



Paradox and Indeterminacy

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PARADOX AND INDETERMINACY *

IN this paper, I shall seek to correct a fundamental misunderstanding of W. V. Quine's indeterminacy thesis, one which has been propagated even by philosophers who are sympathetic to Quine's epistemological position. Quine insists that the indeterminacy of meaning is truly a separate problem additional to that of the underdetermination of theories. Nonetheless, thinkers otherwise favorably disposed to Quine would have the indeterminacy understood as a species of theoretical underdetermination. For example, Richard Rorty¹ maintains that Quine can ultimately distinguish between a manual of translation and a scientific theory only by subscribing to the sort of positivist verificationism that Quine is otherwise committed to rejecting. Both Donald Davidson and Dagfinn Føllesdal,² in their discussions of the indeterminacy thesis, have suggested that Quine's attempt to distinguish *in kind* between scientific theory and a manual of translation is a consequence of his undervaluing the resources, both logical and evi-

* My thanks to Manley Thompson and Daniel Garber for many helpful suggestions. My special thanks to Anthony Perovich for his help with an earlier draft of this paper.

¹ "Indeterminacy of Translation and of Truth," *Synthese*, xxiii, 4 (March 1972): 443-462.

² "Indeterminacy of Translation and Under-determination of the Theory of Nature," *Dialectica*, xxvii (1973): 289-301. Davidson's "Radical Interpretation" appears in this same issue of *Dialectica*, pp. 314-328. For further comments by Davidson on the indeterminacy thesis, see "Belief and the Basis of Meaning," *Synthese*, xxvii, 3/4 (July/August 1974): 309-323. Føllesdal and Davidson have different expectations regarding a manual of translation; Davidson explicitly agrees with Quine that there is no nonempirical basis of meaning. Føllesdal's position on this issue is less than clear. Davidson differs with Quine when Davidson claims that the "degree of indeterminacy" is a function of one's schema for translation. One's choice of method, I shall argue, has no bearing on the indeterminacy of translation.

dential, which are available for an analysis of meaning. Rorty, Davidson, and Føllesdal all assume that it is possible to accept Quine's general epistemological precepts and yet deny his claim that translation is distinguished from scientific theory by some special problem. I will explicate Quine's epistemology so as to indicate why the foregoing assumption is mistaken.

Specifically, I shall be concerned to show that Quine's argument for the indeterminacy of translation of theoretical sentences establishes that one cannot assume, consistent with Quine's basic premises, that the practical command of a language entails that a speaker possesses a semantic theory by virtue of this command of the language. And, in turn, it is only if the speaker possesses some such semantic theory that there would be a fact of the matter, a real question of a "translation" being right or wrong. That is, the semantic theory a speaker uses would need to be the standard, the reference point, with which to contrast any proposed translation (and, of course, the speaker need never be conscious of using this theory). The argument for the indeterminacy thesis is to establish that there is no such reference point by which to evaluate any proffered account of language use, and so there is nothing about which a theory can be right or wrong—no fact of the matter to meaning. On the other hand, Quine has arguments for his contention that scientific inquiry is rooted in theory-independent evidence, and so has an objective base.

The distinction between the indeterminacy of translation of theoretical sentences and the underdetermination of theories arises precisely because of the different conditions needed to assure that each theory has a basis in fact, i.e., in evidence that is theory-neutral or theory-independent. Confusion on this point is nascent in Quine's writings, for although he insists that both semantic theory and scientific theory must develop from the same evidence—observation sentences—he fails to clarify why this evidence counts, in the case of the completed theories, as a fact of the matter only for science. (The phrase 'semantic theory' is to be understood as interchangeable with 'manual of translation'.)

One important consequence of resolving what might seem to be only an exegetical question is the clarification of the metaphilosophical implications of the indeterminacy thesis. I shall suggest why this thesis, if true, does not simply concern the matter of proper philosophical procedure, but mandates, in fact, the excision of almost all of what has been traditionally viewed as the subject matter of philosophy. In what follows below, I offer a brief charac-

terization of the indeterminacy thesis, and the premises of Quine's argument for indeterminacy. My initial concern is to analyze a basic tension between Quine's commitment to one of his principles—the Duhem premise—and his allegiance to certain tenets of empiricism.

I

Quine's claim that the translation of theoretical sentences is indeterminate is the claim that there does not exist, in fact or in principle, an objective ³ account of what theoretical sentences or terms mean. A "theory of meaning," in Quine's view, is to explain notions such as "having meaning," "significance," "analyticity," and "entailment." To construct a "manual of translation" is to construct a theory of meaning; all the problems that Quine canvasses under the rubric of translation are to be understood as representative of the difficulties in developing any such theory. The problem with any "manual" designed to examine the notions of meaning in question, Quine complains, is *not* that we might never know which manual is correct; the complaint is that there is no manual that could provide *the* one correct account of meaning. "The question whether . . . the foreigner really believes *A* or believes rather *B*, is a question whose very significance I would put in doubt. This is what I am getting at in arguing the indeterminacy of translation."⁴ No manual of translation can provide the correct account because there is no correct account, no right translation in the first place, which a manual could re-express.

The indeterminacy is to be understood as a consequence of two well-known Quinean principles—the Duhem premise and the Peirce premise:

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 is the natural conclusion.⁵

Quine surely holds both premises dear; however, I shall show that they are not, *prima facie*, compatible. The central problem is that

³ The provenance of objectivity is in the stimulus meaning of an observation sentence. In general, for Quine, objectivity is guaranteed only by evidence that can be thought of as being pre-theoretic or theory-neutral.

