RECONSTRUCTING QUINE: THE TROUBLES WITH A TRADITION*

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There has developed in the professional literature a distinct tradition of interpreting and reconstructing Quine's epistemology. The tradition has its roots in Gilbert Harman's influential interpretation of Quine as an empiricist and in Harman's reconstruction of Quine's views on meaning. Richard Rorty, and those influenced by what may fairly be called the "Princeton School" of Quine scholarship, have elaborated and extended this tradition.

The members of this school are unified by the attempt to offer a synoptic view of Quine's epistemological project — that is, to specify how the leading themes of Quine's epistemology are (or are not) to be integrated into a comprehensive and systematic account of the nature of human knowledge. Differences certainly exist within the tradition. (For example, Rorty and Harman disagree over whether competing axiomatizations of set theory illustrate Quine's thesis of the indeterminacy of translation.) Nevertheless, the main outlines of a received reading have been agreed upon, and it is possible to specify three fixed theses which together constitute the received reading of the tradition:

(a) Quine is a holist;
(b) Quine is an empiricist (of some stripe);
(c) Quine's views on scientific method and on the under-determination of theories are integral to an understanding of his views on meaning and reference.

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3 This despite Quine's apparent endorsement of Harman's example. See Harman's contribution to Words and Objections, eds. Davidson and Hintikka (Dordrecht: Reidel, 1969) and Quine's reply therein to Harman. (Hereafter cited as WSOS.) Rorty (quite properly and decisively) rejects this example in his Synthese article.
The first two theses appear to be clearly part of Quine's thought. The tradition holds that reconstruction (i.e., determining what Quine either "meant" to say or did say but ought not have) enters in at (c), i.e., in explaining Quine's exclusion of theories of meaning and reference from the set of "objective" theories. Most important, however, the three theses are taken to be both consistent claims and unproblematic starting points for reading Quine.

The theses stressed by the tradition are, in turn, based on three themes which pervade Quine's writings on epistemology. These three themes are:

(i) The Duhem thesis (Quine's holism): The claim that the theoretical sentences (within either natural languages or more formal theories) have their meaning and their evidence only as parts of a theory;

(ii) Under-determination of theories: The claim that it is possible to formulate empirically equivalent but logically incompatible scientific theories;

(iii) The indeterminacy of translation of theoretical sentences: The claim that theories of meaning for natural languages, unlike theories in the natural sciences, have "no fact of the matter." This failing distinguishes meaning claims relative to a manual of translation in kind from claims that a sentence is true relative to a scientific theory. This failure of objectivity holds despite basic methodological parallels between the analysis of meaning and of truth.4

Exactly how do the theses of the received reading "map onto" Quine's characteristic themes? Quine's commitment to the Duhem thesis is indisputable, and to say Quine is a holist is just to say he believes that (i) is true. Problems arise, or so I shall argue, because Quine's views on under-determination ((ii) above) and his commitment to empiricism are understood by the tradition as premises which together yield (iii) — the indeterminacy thesis. (See especially PMN 194–204.) This account of the argument for the indeterminacy thesis I have labeled thesis (c) of the received reading.

In this paper, I sketch an analysis of Quine's epistemology that constitutes a sharp break with the received reading of Quine. Section 1 is destructive in intent. My purpose is to show why the traditional interpretation of Quine is inadequate. Specifically I argue, first, that Quine's holism and his empiricism ((a) and (b) above) can be shown to be inconsistent. Thus, the two claims require reconstruction and not merely interpretation. Second, I shall argue that (c) — attempting to derive Quine's views on meaning from methodological considerations based on his views of scientific theories — fails to provide a cogent explanation of his characteristic objections to translation. These

4 A fourth theme, the inscrutability of reference, is not mentioned in this paper because of considerations of complexity. However, Quine's remarks on inscrutability (and the attendant thesis of ontological relativity) develop naturally and in accord with the general position sketched herein. I detail the development of the inscrutability of reference thesis in my "Semantics Without Foundations", unpublished.
problems with the traditional interpretation of Quine's theme show that the relation of (i) — (iii) must be reconstructed rather differently than the tradition has suggested.

Section II is constructive. In particular, I indicate three "tasks" for the purpose of reconstructing Quine's epistemology into a unified, consistent doctrine. The first task is to resolve the apparent conflict between theses (a) and (b) of the received reading — the Duhem thesis and the privileged place given to supposed evidence of the senses. The second task is to show why the conditions which suffice to make science objective nonetheless leave translation indeterminate — without a fact of the matter to call its own. This requires offering a reconstruction of Quine's argument for indeterminacy which differs radically from thesis (c) of the tradition. The third and final task is a "meta-task", i.e., an attempt to establish why the distinctions I draw in accomplishing the first two tasks successfully integrate Quine's three epistemological themes ((i) — (iii)) in a way the traditional reading cannot. I do not claim, however, that my means of rendering Quine's themes consistent is one which Quine would favor, although my reconstruction frees Quine from certain difficulties and is offered in the spirit of his epistemological enterprise.

