

DORIT BAR-ON

SEMANTIC VERIFICATIONISM,  
LINGUISTIC BEHAVIORISM, AND TRANSLATION

(Received 1 August, 1991)

In a recent article, "Indeterminacy of Translation Again" (Quine 1987, henceforth "TTA"), Quine makes yet another attempt to clarify and defend his well-known thesis of the indeterminacy of translation. This renewed attempt is timely, in view of what must seem — from a Quinean perspective — like meaning-mania. In the three decades since Quine first formulated the thesis (1960: Ch. 2), there has been a surge of optimism in various intellectual camps regarding the prospects of a 'scientific theory of meaning'. If Quine's indeterminacy thesis is correct, however, this optimism is thoroughly misplaced, since the thesis aims to establish that there is no real subject matter for a scientific theory of meaning.

"TTA" brings into sharper focus two crucial elements playing a role in Quine's reasoning about indeterminacy. The first is what I shall call *semantic verificationism*. It is the idea that the semantic facts of any particular language must be in principle accessible to any well-placed linguistic agent; semantic reality could not forever lie beyond the epistemic reach of speakers. This first idea is not explicit in Quine's writings. Articulating it would help clarify aspects in his reasoning which have long puzzled his readers. Closely related to the first idea, but more familiar to Quine's readers is *linguistic behaviorism*: the idea that semantic facts must be construed in behavioral terms; semantic reality is behavioral reality.

The force of these two ideas, I believe, has been underestimated in the ever-growing literature on indeterminacy. In sections 1 and 2, I articulate them and explain the role they play in Quine's rejection of a popular strategy against the indeterminacy thesis — the Chomskian mentalist strategy. In section 3, I outline some positive proposals regarding meaning made by Quine in a little discussed paper, "Use and Its Place in Meaning".<sup>1</sup> In the final section, I argue that, Quine's denial

notwithstanding, these proposals can help rebut the indeterminacy thesis. And they do so in a way that is consistent both with semantic verificationism and with linguistic behaviourism.

(1) INDETERMINACY, UNDERDETERMINATION AND  
SEMANTIC VERIFICATIONISM

In Chapter 2 of *Word & Object* Quine introduces the radical translator, someone who tries to translate a completely alien language into her own language. On the basis of observation of the circumstances under which a native speaker of that language volunteers, say, the sequence of sound 'Gavagai!', she can form the inductive hypothesis that 'Gavagai!', as a one-word sentence, matches our 'Rabbit!' (and similarly for other native utterances). But to get a complete translation manual between Gavagese and English, the translator must correlate Gavagese sentence parts, such as the term 'gavagai', with English expressions ("rabbit"? "undetached rabbit part"? "rabbit stage?") and correlate Gavagese grammatical operations with English ones. Quine calls such correlations "analytical hypotheses".

Quine has argued that all objective evidence available to a radical translator is compatible with incompatible systems of analytical hypotheses, ones yielding conflicting translations of many native sentences into English.<sup>2</sup> This represents the claim of *translational underdetermination*: translation manuals are underdetermined by their evidence. Further, since all the evidence does not support a unique choice of a translation manual, Quine concludes that there is no fact of the matter as to the correct translations of (most)<sup>3</sup> sentences of any language — our own included — into any other language. This is the *translational indeterminacy* claim. Since a translation of a sentence provides a way of specifying its meaning, the indeterminacy thesis says, in effect, that there is no objectively correct choice — known or unknown — among incompatible ways of specifying the meanings of our sentences. So much, then, for any objective theory of meaning. Such a theory would lack a subject matter.

Opponents of indeterminacy must argue either (a) that translational underdetermination does not imply indeterminacy of meaning, or (b) that there is no translational underdetermination in the first place.

Following Chomsky (1975), many critics of Quine have deployed strategy (a), pointing out that Quine himself does not license the implication from underdetermination to indeterminacy in the case of other special sciences, such as chemistry or biology (which also suffer what Quine has referred to as 'second-order underdetermination' — underdetermination relative to physics; see his 1970a). In "ITA", Quine explains his persistence in this differential treatment as follows:

The indeterminacy of translation differs from the underdetermination of science in that there is only the natives' verbal behavior for the manuals of translation to be right or wrong about; . . . In the case of natural science, . . . there is a fact of the matter, even if all possible observations are insufficient to reveal it uniquely. The facts of nature outrun our theories as well as all possible observations. . . (p. 9f.)

Quine is here recapitulating his view that there is something which sets apart linguistics from other special sciences such as chemistry or biology. Elsewhere,<sup>4</sup> I have argued that Quine is right about this, and that this diminishes the effectiveness of strategy (a) against the indeterminacy thesis. The domain of linguistics is special in that, in the case of language, we can in principle talk of underdetermination at two different levels. As in other sciences, we can talk of underdetermination at the *theoretical level*: an empirical theory of a language may be underdetermined by all its evidence. Quine urges us to recognize, in particular, the theoretical underdetermination of semantics, which consists in the fact that there could be incompatible theories about the correct specification of meanings in any language under study (all compatible with the relevant data). It may well be argued, in consonance with strategy (a), that, if theoretical underdetermination is accepted in the case of other sciences without a threat to their objectivity, it should be so accepted in linguistics as well. But Quine's thought experiment of radical translation is designed to convince us, further, of what I call "*ground level* underdetermination", which supposedly infects the process of *acquiring* a language. Let me explain.

Quine often speaks of language as "the complex of present dispositions to verbal behavior, in which speakers of the same language have perforce come to resemble one another" (1960: 27). The process of language acquisition would then be the process of acquiring the relevant dispositions. There is no place for talk of underdetermination in connection with that process. However, in discussing language

acquisition, Quine very often invites us to think of the process as paralleling the process of constructing a translation-manual. Both child and radical linguist, Quine tells us, have no data to go on in probing a language "but the concomitances of . . . utterance and observable stimulus situation" (1969: 81). But this method of associating meanings with expressions — which for Quine qualifies as learning by conditioning, or induction — "is notoriously incapable of carrying us very far in language" (1976: 57). Just as the radical translator must resort to analytical hypotheses to carry her beyond the pairing of native sentences treated as one-word sentences with stimulus synonymous sentences of her own language, so the child must make her own unconditioned contribution to get over the "great hump that lies beyond ostension, or induction". Thus, says Quine, the doctrine that "conditioning is insufficient to explain language learning . . . is of a piece with my doctrine of the indeterminacy of translation" (1976: 58).<sup>5</sup>

The analogy is pursued by Quine to its limit.