⁴ "On the Reasons for Indeterminacy of Translation," this JOURNAL, LXVII, 6 (March 26, 1970): 178–183, hereafter cited as ORIT, pp. 180/1.

⁵ *Ontological Relativity and Other Essays* (New York: Columbia, 1969), hereafter cited as OR, pp. 80/1.

Quine's holistic view of the theory-evidence relation threatens to render vacuous his concomitant contention that theories in natural science provide the regulative principles for determining *all* that can be known. For Quine, natural science is not one species of knowledge; rather it is all there is to epistemology.

The view of science so taken is justified by the claim that it is superior for purposes of accounting for the empirical evidence. That is, sensory evidence is the sole touchstone of objectivity; Quine's harangues against "mentalism" seem to presuppose just this point, i.e., that "ideas" are not objective evidence for anything at all. Yet, only after adopting the scientific standpoint (or, at least, Quine's notion of the proper scientific standpoint) does there appear good reason to believe that the sole source of knowledge lies with a natural-scientific account of what the senses reveal.

What is worse is that Quine himself maintains, as we shall come to see, that there is no distinguishing between what is strictly theoretical and what is not. But if there is no way to isolate the evidence that purportedly supports the epistemologically privileged status accorded science by Quine, then what comes of his claim that science has a basis in "fact" in a way in which no other theory has? We are expected to acquiesce in Quine's identification of epistemology with science because science best accounts for the only objective evidence there is. But if this suggestion is to be made philosophically compelling, then the empirical content of science must have ontological warrant apart from the theory it is said to support.

In this section, these problems are developed in more detail. Section II examines Quine's resolution of these problems. The final section details why the indeterminacy of translation of theoretical sentences is a consequence of this resolution.

Quine's conception of scientific theories as a web of sentences which have their meaning and their evidence only when taken together—the Duhem premise (I also call this Quine's "holism")—must be balanced against the type of epistemological theory Quine proposes to construct. His proposal is to formulate a "naturalized" theory of knowledge, an epistemology constructed using only the resources of natural science. Quine's basic concern, it should be noted, is to win our philosophic allegiance for such a project; he is less concerned with producing a completed theory. If this is correct, his antipathy toward "first philosophy"—seeking standards for knowledge that are prior to and firmer than those which science provides—should be made philosophically compelling. If epistemology is to appear as a chapter of natural science, then it must con-

cern itself with the legitimacy of the science of which it is to be a part. Within the context of his Duhemian view of science, Quine must account both for the genesis of natural science and for the favored status he accords scientific theory to the exclusion of all others (e.g., a science of intentions).

Put another way, Quine must answer two related questions concerning his proposed epistemology. The first is: can scientific theory itself be explained as an outgrowth of man's exposure to empirical stimuli? Quine wants to use science to account for what men know, but what men know includes science. There is, as he puts it, "reciprocal containment" (OR 83). The containment of science in epistemology would require an explanation, using only the resources of this science, of how science can come to be. Otherwise, why accept the claim that epistemology should be a "chapter" of natural science? I call this the "problem of justification from within."

The second question concerning Quine's epistemological position is whether his marked preference for physical theory is anything other than an article of philosophical faith. Quine celebrates both the austere beauty and the ontological economy of physical theory, canonically expressed; it alone is the scheme by which to attempt to limn "the true and ultimate structure of reality."⁶ But surely aesthetic appeal is not meant as a substitute for argument on the critical issue of deciding the nature and limits of epistemological inquiry. One might agree with Quine that the "question of what there is is a shared concern of philosophy and most other non-fiction genres" (WO 275). Yet such agreement still leaves one free to dispute his criterion for distinguishing fact from fiction.

A suspicion that Quine is motivated more by faith than by argument on this point is deepened by his express declaration that "two cardinal tenets of empiricism" have weathered all the vicissitudes of philosophic fortune. "One is that whatever evidence there *is* for science *is* sensory evidence. The other . . . is that all inculcation of meanings of words must rest ultimately on sensory evidence" (OR 75). But it is to scientific theory that one must appeal, in the first place, to make the case for sensory evidence, and so it cannot be science that establishes these tenets as unassailable. To say so would be to beg the fundamental question, viz., why accept natural science as the *exclusive* arbiter of truth to begin with?

Ironically, it is Quine's own criticisms of Carnap *et al.* which present the greatest obstacle to answering the two questions con-

⁶ *Word and Object* (Cambridge, Mass.: MIT Press, 1960), hereafter cited as WO, p. 221.

cerning Quine's own empiricist position. For by his critique of the project of effecting a rational reconstruction of science, Quine means to undercut the belief that one can provide an account of the theoretical language of the theory using only epistemologically primitive notions (or what were considered to be such), i.e., observations and logical and mathematical auxiliaries. Inquiry into the nature of knowledge requires using the theory man has in hand; there exists, Quine insists, no point of cosmic exile (WO 275), no $\pi\omicron\upsilon\tilde{\nu}$ $\sigma\tau\tilde{\omega}$ (OR 6). The questions concerning the legitimacy of Quine's enterprise must be resolved from within the context of present theory.⁷

And, far from resolving either question, the need to use science to account for science leads to an additional and equally serious problem. For it is science that teaches us that man's sensory evidence about his environment is meager and sporadic; how could this science have come about from such a paucity of input? A skeptic's doubts about science as the arbiter of knowledge are doubts which science encourages. The "crucial logical point" (RR 2) for the epistemologist is that scientific inquiry indicates the inadequacies of such inquiry for what is needed, viz., scientific explanation of scientific theory.

