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ON THE NATURE AND RELEVANCE OF INDETERMINACY*

1. Perhaps the strongest case to be made for indeteminacy of translation is the plethora of emerging interpretations of Quine's work. In the belief that Quine is not so inscrutable as all that, we should like to try to separate and develop one important strand in the extant Quine – that which is properly called the indeterminacy of translation – and consider its role in Quine's case against linguistics.

A translation is a theory that the sentences of a given language map in a specified way onto sentences of a home language under a certain relation called synonymy. Given a translation, classical semantics would posit for each translation relation a meaning or idea shared by the alien sentence and its home translate. Now, three distinct theses appear in Quine regarding translation and meaning:

(1) Applying all the canons of good scientific methodology, the totality of possible evidence supports equally well more than one translation of a given language.

(2) There are no translation relations except relative to a given manual of translation, or translation theory.

(3) There are no translation relations, period. (Thus there is nothing there, no fact of the matter, for translation to be right or wrong about.)

And Quine countenances three parallel theses about meanings shared by translates, and also three parallel theses about syntactic structures.[1]

Thesis (1) is the thesis of what Quine calls *indeterminacy* of translation. It is an epistemological thesis concerning what we can tell about a man's meaning. Such indeterminacy arises from the guesswork character of inferences from observation to theory; indeterminacy can, and for Quine does, infect all other kinds of theory as well as translation. We shall develop the notion by appeal to Peirce, comparing Quine's epistemology with Peirce's.

Thesis (2) is a metaphysical thesis of what may be called the *relativity* of translation, the relativity of translation relations to a translation manual or theory. Such relativity is also for Quine generalized to all other theory:

* Thanks are due Professors Jerrold J. Katz and W. V. Quine for advice on earlier drafts. Of course, our opinions are not always theirs.

[1] See Quine's discussion of syntax in 'Methodological Reflections on Current Linguistic Theory', *Synthese* **21** (1970), 386–398.

Foundations of Language **12** (1974) 49–71.

what there is and what it is like is relative to our background theory or conceptual scheme.[2] The result of such relativity is that "both truth and ontology may in a suddenly rather clear and even tolerant sense be said to belong to transcendental metaphysics" (*OR*, 68). Indeed, what we might call Quine's *metaphysical relativity* – encompassing relativity of both ontology and truth (or fact) – is apparently a species of what some philosophers have called *transcendental idealism.*

Relativity is a soul brother of indeterminacy, but probably not much closer kin. Relativity seems clearly motivated in Quine by indeterminacy, but it might also perhaps be sought from somewhat different directions. Nor is it entailed by indeterminacy. One could hold a flat-out realism while claiming our knowledge of reality is inescapably indeterminate; and one might then seek a complicated relation of theory to reality (i.e. truth as correspondence). Our point here though is meager: theses (1) and (2) are distinct and logically independent, though somewise (1) is motivation for (2).

Thesis (3) is a stronger metaphysical claim than thesis (2). There simply are no objective translation relations or meanings in reality, Quine holds. The rejection arises, for Quine, from the indeterminacy of translation. Here Quine sees a disparity between linguistic theory and other theory such as physics; whereas both admit of indeterminacy, the posits of linguistics are objectionable in ways that the posits of physics are not, though both are relative to arbitrary choices of theory. We shall argue that the disparity should for Quine be due not to a disparity of indeterminacy (for there is none) but simply, as Quine also alleges, to explanatory impotence or inferiority in the posits of linguistics. Both meanings and translation relations suffer so.

Our project here, then, is to develop a detailed account of indeterminacy by comparing Quine's epistemology to Peirce's, and to disentangle indeterminacy from Quine's case against meaning and translation.

2. For Peirce empirical inquiry paradigmatically begins with a surprising or unexpected observation (6.468ff).[3] The unexpected may be an irregularity, or it might be a surprising uniformity (6.12, 6.612). Surprise plunges the inquirer into doubt about his beliefs and prompts him to search for new beliefs through which his observation is understandable and explained (5.373, 8.270). Thus he hunts for an hypothesis from which his observation

[2] Cf. Quine, 'Ontological Relativity,' *Ontological Relativity* (Columbia University Press, New York, 1969). Hereafter this volume is cited as *OR*.

[3] References to Peirce are all to *The Collected Papers of Charles Sanders Peirce* (ed. by Charles Hartshorne, Paul Weiss, and Arthur Burks), Harvard University Press, Cambridge 1931–1958, citing volume before and paragraph after the decimal.

would follow. Settling tentatively on such an hypothesis, the inquirer then proceeds to derive observational consequences from it (6.470). If the hypothesis is worthy of the name, it will provide many more observational consequences than can possibly be checked (5.467, 5.597f, 6.525). The inquirer selects a batch of these and proceeds to test them. If they all turn out true, then the inquirer is liable to conclude that *all* testable consequences of this hypothesis are true, and hence that the hypothesis itself is true (2.755, 2.775, 7.203). If some of the consequences turn out false, then the hypothesis is discredited. In this case the inquirer must begin again with his search for an hypothesis which explains his observation (6.472).

The various stages of inquiry are distinguished, according to Peirce, by the different modes of inference involved. The search for and tentative settlement upon an hypothesis is an inferential process Peirce often calls *abduction*. The derivation of observational consequences from an hypothesis takes place by *deduction*. And our conclusions about all the observable consequences are arrived at by *induction* on a sample of observations. Deduction is for Peirce a very firm kind of inference. If the premisses of a good deduction are true, then so must be the conclusion (2.447). Deductive goodness is a matter of *form* or syntactic structure, structure that Peirce spent much of his life developing (3.328). Induction is not entirely a matter of form for Peirce. Inductions vary in *strength* according to the size and diversity of the sample class (2.780) and the "reconditeness" of the traits generalized upon (2.634, 6.526) – what Goodman has called their "projectibility." Their strength is thus not a question of syntax, though all inductions have the form of a generalization. Abductions have very little to do with form. The conclusion of a good abduction must explain the premiss, and so must imply it (6.273, 6.469), but that is apparently the only formal constraint which can be placed on abduction. Consequently, there is very little to guide abduction, and it is, as Peirce says, essentially *guesswork* (7.219).

