THE REFUTATION OF INDETERMINACY*

A WELL-CHOSEN title, unlike a name, informs us about its bearer. The title "Über Sinn und Bedeutung"[1] was well-chosen, and so too was the title *Word and Object*.[2] Gottlob Frege's essay claims that words have sense as well as reference. W. V. O. Quine's book denies precisely this. Its counterclaim is eloquently expressed by the conspicuous absence of a term translating "Sinn" in its title.

It is now widely thought that *Word and Object*, together with Quine's earlier writings in *From a Logical Point of View*,[3] establishes his claim that the traditional intensionalist's notions of sense, synonymy, and analyticity cannot be made objective sense of, and, consequently, must be abandoned in serious studies of language. Accordingly, these works have been a watershed for twentieth-century Anglo-American philosophy, radically changing how philosophers think about language, logic, and nearly every other area of investigation. Quine's skepticism, especially as expressed in his indeterminacy thesis, have all but eliminated intensional approaches to language from the current philosophical scene.

On the metaphysical side, Quine's arguments were instrumental in resurrecting philosophical naturalism. Frege had all but single-

[1] Gottlob Frege, "On Sense and Reference," in *Translations from the Philosophical Writings of Gottlob Frege*, Peter Geach and Max Black, eds. (Oxford: Blackwell, 1952), pp. 56–78.
[2] W. V. O. Quine (Cambridge: MIT, 1960). Hereafter WO.
[3] Cambridge: Harvard, 1953. Hereafter LPV.

handedly stemmed the tide of nineteenth-century naturalism in the philosophy of language, logic, and mathematics.[4] Carnap incorporated Frege's achievement into logical empiricism, giving that philosophy a strong non-naturalist orientation. Frege's sharp analytic-synthetic distinction, as explicated in Carnap's formal semantics, gave abstract objects and necessary truths sanctuary on the analytic side of the distinction. The distinction stood as the principal barrier to a return of an uncompromising naturalism in the spirit of J. S. Mill. This is why Quine, whose sympathies are clearly with empiricism, sets out to attack an empiricism in "Two Dogmas of Empiricism" (LPV, pp. 42–46). The arguments in that paper and his deployment of the indeterminacy thesis against the possibility of identity conditions for senses were widely seen as bringing this barrier crashing down and thereby opening the way for a neo-Millian naturalism of the sort sketched in "Carnap on Logical Truth."[5] Hence, when the linguistic turn shifted the emphasis in philosophy to language, much subsequent philosophy came to be done within a naturalistic framework which might be described as Humean epistemology minus the category of relations of ideas. Matters of fact are all that matter.

   Given the momentous changes Quine's arguments have brought about, it is desirable that they be subjected to careful and continuing scrutiny. In this paper, I examine the argument for indeterminacy from a new angle and find that it does not work. If I am right, there is a straightforward sense in which the indeterminacy thesis is refuted. Skepticism about translation, like skepticism about other things of which common sense assures us, incurs a burden of proof in challenging a common-sense view. If the skeptic provides reasons of sufficient strength to discharge the onus of proof, we are presented with an advance in knowledge whose surprising character marks it as a discovery of the most profound sort. Thus, we attach the importance we do to Quine's argument in large part because it threatens to upset our common-sense view that there is always a right and a wrong translation, even when the options differ the way "rabbit," "rabbit stage," and "undetached rabbit part" do. But, if the skeptic's reasons lack the strength to discharge the onus of proof and can establish only the logical possibility that common sense is wrong, we are presented with nothing more than an "absolute skepticism"[6] which,

[4] Hans Sluga, *Gottlob Frege* (Boston: Routledge & Kegan Paul, 1980), pp. 17–34.
[5] In *The Philosophy of Rudolf Carnap,* Paul A. Schilpp, ed. (LaSalle, IL: Open Court, 1963), pp. 385–406; and also, of course, at the end of "Two Dogmas of Empiricism" in LPV.
[6] This notion is adapted from "The Refutation of Idealism," in G. E. Moore, *Philosophical Studies* (Boston: Routledge & Kegan Paul, 1922), p. 30.

applying to all forms of knowledge, proves too much. In this in-
stance, the skeptic's claim is refuted, for, unchallenged in any spe-
cific way, common sense reasserts itself.[7]

Quine wants to show that the ordinary view of translation as ex-
pressing the same meanings in different languages involves a "scien-
tific mistake" akin to believing in the gods of Homer. To show this,
he sets out to show that the intensionalist tradition from Kant to
Carnap which claims to make objective sense of this view is, at bot-
tom, no better than mythological explanation. Quine goes right to
the heart of the matter: the relation of translation. Translation is
critical because it is the only relation that provides interlinguistic
identity conditions that are discriminating enough to individuate the
fine-grained propositions of intensionalism. Such maximally fine-
grained propositions are what enable intensionalists to claim that
their position is the only one that does justice to the full range of our
ordinary, pretheoretical intuitions about linguistic structure. For ex-
ample, only conditions for propositional identity based on synonymy
seem capable of accounting for the intuition that the sentences "The
sentence 'Two is less than three' means that two is less than three"
and "The sentence 'The even prime is less than three' means two is
less than three" have different truth values. Also, Frege, as is well
known, argued (in "On Sense and Reference") that reference to
such maximally fine-grained propositions is necessary in order to
formulate the principles of logic. Hence, without the relation of
translation, the intensionalist can claim no advantage in the study of
language, and Frege's move to take propositions in logic as senses of
sentences in language does not get off the ground. Thus, if Quine
can establish that no objective sense can be made of equivalence of
meaning for sentences of natural languages, intensionalism will be
discredited as completely as the Homeric creation myths.[8]

Some philosophers offer a quick rebuttal to this line of argument.
They claim that talk about meaning no more requires a statement of
identity conditions to legitimatize it than talk about such things as
nations or works of art require a statement of identity conditions to
legitimatize them. But neither Frege nor other intensionalists with
the hope of vindicating intensionalist semantics as the best scientific
account of language can afford so cavalier an attitude. Although, in
the early stages of research, it might be necessary for intensionalists

[7] It is, of course, one thing having the common-sense view of translation and quite
another having the kind of justification for it that would be provided by a full-
fledged linguistic theory of translation. Although having both is clearly more desir-
able than having only the former, the latter is not necessary to refute the skeptic's
claim.

[8] *Philosophy of Logic* (Englewood Cliffs, NJ: Prentice-Hall, 1970), p. 3.

to tolerate entities without identity, it would not be advisable for them to build such indulgence into the research plan. Hence, I shall accept the need for identity conditions on exactly Quine's terms. If he can show that translation is indeterminate, I shall straight off concede that sense make no sense.

The character of Quine's argument for indeterminacy is indicated in such passages as:

> . . . if the posit of propositions is to be taken seriously, eternal sentences of other languages must be supposed to mean propositions too; and each of these must be identical with or distinct from each proposition meant by an eternal sentence of our own . . . Surely it is philosophically unsatisfactory for such questions of identity to arise as recognized questions, however academic, without there being in principle some suggestion of how to construe them in terms of domestic and foreign dispositions to verbal behavior (WO, p. 205).

