QUINE'S ARGUMENTS
FOR THE INDETERMINACY OF TRANSLATION

Hugh Wilder

February 3, 1975
Abstract:

The purpose of the article is to evaluate Quine's arguments for the thesis of the indeterminacy of translation. After formulation of the thesis, Quine's four main arguments are described and evaluated. The arguments are: (1) the argument from the underdeterminacy of physical theory, (2) the argument from the inscrutability of terms, (3) the argument from the conjunction of the Peircean notion of meaning and the Duhemian thesis about the interanimation of sentences, and (4) the argument from the linguist's reliance on sets of analytical hypothesis. It is contended that none of these arguments is successful in supporting the thesis of the indeterminacy of translation, and that Quine has offered no reason to believe that the degree of determinacy of translation is different from that of physical theory.
Quine's Arguments for the Indeterminacy of Translation

So far as I can see, Quine has offered four main arguments in support of his thesis of the indeterminacy of translation. While there has been much discussion recently of the indeterminacy thesis itself, Quine's own arguments in support of the thesis have not received the attention they warrant. In this paper I examine these arguments, in order to determine what support they do in fact give to the thesis. The conclusion for which I argue is that not one of the arguments for indeterminacy advanced so far by Quine is conclusive. My general strategy in arguing for this conclusion is to show that the indeterminacy thesis is not supported by other aspects of Quine's theory of meaning, and that Quine himself has provided several reasons for thinking that his arguments for indeterminacy are inconclusive.

Quine has formulated the indeterminacy thesis itself in several ways, not all of them unambiguous, nor even compatible with each other. Although my interest here is not in developing an unambiguous statement of the thesis, I believe that it may be simply and accurately formulated -- in the spirit of Quine's most recent defense of the thesis\(^1\) -- as follows:

D1. Translation is underdetermined iff alternative manuals of translation of a native's language can be set up, each manual compatible with all the native's dispositions to assent to and dissent from sentences, and yet each manual yielding incompatible translations of the native's language.

D2. Translation is indeterminate iff (1) it is underdetermined, and (2) there is no question as to

which manual yielding the alternative translations is correct or uniquely preferable.²

The indeterminacy thesis, as I shall take it, claims that translation is indeterminate in the sense of D2.

I

In RI, Quine offers "two ways of pressing the doctrine of indeterminacy of translation to maximize its scope" (RI, p. 183). The first way is based on the "underdeterminacy" of physical theory, and the second is based on the connection between the indeterminacy thesis and the doctrine of the inscrutability of terms. In this section, I shall examine the first way, and contend that Quine's comments on the underdeterminacy of physical theory do not lend support to the indeterminacy thesis. In the second section, I shall argue that Quine's comments on the inscrutability of terms do not -- and probably were not intended to -- constitute an argument for the indeterminacy thesis.

Quine does intend his comments on the underdeterminacy of physical theory to be an argument in support of the indeterminacy thesis: he develops an argument which, in his own terms, "is meant to persuade anyone to recognize the indeterminacy of translation of such portions of natural science as he is willing to regard as underdetermined by all possible observations" (RI, p. 183). The argument may be reconstructed as follows:

(1) Translation is indeterminate to the same degree that physical theory is underdetermined;

(2) physical theory is underdetermined (to a very high degree, according to Quine);

(3) therefore translation is indeterminate.

²To see that not all of Quine's explications of the distinction being drawn are compatible with each other, it is only necessary to notice that Quine's indeterminacy thesis, as originally formulated in Word and Object (Cambridge, Mass.: MIT Press, 1960; hereafter referred to as W&O), p. 27, is actually an underdeterminacy thesis, according to this definition of underdeterminacy, which is taken from RI, pp. 179-80.
In support of (2), Quine develops a notion of the underdeterminacy of physical theory which is analogous to the notion of the underdeterminacy of translation given in D1; it may be defined as follows:

D3. Physical theory is underdetermined iff alternative theories can be proposed, each theory compatible with all possible observations, and yet incompatible with the other theories.

Although Quine does not do so, an analogue of D2 for physical theory could also be given:

D4. Physical theory is indeterminate iff (1) theory is underdetermined, and (2) there is no question as to which one of the alternative theories is correct or uniquely preferable.

Quine argues that physical theory is not indeterminate, but only underdetermined; and, translation is both underdetermined and indeterminate. I shall argue that premise (1) of the argument is false, and that there can be, on Quine's own grounds, no difference in point of degree of determinacy between physical theory and translation. Thus, I shall not argue that physical theory either is or is not underdetermined; nor shall I argue yet that translation either is or is not indeterminate. Rather, I shall contend that Quine himself gives at least one good reason for thinking either (a) that physical theory is both underdetermined and indeterminate, as is translation, or (b) that translation is underdetermined and not indeterminate, as is physical theory. In either case, there will be no difference in point of degree of determinacy between physical theory and translation, and premise (1) of Quine's first argument must be false.

In propositional parlance, what a person believes is also what he means by his verbal expressions of his beliefs. Quine likewise assimilates the problem of translating a foreigner's physical theory to the problem of determining what the foreigner believes: thus, "where physical theories A and B are both compatible with all possible data, we might adopt A for ourselves and still remain free to translate the foreigner either as believing A or as believing B" (RI, p. 180).

This distinction between A and B qua physical theories and A and B qua translations is just what I am challenging here. My strategy will be to assimilate the problem of "translating" our own physical theory to the problem of de-
terminating what we believe ourselves — i.e., to the problem of adopting physical theories as our own. That this assimilation is permissible in the domestic case is, I shall argue, suggested by Quine's remark that "radical translation begins at home." And, in those cases of radical translation, in which there is no question as to what the native means — i.e., in which there is no question as to which one of alternative translations is correct — there is no question as to what the native believes. As Quine says, what he is "getting at in arguing the indeterminacy of translation" is that in certain cases the question about what the native "really believes" "is a question whose very significance I would put in doubt" (RI, p. 179–80).