[T]wo independent Martians could acquire perfect and indistinguishable English through unlike and even incompatible systems of English-to-Martians analytical hypotheses. The corresponding point about English children is that two of them may attain to an identical command of English through very dissimilar processes of tentative association and adjustment of the various interdependent adjectives and particles . . . (1960: 94)

This is to give substance to the puzzling claim which has launched Quine's discussion of radical translation, namely, the "two men could be just alike in all their dispositions to verbal behavior under all possible sensory stimulations, and yet the meanings or ideas expressed in their identically triggered and identically sounded utterances could diverge radically, for the two men, in a wide range of cases" (1960: 26).<sup>6</sup> Thinking in terms of language acquisition, the claim is that two speakers who fully converge in their verbal dispositions may nonetheless diverge radically in their non-inductively acquired (or unconditioned) *semantic judgments*. These are judgments which concern e.g., what expressions in one's acquired language are 'about', how a given expression 'divides its reference' (whether it denotes rabbits or rabbit-stages or . . .), whether or not expressions are synonymous (as opposed to merely stimulus-synonymous), as well as judgments concerning the finer (semantically relevant) structure of sentences. What I refer to as

ground-level semantic underdetermination is underdetermination in these sorts of unreflective, intuitive semantic judgments which speakers come to make in the course of acquiring their language.

It is very tempting to think of an empirical semantic theory as attempting to systematize and explain the ground-level semantic judgments made by the speakers of a given language.<sup>7</sup> What distinguishes an empirical semantics so conceived from special sciences like chemistry or biology is that it ultimately concerns ground-level *judgments* of speakers. Whereas a chemical theory concerns, if you will, ground-level *phenomena*. Ordinary folk may, of course, have all sorts of naive beliefs about the relevant phenomena; but the theory in no way concerns those beliefs. So there would be no relevant analogue to ground-level semantic underdetermination in sciences like chemistry or biology.<sup>8</sup> But then, if — as I believe to be the case — it is ground-level semantic underdetermination which threatens to bring *indeterminacy* of meaning in its train, it would be insufficient to point to the supposedly harmless theoretical underdetermination in other sciences (as per strategy (a) above) in arguing against the indeterminacy thesis.

Why would ground-level semantic underdetermination imply indeterminacy of meaning? Briefly, it seems that the ways in which speakers go about acquiring and understanding their language, their linguistic practices and judgments, give us all there is to be right or wrong about in linguistic matters. If, after normal linguistic training, the speakers themselves end up unable to determine (in the sense that their judgments may irreconcilably diverge) e.g., whether or not two expressions were synonymous, or what a given expression was about, then it seems that there could be even in principle no further facts to decide the matter. The view I refer to as *semantic verificationism* maintains that at least one central aspect of semantic reality — the facts about what expressions mean and about sameness and difference of meaning between expressions in a given language — cannot outrun all the evidence that is in principle available to speakers of the language. The view requires of any acceptable theory of meaning that it somehow guarantee that speakers have (in principle) epistemic access to the facts of their language.<sup>9</sup> To this extent, the view takes the epistemic and the metaphysical to merge in matters semantic. The semantic verificationist sees this kind of merging as forced on us in the linguistic case, thought

not in the case of chemistry, biology, etc. For her, to the extent that there are truths of semantics, they must be in principle accessible to speakers; whereas truths of chemistry, biology, etc. need not.<sup>10</sup>

Given semantic verificationism, one could argue — in partial agreement with Quine — that *if*, say, the ground-level judgments of the speakers of a language do not determine a uniform understanding of some term — if alternative conflicting interpretations of it are systematically possible — then there is no fact of the matter about the meaning of the term. Thus, identifying two possible levels of underdetermination in the linguistic case, and spelling out the semantic verificationist view allows us to make sense of the step Quine takes from translational underdetermination to indeterminacy of meaning.<sup>11</sup> Although it does not justify a differential treatment of linguistics and other sciences at the theoretical level, the verificationist view sanctions moving from ground-level semantic underdetermination to semantic indeterminacy.

Opponents of indeterminacy, then, must either attack semantic verificationism or deny the ground-level underdetermination. As far as I know, the claims of semantic verificationism have not been directly confronted by Quine's opponents. On the other hand, ground-level semantic underdetermination would seem inevitable at least on any view which takes seriously the 'theory'-theory of language acquisition. On such views, a child learning a first language is in crucial respects an amateur theorist of the language spoken in his community. He unwittingly gathers evidence, tests various linguistic hypotheses, and finally settles on a theory expressible in a more or less stable set of linguistic judgments.<sup>12</sup> If acquiring a first language is a process of theory construction (albeit a tacit one), then the theory it issues in — like any theory — would be underdetermined. The linguist's theory would then be an underdetermined theory about the child's theory, which is itself underdetermined.

## (2) LINGUISTIC BEHAVIORISM

Chomskian *mentalism* can be seen as an attempt to maintain the child-linguist analogy, while resisting the apparently obvious result of ground-level underdetermination. Chomskian mentalists agree that the child's choice among competing linguistic hypotheses is seriously underdeter-

mined by the behavioral data available to him; this poses the 'poverty of the stimuli' problem. But they think the problem can be solved by recognizing that, in reality, the child's choice is determined innately, by the structure of his mind. The mind closes doors which behavioral reality leaves open.

Quine has specifically rejected mentalism in the study of language: "In psychology one may or may not be a behaviorist, but in linguistics one has no choice" ("ITA": 5). What I take to be Quine's argument for *linguistic behaviorism* (see *ibid.* and 1970b: 4f.), goes as follows:

- (1) "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" (*ibid.*).

So,

- (2) In learning a language, "we depend strictly on overt behavior in observable situations" (*ibid.*).

So,

- (3) Differences that cannot be "gleaned from overt behavior in observable circumstances" can play no role in the learning of a language (given its "strict dependence" on observable behavior, see (2)).
- (4) What can play no role in the leaning of a language can have no semantic relevance and thus cannot be part of any objective notion of meaning.

So,

- (5) Differences that cannot be recovered from observable behavior can be of no semantic relevance; so they cannot be part of any objective notion of meaning.

In the context of this argument, we can read Quine's indeterminacy thesis as saying, first, that

- (6) Differences that are central to our intuitive semantic judgments (e.g., as between an expression meaning *rabbit* and its

meaning *undetached rabbit part*) cannot be recovered from observable behavior. (This corresponds to the underdetermination claim.)

and hence (conjoining (6) to (5)) that

- (7) Differences that are central to our intuitive semantic judgments can be of no semantic relevance; so they cannot be part of any objective notion of meaning. (This corresponds to the indeterminacy claim.)