The aim of Quine's argument is thus to establish that, in principle, there is no way to construe questions about identity of intensional objects in terms of objective facts about verbal behavior. The reason is that translation is indeterminate in the sense that

> . . . manuals for translating one language into another can be set up in divergent ways, all compatible with the totality of speech dispositions, yet incompatible with one another. In countless places they will diverge in giving, as their respective translations of a sentence of one language, sentences of the other language which stand to each other in no plausible sort of equivalence however loose (*ibid.*, p. 27).

I shall make no objection to the issue being put in terms of speech dispositions. It seems to me much the same thing whether we talk of a speaker's speech dispositions, in particular, dispositions to characterize sentences verbally, or of a speaker's overtly expressed intuitive judgments about sentences. Hence, if there is no way to construe questions of identity of sense in terms of speech dispositions, I am prepared to concede that intensional objects are on a par with Homeric gods.

I shall also make no objection to Quine's statement that "the behaviorist approach is mandatory."[9] The behaviorism he has in mind here is not the dreaded reductive doctrine of days gone by, but merely a way of putting the study of language on a par with other sciences by requiring the linguist's theoretical constructions to be justified on the basis of objective evidence in the form of the overt behavior of speakers. Quine writes:

[9] "Indeterminacy of Translation Again," this JOURNAL, LXXXIV, 1 (Jan. 1987): 5–10.

> In psychology one may or may not be a behaviorist, but in linguistics one has no choice. Each of us learns his language by observing other people's verbal behavior and having his own faltering verbal behavior observed and reinforced or corrected by others. . . . There is nothing in linguistic meaning, then, beyond what is to be gleaned from overt behavior in overt circumstances (*ibid.*, p. 5).

Quine's behaviorism is thus a behaviorism one can live with. (Indeed, if it were not, it would be too controversial in the present cognitive climate to bear the weight of the indeterminacy argument.) Quine's behaviorism merely takes linguists out of their arm chairs and puts them in the field facing the task of having to arrive at a theory of a language on the basis of the overt behavior of its speakers in overt circumstances. Instead of challenging Quine's behaviorism, I shall challenge his claim about what can be gleaned from such behavior in such circumstances. I shall try to show there is intensional grain to be gathered.[10]

Quine's conclusion that the totality of linguistic evidence cannot eliminate incompatible translation manuals is developed in the situation he calls "radical translation." He illustrates this situation with a jungle story about a field linguist trying to choose among putative translations for the expression 'gavagai' in an alien language. Quine argues that the informant's dispositions to verbal response in such a translation situation are "incapable of deciding among 'rabbit', 'rabbit stage', and various other terms as translations of 'gavagai' " (WO, pp. 71/2). Although nothing near a proof, the argument exhibits an unbreakable symmetry among the evidential considerations that can be adduced to justify the various translations. Whatever we can say on behalf of one translation, we can also say on behalf of the others. The reason is that the ostensive acts of the field linguist and the informant cannot refer to a rabbit without referring to a rabbit stage or an undetached rabbit part, nor any of these without the others, and radical translation contains nothing that enables the linguist to impose controls on hypotheses which enable them to choose between extensionally equivalent translation options. Thus, the argument leaves us with no grounds on which to resist its conclusion.

Quine argues that reflection on "the nature of possible data and method" in radical translation suffices to make us "appreciate the indeterminacy":

[10] Why does Quine not think behaviorism is mandatory for the psychologist as well? Does he not suppose that we learn more than just languages by observing others and being corrected by them? Is "introspective semantics" (*ibid.*, p. 9) no good, but introspective psychology alright?

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

But, despite the assurance, there can be doubts. I concede that there
is no doubt about Quine's conclusion if restricted to *radical trans-
lation*. But it is not clear how *actual translation*, to which Quine's
conclusion must also apply if indeterminacy is to matter philosophi-
cally, is related to radical translation. Thus, I think there can be
doubts about the step in Quine's argument from his account of
radical translation to his conclusion as applied to actual translation.

The doubt may be fleshed out as follows. Radical translation, it can
be argued, is Quine's creation. He constructed it so that nothing
beyond referential considerations provide evidence for translations.
Thus, it is guaranteed that any evidential support for one among a set
of referentially indiscernible hypotheses, such as "rabbit," "rabbit
stage," and "undetached rabbit part," can be matched with equal
evidential support for each of the others. But could not Quine's
creation fail to mirror relevant evidential features of actual transla-
tion situations? We have, as yet, no grounds for rejecting the claim of
intensionalists that actual translation situations provide linguists with
evidence relevant to sense differences among the competing hypoth-
eses which can be used to discriminate among them. If linguists in
actual translation situations have such "independent controls,"
there is no evidential symmetry in those situations, and, hence, no
indeterminacy.

My point so far is only that Quine has to say something to make us
"appreciate the indeterminacy" for actual translation. Quine needs
to clarify the relation between radical translation, as he defines it,
and actual translation, as it exists or could exist in the practice of real
linguists. Quine seems to address this need. He presents radical
translation as the limiting case of actual translation, i.e., as the case
where historical differences between the languages and cultural dif-
ferences between its speakers are maximal. It is presented as the
most philosophically perspicacious case of actual translation in virtue
of being the one where the issue about meaning is least likely to be
confused by historical and cultural similarities.

But the issue is not so easily settled. We have been given likenesses
between radical translation and the limiting case of actual transla-
tion, but have not as yet been given a reason for identifying them.[11]

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[11] Note, for example, how in "Indeterminacy of Translation Again" Quine slides
unfalteringly from speaking of "translation" and "our linguist" (pp. 5/6) to speak-
ing of "radical translation" and "our radical translator" (p. 7).

The acceptability of an identification depends on whether actual translation is *in all relevant respects* like radical translation. Now, one relevant respect is surely whether or not, in actual translation, the matching of expressions as synonymous also takes place in the absence of "independent controls." For it is the absence of such controls in radical translation which causes evidential symmetry and indeterminacy. Hence, we must be given a reason for believing such controls do not exist in actual translation.

Quine has what he thinks is a reason: the existence of such controls depends on the existence of intensional objects in the way that the existence of independent controls in physics depends on the existence of physical objects; but to suppose that "translational synonymy at its worst is no worse off than physics" is to "misjudge the parallel" (WO, p. 75). There is a fundamental difference between intensional semantics and a genuine science like physics: in physics, "the parameters of truth stay conveniently fixed most of the time; not so with "the analytic hypotheses that constitute the parameter of translation" (WO, p. 76). Quine explains:

> Something of the true situation verges on visibility when the sentences concerned are extremely theoretical. Thus who would undertake to translate 'Neutrinos lack mass' into jungle language? If anyone does, we may expect him to coin words or distort the usage of old ones. We may expect him to plead in extenuation that the natives lack the requisite concepts; also that they know too little physics. And he is right, except for the hint of there being some free-floating, linguistically neutral meaning which we capture in 'Neutrinos lack mass', and the native cannot (WO, p. 76).