Quine is not here espousing that extreme version of skepticism according to which knowledge or belief is impossible; he is, however, advancing the skeptical claim that there is, in a large number of cases, no question as to what people believe, because, apparently, there is, in these cases, no way of telling what they believe. Quine has dispensed with propositions, but in so doing he does not do away with belief: it is just that propositions are not the things believed.

What is believed, according to Quine, is that certain sentences are true. Quine has developed a "sentential attitude" analysis of belief-sentences, and offers "semantic reformulations" of mentalistic belief-attributions: "instead of:

\[ w \text{ believes that } \ldots \text{[proposition]} \]

we may say:

\[ w \text{ believes-true } \ldots \text{'.} \]

According to this reformulation, we speak of sentences, naming them by quotation, instead of propositions. It should be clear, however, that if what we believe is that

\[ W.V. \text{ Quine, Ontological Relativity and Other Essays (New York: Columbia University Press, 1969), hereafter referred to as OR, p. 46.} \]

certain sentences are true, then it will be important to specify the language to which the sentences belong. Propositions were language-neutral; but, as Quine recognizes, "when we say that w believes-true S, we need to be able to say what language the sentence S is thought of as belonging to...because S might by coincidence exist (as a linguistic form) with very different meanings in two languages" (Ways of Paradox, p. 193).

According to Quine's theory of meaning, however, the sentence S might even have different meanings, in a sense to be explained, in one and the same language. This fact is made apparent with Quine's development of the claim that "radical translation begins at home." He argues, generally, that the notions of intralinguistic and even intrasubjective synonymy are involved in the same indeterminacy surrounding the notion of interlinguistic or translational synonymy. If this is so, then there will be cases in which we will not be able to say just what we mean, and we will not be able to say what we believe.

Quine has argued that it only makes sense to speak of translations as correct or incorrect relative to some scheme of translation: relative to some set of analytical hypotheses. Likewise, we can only say what we mean in the home language -- i.e., give "translations" of our own expressions -- relative to some set of analytical hypotheses. Thus, attempts to give synonyms in the home language are really "translations" of our own expressions according to some set of analytical hypotheses.

In the domestic case -- i.e., in determining intralinguistic and intrasubjective synonymy pairs -- the rule of homophonic translation is dominant. But the important point is that the set of analytical hypotheses which incorporates this rule of homophonic translation is not privileged or intrinsically superior to any other set. Quine observes that we suspend the homophonic rule in many cases, and "construe a neighbor's word heterophonically now and again if thereby we see our way to making his message less absurd" (OR, p. 46). There is no ultimate justification for the rule of homophonic translation, any more than there is any ultimate justification for any set of analytical hypotheses. In attempting to translate a radically foreign theory, we immediately and gladly suspend the rule of homophonic translation, and begin to fish around for another set of analytical hypotheses. We do not expect to get very far by using the homophonic rule in radical translation. We do, however, expect this rule to carry us much of the way in domestic "translation." The only difference, Quine says, between attempting to translate a
radically foreign language and attempting to give synonyms for our own and our neighbor's sentences is the degree of willingness to suspend the rule of homophonic translation (OR, p. 47). In attempting to give synonyms for our own sentences, we are loath to give up the homophonic rule: Quine's point, however, is that we could give it up if we wanted to. If we were to try out another set of analytical hypotheses, and came up with different "translations" of our own sentences, then we could not say whether we really meant the homophonic translation or the translation according to the other set of analytical hypotheses.

As we saw, Quine assimilates the problem of translating a foreigner's physical theory to the problem of determining what the foreigner believes. Now, we may do the same in the domestic case: the problem of determining what we believe is the very same problem of translating (perhaps according to the homophonic rule, as much as possible) our own physical theory. And now the same difficulty which plagued radical translation -- the impossibility of saying which one of alternative manuals of translation is correct -- is seen to also plague the domestic case. Just as there are cases in which there is no question as to what a native really means or believes, so there are cases in which there can be no question as to what we mean or believe. That is, if radical translation does begin at home, then there must be cases in which, when confronted by competing physical theories, there is no question as to which theory is the one we believe.

To see that this is so, let us consider the case of an English physicist's physical theory, $T_1$, which can be "translated" by another Englishman into two new theories, $T_2$ and $T_3$, according to manuals of translation $MT_2$ and $MT_3$, respectively. (One of the manuals could even incorporate the rule of homophonic translation.) According to Quine's indeterminacy thesis, $T_2$ and $T_3$ can be incompatible with each other, even though $MT_2$ and $MT_3$ are each compatible with all the physicist's dispositions to assent to and dissent from sentences.

It can be further imagined -- according to Quine's comments on the undeterminedness of physical theories -- that $T_2$ and $T_3$ are each "compatible with all possible observations." In this case, there can be no question, even for the physicist himself, as to whether $T_2$ or $T_3$ is the correct translation of $T_1$: the physicist will not be able to say whether he really means $T_2$ or $T_3$, nor will he be able to say whether he really believes $T_2$ or $T_3$. But if there is no question for the physicist as to whether he
really believes $T_2$ or $T_3$, then these physical theories will be indeterminate. And, in this example, there can be no grounds on which the physicist could base an adoption of either $T_2$ or $T_3$ as the theory he really believes. Because of this, the theories are indeterminate. It should be noticed that the physicist cannot even say that what he really believes is $T_1$: because $T_1$ may be considered as a translation, according to some further manual, of $T_2$ and $T_3$, and there are no grounds for choosing $T_1$ above the others.

If it is objected that there do remain some grounds on which the physicist could base his adoption of either $T_2$ or $T_3$ as his own physical theory, then it may be replied that these grounds must also be available to him as a basis for adoption of either $T_2$ or $T_3$ as the preferable translation of $T_1$. That is, let us say that the physicist originally adopted $T_1$ as his physical theory because it possessed, among other things, some utility $u$. It may be that $u$ is preserved in $T_2$, but not in $T_3$. If this were so, then the physicist could choose $T_2$ over $T_3$ as the preferable physical theory. In choosing $T_2$ as the preferable physical theory, however, the physicist is also adopting $T_2$ rather than $T_3$ as the preferable translation of his beliefs. In this case, there is a question as to whether $T_2$ or $T_3$ is the preferable translation of $T_1$, and translation is therefore not indeterminate, but only underdetermined.