More generally, I want to urge a reading of Quine which reconstructs his relation to a philosophical tradition (distinct from the aforementioned exegetical tradition). This "other" tradition is that of empiricism, understood as an epistemological doctrine that sees the foundation of knowledge as based on sensory evidence. Quine sees himself as part of an empiricist tradition extending at least from Hume. Yet, Quine has also been a trenchant critic of this tradition. The philosophical problem posed by Quine's own epistemology, I suggest, is that while Quine attempts to preserve what he understands as correct and valuable in this tradition (see fn. 5), Quine also transforms it. This is nowhere more evident, or so I argue below, than in the tensions between his holism and his alleged empiricism. It has yet to be appreciated how Quine has transformed the philosophical tradition with which he identifies himself.

The inability of the Princeton School's exegetical efforts to integrate these themes is a result of not considering the conditions of the possibility of objective evidence for Quine, or, alternatively, not considering why the notion of objectivity is, in fact, problematic for a Quinean epistemologist. My claim is that what distinguishes translation from science proper, and so meaning from truth, is that the Quinean conditions for objectivity fail to obtain for manuals of translation. That is, there are, in Quine's epistemology, necessary conditions for there being a "fact of the matter". What is difficult to appreciate is that these conditions are not related to the presence or absence of particular

observation sentences or to any specific set of methodological canons. Quine is an empiricist for whom impressions do not matter, a positivist for whom a criterion of verification plays no fundamental philosophical role. The necessary conditions of objectivity for Quine's empiricism are best described as transcendental.6

I argue in Section I that the exegetical tradition has misunderstood what is to be taken for granted and what is new in Quine's philosophy. In Section II I indicate how some of Quine's philosophical claims are to be interpreted in light of the changes he has rung on empiricist epistemology.

I

I approach the problems within the interpretative tradition by examining some of the leading examples of the Princeton School of Quine analysis (see fn. 2 above) and a recent book which attempts to provide an overview of Quine's epistemology, Alex Orenstein's Willard Van Orman Quine.7 Orenstein's book is a catechism of the accepted wisdom on interpreting Quine.8

Although Quine's earliest philosophical writings are well-known critiques of the positivists' notions of logical truth and analyticity, there is also a strong tendency to identify him with that tradition. In particular, there is an uncritical acceptance of Quine as an "empiricist". For example, Orenstein maintains that the dominant themes of Quine's writings of the last two decades -- the indeterminacy of translation of theoretical sentences and the inscrutability of reference -- are "continuous with earlier ones and in particular extend his distinctive empiricist methodology to questions about language". (0, 127) On this point Harman, Schuldenfrei, and Orenstein are in accord, insisting that the philosophically important core of Quine's view of meaning is his incorporation of the notion of meaning within an empiricist framework.9


8 Orenstein's effort has been cited precisely for its faithfulness to the received view of Quine's epistemology. See, for example, reviews by Joseph Ullian (Journal of Symbolic Logic 45, June 1980, p. 371) and Graham Priest (Philosophical Quarterly 29, April 1979, p. 173).

9 "One way to see Quine's work is an attempt to draw out the consequences of acknowledging that the theory of meaning too must be empirical." (S. 7) "(H)e does not attempt to convict his opponents of inconsistencies but argues rather that they hold bad empirical theories (of meaning)." (Harman, op. cit., 127)
Indeed, Schuldenfrei believes that the interest of Quine's views results not from any defense Quine provides of empiricism but in the systematic fashion in which Quine develops his post-positivist views. Here Harman concurs; on Harman's account, the Quinean notion of objectivity is validated only after one has the appropriate scientific theory in hand (H 349).

Yet if Quine's "empiricist" notions of meaning and evidence are based on science, they are unacceptable because of two related problems. First, Quine's claim that the foundations of linguistic and scientific theory is sensory evidence seems at odds with his "Duhemian" view of theories. That is, the empiricist tenets to which Quine professes allegiance posit a particular foundation of human knowledge while the view of theories which he defends is explicitly antifoundationalist.

Second, his pronouncements on the failure of objectivity of translation are not based on any claim that translation embodies a bad empirical theory; since Quine acknowledges that the method of translation is scientific, the motivation for distinguishing semantic theory (understanding "semantic theory" as "a theory of meaning") from the rest of science must be based on considerations independent of method. If there is no more to indeterminacy than the postulated possibility of a multiplicity of translations (as Harman suggests), then surely Quine's critics are correct when stating that Quine has not shown that there is a difference in kind between theories of translation and theories in natural science. For the alleged indeterminacy proves to be only under-determination. To interpret Quine's epistemological themes as direct or unambiguous consequences of his empiricism ignores both the problematic character of his empiricism as well as the nature of the problem imputed to translation. I shall examine these points in turn.