There is no domain of linguistically neutral meanings corresponding to the domain of physical objects, and, consequently, no facts against which to judge the truth of analytical hypotheses, since such hypotheses assert that a sentence in the target language expresses the same linguistically neutral meaning as one in the home language. Thus, if there are no meanings, it makes no sense to talk of a scientific choice between competing analytical hypotheses.

As Quine sees it, the correct comparison with physics is this. Theories in physics are underdetermined by the available observational evidence and also by the total possible evidence, but not subject to indeterminacy (WO, pp. 75/6).[12] Underdetermination is only a matter of "empirical slack" which can be taken up methodologically, that is, genuinely divergent physical theories which survive confrontation with the total evidence can be adjudicated by appeal to

[12] Up-dated in "On the Reasons for Indeterminacy of Translation," this JOURNAL, LXVII, 6 (March 26, 1970): 178–183.

methodological canons like simplicity, depth of explanation, etc. The differences between such theories are substantive because there is a physical fact they are about. Intensional semantics, in contrast, suffers from a condition far worse than underdetermination. It suffers from indeterminacy whose etiology is the lack of a fact of the matter for semantic theories to be right or wrong about (WO, pp. 76–79; also LPV, p. 63). Therefore, whereas the condition of physics can be treated methodologically, the illness of intensional semantics is terminal.

But this reason for thinking independent controls do not exist in translation is only as good as the reason Quine has for saying that there are no linguistically neutral meanings. It is not enough for Quine just to claim that "[t]he discontinuity of radical translation tries our meanings: really sets them over against their verbal embodiments or, more typically, finds nothing there" (WO, p. 76). Nothing is established by a mere claim. Moreover, if Quine is employing "the discontinuity of radical translation" to argue for there being no fact of the matter in actual translation, then, once the question is put as we have put it, i.e., in terms of how the step from radical translation to actual translation is justified, this employment seems to beg the question. At this point, all that Quine can legitimately say is that there is no fact of the matter about meaning in the translation situation that he invented. He is not entitled to say that there are no linguistically neutral meanings in actual translation. Recall that our earlier doubts about the identification of radical translation with the extreme of actual translation arose because Quine had not established that the cases do not relevantly differ with respect to the existence of "independent controls." But the existence of independent controls, as we have seen, is not unrelated to the existence of meanings.

Furthermore, even if Quine had secured the step from radical translation to actual translation, he still would not have a basis for claiming that there are no linguistically neutral meanings in actual translation. For, in fact, he has not even ruled out the possibility of meanings in *radical* translation! He has shown that there is no evidential basis for choosing between rival analytical hypotheses, but this establishes no more than the unknowability of meanings. But meanings, like Kant's noumena, could exist even if unknowable. No mere epistemological considerations, such as those in Quine's discussion of radical translation, entail an ontological conclusion such as he draws about linguistically neutral meanings. Such a conclusion introduces an ontological skepticism over and above his already asserted epistemological skepticism. Thus, rather than motivate the

claim that actual translation is indeterminate, the ontological skepticism only increases the burden of proof.

Hence, we have a new question: What is Quine's argument for claiming that there are no linguistically neutral meanings?[13] I think, without doubt, that he has one. It is a mistake for Noam Chomsky to represent Quine as simply stipulating that linguistics, in contrast to other sciences, can have no general theories.[14] It is true that Quine does not at this point explicitly present an argument, or even cite one, but surely a philosopher as acute as Quine must see that an argument is needed to back up his claim. He must know that, without one, indeterminacy of translation is unsupported, and intensional semantics can be accused of nothing more serious than underdetermination. In this case, the omnipresence of divergent translations is of no philosophical interest, indicating nothing more exciting than gaps in knowledge of semantic fact or insufficient applications of scientific methodology.

It would also be a mistake to suppose Quine is simply appealing to some well-known philosophical doctrine like behaviorism to back up his claim that there are no semantic facts. As we have seen, his behaviorism would not be equal to the task. It is not the militant doctrine which brands as scientific heresy everything that cannot be strictly defined in terms of stimulus and response. Furthermore, Quine's claim does not derive from old–fashioned verificationism. If Quine has verificationist scruples, they have to be rather mild ones in order to allow him to countenance highly theoretical entities in science, such as the ten dimensional wonders of contemporary physics and the objects of set theory. Finally, the claim could not be a consequence of physicalism, either. Physicalism would allow meanings so long as they are reducible to brain states. Quine's view is not that intensionalists have been lax in showing that meanings are reducible to physical states, but that they have been deluded in thinking there are any such things to be reduced. It would be absurd for a physicalist to undertake a physicalistic reduction of Homeric gods.

We get a clue to what Quine's argument really is by noting that he makes the claim that there are no linguistically neutral meanings with the confidence of someone introducing a lemma he has already

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[13] We need not interpret Quine as taking the discontinuity of radical translation to be a basis for inferring the absence of linguistically neutral meanings. Perhaps all he means is that such discontinuity helps us see that there are not any meanings. But, even on this interpretation, the question in the text stands, since, here too, without a justification, Quine's case would come down to a bare assertion.

[14] "Quine's Empirical Assumptions," in *Words and Objections*, Donald Davidson and Jaako Hintikka, eds. (Boston: Reidel, 1969), p. 62.

proved and can reasonably expect his readers to know. I submit that
this is exactly it: Quine thinks he has already given a conclusive
argument against meanings in his earlier works, particularly in his
famous paper "Two Dogmas of Empiricism." Indeed, the argument
there is directly to the point. It aims to show that we can make no
objective sense of synonymy, and, if this argument were conclusive,
no respectable theory would quantify over meanings and consider-
ations of parsimony would oblige us to deny there are meanings.

That it is correct to interpret Quine's argument for indeterminacy
to have this reference back to his earlier work is shown not only by
the tone and logic of his reasoning in *Word and Object*, but also by
explicit statements about his overall anti-intensionalist strategy in
various places. In the early paper "The Problem of Meaning in Lin-
guistics" (LPV, pp. 47–64), Quine makes it clear that, even then, he
thought of his criticism of the analytic-synthetic distinction as un-
dercutting the basis for claims that there is a fact of the matter in
connection with meaning. Referring to a restatement of his argu-
ments against synonymy from "Two Dogmas of Empiricism," Quine
says that construction of a lexicon for translating a language from a
radically different culture is not a well-defined task because such
construction suffers from a "paucity of explicit controls" (LPV, p.
63). (Note we even have the same notion of absence of controls
which figures so prominently in *Word and Object*.) Quine completes
the thought, saying:

> The finished lexicon is a case, evidently, of *ex pede Herculem*. But there
> is a difference. In projecting Hercules from the foot we risk error, but
> we may derive comfort from the fact that there is something to be wrong
> about. In the case of the lexicon, pending some definition of synonymy,
> we have no statement of the problem; we have nothing for the lexicogra-
> pher to be right or wrong about (LPV, p. 63).