Premise (1) of the above argument purports to state a fact about language learning. But it is not obvious how to understand it. If (1) is simply taken to assert that language learning requires nothing over and above a behavioristic stimulus-response set-up, then it begs the question against Chomskian mentalism. And the simple-minded behavioristic model of language acquisition alluded to in (this reading of) (1) may seem very dubious, even if one is not convinced of the cogency of the Chomskian notion of innate linguistic knowledge. Accumulating evidence by psycholinguists (of a variety of theoretical persuasions) suggests that reinforcement plays a relatively minor role in actual language acquisition and, perhaps more importantly, that active corrections by the putative "teachers" of language have very little effect on the normal course of language acquisition.<sup>13</sup> So while it is obviously true that in learning our language we observed other people's behavior, and our own attempts at verbal behavior do in fact get reinforced or corrected, it is far from obvious that nothing goes into the learning of a language except what can be recovered from observable behavior and modified through reinforcement/correction.

If, on the other hand, we read (1) so it does not simply assume the truth of linguistic behaviorism, we have no good reason to accept that we "depend strictly on overt behavior" in learning a language (claim (2)). Here too we can accept a weak reading, taking "strict dependence on overt behavior" to imply only the *necessity* of linguistic interaction with other speakers. But this weak reading of (2) will not help us establish that what is not recoverable from linguistic input can play no role in the learning of a language. Chomsky may well accept that exposure to others' speech is at least psychologically necessary to the

acquisition of language. What is primarily at issue, however, is whether observation of behavior is *sufficient* for language acquisition. And the Chomskian mentalist would insist that it must be supplemented by our own innately determined contribution.

The positive argument offered by Quine for linguistic behaviorism is thus highly problematic. Quine, however, may have negative reasons for being dissatisfied with the mentalist approach to empirical semantics and for preferring linguistic behaviorism. On a plausible construal (cf. Putnam, 1988: 4ff.), Chomskian mentalism applied to the study of meaning<sup>14</sup> would hold that there are semantic representations which are innate, universal mental entities and into which all our concepts can be decomposed. Language learning would be explained in terms of mental processes involving these representations. These processes may be triggered by observation of behavior, but they go over and above the acquisition of behavioral dispositions on the part of the learner. What meaning an expression has, and whether two expressions have the same or different meaning, depends on what semantic representations are associated with relevant expressions in the mind/brain of language users.

Like the behaviorist view, the mentalist view just sketched recognizes the need to forge an essential connection between the semantic (and other) facts of a language and certain features of its speakers. But whereas the behaviorist seeks to construe "the very facts about meaning in terms of behavior", mentalism "regard[s] a man's semantics as somehow determinate in his mind beyond what might be implicit in his dispositions to overt behavior" (Quine 1969: 27). For the mentalist, the real semantic facts are construed in terms of certain internal occurrences in the minds/brains of the users of linguistic expressions.<sup>15</sup> And this Quine finds objectionable:

... A language is mastered through *social* emulation and social feedback, and these controls ignore any *idiosyncrasy* in an individual's imagery or associations that is not discovered in his behavior. *Minds are indifferent to language* insofar as they are behaviorally inscrutable. (1970b: 4f., my emphases)

... My aversion [to the mental], within its limits, has a reason: the want of *intersubjective* checkpoints. It is Wittgenstein's *rejection of private language*. It is this, and not mentality as such, that disqualifies any irreducibly intuitive notion of meaning or synonymy or semantic relevance... (1986: 74, my emphases)

Quine here attributes his primary objection to linguistic mentalism to a recognition of the social nature of language and to a Wittgensteinian commitment to its *publicity*. The charge against mentalism is that the metaphysical view of semantic facts it offers fails to take proper account of the public nature of language. The mentalist's meanings — semantic representations speakers associate in their minds with expressions — are entities *hidden* from plain view. To that extent, the mentalist's meanings are in as bad standing as the Wittgensteinian private meanings (see Wittgenstein 1958: #258ff.).

A Chomskian mentalist might protest that mentalist meanings are not intended to be like Wittgenstein's logically private meanings knowable only to an original user. They are not Cartesian entities, but rather postulated internal features of speakers which are meant to be, ultimately, in principle observable features of the brain. Quine, however, seems to think that goings-on in speakers' "mental life between checkpoints" — observable or not — can bear no *semantic* relevance: they are epiphenomenal from the point of view of the intersubjective processes of language learning and use. (As he often points out, in our ordinary, ground-level ascriptions of meaning to others' utterances "no claims are laid regarding hidden ... mechanisms" ("ITA": 9).) Like idiosyncratic images or associations accompanying the use of words, the mentalist's hidden mechanisms, Quine maintains, are private not in the sense of being in principle unobservable, but rather in the sense of playing no active and systematic role in the normal functioning of a language. Thus they are as semantically idle or inert as logically private meanings would be.

The Chomskian mentalist might respond that whether or not there is a role to be played by inner mental mechanisms in the learning of language is a matter to be determined empirically, not by a priori reasoning. The Quinean appeal to the publicity and social nature of language might then seem in danger of begging the question against mentalism no less than the argument for linguistic behaviorism presented earlier.

I think that the appeal to publicity may be better understood in light of our earlier discussion of semantic verificationism (see previous section). Perhaps we can think of Quine's rejection of hidden semantic facts as an expression of the semantic verificationist requirement that

what we take to be the semantic reality lie within the epistemic reach of speakers. This requirement is motivated by the idea that, to put it somewhat obliquely, there can be no more to the facts of a language than what its speakers can make of those facts. From the semantic verificationist perspective, behaviorism may seem to have a certain advantage. If semantic reality is behavioral reality, and semantic facts are construed purely in terms of behavior, the epistemic access required by semantic verificationism seems guaranteed. The behavior of other speakers is epistemically accessible to us if anything is; it is what we observe, in the first instance, in learning a language — it is our data.

The negative reason for a Quinean dissatisfaction with Chomskian mentalism would become the charge that mentalism cannot satisfy the epistemic access requirement. Insofar as the mentalist takes semantic reality to be mental reality, and semantic facts to consist in facts about speakers' minds, the indisputably accessible behavioral facts must be taken by mentalists to be mere evidence for the real facts of semantics. And our everyday semantic judgments must be taken to have a mere conjectural status: they are hypotheses about matters deep in speakers' minds/brains. It is by reference to these hidden matters that the mentalist would judge the correctness, or truth, of our ground-level judgments. But this would seem to break the required link between the epistemic and the metaphysical in semantics.

By grounding linguistic facts in features of speakers' minds mentalism can appear to preserve the desired link. The mentalist might point out that the nature and structure of speakers' minds guarantee — as a matter of fact — that they would make systematic and by and large uniform contributions to the behavioral data they encounter. This in turn guarantees their ability to extrapolate reliably to the mental facts which underpin the facts of their language. In that sense, linguistic facts on the mentalist's picture do *not* 'go beyond what speakers can make of them'.