With regard to the first problem noted above, I have elsewhere detailed the prima facie conflict which arises when Quine's Duhemian view of theories is married to his favored empiricist tenets. (PI §1) The problem, in brief, is that if, given the Duhemian view, all sentences of a theory have their meaning and their evidence only as a related bloc, then there ceases to be the usual type of philosophical distinction between the empirical and the non-empirical (although there remains an intra-theoretical distinction between the two). That is, the old (positivist-empiricist) project for epistemologists was to justify science on the basis of, among other things, a notion of evidence untainted by the science it was to support. Quine's Duhemian outlook effectively eliminates the view that we are able to unambiguously identify the data independent of our scientific outlook.

This might suggest that correctness of the Harman-Schuldenfrei view that Quinean empiricism consists, in essence, of a certain formal philosophy of science plus certain substantive positions on how science ought to be done. (S, 11–12) Yet if Quine's empiricism comes to no more than an embrace of the data sanctified by science, then translation proves to be no less objective than other forms of scientific theory. The alleged indeterminacy cannot be sustained under this description of the Quinean enterprise. For example,
Schuldenfrei attempts to explicate the argument for the indeterminacy thesis in this context by suggesting that the sort of notions needed to make translation determinate are not a proper part of the Quinean enterprise.

What we are doing in Quine's epistemology is specifying the language in which the world is to be described. That description is to be scientific. Sciences tend toward objectivity. The idioms with which determinacy of translation is connected ... are importantly subjective. What is more, they are not necessary for drawing a comprehensive world picture. (S, 10)

The suggestion, in short, is that Quine's views preclude accepting intentional notions, and so determinacy of translation. Yet a clear parallel problem also exists for the natural sciences. For as noted above, accepting Duhem precludes belief in a (theoretically) unsullied access to the "language of nature". And if we reject "givenness" along with propositions, then, e.g., chemistry has no greater claim to a fact of the matter than does translation. Rorty, for example, charges that Quine's choice of physics as "paradigmatically matter-of-factual" is "purely aesthetic". (PMN 203) Or, to put the matter another way, since what have been rejected are "givens" both of the extensional and intensional sort, there is no distinguishing translation from other forms of inquiry on this point. Yet that is precisely what Schuldenfrei attempts to do.

Schuldenfrei thinks that Quine's epistemology is based on a particular view of science. However, Schuldenfrei believes that Quine does not argue for this view of the scientific enterprise. Rather, Quine shows how his view, when developed, yields a consistent and unified epistemological theory. The fact that his view of science yields such a theory is Quine's only "defense" of the view according to Schuldenfrei. The "argument" is clearly circular but we are to be impressed by what the circle encompasses.

Intentional idioms are ruled out of science on the basis of the generalization that science ("real science", that is) tends towards objectivity ("real" objectivity). But the generalization that science tends toward objectivity in this sense depends on not regarding as science those disciplines which deal with intentional subject matter in intentional terms. This is one way to bring out the circularity in Quine's position ... Its unavoidability should be clear from the fact that in doing theory of science, part of which requires saying that a science is, Quine admits to having to adopt a position on substantive science. It is only by "not scrupling to identify ourselves with a substantive scientific position" that we can do methodology; so the circularity is virtually explicit. (S, 12)

However, the circularity here is not benign but vicious. It engenders the following dilemma: Quine's application of his methodological strictures is either arbitrary or inadequate to support his claim regarding indeterminacy. Schuldenfrei's way out involves the first part of this dilemma. But this
strategy commits Quine to at least the following contradictions: a) that scientific method is and is not the final arbiter of truth and reality (for on Schuldenfrei's reading, the substantive ontological issue of what type of posits are permissible is mandated pre-scientifically by Quine for the scientists); b) the canons of science determine objectivity, but at least one discipline which conforms to those canons is not objective, viz., translation. Moreover, the notion of objectivity, as I have protested before, becomes as relevant to translation as to science; science, it would seem, it deemed objective by fiat. However, I believe that Schuldenfrei's discussion confronts the key point, namely, Quine's notion of "evidence".

The last point is particularly significant in light of a dilemma which Rorty imputes to Quine's attempt to integrate the leading themes of his epistemology: "the dilemma facing Quine is this: he should either give up the notion of 'objective matter of fact' all along the line, or reinstate it in linguistics." (IT, 459) Rorty believes this because he thinks the hallmark of science proper is the existence of, and the adherence to, canons (read: conventions) of a certain kind. (IT 447, 449) And on this point there is no distinguishing, e.g., chemistry from linguistics, no distinguishing, that is, the laws of a natural scientific theory from those of translation theory.

Harman's articles (see fn. 1) are the *locus classicus* for the second point, i.e., for the interpretation of the indeterminacy thesis from a methodological standpoint. Harman argues (H 141–147) that because there is a possible multiplicity of translations (since translations manuals are underdetermined), there is no basis for saying which of the non-equivalent translations is correct, or even that there is a correct translation.