By switching to the domestic case, we have seen that either physical theory must be indeterminate or translation must not be indeterminate. In both cases, there is no difference between physical theory and translation in point of degree of determinacy. If no evidence is available for adopting either $T_2$ or $T_3$ as the preferable translation of $T_1$, then neither is any evidence available for adopting either $T_2$ or $T_3$ as the preferable physical theory. On the other hand, if there is evidence for adopting either $T_2$ or $T_3$ as the preferable physical theory, then this evidence may also be used for adopting either $T_2$ or $T_3$ as the preferable translation of $T_1$.

It follows from these considerations that Quine's first argument for the indeterminacy of translation is unsound. In particular, premise (1) is false: there can be, in fact, no difference in point of degree of determinacy between physical theory and translation. Substituting this thesis for premise (1) of Quine's argument, we obtain the following argument for the underdeterminacy of translation:

(1') There is no difference in point of degree of determinacy between physical theory and translation;
(2) physical theory is underdetermined;

(3') therefore translation is underdetermined.

II

Quine's second way of pressing the indeterminacy thesis, presented in RI, is based on a correlation Quine sees between the indeterminacy thesis and the doctrine of the inscrutability of terms. I shall contend that Quine's description of this correlation does not constitute an argument for the indeterminacy thesis, and that it is probably not even intended to constitute such an argument.

As I have indicated, Quine does say that one way of "pressing the doctrine of indeterminacy of translation to maximize its scope" is to press "whatever arguments for indeterminacy of translation can be based on the inscrutability of terms" (RI, p. 182). Quine also says, however, that his comments about the inscrutability of terms are not aimed at "proof" of the indeterminacy thesis, but rather "at helping the reader to reconcile the indeterminacy of translation imaginatively with the concrete reality of radical translation" (RI, p. 182). I shall argue that the inscrutability of terms and the indeterminacy of translation have a common cause, according to Quine, in the linguist's reliance on analytical hypotheses. Quine's comments about this reliance will be examined, in the final section of this paper, as an argument for both the indeterminacy thesis and the doctrine of the inscrutability of terms. For now, it is sufficient to point out that because inscrutability and indeterminacy have a common cause, the two doctrines stand or fall together: no description of a correlation between the inscrutability of terms and the indeterminacy of translation can constitute an argument in support of either doctrine.

Quine's doctrine of the inscrutability of terms concerns the impossibility of imputing, with any rational warrant, a particular "apparatus of individuation" for grammatical constructions to the speech of a native. As Quine says, the whole notion of terms and their denotation is bound up with our own grammatical analysis of the sentences of our own language. It can be projected on the native language only as we settle what to count in the native language as analogues of our pronouns, identity, plurals, and related apparatus; and I urged in Word and Object that there would be some freedom of choice on this score (RI, p. 181-82).
That there is this freedom of choice on what to count as
the native analogues of our own grammatical constructions
is the major claim made in the doctrine of the inscrutabi-
licity of terms. This freedom of choice has its analogue in
the indeterminacy thesis: just as the indeterminacy thesis
claims that in a large number of cases there is no question
as to which one of alternative manuals of translation is
correct or preferable, so the doctrine of the inscrutability
of terms claims that there is no question as to which pro-
jection— if any— of our English grammatical constructions
on the native language is correct or preferable. Which
projection is correct "is a question that remains undecided
by the totality of human dispositions to verbal behavior.
It is indeterminate in principle; there is no fact of the
matter" (OR, p. 38).

That there is this analogy between the freedom of choice
as to what to count as native analogues of our own gram-
matical constructions and the freedom of choice as to what
manual of translation is correct is no coincidence. The
reason for the freedom in the one case — the reason why
terms are inscrutable — and the reason for the freedom in
the other case — the reason why translation is indetermi-
nate — is the same. Both the inscrutability of terms and
the indeterminacy of translation are brought on by the use
of analytical hypotheses in attempting radical translation.

Analytical hypotheses constitute a crucial element of
linguists' manuals of translation. According to Quine, a
jungle linguist can translate native observation sentences—
with only "normal inductive uncertainty" (W&O, p. 68) — by
the "independent evidence of stimulatory conditions" (W&O,
p. 72). In addition, the linguist can settle, by appeal to
this evidence, questions about the translation of truth
functions, about which native sentences are stimulus-
analytic, and about which native occasion sentences are in-
trasubjectively synonymous (W&O, p. 68). As the linguist
proceeds in this work toward a translation of the native
language, he constructs analytical hypotheses, which both
give him hints as to how to settle the above questions, as
well as guide him in the translation of sentences not
translatable on the independent evidence of the stimulatory
conditions.

Analytical hypotheses are therefore used by the linguist
as "a way of catapulting oneself into the jungle language
by the momentum of the home language" (W&O, p. 70). In
particular, analytical hypotheses are used to explain the
syntactic structures of the jungle language; this is most
easily done by noting parallelisms of function between
parts of the native's speech and parts of English. It is
by "projection of prior linguistic habits" by the linguist, onto the jungle language, that the linguist is able to explain syntactic structures of the jungle language. Specifically, "it is only by such outright projection of prior linguistic habits that the linguist can find general terms in the native language at all, or having found them, match them with his own" (W&O, p. 70, emphasis added). The point is that stimulus meanings are never sufficient to determine, with even "normal inductive uncertainty," what native words, if any, are terms.

What words to count as terms, and what to count as other grammatical constructions in the native language, are determined by the linguist's analytical hypotheses. And, unfortunately, the evidence available to the linguist (i.e., stimulus meanings, translations of observation sentences, translations of truth functions, classification of some sentences as stimulus-analytic and some as intra-subjectively stimulus-synonymous) all must woefully under-determine the analytical hypotheses on which the translation of all further sentences depends. To project such hypotheses beyond the independently translatable sentences at all is in effect to impute our sense of linguistic analogy unverifiably to the native mind (W&O, p. 72).