Since abduction is just guesswork, it is a weak kind of inference, one in which we would be wrong to put very much faith (2.642). There are, Peirce notes, inconsistent ways of explaining events (7.202), and an abductive conclusion at any given stage of theory-construction is usually only one of a billion or trillion possible acceptable hypotheses (7.38, 5.172). There are, however, some informal guidelines we can follow in choosing hypotheses for testing and in making tentative selections of theories. Three important classes of traits can help make our abductions educated guesses. The first class of traits includes all those any hypothesis *must* have in order to achieve explanation. One such trait is verifiability (5.197, 5.597): if no event would count as evidence either for or against the hypothesis, then it is unexplan-

atory. A second class of traits guide abduction by assaying which hypotheses have the greatest "antecedent likelihood" (2.511n, 2.662, 5.599, 7.220), that is, by telling us before we begin to test which are most likely to be true. Among such traits Peirce includes psychological naturalness (6.477, 5.591), which is a kind of simplicity (5.60); he also includes generality (6.525, 7.221, 7.410), conservatism with regard to altering antecedent beliefs (1.120, 4.1, 4.37), and modesty of the proposed story (5.26, 7.92). A third class of traits which guide our adoption of hypotheses relates to the economy of research. The more easily, quickly, and cheaply tested an hypothesis, the more preference it should be given for first testing (5.598). The less plausible a theory, generally, the less trouble it will be to refute (6.533, 7.206). Another trait contributing to economy of testing is the precision of the hypotheses' predictions (1.85). Among the various traits Peirce mentions, verifiability is an absolute demand placed on every hypothesis, whereas the other traits considered guide only our initial adoption of a theory for subsequent testing and not our final choice from among hypotheses which have been equally well confirmed (5.594, 5.599). In the end, only the fact of observation, only verification can tell us which theories are true and which false (1.634). So of course verifiability is required. But to the task of verification the traits in Peirce's second and third classes are irrelevant. These traits can of course guide us in pragmatic ways. Thus we might well prefer the simpler of two theories for no other reason than its comparative manageability, ease of calculation. And the second class traits can also reasonably guide our tentative adoption of a theory in the absence of definitive data, even in cases in which such data will be years in coming. But it is such data which must serve as the *final* judge of a theory's *truth*, not feelings of naturalness or considerations of economy.

With so little formal abductive guidelines, and with only testing as the ultimate tribunal, it would seem a fairly obvious claim for Peirce to make that our final system of beliefs is not uniquely determined by any amount of evidence we could collect. The point would not be merely that we can never test all the observational consequences of an hypothesis, and thus that there is always room for error in the inductive stage of inquiry. Indeed, suppose that all the observational consequences could be tested. It would yet seem a natural claim for Peirce to make that different systems of hypotheses might make equal sense of the world, i.e. might explain and predict all events equally well, whether they be actually observed or not. Since final choice of hypothesis is to be constrained only by verification and explanatory ability, there seems no obvious reason why we should not be left with alternatives even if we could somehow take all possible experience into account. In this sense final choice of theory would be *indeterminate*, under-

determined even by all possible evidence. But Peirce does not make this claim. In fact, as is well known, he claims just the opposite. If inquiry is pushed far enough, he says, it will terminate in the "one True conclusion," and every honest scientific inquirer will rest in that conclusion no matter what his initial beliefs were (5.407).

Determinacy of final theory is possible for Peirce because of an independent thesis he holds to be true: his pragmatism. *Pragmatism* for Peirce is the doctrine that the meaning of a sentence is determined by the conditional experiential expectations the sentence's truth would warrant (5.9, 5.400ff). Thus, two sentences have the same meaning, or "intellectual purport" (5.467), just in case we would be warranted in expecting the same experiential outcomes of *any* situation if we believed either of the sentences true. Any difference for experience spells a difference in meaning. Now, suppose there are different theories, taken as sets of sentences, which completely agree in all their predictions for experience. That is, the theories each lead to the very same expectations or predictions for experience, though perhaps through different routes. Two theories which so completely agree would for Peirce be identical in meaning. And so in an important sense they would be not two but one, different formulations of a single truth, the "one True conclusion" from their common stock of evidence. There is multiplicity, holds Peirce, only of notation, not of doctrine.[4]

We may suppose that any inductive gap between such theories was closed by the hypothetical exhaustion of all observations, requiring each theory to have got all the inductive generalizations on observation right. That sets the scene for the question of ultimate abductive determinacy. With Peirce's pragmatic theory of meaning, then, total agreement in observational truths guarantees identity of meaning, and hence identity of theory. That closes any abductive gap that might have existed between the theories at earlier stages of inquiry. In this way Peirce's pragmatism guarantees a unique, determinate final choice of theory.

3. Peirce's picture of the structure of inquiry is one which is familiar to most of us today. Many of its features have been canonized in The Hypothetic-Deductive Method, which is the starting point for most current discussions of scientific practice. It is a picture which, in general outline, Quine adheres to. Our theories, according to Quine, are prompted by macroscopic observations. These lead us to hypothesize about the invisible and inaudible structure which underlies the observable. Quine follows

[4] Though this paragraph makes Peirce sound like a holist, he of course was not one. See below, p. 56.

Peirce in labeling such hypothesis "guesswork."[5] But, again as for Peirce, informal guidelines can be given. These, it seems, fall roughly into the same three classes as for Peirce. Verifiability is for Quine as for Peirce an absolute demand on hypothesis:

> Otherwise the hypothesis predicts nothing, is confirmed by nothing, and confers upon us no earthly good beyond perhaps a mistaken peace of mind. (*WB*, 50)

Other virtues confer antecedent likelihood, or plausibility, on the hypotheses we have not yet confirmed (*WB*, 43). These include conservatism,[6] generality,[7] simplicity (*WB*, 45; *WO*, 19f; *WP*, 234), and modesty of story (*WB*, 51; *WO*, 21). These traits are for Quine, as for Peirce, not for settling issues of truth among equally well confirmed hypotheses, but are for deciding which seems the "more probable," which stands "the better chance of confirmation in future observations" (*WP*, 242). Consequently, simplicity, for instance,

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

Precision, for Quine, looks to be in Peirce's third class of theoretical traits, directing our research by considerations of economy. Precision enhances economy because

> The more precise a hypothesis is, the more strongly it is confirmed by each successful prediction that it generates. This is because of the relative improbability of coincidences. If a prediction based on a hypothesis just happens to come out true for irrelevant reasons, that is a coincidence; and, the more precise a hypothesis, the less room there is for such coincidence. (*WB*, 65)

However, where Peirce does hold, Quine does not hold that unfailing observation and adherence to scientific method is bound to lead to some one predeterminate conclusion. Rather, Quine thinks, even if the scientist were given once and for all the totality of observational truths, still the canons of abduction would allow several "logicially incompatible"[8] sets of acceptable hypotheses about the underlying microscopic reality.[9] The scientist's choice of theory is in this sense underdetermined by observation, even the totality of possible observation. This indeterminacy applies not only to our highly

[5] Quine and Ullian, *The Web of Belief*, Random House, New York, 1970, p. 43. Hereafter this volume is cited as *WB*.