Still, to the extent that the mentalist is willing to accept the possibility that the real facts about meaning — just like facts about the internal structure of chemical substances, or about hidden details of our anatomy — may be ultimately discoverable (if at all) only through scientific investigation, she must accept, it seems, the in-principle possibility that the facts of a language might lie beyond the epistemic

reach of its speakers. To the extent that this involves a commitment to the logical independence of semantic facts from the judgments of speakers, the mentalist view would be flouting the epistemic access requirement.

The Chomskian mentalist may deny this commitment. She may point out that the job of an empirical semantic theory, on the Chomskian view is to systematize and explain the semantic judgments of speakers; these judgments are part of the data to which it is answerable. Since the theory is thus constrained by speakers' judgments, it is wrong to think of the truths it delivers as logically independent of those judgments. I believe this response must be taken seriously, but much more needs to be said before we can determine whether or not it can rescue Chomskian mentalism from the charge at hand. This is a complicated matter, discussion of which is beyond the scope of this paper.<sup>16</sup>

Insofar as Chomskian mentalism represents the only viable alternative to linguistic behaviorism, Quine's belief that it leads to an unacceptable view of language gives him negative grounds for holding that, in the study of language, "the behaviorist approach is mandatory" ("ITA": 5). This approach dictates that speakers' semantic judgments have objectivity only to the extent that they represent legitimate abstractions from behavioral data. Since Quine himself does not think we can make good behavioral sense of, e.g., an alien expression's meaning *rabbit* rather than *undetached rabbit parts*, he despairs of providing an objective account of our intuitive notion of meaning.

If one accepts Quine's reasons for linguistic behaviorism, the only way to escape his indeterminacy thesis is to argue that semantic features which are central to our intuitive notion of meaning *are* behaviorally discernible (*contra* step (6) of the argument on p. 101), thereby blocking the move to the claim that they lack semantic relevance ((7), *ibid.*) One way to do that is to insist on a sufficiently liberal reading of "behavior", so that it will turn out that the semantically relevant facts *can* be "gleaned" from behavior.<sup>17</sup> The burden on a 'liberal behaviorist' would then be to show how this is so. I will not assume this burden here. Rather, I want to turn to Quine's recent discussion of the notion of cognitive meaning. *Pace* Quine, I believe the discussion contains resources for undermining his indeterminacy thesis while still adhering

to his own behavioristic guidelines, and in a way which is virtually dictated by semantic verificationism.

### (3) QUINE'S "USE AND ITS PLACE IN MEANING"

A reader of Quine's writings on indeterminacy is likely to have the following reaction. Surely, if there is such a thing at all as knowing and understanding English (and if we are to count as English speakers), there must be a semantic difference discernible by us between uses of the expressions "rabbits" and "undetached rabbit parts", no less than there is between such expressions as, say, "rabbits" and "elephants". And the difference is not a subjective one, and matter of private associations. It is clearly a semantically relevant difference, manifested in our use of terms, if any is: these expressions are *true of* different things! This is a first point, one which Quine must accept (as he clearly seems to, see e.g. 1969: 47f., and 1981: 20).

But, and this is a second point, if there is a semantically relevant difference between expressions such as our "rabbits" and "undetached rabbit parts", then it seems that there has to be a fact of the matter as to whether some expression in another language is semantically like the one or rather like the other in the respects in which the two English expressions differ. Of course, it may be semantically like neither, in which case we should decline to accept either as an appropriate translation of it. But how can it be a matter of indifference which one we take to be its correct translation?

Quine clearly thinks that whatever we can say about the differences between two English terms such as the above has no bearing on semantic indeterminacy, because it cannot help us to determine a unique mapping correlating a given alien expression with one English term rather the other (or, perhaps, with neither). I now want to take up two questions in order. First, assuming that there is in English — as Quine must admit — a discernible, semantically relevant difference between terms like "rabbits" and "undetached rabbit parts", in what does this difference consist? In the remainder of this section, I will summarize the answer Quine himself provides, in "Use and its Place in Meaning" ("UIPM" hereafter). The second question is whether Quine's

answer to the first question has any bearing on the indeterminacy thesis. In the final section, I will argue that Quine is wrong in thinking that it does not. Whatever objectivity Quine has recovered for *intralinguistic* sameness and difference of term meaning can be carried over to the *interlinguistic* case, to the detriment of the indeterminacy thesis.

Quine's task in "UIPM" is to address the topic of meaning in a positive vein. Faithful to his nominalist commitment, which predates the indeterminacy thesis, to "talk of meaning without talking of meanings" (*op. cit.* p. 45, and see 1953), he reconsiders the question under what circumstances we would count expressions as having the same meaning. Rather than consider translation, as he had done before, Quine here addresses the question of synonymy *within a single language*. He finds that "a conceptual foundation for cognitive synonymy is pretty firmly laid" (p. 51).

Focusing on those aspects of meaning which pertain to truth and falsity, and ignoring the "emotional and poetic" aspects, Quine begins by defining the relation of "cognitive equivalence" for what he calls *occasion sentences* — sentences like "Today is Tuesday", "It is raining in London", or "There goes Bill's ex-wife", which would be assented to on some occasions but not on others.<sup>18</sup> Occasion sentences are cognitively equivalent for a given speaker if, and only if, she is disposed "to give matching verdicts when the two sentences are queried under identical overall stimulations" (p. 52), where an individual's overall stimulation is defined in terms of the triggering of her sensory receptors (p. 50). Such sentences have the same *stimulus meaning*, or are *stimulus synonymous*.<sup>19</sup> Interpersonal cognitive equivalence of occasion sentences — equivalence for a whole linguistic community (or an appropriate sub-community) — is then defined in terms of cognitive equivalence for each individual within it.<sup>20</sup>

Using the notion of cognitive equivalence for whole sentences, Quine is able to define a relation of cognitive synonymy between expressions. Expressions are cognitively synonymous if, and only if, they are interchangeable in occasion sentences while preserving the cognitive equivalence of the sentences. So, for instance, the synonymy of "oculist" and "eye-doctor" in the language of an appropriately circumscribed group of speakers can be grounded in the fact that, for each individual in the group, these two terms are substitutable in all occasion sentences *salva*

stimulus-meaning. (For instance, their verdict on "There is an oculist aboard the plane" will not change when "eye-doctor" is substituted for "oculist".) Once we have a criterion of synonymy for words, we can use word synonymy in formulating a sufficient condition for cognitive equivalence of *standing sentences* — sentences which, unlike occasion sentences, command a stable truth-verdict (assent or dissent) over time.<sup>21</sup> "One standing sentence is cognitively equivalent to another if it can be transformed into the other by a sequence of replacements of words or phrases by cognitive synonyms." (*ibid.*)<sup>22</sup> With this, Quine is satisfied that we have "basic enough for cognitive lexicography" (p. 54).

