quantification into modal contexts.

   Restricting the universe to concepts or other intensional entities has no point. It is the singular terms and not the objects that matter. If the singular terms satisfy the condition

just mentioned, then quantification works whatever kind of objects one quantifies over.

As we noted earlier, Church argued that one can quantify into modal contexts provided the quantifier has an intensional range – a range, for instance, composed of attributes rather than classes. However, what saves his "logic of sense and denotation" from collapse is not this feature but the Frege-inspired reference shift that takes place within modal contexts: What object a variable takes as value depends on the modal operators (or, in Church's case, modal predicates) within whose scope it occurs. Thanks to this feature, Church's system does not have any opaque contexts. All its contexts are referentially and extensionally transparent, and there is no need to have a two-sorted semantics in order to prevent a collapse of modal distinctions. Although it uses symbols like '□', Church's system is not a system of modal logic; instead, it is a purely extensional system.

Carnap proposes in *Meaning and Necessity* (1947) a system of modal logic, S_2, where, in effect, he interprets the quantifiers as ranging over intensions. (This is not clear from his presentation, since he operates with two "identity" relations, identity of extension '≡' and identity of intension '=', where 'a ≡ b' is defined as '$N(a ≡ b)$'. However, only identity of intension has the properties characteristic of an identity relation.) Carnap states that "in order to avoid certain complications, which cannot be explained here, it seems advisable to admit in S, only descriptions which do not contain 'N'."[6] Carnap never mentioned what the complications are. He may not have discovered that one of them was the collapse of modal distinctions. If so, he might have seen, as Quine saw later, that the root of the trouble is singular terms that contain descriptive elements.

The first systems of quantified modal logic that were proposed had no singular terms other than variables. Since variables keep their reference from one possible world to another, the collapse discussed by Quine was not brought to the fore until one got systems of quantified logic that included singular terms other than variables.

5. Several of Quine's insights concerning quantified modalities continue to hold after one has given up one-sorted semantics: the substitutivity of identity, the necessity of identity, and Aristotelian essentialism.

   Two-sorted semantics makes it possible to have contexts that are referentially transparent, so that quantification into them makes sense, and extensionally opaque, so that modal distinctions do not collapse. This is just what Aristotelian essentialism amounts to: We distinguish between necessary and contingent attributes (extensional opacity), and the objects over which we quantify have these attributes regardless of the way in which the objects are referred to (referential transparency).

Quine immediately rewrote the parts of *From a Logical Point of View* that deal with modalities. In the new edition, which was out that same fall, Quine carried out the following revisions. First, he gave up the view that restricting the universe to intensional entities will alleviate the situtation:

Actually, even granting these dubious entities we can quickly see that the expedient of limiting the values of variables to them is after all a mistaken one. It does not relieve the original difficulty over quantifying into modal contexts; on the contrary, examples quite as disturbing as the old ones can be adduced within the realm of intensional objects....

It was in my 1943 paper [NEN] that I first objected to quantifying into modal contexts, and it was in his review of it that Church proposed the remedy of limiting the variables thus quantified to intensional values. This remedy, which I have just now represented as mistaken, seemed all right at the time. Carnap [in *Meaning and Necessity*] adopted it in an extreme form, limiting the range of his variables to intensional objects throughout his system. He did not indeed describe his procedure thus; he complicated the picture by propounding a curious double interpretation of variables. But I have argued that this complicating device has no essential bearing and is better put aside.

By the time Church came to propound an intensional logic of his own [in "A Formulation of the Logic of Sense and Denotation"] he perhaps appreciated that quantification into modal contexts could not after all be legitimized simply by limiting the thus quantified variables to intensional values. Anyway his departures are more radical. Instead of a necessity operator

attachable to sentences, he has a necessity predicate attachable to complex names of certain intensional objects called propositions. What makes this departure more serious than it sounds is that the constants and variables occurring in a sentence do not, without special provision, recur in the name of the corresponding proposition. Church makes such provision by introducing a primitive function that applies to intensional objects and yields their extensions as values. The interplay, usual in modal logic, between occurrences of expressions outside modal contexts and recurrences of them inside modal contexts, is mediated in Church's system by this function. Perhaps we should not call it a system of modal logic; Church generally did not. Anyway, let my continuing discussion be understood as relating to modal logics only in the narrower sense, where the modal operator attaches to sentences. (FLPV 152–4)

Second, Quine then pointed out that the key to the problem lies in the terms, not in the objects:

The only hope lies in accepting the situation illustrated by . . . [(3)] and . . . [(4)] and insisting, despite it, that the object x in question is necessarily greater than 7. This means adopting an invidious attitude towards certain ways of uniquely specifying x, for example . . . [(4)], and favoring other ways, for example . . . [(3)], as somehow better revealing the "essence" of the object. . . .

Evidently this reversion to Aristotelian essentialism . . . is required if quantification into modal contexts is to be insisted on. . . .

Essentialism is abruptly at variance with the idea, favored by Carnap, Lewis, and others, of explaining necessity by analyticity. . . . For the appeal to analyticity can pretend to distinguish essential and accidental traits of an object only relative to how the object is specified, not absolutely. Yet the champion of quantified modal logic must settle for essentialism.

Limiting the values of his variables is neither necessary nor sufficient to justify quantifying the variables into modal contexts. Limiting their values can, however, still have this purpose in conjunction with his essentialism: if he wants to limit his essentialism to special sorts of objects, he must correspondingly limit the values of the variables which he quantifies into modal contexts. . . .

The upshot of these reflections is meant to be that the way to do quantified modal logic, if at all, is to accept Aristotelian essentialism. To defend Aristotelian essentialism, however, is not part of my plan. Such a philosophy is as unreasonable by my lights as it is by Carnap's or Lewis's. And in conclusion I say, as Carnap and Lewis have not: so much the worse for quantified modal logic. By implication, so much the worse for unquantified modal logic as well; for, if we do not propose to quantify across the necessity

operator, the use of that operator ceases to have any clear advantage over merely quoting a sentence and saying that it is analytic. (FLPV 155–6)

Quine never again mentioned his argument in *Word and Object* for the collapse of modal distinctions. He no longer envisaged a strictly formal argument against quantified modal logic but returned to his original reason for rejecting the modal notions: their lack of clarity. As the last passage shows, Quine regarded them as of the same ilk as the notion of analyticity.

NOTES

1. R. Carnap, *Meaning and Necessity* (Chicago: University of Chicago Press, 1947), 196.
2. A. Church, "Review of Quine," *Journal of Symbolic Logic* 8 (1943): 45–7.
3. Carnap, *Meaning and Necessity*, 197.
4. Ibid.
5. This proposal is offered on p. 152 of the first edition of *From a Logical Point of View* (1953). It is deleted from the second edition.
6. Carnap, *Meaning and Necessity*, 184.

9 Quine and Logical Positivism

1. INTRODUCTION

In "Two Dogmas of Empiricism," Quine's most widely cited and reprinted paper, he famously rejects the analytic-synthetic distinction and a verificationist theory of meaning. Both of these had been fundamental tenets of logical positivism (or logical empiricism, as it has also been called), and Quine has been seen as an archcritic of this philosophical movement, one whose criticisms have contributed significantly to its demise during the second half of the twentieth century.[1] And while logical positivism waned, Quine's philosophy waxed and gained ascendancy.[2]

This view of Quine and logical positivism – correct, as far as it goes – leaves out of account the significant fact that contact with members of the Vienna Circle, the chief begetters of logical positivism, and especially with its leading figure, Rudolf Carnap, was crucial for Quine in the early years of his philosophical development and that Carnap's ideas and some ideas of other positivists, notably Otto Neurath, remained important to him throughout his philosophical life. This fact Quine himself insisted on and prominently acknowledged. *Word and Object*, Quine's most important statement of his philosophy, bears the dedication, "To Rudolf Carnap, Teacher and Friend," and Quine chose a passage from Neurath as one of the two epigraphs for that book.[3] In "On Carnap's Views on Ontology," a paper Quine wrote and published at the same time as "Two Dogmas," he declares, even while addressing philosophical differences between himself and Carnap, that "no one has influenced my philosophical thought more than Carnap" (OCVO 203). At the time of Carnap's death, in 1970, Quine wrote of him,

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In later years his views went on evolving and so did mine, in divergent ways. But even where we disagreed he was still setting the theme; the line of my thought was largely determined by problems that I felt his position presented. (HRC 41)

And long after Carnap's death, when neither expressing gratitude to the living nor eulogizing the recently dead could be his purpose, Quine, in the final paragraph of a paper expressing major criticisms of Carnap, declared, "I, like many, have been influenced more by him than by any other philosopher" (CPT 333).

Besides the history of Quine's philosophical development and the testimony of his acknowledgments to Carnap, there are striking indications, internal to Quine's philosophy, of affinities to logical positivism. Crucially there is Quine's own adherence to verificationism – of course, not in the form rejected by him in "Two Dogmas," by which "the meaning of a statement is the method of empirically confirming or infirming it" (TDEa 37). What was wrong with the Vienna Circle's verificationism was not the role it assigned to verification but the unit of language to which verification was taken to apply. "Our statements about the external world face the tribunal of sense experience not individually but only as a corporate body" (TDEa 41). For Quine, "meaning remains centered as always on verification" (EN 89). Quine's diagnosis of where the Vienna Circle went wrong in their verificationism is that they "espoused a verification theory of meaning but did not take it seriously enough" (EN 80).

Quine's espousal of a verificationism in which whole theories rather than individual statements are the units of verification and so of meaning leads to his doctrine of the indeterminacy of translation. According to this doctrine, any translation of a theory expressed in one language into an expression of it in another language

will be as correct as any other, so long as the net empirical implications of the theory as a whole are preserved in translation. But it is to be expected that many different ways of translating the component sentences, essentially different individually, would deliver the same empirical implications for the theory as a whole; deviations in the translation of one component sentence could be compensated for in the translation of another component sentence. Insofar, there can be no ground for saying which of two glaringly unlike translations of individual sentences is right. (EN 80)

For Quine, such indeterminacy appears to be "inescapable" when "we take a verification theory of meaning seriously" (EN 80). Quine recognizes that this doctrine of indeterminacy upsets our preconceptions about language:

Should the unwelcomeness of the conclusion persuade us to abandon the verification theory of meaning? Certainly not. The sort of meaning that is basic to translation, and to the learning of one's own language, is necessarily empirical meaning and nothing more.[4] (EN 81)

It seems natural, then, despite Quine's rejection of what were central doctrines of logical positivism in the 1930s, to see him as working within rather than against the empiricist project of logical positivism. A. J. Ayer (1959), in the introduction to his anthology *Logical Positivism*, declared,

In the United States a number of philosophers like Quine, Nagel and Nelson Goodman conduct logical analysis in a systematic scientific spirit that is probably closer to the original ideal of the Vienna Circle than anything that is now to be met with elsewhere. (pp. 7–8)

And Quine's long-time colleague Hilary Putnam (1990) hailed Quine as "The Greatest Logical Positivist" (in an article for which this accolade served as title):

Quine is often thought to have destroyed logical positivism, with his rejection of the analytic-synthetic distinction and his likening of philosophy to natural science rather than to pure logic, and indeed, a generation of young "scientific realist" philosophers has been inspired by him to denounce logical positivism root and branch. But reading these essays, I must say that I am inclined to class Quine as the last and greatest of the logical positivists, in spite of his criticisms of the movement. (p. 269)

These views of the relation between Quine and logical positivism are in keeping with a characteristic of logical positivism itself, namely, that its adherents held no single doctrine sacrosanct. Joergen Joergensen (1951), a Scandinavian associate of the Vienna Circle, writing its history, declared that "what unites its members is . . . not so much definite views or dogmas as definite tendencies and endeavors. An evidence of this is the often considerable divergence and lively discussion between its members and the amendments in the fundamental views that have occurred several times in the course

of its development" (p. 1). Joergensen's claim is echoed by A. J. Ayer (1978), who in an interview with Bryan Magee, when asked what he now saw as logical positivism's main defects, replied, "I suppose the most important of the defects was that nearly all of it was false," but then went on to say that "it was true in spirit – the attitude was right" (p. 131).

Even so, this account of Quine's relationship to logical positivism requires refinement. It is correct to label Quine a positivist, but despite the fact that he pursued a philosophical enterprise in common with the logical positivists, and despite his lifelong engagement with logic, he should not be described as a *logical* positivist. The reason is that he rejects the logical positivists' understanding that logic (as arising out of, but going beyond, the work of Frege and Russell) establishes the analyticity of mathematical and logical truth and thereby accounts for mathematical and logical truth from an empiricist standpoint.

Nicola Abbagnano (1967), writing about positivism before logical positivism (i.e., positivism during the nineteenth century) states that

the characteristic theses of positivism are that science is the only valid knowledge and facts the only possible objects of knowledge; that philosophy does not possess a method different from science; and that the task of philosophy is to find the general principles common to all the sciences and to use these principles as guides to human conduct and as a basis of social organization. (p. 414)

The last element of this characterization does not apply to Quine, whose interest in (naturalized) epistemology did not extend to using its principles as "guides to human conduct and as a basis of social organization," but otherwise it is as true of Quine as it is of Comte and Mill. It does not characterize the logical positivists, giving no special role to logic as a tool of philosophy.

2. LOGICAL POSITIVISM

The designation 'logical positivism' was introduced to the English-speaking world in 1931 by Albert Blumberg and Herbert Feigl (1931) in an article whose purpose was to expound to an American audience "a new movement in European philosophy" arising from "the convergence of two significant traditions: the positivistic-empirical and

the logical"[5] (p. 281). Empiricism, the doctrine that all knowledge depends ultimately on (sensory) experience,[6] is the taproot of this development and reaches back to Aristotle.

Mathematics is the biggest stumbling block for empiricism, since mathematical knowledge is, on the face of it, obtainable by pure thought alone.[7] Following Michael Ayers, I want to distinguish between "concept-empiricism, according to which all our ideas ultimately derive from experience" and "the stronger, or at least different, view, knowledge-empiricism, according to which all propositional knowledge is empirical, ultimately based on sensory knowledge" (Ayers 1991, 14–5). Aristotle's empiricism is a form of concept-empiricism, and so is Locke's.

David Hume (1711–76) espoused knowledge-empiricism, with the restriction that it does not apply to the propositions of mathematics. He drew a distinction between "matters of fact" and "relations of ideas." Truths of mathematics are relations of ideas, and relations of ideas are known by pure thought:

All the objects of human reason or enquiry may naturally be divided into two kinds, to wit, *Relations of Ideas*, and *Matters of Fact*. Of the first kind are the sciences of Geometry, Algebra, and Arithmetic; and in short, every affirmation which is either intuitively or demonstratively certain. *That the square of the hypotenuse is equal to the square of the two sides*, is a proposition which expresses a relation between these figures. *That three times five is equal to the half of thirty*, expresses a relation between these numbers. Propositions of this kind are discoverable by the mere operation of thought, without dependence on what is anywhere existent in the universe. (Hume [1777] 1975, 25)

As to matters of fact, some are known immediately from "the present testimony of our senses, or the records of our memory" (p. 26), but clearly many matters of fact are not immediately determined by observation (present or remembered). Hume raises the question what kind of evidence we have for matters of fact not established immediately by observation:

It may, therefore, be a subject worthy of curiosity, to enquire what is the nature of that evidence which assures us of any real existence and matter of fact, beyond the present testimony of our senses, or the records of our memory. (p. 26)

(He notes that the sort of philosophy that can deal with this question "has been little cultivated, either by the ancients or moderns.") Addressing this question, Hume argues, "By means of the relation of *Cause and Effect* alone we can go beyond the evidence of our memory and senses." (Of course, by "the evidence of our memory and senses," Hume here means *immediate* evidence.) Thus we must "enquire how we arrive at the knowledge of cause and effect" (p. 27). This leads to the problem of induction (Hume's problem). A cause is not a thing that can be observed directly by the senses. Does the principle of induction establish causes? No, according to Hume. There is no basis in experience for the claim that it does. Custom, or habit, a constituent of human nature, is why we infer from particular experience in the past and present to claims about all future experience. Hume accepts, indeed insists on, the usefulness to us of this habit, but he emphatically denies that it has any justification: "I say then, that, even after we have experience of the operations of cause and effect, our conclusions from that experience are *not* founded on reasoning, or any process of the understanding" (p. 32).

Hume demands that all knowledge other than of relations of ideas be based ultimately only on sensory experience:

[T]hough our conclusions from experience carry us beyond our memory and senses, and assure us of matters of fact which happened in the most distant places and most remote ages, yet some fact must always be present to the senses or memory, from which we may first proceed in drawing these conclusions. (p. 45)

Hume was the first to propound so strictly an empiricist philosophy. His views strongly prefigure logical positivism, as the Vienna Circle recognized and declared. Hume was seen by them as initiating the development of empiricism and positivism further pursued by Auguste Comte (1798–1857), John Stuart Mill (1806–73), Richard Avenarius (1843–96), and Ernst Mach (1838–1916).[8] The Vienna Circle's transformation of positivism into logical positivism was effected through an understanding of logic and mathematics inspired by the work of Frege, Russell, and Wittgenstein.

Corresponding to Hume's distinction between relations of ideas and matters of fact, Kant distinguished between analytic and synthetic judgments (1929, A6–7/B10). For Kant, "Judgments of experience, as such, are one and all synthetic" (B11). He also held that

"[M]athematics gives us a shining example of how far, independently of experience, we can progress in *a priori* knowledge" (A4/B8), that is, knowledge obtained independently of experience. Thus far, Kant's account is compatible with Hume's, but Kant powerfully challenged empiricism by his further doctrine that while mathematical knowledge is a priori, it is synthetic, that is, not merely determined by relations between concepts: "All mathematical judgments, without exception, are synthetic" (A10/B14).

It was Auguste Comte who introduced the terms 'positive philosophy' and 'positivism'[9] as labels for an empiricist philosophy based on a conception of science founded strictly on observation:

[T]he first characteristic of the Positive Philosophy is that it regards all phenomena as subjected to invariable natural *Laws*. Our business is, – seeing how vain is any research into what are called *Causes*, whether first or final, – to pursue an accurate discovery of these Laws, . . . to analyse accurately the circumstances of phenomena, and to connect them by the natural relations of succession and resemblance. (Comte [1853] 1974, 28)

(Compare this statement of Comte's with this declaration of Quine's: "As an empiricist I continue to think of the conceptual scheme of science as a tool, ultimately, for predicting future experience in the light of past experience" [TDEa 44].)