[6] *WB*, 43; Quine, *Word and Object*, MIT Press, Cambridge, 1960, p. 20. Hereafter this latter volume is cited as *WO*.

[7] *WB*, 44; *WO*, 20; Quine, *The Ways of Paradox* Random House, New York, 1966, p. 234. Hereafter this last volume is cited as *WP*.

[8] Quine, 'On The Reasons for Indeterminacy of Translation,' *Journal of Philosphy* **67** (1970) 179. Hereafter this paper is cited as "Reasons."

[9] *WO*, 21ff; Quine, 'Philosophical Progress in Language Theory', *Metaphilosophy* **1** (1970), p. 11. Hereafter this paper is cited as "Phil Progress."

theoretical current physics; it already infects our common-sense belief structure (*WO*, 22). Quine is explicit in saying that indeterminacy emerges from the abductive stages of theory-formation, not from the inductive stages of verification, for there is indeterminacy even after the totality of observational truth has pulled in all inductive slack.[10]

So although Peirce and Quine are in wide agreement about the structure of theoretical investigation, they disagree over the important question of abductive slack in our best theories. It was because of his pragmatic theory of meaning, recall, that Peirce was able to hold that we can ultimately eliminate all but one set of hypotheses. Now, surprisingly, Peirce and Quine appear to agree even in this. Quine says that we ought to "recognize with Peirce that the meaning of a sentence turns purely on what would count as evidence for its truth" (*OR*, 80f). But now we ought to be able to duplicate Peirce's reasoning for Quine, concluding from the pragmatic theory of meaning that theory is determinate, and thereby conclude that for Quine too there is but "one True conclusion." But then of course Quine would be inconsistent, since he holds theory to be indeterminate.

Quine seems to have anticipated this charge:

> It may be protested that when two theories agree thus in point of all possible sensory determinants they are in an important sense not two but one. Certainly such theories are, as wholes, empirically equivalent. (*WO*, 78)

And then theory would seem to be ultimately determinate, just as Peirce held. Quine replies that "this account is fair enough, apart from its glibness on the topic of meaning..." (*WO*, 78). But not only does Quine not draw Peirce's conclusion of determinacy (*WO*, 23), Quine counts such empirical equivalence in favor of *in*determinacy. The ellipsed completion of Quine's quoted reply was: "and it helps to make the principle of indeterminacy of translation less surprising."

How does Quine avert the Peircean conclusion of determinacy? To begin with, let us readdress his acknowledgement of Peirce's theory of meaning:

> The Vienna Circle espoused a verification theory of meaning but did not take it seriously enough. 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. And most sentences, apart from observation sentences, are theoretical. (*OR*, 80f)

– and therefore indeterminacy of theory is "the natural conclusion." Here we find added to Peirce a dash of Duhem: meaning alligns with evidence, but beyond its observation sentences a theory faces its evidence only as a

[10] Quine, 'Replies', *Words and Objections*, (ed. by Donald Davidson and Jaakko Hintikka), Reidel, Dordrecht, 1969, pp. 302f. Hereafter this is cited as 'Replies.' Cf. also *WO*, 68.

whole theory. The result is Quine's *holism*: the smallest linguistic unit that bears empirical meaning is our total theory, "the whole of science." Thus:

> The statement, rather than the term, came with Frege to be recognized as the unit accountable to an empiricist critique. But what I am now urging is that even in taking the statement as unit we have drawn our grid too finely. The unit of empirical significance is the whole of science.[11]

Now, if we are holists and hold to Peirce's theory of meaning, we can and must be determinists it seems. For, different first-place theories will have the same empirical meaning, and so if the unit of empirical meaning is the whole theory then the theories will coincide in empirical meaning and therefore for Peirce in meaning, and therefore they will count as one. Conversely – and this is one of Quine's points against Peirce – if we are to be divisionalists, allocating empirical meaning to sentences individually, then we must be indeterminists. For the allocation may take place in any of several different ways – such is the crux of Quine's argument for indeterminacy of translation (cf. *OR*, 82). Peirce's failing was that he took neither of the viable alternatives, most often sounding like a divisionalist (5.9) and a determinist (5.384). Indeed, therein lies the "glibness" about meaning which Quine saw in the Peircean argument from the pragmatic theory of meaning to determinacy.

Implicit divisionalism, then, undercuts the Peircean argument to determinacy. But that does not explain how Quine himself avoids determinacy. For Quine endorses both the pragmatic theory of meaning and holism, which together entail that all final theories have the same meaning. But for Peirce, empirically equivalent theories count as one, yielding determinacy. The difference with Quine must be that empirical equivalence does not suffice for identity of theories. That is indeed the case. Quine believes that there are *logically incompatible* yet *empirically equivalent* first-place theories. And logically incompatible theories are surely distinct. So Quine finds room for indeterminacy of theory precisely because he finds more in a theory than its empirical significance. That is perhaps surprising because the spillover has to do with what is usually taken up by the traditional notion of meaning and it is that notion which is the object of Quine's critique of determinacy. We address ourselves to this spillover in Section 4. The upshot is that Quine cannot accept Peirce's pragmatic theory of meaning as it stands: meaning aligns with evidence, but not uniquely.

Let us take stock of our comparison of Quine and Peirce. Whereas Peirce endorsed the pragmatic theory of meaning, probably divisionalism and determinacy of final theory; Quine endorses an empiricist theory of

[11] Quine, *From a Logical Point of View*, Harvard University Press, Cambridge, Second Edition, 1964, p. 42. Hereafter this volume is cited as *LPV*.

meaning (which is different from Peirce's), holism, and indeterminacy. Peirce argued that the pragmatic theory of meaning entails ultimate determinacy: final theories are empirically equivalent; the pragmatic theory of meaning entails that theories are identical (in doctrine or meaning) if and only if empirically equivalent; and so therefore there is a unique determinate ultimate theory. Quine differs from Peirce on three points. First, Quine is a holist. Peirce must add holism to his pragmatism to insure uniqueness of final theory. Indeed, given the pragmatic theory of meaning, final theories are identical in doctrine if and only if holism obtains. Second, for Quine, empirical equivalence is not, as it was for Peirce, a criterion of identity for theories. Third, it follows that Quine's theory of meaning must diverge from Peirce's. And so for Quine empirical equivalence of final theories does not guarantee determinacy. The thesis of indeterminacy is that there are distinct empirically equivalent final theories that afford first-place explanation. But now, without clear identity criteria for theories, we are unclear what indeterminacy is. What Quine's criterion must be is not easy to say since he is cool to meaning beyond empirical meaning and yet empirical meaning does not individuate theories. But we shall try.