In general there will be several possible schemes of translation that satisfy all reasonable conditions on such a scheme, yet provide non-equivalent translations. Therefore we cannot speak of translation apart from a scheme of translation; radical translation is always indeterminate; and consequently the postulation of meanings or propositions is not vindicated by the possibilities of translation from one language into another. (H 144)

This characterization is widely accepted and it is the basis for many criticisms of Quine's view. Rorty's recent critique of Quine is based on just this interpretation of Quine's indeterminacy thesis. And Rorty's critique, in turn, has been praised for its incisive analysis of the problem with Quine's indeterminacy

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10 In general, I use the term "methodological" whenever the interpretation involves an appeal to the manner in which a theory of translation is constructed in order to distinguish between a theory of translation and a theory in natural science. My claim throughout is that there is no relevant distinction to be made between translation and science on methodological grounds.
thesis. Harman's account is so pervasively accepted that it is taken to be Quine's account, and a (perhaps) decisive criticism of the soundness of Quine's argument as Harman presents it is understood to be a decisive criticism of the Quinean claim that there is a special problem with regard to the notion of meaning.

The manifest tension between Quine's holism and his empiricism should make one suspicious of any interpretation of his indeterminacy thesis which proceeds straightforwardly from these themes. A clear example of the difficulties this approach incurs is Orenstein's attempt to develop Harman's account (thesis (c) of the received reading). Orenstein (who is not always careful to distinguish under-determination and Quine's Duhemian holism) argues that since the available empirical evidence fails to be compatible with a unique translation, this engenders what Quine calls the indeterminacy of translation.

That is to say, even given that we work within one physical theory (undetermined [sic] as it is), when we turn to translate or conjecture about similar physical theories couched in a radically foreign language, there is no way empirically to characterize the foreign theory, according to Quine. (O, 136–137)

What is the much sought after difference between physical theory and a "manual of translation" (a particular theory of meaning for a language)? This depends on how we interpret Orenstein's statement that "there is no way empirically to characterize the foreign theory". If Orenstein means that we literally cannot formulate any translation, i.e., cannot produce a theory of meaning for a given natural language, this is certainly not a claim that Quine ever makes. If the suggestion is that there is no way to characterize empirically the foreign theory in order to unambiguously determine what the foreigner really means (as distinct from a meaning we impute), then Orenstein has missed Quine's point. Quine is explicit in his insistence that the issue is not one of unknown facts; the claim is that there is nothing to be known, i.e., there is no one true translation: "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." Perhaps Orenstein believes that indeterminacy occurs in translation and not in physics because if we "determine" our physical theory (perhaps by simply deciding to work within one theory and to ignore compet-

11 Richard Bernstein remarks that "many critics have argued that Quine's later work, especially his reflections on the indeterminacy of translation, reveals a blatant contradiction -- or at least a deep tension -- with his own pragmatic and holistic arguments. Rorty locates and specifies this tension better than anyone else." From "Philosophy in the Conversation of Mankind" Review of Metaphysics June 1980, p. 757.

itors), we still, it turns out, do not “fix” or “determine” translation. Thus, while both physical theory and translation may begin as under-determined, translation remains so even if physics does not. But, at best, this interpretation formulates the indeterminacy thesis as an empirical hypothesis, viz., the hypothesis that translation remains unfixed by a determining of physics. However, this suggestion accords neither with the “in principle” cast which Quine gives to indeterminacy nor with Orenstein’s own characterization of the indeterminacy thesis, i.e., that “Quine’s empirical theory of translation has shown . . . that it makes no sense to speak of language independent meanings”. (O, 132) But if what we are entertaining is only a hypothesis about the future course of science, about what notions remain free of the nomological net of physics, then we have been given no reason to think that Quine has shown anything of the sort. In any case, the “derivation” of indeterminacy from one or both of the others (the Duhem thesis and the under-determination thesis) is certainly not as straightforward as Orenstein believes.

Surely Orenstein, Schuldenfrei and Harman are correct in believing that Quine’s arguments, whatever they are, are meant to torpedo any “hint of there being some free-floating, linguistically neutral meaning” which propitious translation makes plain. The synoptic views propagated by the tradition render Quine’s position philosophically vacuous because they would have Quine assume precisely what needs to be shown, i.e., that a “scientific” account of meaning, however limited, represents the very best sense of meaning we can ever hope for. The common error, I have suggested, is a failure to appreciate how difficult it is to state why even natural science is “empirical” — has a “fact of the matter”. Until this is understood, it remains unclear why translation fails to have a factual basis to which to appeal. Failure to clarify this point engenders, in the cases considered so far, not only a general inability to integrate and/or distinguish the three major Quinean themes noted at the outset but also a collapse or a denial of a distinction between indeterminacy and under-determination.

Perhaps the single most important reason for seeking an alternative to the traditional reading is that the traditional reading does not suffice to undercut Quine’s target in all his discussion of indeterminacy, viz., the “museum myth” view of meaning (OR 27). According to the museum myth, meanings are made determinate by virtue of what the speaker “has in mind” or by virtue of some correspondence between words and the world (the ‘Fido’-Fido principle). On the usual reading of Quine, the argument for the indeterminacy should, at best, convince us that our choice of physical theory will not suffice to limit our choice to all but one translation manual. Yet the confirmed believer in

The determinacy of translation can accept that account of the situation and still insist, consistent with what Quine has to say, that it makes sense to inquire after a "science of intentions". In other words, if the point of Quine's argument is to dispel the museum myth, then the museum myth should somehow be shown to conflict with the claim that translation suffers from being "doubly" underdetermined (once qua theory, and again even after a physical theory has been decided upon). But the museum myth is not obviously inconsistent with such underdetermination.