Comte's positive philosophy is structured by his "hierarchy of the positive sciences" (chap. 2 of the introduction to his *Cours de Philosophie Positive*). Comte held that there are six "fundamental sciences" (p. 43): mathematics, astronomy, physics, chemistry, physiology, and social physics (p. 50). (The last of these he later called sociology, and he is generally credited as the originator of this discipline.) The hierarchical order between these fundamental sciences is determined by their "successive dependence" (p. 44). Comte's *Cours de Philosophie Positive* consists of six books devoted successively to each of the fundamental sciences in their hierarchical order.

For our purposes, the significant point in Comte's ordering of the sciences is the place of mathematics in that ordering, and its nature. Mathematics is the most fundamental of all sciences, so fundamental as to make it more than just one among the sciences:

In the present state of our knowledge we must regard Mathematics less as a constituent part of natural philosophy than as having been, since the time of Descartes and Newton, the true basis of the whole of natural philosophy; though it is, exactly speaking, both the one and the other. (p. 49)

As to the nature of mathematics, Comte sees it as

divided into two great sciences, quite distinct from each other – Abstract Mathematics, or the Calculus (taking the word in it most extended sense), and Concrete Mathematics, which is composed of General Geometry and of Rational Mechanics. The Concrete part is necessarily founded on the Abstract, and it becomes in its turn the basis of all natural philosophy; all the phenomena of the universe being regarded, as far as possible, as geometrical or mechanical. (p. 50)

Abstract mathematics is based on "natural logic" (a notion Comte never spells out) and is purely instrumental:

The Abstract portion [of mathematics] is the only one which is purely instrumental, it being simply an immense extension of natural logic to a certain order of deductions. (p. 50)

Concrete mathematics, that is to say, geometry and mechanics, "must, on the contrary, be regarded as true natural sciences, founded, like all others, on observation." (p. 50)

Comte's division of mathematics into abstract and concrete evidently does not correspond to the usual distinction between pure and applied (most branches of mathematics, and certainly the calculus and geometry, are both pure and applied, depending on how they are being used and developed). It might almost be said that for those parts of mathematics that Comte labels as abstract, his position follows Hume and looks forward to Carnap, and for those parts that he sees as concrete, his views bear some affinity to the concept of mathematics as empirical developed soon after by Mill – and even more affinity to the views of Quine developed a century later.

Despite the centrality of mathematics for Comte's positive philosophy ("it is only through Mathematics that we can thoroughly understand what true science is" [p. 55]), it must be borne in mind that the driving force of Comte's philosophy was social and political, leading him to utopian schemes in which positive philosophy would supplant established religion, a development he considered to be well underway owing to the progress of eighteenth-century Enlightenment.

Comte's younger contemporary John Stuart Mill described himself as "long an ardent admirer of Comte's writings" ([1873] 1924, 178). Admiration led to correspondence, but eventually Comte espoused views that Mill abhorred, and they parted company. Comte and Mill

had always had philosophical differences, some of which were quite deep,[10] but the differences that led to their break concerned social philosophy. As Mill stated, "While as logicians we were nearly at one, as sociologists we could travel together no further" (p. 180). It was Mill's political distaste for the oppressiveness of Comte's eventual grandiose utopian conception of positive philosophy as not only supplanting religion but in effect becoming a new religion that led to a decisive break between them (see Mill [1873] 1924, pp. 179–80).

Mill, primarily in his great work *A System of Logic* (1843), propounded a form of knowledge-empiricism that was more far-reaching than Comte's and was directed against the Kantian doctrine that there can be a priori knowledge of substantive (i.e., synthetic) truths:

The German, or *a priori* view of human knowledge, and of the knowing faculties, is likely for some time longer (though it may be hoped in a diminishing degree) to predominate among those who occupy themselves with such inquiries, both here and on the Continent. But the 'System of Logic' supplies what was much wanted, a text-book of the opposite doctrine – that which derives all knowledge from experience. (Mill [1873] 1924, 190)

Mill sought to bring mathematics within the canon of inductively based scientific truth by the heroic expedient of denying that mathematical knowledge is a priori, even while accepting that the means by which further mathematical truths are obtained from existing mathematical truths is deduction.[11] Mill's defence of empiricism rests on his claim that the starting point for deductions in mathematics, namely, its axioms, consists of truths established from experience by the empirical method of induction:

The Science of Numbers is thus no exception to the conclusion we previously arrived at, that the processes even of deductive sciences are altogether inductive, and that their first principles are generalisations from experience. (Mill 1973, bk. 2, chap. 5, §2)

In common with Comte, Mill's espousal of empiricism was motivated by its implications (as he saw it) for political philosophy (this is true also of some, though not many, twentieth-century empiricists, most notably Neurath, but certainly not Quine). For Mill,

The notion that truths external to the mind may be known by intuition or consciousness, independently of observation and experience, is, I am

persuaded, in these times, the great intellectual support of false doctrines and bad institutions. (Mill [1873] 1924, 191)

Though Mill is a key figure in the development of empiricism, it should be noted that he did not apply to himself the label 'empiricist'. Rather, in the usage of political debate in Britain during this time, the term 'empirical' and its cognates designated positions he opposed. For example,

I saw that Macaulay's conception of the logic of politics was erroneous; that he stood up for the empirical mode of treating political phenomena against the philosophical.[12] (Mill [1873] 1924, 133)

For the Vienna Circle, the most important figure after Comte and Mill in the development leading to logical positivism was the Austrian physicist, psychologist, and philosopher Ernst Mach. Mach held that all knowledge rests on a phenomenal basis. The Vienna Circle also counted Mach's contemporary Richard Avenarius among its predecessors. Neither of these philosophers exerted any direct influence on Quine, so I won't discuss them further in this chapter.

Mill's attempt to uphold knowledge-empiricism by claiming that the axioms of mathematics are established by induction from sensory experience was decisively rejected by Gottlob Frege (1848–1925) in his *Foundations of Arithmetic*, published in 1884. Some of Frege's attack is pure derision, but overall his criticisms raised serious issues for any attempt to espouse empiricism with respect to the truths of mathematics. In particular, Frege noted that "Mill always confuses the applications that can be made of an arithmetical proposition, which often are physical and do presuppose observed facts, with the pure mathematical proposition itself" (1953, §9). At the same time, Frege offered his own quite stunning account of arithmetic, aiming to establish it as analytic by deriving it within pure logic from purely logical definitions. Frege took his logicist derivation of arithmetic to have refuted Kant's claim that all mathematics is synthetic. However, Frege's project was not to save empiricism from the Kantian synthetic a priori, and he held that Kant's conception of mathematics as synthetic and a priori, while wrong in the case of arithmetic, was correct for geometry.

Frege's claim to have shown that arithmetic is analytic foundered. In 1902, just as he was preparing to publish the second volume of his

Fundamental Laws of Arithmetic, which was to have provided a fully explicit formal derivation of arithmetic from logic, Bertrand Russell (1872–1970) wrote to Frege, informing him that his axiom system was inconsistent. Frege was devastated, and Russell devoted the next decade to finding a consistent system of logic that would allow the derivation of arithmetic and of mathematics more generally. The result was the three-volume *Principia Mathematica* (1910–13), written jointly with Alfred North Whitehead.

The system of *Principia Mathematica* is apparently consistent but makes use of two axioms that are difficult to defend as logically or analytically true, or even true at all, namely, the axiom of infinity and the axiom of reducibility. Even so, despite these vicissitudes, by the end of the first decade of the twentieth century, the possibility of establishing mathematics as analytic on the basis of the significant development of mathematical logic that had already occurred seemed highly promising. It also heightened the prospects for a type of empiricism that viewed all substantive knowledge, what Hume had called "matters of fact," as based ultimately on sensory experience and all knowledge of relations of ideas, most crucially mathematics, as based on logical definition and deduction. The philosophers who did most to achieve and espouse this position were a group based in Vienna who came to call themselves the Wiener Kreis, the Vienna Circle, with organized allies in Berlin and kindred souls in Poland and Scandinavia.

The beginnings of the Vienna Circle date from 1907, when three members of the faculty of the University of Vienna, the mathematician Hans Hahn, the economist Otto Neurath, and the physicist Philipp Frank, came together to discuss philosophy of science. In 1922, at the instigation of these three, Moritz Schlick was appointed Professor of the Philosophy of the Inductive Sciences at the University of Vienna, a post held earlier by Ernst Mach. A lively discussion group gathered around Schlick. In 1926, Rudolf Carnap came to Vienna as Privatdozent (the lowest rank in the German and Austrian academic system) from Jena, where he had written a doctoral thesis on space and also been influenced by Frege, whose lectures on logic he had attended. Carnap quickly established himself as the leading figure within the group.[13]

In 1928, members of the group established an organization, which they called the Verein Ernst Mach (Ernst Mach Society), for the purpose of "propagating and furthering a scientific outlook" and

"creating the intellectual instruments of modern empiricism." One way in which these goals were to be served was by publication of a monograph series, Veröffentlichungen des Vereines Ernst Mach, the first monograph of which, published in 1929, was dedicated to Moritz Schlick as a thank-offering for his decision to remain in Vienna despite a tempting call to a chair in Bonn. This publication took the form of a philosophical manifesto in which the Vienna Circle set out its "Scientific Conception of the World." The preface, signed on behalf of the Ernst Mach Society by Hans Hahn, Otto Neurath, and Rudolf Carnap, declared, "On this occasion [when Schlick decided to remain in Vienna], for the first time it became clear to him and us that there is such a thing as the 'Vienna Circle' of the scientific conception of the world, which goes on developing this mode of thought in a collaborative effort" (Verein Ernst Mach 1973, 299). (This brief account of what are sometimes called the first and second Vienna Circles[14] follows Passmore 1967, 52.)

Beyond Frege and Russell, much the most important contemporary influence on the Vienna Circle was Wittgenstein, first through his *Tractatus* and later through meetings between Wittgenstein and a few members of the Circle, initially including Carnap and Feigl but eventually just Schlick and Waismann. For an account of the impact of Wittgenstein on the Vienna Circle, see Carnap's (1963a) "Intellectual Autobiography" (pp. 24–9). For Carnap personally,

Wittgenstein was perhaps the philosopher who, besides Russell and Frege, had the greatest influence on my thinking. The most important insight I gained from his work was the conception that the truth of logical statements is based only on their logical structure and on the meaning of the terms. Logical statements are true under all conceivable circumstances; thus their truth is independent of the contingent facts of the world. On the other hand, it follows that these statements do not say anything about the world and thus have no factual content. (p. 25)

The Vienna Circle also attributed to Wittgenstein the principle of verification, that is, that the meaning of a statement is its method of verification. Moritz Schlick, in the first issue of the Circle's new journal, *Erkenntnis*, described Wittgenstein's *Tractatus* as "the decisive turning-point" in philosophy (1979a, 155).

The issues considered by the Vienna Circle changed over time, reflecting the progress of their thought as well as the personal diversity of the members. But overall the various issues they considered

belonged to one side or the other of the single coin of empiricism. On one side lay attempts to understand the ways in which knowledge of matters of fact depends on experience. Carnap's *The Logical Structure of the World* [1967] is the central text in this development. Also on this side of the coin is the debate between Carnap and Neurath in *Erkenntnis* (the journal established by the Vienna Circle in 1930) on protocol sentences (see Carnap [1987], Neurath [1983a], and Carnap [1937]). On the other side of the coin lay attempts to give an account of mathematics and logic compatible with empiricism, which meant showing that if knowledge of mathematics and logic does not depend on experience, then it is knowledge that is empty, or purely formal, in a sense to be made precise. The central text on this side is Carnap's *Logical Syntax of Language* [1934].

Note that in speaking of the two sides of the coin of empiricism, as I did just now, I was tacitly presuming the synthetic-analytic distinction. Quine questioned whether the distinction itself could be maintained on an empiricist basis. In so doing, he shifted the line of development within empiricist philosophy away from the separate pursuit of the two projects to which Carnap had successively contributed by his *Logical Construction of the World* and *Logical Syntax of Language*, onto a line of development concerned centrally with establishing what empiricism must take as its account of meaning.

The Vienna Circle and its allies had espoused verificationism, proclaiming that "The meaning of a proposition is the method of its verification." (for example, Schlick [1936], p. 458, and Reichenbach [1938], p. 49). This slogan was largely, in their hands, a polemical tool for dismissing metaphysical philosophy as meaningless, rather than the basis of a substantial investigation into the nature of meaning. I take it that this is at least part of what Quine was getting at when he said of the Vienna Circle that they "espoused a verification theory of meaning but did not take it seriously enough" (quoted above in §1).

Quine's demand for serious investigation of verificationist meaning is reflected, I think, in Carnap's later focus on semantics, following the period of his *Logical Syntax of Language*. If so, it shows that Quine was overlooking the impact he had on Carnap when he said, in tribute to his old teacher and friend, "[E]ven where we disagreed he was still setting the theme; the line of my thought was largely determined by problems that I felt his position presented" (quoted in §1).

Carnap and the other members of the Vienna Circle were convinced, following Frege, that Mill's attempt to establish mathematics as a species of empiricist knowledge, grounded by induction, was untenable:

The conception of mathematics as tautological in character, which is based on the investigations of Russell and Wittgenstein, is also held by the Vienna Circle. It is to be noted that this conception is opposed not only to apriorism and intuitionism, but also to the older empiricism (for instance of J. S. Mill), which tried to derive mathematics and logic in an experimental-inductive manner as it were. (Verein Ernst Mach 1973, 311)

That the viability of empiricism demanded a solution to this problem was clearly recognized within the circle, as shown in the following quotations. In 1930, Hahn asserted, "Only the elucidation of the place of logic and mathematics...(which is of very recent origin) made a consistent empiricism possible" (1980, 21). In the same year, Carnap gave this account of the importance for empiricism of establishing the analyticity of mathematics:

[E]mpiricism, the view that there is no synthetic *a priori* knowledge, has always found the greatest difficulty in interpreting mathematics, a difficulty which Mill did not succeed in overcoming. This difficulty is removed by the fact that mathematical sentences are neither empirical nor synthetic *a priori* but analytic. (1959, 143)

This view was echoed by Blumberg and Feigl in their 1931 paper:

Logic is *a priori* because it is analytic. Thus, the difficulties which the older empiricism and positivism encountered in attempting to account for logic and mathematics on an empirical basis disappear. Empiricism, the denial of synthetic judgments *a priori*, is now in a position to develop a theory of knowledge capable of doing full justice to logic and mathematics. (p. 285)

Carnap summed up this development in his "Intellectual Autobiography" as follows:

Thus we arrived at the conception that all valid statements of mathematics are analytic in the specific sense that they hold in all possible cases and therefore do not have any factual content.
What was important in this conception from our point of view was the fact that it became possible for the first time to combine the basic tenet of empiricism with a satisfactory explanation of the nature of logic and

mathematics. Previously, philosophers had only seen two alternative posi-
tions: either a non-empiricist conception, according to which knowledge in
mathematics is based on pure intuition or pure reason, or the view held,
e.g., by John Stuart Mill, that the theorems of logic and of mathematics are
just as much of an empirical nature as knowledge about observed events, a
view which, although it preserved empiricism, was certainly unsatisfactory.
(1963a, 47)

The central importance for empiricism of accounting for mathemat-
ics and logic and the possibilities for such an account were also ex-
pounded by Ayer (1936) in *Language, Truth and Logic* (pp. 72–5).

   Had logicism succeeded, that is, had the truths of mathematics
been shown to be derivable by pure logic from explicit definitions
in the language of pure logic (and not just for arithmetic, which
was Frege's goal, but for all of mathematics, including Euclidean
and non-Euclidean geometries), then mathematics would have been
shown to be analytic – so long as logical truth itself was analytic. By
1930, though, it was clear that no part of this program had succeeded
sufficiently to establish such a result. However, axiomatic founda-
tions for various branches of mathematics had succeeded very well,
starting with Hilbert's (1899) refinement of the axiomatic foundation
for geometry, and over the succeeding years axiomatic foundations
for other branches had been achieved, including set theory, which
in turn provided a foundation for essentially the rest of mathemat-
ics. Frege had balked (see Kluge 1971), but Hilbert's view largely
prevailed.

   The use of axiomatic foundations for the (philosophical) purposes
of empiricism required acceptance of a weaker notion of analytic-
ity, in which the definitions of the concepts are implicit rather than
explicit. Immediately after Carnap's "The Logicist Foundations of
Mathematics," in which he had done his best to champion the logi-
cism of his old teacher Frege, and under the impact of Gödel's incom-
pleteness theorems, Carnap embraced this weaker notion of analyt-
icity. For the Gödel result showed that no formalization of logic, of
the kind Frege had so brilliantly established in *Begriffschrift* (1879),
could deliver all truths of mathematics. Carnap espoused conven-
tionalism, formulated as a principle of tolerance in logical syntax:

It is not our business to set up prohibitions, but to arrive at conventions. . . . In
logic, there are no morals. Everyone is at liberty to build up his own logic,
i.e. his own form of language, as he wishes. All that is required of him is

that, if he wishes to discuss it, he must state his methods clearly, and give syntactical rules instead of philosophical arguments. (1937, 51)

Note that Carnap's notion of language is richer than the usual one and includes both formation rules, which constitute the grammar of the language, and transformation rules, the axioms of the system. The only constraint is pragmatic, what advances the science carried out in these languages.

On the account just given, Carnap was forced to his tolerant conventionalism by the need to uphold a weaker form of analyticity than that given by explicit definition. But there is overdetermination here, and the principle of tolerance chimes well with the Vienna Circle's antimetaphysical rejection of pseudoproblems in philosophy. The fruitless debates that had raged in the 1920s between Hilbert and Brouwer and their followers could not be rejected out of hand, since the leading protagonists were two of the greatest mathematicians of their time. But here, it seemed, was a way of resolving, or better, dissolving, the apparent issues between them. Whether tolerance does or does not achieve this result has been disputed, but Carnap and his followers were certainly convinced that it did and thus had a positive reason for holding to it.

Espousing tolerance was not by itself enough to save analyticity for mathematics in the face of Gödel's first incompleteness theorem. Carnap had also, in effect, to replace logical deduction by logical consequence. Tarski is generally credited with discovering the correct notion of logical consequence, but Carnap himself achieved that for two particular languages he investigated in *The Logical Syntax of Language*. What he did not achieve was a completely general account of this key notion that could be applied uniformly to a wide class of languages. Nonetheless, his accomplishment on this score is considerable. By his notion of rules of consequence, as opposed to rules of deduction, he arrived at truth definitions for his formal languages I and II (for a detailed assessment of the semantic content of *The Logical Syntax of Language*, see Coffa 1987, 285–326).