(There is a plausible alternative view of Quine. We might think that Quine accepts holism, Peircean pragmatism, the identity of empirically equivalent theories, and uniqueness of final theory, but then argues *reductio ad absurdum* against the conjunction of determinism with divisionalism by contending that divisionalism leads to indeterminacy, all the time speaking with the vulgar. Glibness about meaning might then be any uncritical appeal to the notion, any appeal besides one to stimulus meaning, or to empirical meaning. Most of the discussion of the next section, and all discussion subsequent to that, is independent of this question of interpretation.)

4. There is an intricate interplay in Quine between translational theory and theory in general. Given Quine's epistemology and his account of the enterprise of translation, empirical theory in general is indeterminate if and only if translation is indeterminate. Indeed, translation already plays a role in the individuation of theories. To the interplay we now turn.

Psychology is the science of human behavior. It includes as a proper part linguistic theory, the theory of human verbal behavior. The linguist standardly launches his enterprise by selecting a certain set of sequences of phonemes of his subject's language as grammatical or meaningful, and a subset of these as sentences. Which sets are chosen depends partly on further psychological theory and the ways in which the linguistic theory fits in with it[12] – on relations between beliefs, desires, and behaviors linguistic

[12] Cf. Quine, *Philosophy of Logic*, Prentice-Hall, Englewood Cliffs, 1970, pp. 19ff.

and otherwise. Settling tentatively on a list of sentences of his subject's language, the linguist may try to develop a *theory of meaning* for the alien language in the form of a *manual of translation* between alien sentences and sentences of his own language. The translation will be a pairing of each alien sentence with some one translate sentence of the home language, or perhaps with some small set of "equivalent" ("synonymous") sentences of the home language. Since the subject's sentences will be in principle infinite in number, the linguist can accomplish his task only by giving rules of some kind which translate alien sentences by appeal to a finite stock of alien sentence parts – phonemes, morphemes, or other lexical items. Thus the task of translation must include a theory of lexical structure, a *theory of syntax*, for the alien language. The theory of syntax will pick out certain lexical particles that contribute to the translation of sentences in systematic ways. The translation will then work on an alien sentence by assessing the semantic or translational import of the lexical particles picked out by the syntax and combining lexical particles of the home language so as to get a sentence like the original alien sentence in meaning. The linguist's resultant theory of translation will aid the bigger psychological enterprise by saying more surely what the subject believes, wants, fears, refers to, and so on. We should emphasize the holistic drive of the enterprise: it is the whole battalion of hypotheses that marches on, including syntactic, translational, and psychological hypotheses.

The linguist's efforts toward a translation will in outline conform to the hypothetic-deductive method, to Peirce's view of the stages of inquiry. He will start where his data are firmest and work upward and outward. He begins by making educated guesses as to what his subject sees and hears in certain situations, or under certain stimulations, and then he attempts to correlate those situations or stimulations with his subject's readiness to assent to or dissent from sentences queried in these situations. The subject's dispositions here will be more readily inferred and more revealing the more observational the sentence queried. For the more observational a sentence, the more the subject's response tends to be prompted by the current sensible situation, and the more he tends to revise his response with changes in the passing show. As the sentences the linguist tries to translate become less and less observational, his subject's responses to queries become less and less keyed to the current scene, and so there become fewer clues to translation. As the cases become harder and harder, the linguist relies more and more on his lexical hypotheses. He sees resemblances between structures in the more observational sentences which lead him to his lexical theory. And this theory in turn allows him to ascribe structure to the less observational sentences and thereby to translate them analogically. But how are the

lexical hypotheses reached? Fairly clearly, by abductive inference (cf. *WO*, Section 15). The linguist makes a bold guess and then proceeds to see how well his guess works out, how comprehensible his subject's verbal behavior then becomes. Here he must look to the larger psychology, for he must see his subject's verbal behavior as part of a total pattern of behavior, and evaluation of his translation revolves around the comprehensibility of this total behavior.

Quine claims that such abductive guesses – specifically, "analytical hypotheses" – are underdetermined by even the totality of possible relevant evidence, viz. all possible observations of the subject's behavior in various circumstances of history (*WO*, 72; *OR*, 33). Consequently, there is more than one manual of translation which will fit into the overall psychology and at the same time maximize our understanding of the subject's linguistic behavior. This is the thesis of *indeterminacy of translation*. Importantly, it is fairly clear what a theory of translation here is: it is a mapping, a function, from the sentences of the one language into sets of sentences of the other language. Translational theories differ from one another, then, just in case they differ as functions; that is, just in case there is some alien sentence to which they assign different sets of ("equivalent") sentences of the linguist's home language. Quine's claim here is that there are different manuals of translation (different mappings) such that, in spite of their differences in sentence pairings, each fits with some overall psychology to get all the subject's speech dispositions right and therefore correctly predicts all his verbal behavior (cf. "Replies," 295).

Having settled fairly clearly on what a translation theory is, and what translational indeterminacy amounts to, let us turn our attention again to the larger issue of empirical theory generally. Here theories are thought of as being comprised of sets of sentences. The set of sentences in a theory we may call the *text* of the theory. Our problem is then to say when two texts are the same theory for Quine. Obviously there is no answer to this question. A body of uninterpreted sentences could mean anything, and so there is no way to tell when two texts agree on theory until given some method of interpretation. A sufficient interpretation for Quine would be a good translational mapping of the text into something we understand, say our home language English (*OR*, 51). Indeed, for Quine, that would be the best we could do, given the indeterminacy of translation. So let us try considering the theory to be the deductive closure of the result of translating the text into English by some adequate manual of translation. Note that the theory will contain not just the translations of the text, but also everything implied by those translations. Alternatively, we could consider the theory to be the pair consisting of the text and the translational mapping (*OR*, 51).

It is important to notice here that a change in translational function is liable to bring about a change in theory. Theories will now be *identical* if and only if they are the same sets of English sentences. That is, if the translations of the respective texts contain or imply all the same sentences of English. Such would be Quine's criterion of identity for theories. Two theories are *incompatible*, on the other hand, if they contain contradictory English sentences; that is, if the translations of the respective texts contain or imply sentences of English which are contradictories of one another. Incompatible theories are distinct, but distinct theories need not be incompatible.

The claim that a theory such as physics is indeterminate now becomes clear for Quine. The claim is that there are distinct – and perhaps incompatible – physical theories which maximize correct observational prediction. And it is also now clear what it is for Quine to say that theories are distinct or incompatible. Distinct empirically equivalent theories would simply be distinct deductively closed sets of English sentences whose observational subsets are identical. This is a more complicated matter than for Peirce just because Quine's account of what there is of meaning is more complicated than Peirce's. For Quine meaning does not align uniquely with empirical significance, but is tied to observational evidence in a looser way. This allows Quine to circumvent the deterministic conclusion holism would force on him were he to accept Peirce's pragmatic theory of meaning. In fact, as is now clear, he must diverge from Peirce on this. (Cf. the end of Section 3.)