Consider the following formulations of Quine's claim that translation is indeterminate.

(from WSOS p. 304, emphasis mine)

A conviction persists, often unacknowledged, that our sentences express ideas, and express these ideas rather than those, even when behavioral criteria can never say which. There is the stubborn notion that we can tell intuitively which idea someone's sentence expresses, our sentence anyway, even when the intuition is irreducible to behavioral criteria. This is why one thinks that one's question 'What did the native say?' has a right answer independent of choices among mutually incompatible manuals of translation. In asking "But why should all of this occasion any surprise or concern?" Chomsky did not dismiss my point. He missed it.

(from WO pp. 75--76, emphasis mine)

May we conclude that translational synonymy at its worst is no worse off than truth in physics? To be thus reassured is to misjudge the parallel. In being able to speak of the truth of a sentence only within a more inclusive theory, one is not much hampered . . . In short, the parameters of truth stay conveniently fixed most of the time. Not so the analytical hypotheses that constitute the parameters of translation. We are always ready to wonder about the meaning of a foreigner's remark without reference to any one set of analytical hypotheses, indeed even in the absence of any . . .

(from ORI pp. 180--181, emphasis mine)

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. (He also says, a little later in this article, that the problem is not one of "hidden facts").

Rather than continue to multiply examples, let me say what I take these remarks to establish. They establish, on my reading, that Quine's claim is not an epistemic one vis-a-vis incompatible manuals of translation tied for first place on the basis of available evidence. The problem is not that we have no rational criteria by which to choose among such manuals (although that fact,
should it obtain, would be a problem, but not the one Quine is concerned about in arguing for indeterminacy). The point being made in each of the passages cited is a semantical/metaphysical claim, viz., that the worry about which translation scheme is correct is a pointless worry because there exists (even if unknown) no one correct translation. Quine’s worry, I suggest, is that we are apt to believe that a person knows what he or she means, even if we do not, and that the “best” translation is precisely the one which would capture just their meaning. But to believe this is to accept the museum myth.

The argument for indeterminacy, if it is to be compelling, should show that this worry about which translation properly captures what the native (or I) “really” meant is pointless. And the argument, in order to do this, must show that the assumption that there is a determinate meaning is implausible. To show that there exist competing translations (which Quine never actually shows) would only underscore the difficulty of the supposed problem of finding the correct translation; it does not show that the intuition that there exists one such translation is one whose significance is to be doubted.

In other words, we think we know what we mean (in the sense Quine opposes) when we make certain statements. To be told that there is “no fact of the matter” seems to suggest that, perhaps, behavioral evidence does not always determine what it is another interprets me as saying. But when Quine claims that indeterminacy applies to homophonic translation — our understanding of our own utterances (see remarks on homophonic translation OR 46f) — surely this is deeply counter-intuitive. Similarly, the suggestion that a manual of translation is best understood as a method of extracting certain behavioral responses from others by means of the appropriate stimulations is also counter-intuitive.

Why does the problem arise, i.e., the problem of having to undercut the museum myth given Quine’s other assumptions? As I read Quine, the problem develops in the following way. Given Duhem, we must drop the claim that our scientific theory is isomorphic with, or somehow a mirror of, Reality. Quine explicitly rejects, e.g., the Peircian view that we approach truth as a limit by continuous application of scientific method to experience. (WO 23) We are left with a very wishy-washy type of “realism” with Quine; he will call scientific posits “real” certainly, but quickly adds that all this means is that statements affirming the existence of such posits are assigned “true” under the intended interpretation of current theory. However, even though Duhemian considerations suffice (at least for Quine) to prevent us from being realists of some classical stripe (and so believing that our theoretical posits correspond to the way the world really is), this consideration does not (obviously, anyway) cut against what might be called “intentional realism”. For we can accept the Duhem thesis and accept the Peircian claim that the meaning of a sentence turns on what evidence we have for its truth and yet insist that translation is determinate, even if unknowable. What must be shown is how the premises Quine cites counts against intentional realism and not just classical realism.
Quine's position evolves in the course of his attempts to salvage what he deems important and viable from the old empiricist notion of evidence.\textsuperscript{14} The important point is that the problems which face such a salvage attempt are the ones which Quine so forcefully poses for the old empiricism: the problems raised by his critique of the analytic-synthetic distinction, on the one hand, and, on the other, his embrace of a Duhemian holism. The result of this critique is to make problematic whether there is an account of evidence distinct from what current science endorses—in other words, if there is a notion of evidence which validates the intuition that science is tied to the natural world. A defense of such a concept of evidence would be philosophically significant because it would provide a theory-independent touchstone for inquiry. In other words, it would explain how our beliefs about the world are objectively anchored in experience. Moreover, with the appropriate concept of evidence in hand, one could hope to adjudicate the claim that translation lacks a fact of the matter.