3. "rudolf carnap, teacher and friend"

In September 1932 Willard Van Orman Quine arrived in Vienna. Aged 24, he had completed his Ph.D. at Harvard the previous spring, in his second year as a graduate student, and had been awarded a one-year

travelling fellowship. He chose to start his fellowship year in Vienna, on the advice of two friends, Herbert Feigl, a young member of the Vienna Circle who had come to Harvard on a postdoctoral fellowship in 1931, and John Cooley, a fellow graduate student with Quine at Harvard, who had happened upon Carnap's *Logical Construction of the World*. (TL 86)

Quine's career as a logician-philosopher had begun in 1928 with an undergraduate thesis on *Principia Mathematica*[15] that he wrote at Oberlin College, essentially on his own.[16] He then went to Harvard as a graduate student in philosophy and wrote a doctoral dissertation on *Principia Mathematica*, a revision of which he published as his first book, *A System of Logistic* (1934). His supervisors for the Harvard Ph.D. were Alfred North Whitehead, co-author with Russell of *Principia Mathematica*, and C. I. Lewis. But Whitehead's interests had, in the fifteen years since the completion of *Principia Mathematica*, moved completely away from the problems he had worked on with Russell.[17] And Lewis, while a keen student of *Principia Mathematica* and an important figure for having established the axiomatic treatment of modal logic (see Lewis 1918 and Lewis and Langford 1932), was for this latter fact uncongenial to Quine, who was from his earliest days determined to reject all purely intensional notions from logic and philosophy.[18] As at Oberlin, Quine worked essentially on his own in writing his thesis. Quine's postdoctoral travelling fellowship brought him for the first time into contact with leading kindred spirits, the most important of whom, both for Quine and the wider world, was Carnap. The impact of these contacts is palpable in the title Quine gives to the section of his autobiographical essay in *The Philosophy of W. V. Quine* that describes these events: "Wiedergeburt in Mitteleuropa" ("Born Again in Central Europe") (A 12).

During that autumn in Vienna, Quine seems at first to have been philosophically somewhat isolated, though he put that time to good used by becoming fluent in German. Later that term, "after an overdue interview with Schlick, the philosophical complexion of Vienna improved," for Schlick told him of the Vienna Circle and invited Quine to attend its weekly meetings (TL 94).[19] Quine was even invited to lecture to the circle, which he did, in German, presenting material from his Ph.D. dissertation. Another visitor to the circle at this time was A. J. Ayer, two years Quine's junior and fresh from an

undergraduate degree at Oxford. Ayer became a committed partisan of logical positivism with the publication three years later of *Language, Truth and Logic*.

When Quine arrived in Vienna, he knew of Wittgenstein as an exalted figure in the philosophical firmament. In *The Time of My Life*, he quotes these lines from a letter he wrote to his parents soon after arriving in Vienna:

I have written a note to the great Wittgenstein. He now teaches in Cambridge, England, but . . . probably spends his vacations here in Vienna. I want an audience with the prophet. It remains to be seen whether he . . . will act on my request (for he doesn't know how nice I am). (TL 88)

Quine comments, "Of course he did not answer. But I had excused him in advance, without yet knowing his ways. I have never seen Wittgenstein." Quine doesn't sound too disappointed, and the impact of Wittgenstein on his philosophy seems largely to be mediated through Wittgenstein's impact on others, notably Carnap. It was Carnap who was for Quine "Vienna's main attraction" (TL 94), though, as it turned out, Carnap had moved from Vienna to Prague the year before Quine's visit.

After five months in Vienna, at the beginning of March 1933, Quine left for Prague to spend time with Carnap. In six weeks there he attended Carnap's lectures, read the manuscript of *Logische Syntax der Sprache* "as it issued from Ina Carnap's typewriter" (A 12), and had lengthy and frequent discussions with Carnap on philosophy and logic. Thirty-seven years later, at a memorial meeting for Carnap, Quine paid the following tribute to the importance of this contact in his intellectual development:

It was my first experience of sustained intellectual engagement with anyone of an older generation, let alone a great man. It was my first really considerable experience of being intellectually fired by a living teacher rather than by a dead book. I had not been aware of the lack. One goes on listening respectfully to one's elders, learning things, hearing things with varying degrees of approval and expecting as a matter of course to have to fall back on one's own resources and those of the library for the main motive power. One recognizes that his professor has his own work to do, and that the problems and approaches that appeal to him need not coincide in any very fruitful way with those that are exercising oneself. I could see myself in the professor's place, and I sought nothing different. I suppose most of us go through life

with no brighter view than this of the groves of Academe. So might I have done, but for the graciousness of Carnap.[20] (HRC 42).

After Prague, where he had "reaped philosophy" (A 13), Quine went on to imbibe logic in Warsaw, attending the seminars and lectures of Tarski, Leśniewski, and Łukasiewicz and meeting the many other logicians active at that time in Warsaw. At the end of his year in Europe, Quine returned to Harvard as a junior fellow of Harvard's newly founded Society of Fellows, which gave him three years of unfettered research.

The senior fellows of the society included members of the Harvard Corporation, which had governing responsibility for the university. Over lunches and dinners of the society, Quine instilled in the corporation's members such a sense of the importance of Carnap's philosophy that it was determined to award Carnap an honorary degree at the ceremonies marking the tercentenary of Harvard College, in 1936. In anticipation of Carnap's visit to Harvard for these ceremonies, Quine was asked to expound Carnap's philosophy, which he did in a series of three lectures given in November 1934 (and published by Creath in 1990).[21] Quine had lectured in Vienna and Warsaw on the *logical* results from his dissertation; these Harvard lectures were his first in philosophy. They loyally expounded Carnap's views, as set out in *Logische Syntax der Sprache*, which had been published earlier that summer (and would appear in English translation three years later).

When Quine's term as junior fellow ended, in 1936, he was appointed a faculty instructor at Harvard. Half the courses he taught were on mathematical logic (these gave rise to his "New Foundations for Mathematical Logic" and *Mathematical Logic*) (A 17). His other courses included "one on logical positivism, primarily on Carnap" (TL 130),[22] about which Quine comments, "It was generous of my senior colleagues to let me go so nearly my own way."

For Quine, Carnap was certainly the leading figure of logical positivism, though he also later stressed the importance of distinguishing Carnap from the movement. In "Carnap's Positivistic Travail," he writes,

The significance of the Vienna Circle, as a concerted movement, can be overestimated. We are told of the evolving doctrine of the Circle when what is really concerned is the doctrine of an individual, usually Carnap.... When one speaks of the Vienna Circle or logical positivism, one thinks primarily

of Carnap. We do better to think of him as Carnap, and to sort out the various influences upon him, Viennese and other, according to their merits." (CPT 325)

But as already noted (see n. 13), the Vienna Circle itself recognized Carnap as the member whose thought most influenced the philosophical development of the circle.

Quine has described himself as "very much Carnap's disciple for six years" (HRC 41), which is to say from when they first met, in 1933, to 1939 (Quine could not have been described as Carnap's disciple before they met). Yet also during this time Quine was finding his own way, and in 1936 he published his first purely philosophical paper, "Truth by Convention," which contains in embryo many of the elements most central to his philosophy as he went on to propound and develop it over the succeeding sixty years, in which disagreement with Carnap became an abiding theme. As Quine describes it, "The Three Lectures [on Carnap in 1934] were uncritical. 'Truth by Convention,' which I wrote shortly afterward for the Whitehead *Festschrift*, drew upon the lectures but showed already the beginnings of my misgivings over analyticity: the seeds of my apostasy" (A 16). Even so, Quine recalls that he had not seen it that way at the time: "I had not thought to look on my strictures over analyticity as the stuff of revolution. It was mere criticism, a negative point with no suggestion of a bright replacement. I had not felt moved to follow 'Truth by Convention' with more of the same" (TDR 267).

In a letter Carnap wrote to Quine from Prague dated March 15, 1936, he says, "I bought the book 'Essays for Whitehead' and I studied your paper ['Truth by Convention'] with very much interest. I am very keen of discussing it when we meet in April" (Creath 1990, 206). Referring to this passage, Quine remarks, "I have no record of his reaction, not remembering the discussion. There must have been frequent discussions, for he was around Harvard all that summer" (TDR 267).

Much of Quine's correspondence with Carnap dealt with practical matters, but in 1938 he wrote to Carnap to express concern at a shift in Carnap's thinking: "Next I proceed to inveigh against your recent intensional propensities, as reported by Hempel" (Creath 1990, 240). As Quine notes in "Two Dogmas in Retrospect," "The trend began when Tarski persuaded him that his 'Thesis of Syntax' was untenable: the thesis that 'philosophy is the syntax of the language of

science.' *Semantics* was wanted, not just syntax" (TDR 267). Quine's passionate opposition to (one might say intolerance of) the intensional leads him to insensitive overstatement: "I fear your principle of tolerance may finally lead you even to tolerate Hitler" (TDR 241). Carnap replies patiently to Quine's outburst in a letter dated February 11, 1938:

Your sermon against my sin of intensionality has made a great impression upon me. But I may say as an apology, I do not indulge in this vice generally and thoroughly. I used an intensional meta-language only for certain special purposes and I found it useful and even necessary for these purposes, namely for the investigation of the relation of translation between an extensional and an intensional language. It seems to me that certain interesting results are found in this way. Although we usually do not like to apply intensional languages, nevertheless I think we cannot help analyzing them. What would you think of an entomologist who refuses to investigate fleas and lice because he dislikes them? Now, for a syntactical analysis of an intensional language an extensional meta-language will do; but not for a semantical analysis. (Creath 1990, 245)

In "Two Dogmas in Retrospect," Quine quotes the pre-penultimate and penultimate sentences of this passage and comments sardonically, "Well, the fleas and lice proved addictive. By 1946 he was championing modal logic" (TDR 267).

The next important philosophical exchange between Quine and Carnap was in the fall and winter of 1940–1, when Carnap was at Harvard as a visiting professor. Tarski was also at Harvard that year on a "makeshift research appointment" (TL 149). Quine, Carnap, and Tarski met for regular discussions, which Quine described in these terms:

By way of providing structure for our discussions, Carnap proposed reading the manuscript of his *Introduction to Semantics* for criticism.

My misgivings over meaning had by this time issued in explicit doubts about the notion, crucial to Carnap's philosophy, of an *analytic* sentence: a sentence true purely by virtue of the meanings of its words. I voiced these doubts, joined by Tarski, before Carnap had finished reading us his first page. The controversy continued through subsequent sessions, without resolution and without progress in the reading of Carnap's manuscript. (TL 150)

(For an expression by Tarski of doubts he had about the notion of an analytic sentence around that time, see his letter to Morton White of

September 23, 1944, published by White [1987] as "A Philosophical Letter of Alfred Tarski.")

Carnap was not deterred from publishing his *Introduction to Semantics*, which appeared in 1942, and there was a substantial exchange of letters between Quine and Carnap over it during 1943. As Quine recalled,

I wrote him a long letter about his *Introduction to Semantics*. One issue was analyticity and another was my criterion of ontological commitment as applied to abstract objects. The two issues were linked, for Carnap viewed his appeal to abstract objects as empty convention, and their quasi-existence [as] analytic. (TDR 267)

The year after *Introduction to Semantics* appeared, Carnap published *Formalization of Logic*, and in 1947 he published *Meaning and Necessity*. (Carnap saw these three volumes as forming a single work, to which he gave the collective title *Studies in Semantics*.)

Carnap attempted to meet Quine's objections, both published and from their correspondence. In *Meaning and Necessity*, §44, he discussed Quine's "Notes on Existence and Necessity" (1943) and quoted extensively from comments Quine wrote to him on October 23, 1945, and January 1, 1946.[23] In 1950 Carnap published "Empiricism, Semantics, and Ontology," an important article in which he responded to Quine's rejection of any notion of meaning based on the notion of analyticity (or of analyticity based on meaning) so far as these notions are bound up with Quine's treatment of ontology, summed up in his famous dictum "To be is to be the value of a variable."

After the war, Quine began finally to see that his rejection of analyticity might indeed be "the stuff of revolution," and in 1947 he focused on the issue of analyticity in a triangular correspondence with Nelson Goodman (who had joined in some of the discussions of 1940–1 between Carnap, Tarski, and Quine) and Morton White (who had corresponded with Tarski during the war over this and related issues). Word of these developments got around, and in 1950 the program committee of the American Philosophical Association invited Quine to present a paper on this subject at the December meeting, held in Toronto. Quine's response was "Two Dogmas of Empiricism." "Truth by Convention" had made no reference to Carnap. "Two Dogmas" was forthrightly critical of him.

Five weeks after presenting "Two Dogmas" at the Toronto meeting, Quine gave a paper to Carnap's seminar at the University of Chicago[24] (part of which he published under the title "On Carnap's Views on Ontology"). In it, he again addresses his disagreements with Carnap:

[A]n issue has persisted between us for years over questions of ontology and analyticity. These questions prove to be interrelated; their interrelations come out especially clearly in Carnap's paper "Empiricism, semantics, and ontology." I shall devote particular attention to that one paper in an effort to isolate and reduce our divergences. (OCVO 203)

In "Empiricism, Semantics, and Ontology," Carnap had accepted Quine's notion that ontological commitment is carried by the variables of a given theory, but he did this in the context of a distinction between internal and external questions, which Quine rejected on the basis of his rejection of the analytic-synthetic distinction.

In 1952 Carnap published a short paper, "Meaning Postulates," in response to Quine's attack on his reliance on the notion of analytic truth. Also in 1952 he wrote a note replying to Quine's attack on his use of the notion of analyticity, which he left unpublished (Creath 1990, 427–32; see also p. x for Creath's remarks on this note). At this time a forum for a significant public reply to Quine on these issues was promised by Paul Arthur Schilpp's invitation to Carnap to be the subject of a Library of Living Philosophers volume. Schilpp issued invitations to contributors during 1953. Quine completed his contribution, "Carnap and Logical Truth," in time to meet Schilpp's deadline, in the spring of 1954. The volume was not published for another nine years. In 1956 Quine published a Carnap-free half of the essay under the title "Logical Truth," and in 1957 he published the whole of it in Italian translation in *Rivista de Filosofia*. The entire essay appeared in English in 1959 in an issue of *Synthese* honoring Carnap, and finally, in 1963, it appeared in the Schilpp volume, with Carnap's reply.

This was the first publication of Carnap's main reply to Quine's sustained critical discussion of his views in "Carnap and Logical Truth," though Carnap had already published a paper, "Meaning and Synonymy in Natural Languages" (1955), consisting of material that had outgrown his reply to Quine for the Schilpp volume (see Creath 1990, 37). Also during this period Carnap made one more attempt

at elucidation of analyticity, in his paper "Beobachtungssprache und theoretische Sprache" (1958), which was dedicated to Paul Bernays; English abstracts were published in 1959 and 1960. He published these last ideas on analyticity in English in *Philosophical Foundations of Physics* (Carnap 1966, chaps. 27 and 28). In his typically undogmatic way, he said of this material, "I offer it as a tentative solution to the problem of defining analyticity for the theoretical language. Earlier, although I did not share the pessimism of Quine and Hempel, I always admitted that it was a serious problem and that I could not see a satisfactory solution" (Carnap 1966, 273). He allowed himself cautious optimism that he had now found the needed solution to the problem of analyticity that so exercised Quine: "No difficulties have yet been discovered in this approach. I am now confident that there is a solution and that, if difficulties appear, it will be possible to overcome them" (p. 274).

Quine seems never to have responded to this final attempt by Carnap to vindicate analyticity, and the delayed publication of the Library of Living Philosophers volume on Carnap marked the end of the debate between Quine and Carnap rather than a stimulus to it. Carnap lived for another seven years, during which he devoted himself to attempting to complete his work on the logical foundations of probability (he had published vol. 1 of his *Logical Foundations of Probability* in 1950, then abandoned his plan for vol. 2 in favor of a collaborative enterprise, the results of which were published after Carnap's death by Richard Jeffrey; see Carnap and Jeffrey 1971).[25]

Given Carnap's importance for logical positivism, and for Quine, understanding Quine's relationship to logical positivism turns essentially on understanding Quine's relationship to Carnap. That relationship is complex for reasons some of which are deep and even intrinsic to the nature of what turned out to be, from Quines point of view, quite different conceptions of philosophy, some of which are accidental, to do with the two philosophers' varying interests over a period of nearly four decades (from when Quine first learned of Carnap's *Logical Construction of the World*, about 1932, to Carnap's death in 1970). During this time they variously influenced and understood each other while pursuing independent lines of thought. An indication of the complexity of their disagreement is its asymmetry. Quine saw the differences between himself and Carnap as fundamental; Carnap, on the other hand, recognized that there were

differences but saw them as bridgeable. As for the accidental reasons, the debate between Carnap and Quine is confused by the fact that Carnap was moving to embrace modal logic, which Quine abhorred (and, as noted, had done from before his contact with Carnap), at the same time that Quine was coming to realize that he had fundamental disagreements with Carnap's philosophy of logical syntax, which initially he had thought he accepted wholeheartedly.