Interestingly, the indeterminacy of theory in general follows for Quine from the indeterminacy of translation (the argument is suggested by *WO*, 78 and *OR*, 45ff). It is our own theory at issue of course. Let T be the text of any first-place theory we hold, a part of English. The theory we may take to be the pair (T, M) with the M homophonic translation manual from T onto itself. Suppose translation is indeterminate. In particular, then, translation of home into home language is underdetermined, even homophonic translation. For, "On deeper reflection," Quine holds, "radical translation begins at home": "The problem at home differs none from radical translation ordinarily socalled [i.e. translation from a remote language with no lexical tradition to call on] except in the willfulness of this suspension of homophonic translation" (*OR*, 46f). Thus there is some manual M' such that M' and M translate T differently – i.e. yield distinct sets of translates when these are closed under deduction – and yet M' translates T just as well as does M. But then the theory (T, M') is just as good as the theory (T, M). For if $\bar{T}'$ and $\bar{T}$ are respectively the deductive closures of the translations of T under M' and under M, our theory expressed by $\bar{T}$ could be revised into the theory expressed by $\bar{T}'$ and yet, Quine holds, "no conflicts with experience could ever supervene, except such as would attend our present sensible views as

well" (*WO*, 78), that is, here, our views expressed by $\bar{T}$. The reason – and this is the crux – is that the process of developing a translation of T involves as an integral part laying out the evidential relations between observational sentences in T and theoretical sentences in T. Any adequate translation will get the observation sentences the same; indeterminacy comes with translating the theoretical sentences.[13] To say there are alternative coöptimal translations M and M' is to say that, so far as we could ever tell (from observations of behavior in various circumstances, or from speech dispositions posited in principle determinately on the basis of the totality of such possible observations of behavior), a subject declaring $\bar{T}$ could in any and all observable circumstances just as well declare $\bar{T}'$ – his "meaning" would so far as we could tell be the same. But we are the subject: $\bar{T}$ is a part of English and expresses a body of our beliefs. So, since so far as we could tell we might mean either $\bar{T}$ or $\bar{T}'$, the theory (T, M) is just as good as (T, M'). That is, the process by which we learn the "meaning" of our own theory $\bar{T}$ – i.e. how albeit indeterminately to translate T – is the same process by which we would verify $\bar{T}$, deriving observational sentences from theoretical sentences and verifying the observational. The only difference is that the former project mentions sentences of T whereas the latter uses them to assert the conditions which they would also be used to assert (according to the translation) as part of the mentioned theory. This would not be true for translating just any subject's theory or text. It is true at home just because the canons of evidence that operate in the theory itself are a part of our canons of evidence that operate in our translational theories, in particular when translating home into home. So, since our original theory (T, M) was first-place, the alternative theory (T, M') is also first-place. And since there are alternative first-place theories, the original theory was indeterminate. In this way, translational indeterminacy entails indeterminacy of theory in general, for Quine.

Conversely, the indeterminacy of translation follows from the indeterminacy of theory in general. For, as we took pains to show, a translation *is* a theory in the requisite sense, proceeding from observation to hypothesis by abduction. Thus, for Quine, theory in general is indeterminate if and only if translation is indeterminate. This conclusion is the knot that ties Quine's epistemology and his philosophy of language so tightly together.

5. Let us concede, at least for the sake of argument, that translation is indeterminate. So what? One important consequence Quine draws is the dismissal of meaning and mentalistic semantics, the classic idea accord-

[13] *WO*, 68. But see Edwin Martin, Jr., 'The Intentionality of Observation', *Canadian Journal of Philosophy* **3** (1973).

ing to which our words serve to express ideas or meanings or propositions which are in our minds ("Replies," 304). Consider:

> In the old days when the idea was supreme, there was a comfortable illusion of determinacy of translation. To understand a language was to get its labels on the right ideas. ... As soon as we recognize that there is nothing in meaning that 's not in behavior, on the other hand, we are bound to expect ultimate indeterminacies of translation. ("Phil Progress," 9)

Quine's argument against meanings is not simply a behavioristic begging of the question of mentalism:

> Meanings..., those very models of mental entities, end up as grist for the behaviorist's mill. ... [But] the naturalist's primary objection to this view [of words as labels for meanings] is not an objection to meanings on account of their being mental entities, though that could be objection enough. The primary objection persists even if we take the labeled exhibits not as mental ideas but as Platonic ideas or even as the denoted concrete objects. Semantics is vitiated by a pernicious mentalism as long as we regard a man's semantics as somehow determinate in his mind beyond what might be implicit in his dispositions to overt behavior. (*OR*, 26f)

So Quine's *primary* argument against meanings is based on the indeterminacy of translation: other difficulties "are merely by the way" (*WO*, 206). Though he would also reject meanings as mental entities, the argument from indeterminacy is stronger in that it generalizes to any sort of theory of what a man means. The argument is roughly abductive:

> Whistling in the dark is not the method of true philosophy. Let us review the situations that prompted the positing of propositions, and consider what can be done without that expedient. Now to begin with it is a mistake to suppose that the notion of propositions as shared meanings clarifies the enterprise of translation. The totality of dispositions to speech behavior is compatible with alternative systems of sentence-to-sentence translation so unlike one another that translations of a standing sentence under two such systems can even differ in truth value. Were it not for this situation, we could hope to define in behavioral terms a general relation of sentence synonymy suited to translational needs, and our objection to propositions themselves would thereby be dissipated. Conversely, since the situation does obtain, the positing of proposition only obscures it. The notion of proposition seems to facilitate talk of translation precisely because it falsifies the nature of the enterprise. It fosters the pernicious illusion of there being a uniquely correct standard of translation of eternal sentences. (*WO*, 207f)

Given Quine's picture of theory construction, where the relation of theory to observation is not reductive but abductive (cf. *OR*, 73–78), we should take the call for a behavioral "definition" of synonymy as a call not for behavioral reduction but for accountable behavioral evidence of synonymy. Stimulus meaning, to be sure, is so defined behaviorally, but translation seeks sameness of meaning beyond stimulus meaning (and there Quine's worries about determinacy set in). Then Quine's argument against meanings is this:

(A) Meanings (or propositions) are posited as expressed by sentences and preserved in translation.

It follows that

(B) there is an objective matter of fact (viz. sameness of meaning, objective translation-relations) for translation to be right or wrong about,

and therefore that

(C) there is exactly one correct manual translating a given language into another,

and therefore (general indeterminacy aside)

(D) we could hope to determine (from behavioral data) what is the correct translation.