#### (4) "COGNITIVE SYNONYMY" AND TRANSLATION

Back to "rabbit" and "undetached rabbit part". We can use the "UIPM" tests to declare these expressions as different in cognitive meaning (and hence see that they cannot be accepted *simultaneously*, as alternate translations of "gavagai"), because interchanging them in some occasion sentence will alter its stimulus meaning. Substituting "undetached rabbit parts" for "rabbits" in the occasion sentence

(R) There are three *rabbits* by the bush.

will result in the *non-stimulus* synonymous occasion sentence

(U) There are three *undetached rabbit parts* by the bush.

There will be many occasions in which speakers will assent to (R) and dissent from (U), and vice versa (cf. Hookway 1988: §8.2). We may safely assume that analogues of the "UIPM" tests can be applied by Gavagese linguists to determine relations of intralinguistic synonymy and heteronymy among Gavagese terms (in particular, between the term "gavagai" and other Gavagese terms). This would serve to ground cognitive synonymy of terms *within* Gavagese in the verbal dispositions of its native speakers.<sup>23</sup>

The indeterminacy thesis, however, concerns (in the first instance) the possibility of grounding *interlinguistic* synonymy relations in the behavioral dispositions of speakers. Can the "UIPM" tests help us with this task? You might think they could, in the following way. If the



sentence resulting from placing "gavagai" in the native translation of our sentence schema

(C) There are three \_\_\_\_ by the bush

will receive the same verdict as our above (R) (or (U)) on all occasions, then, other things being equal, "gavagai" should be translated as "rabbit" (or "undetached rabbit part"). But, as Christopher Hookway points out (1988: 133), this presupposes the availability of an accepted translation for (C). Presupposing that we have established synonyms for the expressions in (C) when trying to defend the idea of interlinguistic synonymy would be question-begging.

A radical translator could determine whether Gavagese speakers assent to/dissent from some Gavagese occasion sentence under the same conditions in which English speakers assent to/dissent from (R) (or (U)); that is, whether or not a Gavagese occasion sentence is stimulus synonymous to (R) (or (U)). However, as Quine points out (1960: 46f. and "UIPM": 49f.), the notion of stimulus synonymy of occasion sentences can only be of use in attempts to rescue our intuitive notion of meaning if we limit ourselves to a single speaker. (The fact that the stimulus conditions of one speaker's assent to "John is a bachelor" differ from the stimulus conditions of *another* speaker's assent to "John is an unmarried man" does not attest to the non-stimulus synonymy of these two occasion sentences; the two speakers may have different collateral information about John.)

Thus, if we are to make use of the "UIPM" tests in the interlinguistic case, we must appeal to the verbal dispositions of bilinguals. Toward the end of "UIPM", Quine concedes that "if we have a whole sub-community of bilinguals [we can] summate over the individuals, as we did in the monoglot case, and derive a bilingual relation of cognitive equivalence of occasion sentences at the social level" ("UIPM": 54). We could then presumably determine, following the "UIPM" method, whether "gavagai" is a term synonymous with "rabbit", or "undetached rabbit part" or ... Yet Quine concludes "UIPM" by recapitulating his earlier insistence (see 1960: 72f.) that bilingualism cannot help refute the indeterminacy thesis:

The polyglot case thrives, it would seem, just to the extent that it can be treated as monoglot. Thus the theory I have been developing here has no bearing, that I can see, on the indeterminacy of translation. ("UIPM": 54)

What Quine is alluding to here is the fact that to test the synonymy of "gavagai" and "rabbit" by appeal to the dispositions of a bilingual speaker, we would first have to substitute the alien term "gavagai" for "rabbit" in, e.g., the English context "There is a . . . by the bush" (or the English term "rabbit" for "gavagai" in the corresponding Gavagese context). We would then check whether the bilingual's verdict regarding the new sentence, viz.

(H) There is a GAVAGAI by the bush.

is different from his verdict regarding the English sentence "There is a rabbit by the bush". But (H) is neither a sentence of English nor a sentence of Gavagese. If anything, it belongs to a new hybrid language. Inasmuch as we are confined to dispositions or bilinguals toward sentences like (H), we are 'treating the polyglot case as monoglot'.<sup>24</sup>

It is not obvious to me that we should be confined to bilinguals' dispositions to hybrid sentences. It seems as though we could make considerable progress in establishing cross-linguistic term synonymy using the dispositions of bilinguals toward *non*-hybrid sentences of the relevant languages. Consider, first, the question whether Gavagese has two expressions which are semantically related in the way our "rabbit" and "undetached rabbit part" are. The methods of *Word & Object* (see especially sections 10 and 11) allow us to discover that Gavagese has (if it does) two one-word observation sentences, say, "Gavagai!" and "Bagavagu!" that are stimulus synonymous to each other *and* to our (stimulus synonymous) "Rabbit!" and "Undetached rabbit part!". In addition, we may discover that "gavagai" and "bagavagu" occur as terms in Gavagese occasion sentences which are non-stimulus synonymous for Gavagese speakers. By the "UIPM" tests, the terms "gavagai" and "bagavagu" will then be non-synonymous in Gavagese.

Schematically, we are envisaging the following situation:<sup>25</sup>

English:	Gavagese:
[ "Rabbit!" SS "Undetached rabbit part!" ]	SS [ "Gavagai!" SS "Bagavagu!" ]
R: There are 3 rabbits by the bush NSS	G: . . . gavagai . . . NSS
U: There are 3 undetached rabbit parts by the bush.	B: . . . bagavagu . . .
"rabbit" NS "undetached rabbit part"	"gavagai" NS "bagavagu"
<small>'SS': stimulus synonymous; 'NSS': non-stimulus synonymous; 'NS': non-synonymous</small>	

This much similarity between the semantic situation of Gavagese and English may well be *necessary* for us to think that the term “gavagai” (or “bagavagu”) is (cognitively) synonymous to “rabbit” as opposed to “undetached rabbit part” (or vice versa).<sup>26</sup> But the similarity envisaged would not, on its own, provide a *sufficient* reason for declaring “gavagai” (or “bagavagu”) as synonymous with “rabbit” rather than “undetached rabbit part”. We need in addition information about semantic relations between the English and the Gavagese occasion sentences. This is where the dispositions of bilinguals become crucial. Suppose, for instance, we could establish the stimulus synonymy of R with G and of U with B.<sup>27</sup> Then it seems we would have a good reason to think that “gavagai” is synonymous with “rabbit” as opposed to “undetached rabbit part”. But we *could* establish synonymy (or otherwise) between English and Gavagese occasion sentences using the verdicts of bilinguals to *non-hybrid* sentences.<sup>28</sup>

Rather than develop the foregoing proposal so as to rule out all Quinean sceptical possibilities, I would like to go back to Quine’s claim that his indeterminacy thesis is left unscathed, as long as the polyglot case is parasitic on the monoglot case. I wish to argue that Quine is wrong about this.