Quine counters this logical point by noting that the notion of what an epistemological theory should be like has been altered. The logical empiricists did not err in attempting to explain bodies in terms of sensory data; their error lay in thinking that this could be done by determining the empirical content of individual statements. In abandoning the attempt to account for science in this manner, we retain our theory for its practical benefits. The concern of epistemology is no longer a translation of scientific theory into more basic notions; the epistemologist is now to accept current scientific theory as the "medium for such epistemological inquiry [because] we can choose no better than the selfsame world theory we are trying to improve, this being the best available at the time."⁸ The logical point now becomes of no concern because the purpose of epistemology is no longer one of translation or reconstruction. The new epistemologist sees science as justified from within on practical grounds alone; epistemology is to be located within science because there seems to be no better place for it.

⁷ *Roots of Reference* (La Salle, Ill.: Open Court, 1973), hereafter cited as RR, p. 34.

⁸ *The Ways of Paradox* (New York: Random House, 1966), hereafter cited as WP, p. 241.

Yet, the other question remains: why accept scientific theory as the medium of epistemological inquiry to the exclusion of all others? For Quine does not claim that science succeeds best with respect to facilitating man's understanding of one kind of knowledge; he claims that all knowledge is of one kind—that kind identified with the extensional scheme of modern science. It is with respect to supporting this claim that the Duhem premise presents the severest obstacle to the theory of knowledge envisioned by Quine. For at the heart of Quine's theory are to be those tenets of empiricism which he holds to be unassailable; i.e., that the initial evidence for science, and for semantics, can only be sensory evidence. Yet, in accord with the Duhemian precept, he asserts that the conditioning of sentences to sentences is so interwoven that "even where the conditioning to non-verbal stimulation is so firm . . . there is no telling to what extent it is original and to what extent it results from a shortcutting, by transitivity of conditioning, of old connections of sentences with sentences" (WO 13). If there is "no telling" when the connection of a sentence to sensory evidence is mediated by theory and when not, how then is it ever possible to identify the nontheoretical empirical content of the theory? Yet it is critical that Quine be able to achieve just such a separation; without it, the suspicion, noted above, that Quine favors a naturalized epistemology for no compelling reason deepens into a conviction that he offers no more than his own predilections. If there is no distinction to be made between a theory and its empirical content, why hold to Quine's tenets?

The basic strategy in defense of the claim that scientific theory is objective in a way in which no other theory is is to investigate the relation between scientific theory and the sentences held true by the man in the street. Quine maintains that both are theories in the Duhemian sense; more importantly, he argues that the theories are intimately related. Science is but an outgrowth of, and a refinement on, the truths one learns when learning one's mother tongue. There is no real break in the movement from the sentences learned first to the upper reaches of physical theory: "We are working up our science from infancy onward" (RR 138). The nonverbal sensory stimulations that are involved in first learning to speak become the empirical foundations of science. In order, then, to examine the empirical basis of science, one should study the process of language acquisition.⁹ Such study, Quine contends, offers the *only* possibility

⁹ "The Nature of Natural Knowledge," in Samuel Guttenplan, ed., *Mind and Language* (New York: Oxford, 1975), hereafter cited as ML, pp. 74/5.

for discerning the bare empirical content of the theory that results. As he says, "The paths of language learning . . . are the only connection there is between observation and theory" (ML 79). The study of language learning is needed not only to make sense of the theory-evidence distinction; it is expected also to support Quine's claim that the evidence from which we initially learn what words mean is the same as that from which science develops. "The two roles of observations, their role in the support of theory and their role in the learning of language, are inseparable. . . . The evidence relation and the semantical relation of observation to theory are coextensive" (RR 38). This "partnership," as Quine calls it, between "the theory of language learning and the theory of scientific evidence" (ML 74) is no marriage of convenience; science gains insight into its objective foundation by this relationship if by any.

Yet the problem of determining the theory-independent content of a natural language is as intractable as that of determining it in scientific theory, and for just the same reason. Individual sentences of a natural language, taken apart from other sentences of the common belief structure, are bereft of meaning and of empirical import. The distinction between "language" and "theory" collapses for Quine, inasmuch as there is no differentiating between those sentences whose truth is established by certain semantic or syntactic rules alone and sentences that are true because uniquely corroborated by facts.¹⁰ The "blurring" of the analytic-synthetic distinction, in turn, counts against making sense of single sentences considered apart from the language within which they have a use. The result is a well-known Quinean image. The sentences believed true by a community of language-users are seen as "a single connected fabric" including all sentences which, "primarily as a whole, is multifariously linked to non-verbal stimulations" (WO 12). But, it would now seem, Quine's attempt to show that his choice of tenets is nonarbitrary and so to defend scientific theory as the exclusive framework within to judge all matters ontological, has reached dead end. For at the only point at which the purported empirical content might be shown to exist apart from the theory, Quine's own Duhemian views fatally intrude. Given the Duhem premise, Quine seems unable to establish that epistemology is contained in science, because there is no argument for his claim that only the sensory evidence yields clues to the nature of reality. Surrendering the

¹⁰ "Replies," in Davidson and J. Hintikka, eds., *Words and Objections: Essays on the Work of W. V. Quine* (Dordrecht: Reidel; New York: Humanities, 1969), hereafter cited as *WsOs*, p. 310.