But

(E) translation is indeterminate; in principle we cannot determine any uniquely correct translation.

Therefore, Quine maintains, we should abandon the hypothesis that there is any uniquely correct translation, and therefore abandon the hypothesis of meanings preserved in translation and present in language generally. Therefore, that is,

(F) there is no uniquely correct translation manual, and thus

(G) there is no fact of the matter (no set of translation relations) for translation to be right or wrong about

– to suppose otherwise is a "pernicious illusion" insofar as it leads us to assume determinacy of translation; and so

(H) there are no meanings expressed in language and preserved in translation.

Such is the argument. Indeterminacy is *the* flaw that does in meanings; were translation determinate, meanings could be accepted. Thus the hypothesis of meanings ((A)) is refuted by the falsity of its consequence of determinacy of translation ((D)). The crux is the inference from (C) to (D), or contrapositively from (E) to (F). The epistemological thesis (E) does not *entail* the metaphysical thesis (F): it could be that there is a correct translation even though in principle we can never know which it is, such perhaps being the limitations of the human mind. The leap looks abductive: if we cannot determine a uniquely correct translation, perhaps that is best explained by assuming there is none. This abduction is apparently the heart of Quine's move, noted at the outset of this essay, from the epistemological thesis of

indeterminacy of translation (here (E); thesis (1) of Section 1) to the metaphysical thesis that there is no fact of the matter of translation (here (G); thesis (3) of Section 1). In this sense, indeterminacy of translation motivates, or lends abductive support to, but does not entail dismissal of translation relations and therewith of meanings.

If indeterminacy undercuts translational and meaning posits, then by parity of reason, one might well think, the indeterminacy of physics should serve to show that there really cannot be any such things as atoms. For, the atomic truth is indeterminate in the same way that translational truth is, its posits indeterminate even relative to all possible observational data.

That physics is indeterminate, Quine believes, as we have considered. That its indeterminacy arises from the same abductive sources as translational indeterminacy, Quine accepts, as we have also seen. That atoms go by the board with meanings, however, Quine denies. This disparity of ontological status results, Quine has said, from a disparity in the indeterminacies of physics and translation. He writes:

> Just as we may meaningfully speak of the truth of a sentence only within the terms of some theory or conceptual scheme..., so on the whole we may meaningfully speak of interlinguistic synonymy only within the terms of some particular system of analytical hypotheses.
>
> 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; for one is always working within some comfortably inclusive theory, however tentative. ... In short, the parameters of truth stay conveniently fixed most of the time. Not so the analytical hypotheses that constitute the parameter 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... (*WO*, 75)

In both physics and translation, then, indeterminacy arises from abduction and forces relativity of truth (and ontology) to an indeterminate choice of theory. The difference, Quine tells us, is that "we are always ready to wonder" about a piece of translation. But surely the same holds of physics! We are always ready to wonder about a piece of atomic truth, without reference to any one set of atomic hypotheses, indeed even in the absence of any. It cannot, then, be mere indeterminacy that does in meanings, else it would do in atoms too. The point here could only be that we are *more* ready to shift the parameters of truth (our hypotheses) in the case of translation than in the case of physics. Perhaps this means that translation is "too" indeterminate: hypotheses are too unsettled to qualify as serious science. But then it would be not indeterminacy per se but theoretical inferiority that spells the death of meanings. We shall return to this possibility.

Quine elsewhere isolates the source of the disparity he finds between these indeterminacies. The indeterminacy of translation is of the same kind and

origin as that of physics, both instances of the indeterminacy that affects empirical or natural theory in general. But unlike the indeterminacy of physics, translational indeterminacy is *additional* to the indeterminacy of our whole theory of nature. He says:

> Though linguistics is of course a part of the theory of nature, the indeterminacy of translation is not just inherited as a special case of the underdetermination of our theory of nature. It is parallel but additional. ("Replies," 303)

We might think of our theory about the world as coming in stages. First is the macroscopic part of our theory, positing everyday middle-sized physical objects and rough laws about their behavior. Even this is not uniquely fixed by all sensory stimulation possible; here we already pass indeterminately by abduction from stimulation to theory. Within this macro theory we frame observations, and on the basis of these data we pass abductively to hypotheses about microscopic reality. If our macroscopic musings were already indeterminate, then of course our microscopic theorizing will also be indeterminate since it is based on the former and finds ultimate verification in the same range of stimulations as the former. But the microscopic portion of our overall theory has an indeterminacy of its own which is additional to that inherited from our macroscopic beliefs. For, even relative to those macroscopic beliefs, even relative to the totality of assumed macroscopic truth, microscopic theory is indeterminate (*WO*, 22). In this sense the indeterminacy of our microscopic beliefs is additional to the indeterminacy of our macroscopic beliefs. Next, we may suppose, we ascend to the linguistic enterprise. Our linguistic theory, featuring translation, inherits the indeterminacy in our previous hypotheses – the macroscopic and the microscopic as well if these play any role (say, if neural states are considered in our developed linguistic theory). But our linguistic theory also begets an indeterminacy all its own. For, even relative to the totality of lower level theory, linguistics is indeterminate. And this indeterminacy is additional to that inherited from antecedent theory. It is parallel – i.e., of the same kind and origin – but additional. Of course the distinguishable stages of our constructed theory might be thirty or three hundred, each consisting in the addition of hypotheses to help explain prior beliefs and each ushering in its own indeterminacy. Thus chemistry, geology, economics, and all the rest find niches in the overall scheme. Within linguistic theory, indeed, hypotheses of meanings supervene on translations as posits of entities standing commonly in a certain relation to intertranslatable sentences.

Given that linguistic indeterminacy is additional to the indeterminacy of lower-level theory, what makes the indeterminately posited entities of linguistics (viz. meanings and synonymies) objectionable in ways the in-

determinate theoretical entities of physics are not? The trouble cannot be that linguistics is not a low or low enough level of theory, for neither are many parts of physics. Nor, apparently, that linguistics has, by virtue of its level in our theory, accumulated an unacceptable degree of indeterminacy; for parts of physics are doubtless high-level yet acceptably indeterminate, and anyway indeterminacy as defined does not admit of degrees. What is crucial to the case against meanings, we think Quine wants to say, is not just that linguistic indeterminacy is additional to lower-level indeterminacy, but that the indeterminacy of linguistics is additional to that of our *total theory of nature*. So that linguistics is indeterminate even relative to "the whole truth about nature" ("Replies," 303). Now this can be so only if linguistic theory itself falls outside the total theory of nature, only if linguistics failed to qualify for theory of nature. If linguistics is part of our theory of nature, then its indeterminacy is surely part of the indeterminacy of the overall theory, and so linguistic indeterminacy could not be additional. But then the fatal faw of linguistic theory *should* for Quine be, not indeterminacy, not even high level indeterminacy, but simply *failure to qualify* for the theory of nature, failure to fall within acceptable theoretical bounds.