In order to integrate Quine's various themes I present them as part of an argument. The conclusion states, in effect, that there are no "facts of meaning" as there are facts of nature. Such an argument precludes, on pain of triviality, assuming the exclusive correctness of a scientific method which rules out intentional entities. This argument represents the core of my proposed reconstruction of Quine.

My argument in outline is as follows:

(A) Given holism, there is no making sense of the notion of truth or of the notion of meaning \textit{apart} from one or another theory. (The concept of truth is, as Quine likes to say, immanent, i.e., explicated only within a given scientific theory.)

(B) From \textit{within} our "web of belief," we posit an intersubjectively available stimulus to which speakers or potential speakers of a language are jointly capable of responding. This shared stimulus is a necessary condition of language learning. That is, in order to explain the social and public character of language, and the fact that it is teachable, we assume that there is a fact of the matter to which we respond and around which our use of language is initially co-ordinated. (The argument for this claim involves appreciating what I call the "paradox of language learning" and understanding the significance of this paradox for anyone who subscribes to a holist \textit{cum} empiricist view of knowledge. The argument is detailed in PL, especially pp. 355—363.)

\textsuperscript{14} Quine insists that empiricism, as he understands it, is properly understood as "a theory of evidence". See his \textit{Theories and Things} (Belknap Press, 1981) p. 39 (Hereafter cited at TT.) I take it that Quine's point is that empiricism does not underwrite the claim that our theories correspond to the way the world is. Rather, empiricism, \textit{qua} theory of evidence, supports our belief in a loose (under-determined) fit between our sensory experience and the way our theory explains the way the world is. The \textit{truth} of a sentence is determined by its place in our theory. Our acceptance of a theory is based on our belief that it accords with and explains experience better than any of its competitors.
(C) Given that we must assume there is an intersubjective fact of the matter as a necessary condition of language learning, there is a warrant for a belief in objective evidence which arises from considerations within our language/theory.

(D) Natural science is about this intersubjective/public domain. Hence, theories in natural science are about (have) a fact of the matter.

(E) A belief in a fact of the matter for semantic theories is not warranted by appeal to any absolute standards (given (A)); moreover, such a belief is not warranted within the Quinean web as a necessary condition for there being a public and teachable language. (See PI 363–367) We “misjudge the parallel” (WO, 75) between the relativized notions of truth and of meaning if we think that the internal considerations which mandate attributing a fact of the matter to scientific theories also extend to semantic theories.

(F) Since there is no warrant, either internal to theories or external to them, for attributing a fact of the matter to semantic theories, such theories are indeterminate, i.e., not ultimately about “facts” in the way we are to assume that scientific theories are.

Together (A)–(F) represent the argument which I suggest a Quinean should assert in defense of the indeterminacy thesis. (I show in “Semantics without Foundations” that Quine is not consistent in his characterizations of his own argument. The version I favor is the one Quine gives at OR 80–81.)

This argument proceeds without appeal to a methodological distinction between science and translation, thus avoiding the pitfalls encountered in the traditional formulation. In addition, the argument is consistent with the Duhem premise. My analysis indicates how the requisite Quinean conception of evidence develops naturally from within a holistic view of language and knowledge.

Indeterminacy obtains, on my account, due to an inability to warrant an assumption in the case of translation which is warrantable for the natural sciences. This (the warranting in the one case and not the other) is the explanation for the asymmetric relation between truth relative to natural science and meaning relative to semantic theory. We “misjudge the parallel” between science and translation when methodological similarities are taken to be all important, i.e., when we believe, like Rorty, that from within the web of belief all posits look alike, be they mental or physical, extensional or intensional.

What I offer in the remainder of this section is an explanation of the first three premises of the foregoing argument. The fourth premise—(D) above—I, following Quine, simply take to be a true statement about the natural sciences. The final two steps in my argument presuppose the first four.

The question of interpreting (and ultimately integrating) Quine’s themes has been transformed, in light of the above considerations, into a problem of what are the preconditions of a theory which are germane to establishing the presence or absence of a “fact of the matter” of intra-theoretically affirmed
beliefs. What sense can be made of “objectivity” apart from an appeal to the conventions/canons of one or another discipline?