Consider again the proposal that we determine the synonymy or otherwise of terms like “rabbit” and “gavagai” by consulting the reactions of bilinguals toward hybrid sentences such as (H) (“There is a GAVAGAI by the bush”). Quine presumably thinks the proposal has no bearing on the indeterminacy thesis, because the thesis concerns translation — the mapping between expressions of two distinct languages — whereas the proposal apparently capitalizes on treating the two languages involved as if they were one. But this is misleading. Following the proposal, we would indeed be basing claims of interlinguistic term synonymy, in the first instance, on the verbal dispositions of a single speaker toward sentences of a fictitious single language. However, by all standard measures (behavioral and others), bilinguals can still be seen as members of two perfectly distinct, independently existing linguistic communities. And the synonymy relations established by appeal to their dispositions toward hybrid sentences can still be regarded as obtaining between terms of distinct languages. An English-Gavagese bilingual is different from a monolingual English speaker (or

a monolingual Gavagese speaker) precisely in being in a position to pass judgment on hybrid sentences like (H). And the proposal trades on the idea that it is the dispositions of such bilinguals that are the right place to start when considering the synonymy (or otherwise) of “rabbit” and “gavagai”. Thus, the proposal is not an attempt to assimilate a problematic two-language case to an unproblematic single-language case. Rather, it draws our attention to the verbal dispositions which are the *relevant* ones for grounding semantic mappings between two languages.

This idea should seem very natural to a semantic verificationist. In the monoglot case, semantic verificationism holds that the facts about what expressions mean and about sameness and difference of meaning in a given language must be in principle within the epistemic purview of its speakers; it is the linguistic practices and judgments of the given linguistic community which provide the final court of appeal in such semantic matters. When we turn to questions of interlinguistic sameness and difference of meaning, it seems natural to think of the relevant community of speakers as being none other than a subcommunity of bilinguals. Applied to the polyglot case, semantic verificationism would then hold that it makes no sense to suppose that there could be a difference/sameness of meaning between expressions of languages spoken by two distinct communities which might forever escape such a subcommunity. But then the tests for interlinguistic synonymy *should* tap what is discernible by bilinguals.

Quine’s linguistic behaviorism requires that we cash out discernible semantic facts in terms of the behavioral dispositions of the relevant community of speakers. We have argued that, in the polyglot case, the relevant community is a bilingual community. Since Quine accepts that we could establish community-wide interlinguistic synonymy relations using the dispositions of subcommunities of bilinguals, it would seem that we are no worse off interlinguistically than we are intralinguistically.<sup>29</sup>

Quine might object that we are worse off in the polyglot case for the following reason. In the monoglot case, we typically already have at hand the relevant community of speakers, with the relevant dispositions in which to ground judgments of cognitive (non-) synonymy. In the polyglot case, on the other hand, we typically need to appeal to a

hypothetical community of bilinguals. Quine might ask: What guarantee do we have, that the dispositions of hypothetical bilingual speakers will line up in the way required for community-wide cognitive synonymy? Is it not perfectly possible that, on the contrary, there should be divergence across bilinguals precisely of the sort envisaged by the indeterminacy thesis? (See 1960: 72f.)

I think this objection loses its force, given the considerations of "UIPM". We may concede that it is *logically possible* that different bilinguals should have dispositions which would support different interlinguistic synonymy verdicts. But the "UIPM" position is that the facts of cognitive synonymy are to be sought not in logically possible dispositions but rather in what are *in fact* the dispositions of the relevant speakers. (After all, it is logically possible that the dispositions of speakers of a single language should not line up in the way required for Quine's notion of social monoglot synonymy.) In the polyglot case, we have argued, the relevant speakers are bilinguals. Where there is an existing community of bilinguals, we can look at the dispositions they do in fact have in order to ground interlinguistic synonymy, in complete analogy to the monoglot case (as Quine agrees). Where (as in most cases) there is no existing community of bilinguals, the question to be asked is still what dispositions hypothetical bilinguals *would in fact* exhibit, and not what dispositions it is logically possible for them to exhibit. This would preserve the connection established in "UIPM" between the facts of (cognitive) synonymy and the dispositions of relevant speakers.

Accepting Quine's (implicit) semantic verificationism and his (explicit) linguistic behaviorism means accepting that questions of interlinguistic synonymy can be regarded as factual only to the extent that there are facts about the behavioral dispositions bilinguals would have involving the relevant linguistic expressions. But we have every reason to think that there are such facts. Not only are there bilinguals, whose dispositions can be tested in just the way prescribed by "UIPM"; there are also very reliable ways of 'producing' bilinguals, so as to give empirical substance to the notion of a hypothetical community of bilinguals to which we have appealed above. As we have conceded, bilinguals *could* (logically) diverge in the way envisaged by the indeterminacy thesis. But whether they would or not is an empirical question. We have good reasons to believe that they would not.

Quine may also object that establishing the objectivity of interlinguistic synonymy relations among terms fails to remove the real obstacle to the objectivity of empirical semantics. The real obstacle, Quine has argued (see, e.g., 1969: 26–68 and 1981: 1–23), is that, ultimately, there is no fact of the matter as to what each term in our own language is about. However, to argue for this claim, Quine must — and does — make use of considerations about translation, or mapping, between two languages. A crucial step in his argument appeals to the possibility of systematic alternative term translations.<sup>30</sup> I have tried to argue that, using the "UIPM" tests, one can establish whether a term of another language should be translated by our "rabbit" rather than by our "undetached rabbit part". But, assuming that there is such a thing as understanding our own terms (which Quine does not wish to deny, see e.g. 1981: 20), and assuming that we do in fact understand our terms "rabbit" and "undetached rabbit part", whichever expression gives the correct translation of some term in another language can serve to *specify the meaning* of that term. Since the same can be done for our terms (in another language), it will turn out that an argument which establishes the objectivity of interlinguistic relations of sameness/difference of term meaning would *ipso facto* take the sting out of Quine's claim that the meaning of our own terms is indeterminate.