Quine argues that rival sets of analytical hypotheses can be set up, each one compatible with all the native's dispositions to assent to and dissent from sentences, and with all the other independent evidence, just reviewed, which is available to the linguist. And, in many cases, there is no question as to which set of analytical hypotheses is correct or uniquely preferable. As we shall see, this impossibility of choosing between rival sets of analytical hypotheses is held to bring on the indeterminacy of translation; it should be clear that it also brings on the inscrutability of terms. Since there is no question as to which set of analytical hypotheses is correct, neither is there any question as to what is the correct description of the syntactic structure of the native language.

It should be clear, also, that Quine cannot argue from the inscrutability of terms to the indeterminacy of translation. That Quine does not intend to base any arguments for the indeterminacy thesis on the doctrine of the inscrutability of terms is suggested in the following remarks:
I first urged the inscrutability of reference with the help of examples like the one about rabbits and rabbit parts. These used direct ostension, and the inscrutability of reference hinged on the indeterminacy of translation of identity and other individuative apparatus (RI, p. 182).

The inscrutability of reference (and of terms) hinges on the indeterminacy of translation insofar as the indeterminacy of translation just is the impossibility of choosing between rival sets of analytical hypotheses. In this sense, the doctrine of the inscrutability of terms is a part of the general indeterminacy thesis. Analytical hypotheses are used to guide the linguist in his translation of the native language; some of these hypotheses determine (for the particular manual of translation, of which they are a part) what to count as terms in the native language. Terms are inscrutable when there is no question as to whether these analytical hypotheses are correct. Sometimes, apparently, that these analytical hypotheses are indeterminate does not entail that the whole set of hypotheses which helps to make up a manual of translation are indeterminate. ("Inscrutability of terms need not always bring indeterminacy of sentence translation in its train" (RI, p. 182). Whether this is correct, the point remains that indeterminacy arises when there is no question as to which one of rival sets of analytical hypotheses (each one of which will include hypotheses concerning grammatical constructions of the native language) is correct. Hence, that terms may be inscrutable does not constitute an argument for the general indeterminacy thesis; in the final section of the paper, we shall examine the question as to whether the use of analytical hypotheses does bring on the indeterminacy of translation.

III

First, however, it will be helpful to examine a different argument advanced by Quine in support of the indeterminacy thesis. This argument is based on a conjunction of the Peircean notion of meaning with the Duhemian thesis about what Quine has called the "interanimation of sentences." The argument is stated as follows:

If we recognize with Peirce that the meaning of a sentence turns purely on what would count as evidence for its truth, and 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 (OR, pp. 80-81).

This argument seems, however, to have a crucial premise missing. I shall argue, via a comparison of the case of scientific theory with the case of radical translation, that the underdeterminacy of translation is compatible with the conjunction of the Peircean notion of meaning and the Duhemian thesis of the interanimation of sentences, just cited in support of a version of the indeterminacy thesis. The conjunction of the Peircean notion and the Duhemian thesis alone does not support the indeterminacy thesis.

Quine himself has often urged a comparison of scientific theory with radical translation. First, Quine has urged that the Peircean notion of meaning is essential to both scientific theories and translation. With respect to scientific theories, Quine claims that: "two cardinal tenets of empiricism remained unassailable... and so remain to this day. One is that whatever evidence there is for science is sensory evidence. The other... is that all inclination of meanings of words must rely ultimately on sensory evidence" (OR, p. 75). Therefore, for whatever sentences in scientific theories that do have meanings, it is this kind of meaning— the Peircean kind— that they have.

Furthermore, Quine holds that it is this very same notion of meaning which is essential to translation: "The sort of meaning that is basic to translation, and to the learning of one's own language, is necessarily empirical meaning and nothing more" (OR, p. 78). It is this sense of meaning which Quine attempts to reconstruct in his notion of stimulus meaning; and stimulus meaning is the only evidence available to the linguist in carrying out his translations.

It is also important to note that Quine thinks that the Duhemian thesis, "that theoretical sentences have their evidence not as single sentences, but only as larger blocks of theory," is also essential to both scientific theory and translation. With respect to scientific theory, it is well-known that Quine believes that not all individual sentences have their own meanings, in Peirce's sense. As Quine says, "the typical statement about bodies has no fund of experiential implications it can call its own. A substantial mass of theory, taken together, will commonly have experiential implications" (OR, p. 79).
In addition, Quine says that "the crucial consideration behind my argument for the indeterminacy of translation was that a statement about the world does not always or usually have a separable fund of empirical consequences that it can call its own" (OR, p. 82). As we have seen, Quine believes that when indeterminacy of translation obtains, one thing that must be the case is that two translations of, say, theory \( T \), can be given, such that each translation preserves the net empirical consequences of \( T \), and yet each is incompatible with the other. Quine contends that it is because individual sentences do not have their own stock of empirical implications that translations can be given which are incompatible with each other, and yet yield the same empirical implications. As Quine says,

> if English sentences of a theory have their meaning only together as a body, then we can justify their translation into Arunta only together as a body. There will be no justification for pairing off the component English sentences with component Arunta sentences, except as these correlations make the translation of the theory as a whole come out right. Any translations of the English sentences into Arunta sentences will be as correct as any other, so long as the net empirical implications of the theory as a whole are preserved in translation (OR, p. 80).

This passage suggests that two translations of the English theory into Arunta could be given: each translation would preserve the net empirical implications of the theory as a whole, but each translation would pair off different component English sentences with component Arunta sentences. But, that such alternative translations can be given shows only that the translation of the theory is underdetermined. Quine has not shown yet that there is no way to choose between the alternatives: and this is to say that he has not yet shown that translation is indeterminate.