Of course Quine takes the theory of nature to be all and only *physical* theory (cf. "Replies," 303), so that the fatal indeterminacy he charges linguistic theory with is indeterminacy in addition to that of physical theory, indeterminacy relative to even the whole physical truth. But again, indeterminacy, even this additional indeterminacy, is not the trouble. The trouble is that linguistics, so Quine claims, falls outside of physical theory. To dismiss linguistics for this reason is simply to register a physicalist bias. Yet meanings were not to be dismissed, on the present argument, simply because they are mental. If linguistic theory, even if ultimately committed somehow to nonphysical entities, qualified for theory of nature, it and its indeterminacy would be acceptable – to a naturalist, though not to a physicalist.

6. The trouble with linguistic theory, then, is simply that it is *bad science*. Thus:

> It is proverbial, or used to be, that man in his study of nature falls back on the old-time religion to fill in where his scientific explanations leave off. It is at least equally true that man in his study of language falls back on the old-time mentalistic semantics to fill in where his scientific explanations leave off. Mentalism, supernaturalism, and other unwholesome cultures thrive in dark places.[14]

And why is it that old-time religion and mentalistic semantics are unscien-

[14] Quine, *The Roots of Reference*, Open Court, La Salle, 1974, p. 36.

tific and supernatural? Surely because they *fail to explain* what is happening in nature:

> I think the behaviorists are right in holding that talk of ideas is bad business even for psychology. The evil of the idea idea is that its use, like the appeal in Molière to a *virtus dormitiva*, engenders an illusion of having explained something. And the illusion is increased by the fact that things wind up in a vague enough state to insure a certain stability, or freedom from further progress. (*LPV*, 48; cf. also "Phil Progress," 5)

Since mentalistic semantics and mentalistic psychology fail to explain, they are gratuitious (*WO*, 21). Ideas, meanings, propositions are superfluous superstructure, playing no role in our theory of nature. "The bodily states exist anyway," Quine remarks, "why add the others?" (*WO*, 264) Translation relations of synonymy suffer a like status, leaving translation less than scientific. Indeed, any sort of theory of what a man means will fail to qualify for our theory of nature insofar as it builds, as indeed it must, on translation.

"Linguistics," Quine holds, "is of course a part of the theory of nature" ("Replies," 303). Insofar, that is, as it qualifies as good science. For:

> knowledge, mind, and meaning are part of the same world that they have to do with, and ... they are to be studied in the same empirical spirit that animates natural science. (*OR*, 26)

This is the *naturalism* Quine shares with Peirce and Dewey, and it is a recurrent theme in *Ontological Relativity*. Continuing:

> Meanings are, first and foremost, meanings of language. Language is a social art which we all acquire on the evidence solely of their people's overt behavior under publicly recognizable circumstances. Meanings, therefore, those very models of mental entities, end up as grist for the behaviorist's mill. Dewey was explicit on the point: "Meaning... is not a psychic existence; it is primarily a property of behavior." (*OR*, 26f)

Here naturalism seems to have merged with a *behaviorism*. Indeed Quine has often spoken sympathetically of behaviorism. But Quine's "behaviorism" is of a very broad sort which he has suggested might better be called simply *empiricism*, albeit empiricism in a "distinctly modern" and "*externalized*" sense.[15] There are two points here. First and foremost: "all criteria" must be couched in "observation terms," all theory must face the tribunal of intersubjective observation. Second: "The old empiricist looked inward upon his ideas; the new empiricist looks outward upon the social institution of language," rejecting "the naive mentalism that typified the old empiricism." This externalized empiricism, or broad behaviorism, we should recognize, is the naturalism cited a moment ago.

If meanings qualify for a role in our scientific theory of nature, meanings must be accountable to observation. The accounting includes explanation,

[15] Cf. Quine, 'Linguistics and Philosophy', *Language and Philosophy* (ed. by Sidney Hook), New York University Press, New York, 1969. Hereafter this paper is cited as "Ling & Phil."

prediction, and verification. The observations include those of behavior and also perhaps data on neural structure. Introspection may offer support but only "as a means of arriving at conjectures... [which] can eventually be made sense of in terms of external observation" ("Ling & Phil," 97). So meanings must for Quine be theoretical entities, abductive posits. At the level of semantics and traditional psychology, meanings or ideas might be taken to be "functional" states of the human organism, internal states or mechanisms posited and individuated according to their "functions," their causal relations with stimuli, other internal states, and overt behaviors.[16] (Or perhaps they are states at some level of abstraction higher yet.) For, it is in terms of dispositions to verbal behavior that Quine would explicate or evidence meanings (*OR*, 27), and dispositions seem for Quine to be what others have called functional states of the organism realized by internal mechanisms (cf. *WO*, 223; *OR*, 136). Indeed:

> To take an easy example, acceptance of a sentence is for me... the disposition to assent to it; and for me a disposition, in turn, is a hypothetical state of the internal mechanism. ("Replies," 296)

Some of the internal mechanisms operative in a man's psychology are even innate (*WO*, Section 17; *OR*, 31; "Ling & Phil," 95f). And some meaning structures might be innate, as Chomsky urges. All this, provided meanings qualify as explanatory.

So conceived, meanings or ideas are not supernatural: linguistic and psychological theory positing them falls within the theory of nature and is compatible with the sort of naturalism espoused by Quine, provided the posits qualify. Now, so far, meanings or ideas might be *either* mental entities, with the theory of nature committed to a metaphysical dualism, *or* physical entities, with the theory of nature committed to physicalism. For naturalism as defined does not in itself entail *physicalism*, or materialism, the metaphysical doctrine that the ultimate constituents of the world are those of physics, the elementary particles and force fields that constitute matter and energy (and abstract objects if needed). (Note that physicalism, thus construed, does not require unity of science in the sense of a reduction of each branch of physical theory (e.g. chemistry, botany) to physics.) Quine, however, is not only a naturalist but also a physicalist. For, as we have seen, he rejects mental entities, on grounds that they do not aid our explanation of nature. So meanings or psychological entities are, if acceptable, more or less function-

[16] See Jerry A. Fodor, *Psychological Explanation*, Random House, New York, 1968, pp. 107ff; Hilary Putnam, 'Psychological Predicates', *Art, Mind, and Religion* (ed. by W. H. Capitan and D. D. Merill) University of Pittsburgh Press, Pittsburgh, 1965; N. J. Block and J. A. Fodor, 'What Psychological States Are Not', *Philosophical Review* **81** (1972).

ally posited physical – specifically, neural – mechanisms within the organism: "beliefs and other mental states [have] the status of hypothetical states of the nervous system." ("Replies," 296. The claim directly precedes the sentence just quoted above; we separate the two because we wish to separate the two independent theses Quine has. Cf. also "Reasons", 180; *OR*, 146f.)