This last point reflects a characteristic difference between Quine and Carnap. Quine is a systematic philosopher and his philosophy is nearly all of a piece. As noted, his first philosophical essay, "Truth by Convention," contains in germ many of the main elements of his later philosophy, and there is nothing in that paper he later abandoned or even significantly modified. Carnap's thinking, by contrast, while remaining true to some key aspirations, underwent more or less substantial shifts, if not of content then of emphasis. Quine engaged with three successive phases of Carnap's philosophical development: the logical construction of the world, the logical syntax of language, and semantics.[26]

What first attracted Quine to Carnap was Carnap's contribution, in his *Logical Structure of the World*, to the empiricist reduction of knowledge of the external world to sensory data. In the end, Carnap's reduction did not live up to Quine's or Carnap's hopes for it. As Quine describes the situation, "[T]he construction which Carnap outlined in *Der logische Aufbau der Welt* does not give translational reduction. . . . We must despair of any such reduction. Carnap had despaired of it by 1936, when, in 'Testability and meaning,' he introduced so-called *reduction forms* of a type weaker than definition" (EN 76–7). Quine's doctoral student, and later colleague, Nelson Goodman devoted his dissertation to the project, which gave rise to his book *The Structure of Appearance* (1951). Quine's own treatment focused on characterizing a class of observation sentences and analyzing their role in the edifice of what is known. Major treatments of it by Quine occur in *Word and Object*, "Epistemology Naturalized," *Roots of Reference*, and *From Stimulus to Science*. This work of Quine's carries on from debates in the early 1930s within the Vienna Circle, mostly between Carnap and Neurath, over protocol sentences.[27]

By the time Quine came into personal contact with Carnap, Carnap's attention had moved on from the problem of the sensory basis of empirical knowledge to the issue of how to show that all knowledge

known not on an ultimate basis of sensory experience, in particular mathematics and logic, is analytic. The result of Carnap's investigations was *The Logical Syntax of Language*. This second attempt to establish a logical positivism – that is, to use logic to uphold a modified form of positivism – had been necessitated by the realization that key axioms of *Principia Mathematica* could not be defended as logically true (see Carnap 1931) and by the impact of Gödel's incompleteness theorems. Carnap's solution was to employ a conventionalist notion of logic and analyticity, relativized to the choice of language according to his principle of logical tolerance, and it was also dependent on using a broader notion of logical consequence, going beyond formal deduction, to overcome the limitations on deductive systems revealed by the incompleteness theorems.

Quine was greatly taken by this means of upholding empiricism, though he soon had misgivings, which focused on the issue of whether the distinction between analytic and synthetic, on which it depended, could itself be drawn on an empiricist basis. It is a striking fact that for all Quine's initial enthusiasm for *The Logical Syntax of Language*, he never published anything about it that he later had occasion to retract. Thus the Harvard lectures, which expounded and endorsed logical syntax, were never published (by him), and his review of *The Logical Syntax of Language*, which he published in 1935, did not endorse those key claims of Carnap's. Here is a sentence from the first of the three lectures on Carnap: "All such sentences, in other words all mathematics and logic, become analytic: direct consequences of our definitions, or conventions as to the use of words" (Creath 1990, 60). And consider this ringing declaration of faith in the analytic-synthetic distinction:

When we adopt such a syntax, in which the *a priori* is confined to the analytic, every true proposition then falls into one of two classes: either it is a synthetic empirical proposition, belonging within one or another of the natural sciences, or it is an *a priori* analytic proposition, in which case it derives its validity from the conventional structure, or *syntax*, of the language itself – "syntax" being broadly enough construed to cover all linguistic conventions. (p. 66)

This passage leads immediately to an endorsement of Carnap's conception of philosophy as separate from science, another point Quine would later repudiate:

Syntax must therefore provide for everything outside the natural sciences themselves: hence syntax must provide not only for logic and mathematics but also for whatever is valid in philosophy itself, when philosophy is purged of ingredients proper to natural science. (p. 66)

Famously, the most visible difference between Quine and Carnap is their disagreement over the analytic-synthetic distinction. Something that confuses this difference is that Quine offers as his main argument against the analytic-synthetic distinction the holistic nature of knowledge: "[O]ur statements about the external world face the tribunal of sense experience not individually but only as a corporate body" (TDEa 41). This passage, to which is attached, in its reprinting in *From a Logical Point of View*, a footnote acknowledging Duhem, is among the most quoted from Quine. (In this chapter, I find myself quoting it twice and alluding to it at a number of other places.) Not so usually quoted is the run-up to this passage, in which Quine attributes this idea of holism to Carnap: "My countersuggestion, issuing essentially from Carnap's doctrine of the physical world in the *Aufbau*, is that . . . "

There are a number of oddities about this attribution. One is that it tacitly draws attention to an important difference between Quine and Carnap that Quine never addresses. In Quine's view, "[T]he two dogmas are, indeed, at root identical" (TDEa 41). But while Carnap is at one with Quine in rejecting sentential reductionism on the basis of holism, he cleaves firmly to the other dogma, the key role played by the analytic-synthetic distinction. Of course, 'cleaves to' is a referentially opaque context, so there is no logical contradiction between Quine's claimed identity and Carnap's differentiation. But it calls that identity into question. I won't pursue this particular issue further here, except to note that Quine's claim of identity between the "two dogmas" has been questioned (see Hofstadter 1954, 399–400; Pasch 1958, 11–23; and Isaacson 1974, 185–202).

Another oddity is why Quine cites the *Aufbau* (*The Logical Structure of the World*), where the doctrine of holism, insofar as it informs that work, is entirely tacit, rather than *The Logical Syntax of Language*, where the doctrine is formulated explicitly and precisely. Quine explains his imputation of holism to the Carnap of the *Aufbau*:

Already in his *Scheinprobleme*, 1928, there is a hint of the Duhemian holism, and that same year in the *Aufbau* the very mechanism of the Duhem effect is strikingly and imaginatively depicted. What I think of in the *Aufbau* is Carnap's account of the assigning of perceived colors to positions in physical space. Direction from the eye is determined directly by the positions of the color in the visual field, but distance from the eye is assigned in the light only of systematic considerations affecting all the assignments together. The guiding principle is the principle of least action: so choose the distances as to minimize the differences of color within short intervals of space and time. This is a very perceptive caricature of the role of simplicity considerations in scientific theory, and it is holistic. It is one of Carnap's deepest insights, and we can only regret that it did not play a fundamental role in his subsequent philosophy. (CPT 331)

But Duhemian holism did play a fundamental role in Carnap's subsequent philosophy, as is explicit in the following key passage from *The Logical Syntax of Language*:

There is in the strict sense no refutation (falsification) of an hypothesis; for even when it proves to be L-incompatible with certain protocol-sentences, there always exists the possibility of maintaining the hypothesis and renouncing acknowledgement of the protocol-sentences. Still less is there in the strict sense a complete confirmation (verification) of an hypothesis. When an increasing number of L-consequences of the hypothesis agree with the already acknowledged protocol-sentences, then the hypothesis is increasingly confirmed; there is accordingly only a gradually increasing, but never a final, confirmation. Further, it is, in general, impossible to test even a single hypothetical sentence. In the case of a single sentence of this kind, there are in general no suitable L-consequences of the form of protocol-sentences; hence for the deduction of sentences having the form of protocol-sentences the remaining hypotheses must also be used. Thus *the test applies, at bottom, not to a single hypothesis but to the whole system of physics as a system of hypotheses* (Duhem, Poincaré) [emphasis in the original].
    No rule of the physical language is definitive; all rules are laid down with the reservation that they may be altered as soon as it seems expedient to do so. This applies not only to the P-rules but also the L-rules, including those of mathematics. In this respect, there are only differences in degree; certain rules are more difficult to renounce than others. (Carnap 1937, 318)

There is every reason to suppose that Quine had read this passage, if not when it "issued from Ina Carnap's typewriter," while Carnap was composing the first draft of the book in 1932, then in 1935, when

Quine reviewed the book. But strangely, he seems to have retained no conscious memory of this clear statement of Duhemian holism as an element of Carnap's philosophy. In 1990, in a lecture marking the fortieth anniversary of "Two Dogmas," Quine declared,

In a footnote to "Two Dogmas" I noted Duhem's priority in stressing holism. As a matter of curiosity, however, I might mention that when I wrote and presented "Two Dogmas" here forty years ago, and published it in the *Philosophical Review*, I didn't know about Duhem. Both Hempel and Philipp Frank subsequently brought Duhem to my attention, so I inserted the footnote when "Two Dogmas" was reprinted in *From a Logical Point of View*. (TDR 269)

I conjecture that the selectivity of Quine's perception and memory of the role of the holism in Carnap's philosophy reflects the fact that Quine's holism is bound up with his rejection of the analytic-synthetic distinction (e.g., it is the basis of his impossibility argument in "Two Dogmas" as opposed to the weaker arguments there that fault attempted elucidations of analyticity). In the *Aufbau*, where the analytic-synthetic distinction is not invoked and is not at issue, what Quine perceives as Carnap's imaginative and perceptive depiction of the Duhem effect is thereby compatible with Quine's rejection of the analytic-synthetic distinction. In *The Logical Syntax of Language*, Carnap's explicit holism is bound up with invocation of the analytic-synthetic distinction. What is really at issue are opposed conceptions as to what constitutes (scientific) philosophy. And what obscures the debate between Quine and Carnap is that they seem never to have focused on this fundamental aspect of their differences.[28]

For Quine, philosophy is continuous with science, whereas for Carnap, it is *about* science and distinct from it. For Carnap, philosophy surveys possible forms of expression. Which form of expression we choose is pragmatically and holistically constrained by our experience of the world. But we are free to choose (that is the import of Carnap's principle of logical tolerance), and this freedom to choose means that language is conventional.[29] Analytic truths are those that are true just on the basis of whatever conventions have been chosen. For Quine, philosophy cannot stand apart from science, and we cannot choose the conventions that govern our languages of science. This is why Quine so likes Neurath's image of rebuilding

the boat while on the high seas, unable to put in to dry dock. Interestingly, Neurath was not expressing by this image Quine's conception of philosophy, and Neurath had no disagreement with Carnap over the analytic-synthetic distinction. Neurath's own distinction between "logic" and "behaviouristics," as in the following passage, is sharply at odds with Quine's rejection of the analytic-synthetic distinction:

The question which contradiction can just be tolerated, which not, how one behaves altogether in the development of the whole of science, is a question of behaviouristics, of history of science, of behaviouristics of scholars. But the discussion of contradictions, the discussion of the question, which groups of statements are logically of equal content, belongs to the sphere of logic. If I am occupied with the behaviour of people who produce encyclopaedias, I am concerned with behaviouristics; if I am occupied with the logical interconnecting of statements themselves, I am not concerned with behaviouristics. (Neurath 1983b, 169)

Interestingly, Paul Grice and Peter Strawson, not themselves adherents of logical positivism but pursuing philosophy as an investigation different in kind from scientific inquiry, mounted a defense of the analytic-synthetic distinction consonant with Carnap's:

Now for the doctrine that there is no statement which is in principle immune from revision, no statement which might not be given up in the face of experience. Acceptance of this doctrine is quite consistent with adherence to the distinction between analytic and synthetic statements. Only, the adherent of *this* distinction must insist on another; on the distinction between that kind of giving up which consists of merely admitting falsity, and that kind of giving up which involves changing or dropping a concept or set of concepts. (Grice and Strawson 1956, 156–7)

Particularly striking is what amounts to a defense (in my view, a very powerful one) of Carnap's method of logical syntax in their discussion of Quine's recognition that there is such a thing as "the explicitly conventional introduction of novel notation for purposes of sheer abbreviation" and his concession that in such cases a kind of synonymy is established:

Here the definiendum becomes synonymous with the definiens simply because it has been created expressly for the purpose of being synonymous with the definiens. Here we have a really transparent case of synonymy created

by definition; would that all species of synonymy were as intelligible. For the rest, definition rests on synonymy rather than explaining it. (TDEa 26)

Grice and Strawson (1956) comment trenchantly on this passage.[30]

Now if we are to take these words of Quine seriously, then his position *as a whole* is incoherent. It is like the position of a man to whom we are trying to explain, say, the idea of one thing fitting into another thing, or two things fitting together, and who says: "I can understand what it means to say that one thing fits into another, or that two things fit together, in the case where one was specially made to fit the other; but I cannot understand what it means to say this in any other case." (pp. 152–3)

Note that the force of this defense of meaning as something that can be conventionally stipulated does not turn on the reification of meanings, which need be no more thinglike than a usage.

Quine and Carnap both made occasional statements, in passing, of their respective conceptions of philosophy. Quine, in his paper "Natural Kinds" (dedicated to Carl G. Hempel), declared the following:

[M]y position is a naturalistic one; I see philosophy not as an *a priori* propaedeutic or groundwork for science, but as continuous with science. I see philosophy and science as in the same boat – a boat which, to revert to Neurath's figure as I so often do, we can rebuild only at sea while staying afloat in it. There is no external vantage point, no first philosophy. All scientific findings, all scientific conjectures that are at present plausible, are therefore in my view as welcome for use in philosophy as elsewhere. (NK 126–7)

And in "Epistemology Naturalized" we find the following:

We are after an understanding of science as an institution or process in the world, and we do not intend that understanding to be any better than the science which is its object. This attitude is indeed one that Neurath was already urging in Vienna Circle days, with his parable of the mariner who has to rebuild his boat while staying afloat in it. (EN 84)

In "Ontological Relativity" Quine again articulated a conception of philosophy as of a piece with science:

With Dewey I hold that knowledge, mind, and meaning are part of the same world that they have to do with, and that they are to be studied in the same empirical spirit that animates natural science. There is no place for a prior philosophy. (OR 26)

Quine saw the abandonment of Descartes' goal of a first philosophy as the final milestone in the development of empiricist philosophy:

In the past two centuries there have been five points where empiricism has taken a turn for the better. . . . The fifth is naturalism: abandonment of the goal of a first philosophy prior to natural science. (FME 67)

Here too Quine put himself in the same boat with Neurath:

The naturalistic philosopher begins his reasoning within the inherited world theory as a going concern. He tentatively believes all of it, but believes also that some unidentified portions are wrong. He tries to improve, clarify, and understand the system from within. He is the busy sailor adrift on Neurath's boat. (FME 72)

The naturalistic philosopher looks to science, rather than to first philosophy, in the quest for understanding reality. Quine indeed characterized naturalism as "the recognition that it is within science itself, and not in some prior philosophy, that reality is to be identified and described" (TPT 21).

   Quine has been quoted as declaring that "philosophy of science is philosophy enough."[31] This declaration sounds akin to Carnap's assertion in *The Logical Syntax of Language*: "Of the so-called philosophical problems, the only questions which have any meaning are those of the logic of science" (1937, 8). However, Carnap immediately glosses this view as follows: "To share this view is to *substitute logical syntax for philosophy*" (emphasis in the original). On this issue Quine (ultimately, though not then) parted company. Following is what Carnap meant by his claim that "the logic of science is syntax":

He who wishes to investigate the questions of the logic of science must, therefore, renounce the proud claims of a philosophy that sits enthroned above the special sciences, and must realize that he is working in exactly the same field as the scientific specialist, (1937, §86)

This sounds exactly like (later) Quine. But this passage continues in a way that leads to divergence:

only with a somewhat difference emphasis: his attention is directed more to the logical, formal, syntactical connections.

Further along Carnap expresses his commitment to conventional-ism, which, though tempered by pragmatism, is unacceptable to Quine:

The syntactical investigation of a system which is already given is indeed a purely mathematical task. But the language of science is not given to us in a syntactically established form; whoever desires to investigate it must accordingly take into consideration the language which is used in practice in the special sciences, and only lay down rules on the basis of this. In principle, certainly, a proposed new syntactical formulation of any particular point of the language of science is a convention, i.e. *a matter of free choice* [emphasis added]. But such a convention can only be useful and productive in practice if it has regard to the available empirical findings of scientific investigation. (p. 332)

Quine rejects the possibility of standing outside science, which he came to see as the impossible vantage point required for Carnap's project of logical syntax, of giving "syntactical rules rather than philosophical arguments" (Carnap 1937, 52). For Quine, "Scientific language is in any event a splinter of ordinary language, not a sub-stitute" (SLS 228).

The differences between Quine and Carnap turn ultimately on rejec-tion versus acceptance of the analytic-synthetic distinction. Tenabil-ity of the distinction licences the sharp division between philosophy and science. Rejection of the distinction, gradualism (PL 100), leads to Quine's view that philosophy is continuous with science. It also is constitutive of what empiricism itself is taken to be. Tenability of the distinction allows a disjunctive formulation: Knowledge of a sen-tence in a given language is either purely based on knowing that lan-guage or based ultimately on sensory experience. Quine's "more thor-ough" empiricism dispenses with the first disjunct.[32] Carnap's form of empiricism allows the possibility of establishing mathematics as analytic so that empiricism can account for mathematics and logic without having to claim that mathematical and logical truth rests ul-timately on sensory evidence. Untenability of the analytic-synthetic distinction means that mathematical and logical truth must rest ul-timately on sensory evidence, so that mathematics has empirical content.

Quine rejected the analytic-synthetic distinction because it could not, in his view, be established on an empiricist basis. For Quine,

being established on an empiricist basis means being grounded, ultimately, in the evidence of our senses. If the analytic-synthetic distinction cannot be established on an empiricist basis, then it is untenable, because philosophy does not stand separate from science. Science is grounded in the evidence of our senses, and Quine has arguments to show that the analytic-synthetic distinction cannot be so grounded. This is a tenable position. But the crucial question, in relation to the debate with Carnap, is whether it is a compelling position. I think it is not.

For Carnap, philosophy is the logic of science. Science determines what that logic should be, not normatively, but pragmatically, in the sense of what maximizes our chances for scientific success. This determination constitutes a favoring of one logic over another. What it is to be *a* logic is prior to any such determination. It is how we lay out the options for scientific language. Carnap's principle of logical tolerance enshrines the freedom we have to explore our options for scientific language. But the very basis of its operation is the analytic-synthetic distinction, by which we distinguish between science on the one hand and logical syntax (later, also semantics) on the other. Syntax and semantics constitute the conventions of scientific language, which we pragmatically choose. Philosophy, as the logical syntax (and semantics) of language, itself depends on the analytic-synthetic distinction. At the same time, this conception of philosophy validates the analytic-synthetic distinction: If philosophy is the logical syntax and semantics of language, then its subject is that part of our knowledge that is analytic.

What this analysis of Quine's and Carnap's positions shows is that neither can address the other *within* his own conception of philosophy. Their debate comes down, rather, to a take-it-or-leave-it standoff. The marker of their differing conceptions of philosophy is the analytic-synthetic distinction.

Despite the fact that within their positions there is no basis for winning one side over to the other, the impasse can be broken by testing each position by the criterion of success in accounting for mathematics, the hard case for empiricism. Quine considered that holism provided an account of mathematics as imbibing empirical content from the confirmed theories in which it figures. Carnap espoused holism, but not to this effect, and analyticity of mathematics is the cornerstone of his philosophy of mathematics. The tenability

of Quine's philosophy of mathematics would show in any case that the analytic-synthetic distinction was unnecessary for empiricism, thereby leaving those who would defend it with little to be gained from doing so.