So, if a definition of synonymy is ruled out by "Two Dogmas of
Empiricism," then there is nothing for the lexicographer to study.

In the recent paper "Indeterminacy of Translation Again," Quine
provides further evidence for taking "Two Dogmas of Empiricism"
to provide the argument for his claim in *Word and Object* that there
are no meanings:

> Considerations of the sort we have been surveying are all that the radical
> translator has to go on. This is not because the meanings of sentences are
> elusive or inscrutable; it is because there is nothing to them, beyond what
> these fumbling procedures can come up with. Nor is there hope even of
> codifying these procedures and then *defining* what counts as translation
> by citing the procedures; for the procedures involve weighing incom-

mensurable values. How much grotesqueness may we allow to the na-
tive's beliefs, for instance, in order to avoid how much grotesqueness in
his grammar or semantics? (p. 8).

The point here is that no comparison is possible between hypotheses
about beliefs and hypotheses about meaning, because such a com-
parison assumes an analytic-synthetic distinction. If there were such
a distinction, the linguist could, in principle, decide whether a piece
of information belongs in the theory of the informant's language or
in the theory of his or her extra-linguistic beliefs. Without an ana-
lytic-synthetic distinction, such decisions involve "weighing incom-
mensurable values."

Given that "Two Dogmas of Empiricism" is intended to supply the
argument for the claim that there are no meanings which does not
explicitly appear in *Word and Object*, Quine's overall argument for
indeterminacy of translation can be reconstructed as follows. Assum-
ing that "Two Dogmas of Empiricism" removes the possibility of a
linguistically universal synonymy relation,[15] there can be no identity
conditions for intensional objects, and, as a consequence, we must
abandon the idea of linguistically neutral meanings serving as the
common content of a sentence and its translation. Thus, there is no
parallel between semantics as conceived in traditional intensionalism
and *bona fide* sciences like physics. In physics, there are objects of
study, and so physics suffers only from underdetermination. In se-
mantics, there are no objects of study, and, hence, there can be no
evidence to provide controls on analytical hypotheses and to make
objective sense of talk about rational choices among theories of
meaning. Thus, there is nothing to distinguish actual translation
from radical translation, and Quine can identify the latter with the
limit case of the former. The symmetry argument for radical transla-
tion transfers to actual translation, and he can conclude that, in
actual translation, divergent systems of analytical hypotheses fit the
totality of speech dispositions to perfection. He can then say, justifi-

[15] Another line of argument is this. Quine's aim is to show, contra Alonso Church,
Frege, and Carnap, that there are no intensional objects for intensionalists to use in
their statement of principles of logic. Because it is into logic that intensionalists
propose to introduce senses, the relation of synonymy which provides the identity
condition must be specified, as Quine puts it in "Two Dogmas of Empiricism" (pp.
33/4), for variable 'S' and 'L'. The reason is that principles of logic express a
language neutral implication relation. That is to say, logic concerns a notion of
implication which is specified for variable 'S' and 'L', not a notion like implication-
in-Italian. Thus, if Quine can show there is no linguistically universal notion of
synonymy, there will be no linguistically neutral meanings, and, hence, nothing for
intensionalists to appeal to extend logic in a naturalistically dubious direction.

ably, that the limits of possible data for radical translation make the
indeterminacy of translation certain.[16]

Having located the "missing" argument, I now want to show that it
does not work, and, hence, that the argument for indeterminacy
does not work either. The argument in "Two Dogmas of Empiri-
cism" takes the form of a proof by cases. It begins with an enumera-
tion of the areas where it would be reasonable to look for an explana-
tory paradigm to use in trying to make objective sense of the con-
cepts in the theory of meaning. The areas are definition generally,
logical theory, and linguistics. Quine asks whether the methods for
explaining concepts in any of these areas can explain synonymy and
analyticity. He examines the areas in turn. He argues that the avail-
able paradigms in the case of definition either assume prior synon-
ymy relations or else have nothing to do with meaning (LPV, pp.
24–27). In the case of logical theory, he argues that Carnapian
meaning postulates and semantical rules shed no light whatever on
the nature of synonymy and analyticity (LPV, pp. 32–37). In the case
of linguistics, he argues that the methods for defining concepts are
demonstrably unable to provide noncircular definitions of these
concepts (LPV, pp. 27–32). Since these cases exhaust the areas
where we might expect to find an explanatory paradigm appropriate
to logico-linguistic concepts like analyticity and synonymy, Quine
concludes that there are no methods for clarifying synonymy and
analyticity.

Quine's argument in the case of definition is absolutely compel-
ling. So is his argument in the case of logic. But the final argument
needed to complete the proof by cases, the argument for the case of
linguistics, is quite a different matter. Once its structure is revealed,
it will be clear that it does nothing to establish that attempts to
explain analyticity and synonymy in linguistics must fail. I shall dis-
cuss this argument in some detail both because of its importance to
the indeterminacy thesis and because the unfamiliarity of philoso-
phers with linguistics is surely a major factor in the readiness with
which they have accepted Quine's account of its methods.

Quine's argument begins by identifying *substitution criteria* as the
proper method in linguistics for defining concepts like analyticity
and synonymy. He explains as follows:

> So-called substitution criteria, or conditions of interchangeability, have
> in one form or another played central roles in modern grammar. For the
> synonymy problem of semantics such an approach seems more obvious
> still. However, the notion of the interchangeability of two linguistic

[16] "Indeterminacy of Translation Again," p. 9.

forms makes sense only in so far as answers are provided to these two questions: (a) In just what sorts of contextual positions, if not in all, are the two forms to be interchangeable? (b) The forms are to be interchangeable *salvo quo?* Supplanting one form by another in any context changes something, namely form, at least; and (b) asks what feature the interchange is to leave invariant. Alternative answers to (a) and (b) give alternative notions of interchangeability, some suited to defining grammatical correspondences and others, conceivably, to defining synonymy (LPV, p. 56).

It is important to recognize that the substitution criteria Quine borrows from "modern grammar" are not simply the customary substitution operations in logic and mathematics. To be sure, like those operations, substitution criteria specify a concept on the basis of a feature which remains invariant when and only when the elements that replace one another in the chosen context belong to the extension of the concept. But, in the case of the substitution criteria from "modern grammar," there is the special requirement that statements of the context and of the invariant feature not contain concepts belonging to the family of the concept to be defined. Thus, definitions taking the form of substitution criteria may fail either because the feature does not correlate with all and only expressions in the extension of the concept or because the special requirement is not met, that is, the feature or the substitution context is stated using the concept to be defined or a concept in its family.