Duhem premise would mean accepting that at least some statements are known true apart from all theoretical considerations. In the first instance, Quine cannot make the case he wants for a naturalized epistemology; in the latter instance, the focus of epistemological inquiry becomes centered on nontheoretical knowledge, and so returns to "first philosophy." There is, then, at least a surface tension between Quine's Duhemian outlook and the tenets of empiricism to which he subscribes. Ending the strain requires distinguishing between theory and evidence, but this he explicitly says he cannot do. As a result, his philosophic tenets seem ever more to be mere articles of faith.

II

In order to dissipate the tensions between his empiricist tenets and his epistemological premise, Quine needs to rely on an argument which he only passingly acknowledges. Indeed, Quine seems not to have recognized the importance of this argument to his own system. One result of not appreciating this argument has been a severely misplaced emphasis on the importance of Quine's criteria for what he calls "observation sentences"; I show that these criteria, in and of themselves, are of no real philosophical importance. That is, in order to establish that scientific theories have an objective basis, no appeal to the criteria for observation sentences is needed. The crux of the case for a naturalized epistemology lies elsewhere.

In order to understand how Quine resolves the problems developed in section I, we must first note a paradox which results from his holistic view of natural language. The sentences of one's native tongue are taken to be an interrelated group and to have their meaning and their evidence only as such. Yet language is learned a bit at a time, and so in isolation from the containing theory which provides each bit with whatever semantical and empirical import it may have. Thus, it would seem, verbal stimulations consisting of individual sentences must be meaningless for a child. But this suggests the paradoxical situation in which each person learns meaningful discourse by parroting what must be, for the learner, nonsensical verbal segments. Quine realizes that this paradox of language learning is a consequence of his holism. Individual sentences should make no semantic sense, but languages is not learnable unless they do make sense. "Such relativity should be awkward, since, conversely, the individual component sentences offer the only way into the theory."¹¹ This "relativity" of individual sentences to

¹¹ "Meaning and Translation," in H. Morick, ed., *Challenges to Empiricism* (Belmont, Calif.: Wadsworth, 1972), hereafter cited as MT, p. 75.

larger portions of the language presents severe problems in accounting for the acquisition of a sophisticated theoretical language. Quine concedes that here "subtleties and obscurities crowd in" (RR 35), for the theoretical sentences are not a deductive consequence of sentences learned singly, nor are theoretical sentences reducible by "continuous derivation" to observation terms (ML 77). The problem raised by the paradox applies to all those sentences for which there is no evidence that "can be dreamed of that does not include verbal stimulations from within the language" (MT 94). The point of the paradox is that verbal stimulations alone will not suffice to make meaning learnable, because the verbal stimulations are meaningless if taken in isolation from their place in language.

There are two fundamentally important points which the paradox brings into sharp relief. First, the basis for Quine's contention that there *must* be theory-neutral evidence is uncovered by the resolution of the paradox of language learning. His discussion of observation sentences concerns his claim that we can know which sentences do, in fact, correlate with such theory-neutral stimulations. Thus, the reason for maintaining that some sentences are observation sentences is different from the attempt to identify which sentences are. The criteria Quine formulates for identifying observation sentences are not, then, part of his proof that there need be any.

The paradox of language learning is to prove that there must be nonverbal stimulations of the sort which make language accessible. These stimulations are the empirical evidence for the language that one first learns. This is the only link there is between science and unvarnished fact. "The notion of a reality independent of language is carried over by the scientist from his earliest impressions, but the facile reification of linguistic features is avoided or minimized" (WP 220). Unless there is some sense to be made of a link between verbal and nonverbal stimulations *prior* to having learned a more developed discourse about reality, Quine argues, there should be no sense of reality "carried over" from the earlier to the later ways of talking. More importantly, without some intelligible pre-theoretic link between verbal and nonverbal stimulations, there would seem to be no explaining why people should develop similar accounts of reality. Conversely, if there are "reference points" that can be shared both by those lacking the theory (e.g., infants) and by those holding a set of beliefs, then this abates the paradox inherent in a "full-blown" Duhemian view. An unmitigated Duhemian stance, on the other hand, leaves one without an explanation of why lan-

guage is teachable, for there would be *no* basis for communication between those within a theory and those lacking it.

Even given the Duhem premise, Quine's claim that both scientific and semantic theory must be rooted in sensory evidence remains tenable. In order to explain why beliefs can be objective—publicly shared—the Duhemian view mandates that there be non-verbal stimulations to aid in the language-learning process. In short, there are good reasons, given Quine's holism, for his view that scientific theory examines facts not of man's making. This argument, in turn, supports the privileged epistemological status accorded natural science, since science is best equipped to explore the inferences that men are apt to draw from their initial and ongoing exposure to such evidence.