For Carnap, mathematics was the fundamental issue for his attempt to uphold empiricism. Quine saw that this was so:

> I think Carnap's tenacity to analyticity was due largely to his philosophy of mathematics. One problem for him was the lack of empirical content: how could an empiricist accept mathematics as meaningful? Another problem was the *necessity* of mathematical truth. Analyticity was his answer to both. (TDR 269)

The case of mathematics was not central to Quine's formulation of empiricism, but he held that his empiricism could account for mathematics, as of course he realized it must. He followed the preceding characterization of Carnap's philosophy with this account of his own:

> I answer both with my moderate holism. Take the first problem: lack of content. Insofar as mathematics gets applied in natural sciences, I see it as sharing empirical content. Sentences of pure arithmetic and differential calculus contribute indispensably to the critical semantic mass of various clusters of scientific hypotheses, and so partake of the empirical content imbibed from the implied observation categoricals....
>
> What then about the other problem, that of the necessity of mathematical truth? This again is nicely cleared up by moderate holism, without the help of analyticity. For let us recall that when a cluster of sentences with critical semantic mass is refuted by an experiment, the crisis can be resolved by revoking one *or* another sentence of the cluster. We hope to choose in such a way as to optimize future progress. If one of the sentences is purely mathematical, we will not choose to revoke it; such a move would reverberate excessively through the rest of science. We are restrained by a maxim of minimum multilation. It is simply in this, I hold, that the necessity of mathematics lies: our determination to make revisions elsewhere instead. I make no deeper sense of necessity anywhere. (TDR 269–70)

Quine's philosophy of mathematics has to be gleaned from scattered remarks, such as those just quoted. No section, let alone chapter, of *Word and Object* is devoted to mathematics, and indeed there is no entry for mathematics in the index of this most central text for Quine's philosophy. No one of Quine's myriad papers is devoted to

the problem of giving an empiricist account of mathematics. Only in his penultimate book, *Pursuit of Truth*, does he devote an entire section to mathematics (§40, "Truth in Mathematics," one page long), and only in his final book, *From Stimulus to Science*, does he devote an entire chapter to it (chap. 5, "Logic and Mathematics," seven pages). (This first and last chapter on logic and mathematics itself raises a major issue, which we will touch on toward the end of the next section, namely, that Quine there seems to abandon the idea that mathematics has empirical content, which in earlier writings is accounted by Quine as an important point of disagreement with Carnap.)

4. QUINE'S POSITIVISM

When Quine arrived in Vienna in 1932, he was already publishing work in logic, but nothing of this gave expression to a recognizable philosophy. Even so, he was by this stage a determined empiricist and inclined toward behaviorism. Recalling his undergraduate studies, Quine writes, "My mathematics courses brought high marks but often imperfect understanding. I got more pleasure from Stetson's course in psychology, where we read Watson on behaviorism." (TL 59). But by Quine's account, his behaviorist proclivities predated this course:

The distrust of mentalistic semantics that found expression in "Two Dogmas" is thus detectable as far back as my senior year in college [when he wrote his thesis on *Principia Mathematica*]. Even earlier I had taken kindly to John B. Watson's *Psychology from the Standpoint of a Behaviorist*, which Raymond Stetson had assigned to us in his psychology class. Nor do I recall that it shocked any preconceptions. It chimed in with my predilections. (TDR 265–6)

Quine has described the philosophy he studied at Oberlin as "meager" (TL 82). At Harvard his study of philosophy was dictated by his determination to pass the preliminary examinations at the end of his first year, with the aim of completing his Ph.D. in two years. This meant devoting himself to the chore of historical courses, such as "Woods on Plato, Prall on Leibniz, and Lewis on Kant" (TL 82). We have already noted how little Quine was guided by his thesis supervisors at Harvard, A. N. Whitehead and C. I. Lewis. As at Oberlin,

his dissertation project was his own, both in its logic and its (implicit) philosophy. "Outwardly my dissertation was mathematical, but it was philosophical in conception; for it aspired, like *Principia*, to comprehend the foundations of logic and mathematics and hence of the abstract structure of all science" (TL 85). But more philosophically important, Quine's aim was to recast the work of Russell and Whitehead so as to dispense with intensional objects, the propositional functions whose identity conditions are so obscure. Russell had (strangely, we may think) sought to dispense with classes in favor of propositional functions. Quine did exactly the opposite.

Properties were vaguely assumed in *Principia* as further denizens of the universe, but they serve no good purpose that is not better served by classes, and moreover they lack a clear criterion of identity. Two sentences may be true of just the same objects and still not be viewed as ascribing the same property; they must be alike in *meaning*, and likeness of meaning eludes definition.

The notion of a property is one of various notions, called *intensional*, that depend thus on the nebulous notion of meaning. Other examples are necessity, possibility, and idioms of propositional attitude such as belief, hope, regret. My critique of meaning and intensions became more explicit down the years, but we see its beginnings in my dissertation, where I shunned properties. (TL 85–6)

Quine's undergraduate and doctoral theses both exhibit empiricist instincts for which there is no evident pedagogic source. Indeed, until his few months with Carnap in Prague and with Tarski et al. in Warsaw, Quine seems to have been, apart from compulsory study of the history of philosophy, entirely an autodidact in logic and philosophy.

In Vienna Quine found kindred spirits whose antimetaphysical standards of clarity and dedication to an empiricist understanding of the world chimed with his. It inclined him to imbibe a whole philosophy, in the form of Carnap's doctrine that philosophy is the logical syntax of language, where previously he had had only instincts. Within a few years, however, tensions developed between his own empiricist proclivities and the Vienna Circle's espousal of conventionalism and the analytic-synthetic distinction. There are clear indications of this shift already in his 1936 paper "Truth by Convention." In "Two Dogmas," Quine formulates explicitly his rejection of Carnap and the Vienna Circle. They are not sufficiently empiricist. He

makes this point in the final paragraph of "Two Dogmas," though the point is a little obscured by his use of the word 'pragmatism' instead of 'empiricism':

Carnap, Lewis, and others take a pragmatic stand on the question of choosing between language forms, scientific frameworks; but their pragmatism leaves off at the imagined boundary between the analytic and synthetic. In repudiating such a boundary I espouse a more thorough pragmatism.[33] (TDEb 46)

The next most significant development in Quine's philosophy from the point of view of propounding empiricism is his naturalized epistemology, according to which philosophy is continuous with science:

Philosophers have rightly despaired of translating everything into observational and logico-mathematical terms. . . . But I think that at this point it may be more useful to say rather that epistemology still goes on, though in a new setting and a clarified status. Epistemology, or something like it, simply falls into place as a chapter of psychology and hence of natural science. It studies a natural phenomenon, viz., a physical human subject. This human subject is accorded a certain experimentally controlled input – certain patterns of irradiation in assorted frequencies, for instance – and in the fullness of time the subject delivers as output a description of the three-dimensional external world and its history. The relation between the meager input and the torrential output is a relation that we are prompted to study for somewhat the same reasons that always prompted epistemology; namely, in order to see how evidence relates to theory, and in what ways one's theory of nature transcends any available evidence. (EN 82–3)

Quine sees this account as a "dislodging of epistemology from its old status of first philosophy" (EN 87).

In 1975 Quine presented a paper under the title "The Pragmatists' Place in Empiricism," part of which he then published in *Theories and Things* under the title "Five Milestones of Empiricism." He there claims, "In the past two centuries there have been five points where empiricism has taken a turn for the better" (FME 67). The fifth is

naturalism: abandonment of the goal of a first philosophy. It sees natural science as an inquiry into reality, fallible and corrigible but not answerable to any supra-scientific tribunal, and not in need of any justification beyond observation and the hypothetico-deductive method. . . . Naturalism does not repudiate epistemology, but assimilates it to empirical psychology. Science

itself tells us that our information about the world is limited to irritations of our surfaces, and then the epistemological question is in turn a question within science: the question how we human animals can have managed to arrive at science from such limited information. (FME 72)

Quine cites Russell as an eventual exponent of naturalism, which denies that philosophy is prior to science and sees it rather as science gone self-conscious. In a 1966 paper, "Russell's Ontological Development," he identifies in Russell "an increasing naturalism, an increasing readiness to see philosophy as natural science trained upon itself and permitted free use of scientific findings" (ROD 85). He goes on to quote the following passage from *Our Knowledge of the External World*:

There is not any superfine brand of knowledge obtainable by the philosophers, which can give us a standpoint from which to criticize the whole of the knowledge of daily life. The most that can be done is to examine and purify our common knowledge by an internal scrutiny, assuming the canons by which it has been obtained. (Russell 1914, 71; quoted on p. 85 of ROD)

Quine's empiricist rejection of the analytic-synthetic distinction takes him back to early positivism, as propounded by Auguste Comte. The turn to naturalism, so central to Quine's philosophy (cf. his "Epistemology Naturalized"), he traces back to Comte: "Naturalism had a representative already in the 1830s in the antimetaphysician Auguste Comte, who declared that 'positive philosophy' does not differ in method from the special sciences" (FME 72).

Hilary Putnam (1994) also traces positivism back to Comte, declaring that "Auguste Comte deserves the title of the father of positivism" (p. 295), though at the same time noting that "his positivism was in certain respects quite unlike any of the twentieth-century views that have associated themselves or been associated by others with that name." He goes on to say (and this assessment is in line with the acknowledgement of Comte by Quine just quoted), "Comte's lasting legacy and the source of his enormous influence on European and American thought is the particular concept of 'positive knowledge' that he bequeathed to us." As is the case with Quine, Comte's positivism led him to reject psychology as a science except insofar as it is based strictly on observation of behavior:

[T]he mind may observe all phenomena but its own . . . there can be nothing like scientific observation of the passions, except from without, as the stir of the emotions disturbs the observing faculties more or less. It is yet more out of the question to make an intellectual observation of intellectual processes. The observing and observed organ are here the same, and its action cannot be pure and natural. In order to observe, your intellect must pause from activity; yet it is this very activity that you want to observe. If you cannot effect the pause, you cannot observe: if you do effect it, there is nothing to observe. The results of such a method are in proportion to its absurdity. (Comte [1853] 1974, 33)

The members of the Vienna Circle likewise counted Comte among their predecessors. But they saw that to deal with the case of mathematics they had to go beyond Comte. How does Quine manage to stick with Comte's naturalism? It is in answering this question that we see how Quine's holism differs from that of Carnap and plays a different role for him. For Carnap, holism renders the question 'What is analytically true?' relative – relative to the choice of an entire language. As noted, that choice is, for Carnap, pragmatically constrained. But once made, it carries with it, *inter alia*, the determination of mathematical and logical truth. For Quine, there can be no such conventionalism, and mathematical truth is true, not by convention, but by its participation in science, which "faces the tribunal of experience" as a whole, mathematics included:

Mathematics and logic are supported by observation only in the indirect way that those aspects [i.e., the most general and systematic] of natural science are supported by observation; namely, as participating in an organized whole which, way up at its empirical edges, squares with observation. I am concerned to urge the empirical character of logic and mathematics no more than the unempirical character of theoretical physics; it is rather their kinship that I am urging, and a doctrine of gradualism. (PL 100)

This gradualism seems akin to Mill's account of mathematics, but gone holistic. Quine rejects Mill's claim that the axioms of mathematics are established as true by empirical induction, yet he seems here to be at one with Mill in holding that mathematics does have empirical content, through its participation in scientific theories that are confirmed by our experience of the world: "Mathematics does not imbibe its empirical content in the inductive way that John

Stuart Mill supposed, but it imbibes it in the hypothetico-deductive manner of theoretical science"[34] (CPT 333).

Is Quine's attempt to apportion empirical content to mathematics more successful than Mill's? This is large issue, not simply settled, but in brief I think that ultimately it is not. The confirmation of, say, Newtonian mechanics by celestial observation did not make the calculus more probable than it had been. Conversely, if a bit of incorrect mathematics enters into a theory later found lacking by the tribunal of experience (e.g., a mistake in arithmetic while putting up some shelves), the response will be to find the error in the mathematics. The failure of the shelves to fit in the intended space alerted us to the fact that we should check the mathematics – and by the criteria for correctness *particular to mathematics*. The mathematics itself is *sui generis* with respect to the theories of the world into which it so essentially enters.

But is it actually Quine's position that mathematics shares the empirical content of confirmed theories in which it is applied? A late formulation by Quine seems to show that this is not his position, or if it was, that he ultimately abandoned it. In his final book, *From Stimulus to Science*, Quine wrote as follows:

The accepted wisdom is that mathematics lacks empirical content. This is not contradicted by the participation of mathematics in implying the categoricals, for we saw (Chapter IV [p. 48]) that such participation does not confer empirical content. The content belongs to the implying set, and is unshared by its members. I do, then, accept the accepted wisdom. No mathematical sentence has empirical content, nor does any set of them. (FSS 53)

Also in keeping with the accepted wisdom, of mathematics as independent of sensory experience, is this passage: "I have stressed the kinship of mathematics to natural science, but there is no denying the difference. Pure mathematics has the advantage of being deducible from first principles without sensory disruption" (RM 416).

But in other late publications Quine continued to promulgate what appears to have been his earlier view of mathematics. Consider, for example, the following revision to his *Pursuit of Truth*:

[A]nalyticity served Carnap in his philosophy of mathematics, explaining how mathematics could be meaningful despite lacking empirical content, and why it is necessarily true. However, holism settles both questions

without appeal to analyticity. *Holism lets mathematics share empirical content where it is applied* [emphasis added], and it accounts for mathematical necessity by freedom of selection and the maxim of minimum mutilation. (PTb 55–6)

And here is what he says in his 1996 "Philosophical Self-portrait":

Mathematics, in so far as applied, is of a piece with natural science; for the applied mathematical sentences are in the block of sentences that jointly imply the [observation] categorical. Mathematics thus imbibes empirical content in so far as applied. (APSP 465–6)

   Quine was asked specifically to clarify his position on this issue. Roger Gibson, in his essay "Quine's Philosophy: A Brief Sketch" (1998), writes that "in his latest book, *From Stimulus to Science*, Quine seems to abandon the idea that even applied mathematics partakes of empirical content" (p. 677), and Gibson quotes the passage from that book extracted above. Gibson goes on to remark that "[t]his apparent conflict with Quine's *Pursuit of Truth* account might be merely terminological ('partake' v. 'confer') or it might reveal something more profound about Quine's conception of cognitive meaning and how his view differs from that of the logical positivists (or both)" (p. 678). The word 'partake' as cited by Gibson comes from a passage in "Two Dogmas in Restrospect" that I quoted earlier:

Sentences of pure arithmetic and differential calculus contribute indispensably to the critical semantic mass of various clusters of scientific hypotheses, and so partake of the empirical content imbibed from the implied observation categoricals. (TDR 269)

It is joined by the words 'share' and 'imbibes' in the late passages of Quine's earlier view that I have just quoted.
   Quine replied to Gibson's query as follows:

Gibson has found, to my chagrin but gratitude, a disagreement between my consecutive little books *Pursuit of Truth* and *From Stimulus to Science* regarding empirical content of mathematics. I rest with the later position, namely, that mathematics lacks empirical content. The point is that no set of mathematical truths implies any synthetic observation categoricals. (RGQ 685)

   This reply is very odd. If what he said in *From Stimulus to Science* expresses a new view, one that disagrees with his long-stated view of

mathematics, it seems remarkable that he was unaware in writing it that he had significantly shifted in his position regarding what, by the account I have developed here, is the central issue for empiricism (and which impelled the Vienna Circle to move from positivism to logical positivism). But this passage reads as if he was unaware that he had shifted his position until Gibson pointed it out to him. Also, if it is a new position, it seems strange not to identify the motivation for the shift by explaining why the earlier position had to be abandoned. But all he does is "rest with the later position." On the other hand, perhaps he misspoke himself in this reply, and where he said 'later position' should have said 'later formulation' (i.e., stated that what he had written in the passage in question is a better way to express a long-held position).

These two possibilities (that the new passage is just a better formulation of a long-held position vs. that it marks the radical abandonment of what seemed to be the point of real difference between himself and Carnap) are not dichotomous. There is also the possibility that the passage expresses a new position but one that is consonant with the main elements of Quine's philosophy overall. But clearly this is not the place to embark on a detailed investigation into Quine's account of mathematics.[35]

5. CONCLUSION

In considering the relation between Quine and logical positivism, we have naturally focused on the relation between Quine and Carnap. Insofar as Quine held that mathematics, when applied, has empirical content, there is a substantive philosophical difference between Quine's and Carnap's views, one that turns centrally on acceptance versus rejection of the analytic-synthetic distinction. Quine's philosophy remains true to the spirit of positivism, going back to its early progenitors, especially Comte, and differs in this important way from that of Carnap and the logical positivists.

If, as some of his late writings suggest, Quine's position was ultimately that mathematics has no empirical content, the difference between him and Carnap becomes much less clear. If Quine was prepared to agree with Carnap (and all logical positivists) that mathematics lacks empirical content, then his use of holism was not clearly different from Carnap's. The way may then be open to sustain

Carnap's view of their debate, that there was no unbridgeable gap between them.

NOTES

1. "There are no longer any logical positivists left" (Wisdom 1963, 335). "Logical positivism is dead, or as dead as a philosophical movement ever becomes" (Passmore 1967, 56). "[T]he criticisms brought against the logical empiricists' program, and against Carnap in particular, by Quine in the early 1950s are quite generally regarded as having been successful – not just in the historical sense (namely, that those criticisms were of great influence upon philosophical opinion, and may even be said to have initiated the decline of logical empiricism as a living enterprise), but in the substantive philosophical sense that this entirely favorable reception of Quine's criticisms was warranted" (Stein 1992,

275). "These anti-positivist doctrines [of Quine] undermine the Vienna Circle's conception of philosophy" (Hacker 1996, 195).

2. Stuart Hampshire (1971) refers to Quine as "the most distinguished living systematic philosopher" (p. 27), and G. H. von Wright (1993) writes that "of contemporary philosophers he is, in my opinion, the greatest" (p. 45).

3. "We are like sailors who must rebuild their ship on the open sea, never able to dismantle it in dry-dock and to reconstruct it there out of the best materials" (Neurath 1983a, 201). Quine quotes or cites this simile many times throughout his writings, e.g., IOH 79, WO 4, EN 84, NK 126–7, and FME 72.

4. "Epistemology Naturalized," which has furnished the last several quotations, was for Quine an occasion to reflect on the relationship of his philosophy to that of the Vienna Circle. He delivered it in Vienna (to a meeting of the World Congress of Philosophy) in 1968, his first return to that city after 1933. He says of that paper, "I indulged my mood of reminiscence, pitting naturalism against phenomenalism in a historical perspective" (TL 350).