Nor does the comparison of translation with scientific theory lead one to suspect that translation is indeterminate. For, as we have seen, the Peircean notion of meaning and the Duhemian thesis of the interanimation of sentences are both essential to scientific theories as well as to translation. And, Quine contends that in the case of scientific theories the conjunction of the Peircean notion and the Duhemian thesis entails the underdeterminacy of the theories, not their indeterminacy.

That is, Quine argues that it is because the only meaning that sentences of a theory have is empirical, or Peircean,
meaning, and because not all component sentences of a theory have their own separable empirical meanings, that alternative theories can be proposed, each of which has the same net empirical meaning. Quine has made it clear that he does think that scientific theories are underdetermined in this sense: that is, he does think that alternative theories can be proposed, each of which has the same net empirical meaning, and yet each being incompatible with the other alternatives. I am not sure that it makes sense to speak of "alternative" theories, in such cases; as Quine himself says, when two theories have the same net empirical meaning, "they are in an important sense not two but one" (W&O, p. 78). I do not need to argue here, however, that scientific theories cannot be underdetermined in Quine's sense. It is sufficient for my purposes to point out that for Quine, not only are theories underdetermined, but it is the conjunction of the Peircean notion of meaning with the Duhemian thesis of the interanimation of sentences which brings on this underdeterminacy.

Thus, Quine thinks that two theories which have the same net empirical meaning can still be incompatible with one another: the conflict between them, which makes them alternative theories and not one, is "a conflict of parts seen without the wholes" (W&O, p. 78). Apparently, the theories will conflict because corresponding parts of the two theories do not have the same empirical meaning, even though the net empirical meaning of the theories is the same. The important point is not whether it makes sense to speak of alternative theories in this sense. Rather, the point is that it is only because, first, the only meaning sentences of a theory have is empirical meaning, and, second, component sentences of a theory do not usually have their own separable empirical meanings, that there can be such a "conflict of parts seen without wholes" for Quine.

Scientific theories are underdetermined for Quine, and not indeterminate. In other words, "conflicts of parts seen without wholes" are resolvable conflicts. As we shall see more clearly in section IV, Quine thinks that there are grounds on which we may base an adoption of one theory over another, even when the two theories have the same net empirical meaning. This point is crucial: the conjunction of the Peircean notion of meaning with the Duhemian thesis of the interanimation of sentences entails the underdeterminacy of scientific theories, and not their indeterminacy.

Now, Quine also contends that the conjunction of the Peircean notion of meaning with Duhem's thesis brings on the same sort of conflict between alternative translations. That is, the conflict between two manuals of translation,
where each manual is compatible with all the relevant data, and yet each yields a translation incompatible with the translation yielded by the other manual, is a conflict "of parts seen out of context," or without the whole (W&D, p. 78). The conflict between such alternative manuals of translation is exactly the same conflict that arises between alternative scientific theories, when each alternative has the same empirical meaning. This is simply to say that both translation and scientific theories are underdetermined. Furthermore, it is, according to Quine, the conjunction of the Peircean notion of meaning with the Duhemian thesis of the interanimation of sentences which brings on the underdeterminacy in each case.

Now, underdeterminacy is not incompatible with indeterminacy; as I have said, Quine thinks that translation is both underdetermined and indeterminate. It should be clear, however, that the conjunction of the Peircean notion of meaning with Duhem's thesis cannot, by itself, account for the indeterminacy of translation. This is so, because the conjunction does not entail the indeterminacy of scientific theories. If translation is indeterminate, then this indeterminacy is not brought on solely by the conjunction of the Peircean notion with Duhem's thesis. Since this conjunction does not entail the indeterminacy of scientific theories, there is no reason to think that it should entail the indeterminacy of translation -- unless, of course, there are relevant differences, as yet unspecified, between scientific theories and translation. Obviously, Quine does think that there are such relevant differences; he attempts to spell them out in his discussion of the translator's reliance on analytical hypotheses, to which I now turn.

IV

I argued in the first section of this paper that Quine himself has offered grounds for thinking that there can be no difference in point of degree of determinacy between physical theory and translation. I do not want to defend this thesis here. Nevertheless, Quine's final argument in support of the indeterminacy thesis is, on at least two occasions, presented as a direct challenge to our first conclusion, that there can be no difference in point of

degree of determinacy between physical theory and translation. In this section I contend that Quine's final argument is not conclusive: specifically, it is held that Quine's argument neither damages the conclusion reached in the first section of this paper, nor supports Quine's indeterminacy thesis.

The final argument holds, very roughly, that it is the linguist's reliance on manuals of translation -- made up, in part, by sets of analytical hypotheses -- which brings on the indeterminacy of translation. Scientific theories are not indeterminate, because, in constructing and evaluating them, scientists do not need to rely on analytical hypotheses. This argument, therefore, constitutes Quine's attempt to spell out the relevant differences between translation and scientific theories which was left unspecified in the third argument for indeterminacy, just considered.

The final argument is presented, then, in order to counter the claim that "translational synonymy is at its worst no worse off than truth in physics" (W&O, p. 75). If our own first conclusion is correct, and there is no difference in point of degree of determinacy between physical theory and translation, then translational synonymy would be "no worse off than truth in physics." And, if truth in physics is underdetermined, then translational synonymy would also be underdetermined, and not indeterminate. Quine argues, however, that translation is worse off than truth in physics; translation is indeterminate and truth in physics is underdetermined. The argument for this conclusion is suggested in the following passage:

In being able to speak of the truth of a sentence [in a scientific theory] only within a more inclusive theory, one is not much hampered; for one is always working within some comfortably inclusive theory, however tentative.... [T]he 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; yet two sets of analytical hypotheses equally compatible with all linguistic behavior can give contrary answers, unless the remark is one of the limited sorts that can be translated without recourse to analytical hypotheses (W&O, pp. 75-76).