Given that substitution criteria are the proper method for defining linguistic concepts, Quine has an easy time demonstrating that the concepts of analyticity and synonymy cannot be defined in linguistics. Suppose we wish to define synonymy. The context must be either intensional or extensional. If we choose an intensional context, say "Necessarily, _____ ," we can use truth as the feature which is to remain invariant in substitutions, but then we violate the requirement of noncircularity at the outset because the context has to be characterized using synonymy or other concepts from the theory of meaning.[17] If we choose an extensional context, we get the definition off the ground, but then we can no longer use truth as the feature which is to remain invariant. For, in extensional contexts, truth will not discriminate coextensive but nonsynonymous expressions from synonymous expressions. It is thus necessary to move to something stronger like necessary truth or analyticity. But this vio-

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[17] Quine observes that Carnap's possible worlds account of necessity uses meaning relations among state descriptions to guarantee logical independence (LPV, p. 23).

lates the noncircularity requirement, too, because such notions are defined in terms of synonymy.

This, then, is the critical argument on which the argument for indeterminacy in *Word and Object* depends. The trouble with it is that the assumption from which it proceeds, that substitution criteria are the proper way to clarify concepts in linguistics, is easily knocked down. There is nothing in favor of the assumption, and there are strong *a priori* and historical arguments against it.

Quine does not justify relying on substitution criteria. Rather, he notes that this approach to definition has "in one form or another played central roles in modern grammar." This, however, is only to say that the approach occupied a central place in the phonological and syntactic investigations which took place during the Bloomfield-ian period in linguistics (roughly 1930s–1950s). Quine offers nothing beyond their place in Bloomfieldian linguistics to show that substitution criteria are indispensible to the science of language and hence a valid approach to defining synonymy. But the fact that one school of thought in a science at one time in its history has a particular methodological practice means very little, given how frequently we see schools of thought come and go in science and old paradigms replaced with new ones. In fact, even as Quine wrote, the positivist foundations of Bloomfieldian linguistics were being eroded by what we can now see was a successful critique of logical positivism in the philosophy of science.[18]

An *a priori* reason for thinking that substitution criteria are neither the only nor the preferred form of definition in linguistics is that physics, mathematics, and logic provide an example of another form of definition which could be adapted for concepts in linguistics. This is the familiar approach of defining a concept on the basis of an axiomatic or recursive specification of the relations between it and other concepts in its family, as, for example, in the Dedekind-Peano axiomatization of arithmetic concepts, or the axiomatization of the truth-functional connectives in a standard sentential calculus. The difference between such *theoretical definitions*, as I shall call them, and substitution criteria is that theoretical definitions can use concepts belonging to the same family as the concept to be defined. Effective theoretical definitions explain such concepts in a way which captures the structure of the primitives in the family of concepts. The degree of relatedness exhibited among the concepts in the fam-

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[18] To see how important a role positivism played in shaping Leonard Bloomfield's reconstruction of linguistics, see his "Language or Ideas?" *Language*, XII (1936): 89–95; reprinted in *The Philosophy of Linguistics*, J. J. Katz, ed. (New York: Oxford, 1985), pp. 19–25.

ily is thus a measure, not of circularity, but of the systematizing power of the explanation.

This difference emerges sharply when we try to impose a substitution criteria definition on subjects like logic and mathematics. Imitating Quine's demand that semantic concepts be defined by substitution criteria, we could demand that truth-functional connectives and numbers be defined by them. If this were a legitimate demand, we would be able to construct an argument directly parallel to Quine's showing that sentential logic and arithmetic make no objective sense. For, as with synonymy, there is no noncircular property which is invariant on all and only substitutions of logically equivalent propositions, on the one hand, and substitutions of numerically identical quantities, on the other. Now, such an argument is clearly a *reductio* of the idea that legitimacy of logic and mathematics depends on their identity relation being definable by means of substitution criteria. Thus, it is open to intensionalists to say that Quine's own argument is a *reductio* of the idea that the legitimacy of the theory of meaning depends on its identity relation being definable by means of such criteria. The circularity that Quine exhibited can thus be seen as a product of imposing an inappropriate definitional paradigm on the theory of meaning.

These *a priori* considerations show that there is an alternative definitional paradigm possible for linguistics which, in certain ways, is more promising than substitution criteria. It does not matter, given the logic of my argument, whether such an alternative ever actually finds its way into linguistics. It could be introduced at any time as another, and preferable, way of defining the family of semantic concepts. What could prevent it? Surely not Quine's scruples about "misjudging the parallel." These scruples can have force, as we have seen, only *after* we are given a persuasive argument against linguistically neutral meanings, for only then is there a relevant distinction between intensional semantics and genuine science. At this stage, however, Quine has yet to put such an argument together.

Furthermore, it would do no good to appeal to the arguments Quine used to show that Carnapian meaning postulates and semantical rules fail to explain analyticity and synonymy (LPV, pp. 32–36). Systems of meaning postulates and semantical rules are one way of constructing a theoretical definition for semantic notions, but not the only way. The fact that one way fails to explain them does not show that others will fail. Moreover, Carnapian systems have an idiosyncratic feature which is what makes them subject to Quine's criticism that they are unilluminating about analyticity and synonymy, namely, they do not concern these sense concepts, but the

broader concepts of necessary truth and necessary equivalence. The postulates of such systems are not about the senses of words but about their referents. They do not describe the structure of senses, in the way phrase markers describe the syntactic structure, but, like the logical postulates they are modeled on, they just constrain the extensional interpretations of a language. Ironically, so-called meaning postulates are not about meaning.[19]

There is also a historical reason for thinking substitution criteria do not deserve the place Quine accords them. Quine supposed that the methodology of Bloomfieldian linguistics is an indispensible aspect of the science of language, and, hence, something that he could rely on in philosophy. This supposition was soon challenged. In the early 1960s, the field of linguistics underwent what has come to be called "the Chomskian revolution." One of the principal changes which the revolution brought about was a paradigm shift from substitution-criteria definition in taxonomic grammars to theo-retical definition in generative grammars.[20] Chomsky explicitly mod-eled his conception of a generative grammar on formal systems in logic. The theorems, instead of being a class of logical truths, are the well-formed sentences of a language. An optimal generative gram-mar for a language L generates all and only the well-formed sen-tences of L. It is thus a recursive definition of the notion "sentence of L." The derivations of a sentence provide a description of its grammatical structure.

Chomsky carried the analogy with logic further. He modeled his conception of general linguistics (i.e., the study of linguistic univer-sals) on metalogic. The definitions of language neutral concepts were to be given in terms of theoretical definitions stated in the metathe-ory for generative grammars (i.e., linguistic theory, in Chomsky's terminology). For example, Chomsky defined the concept of a syn-

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[19] It is important to note that Carnap's work represents a significant break with the Fregean tradition preceding it. Whereas that tradition characterized analyticity in terms of intensional notions (e.g., Frege characterized it in terms of laws of logic plus definitions, i.e., statements of sameness of sense), Carnap abandons the use of intensional notions in his meaning postulates approach, characterizing analyticity exclusively in terms of extensional notions. This is why Quine rightly criticizes him for failing to explain what analyticity is. If analyticity can be explained, you have to talk about sense structure to do it. This is also why Quine's criticism does not apply to other intensionalist accounts of analyticity. See my *Cogitations* (New York: Oxford, 1986), pp. 41–97.