Such for Quine are meanings *if* they are. But, as we know, Quine believes there are no such things. Why? Indeterminacy, we have argued, is a bad reason. What other reasons has Quine? He rejects the mental as bad science, presumably because the postulation of irreducible mental entities has not been shown to have any explanatory value, and perhaps because the history of the successes of science suggests that the road to further progress lies with physical science. That presumably is the basis for his physicalism. But what reason, indeterminacy aside, would Quine have to reject meanings and ideas, as he has, even when they are taken as hypothetical states of the nervous system? The answer lies within:

> The metaphor of the black box, often so useful, can be misleading here. The problem is not one of hidden facts, such as might be uncovered by learning more about the brain physiology of thought processes. To expect a distinctive physical mechanism behind every genuinely distinct mental state is one thing; to expect a distinctive mechanism for every purported distinction that can be phrased in traditional mentalistic language is another. ("Reasons," 180)

The trouble with meanings or ideas, then, even taken as hypothetical states of the nervous system, is that we have probably got hold of the wrong states. That gerrymandering of the mental – here a meaning, there a meaning – which is part and parcel of translation and traditional intentionalist psychology very likely will not coincide with that discovered of these allegedly same states by advancing physical theory.

> [A cat's] wanting or fearing is a strictly physiological affair, granted, and our evidence for it is our observation of the cat's overt behavior. But the particular range of possible physiological states, each of which would count as a case of wanting to get on to that particular roof, is a gerrymandered range of states that could surely not be encapsulated in any manageable anatomical description even if we knew all about cats. (*OR*, 146f)

The mental states, so gerrymandered, are not natural: they do not figure in causal explanation of behavior and so are not a part of theory of nature. Mental states, and meanings, are unnatural and causally irrelevant sets or fusions of distributions of elementary particles. Anatomically or physiologically described states, on the other hand, are natural and causally relevant sets or fusions. But then, in many or even most cases in which linguistic or psychological theory posits a meaning, there is simply no genuine (natural, causally relevant) mechanism there. And so there is no fact of the matter of meaning.

The fate of mistaken gerrymandering is at present largely prognosis. Why think it so? Presumably, Quine might hold, because the translational-psychological posits are just too premature, too loosely tied to any evidence, too far underdetermined by the evidence. Here Quine's picture and assessment of the enterprise of translation are relevant, since translation is perhaps the major effort to decide when two people have the same thing in mind. Given Quine's picture, the prognosis is tempting.

We can now get a better slant on the substance of Quine's longstanding worry about identity conditions for meanings. "No entity without identity" might better read "No entity without evidence of identity." For what is obscure for Quine is exactly which linguistic utterances in which circumstances to take as evidence of the same proposition on the speakers' minds (cf. *OR*, 144f). Criteria of identity for entities of a given kind, in the sense of necessary and sufficient conditions on identity, are a valuable part of the theory of that kind of entity, if such conditions can be had.[17] These might come for meanings with the development of physiology, plus no doubt a heightened understanding of such notions as dispositional and functional states. But perhaps more important are identity criteria in the sense of specifications of observable situations that would count as evidence of the identity of theoretical posits. A theory for whose posits we have inadequate evidence of identities will not serve as well. This is especially true of translation, whose explicit purpose is to say when the same meaning is had or expressed by different persons. Indeed, translation serves psychology as perhaps the major enterprise to systematize evidential criteria of identity of meanings – behavior other than verbal often counts as evidence of what is on or in a man's mind, but the best way to find out is often to ask him. Yet translation, Quine thinks, is an inadequate guide to sameness of meaning. That is what we should salvage of his account of the indeterminacy of translation: it is not that translation is indeterminate; it is that translation is too weakly tied to its evidence, the totality of behavioral cues are even in principle inadequate evidence of sameness of meaning for linguistics cum meanings to qualify as good science. Such is Quine's claim.

7. Quine has argued that translation relations and therewith meanings (and syntactic structures) are vitiated by indeterminacy. But, we have tried to show, his objection should be much simpler. The rejection of meaning is not supported by indeterminacy, but is based, rather, on the naturalistic objection that linguistic theory is theoretically inferior and explanatorily impotent, unqualified as part of our theory of nature. If physics were determinate, then

[17] Cf. Quine, Review of M. K. Munitz (ed.), *Identity and Individuation*, *Journal of Philosophy* **69** (1972), 489.

indeterminacy would be damaging to linguistics. But Quine holds that abductive indeterminacy infects all theory. Physics withstands it; and there is nothing significantly different about the indeterminacy of linguistics, so there is nothing specially damaging about it. Indeterminacy, it seems, turns out to have been something of a red herring in Quine. If it is a fact of theoretical life, it is for that very reason important to epistemology generally, but not particularly relevant to meaning (or syntax).

Quine's call for behavioral criteria of linguistic universals, syntactic structures, meanings, translation relations, and related mental structures is best viewed as a request for observational evidence for abductive theory. He should *not* be seen as asking for definitional reduction of hypothesis to observation. Such reduction would of course be one way of achieving determinacy. But theory for Quine, we know, proceeds by underdetermined abductive leaps.

The proper course in defending linguistic theory against Quine, then, is not to argue for the determinacy of linguistics, but rather to try to marshall behavioral evidence for one set of mental structures over others. Consequently psycholinguistic work such as that of Fodor and Bever's[18] should be of the right sort to persuade Quine that attributions of linguistic structure concern matters of fact and are objectively true or false. Objectivity, of course does not entail determinacy.

Linguistic theory falls outside our ultimate scientific theory of nature, for Quine. Yet "Translation is fine and should go on" ("Replies," 304). It serves very practical and real needs of everyday life. The same is true of the whole traditional intentionalist psychology of which linguistics is a part: it is supernatural but practically indispensable (cf. *WO*, 221; *OR*, 146; "Replies", 336). We justifiably slacken our standards of scientific rigor in order to bring home the bread even without a satisfying explanation of what moved the grocer. We can say he meant or believed this or that. Only, for Quine, we should not think that we have *explained* anything thereby (compare *WO*, 210, 221; *OR*, 23f on austerity). Translation and traditional psychology serve rather different purposes than ultimate scientific explanation. Truly they belong to the social arts. And one lasting significance of Quine's work on translation will have been to detail the social or intersubjective contribution to meaning and the intentional.

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[18] Cf. 'The Psychological Reality of Linguistic Segments', *Journal of Verbal Learning and Verbal Behavior* 4 (1965).