5. According to Passmore (1967), "Logical positivism is the name given in 1931 by A. E. Blumberg and Herbert Feigl to a set of philosophical ideas put forward by the Vienna Circle" (p. 52). G. H. von Wright, in a 1993 essay entitled "Analytic Philosophy: A Historico-critical Survey," traces the term to Scandinavian visitors to the Vienna Circle: "The name was not an invention of the circle itself. Its origin seem[s] to be the titles of two books appearing in Scandinavia: *Der logistische Neupositivismus* by Eino Kaila (1930) and *Logistischer Positivismus* by Ake Petzäll (1931)" (p. 34). (Kaila was von Wright's teacher, whom he acknowledges equally with Wittgenstein as a major influence [p. 1].)

6. "The crucial insight of empiricism is that any evidence for science has its end points in the senses" (OME 225), and "whatever evidence there *is* for science *is* sensory evidence" (EN 75). Quine labels this one of "two cardinal tenets of empiricism," both of which he considers "unassailable" (the other is "that all inculcation of meanings of words must rest ultimately on sensory evidence" [EN 75]). So while Quine rejects "dogmas of empiricism," he embraces its unassailable cardinal tenets.

7. For many, this is part of the allure of mathematics, as Carnap recollected it was for him in his late teens. Writing of his student years in the gymnasium, he notes, "The subjects I liked most were mathematics, which attracted me by the exactness of its concepts and the possibility of proving results by mere thinking, and Latin with its rational structure" (Carnap 1963a, 3). (It may be that one can detect here the kernel of his later philosophy of mathematics.)

8. See the manifesto of the Vienna Circle (Verein Ernst Mach 1973, 304).

9. Schlick (1979b) writes that "Auguste Comte invented the term" 'positivism' (p. 259), though Abbagnano (1967) notes that the term had been used by Comte's mentor Henri Comte de Saint-Simon to designate scientific method and its extension to philosophy (p. 414). In any case, Comte was the first to use this label for a distinctive philosophy.

10. For example, Mill ([1865] 1993) considers it a "grave aberration in M. Comte's view of the method of positive science" that "he rejects totally, as an invalid process, psychological observation properly so called, or in other words, internal consciousness, at least as regards our intellectual operations. . . . Our knowledge of the human mind must, he thinks, be acquired by observing other people" (pp. 62–3). Mill objects that Comte does not show "how we are to observe other people's mental operations, or how interpret the signs of them without having learnt what the signs mean by knowledge of ourselves" (p. 63).

11. Mill draws a distinction between propositions that are "real" and those that are "purely verbal" (1973, bk. I, chap. VI, §4), and he declares, in a footnote there, that "This distinction corresponds to that which is drawn by Kant and other metaphysicians between what they term *analytic*, and *synthetic*, judgments; the former being those which can be evolved from the meaning of the terms used." *A System of Logic*, which contains Mill's most developed discussion of mathematics, has only a few other references to Kant, none of which are substantive, and in particular there is no reference to Kant's doctrine that mathematics is synthetic but a priori. So his views are contrary to Kant's, but Mill does not address Kant's arguments directly.

12. For more on this point, see Anschutz 1953, chap. 4, esp. pp. 73–7.

13. "The most decisive and rapid development of ideas began in 1926 when Carnap was called to the University of Vienna. His contributions to axiomatics and particularly his theory of the constitution of empirical concepts (as published in *Der logische Aufbau der Welt*) proved a very stimulating source of discussions. In the same year also, Ludwig Wittgenstein's *Tractatus Logico-Philosophicus* was studied by the Circle. The philosophical position of Logical Positivism in its original form was the outcome of these profoundly incisive influences. Though many of the basic ideas had already been enunciated in a general manner by Schlick, they were formulated more precisely, stated more fully and radically, by Carnap and Wittgenstein, quite independently. These two men exerted an enormous influence upon Schlick, who was about ten years their senior." (Herbert Feigl 1947, 407–8, quoted in Joergensen 1951, 2–3). Also, "Rudolf Carnap was the leading figure among the originators and the moving spirits of the stream of philosophical thought known as logical positivism or logical empiricism" (Hempel 1973, 253).

14. See Haller 1991.

15. "I had been fascinated by a symmetrical little formula in Couturat [*Algebra of Logic*, cited p. 59] having to do with ways of combining classes. By trial and error I found the general law of which that formula was a special case. The goal I set myself for my honors thesis was a proof of this law within the system of *Principia*. . . . The proof took eighteen pages of symbols. Three years later in Vienna I got the eighteen pages down to three for the *Journal of the London Mathematical Society* ["A Theorem in the Calculus of Classes," vol. 8 (1933): 89–95]" (TL 72–3).

16. "Nobody at Oberlin knew modern logic; however, the chairman of the mathematics department, William D. Cairns, made inquiries and got me the books. They were Venn's *Symbolic Logic*, Peano's *Formulaire de mathématiques*, Couturat's *Algebra of Logic*, Whitehead's *Introduction to Mathematics*, Keyser's *Mathematical Philosophy*, Russell's *Principles of Mathematics*, and the crowning glory, Whitehead and Russell's *Principia Mathematica*" (TL 59).

17. "I went to Whitehead's flat every two weeks to report my progress and problems. He would listen until I reached a point suited to a philosophical tangent on his part. The sessions impressed me but yielded little logic" (TL 84).

18. Intensional is opposed to extensional. A predicate is extensional if it is determined by the things to which it applies, extensional if not. An operation that applies to sentences is extensional if the outcome depends only on the truth-value(s) of the sentence(s) to which it applies – e.g., the operation that goes from sentences 'p' and 'q' to the sentence 'p if and only if q' as opposed to the operation that goes from sentences 'p' and 'q' to the sentence 'p means the same as q'. Similarly 'not p' as opposed to 'necessarily p' or 'so-and-so believes that p'. For a taste of Quine's strictures on modal logic, see TGMI.

19. This recollection by Quine seems odd, given that, also by his account, he had come to Vienna in part on the advice of Herbert Feigl, who had been a member of the Vienna Circle from its informed inception in 1924 (see Feigl 1974, 7), and that the circle had gone public, with publication of its manifesto, by the time Feigl met Quine at Harvard in 1930.

20. Quine republished this passage in both of his autobiographies (A 12–3 and TL 98).

21. There was a fourth lecture, entitled "Logical Positivism," which Quine delivered at Radcliffe College on December 17, 1934, but no complete text seems to have survived (Creath 1990, ix). The aim of this lecture was to make some of this material available to the female students of Radcliffe College, who at that time were taught separately from Harvard men.

22. Cf. Quine's letter to Carnap dated February 4, 1938, in which he reports, "Last term I gave a course on 'Logical Positivism', which is to say 'Carnap'" (Creath 1990, 239).
23. For the whole of Quine's notes, and the exchange of letters between Quine and Carnap over them, see Creath 1990, 387–97.
24. Following his invitation to participate in the Harvard Tercentenary Celebration in September 1936, Carnap was invited to teach at the University of Chicago for the winter quarter of 1936, which led to a permanent position there. Despite being considerably junior to Carnap both in age and academic rank, Quine played an important role in helping Carnap to leave Prague and establish himself in the United States, to which transition a great deal of their correspondence in those years is devoted (see Creath 1990).
25. One might think that this was a shift of interest, which would account for his not pursuing that old debate, though Creath (1990) speculates that "if [Carnap] had been able to provide the sort of theory that he sought, it would have been a powerful step forward even with respect to the analyticity debate, for it would have provided a detailed workable account of confirmation in which analyticity played a crucial role. Nothing carries conviction quite like a concrete example, so Carnap would have been in a strong position to demand that Quine provide an equally detailed and workable confirmation theory in which intensional notions such as analyticity did not appear" (p. 40).
26. Two other phases of Carnap's philosophy, not temporally distinct from the three just mentioned, did not engage Quine's attention. These were Carnap's work in philosophy of physics (to which belongs his doctoral thesis, on space and time; a late book, *Philosophical Foundations of Physics* [1966]; and some work on entropy, posthumously published), and his work on probability and inductive logic.
27. See Carnap (1934a), Neurath's (1983a) comments on this paper of Carnap's, and Carnap's (1987) reply to Neurath. It was Neurath who demanded that empiricism be restricted to physicalism. Note that the epigraph that likens us to sailors who must rebuild their ship on the open sea, which Quine uses for *Word and Object*, is taken from this paper of Neurath's (p. 92). Quine's notion of observation sentence, which he introduces in *Word and Object* (§10), addresses the philosophical issues for which the Vienna Circle had invoked the notion of protocol sentence (Quine makes no direct reference to the Vienna Circle's use of this notion but briefly invokes the notion of a "protocol language" [p. 2], which, for its entry in the index of the book, carries the advice, "See also, Observation sentence"). These issues continued to live in Quine's philosophy for the remaining four decades of his life. In particular, the

problem of how sensory experience, which is by its nature subjective, can provide a basis for objective science, persisted as an issue for Quine (it had also exercised Carnap and Neurath). Quine discusses the Vienna Circle debate on *Protokollsätze* in "Epistemology Naturalized" (p. 85). Some key texts for this later development are the following: Davidson 1974, VITD, Davidson 1990, TI, Bergström 1990, CB, PTa, and FSS. For an excellent brief account of Quine's notion of observation sentences from *Word and Object* onward, see Gibson 1998, 672–6. For an exhaustive discussion of the original debate between Neurath and Carnap over protocol sentences, with some discussion on its relation to Quine's subsequent philosophy, see Uebel 1992. It has been suggested, e.g., by Koppelberg (1990), that "there exists a close systematic affinity between the naturalistic attitudes and arguments of Quine and Neurath" (p. 204). Quine pronounces himself "impressed by the extent of agreement between Neurath and me that Koppelberg has found" (CK 212) but notes that "there was little scope for influence," as Neurath was in Moscow while he was in Vienna during 1932–3. I note also that Neurath's physicalism did not chime with Quine predilection for behaviorism in the manner of Watson: "Physicalism allows us to formulate more than one behaviouristics [actual methodologies of science] and can be in agreement with more than a single type of scientific psychology, as it is represented on the whole, for example by American 'behaviourism'. It is precisely the Vienna Circle that stresses that behaviourism as formulated by Watson seems too narrow from the standpoint of a pure special science and also contains all sorts of things that cannot withstand physicalist criticism" (Neurath 1983b, 164). Neurath, following Carnap, did embrace a holistic view of verification, at the level of theories rather than sentences: "However, as has already been shown by Duhem, Poincaré and others, we cannot say of isolated positive statements that they are 'valid'; this can be said only in connection with masses of statements to which these positive statements belong" (p. 161).

28. For perceptive discussion of the role of Carnap's and Quine's differing conceptions of philosophy in their philosophical disagreements, see George 2000 (the whole of that paper deals with this issue) and Hacker 1996, 195.

29. Note that the principle of logical tolerance, which Carnap adopts in his *Logical Syntax of Language*, remains a central principle in his later espousal of semantics: "The *principle of tolerance* (perhaps better called 'principle of conventionality'), as explained in §17 [of *The Logical Syntax of Language*], is still maintained. It states that the construction of a calculus and the choice of its particular features are a matter of convention"

(Carnap 1942, 247). For a good account of the continuity between Carnap's move to semantics and his earlier espousal of syntax, see Sarkar 1992, 220–3.

30. But see Orenstein 2002, 107–14, for cogent exposition and defence of Quine's rejection of the claim that logic and mathematics are true by convention and thus analytic.

31. Peter Strawson attributes this declaration to Quine in Strawson 1982 (p. 12) and Strawson 1990 (p. 310), on both occasions without reference. Robert Cummins also attributes this declaration to Quine and uses it as the epigraph of his book *Meaning and Mental Representation* (1990), also without reference. I have been unable to locate this sentence in any of Quine's publications, nor have a number of Quine experts I consulted. However, Strawson's (1990) paper is published with a comment by Quine (CS), and we may suppose that Quine would there have demurred from this imputation to him had he found it alien.

32. "The lore of our fathers is a fabric of sentences. . . . It is a pale grey lore, black with fact and white with convention. But I have found no substantial reasons for concluding that there are any quite black threads in it, or any white ones" (CLT 406).

33. In "Two Dogmas in Retrospect," Quine remarks of this passage that it "had unforeseen consequences. I suspect it is responsible for my being widely classified as a pragmatist. I don't object, except that I am not clear on what it takes to qualify as a pragmatist. I was merely taking the word from Carnap and handing it back: in whatever sense the framework for science is pragmatic, so is the rest of science" (TDR 272).

34. Cf. Gibson 1982, 174.

35. I plan to do this in another paper.

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10 Quine and Logic

I

No one since Russell has contributed so much to both philosophy and logic as Quine. No major philosopher has given anything like so much to logic, nor has any important figure in logic borne Quine's stature as a philosopher. His work in the two fields, though distinguishable, has been very much related. He has been a pioneer philosopher of mathematical logic, while he has integrated his view of logic into the very core of his epistemology. I will discuss how logic fits with the rest of Quine's philosophy and what motivated his choices as an expositor and teacher of logic. I will examine in particular aspects of his text *Methods of Logic*, now in its fourth edition.

Logic plays a central role in Quine's philosophy. Quine is preeminently an epistemologist. His epistemology is wedded in interlocking ways to empiricism, naturalism, and physicalism; together they lead him to see science not only as the arbiter of what is to be believed about the world but also as providing the context in which we must make our philosophy. Rather than pursue some first philosophy, we look to science for our bearings: "[I]t is within science . . . that reality is to be identified and described" (TPT 21). And "the basic structure of the language of science . . . is the predicate calculus: the logic of quantification and truth-functions" (FM 160).

To understand the consequences of scientific theories and the relationships among their constituents, we must be prepared to *regiment* scientific discourse into the idiom of first-order logic: "Implication . . . is the lifeblood of theories. . . . It is what relates a theory to its checkpoints" (FSS 51). Not that there is a unique translation

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into logical form, and not that alternative logics might not have been employed. But those other logics are seen by Quine either to be intertranslatable with standard logic, as with predicate functor logic, or else so mired in foreign notions as to block clear understanding, as with systems using modal operators. Quine has shunned the modalities for a host of reasons, many of these connected with his spurning of the analytic-synthetic distinction. Indeed he writes, "Avoidance of modalities is as strong a reason for an abstract ontology as I can well imagine," and avers that "my extensionalist scruples decidedly outweigh my nominalistic ones" (RP 397). So Quine has unwaveringly kept his logic and his ontology extensional, eschewing the notion of necessary truth and the machinery of possible worlds that has lured so many contemporary philosophers. It is quantificational logic that serves Quine as the framework for systematizing our scientific discourse and building our philosophy.

Quine sees regimentation as having further importance for the philosopher. It is a prerequisite for ontology; as Gibson summarized the point, only relative to regimented language do ontological questions make sense.[1] As all who know Quine's philosophy are aware, he takes ontological questions extremely seriously, not sloughing them off à la Carnap onto the pragmatic issue of choice of vernacular. Saving on assumptions is a major mandate for Quine, and ontological assumptions especially are to be minimized. A theory's ontology consists of all that needs admittance to the universe of discourse for the regimented pronouncements of the theory to be true. There are inevitably alternatives, owing to the inscrutability of reference and, more particularly, the Skolem-Löwenheim theorem. The theory's ontology may be varied provided its ideology – the construal of its predicates – is concomitantly altered. Indeed, "Structure is what matters to a theory, and not the choice of its objects" (TPT 20). Still, a theory's ontology is only broachable at all in terms of a regimented version of it.

Quine observes that pursuant to our regimentation we can apply formal devices to rid our official version of any singular terms – terms that purport to refer to objects – beyond variables. Definite descriptions are eliminable in Russell's way, and names may, at worst, be recast as descriptions. So we may arrive at a refinement of our regimentation that supports Quine's well-known slogan "To be is to be a value of a variable," his answer to Berkeley.

Quine's early work in logic and foundations grew out of his focus on *Principia Mathematica*.[2] He sharpened that work's apparatus and recommended improvements, ever with an eye to conceptual, notational, and ontological economies. This was what his dissertation and resultant first book were about. His work in set theory reached a high point with the system ML of his book *Mathematical Logic*. ML is the impredicative enlargement of an earlier Quine system, the NF of his paper "New Foundations for Mathematical Logic." In each the provision for set existence adopts a central idea of *Principia*, where paradox was avoided by requiring a set to have "higher type" than its members. ML's sets are just those of NF, but ML allows additionally for the existence of classes that are not sets, classes that do not themselves bear the membership relation. Sets are all classes, but not all classes are sets. So ML has a "two-sorted" universe. ML's extra level, allowing broader scope to its quantifiers, enables sharpened definitions and a development somewhat smoother than that of NF.

It was in ML that Quine placed his hope. Though born of the type-theoretic insight, it lacked type theory's ugly encumbrances; it was stark and elegant in its formulation. But ML has been plagued by problems, and it has not gained favor among set theorists. Early on, Rosser and Lyndon discovered an inconsistency in the system; Wang offered a repair that was in fact a gain in elegance as well. Deep problems with ML's ordinals became apparent. But worse, it was found that the status of the class N of natural numbers, of all things, was in question. You would want N to be a set; the development of standard mathematics demands it. Yet it was Rosser again who showed that set-hood of N is unprovable in ML, unless ML is inconsistent.

Quine's systems of set theory are seen by most as technical curiosities rather than as serious contenders for adoption. While ZF, the reigning theory, has thrived as the whole subject has flourished, ML and NF are charged with not capturing the basic intuitions or insights about the realm of sets as an iterative hierarchy. The current attitude is that formal adequacy of a set theory is not enough; reflection of how the realm is to be conceived is wanted as well. Quine, no champion of intuition in the foundations of mathematics, has had other goals: In Dreben's phrase, he has undertaken "syntactic exploration" in quest of an appealing apparatus strong enough to float standard mathematics while guarded enough to keep off the reefs of paradox.

Nonetheless, it should be noted that there are at most six set theories that are immediately recognizable to those who study the foundations of mathematics, and two of them are Quine's. Along with ZF, ML, and NF, there are von Neumann-Bernays-Gödel (VBG), Morse-Kelley (MK), and *Principia*'s type theory. ML and NF are of course related, but less closely than ZF and VBG and no more closely than ZF and MK. Actually NF is studied much more than ML now, largely with an eye to finding proof of consistency relative to some safer theory. There is no doubt that Quine's proposed theories have advanced the discussion of the subject even while they have turned out not to be fully viable.