[20] One of the most influential examples of theoretical definition in the early stages of generative syntax was Chomsky's rules for the English auxiliary system; see *Current Issues in Linguistic Theory* (The Hague: Mouton, 1964), p. 36.

tactically well-formed sentence of a language as a sentence whose syntactic representation can be generated in an optimal grammar of the language, and the concept of two constituents being of the same syntactic type as identity of their syntactic representations in an optimal grammar.[21]

In the context of the present discussion of Quine, it is hard to exaggerate the importance of the shift to the new paradigm of generative grammar. The new paradigm, together with the theoretical definitions worked out for concepts in phonology and syntax, provided linguists and philosophers with a model for theoretical definitions in semantics (just as substitution criteria in phonology and syntax had provided Quine a model). Of special significance in this connection is the possibility opened up by the metatheory for generative grammars of giving language-independent definitions of language-neutral concepts. This enables us to avoid the use of language-specific definitions such as meaning postulates, and, thus, as will be explained below, to escape Quine's criticism of such definitions. Theoretical definitions in linguistic theory provide a way of defining concepts in the theory of meaning for variable 'S' and 'L' because, in defining a concept at the level of linguistic theory, they define it in terms of features of optimal generative grammars for every natural language.

Research in semantics within the generative framework during the 1960s and early 1970s exploited the model of theoretical definitions in generative phonology, generative syntax, and linguistic theory.[22] The aim of this research was to define concepts such as meaningfulness, ambiguity, synonymy, analyticity, etc., on the same basis as syntactic concepts like well-formedness. Constructing such definitions involved two steps. First, it is necessary to develop a conception of the formal representation of sense structure which parallels the formal representation of constituent structure provided in syntactic markers. But, instead of describing the way sentences are built up from constituents like nouns, verbs, prepositions, etc., as syntactic markers do, semantic markers have to describe the way senses of sentences are built up from the senses of their syntactic constituents.

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[21] *Syntactic Structures* (The Hague: Mouton, 1957), pp. 49–60, and especially pp. 53–55 for discussion of theoretical definitions.
[22] See my *Semantic Theory* (New York: Harper & Row, 1972); *Propositional Structure and Illocutionary Force* (Cambridge: Harvard, 1980); "Common Sense in Semantics," in *New Directions in Semantics*, Ernest LePore, ed. (London: Academic, 1987), pp. 157–233.

Second, it is necessary to find the formal features of semantic representations which, from sentence to sentence and language to language, correlate with particular semantic concepts.

Much of this research was focused on the concept of analyticity. A type of semantic representation was developed which makes it possible formally to describe analytic sense structure, namely, *decompositional semantic representation.*[23] The symbols in such representations stand for component senses of the sense being represented and for their relations to one another. A decompositional representation of "bachelor," for example, would contain symbols for the component senses 'human', 'adult', 'male', and 'single'. Decompositional representation is the only type that can explicate the Kantian concept of analyticity, that is, judgments which add

> . . . nothing through the predicate to the concept of the subject, but merely break . . . it up into those constituent concepts that have all along been thought in it.[24]

This is because it is the only type of semantic representation which marks the presence of the sense of the predicate in the sense of the subject in sentences like 'Bachelors are male' where both terms are syntactically simple.

Relative to an assignment of decompositional representations to sentences, we can define analyticity in terms of the semantic representations of the full predicate and each of its terms but one being formally contained in the semantic representation of the remaining term.[25] Similarly, we can define meaningfulness and synonymy. Semantic well-formedness can be defined in terms of the generability of at least one semantic representation for an expression. Semantic identity can be defined in terms of sameness of semantic representation for two expressions.

The first thing to note about such a definition of analyticity is that it makes no reference to thought processes, and, accordingly, avoids Frege's criticisms of Kant's psychologism. The second thing to note is that the definition is broader than Kant's, which is restricted to

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[23] *Cogitations,* pp. 75–90.
[24] Kant, *The Critique of Pure Reason,* Norman Kemp Smith, trans. (New York: Humanities, 1929), p. 49.
[25] This is but the sketchiest of presentations, but see the references in footnote 22. A full presentation is found in G. E. Smith and J. J. Katz, *Supposable Worlds* (Cambridge: Harvard, forthcoming).

subject-predicate sentences. Our definition covers non-subject-predicate sentences like 'Smith marries those he weds' and 'Jones buys books from those who sell her books'. This feature avoids another of Frege's criticisms of Kant. Finally, note that the definition avoids both of Quine's criticisms of Carnap's explication. As already indicated, the definition, being in the metatheory of generative grammars, meets Quine's demand that the concept of analyticity be defined for variable 'S' and 'L'.[26] It also meets Quine's demand that a definition tell us what property is attributed to a sentence when marked "analytic." The definition says the property is that of having a redundant predication—the referential upshot of which is that the truth conditions of the sentence are automatically satisfied once its terms take on reference.

Although more has been accomplished than the above sketch indicates, nothing even approaching a full theory of decompositional semantics has been developed. The fact that such a theory is still far from complete is, however, not something that Quine can use to block the criticism I am making of the argument in "Two Dogmas of Empiricism." In particular, it is not open to him to argue that acceptance of the criticism must be withheld until the theory of decompositional semantics is developed far enough to see whether it works. Since Quine's argument claims to rule out every possibility of showing that intensional concepts can be made sense of, showing that his argument overlooks a possibility is showing that it fails.[27]

I now wish to trace out the consequences of Quine's failure to support his claim that there are no linguistically neutral meanings. The immediate consequence is that we are free to entertain the existence of meanings in the same spirit with which scientists beginning the study of a new field presume the existence of facts and laws

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[26] Quine has somehow not seen the point that a theoretical definition of analyticity framed in the metatheory of generative grammars defines the notion for variable 'S' and 'L', which I first made in my "Some Remarks on Quine on Analyticity," this JOURNAL, LXIV, 1 (Jan. 19, 1967): 36–52. See Quine, "On a Suggestion of Katz," ibid., pp. 52–54; and also "Methodological Reflections on Current Linguistic Theory," in Semantics of Natural Language, Donald Davidson and Gilbert Harman, eds. (Dordrecht: Reidel, 1972), pp. 449–450. In these papers, he confuses a theoretical definition of analyticity with a technique for questioning informants to elicit judgments about analyticity for the sentences of a particular natural language.

[27] Moreover, theoretical definitions of semantic structure occupy a natural place in the theory of generative grammar, extending that theory by adding a theory of semantic structure to the theories of phonological and syntactic structure. Further, as we indicated, the use of theoretical definition is what makes Quine's criticism of vicious circularity inapplicable to fields like logic and mathematics.

to be discovered. Without a reason to think that the theory of mean-
ing suffers from anything worse than underdetermination, we may
pursue the parallel with well-developed sciences. We may take the
view that such a course of action is itself the best way to determine
whether there are meanings. As with other sciences, trying to con-
struct a theory will, in the long run, show whether the initial pre-
sumption is correct.