\* \* \*

Using the thought experiment of radical translation, Quine has long sought to undermine the objectivity of our ordinary notion of meaning by arguing that there are no objective facts concerning correct translation. The tests he develops in "UIPM" allow him to give objective sense to the notion of cognitive synonymy within a language even on the austere linguistic behaviorism he advocates: relations of intralinguistic synonymy (and heteronymy) are grounded in the verbal dispositions of the speakers of the language. I have argued that, analogously, we should think of relations of interlinguistic synonymy as grounded in the verbal dispositions of (actual or hypothetical) bilinguals. By the standards of Quine's "UIPM", this should be enough to give sense to the idea of correct translation. If so, then both translation and meaning would be safe from Quinean indeterminacy.<sup>31</sup>

## NOTES

<sup>1</sup> Reprinted in (Quine 1981). One place this paper is discussed is (Hookway 1988: Ch. 8).

<sup>2</sup> Quine variously talks about the evidence as including only dispositions to verbal behavior, all behavioral dispositions, or, sometimes, all possible evidence non-intentionally characterized. He clearly thinks (see "TTA", and our discussion below) that the evidence *relevant* for meaning does not go beyond all the behavioral dispositions of speakers. But he thinks the indeterminacy thesis holds even if we include other sorts of possible evidence (say, about brain physiology, physical environment, etc.), as long as the evidence is not described in intentional terms. This is what he means when he says that "Brentano's thesis of the irreducibility of intentional idioms is of a piece" with the indeterminacy thesis (1960: 221).

<sup>3</sup> Quine often claims that no indeterminacy infect the translation of observation sentences (e.g., "This is red"; "Here is a ball"). At most, however, he could argue that the translation of such sentences *thought of as one-word sentences* is determinate. (For discussion, see my 1987: Chapter II.) For simplicity, I shall ignore this qualification.

<sup>4</sup> See my 1990. A fuller development of claims made in the present section occurs in my 1991c.

<sup>5</sup> The question which launches Quine's discussion of translation in *Word & Object* is: "how much of [our own] language can be made sense of in terms of its stimulus conditions" (1960: 26). And his answer is: Not much.

<sup>6</sup> Relevant here is also Quine's famous figure of the elephantine form overlying very unlike configurations of twigs and branches (1960: 8&94).

<sup>7</sup> Quine's point is in good part that an empirical semantics so conceived would lack a subject-matter; see below.

<sup>8</sup> More to the point would be the comparison (which I pursue further in "Translation and the Subject Matter of Linguistics") of linguistics to sciences which do concern ground level judgments, like the theory of perception, or economics.

<sup>9</sup> The nature of the epistemic access of speakers to linguistic facts is a delicate matter which deserves careful examination. I discuss it in 1991b.

<sup>10</sup> I thus want to distinguish what I call "semantic verificationism" from the so-called "verification theory of meaning" or from Dummettian verificationism, which holds that the meaning of any sentence — in any domain — consists in its verification or assertibility conditions. (It is this latter view which is taken by Dummettians to entail that there can be no verification-transcendent truths.) I discuss these matters in 1991b.

<sup>11</sup> Although Quine never explicitly endorses semantic verificationism as we expressed it, and may balk at our talk of 'epistemic access' and 'semantic judgments'.

<sup>12</sup> See e.g. Chomsky (1975: Chapters 1 & 2).

<sup>13</sup> So, for example, after using the correct past tense form *went* for a little while, children will start to 'overgeneralize' and say *goed* instead of *went*, and will continue to do so despite corrections, until a later (apparently clearly marked) stage, where they go back to *went*.

<sup>14</sup> As Putnam points out (*op. cit.*), Chomsky's mentalism has concerned explicitly only syntax. The application of Chomsky's ideas to semantics is primarily due to Fodor (1975, and elsewhere).

<sup>15</sup> We can distinguish (Cartesian) *dualist* mentalism — the view that the representations which are said to constitute meanings are features of disembodied Cartesian minds — from the more fashionable *physicalist* (or physicalist-functionalist) mentalism — which takes the representations to be features of a mental mechanism which is physically

realized in speakers' bodies. Quine, himself a self-proclaimed physicalist, rejects linguistic mentalism even in its physicalist version.

<sup>16</sup> Again, see my 1991b and 1991c.

<sup>17</sup> Arguably, Quine's notion of behavior is unreasonably stark, and quite distant from the ordinary one. There may well not be a complete and coherent level of description of human behavior relative to which we can discern assent and dissent behavior but not, say, whether a speaker is concerned with whole bodies rather than their undetached parts.

<sup>18</sup> A special sub-class of occasion sentences is the class of *observation sentences*. These are occasion sentences which would be assented to/dissented from by *anyone* present on the occasion of utterance (e.g., "There goes a rabbit"; "This is red"). Interestingly, by this 'social' definition, "It hurts!", which would intuitively (and by some of Quine's *other* definitions) count as an observation sentence, would turn out not to be an observation sentence.

<sup>19</sup> Quine discusses the notion of stimulus meaning in (1960: §8). For one telling criticism of this notion, see Paul Ziff (1972). I am here not so much concerned to evaluate Quine's attempt to make sense of the notion of cognitive meaning as to examine how the materials used in this attempt can be used to undermine his indeterminacy thesis.

<sup>20</sup> The definition of cognitive synonymy for a (sub-)community does not assume equating of stimulations across individuals. Whereas in (1960: §8) Quine spoke freely of comparing stimulus meaning (at least of observation sentences) across individuals, even when they were members of distinct linguistic communities, here Quine denies that we can equate stimulations across individuals, since individuals do not share the same receptors and need not even have "exactly homologous receptors" (see p. 50f.).

Notice that the definitions Quine provides here are doubly individualistic in Tyler Burge's sense (cf. his 1979). First, Quine individuates stimulus meaning in terms of "the external forces that impinge on the interrogated subject at the time, . . . only insofar as they affect his nervous system by triggering his sensory receptors" ("UIPM": 50). From which it follows that if the same receptors were triggered on two different occasions (and the individual would be disposed to give the same verdict to the sentence) the sentence would be said to have the same stimulus meaning, regardless of the nature of the cause of the stimulation. Secondly, Quine defines community-wide cognitive synonymy — and thus cognitive meaning — of occasion sentences (and then of terms) in terms of a summation over cognitive meaning for each separate individual within it. For the anti-individualist, the meaning of a speaker's utterance will be partly determined by her social environment — by how words are used in her linguistic community. Whereas for Quine community meaning is a completely derivative notion.

<sup>21</sup> Since standing sentences "grade off into occasion sentences" depending on the length of time one considers, Quine thinks we can extend the criterion of cognitive synonymy for occasion sentences (i.e., "the criterion of like verdicts under like stimulation") to standing sentences, "provided that we take it only as a necessary condition . . . and not a sufficient one" (p. 53).