The logic of the framework for science is quantificational logic, possibly with identity. This is where Quine marks the boundary between logic and mathematics; as soon as one enters into genuine set theory, one has crossed into the farther territory. Set theory is mathematics. Logic "has no objects it can call its own; its variables admit all values indiscriminately" (FSS 52). Set theory postulates specific sets, abstract objects determined by what bears them the membership relation. This typifies standard mathematics, with its numbers, functions, and topological spaces. Logic has at most identity as its own predicate, and Quine is sometimes grudging about allowing even it. Often he confines his attention to logic without identity, where he likes to point out that identity is definable in terms of a theory's other predicates if there are only finitely many of them. A compelling reason that Quine draws the boundary where he does is the fact that predicate logic has a complete proof procedure, one adequate to establishing validity wherever it lies. Set theory, like any part of mathematics into which number theory can be embedded, can have no such complete proof procedure; whatever our method of proof, there must, by Gödel's Incompleteness Theorem, be sentences that can be neither proved nor refuted by it – unless the method is so indiscriminate as to allow proof of everything.

Quine sees what is called second-order logic as lying on set theory's side of the border; in quantifying over functions and predicates, it assumes existence of subsets of and functions on the domain. And perhaps the assumptions are the more perilous for being made less overtly than in axiomatic set theory.

Quantificational logic for Quine is, of course, the classical kind. While acknowledging that he has found some attractive features in

the predicative proposals of Weyl and Wang, he speaks of himself as "never tempted to embrace constructivism at the cost of trading our crystalline bivalent logic for the fog of intuitionism" (RW 648). Of intuitionism he has also written that it muddies "the distinction between saying a sentence and talking about it" and that it "lacks the familiarity, the convenience, the simplicity, and the beauty of our logic" (PL 87).

What has caught Quine's eye with less disfavor is the intriguing mechanism of branching quantifiers. Is their exclusion from our logic an arbitrary matter? In "Existence and Quantification" (p.109), he attributes to Henkin consideration of sentences like this one:

(1)   Each thing bears P to something y and each thing bears Q to something w such that Ryw.

It turns out that the two symmetric ways of rendering this in standard quantificational form are not equivalent to each other, nor is either equivalent to the natural construal of the branched

$$(2) \qquad \begin{matrix} \forall x \exists y \\ \forall z \exists w \end{matrix} (Pxy . Qzw . Ryw),$$

a form recommended by Henkin to make (1)'s intended dependencies explicit.[3]

It has been debated whether English has sentences that are best cast in terms of branching quantifiers. Barwise analyzed a broad array of candidates and explored semantics for them.[4] One was Hintikka's favorite example: "Some relative of each villager and some relative of each townsman hate each other." Perhaps more seductive was Gabbay and Moravcsik's "Every man loves some woman (and) every sheep befriends some girl that belong to the same club." Barwise concluded that "branching quantification does occur naturally in English."

Henkin noted that there is a more conventional way of setting down a sentence like (1). It would be the conspicuously second-order

$$(3) \qquad \exists f \, \exists g \, \forall x \, \forall z (Pxfx . Qzgz . Rfxgz).$$

This sentence quantifies over functions; (2) did not but departed from the standard use of quantifiers. Might there be sufficient reason to extend logic to include such branched schemata as (2) or even such

second-order forms as (3)? Quine paused to examine this question in "Existence and Quantification." We will return to it later.

Mathematics is part of total science for Quine, and continuous with it. "Pure mathematics . . . is firmly imbedded as an integral part of our system of the world" (RP 400). As Dreben put it, it is not *sui generis* for Quine. Clearly Quine does not regard mathematics – or even logic, for that matter – as distinguished by its a priori necessity. A "score" on which logic and mathematics can be contrasted with the rest of science is "their versatility: their vocabulary pervades all branches of science" (RP 399). Yet, Quine continues, "Where I see a major discontinuity is not between mathematical theory and physical theory, but between terms that can be taught strictly by ostension and terms that cannot." Thus "the objects of pure mathematics and theoretical physics are epistemologically on a par. . . . Epistemologically the primary cleavage is between these on the one hand and observables on the other" (RP 402). Quine further insists that "all ascriptions of reality must come . . . from within one's theory of the world" (TPT 21) and acknowledges that "I see all objects as theoretical" (TPT 20). In particular, for him the likes of classes and numbers are reified because of their usefulness for scientific theory (see TPT 15).

In Quine's underappreciated book *Set Theory and Its Logic*, he takes great pains to show how much of what passes as set theory is translatable into quantificational logic and so devoid of commitment to sets. His device of virtual classes, dating back to his 1944 book *O Sentido da Nova Lógica*, allows us to "enjoy a good deal of the benefit of a class without its existing" (STL xii). The trick is a triclausal contextual definition that allows elimination of abstracts (expressions with form '$\{x: \ldots x \ldots \}$'), withholding imputations of reference to them except when they precede 'ϵ'. The device is implicit in other treatments of the subject, but since *Set Theory and Its Logic* it has been recognized increasingly as the best treatment of abstracts when the universe is limited to sets. Seeking both ontological and notational economies was always high among Quine's priorities.

Set Theory and Its Logic builds its development of set theory on an amazingly sparse basis. It adopts only a small core of axioms, mainly neutral between divergent theories. When a proof requires more sets than the meager assumptions provide for, their existence is taken as

a special hypothesis for the purpose at hand. Typical theorems thus have their own comprehension premises, letting us see clearly just what is required for their proof. Even the existence of infinite sets needs hypothesization, since the battery of adopted axioms does not deliver it. Quine has called the book's treatment "pedagogically my preferred approach to the subject," identifying his "expository objectives: clarity, elegance, and congenial philosophical perspective" (RW 645). The book's explicitness lies at the other end of the spectrum from the usual presentation of second-order logic, with its covert assumptions of existence.

The final third of *Set Theory and Its Logic* surveys and compares five set theories, all but MK of the theories mentioned together earlier. Quine makes comparisons in terms of predicativity, safety, and strength; issues of relative consistency are treated masterfully.

Quine wrote some twenty books, six of which are treatises entirely devoted to symbolic logic and set theory and another of which is a collection of logic papers culled from his enormous output of technical articles. There are scores of innovations and insights: an ingenious inversion trick for defining 'natural number', a definition of ordered pair on which everything is one, a penetrating analysis of ω-consistency, reduction of logic's basis to inclusion and abstraction, a host of proof procedures, and a rendering of Gödel's Incompleteness Theorem by way of concatenation theory. This only scratches the surface. Quine's contribution to the field is not to be measured in terms of proved theorems, proposed theories, or striking simplifications, however many there were of each. Much of what he taught us has come to be taken for granted, as he has been one of the dominant influences on students of logic for more than half a century. He helped to shape the field of mathematical logic as much as anyone. He showed us patiently and caringly how to see the subject, how to express ourselves in it, and how to connect it with the rest of what we would know.

II

Quine wrote that "the pedagogical motive has dominated my work in logic" (RW 644). He had found no suitable text for teaching formal logic in 1936, when he began his instructorship at Harvard. This started him on a "project . . . of pedagogical engineering" that led first to his *Elementary Logic* (1941) and subsequently to the four editions

of *Methods of Logic* (1950, 1959, 1972, and 1982), each after the first a considerable enlargement of the one before it. I have used all four editions of the text in what seem myriad offerings of my class in basic symbolic logic. I first learned the subject from Quine, studying its mimeographed predecessor.

Methods of Logic is like no other text in the subject. First of all, it is very appropriately titled. It serves up a veritable feast of methods, thoughtfully prepared for application and well garnished with their rationales. The book encompasses all that Quine sees as falling within logic's realm, and indeed somewhat more. *Methods* builds on no abstract system with formal rules. In it Quine gives us machinery ready to use and not bogged down in technicalities that serve no practical purpose. Still, the book is crisp, precise, and painstaking, imparting regard for rigorous thinking at every turn.

Early we find Quine shunning the distinction between a sentence letter and its double negation. Since this is classical logic, the two will cash out in the same way (although in chap. 13's axiom system, a brief departure, they need to be distinguished). Similarly, it is efficient to take both conjunction and alternation (Quine favors 'alternation' over 'disjunction') as capable of combining any finite plurality of components, not just two. Quine is eschewing fussiness that would slow application of the mechanism.

Since the symbolic expressions that are the heart of the subject are not part of a formal system, Quine does not call them formulas. He emphasizes that what they do is schematically represent the sentences that realize them; they serve as "logical diagrams," and he calls them *schemata*. Thus '*pvqvr*' is a schema, representing any threefold alternation. A schema is fleshed out by taking actual sentences, and later predicates, to supplant its schematic letters. We think of the letters as proxies. Quine's schemata are ready to use. They are symbolizations that might be given to the sentences, however complex, that they represent.

That view of logic's symbolic expressions lays bare what is always implicit in the subject's development: The logical truths are just those that realize valid schemata, or, turned around more usefully, a sentence is a logical truth just in case it has some correct symbolization that is valid.

For testing validity or consistency of truth-functional schemata, Quine favors his method of truth-value analysis. In addition to giving all the information of truth tables, this technique allows us to

combine cases when they fall together, thus shortening the work. It is geared to reaching its result with maximum efficiency, and it is hard to imagine anyone who has learned it retreating to truth tables. And there can be greater savings of effort even at this level. When the question is of implication between truth-functional schemata, it will sometimes be apparent that the verdict hinges on a single telltale assignment of truth-values. In this case Quine's stunningly quick "fell swoop" is better still than the "full sweep" of truth-value analysis.

In effect Quine gives another test of consistency and another of validity for truth-functional schemata. We are shown how to find both alternational and conjunctional normal equivalents; they are, respectively, (a) alternations of conjunctions of single letters and their negations and (b) conjunctions of alternations of the same. But the former is consistent if and only if it has a consistent alternant, which is readily settled by inspection; and the latter is valid if and only if each of its conjuncts is, again immediately discernible.

In addition, Quine includes techniques for simplifying truth-functional schemata that build on some of his own early research. He exhibits a sparse axiomatization due to Łukasiewicz that delivers as theorems exactly the valid schemata that are cast in terms of negation and the conditional. He disparages its use, partly because axiom systems for this decidable portion of logic provide only half a loaf, offering no verdicts of nonvalidity and so furnishing less information than full-fledged tests. There is even a look at the duality between conjunction and alternation, with some attendant laws. Though the text is mainly focused on application, Quine blends in enough metatheory to prepare the student who would continue in the field for what lies ahead.

As preamble to quantification theory, Quine pauses to lay out the venerable depictions of Venn diagrams and the more broadly applicable "algebra of monadic predicates or classes." For the latter he gives his elegant method of existential conditionals, which appeared in his 1954 article "Logic, Symbolic" in the *Encyclopedia Americana*. This amounts to a validity test for monadic quantificational schemata, those in which all predicates are 1-placed. It is adapted from a method that Quine used in a 1945 paper (see OLQ). The reflective student will realize that this machinery, with an algorithm that ultimately turns on truth-functional tests, is more akin

to truth-functional logic than to the full-blown logic of quantification of which it is a precursor. The method is another illustration of Quine's emphasis on efficiency of technique in an area's coverage.

Rules for the manipulation of quantifiers follow, together with reasons for them. Canons and examples of translation into the symbolic idiom are offered. There can be no complete method here, and students seem ever to want additional examples. In practice I have found it useful not only to proliferate illustrations of translation into logical notation, but also to emphasize translations from symbols back into English. Symbolic logic is a language, and students tend to need extensive drill to succeed in acquiring it.

Since no formal system is afoot, there is no need to distinguish between what are usually contrasted as consistency, a syntactic property, and satisfiability. Quine uses the former term exclusively in *Methods*, but he gives it the semantic sense. To be consistent is to be sometimes true. Ultimately he shows that his principal proof procedure is both sound and complete, whence the distinction lapses anyway. What cannot be shown to generate contradiction is just what has a satisfying interpretation.

That principal formal method for pure quantification theory is what Quine calls the main method, a variant of what is sometimes called semantic tableaux. It is one of several methods of the same strength included in the book. Three others, an extension of Skolem's method of functional normal forms, a variant due to Dreben, and Herbrand's method, are easily shown to be equal in power to the main method; the rest are somewhat further afield: a system of natural deduction for which Quine discusses strategies and two complete axiom systems outlined in passing. Quine's stress continues to be on the method most efficient for the purpose at hand. The present purpose is cast as that of establishing joint inconsistency of finite collections of quantificational schemata.

The main method is unsurpassed in both simplicity and applicability for this end.[5] First you need the notion of an *instance* of a quantification, of any schema with an initial quantifier that governs the whole schema. An instance is obtained when you drop that initial quantifier and supplant all the variable occurrences just freed by free occurrences of some one variable, called the instantial variable. I like to call it the pinch-hitting variable, though this has the peculiarity of allowing a variable – the one of the dropped quantifier – to

pinch-hit for itself. The result of just dropping that outermost quantifier will always pass muster as an instance.

Now the main method: Transform each of the schemata whose joint inconsistency is sought into prenex form, and do so in such a way that no variable free in one of them is quantified in another (thus forestalling interference with needed instantiations). Then take instances of schemata that you have and that you generate, subject only to one vital restriction. When you take an instance of an existential quantification, the pinch-hitting variable must not be free in any earlier line; for short, it must be new. So no moving from '∃xFx' and '∃x − Fx' to 'Fy' and '−Fy', for this requires 'y' to be new at two different junctures. A good thing, too, in light of the compatibility of the two quantifications. That is all you do, generate instances in accord with the restriction (which does not limit instantiations of universal quantifications). When and only when you assemble a truth-functionally inconsistent collection of unquantified lines have you succeeded in proving joint inconsistency of the original batch. And of course whether such a truth-functionally inconsistent set (call it a *tfi combo*) has been assembled is routinely checkable by any method suitable for testing truth-functional consistency. Thus the main method reduces inconsistency of quantificational schemata to something that can be verified.

But their consistency is not thereby so reduced, since for the initial batch to be consistent is for *no* tfi combo ever to accrue. In general, it cannot be inferred that because none has surfaced so far none can be obtained. Proofs of inconsistency, even for short schemata, can run as long as you wish. So the main method, though shown to be complete (adequate to proving inconsistency wherever it lurks), is no decision procedure, no algorithm always emitting a right answer. From Church, of course, we know that no such algorithm exists.

Since validity is just inconsistency of the negation, any complete method for showing inconsistency of quantificational schemata confers on us a complete method for establishing validity as well, but still without hope of an algorithm for the purpose. Given a quantificational schema we know that a complete proof procedure will allow verification of its validity if it is valid, yet will in general reveal nothing if it fails of validity.

Keeping our focus on validity, there are some natural subclasses of the quantificational schemata that do have validity tests. As

noted, we can determine the validity of monadic schemata by the method of existential conditionals. But a broader algorithm is presented in *Methods*, what Quine calls the method of pure existentials. It tests the validity of any prenex schema in which all the universal quantifiers precede any existential quantifiers; this is the Bernays–Schönfinkel class. And it effectively encompasses the monadic class: as Quine teaches, any monadic schema can be purified, whereby no quantifier is in any other's scope. Then in prenexing the purified equivalent, quantifiers can be drawn out in any order, and in particular can be so drawn out as to make the resultant schema qualify for the pure existentials method. Thus every monadic schema has a readily findable equivalent that is subject to the test.

Actually, these observations might seem to lead to paradox. Any prenex obtained from a pure schema by drawing out quantifiers in accord with the "rules of passage" will have the property that it is equivalent to any result of permuting its quantifiers. For they might equally well have been drawn out in any other order. To the hasty mind this might suggest that any prenex of a monadic schema maintains equivalence under permutation of its quantifiers. But what of

(4) $\forall y \exists x\, (Fx \longleftrightarrow Gy)$

and

(5) $\exists x \forall y\, (Fx \longleftrightarrow Gy)?$

It is easily seen that they are *not* equivalent. Of course what follows from what we saw above is simply that a monadic schema has *some* prenex equivalent with permutable quantifiers, not that all its prenex equivalents are such. In fact permutable prenexes for (4) and (5) are '$\forall y \forall v \exists x \exists u\, (Gy \rightarrow Fx \,.\, Fu \rightarrow Gv)$' and '$\forall y \forall v \exists x \exists u\, (Fx.\, Gy.\, V.\, -Fu.\, -Gv)$', respectively.

Like the monadic case, the Bernays–Schönfinkel class is finitely controllable.[6] For any schema in the class one can specify a pivotal-sized finite universe in which the schema, if not valid, must be falsifiable. For monadic, the size is 2^n, where n is the number of monadic predicates in the schema. For a schema '$\forall x_1 \cdots \forall x_n \exists y_1 \cdots \exists y_m B\,(x_1, \ldots, x_n, y_1, \ldots, y_m)$' the pivotal size is n plus the number of free variables (but at least 1). Why? Since initial universal quantifiers are "indifferent to validity", we can assume our given schema of this kind to be without free variables. For it not to be valid its negation,

equivalent to '$\exists x_1 \cdots \exists x_n \forall y_1 \cdots \forall y_m - B(x_1, \ldots, x_n, y_1, \ldots, y_m)$', must be satisfied in some nonempty universe. Therein there must be $a_1, \ldots, a_n$ for which '$-B(a_1, \ldots, a_n, y_1, \ldots, y_m)$' holds for *every* choice of $y, \ldots, y_n$.[7] But then if the universe were chopped down to just $a_1, \ldots, a_n$, that last schema would still hold for all remaining choices of $y_1, \ldots, y_m$, there being, if anything, fewer such choices than before. So the schema fails in the universe with just $a_1, \ldots, a_n$, which is what we sought to show. Had all quantifiers been universal a 1-element universe would have sufficed.

The pure existentials test itself is a simple truth-functional one: drop all initial universal quantifiers, obtaining a schema S all of whose quantifiers are existential; form the alternation of all the ways of substituting free variables of S for those governed by its existential quantifiers; and test that alternation for truth-functional validity. (If there are no free variables in S to substitute you just supply one of your own.) Now Quine never says it, but the alternation to be tested is precisely the *expansion* of S over a universe consisting of objects named by its free variables. Quine has observed in Chapter 22 that existential quantifications amount, in fixed finite universes, to such alternations. Fixing the universe as just indicated, what we have formed is the relevant expansion, the truth-functional articulation over that universe of what S says. Since the universe has the pivotal number of objects, we know that validity of the expansion assures validity of the schema. And even without that realization, we know that nonvalidity of the expansion guarantees nonvalidity of the original. For, as Quine's argument about (5') on page 184 illustrates, from a nonvalid expansion we can construct at once a falsifying interpretation for the quantification with which we began.