The objectivity of natural science is established by adducing what preconditions are necessary for there to be a language like ours, viz., one that is socially shared and teachable and yet comprises a theory in Duhemian sense. (Recall (B) above.) It is the learnability/teachability of language which Quine consistently points to as the epistemologically significant trait. But why is the teachability of language of such moment for Quine’s philosophic position? The answer lies in what I have called the “paradox of language learning”. (PI, 355–356) The paradox is as follows: we cannot explain how, on the basis of the available evidence, a child arrives at his or her mastery of the more theoretical portions of language. For there is no rational reconstruction of the process which takes us from sentences learned first to the point where we have become accomplished language users. (See references in fn. 15.) Hence, it seems, Quine is correct in claiming that only sentences taken as an inter-related group have meaning and evidence for their truth. However, if we maintain that both the sentences accepted as true in natural language and natural science have their meaning and their evidence only as a related bloc, then someone with no knowledge of a language, e.g., an infant, should find any single sentence to be meaningless, and so incomprehensible. But children initially learn to speak by first learning single sentences. So Quine must be wrong in claiming that sentences have significance only within a theoretical context. The paradox, in short, is generated by contrasting how language is normally learned with our inability to reconstruct how this learning process is possible given the available evidence.

In order to resolve the paradox, I argue that one must draw an admittedly unQuinean distinction between the stimulus meaning of an observation sentence and the Ding an sich which is the causal source of the stimulus meaning. (See PI) Stimulus meanings, and the concomitant observation sentences, are already identified within a theory-laden account of the world. Yet it is because these stimuli are supposedly causally connected to the world, a world which we cannot characterize apart from our theory of it, that they serve to tie our remarks to some objective basis. Without this objective basis, observation sentences could not function as the “gateway” to language, and so the paradox engendered by the holism would prove crushing. In short, I am claiming that the notion of objectivity has two distinct foci in Quine’s epistemology. The commonly remarked upon one is the one which takes observation sentences as determining what evidence there is for any theory.

Yet this notion, if completely definitive of objectivity for Quine, fails to explain why physics is matter-of-factual but translation is not. For observation sentences are products of theories, and the field linguist has, in this respect, observational data. The added (and uncommon) notion of objectivity which I am introducing involves a thing-in-itself as the causal source of experience. And it is only what I am calling the uncommon notion of objectivity which will justify Quine's distinction between meaning and truth.

The positing of observation sentences includes the assumption that these sentences have theory-independent causal meanings (whatever the stimulus meanings are), although we can never say what that meaning is. Holism precludes the possibility of looking past the veil of theory. The philosophically critical point is this: the claim that there are some sentences that are observation sentences is defended by arguing that they are a necessary condition for our language to be as it is. This is the essence of the argument for premise (C). What makes observation sentences observational, however, is some unknown and unknowable source of shared stimulations. The thing-in-itself which makes observation sentences observational is promulgated as a formal (necessary) condition of learning. (See Quine remarks on empiricism *qua* theory of evidence in IT, 39–40) However, the claim that any specific sentence is an observation sentence is an empirical hypothesis formulated within the scientific study of language, and so subject to correction and revision as are all our scientific statements.

Quine claims that no particular sentence, even those which current science believes to be observation sentences, is incorrigible; this claim is compatible with the claim that some sentences must function in this fashion. But the general, philosophic claim can leave unspecified which sentences of our corpus are observational. The general claim sets a necessary condition for allowing language to be learned under social circumstances; determining which sentences are "observational" is a matter of educated guess rather than philosophic analysis. The existence of an empirical (sensory) correlate — the thing in itself — becomes a formal (necessary) condition for there being a language; the objectivity condition, then, is not experimentally ascertained, but, as it were, logically mandated in order to account for why languages are learnable, the Duhem premise notwithstanding. The Duhemian constraint is epistemological, i.e., that we cannot know independently of a theory which are the sentences with theory independent meanings.

This solves the first task of interpretation noted at the outset. Granting that there must be sentences which function more or less as Quine says observation sentences do, the tenets of empiricism to which Quine subscribes no longer conflict with his Duhemian outlook. For the claim that language is based on responses to a world language-users share is a necessary condition for having a socially shared language like ours. That is, the claim that there are observation sentences represents an inference to the best explanation about how language is taught and so shared. Quine's claim about observationality, on my interpretation, is not a claim that some particular sentence in our
corpus of accepted truths is incorrigible. *Hence, there is no conflict with the Duhem thesis.* Insofar as the language we learn to speak, and which natural science sets out to refine, is about what there is, the scientific study of language attempts to articulate those factors which first give shared signs their social dimension — what Quine calls the stimulus meaning of an observation sentence.

Of course, any characterization of the stimulus meaning occurs within the theory (language) we have learned to articulate, and there is no path back to the sensory starting points. *This is how acceptance of the Duhem thesis transforms empiricism.* But if the stimulus-meaning-in-itself is fated to remain a transcendent object of scientific inquiry, there remains nonetheless a formal guarantee that there is a fact of the matter about which to inquire; the existence of a “theory-independent” world is a necessary condition for there being a language like ours. Insofar as natural science probes what is essentially public about our social perceptions, it has a fact of the matter. This science is Duhemian in nature; whether this science is also genuinely under-determine, i.e., whether there can be logically incompatible and empirically equivalent theories which cannot be made equivalent by any possible reconstrual of their predicates, is another question. However, even if theories are not what I (following Quine) have just called genuinely under-determined, this does not affect their Duhemian nature. The Duhemian thesis does not require that theories be under-determined, for single theoretical sentences, e.g., “protons have mass”, lack empirical content *qua* single sentence whether or not there is a genuine alternative to current theoretical physics. Nor does the Duhem thesis of itself entail that theories will be under-determined, for the Duhem thesis is a thesis about the truth conditions for single theoretical sentences, while the under-determination thesis is one about the ability to formulate entire theories that satisfy certain desiderata, in particular, that the competing theories account for all the available evidence in logically incompatible fashions.