The paradox suggests a second noteworthy point: one must avoid confusing the insight that a structure can be imposed on a natural language with the assumption that, in learning to speak, any language learner acquires something like this imposed theory. Most importantly, it is *not necessary* to assume that a language learner acquires any semantic theory at all. It is not the particular theory that may go awry; the entire mode of explanation, whatever its benefits, need not describe, even in the most general way, how semantic facility with a language is actually acquired. In his own speculations on the genesis of reference, Quine enters just such a caution. "I have asked how our ontological notions are possible, not why they are right. Even in the case of bodies, those prototypical objects of reference, I offered no hope of justification" (RR 136). Obviously, each person resolves the paradox in learning to speak. But, Quine warns, we must not confuse speculation on how this happens with the belief that even our method for speculating on this matter necessarily reflects the actual process.

The reason for Quine's despair on this point is *not* related to the problem of underdetermination, that is, the problem of the case in which one has several empirically equivalent but logically incompatible semantic theories, all tied for first place. Quine's claim is stronger than such a formulation of the problem assumes. For he claims that one errs even if one thinks that there is a theory to discover. He asserts that the very question lacks significance. Quine's "no hope" view of the matter is better understood by considering what the scientific study of language learning actually does reveal. First, it suggests that learning is an inductive procedure, a process of linking heard bits of language "tenuously and conjecturally to what [the child] knows, until by dint of trial and social correction

he achieves fluent dialogue with his community" (ML 74). But what is to guarantee that the inductive leaps land all speakers in the same spot? What ensures that the end product is any theory at all? Outward conformity is achieved, but the observable input does not support the suggestion that the child is necessarily acquiring a semantic theory. Second, the empirical evidence involved in each case is unique, in some respect. Conformity in semantics is not mandated by the evidence to which the language learner is exposed. The study of the language-acquisition process counts against the belief that speakers of a language have in common either the evidence for a semantic theory or some common theoretical structure.

But is not the very similarity of output itself good evidence for the view that people share a semantic theory, a common manual of translation? *No; because the verbal output is evidence for the presence of a semantic theory only on the prior assumption that the possibility of any such output can be accounted for only if the speaker implicitly possesses some such theory.* Quine is not opposed to the suggestion that one might regiment word and sentence usage according to an imposed theory. The problem is to avoid confusing any gain in insight with the belief that a speaker must be employing some theory. The benefits of a translation scheme are not proof that the very ability to speak requires that such a scheme be possessed. Quine is not worried about the underdetermination of manuals of translation; his is the deeper point that what is open to doubt is the chosen mode of explanation.

The distinction between Quine's worries about indeterminacy and his concerns about underdetermination is a distinction between knowing the conditions under which language is possible and providing the best possible explanation of verbal behavior. This distinction is apt to be overlooked because the study of language learning differs from the enterprise of acquiring one's native language. The study begins within a developed theoretical context, the resources of which a linguist wants to use in order to formulate a manual of translation. However, from a philosophical viewpoint, one must be chary of any theory so developed. Philosophical caution is needed not because of questions relating to the correctness of the individual theory, but because the investigation begins on the assumption that there exists a theory for the linguist to discover. Yet it is just this belief that requires warrant. The *only* warranted assumption to be established is that language learning must involve nonverbal stimulations. In order to justify any further assumptions about what is involved in language learning, further arguments

must be adduced. With respect to the question of warranting assumptions, the beleaguered field linguist is just a case in point. When confronted with the now proverbial problem of radical translation, the example of the linguist suggests just what facts there are to go on and what assumptions must be made if translation is to be ongoing. The sentences initially accessible will be those which correlate with some event perceived by both native and translator. And so it is as Quine predicted—nonverbal stimulations from outside the language are the key to gaining entry. However, if sense is to be made of most of what the native says, the field linguist *must* assume that the language is susceptible to some intelligible formulation. How else is communication to be established? But why believe that there is some theory involved in the native's use of language?

The argument that there exists objective evidence develops from considerations on the paradox of language learning. Quine's discussion of the data available for translation takes its lead from this. For it is the paradox which suggests that some sentences must be correlated with nonverbal stimulations so that they can be learned individually, apart from their associated belief structure; these are observation sentences. Quine's criteria for identifying observation sentences (ML 72) are not doing any important work in his theory; the criteria are revisable as need be. The case for the existence of objective empirical evidence is made without appeal to any set of criteria for their identification. Quine accepts as necessary only what his epistemological premises and tenets require him to accept; and, given these, there must be observation sentences. The premises and tenets say nothing, however, about whether or how it is possible to isolate such sentences.

As is to be expected, what is crucial to the notion of an observation sentence is what Quine calls its "stimulus meaning." The stimulus meaning is just the nonverbal correlate that makes the sentence learnable as an individual sentence. Observation sentences are a subset of what Quine calls "occasion sentences"; all such sentences have at least some empirical factor involved in satisfying their truth conditions on the occasion of utterance. However, most occasion sentences have an important point in common with sentences whose truth conditions are satisfied without appeal to evidence on each occasion of use (standing sentences). All require what Quine calls "collateral information"—information from within the theory. Thus, their truth conditions cannot be determined by observations alone. It is because their truth conditions are linked only to the

passing perceptual scene that sentences are observation sentences: "their stimulus meanings may without fear of contradiction be said to do full justice to their meanings" (WO 42). Quine acknowledges, in short, that the Duhem premise, to be retained, requires modification. Specifically, an empiricist has to be able to account for how initial entry into the web of sentences is possible; Quine stresses that this is the function observation sentences are to fulfill. And, by fulfilling this function, observation sentences become the empirical evidence on which man's discourse about the world is built (OR 88). The "Peirce premise"—Quine's other precept—is just a way of restating the fact that nonverbal stimulations are needed in order to learn language and that they provide what objective content there is to language.