   As a consequence of thus being able to pursue the parallel,
Quine's argument that actual translation is indeterminate breaks
down. Recall that this argument depends critically on whether actual
translation, like radical translation, lacks "independent controls." As
Quine claims: "when we reflect on the limits of possible data for
radical translation, the indeterminacy is not to be doubted."[28] We
conceded that the constraints on the choice of analytical hypotheses
in radical translation are too weak to make translation determinate,
but questioned why the constraints in actual translation are sup-
posed to be no stronger. Pressing this question, we found nothing to
stop us from making the working assumption that there are linguis-
tically neutral meanings. But, if we can assume a fact of the matter
with respect to which analytical hypotheses can be judged, we can say
what constraints there are in actual translation over and above those
in radical translation. This, in turn, will explain how we find out
whether the assumption is true.

   The constraints on choices between analytical hypotheses are di-
rectly parallel to those we use to make choices among corresponding
hypotheses in other sciences. The criteria for correctness in transla-
tion will be the customary blend of data and methodological consid-
erations (i.e., the data must be explained as economically as possible,
with as much scope and depth as possible, etc.). The data come from
overtly expressed judgments of speakers reflecting their knowledge
of the language. In radical translation, the data are restricted to
judgments about the reference of expressions. But, on our working
assumption, the data in actual translation include judgments about
the senses of expressions, too. The linguist can ask whether an ex-
pression has a sense (i.e., whether it is meaningful or not), whether
an expression has more than one sense (i.e., whether it is ambigu-
ous), whether the sense of one expression is the same as that of
another (i.e., whether they are synonymous), whether the sense of
one expression is the opposite of the sense of another (i.e., whether

[28] "Indeterminacy of Translation Again," p. 9.

they are antonyms), whether the sense of a sentence involves redundant predication (i.e., whether it is analytic), and so on. The possibility of putting such questions to informants automatically provides the possibility of evidential controls. Such controls take the form of the requirement to prefer those hypotheses which provide the best account for the data about sense structure, where the choice among accounts which handle the same range of data is made on the basis of standard methodological considerations.[29]

To give some concrete examples of semantic evidence, I shall tell a jungle story of my own. Imagine an actual translation situation in which the linguist and informant come from disparate cultures and in which the home and target languages have disparate histories. The linguist is faced with the choice of translating 'gavagai' as "rabbit," "rabbit stage," or "undetached rabbit part." In my story, unlike in Quine's, the linguist simply queries the informant about the senses of expressions. (We shall consider the question of how they communicate below.) The linguist can ask whether "gavagai" is synonymous with one of these English words. The informant may judge it to be synonymous with one of them, judge it not to be synonymous with any, or provide no useful judgment. In the first two cases, the linguist has acquired some data. In the third, the direct approach did not work. But here, as elsewhere in science, there is a wide range of less direct approaches. For example, the linguist may ask whether "gavagai" bears the same relation to a native expression that "finger" bears to "hand" or "handle" bears to "knife." Or, the linguist could ask whether "gavagai" is closer in sense to "infancy" and "adolescence" than it is to "infant" and "adolescent." Or, the linguist could ask whether the expression obtained when "gavagai" is modified by the native word meaning "undetached" is redundant like "unmarried bachelor." Or, whether the expression obtained when "gavagai" is modified by the native word meaning "detached" is contradictory like "married bachelor." There are indefinitely many further questions to ask, and, of course, indefinitely many further informants to ask them of. So why should a body of data obtained in this way, in principle, not settle any question of translation as satisfactorily as a comparable body of data in other sciences would settle comparable theoretical questions?

Two Quinean thoughts arise here. First: there is the problem of

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[29] Note that our approach can adopt a holism which insists that translation schemes can only be tested (or even constructed) as components of a comprehensive grammatical theory of natural languages.

how linguists come by the hypotheses which they are supposed to verify by asking such questions about the foreign language. Second: there is the problem how linguists and informants are able to communicate.

It is hard to see why the first is much of a problem. The hypotheses can be based on guesses based on nothing, hunches educated by experience, prejudices stemming from a cherished theory, or what have you. This is surely how it is in other sciences. Yet Quine views presumptions about the semantics of an alien language deriving from the semantics of one's own language as begging the question.[30] He is surely right in the case of radical translation. If a linguist in such a situation takes "gavagai" to be an object term, like the English "rabbit," then, in the absence of "independent controls," there is no way to know whether the linguist has not merely read the grammatical categories of his or her own language into the alien language. But, for the case of actual translation, where, as we have shown, there can be such controls, presumptions about the semantics of the alien language, even those reflecting linguistic chauvinism, beg no question. Evidential controls enable linguists to revise mistaken presumptions and validate correct ones. Falsifications of the category structure of an alien language, accordingly, are on a par with the animistic or anthropomorphic theories of nature in early science. Presumptions on the part of the linguist may retard (or, for that matter, advance) the progress of translation, but, as long as evidence can be brought to bear on such presumptions, they have no philosophical significance.

The problem of communication between linguist and informant may at first seem more serious. Quine always represents radical translation situations as ones in which the linguist confronts the informant across an impassable language barrier. Indeed, Quine's assumption that they do not know each other's language has made his readers wonder how an informant knows what the linguist's interests are and how a linguist knows what behavior of the informant counts as assent. It is Quine's assumption of monolingualism—rather than his behaviorism or anything else—that is the truly unrealistic element in his thinking about translation. Actual translation can no more proceed without a bilingual than grammar construction can proceed without a native speaker.

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[30] In WO, p. 72, Quine writes: "To project [analytical hypotheses] beyond the independently translatable sentences at all is in effect to impute our sense of linguistic analogy unverifiably to the native mind."

There is, then, a sense in which the whole question of bilingualism is beside the point. Quine accepts the fact that indeterminacy arises equally well within a single language, between idiolects of different speakers, and, further, that it arises even within a single idiolect, between different temporal stages. Here there is no question of bilinguals, unless, of course, bilingualism is understood as fluency in two idiolects or two idiolect stages. Is Quine to be construed as claiming that English speaking linguists and their English speaking informants do not share a language in which they can communicate?

I am, of course, not saying that bilinguals must exist, but only that their existence is an empirical condition for translation. The existence of bilinguals is comparable to the existence of conditions that enable us to conduct the experiments necessary to decide among rival physical theories. Moreover, even if there were no bilinguals in the case of some alien language, we know a sure-fire method for creating them on demand. The method takes rather a long time, and its implementation involves various practical, social, and moral problems, but it works, as those who have acquired bilingual fluency growing up in a bilingual home can attest.

To be sure, Quine thinks introducing bilinguals begs the question. The feeling that a question is begged is strongest with respect to the possibility of full fluency in both languages, including, as it would, fluency in the metalinguistic vocabulary of the language, e.g., the expressions for relations like 'translates' or 'is synonymous with'. The idea of an informant who speaks the whole language like a native seems to go too far. But why? We are not supposing the actual existence of such informants, but only the possibility of their existence. Accepting the possibility of fluent informants (even their actuality) does not settle the question of the existence of meanings, anymore than accepting the possibility of reliable meters in a physics experiment (even their actuality) settles the question the experiment is to decide.