This argument suggests a twofold comparison of scientific theory with translation: a comparison of, first, specific truths in physics with specific cases of trans-
lational synonymy, and second, of ways of assessing alternative scientific theories with ways of assessing alternative manuals of translation. This twofold comparison is also developed in Quine's second formulation of the argument under consideration, in his "Reply to Chomsky"; in order to see how the comparison is crucial to his indeterminacy thesis, we must look closer at his contentions concerning scientific truth and the ways of assessing alternative scientific theories.

As we have seen, Quine holds that specific cases of translational synonymy are determined by particular manuals of translation. Manuals of translation are the "ultimate parameters" of affirmations of translational synonymy. What manuals do is to guide the linguist in translation: i.e., to determine which sentences in the linguist's language are synonymous with which sentences in the native's language. Granted, observation sentences of one language may be equated with observation sentences of another language, without the help of manuals of translation; but apart from these cases of synonymy, which can be settled by appeal to the independent evidences of stimulus meaning, there are no cases of translational synonymy determined apart from any manuals of translation.

Similarly, what scientific theories do, according to Quine, is to say which sentences of a given language are true. In the same sense that translational synonymy is manual-relative, so is scientific truth theory-relative. Again, observation sentences carry their own evidence with them, so to speak, and may be determined to be true independently of any particular theory. But, apart from these cases of scientific truths, no other sentences can be said to be true, apart from any scientific theory. As Quine says, theory in physics is the ultimate parameter of particular affirmations of truth (Words and Objections, p. 303); which sentences of a given language are true is determined by which theory is adopted.

There is a similarity then between scientific truth and translational synonymy: both are relative, to theories and to manuals of translation, respectively. Manuals of translation and scientific theories are analogous, therefore, insofar as particular translations and particular truths are determined relative to them. There is also, however, a disanalogy between manuals of translation and scientific theories: this disanalogy arises in the ways -- or lack thereof -- in which alternative scientific theories and alternative manuals of translation may be assessed. It is this disanalogy which is held to account for the indeterminacy of translation, as opposed to the underdeterminacy of
scientific theory.

The point is that according to Quine there are ways of assessing alternative scientific theories; they may be compared on common ground, and evidence may always be cited, or be expected to be forthcoming, in support of rational adoption of one physical theory over another. And, Quine holds that, in most cases, there simply is no way of comparing alternative manuals of translation; evidence cannot be expected to be forthcoming, which would support the adoption of one manual of translation over its rivals.

I shall argue that the common ground on which alternative scientific theories may be compared is provided by what Quine calls our "aggregate science," "total evolving doctrine," "world theory" (W&O, pp. 24 and 25) -- or, in Neurath’s metaphor, adopted by Quine, our boat, in which we are afloat. It is precisely this aggregate science which is held to be lacking -- or, more accurately, is held to be of no avail -- in radical translation.

Quine’s final argument in support of indeterminacy may now be stated more fully. Linguists try to tell us which sentences of one language are synonymous with which sentences of another language; scientists try to tell us which sentences of a given language are true. In trying to determine translational synonymies, linguists must appeal to some manual of translation. In trying to determine what sentences of a given language are true, scientists must appeal to some theory. As we have seen, however, both manuals of translation and scientific theories are underdetermined, according to Quine: just as alternative manuals of translation may be set up, each compatible with all the evidence relevant to translation, so can alternative scientific theories be proposed, each compatible with all the relevant evidence. Quine argues, however, that in translation, in many cases, there is no analogue of the aggregate science, which affords the scientist the common ground on which he can assess rival scientific theories. Because of this, there can be no question as to which one of alternative manuals of translation is correct; and, because the alternative manuals might yield translations which are incompatible with one another, there can be no question as to which one of alternative translations is correct. Therefore, translation is indeterminate.

According to Quine, scientific theories are proposed and evaluated in the context of our aggregate science. The aggregate science is constituted simply by the totality of

6 I discuss the crucial exceptions to this rule below.
particular scientific theories accepted at any particular time. Therefore, although truth in physics is relative to the accepted physical theory, it is also absolute, in the sense that we take seriously — and must take seriously — our own evolving aggregate science, according to which the theory in question is judged as being acceptable (cf. W&O, pp. 24-25). Thus, when we wonder which sentences are true, we always fall back on the accepted theory for our answer; and when we wonder which theory to accept, we fall back on our aggregate science. Likewise, when we wonder what a native means, we fall back on some manual of translation; when we wonder which manual to accept, however, we have, according to Quine, nothing on which to fall back. In science, we are not free to wonder what is true, apart from all theories — because we cannot get out of our aggregate science. In translation, however, we are free to wonder about the meaning of a native's remark, without reference to any manual of translation — because there is no analogue, in translation, of the aggregate science.

Quine contends that the aggregate science provides the criteria that are used in evaluating particular scientific theories. He says that "it is usually redundant to cite the theory as parameter of our assertions" of particular truths, because "no higher standard offers." Citation of theory as the parameter of truth "ceases to be redundant only when we are contrasting alternative theories at a deep level, e.g., with a view toward a change" (Words and Objections, p. 303). If theories can be contrasted at all, there must be some inclusive theory which provides the grounds for the contrast; and this more inclusive theory which must be cited as the parameter of acceptability for particular theories, is what Quine calls the aggregate science.

Because the aggregate science is constituted by the totality of accepted theories, it will change or evolve as particular theories are accepted or rejected. And, as the aggregate science evolves, the criteria of acceptability for particular theories will also change. This is to say that, according to Quine, what we call the "canons of scientific method" are in a process of evolution: what we deem to be valuable characteristics of scientific theories changes as the aggregate science evolves.

Nevertheless, as I have suggested, although the criteria of acceptability might change, at any particular time they are comfortably fixed and stable, so that, as Quine says, theories can be considered, accepted, and rejected "as earnestly and absolutely as can be" (W&O, p. 25). Within the larger context, hypotheses can be evaluated as absolutely as can be: at any particular time, there are never any
"higher" authorities to appeal to, beyond that of the current aggregate science.