The Peirce premise appears ad hoc unless one understands both the tension between Quine's empiricism and his holism and how Quine resolves this strain. The claim that the Peirce premise is just such an extraneous accretion to Quine's epistemological position is developed by Christopher Boorse.¹² Boorse contends that Quine's account of observation sentences is inconsistent with the Duhem premise. In Boorse's view, there is a Quine₁ who espouses and defends a modernized, "theory-laden" empiricism which emphasizes that the "nonobservational constraints on theory choice are basic to scientific method" (JP 370). Boorse applauds the position of Quine₁ and contrasts it to that of Quine₂, a philosopher he finds committed to "semantic positivism," i.e., the position "that there is a special class of sentences, observation sentences, which report sensory experience in a pristine form untinged by theory" (JP 369). The problem here is easy to see. According to Quine₁, the meaning of all sentences is context-dependent; according to Quine₂, some sentences have a meaning that is context-independent.

Boorse's position is to adopt a view of language in a full-blown Duhemian form. All sentences, he contends, have their truth conditions determined intratheoretically; this context-dependence holds not only for grammatically complex cases, but also for the simple cases, including one-word sentences (JP 374). Having identified himself with what he takes to be the position of Quine₁, Boorse goes on to maintain that the position of Quine₂ arises from no systemic need of Quine₁. The doctrine of observation sentences maintained by Quine₂ is a philosophic atavism, "understandable only as an outgrowth of his commitment to semantic positivism" (JP 374). Since

¹²"The Origins of the Indeterminacy Thesis," this JOURNAL, LXXII, 13 (July 17, 1975): 369-387, hereafter cited as JP.

the positions of Quine₁ and Quine₂ are inconsistent and since Quine's best arguments are those for the Duhemian view, Boorse concludes that only the doctrine of Quine₁, as he has interpreted it, is deserving of continued philosophic attention.

Ironically, what Boorse fails to appreciate is that, in order to be an empiricist, especially an empiricist of the "theory-laden" variety, one needs a position at least similar to that of Quine₂. Indeed, Boorse clearly recognizes that a consequence of his holism is that "the child learner has not understood his first observation sentence until he has the theory in his grasp" (JP 374). So one cannot learn the meaning of any single sentence unless one knows the meaning of the sentences to which it is related in the language. But if this is correct, then Boorse embraces, rather than resolves, the paradox.

The pernicious consequence of a position such as that taken by Boorse is that it renders unintelligible the process of learning a language. As noted above, Boorse believes that we do not know the meaning of any single sentence until we know the theory of which it is a part. Any one sentence, taken apart from the theory, is meaningless. In Boorse's view, then, the picture of the language-learning process would seem to be the following. A child (for, of course, we are interested in the case of someone learning his first language) juggles whatever semantic bits happen to come his way; but none of these bits are, from the start, semantically significant for the child. Moreover, none of these bits need have any semantic relation to each other; even one-word sentences, Boorse maintains, have their meaning constituted only intratheoretically. Despite being unable to gain any firm semantic toehold, the child acquires not only a language, but one reasonably close to that spoken by his elders.

Part of Boorse's problem is that he cannot understand how stimulus meanings can have semantic import. He compares learning meaning in this fashion to conditioning a rat to press a bar (JP 374). Here he misses the crucial point, which is that, if some sentences did not have these stimulus meanings, there would be no "social checkpoint" by which to correct the child and further his progress. Stimulus meanings do "full justice" to the meaning of observation sentences because they exhaust its empirical content. Boorse's complaint that the stimulus meaning does not yield the natural-language synonymy conditions reflects a basic misunderstanding of what is at issue. Quine never says that it does or that it should fulfill this function. Quine is concerned with the initial relation of semantics to sensory evidence. Without such a relation, without a social reference point available from the outset of the

learning process, the entire process of language learning becomes solipsistic, each speaker left to create all meaning on his or her own.

An unmitigated Duhemian view of language learning provides no explanation of why people speak the same language at all. On Boorse's account, we cannot correct the child until the child can understand us (i.e., until he speaks the language), but at that point it is too late. In the absence of social control, it is quite miraculous that language is perpetuated. Boorse may find the notion of observation sentences curious, but it is not nearly so curious as the view of language learning which results if the notion is dispensed with. Properly understood, observation sentences provide the needed link between verbal stimulation from within the containing theory and theory-independent stimulations.

The position now is this. At the beginning of section 1 the question was raised whether Quine could give reasons for a naturalized epistemology in light of the tension between his holism and his empiricist tenets. Quine effects what I called "justification from within" by altering the traditional view of what an epistemological theory should be. Given the altered view, practical considerations argue for scientific theory as the "medium" for epistemological inquiry. There remains, however, the problem of explaining why epistemology is to be contained in science. In order to resolve this difficulty, Quine's first step is to argue that science is continuous with ordinary discourse about reality; science refines man's more casual talk about the world. His second step is to argue that language would not be learnable, given his Duhemian view of it, unless certain sentences were individually correlated with nonverbal stimulation from outside the theory.