Quine's argument for the method's soundness turns not on the pivotal size of the expansion's universe, but on the fact that an existential quantification is implied by any of its instances. Every alternant of the expansion stands in the ancestral of the "instance of" relation to S, so validity of the expansion assures that of S, and in turn that of the original schema.

It is noteworthy that Quine says so little about interpretations that falsify (or satisfy) quantificational schemata. It is mainly in his chapters on metatheory that they are mentioned. On page 215 we find a satisfiable schema with no finite model, an "infinity schema", with the observation that if we could confirm this status for all schemata

that have it we would have a complete procedure for establishing consistency, and so even a test for it. We can confirm inconsistency, and we can confirm finite satisfiability. It is the third category that derails us.

Quine has made a decision about interpretations that reflects this theoretical consideration. There can be no complete method for generating satisfying (or falsifying) interpretations with assurance that they are such. *Methods* is a book that concentrates on complete methods; here there is none to offer. I think a good case can be built for making a different decision about interpretations. It has been my experience that one of the great aids to grasping what validity amounts to is the construction of models. It is, first of all, how we see nonvalidity of arguments that we map by Venn-type diagrams. We set down the information given by the premises and then find an allowable placement of objects that belies the purported conclusion. Or, the inequivalence of (4) and (5) above, to take a handy example, is most compellingly grasped by consideration of a two-element universe in which (4) holds and (5) fails. When the universe of an interpretation is finite, expansion serves as a canonical way of evaluating schemata. If there is one strand in the teaching of elementary logic that Quine might have pursued further it is, by my lights, this matter of looking at interpretations.

Quine observes that when, in a finite set of prenex schemata, no universal quantifier precedes any existential one, the main method provides a *test* of the set's inconsistency. Quine's argument for the main method's completeness makes this apparent: for this is the case where his "rigid routine" produces just a finite stock of instances. It is also, of course, precisely the situation in which pure existentials can be applied for the same purpose, to the negation of the conjunction of the schemata in the set.

Methods is loaded with other materials, more than can be squeezed into a semester-long course. There is metatheory, proof of the main method's soundness and completeness and, from the latter, a form of the Skolem-Löwenheim Theorem. It is shown that if systematic application of the method does not issue in any tfi combo, there will be an interpretation in a universe of positive integers that satisfies all the schemata in the entire train, including those in the initial collection. Indeed, the collection need not be finite here, so that a proof of quantificational compactness is also at hand.

Identity is added to the machinery late; axioms for it are specified that swell the main method to one sound and complete for the enlarged realm, quantification theory with identity. Neither soundness nor completeness of the augmented method is argued, though soundness requires just one step beyond the soundness result obtained for the main method, inasmuch as all the identity axioms are themselves valid.

Quine gives perspicuous treatment to definite descriptions and the means of eliminating them. As noted earlier, this lets us pare down the singular terms to just the variables. Function symbols are not broached; they are often reckoned among the paraphernalia of logic, but predicates and identity can be utilized to do their jobs. There is a foray into predicate functors and how they enable elimination of variables. Finally, there is just a sample, though a brisk one, of some set theory.

Branching quantifiers would not have been likely grist for *Methods*, even if Quine had seen them as belonging within the province of logic. But, largely because of shortcomings with regard to completeness, he does not see them as belonging there. The form (3) exhibits the simplest set of quantificational dependencies not readily expressible by a first-order schema.[8] The form is what Quine calls *functionally existential*: "all its function quantifiers are out in front and existential" (EQ 110). Now Quine observes that the Skolem method of functional normal forms is easily adapted to apply to such schemata, and is then a complete method for establishing their inconsistency. So far so good. Correspondingly, this gives a complete method for establishing *validity* – but now of *functionally universal* schemata, those with function quantifiers out front and universal. In general the negation of a schema in one of these newly dubbed classes has an equivalent not in that same class but in the other. The method cited, then, fails to supply the functionally existential class with a complete means of proving validity; nor does it offer a complete means of showing inconsistency for the functionally universal class. In fact a result of Ehrenfeucht shows that there *can* be no complete procedure for showing either of these.[9]

Strictly, Ehrenfeucht's argument was for functionally existential schemata in logic with identity. In affirming that the functionally existential schemata are not "covered by any proof procedure",

Quine thus added "at any rate when identity is included" (EQ 112). But the demurral is not needed, since a complete proof procedure for the class without identity would yield one for the class with identity; from any schema or sentence of the latter we can find, by standard construction, one of the former that is valid if and only if the original was.

So "classical, unsupplemented quantification theory is . . . maximal: it is as far out as you can go and still have complete coverage of validity and inconsistency by the Skolem proof procedure." Or, it is safe to say, by any proof procedure. That Quine draws his boundary for logic where he does is no accident. He continues, "Classical quantification theory enjoys an extraordinary combination of depth and simplicity, beauty and utility. It is bright within and bold in its boundaries"(EQ 111–113). Not only does logic serve the purpose of helping to moor Quine's epistemology; it is clear that his work in it has been a labor of love.

NOTES

1. Roger Gibson, Jr., *The Philosophy of W. V. Quine* (U. of South Florida, 1982), 109.
2. For more details on Quine's work in set theory see my "Quine and the Field of Mathematical Logic," in *The Philosophy of W. V. Quine*, ed. Hahn and Schilpp, Open Court, 1986 (expanded ed. 1998), 569–89.
3. See Leon Henkin, "Some Remarks on Infinitely Long Formulas," in *Infinitistic Methods*, Pergamon Press, 1961, especially pp. 181ff. The branched schema (2) and the two standard ones alluded to may be seen to be pairwise inequivalent by considering interpretations in a universe of just two members.
4. See Jon Barwise, "On Branching Quantifiers in English," *Journal of Philosophical Logic*, vol. 8, no. 1, Feb., 1979, pp. 47–80.
5. As is remarked later, application of the main method need not be limited to finite collections.
6. A class is finitely controllable if and only if any nonvalid schema in it is falsifiable in some finite universe. A study of such classes may be found in Dreben and Goldfarb, *The Decision Problem*, Addison-Wesley, 1979. For any finitely controllable class there is a validity test. The next natural finitely controllable class beyond the Bernays–Schönfinkel is the Ackermann class, consisting of all prenex schemata with at most

one existential quantifier. But any algorithm for it would have been too sophisticated for inclusion in *Methods.*

7. $a_1, \ldots, a_n$ need not be distinct, but in the absence of identity and function symbols they can safely be regarded as n different elements.

8.   See Henkin, *op. cit.*

9.   Ehrenfeucht's result is reported by Henkin, *ibid.*

11 Quine on Quine

Editor's note: Burton Dreben's unique contribution to this volume merits an explanation. Several years ago, when this volume was conceived, I consulted with Professor Quine about whom I might invite to write an essay about Quine on truth. He recommended Dreben; subsequently Professor Dreben accepted the invitation. Sadly, however, he died of lymphoma on July 11, 1999, at the age of 71, and without completing the essay on truth. Shortly before Burt's death, however, Professor Quine, Professor Ernest Lepore, and I were dinner guests at the home of Burt and his wife, Juliet Floyd, in Boston. Sometime that evening Burt handed me a folder containing his essay "Quine on Quine." Burt had written the essay for presentation at the Twentieth World Congress of Philosophy, which was held in Boston in the summer of 1998. The essay appears here in the exact form in which I received it. Burt was more than the world's leading expert on Quine's philosophy, he was Quine's favorite sounding board, and at times Quine's bulldog. He will be sorely missed.

W. V. Quine, who turned ninety on June 25, has been central to the analytic tradition in philosophy for more than sixty years. A sign of his centrality is that one of the three award-winning essays at the special APA session on American Philosophy from Other Perspectives – a session to which Quine must soon go in order to comment on the essay – is devoted to him and Carnap. A second paper is on John Rawls, Quine's Harvard colleague of many years, and who acknowledged Quine's influence on him in the preface to *A Theory of Justice*. I could think of no better way to give a perspective

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on analytic philosophy, the topic of this session, and to prepare us
for Quine's session with Donald Davidson starting at 4 o'clock this
afternoon, than to let Quine speak on Quine. I have woven a gar-
land from about 20 short papers, actually responses to works about
him, that Quine has written and sent off for publication in the past
two years. All except perhaps two of the sentences that follow are
Quine's. In particular, for the indexical word 'I', hear the proper name
'Quine'.

"What a speaker means by his words can be known by others." I
agree, but what I am accepting is no more than this: "What para-
phrases the speaker would be prepared to accept, in describable
circumstances, can be known by others." This is behaviorally ac-
ceptable, and my intention in mentioning meaning runs no deeper.
[K]nowing what expressions mean consists, for me, in being disposed
to use them on appropriate occasions. I view the learning of language
as acquisition of speech dispositions. There is nothing in linguistic
meaning beyond what is to be gleaned from overt behavior. [But] we
still have *dispositions* to observable behavior to work from. (There is
observable behavior and dispositions to observable behavior. "Each
disposition, in my view, is a physical state or mechanism" [RR 10].
"It is just one or another physical property . . . of the internal structure
or composition of the disposed object" [FSS 21].) [L]inguistic behav-
iorism can accommodate only intersubjective meaning. In support of
linguistic behaviorism, I expect no deductive argument. The doctrine
rests only on our observation of language acquisition and the empir-
ical implausibility of supplementary channels such as telepathy.

[L]inguistic behaviorism . . . disciplines data, not explanation. On
the explanatory side my readers are familiar rather with my recourse
to innate endowments. I cite instinct and hence natural selection to
explain induction, and to explain also our innate subjective standards
of perceptual similarity and their preestablished intersubjective har-
mony. All this is essential to language readiness. Behaviorism wel-
comes genetics, neurology, and innate endowments. It just excludes
mentalistic explanation. It defines mentalistic concepts rather, if at
all, by their observable manifestations in behavior.

[Some] adduce . . . samples of current linguistics under the mis-
conception that I, trammeled by behaviorism, underestimate the
translator. [They] misinterpret my conjecture of the indeterminacy
of translation. I postulate two ideal manuals of translation both of

which translate the alien language impeccably, and I conjecture that they may, even so, sustain incompatible translations of some alien sentences on highly theoretical matters. Both manuals cover the ground to perfection, but in partly incompatible ways. They have missed nothing; the indeterminacy is objective. The point of my conjecture was a challenge to synonymy and hence to the reification of meanings, notably propositions.

In *Word and Object* I based the conjecture on the cantilever character of the scaffold of analytical hypotheses that relates the theoretical reach of language to the linguist's evidence in verbal behavior. Conflict between the two manuals seems likely over one sentence or another on whose truth value the natives are open-minded. I see it not as a failure of translation, but as a commendable rounding out of translation beyond the bounds of actuality. It would be a case where there was no reality to uncover, but only a blank to fill.

Unlike the indeterminacy of reference, which has its simple and conclusive proof in proxy functions, the indeterminacy of translation was always a conjecture, albeit a plausible one. It is a dismissal neither of translation nor of meaning. I have questioned the reification of meanings, plural, as abstract entities, and this not on the score of their abstractness, but of their individuation; for there is no entity without identity. Seeing meaning as vested primarily in the sentence and only derivatively in the word, I sought in vain an operational line on sameness of sentential meaning by reflecting on the radical translation of sentences. My thought experiment in radical translation, in *Word and Object*, was meant as a challenge to the reality of propositions as meanings of cognitive sentences. [To repeat,] since there is no entity without identity, no reification without individuation, I needed only to challenge *sameness* of meaning of cognitive sentences. There are no meanings without sameness of meaning. [And] for pure sameness of meanings unsullied by shared origins of words or mutual influences of cultures, where better to look than in radical translation?

My conclusion was that the only overall test of a good manual of radical translation was fluent dialogue and successful negotiation, and my conjecture was that two manuals could pass muster and still conflict in translation of some sentences remote from observation and from social and commercial concerns. What was challenged was the philosophical notion of propositions, the meanings of sentences.

[Some see] my reservations about meanings as directed against meaning, and diagnose them as rooted in my physicalism. I demur on both points. I just sought a definition of sentential synonymy that one could in general see how to apply.

[In *Word and Object*, I wrote,] "[M]anuals for translating one language into another can be set up in divergent ways, all compatible with the totality of speech dispositions, yet incompatible with one another. In countless places they will diverge in giving, as their respective translations of a sentence of the one language, sentences of the other language which stand to each other in no plausible sort of equivalence however loose" (WO 27). My deprecatory "no sort" and "however loose" were by way of apology for my undefined term 'equivalence', sorely in need of definition. [A]ppeal to 'equivalence' begs the whole question about meanings. In answer [see *Pursuit of Truth*] I have suggested applying the two manuals alternately sentence by sentence to a text that each manual separately makes coherent sense of, and seeing if the result visibly bewilders English listeners. [W]here I appeal to the intuitive notions of coherence and interchangeability I sense nothing to apologize for. These are behaviorally recognizable in the observable reactions of native listeners to the translations. (By an intuitive account I mean one in which terms are used in habitual ways, without reflecting on how they might be defined or what presuppositions they might conceal [WO 36, n. 1].)

So I have not been subjecting translation to a behaviorist onslaught. [Do not] overestimate the austerity of my behaviorism. One could scarcely miss the central role that I ascribe to empathy, both in translation and in language learning. Radical translation begins with it in *Word and Object*: the linguist pictures himself in the native's place at the outset, in guessing at an observation sentence. The word 'empathy' does not occur there, but it does in my later writings. Early and late I recognized empathy as the strategy in radical translation. My use of the word 'empathy' is only recent and has been noticed, but I had already recognized the radical translator's approach as empathetic[1] in *Word and Object* and indeed nine years before. "The lexicographer," I wrote, "...depend[s]...on a projection of himself, with his Indo-European *Weltanschauung*, into the sandals of his Yalaba informant."[2] It is by empathy that we estimate our interlocutor's perceptions. Their neural implementation is as may be.

I countenance mentalistic predicates when their applicability is outwardly observable enough for practical utility. Our materialistic predicates, after all, are likewise vague in varying degrees, and I would apply the same standards. But mental entities I dispense with, and extensionalism I insist on for applicability of our quantificational logic.

Two acceptable manuals of translation might translate a foreign sentence into English sentences that both translators recognize as opposite in truth value. The two translations would be English sentences on whose truth values neither translator had an opinion except for agreeing that they must be opposite. Probably the foreigner was likewise open-minded about the truth value of his original sentence. Open-mindedness does not banish truth values.

A good manual will seldom state an integral translation for a sentence, but will support many by implication as acceptable paraphrases of one another. I picture my fiction of a manual of translation as an exhaustive account in the home language of the vocabulary and grammar of the foreign lenguage. The manual should afford, by implication, many equivalent translations of a sentence.

Two rival manuals will disagree on what set of translations of a foreign sentence they by implication support. This is where, by my lights, open-mindedness does give way to truth-valuelessness: there is no fact of the matter. Such is indeterminacy as distinct from under-determination. But I anticipate.

 [I] see science in the broadest sense as an inclusive, loosejointed theory of reality. Linguistics is part of it. The whole system becomes more closely knit here and there, as science progresses. Our successes in prediction and technology assure us that we are on the right track on the whole, but some irreducibly different turn, deep in the fundamentals, might have fared as well; such is the *conjecture of under-determination*.

My *conjecture of indeterminacy of translation* is a different sort of thing. It is that in the general interlinguistic case the notion of sameness of meaning is an objectively indefinable matter of intuition. This implies that the notion of meanings as entities, however abstract, is untenable, there being no entity without identity. I reject introspection as an objective criterion, however invaluable heuristically.

 [M]y conjecture of indeterminacy of translation concerned not terms like 'gavagai' but sentences as wholes, for I follow Frege in

deeming sentences the primary vehicles of meaning. The indeterminacy ascribed to 'gavagai' comes under the head rather of indeterminacy of reference, or ontological relativity. This indeterminacy is proved, unlike my conjecture of the indeterminacy of holophrastic translation. Its proof is trivial and undebatable. In essence it comes down to the equivalence of 'x is an F' to 'the proxy of x is the proxy of an F'. It does not imply the indeterminacy of holophrastic translation, because the indeterminacy of reference of a term can commonly be pinned down by the rest of the sentence.

If we take 'gavagai' not as a term but as a one-word sentence, 'Lo, a rabbit', it still does not illustrate the indeterminacy of holophrastic translation. It is an observation sentence, and hence, according to *Word and Object*, determinate in translation. 'Lo, a rabbit', 'Lo, undetached rabbit parts', and 'Lo, rabbithood' are all equivalent.

[M]isunderstanding surfaces also where [some have] me assuming that the only route to meaning is via translation. This was not the idea. [And do not] misinterpret my thought experiment in radical translation as an inquiry into the child's acquisition of language. That is quite another matter, and a fascinating one. But in my writings I have limited my concern with it to the minimum necessities of ontology, the structuring of science, and the meeting of minds regarding events in the external world: [three] traditional concerns of philosophy.

The abdication of epistemology to psychology, in which I connive, is less abject than [some] see it. The pertinent motivations and aptitudes remain those of the analytic philosopher rather than the experimental psychologist. Analysis of reification was called for, which had been passed over by psychologist and philosopher alike. This analysis branched into settling on what counts as reification and what service it discharges in the structuring of science and our spatiotemporal conception of the world. An incidental question, germane to epistemology but not traditional, was as to what aspects of our ontology are essential to science and what ones are merely subjective. There is philosophical progress here for which we would not look to psychology.

Another dimension into which these speculations lead is subjective similarity of perceptions. This is recognizable as psychology, but I doubt that the intersubjective harmony of these subjective standards was looked into and accounted for until motivated by

naturalized epistemology, where it is seen to underlie both communication and induction.

I see naturalized epistemology rather as enlivening than as superseding its eponym. [But] I have written at least twice (e.g., in PTb 19) that I stretch the term perhaps unduly.

NOTES

1. Not 'empathic', please. That, like 'phonemic' for 'phonematic', smacks of "little Latin and less Greek."
2. "The Problem of Meaning in linguistics," presented at a linguistics conference in 1951 and published in *From a Logical Point of View* in 1953. The quotation is from p. 63.

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