Quine believes that according to our current aggregate science there are several different criteria which are used in assessing theories. Some of these criteria concern the degree of confirmation of the theories: only theories which are highly confirmed, however this is explicated, are acceptable. There are other utilities, however, beyond degree of confirmation, which must also be considered in assessing theories. Just because, say, theories $T_1$ and $T_2$ are both compatible with all possible observations, it does not follow that $T_1$ and $T_2$ are equally acceptable. As we have seen, Quine says we might still adopt, in this case, $T_1$ over $T_2$: the question whether $T_1$ or $T_2$ is more acceptable is still open, and there are grounds remaining which may be adduced in support of an answer to this question. The criteria on which the theories may be assessed are provided by the more inclusive framework within which $T_1$ and $T_2$ were proposed and are being evaluated.

It is just this aspect of scientific theories which Quine claims lack an analogue in the case of translation. That is, in many cases there simply are no grounds which may be adduced in support of adoption of one manual of translation over another. There comes a point, in other words, at which adoption of one manual over another is completely arbitrary. In many cases, alternative manuals of translation cannot be compared and evaluated according to any common standard: it is in this respect that manuals of translation differ from scientific theories. And it is because in these cases alternative manuals cannot be compared, that there is no question as to which one of the alternative manuals is correct or preferable.

Apparently, Quine thinks that alternative manuals of translation often cannot be compared and evaluated because, in these cases, there is no inclusive theory -- or "manual," in this case -- which would provide criteria according to which we could assess the alternative manuals. There is no "higher theory" to which we can always appeal in wondering whether a manual is correct. This is what Quine means, apparently, when he says that we can "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."

I believe, however, that this claim is simply irrelevant to the problem of the evaluation of rival manuals of translation. I shall argue that there is machinery available for the evaluation of sets of analytical hypotheses, and of
the manuals of translation to which the sets belong, even though it may be true, as Quine claims, that we can "wonder about the meaning of a foreigner's remark without reference to any one set of analytical hypotheses." Quine explicitly denies that such machinery is available; I will argue, first, that his comments about analytical hypotheses do not support that denial, and, second, that Quine himself seems to suggest several good reasons for thinking that the machinery is available.

If rival sets of analytical hypotheses -- and hence manuals of translation -- could be evaluated, what would be the grounds of this evaluation? The grounds are not to be found, I suggest, in other sets of analytical hypotheses or manuals of translation. If this is so, then Quine's conclusion, that translation is indeterminate, does not follow from the assertion that we can wonder about the meaning of a remark without reference to any one set of analytical hypotheses.

In order to see where grounds might come from which would allow for assessment of alternative manuals of translation, we need only look to Quine's own discussion of those cases in which translation is not indeterminate. In these cases, evidently, there are grounds available for comparison of alternative manuals of translation, and there is evidence available, or forthcoming, which may be cited in making rational decisions as to which one of alternative manuals is preferable. In these cases, I shall argue, the very same machinery is available to the linguist in assessing manuals of translation as is available to the scientist in assessing scientific theories. And, I shall argue that Quine has offered no reason for thinking that this machinery is not available in all cases of translation.

So far, Quine has suggested three general cases in which there are grounds which may be adduced in support of a choice of one translation over another. The final two cases to be considered are the most important, because they illustrate the possibility of assessing alternative manuals of translation, which is the point at issue.

In the first case (case "a"), it is simply noted that because certain sentences can be translated without recourse to analytical hypotheses, there are grounds which support certain translations, in these cases, over alternatives. As Quine says, the question as to which translation is correct is open in those cases in which the expression to be translated "is of one of the limited
sorts that can be translated without recourse to analytical hypotheses" (W&O, p. 76). Specifically, observation sentences and truth functions can be translated without the aid of analytical hypotheses, solely on the basis of stimulus meaning. Therefore, there are rational grounds available for supporting answers to questions concerning the correctness or incorrectness of translations of observation sentences and truth functions.

Quine says that there is "uncertainty" in these cases, but it is the "normal inductive" kind of uncertainty (W&O, p. 68). This remark may be taken as admitting an important parallel between the problem of ascertaining theoretical truths in science and that of ascertaining the correct translation of observation sentences and truth functions in radical translation. Affirmations in science of the truth of theoretical sentences are occasioned by the same "normal inductive uncertainty" now seen to surround the translation of observation sentences and truth functions. In our terms, both scientific theories and cases of translation of observation sentences are underdetermined, and not indeterminate.

In discussing the two cases in which Quine suggests that alternative manuals can be compared and evaluated, it will be helpful to have before us Quine's own words. He says:

Where physical theories A and B are both compatible with all possible data, we might adopt A for ourselves and still remain free to translate the foreigner either as believing A or as believing B.

Such choice between A and B in translation could be guided by simplicity. By imputing B to the foreigner we might come out with shorter and more direct translations, and with less in the way of elaborate contextual paraphrases, than by imputing A to him. That is one possibility. A second is that both choices, A and B, require forbiddingly circuitous and cumbersome translation rules. In this case we might regard the foreigner as holding neither A nor B....(RI, p. 180, emphasis added).

Quine goes on to describe a third possibility, in which "A and B are both reasonably attributable." "In this event," Quine says, "our choice would be determined simply by the accident of hitting upon one of the two systems of translation first." In other words, in this case translation is indeterminate. As Quine says, the indeterminacy
of this third case is "what I am getting at in arguing the indeterminacy of translation" (RI, p. 181).

This last remark suggests that in the first two cases (case "b" and "c" respectively) presented in RI, translation is not indeterminate. Case b suggests that considerations of simplicity can enter into judgments concerning the acceptability of manuals of translation. It is widely acknowledged, however, that considerations of simplicity also enter into questions concerning the acceptability of scientific theories. If a theory is overly complex, requiring the postulation of many ad hoc hypotheses, or if it is simply more complex than competing theories, then it is judged as unacceptable. In Quine's terms, that simplicity is a valuable characteristic of scientific theories is determined by our current aggregate science; simplicity is one characteristic of theories which our current aggregate science holds to be valuable.