This point is crucial; for it is meant to prove that there is a case to be made for theory-neutral evidence, and for this evidence being sensory evidence. Specifically, Quine argues that a full-blown Duhemian language is not teachable; the semantics of such a language are solipsistic. Conversely, language can be taught under socially controlled conditions (as ours is) only if the process involves observation sentences. It is because the full-blown Duhemian language allows for no observation sentences that it is not teachable. Quine has maintained that it is the teachability of language which argues for its being objective. The Duhemian language, in short, has no claim to objectivity. In order to preserve the favored epistemological position accorded science, Quine must assume that observation sentences are involved in language learning. Since language learning has its origins in the empirical and since natural

science is the discipline *nonpareil* for the study of sensory stimulations (stimulus meanings), the objective content of human discourse ought to be analyzed by epistemological inquiry carried out within science.

Observation sentences are the point of origin for both semantic theory and scientific theory. Yet, Quine asserts, they provide a basis to science and not to semantics. This failure of semantic theory to have a fact of the matter cannot be pinned to the underdetermination of semantic theory, because science shares in the problem of underdetermination. The indeterminacy of translation of theoretical sentences cannot result from the inductive methods used to construct manuals of translation, for the scientific method is also inductive. Finally, the distinction between a manual of translation and a scientific theory, between knowing what a sentence means and knowing why it is true, cannot be ascribed to a difference in point of origin, because Quine takes pains to emphasize that the point of origin is identical. For all this, the theories are different in kind; the parallel fails at a critical point.

But, in light of the noted similarities, how could one theory fail to be objective? What could account for this disparity? Quine claims that there are no facts of meaning but there are facts of nature; the indeterminacy of meaning is not paralleled by an indeterminacy of nature. If the indeterminacy of sentence meaning is a consequence of the Duhem premise and the Peirce premise and if these premises apply to scientific theories as well as to manuals of translation, how can there be a problem for one which does not hold for the other?

III

In order to appreciate why the distinction between truth and meaning which Quine draws reflects certain consequences of his assumptions, it is best to begin by noting why Quine claims that "we can judge truth as earnestly and absolutely as can be" (WO 25) within the context of scientific theory. Pending revision, the canonical idiom frees science from any lingering dependence on the theoretical reaches of the home language, even though physical theory is, initially, an outgrowth of this unrefined mode of discourse (WP 226). Science has a fact of the matter because it has "refined" ordinary talk of reality. The claim is not that present theory is, for all this, the best that could be; indeed, Quine's expectation is that science will continue to change and evolve. His point is that scientific theory, even as given, is the only proper framework within which to develop an account of the sensory facts of life.

We come now to the heart of the position that Quine is opposing. His argument is with those who think that it still makes sense to ask, even conceding the above, which manual of translation best interprets the sounds people make. A person's utterances would be taken here as evidence that the speaker held—if only implicitly—a particular theory, a manual for his language. Which interpretation, one would like to say, best captures the sense of another's words, i.e., best approximates his manual of translation. Quine denies that there remains, after science has done its job, some *further* account of the evidence that needs to be given. Rather, Quine insists that the indeterminacy withstands "the whole truth about nature" (WsOs 303). What sensory evidence there is, indeed, all the evidence there could be—the whole truth about nature—does not resolve the problem of indeterminacy.

The indeterminacy is immune to what the facts can tell us. This point is important, because it indicates that Quine is opposing those who would believe that the empirical evidence from which we develop our manual of translation carries with it—in some as yet unexplained sense of 'carries'—a significance which bears on the "real" or "actual" meaning of a theoretical sentence. Quine's point is that there exists no such aspect to the evidence; there is no "meaning" that it "carries."

If one believes that the evidence has both semantical and empirical import, one can then reasonably seek a theory that accounts for this import. What would here give semantic theory a fact of the matter is that the empirical evidence (people's utterances) would be assumed to have a meaning; a manual of translation that accounted for more utterances than another, other things being equal, could then be said to be objectively better. In other words, what would be needed to give semantic theory a fact of the matter would be the existence of a contrast between the manual imposed on one's verbal behavior and the semantic theory a person *really* uses. For only in this case are the utterances evidence for a semantic theory; i.e., only in this case do they have, in some clear sense, a "meaning" which is other than the creation of an imposed manual of translation.

The expectation that the question "What did the native say?" has a right answer depends precisely upon the assumption that the utterance has some predetermined meaning; the empirical evidence "carries" this meaning, and so it is objective evidence for a manual of translation. The problem is in no way abated for the linguist who believes in there being a right answer if underdetermination is ignored. "But in this expectation, even as hedged by this last

proviso, he is mistaken" (WsOs 304). The expectation is defeated—there is no hope of justifying one manual over another—because it requires an assumption which, for a Quinean empiricist, is radically mistaken. The assumption is that there exists a semantic theory to be "recaptured" by careful translation. The problem most definitely is *not* that our knowledge of another's semantic theory is underdetermined; the problem is that the available empirical evidence counts against assuming, in the first place, that there need exist such a semantic theory. There is no warrant, as argued earlier, for the claim that language use *necessarily* involves the use of any kind of theory. Quine's Duhemian views and his empiricist tenets lead him to conclude that meaning, too, is rooted in nonverbal stimulations—the Peirce premise. But he is committed, by the logic of his position, to no more than that with respect to language learning. In lieu of such a warrant, there is no basis for claiming that theoretical sentences have a fact of the matter.