Admittedly, entertaining the prospect of bilingualism is, in itself, entertaining the prospect of translation, since knowledge of translation is what makes a bilingual bilingual. But, if there is no harm in entertaining the prospect of translation, there is none in entertaining the prospect of bilingualism.

Thus, no question is begged. But, further, no risk is run. Although bilinguals, like field linguists with presumptions, can cause falsifications in translation, such falsifications, like false grammatical categories read into a target language, can be corrected. As long as we have evidential controls, the difficulties posed by deficient or devious

informants are, in principle, no different from those posed by a
faulty meter in a physics experiment.

Let us sum up. Radical translation is indeterminate because it is
restricted to using referential features of words. But there is nothing
to show that actual translation is similarly restricted. With the use of
evidence about the senses of words in constructing theories about
actual translation, the evidential symmetry in radical translation does
not arise in actual translation. Intensionalist semanticists may cherish
the same hopes for success as other scientists. They are not even
required to establish that facts about meaningfulness, ambiguity,
synonymy, antonymy, redundancy, etc., are, in principle, sufficient
to enable linguists to make a unique selection among a number of
translation schemes. Either all but one of the translation schemes can
be eliminated, given total evidence and methodological consider-
ations, or else, since synonymy is the identity relation for meanings,
the uneliminated schemes count equivalently as ways of expressing
the truth about the semantic relation between the languages.

Of course, none of this is to say that no form of skepticism about
determinate translation remains. There is no metaphysical insurance
policy against nature being counterinductive or linguistic investiga-
tion being counterproductive: linguists can make systematically mis-
leading projections, and informants can produce systematically mis-
leading evidence. But such possibilities lead to nothing more than an
absolute skepticism which would obliterate knowledge in all fields.
Thus, once Quine's semantics relative skepticism goes, nothing pre-
vents the common-sense view of translation from reasserting itself.

Life without meaning is not the trouble-free paradise it is some-
times made out to be. Quineans have always had trouble explaining
in what respect allegedly competing analytical hypotheses compete.
It is easy, from a common-sense standpoint, to appreciate the respect
in which "rabbit," "rabbit stage," and "undetached rabbit part"
represent rival translations, but what, from the standpoint of inde-
terminacy, is supposed to be the semantic difference? Quine's inde-
terminacy thesis is predicated on the existence of a conceptual dis-
tinction of some sort among such translations, yet the nature of the
choice within a radical-translation situation seems to preclude the
possibility of any difference upon which the distinction might rest.

From the perspective of the present paper, this problem is an
illusion created by superimposing radical translation onto actual
translation. The conceptual distinctions, on which framing rival hy-
potheses in a case of radical translation depends, derive from the
intuitively recognized differences in meaning between "rabbit,"

"rabbit stage," and "undetached rabbit part." The sense of a dis-
tinction without a difference comes from the fact that these differ-
ences cannot exist in radical translation, so they recede into the
background when radical translation is identified with the extreme
case of actual translation. Yet the intuitive recognition of them stays
with us, and re-emerges in connection with the indeterminacy thesis
to give content to talk of "divergent translations" and "incompatible
translations" (WO, p. 27). As a consequence, we get the curious
duck/rabbit shift. The illusion disappears if we do not identify radi-
cal translation with a case of actual translation.

Another problem which makes life with indeterminacy less than
idyllic is that translational indeterminacy is a slippery slope. Quine's
argument from the absence of "independent controls" in the case of
translation between languages, if sound, would also show that trans-
lation between dialects of a language must be indeterminate. Fur-
ther, the same argument would show that translations between idio-
lects of a dialect must be indeterminate. Nor does the slide stop here.
We also have to accept indeterminacy in the case of stages of the
same idiolect. Hence, in accepting the initial indeterminacy argu-
ment, we are buying a linguistic solipsism of the moment: one's own
words of other moments stand to one's words of the present in
exactly the relation that "gavagai" is supposed to stand to "rabbit,"
"rabbit stage," and "undetached rabbit part." Quine himself might
be willing to live with indeterminacy so close to home, but few others
have been happy with the prospect.[31] The solution is simply not to
take the fatal first step onto the slope, the step of conceding inde-
terminacy in translation between languages. The present paper ex-
plains why there is no need to take it.[32]

[31] See "Ontological Relativity," in Ontological Relativity and Other Essays
(New York: Columbia 1969), p. 46, where he remarks: "On deeper reflection,
radical translation begins at home." The full force of such deeper reflection is
brought out by the "theorem" that Hilary Putnam proves from indeterminacy in his
Reason, Truth, and History (New York: Cambridge, 1981), pp. 22–48. A side
benefit of my refutation of indeterminacy is that one no longer needs to accept
Putnam's proof.

[32] I have said nothing in the text about what Quine calls "pressing from below."
As Quine puts it: "By pressing from below I mean pressing whatever arguments for
indeterminacy of translation can be based on inscrutability of terms. I suppose that
Harman's example regarding natural numbers comes under this head, theoretical
though it is. It is that the sentence '3 ϵ 5' goes into a true sentence of set theory
under von Neumann's way of construing natural numbers, but goes into a false one
under Zermelo's way" ("On the Reasons for Indeterminacy of Translation," p.
183).

But Quine is wrong in thinking that the nature of the choice between the two ways
of construing natural numbers puts pressure on us to recognize indeterminacy. The

Quine is widely regarded as having terminated the Fregean program of taking senses to mediate between word and object. Those of us who still keep the intensionalist faith are frequently made to feel only slightly less benighted than a philosopher who might still be pursuing Hilbert's program. But, if my criticism in this paper is correct, Quine's indeterminacy thesis does not put an end to the intensionalist tradition. Quine cannot be included among those who, like Kurt Gödel, terminated an entire philosophical program. Rather, Quine belongs among the philosophical skeptics who, like Hume, forced subsequent philosophizing to become far clearer about fundamental concepts. Just as Hume's *Treatise* did not eliminate the concept of causality, so Quine's *Word and Object* does not eliminate the concept of meaning. Yet, just as discussions of causality have not been the same since Hume, so discussions of meaning have not been the same since Quine.

<div align="right">JERROLD J. KATZ</div>

Graduate Center,
City University of New York

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consideration he raises is relevant if, and only if, it has already been shown that the analytic-synthetic distinction cannot be drawn. If the distinction can be drawn, pressing from below is ineffective. For it can hardly matter to questions of translation, which are on the analytic side, that a mathematical sentence comes out true within one set of synthetic statements and false within another. On the other hand, if the distinction cannot be drawn, pressing from below is unnecessary. For then, as we have already seen, the gap in Quine's argument in *Word and Object* is filled, and the argument goes through without a hitch. Hence, my criticism of Quine's argument in "Two Dogmas of Empiricism" is as well a criticism of his claim that examples like Harman's are a consideration in favor of indeterminacy.