Simplicity is also, however, an important characteristic of manuals of translation. The crucial point in case b is that even though in it we were wondering about which manual of translation to accept -- i.e., about what the native meant, apart from any one set of analytical hypotheses or manual of translation -- there was some utility which could be used in evaluating the two competing manuals. This utility cannot have been contributed by any set of analytical hypotheses, but must have been contributed by some more inclusive theory, in the context of which the competing manuals were constructed and are being evaluated. In other words, that simplicity is a valuable characteristic of translations, and that manuals yielding simple translations are, prima facie, preferable over manuals yielding complicated translations, are judgments which cannot be determined by any particular manual of translation. For, if simplicity were only valuable according to the manual yielding theory B, then the manual yielding theory A would not necessarily be unacceptable, even though it yields a more complicated translation. Any utilities, such as simplicity, which are used in evaluating alternative manuals, must be determined to be utilities by some more inclusive theory, within which both manuals have been constructed and are being evaluated. In this case, the consideration used in evaluating the alternative manuals -- that of simplicity -- is also used in evaluating alternative scientific theories. It does not seem implausible, then, to conclude that in this case, anyway, the grounds of the evaluation of both alternative manuals and alternative scientific theories is the same: namely, our current aggregate science. It is this, and not any manuals of translation or set of analytical hypotheses, that determines that simplicity is a valuable characteristic.
of both translations and scientific theories.

In case c, Quine points out that there are grounds available for accepting or rejecting translations, and the manuals yielding these translations, in cases in which neither of two alternative translations, both compatible with all the native's dispositions to assent to and dissent from sentences, are simple enough. "In this case," as we saw, "we might regard the foreigner as holding neither A nor B." This remark makes it clear that in some cases, anyway, there are grounds which may be adduced in support of a rejection of manuals of translation. In such cases, although no alternative manual may be selected as the correct or preferable manual, the question of the acceptability of manuals is still open: translation is not, in these cases, indeterminate.

If translation, and manuals yielding these translations, are rejected rather than accepted, then these rejections must still be based on some criteria. And again, the criteria cannot come from any particular manual of translation, or set of analytical hypotheses. In case c, the operative criterion is that of simplicity, as it was in b. This time, application of the criterion dictates rejection of both manuals rather than acceptance of one. This illustrates even more clearly the fact that the grounds of rejection cannot come from either of the alternative manuals, since both were rejected. Again, the judgment that simplicity is a valuable characteristic of manuals of translation must come from some more inclusive theory. And, again, it does not seem implausible to assume that this inclusive theory is that same aggregate science, which, according to Quine, provides the grounds for evaluation of scientific theories.

In some cases, then, it seems that our current aggregate science provides grounds for the evaluation of alternative manuals of translation. As we have seen, Quine holds that there are always some grounds available on which to base answers to questions about the acceptability of scientific theories: if there were any cases in which grounds were not available, then theory would be indeterminate, which it is not. Further, we have seen that Quine holds that the grounds, which are always available, for evaluating scientific theories are determined by our current aggregate science. That is, the standards according to which we judge scientific theories are determined by our aggregate science. The question is raised, then, as to why these standards are not always available to the linguist, in evaluating manuals of translation -- especially if these standards are available some of the time, as they seem to be.
So far as I can see, Quine provides no argument in support of his contention that in some cases the standards determined by our aggregate science are not available to the linguist in evaluating manuals of translation. In particular, we have seen that his comment concerning the linguist's reliance on analytical hypotheses do not support this contention. I conclude, therefore, that Quine's final argument, just considered, does not support his indeterminacy thesis.

Before concluding, it might be well to answer an objection that might be raised against my reading of the passage from HI, in which Quine discusses cases in which, according to my reading, translation is not indeterminate. It might be argued that in this passage Quine is not discussing cases in which translation is not indeterminate. Rather, so the argument might go, Quine is saying that if only translation were not indeterminate, then considerations of simplicity could enter into judgments concerning the acceptability of manuals of translation. Thus, Quine would be interpreted as saying that "choice between A and B in translation could be guided by simplicity," were it not for the fact that translation is indeterminate.

I do not think, however, that this objection will stand up. In the first place, Quine's comment that it is his third case, in which both alternative translations are equally simple, that he is "getting at in arguing the indeterminacy of translation," renders implausible the view that indeterminacy also obtains in cases b and c. If it did obtain in these cases, Quine would have said so.

In the second place, even granting our critic's claim that Quine is describing in cases b and c utilities which could enter into judgments concerning the acceptability of manuals of translation, were translation not indeterminate, it may be replied that Quine has given no reason for thinking that such utilities cannot enter in. That is, let us grant that Quine is saying that in cases b and c simplicity does not enter, because translation is indeterminate. But, Quine has given no reason, so far as I know, as to say simplicity should not guide choices between alternative translations. Nor has he given good reasons for thinking that translation is indeterminate, as I have argued here.

That translation is indeterminate cannot be the reason why consideration of simplicity cannot enter into judgments about the acceptability of manuals of translation. If anything, it must be because considerations of simplicity do not enter in that translation is indeterminate. But, to repeat, Quine has offered no reason at all for thinking that considerations of simplicity cannot enter in.
I conclude, therefore, that it does not seem implausible to think that the "canons of scientific method," which are determined by our current aggregate science, and provide common ground on which rival scientific theories can be assessed, also provide common ground on which rival manuals of translation can be assessed. And, if rival manuals of translation may be compared and evaluated, and if evidence may be cited in support of adopting one manual over others, then translation is underdetermined and not indeterminate. It has not been my purpose in this paper to define a thesis of the underdeterminacy of translation; rather, it has been my purpose to point out that no argument has yet been offered by Quine which supports his thesis of the indeterminacy of translation.

7Thanks are due to Richard Rudner and D. J. Hockney for reading and commenting on an earlier draft of this paper.

Hugh T. Wilder
Department of Philosophy
Miami University
Oxford, OH 45056