The empirical evidence involved in language learning is not evidence for the claim that the outward conformity of verbal behavior can only be the result of possessing some semantic theory, that is, something necessarily corresponding to any mode of explanation the linguist chooses. Indeed, given the construction, *mirabile dictu*, of a manual of translation that provides for semantic transformations in each and every language, this would be neither a necessary nor a sufficient condition for the conclusion that semantic theory has a fact of the matter. For the question, as Quine has cast it, has become one of transcendental metaphysics, one of determining the conditions of the possibility of being able to use a language. A choice of manuals is subject to certain practical constraints, but there exists "no real question of right choice" until someone establishes that there is such a choice to make.

The indeterminacy is not due to a lack of evidence; rather it is due to the fact that what we want evidence for does not exist until man, in a fit of science, creates it. "For the arbitrariness of reading our objectifications into the heathen speech reflects not so much the inscrutability of the heathen mind, as that there is nothing to scute" (OR 5). The absence of any fact of the matter to meaning is due to this "indeterminacy of correlation" (WO 78). We have nothing to go on but empirical evidence, but the empirical evidence is nothing to go on for the purpose which some translators desire—the right answer to the question of what the native means. Translations fail to answer this question because "there is nothing for

such a correlation to be uniquely right or wrong about" (OR 25). Quine, of course, is not claiming to have shown that a shared semantic theory is nonexistent, or that it is impossible that one might exist. The point is that we have no proof that such a theory need exist, and so no warrant for assuming a fact of the matter to translation. This is why, when "we take a verification theory of meaning seriously, the indeterminacy would appear inescapable" (OR 80). Taken seriously, within the context determined by the Duhem premise and the Peirce premise, the hope of verifying meaning is forsaken, not because the relevant standard of verification is inaccessible, but because the relevant standard does not exist.

The indeterminacy of translation of theoretical sentences is *not* a consequence of an inability to discover which manual of translation "really" fits the native's utterances. Neither is Quine maintaining that, in the study of language, it is wrong to develop semantic theories. Such theories may have some insight to offer, but we go astray if we think of such theories as scientific, as having a fact of the matter about which to be right or wrong.

Rational reconstruction in science would, of course, require determinacy of translation; Quine has shown that, once one forsakes the old empiricism, there is no basis for assuming semantic determinacy within an austere empirical theory of knowledge. There are reasons, however, for believing in the special objectivity of science. "In language learning there is the multiplicity of individual histories capable of issuing in identical verbal behavior. Still one is ready to say of the domestic situation in all positivistic reasonableness that if two speakers match in all dispositions to verbal behavior there is no sense in imagining semantic differences between them" (WO 79). One is ready to say this only if one fails to realize that there is no basis for maintaining sameness of semantic theory either. An empiricism without dogmas is, for Quine, one in which meaning must be assumed to be indeterminate.

The foregoing discussion has indicated why, then, the indeterminacy of translation of theoretical sentences is the "natural conclusion" (OR 81) of those premises basic to Quine's epistemology—the Duhem premise and the Peirce premise. If my explication is basically correct, then it is mistaken to assume, *pace* Rorty, Føllesdal, and Davidson, that Quine's position is compatible either with the view that a manual of translation could be epistemologically significant or with the claim that the "degree of indeterminacy" is lessened given an efficacious schema of translation. There is no science of the second intension (ORIT 179).¹³

On this account of the indeterminacy thesis, the consequences of Quine's view are more important, I suggest, than has so far been appreciated. The indeterminacy thesis is, to be sure, an attack on the "myth of the museum." But if it were no more than another case of a philosopher inveighing against "pernicious mentalism" it would be difficult to understand what is really new about the thesis. What, in short, is finally at issue in asserting that there is no fact of the matter to translation?

The substantive philosophical consequence, I would argue, is the fact that the indeterminacy thesis leaves, in its wake, a desiccated body of knowledge about which to philosophize. Those wishing to scale the philosophic heights must now follow the path of semantic ascent, and though the summit reveals (as always) truth, it is truth relative to current scientific theory, preferably in canonical form. If philosophy is to be a nonfiction genre, then what is involved is not a simple retrenchment behind the lines defined by natural science. Rather, one is forced to acknowledge that beyond those lines there are no battles of objective import to fight. The indeterminacy thesis is not just a philosophical doctrine; it is also a thesis about the possibilities of philosophy. Richard Rorty appreciates the sense in which Quine's views, taken with full seriousness, imply a wholesale reshaping of one's sense of the philosophic possibilities. "If there are no meanings to analyze, . . . if there are no reductionisms to advocate as the result of analyzing meanings, then perhaps there are *no* central or foundational questions in philosophy."¹⁴ If translation is indeterminate, then the problem is to discover whether purposeful inquiry is possible anywhere except within science canonically expressed. Whether one agrees with Quine, or whether one opposes him, the problem now is the one of ascertaining what philosophical issues still remain. Quine has shifted the burden of proof to those who would claim that the philosophy of science is not philosophy enough.

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¹³ My account has examined only one aspect of Quine's views on indeterminacy, viz., the indeterminacy of translation of theoretical sentences. There is at least one other form of indeterminacy: the indeterminacy of translation of the apparatus of identity and individuation. This latter form is the basis for holding that reference is inscrutable. The arguments for the inscrutability are different from those I discuss here.

¹⁴ Review of Ian Hacking: *Why Does Language Matter to Philosophy?*, this JOURNAL, LXXIV, 7 (July 1977): 416-432, p. 432.