*The saving grace of science is found in a transcendental argument for a necessary condition of language.* The sin of semantics, conversely, is that no parallel necessary condition can be adduced in its favor. The failure to show that a *shared semantic theory* (Fregean sense, Platonic Ideas, and other artifacts of the “museum” view of meaning) is a necessary condition for language learning is precisely where the parallel between physical theory and semantic theory ceases (WO, 75–76). That is, we need not assume that there exist, prior to our efforts at “translation” — our attempts to formulate a semantic theory — a semantic theory which speakers of a language possess. What frustrates the search for the right translation is not the bounds of sense. The point is that there just is no meaning in itself which, were we more God-like, we might glimpse lying beyond the veil of human perceptual capacities. The need for there necessarily lying beyond the veil of human perceptual capacities. The need for there necessarily being a shared semantic theory is not established by the argument which applies to scientific theory. The considerations which give sense to calling science objective are simply not germane to the sense of objectivity wanted in semantic theory, i.e., to showing that there is a “right” translation, even if both unknown and unknowable. The “in principle” cast
given to the indeterminacy claim is justified by this construal, for the preceding account, if cogent, removes the question of whether there is a "fact of the matter" to semantics from the realm of empirical inquiry. The central epistemological issue is what is a necessary condition for there being a teachable language, given the Duhem premise and the attendant paradox of language learning. Further, Quine's proposed condition is established by a philosophic argument which makes no special appeal to received scientific doctrine and so is not circular in the way in which Schuldenfrei's explication is and sometimes Quine's own analysis threatens to be. The above accomplishes the second task of interpretation by suggesting how science but not translation satisfies the formal conditions required for objectivity.\(^{16}\)

The reading I have suggested also accomplishes the third task — the "meta-task" — of interpretation, i.e., the task of integrating and preserving Quine's theses. This is because it does not, like those who subscribe to the Princeton School, ignore the apparent contradictions in Quine's exposition of his theory of knowledge, nor does it ignore the crucial distinction between Quine's "theory of theorizing" and his naturalized epistemology — his epistemology done within science. The foregoing interpretation of the indeterminacy thesis does not reduce the indeterminacy thesis to a case of under-determination, as does Harman's reading; my interpretation, if viable, establishes just the "in principle" distinction between truth and meaning, between, that is, physical science and translation, which makes Quine's indeterminacy thesis of philosophic interest.

What I have attempted to do is to follow out Quine's thought with regard to where the parallel fails between physical theory and translation theory. I've done this by suggesting that there is one metaphysical (or metaphysical-like) claim which Quine makes from within his Duhemian view of physical theory and that he denies that a parallel claim is supported within translation theory. If this account is plausible, then I have shown: a) why Quine says that physical theory and not translation theory has a fact of the matter; b) why observation sentences, which are the evidence for physical theory, are simply not relevant in the case of translation theory; c) why we are just mistaken to think that there is an answer to the question, "What did the native mean?" a question we are tempted to ask even in the absence of a specific manual of translation for native; d) that the parallel between physical theory and translation fails for deep reasons and not because of some methodological

\(^{16}\) The foregoing analysis presupposes, but does not defend, a sharp distinction between Quine's remarks on epistemology generally, his "theory of theorizing", and his remarks which properly apply only to a naturalized epistemology, i.e., epistemology seen as part of natural science. His remarks on the indeterminacy presuppose the former perspective, his remarks on the inscrutability of reference the latter. I have argued elsewhere (TN) that Quine confuses and conflates two distinct notions of epistemology within his writings and that the philosophically interesting and important sense is to be found by attending to his remarks regarding the epistemological import of the Duhem thesis. Given his theory of theorizing, Quine then goes on to sketch some further philosophic implications of epistemology carried out within a scientific framework.
quirk or a priori restriction which Quine places on translation schemes; e) my reconstruction of Quine's argument would defeat the intentional realist, or, at least, show why the assumption of intentional realism (and so an important variant of the museum myth) is not in harmony with Duhem and Peirce.

Yet if the reconstruction of Quine's position, and so his practice, which I have developed is right, then the real twist to Quine's epistemology is that Quine's most interesting epistemological arguments are not a part of empirical psychology. In his epistemology, Quine emphasizes and develops the fundamental importance of the social constraints which influence the development of knowledge (see especially FOM). Yet his reductionist epistemological programme ignores the significance of these social factors. Ironically, then, Quine's most interesting arguments are also arguments for following his practice of examining "epistemology socialized" and against accession to his explicit proposals for the "naturalizing" of epistemological inquiry.