<sup>22</sup> This condition is closely reminiscent of Carnap's "structural isomorphism", in (1956: §14).

<sup>23</sup> Indeed, the facts of intralinguistic synonymy/heteronymy can be established not only by Gavagese lexicographers, but also by a radical translator. This is so given that a radical translator can in principle determine which occasion sentences of a native language are stimulus synonymous for a given speaker, even without knowing their meanings (as Quine himself notes, 1960: 47ff.).

<sup>24</sup> It may be objected that, given the diversity in surface grammar among languages, one could not expect the 'hybrid' proposal to have general applicability. In many cases,

differences in surface grammar would not allow the kinds of term substitutions required by the bilingual version of the "UIPM" test. (Quine's own Japanese example, 1969: 35–8 may be a good case in point.) Two points are in order. First, Quine has always warned against confusing his indeterminacy claim with claims about difficulties in translating between radically different languages. Quinean indeterminacy is supposed to obtain between languages with very similar surface grammars (indeed, it is supposed to obtain homophonically). Secondly, and relatedly, to the extent that grammatical differences stand in the way of applying the 'hybrid' test in a given case, we would have a measure of the semantic differences between the relevant two languages, and evidence for the unacceptability of given translations, rather than support for the acceptability of more than one translation (which is what the indeterminacy thesis claims).

In any event, since Quine himself does not invoke grammatical diversity as a reason against 'hybrid' tests, I shall ignore the issue in what follows. Instead, I shall discuss the complaint that such tests — even if applicable — would have no bearing on the indeterminacy thesis.

<sup>25</sup> Needless to say, the picture here oversimplifies greatly, in making it appear that English and Gavagese exhibit syntactic isomorphism. But see previous note.

<sup>26</sup> If nothing like this similarity obtains, then suspicion may arise that the distinction we draw in English between "rabbit" and "undetached rabbit part" is not semantically relevant in Gavagese. Gavagese may be semantically poorer than English in that respect. Perhaps "gavagai" should be regarded as a 'vague' — or rather 'non-specific' term — with respect to the distinction marked in English. If so, then there would indeed be at least no semantic reason to prefer "rabbit" over "undetached rabbit part" as a translation. Gavagese speakers may in turn draw semantic distinctions which we do not. There are good reasons to expect a great variety of semantic 'mismatches' among languages. The existence of such mismatches should erode any *a priori* confidence that there are always exact interlinguistic (cognitive) synonyms to be found. But it does nothing to support the indeterminacy thesis. (See Kirk 1987: Ch. 1 and my 1991a.)

<sup>27</sup> This assumes that Gavagese has sentences with the same stimulus meanings as (R) and (U) and their likes, which it may not. But whether it does or not is a determinate, factual question for Quine.

<sup>28</sup> One may worry that the proposal somehow illegitimately trades on an assumption that there is a unique translation manual from *English into Gavagese*. (Christopher Hookway, in correspondence, has raised what I take to be this issue. Any misunderstanding here is my own.) Suppose, however, that there are many acceptable English-Gavagese translation manuals, M1, M2, etc. Let M1 translate (R) by (R1) and (U) by (U1); let M2 translate (R) by (R2) and (U) by (U2). Since by Quine's standards translation manuals must preserve stimulus meaning, it will turn out that (R1) and (U1) will be non-stimulus synonymous, due to the non-stimulus synonymy of (R) and (U); similarly for (R2) and (U2). Furthermore, (R1) and (R2) — as well as (U1) and (U2) — will have to be stimulus synonymous to each other, due to their respective stimulus synonymy with (R) and (U). Given these provisions, I think my proposal should still go through.

<sup>29</sup> It should be noted that, insofar as Quine does not think there is a set of conditions which *both* necessary and sufficient for the cognitive synonymy of standing sentences (see "UPIM", p. 53f.), one may still need to argue for the full objectivity of such synonymy.

<sup>30</sup> For discussion, see my 1987: Ch. II.

<sup>31</sup> I wish to thank Bill Lycan, Mike Resnik, and especially Keith Simmons, for reading earlier drafts of this paper and providing helpful comments.

## REFERENCES

- Bar-On, Dorit, 1986, 'Semantic Indeterminacy and Scientific Underdetermination', *Pacific Philosophical Quarterly*, 245–263.
- Bar-On, Dorit, 1987, 'Indeterminacy of Translation: Theory and Practice', unpublished doctoral dissertation.
- Bar-On, Dorit, 1990, 'Scepticism: The External World and Meaning', *Philosophical Studies* 60, 207–231.
- Bar-On, Dorit, 1991a, 'Indeterminacy of Translation: Theory and Practice', forthcoming.
- Bar-On, Dorit, 1991b, 'Anti-Realism and Speakers' Knowledge', in preparation.
- Bar-On, Dorit, 1991c, 'Translation and the Subject-Matter of Linguistics', in preparation.
- Burge, Tyler, 1979, 'Individualism and the Mental', in *Midwest Studies in Philosophy* 10, French, Uehling and Wettstein (eds.)
- Carnap, Rudolf, 1956, *Meaning and Necessity* (Chicago and London: The University of Chicago Press).
- Chomsky, Noam, 1975, 'Quine's Empirical Assumptions', in Davidson & Harman (eds.), *Words and Objections* (Dordrecht, Holland: D. Reidel).
- Chomsky, Noam, 1976, *Reflections on Language*. Fontana/Collins.
- Fodor, Jerry, 1975, *The Language of Thought* (New York: Crowell).
- Hookway, Christopher, 1988, *Quine* (Cambridge: Polity Press).
- Kirk, Robert, 1986, *Translation Determined* (Oxford: Clarendon Press).
- Quine, W. V., 1953, *From a Logical Point of View* (Cambridge, Mass.: Harvard University Press).
- Quine, W. V., 1960, *Word & Object (W&O)* (Cambridge, Mass.: MIT Press).
- Quine, W. V., 1969, *Ontological Relativity and Other Essays* (New York: Columbia University Press).
- Quine, W. V., 1970a, 'On the Reasons for Indeterminacy of Translation', *Journal of Philosophy* 178–183.
- Quine, W. V., 1970b, 'Philosophical Progress in Language Theory', *Metaphilosophy* 1, 2–19.
- Quine, W. V., 1975a, 'On Empirically Equivalent Systems of the World', *Erkenntnis* 9, 313–328.
- Quine, W. V., 1975b, 'The Nature of Natural Knowledge', in Guttenplan (ed.) *Mind and Language*, pp. 57–81.
- Quine, W. V., 1976, 'Linguistics and Philosophy' as reprinted in *The Ways of Paradox* (revised edition) (Cambridge, Mass.: Harvard University Press).
- Quine, W. V., 1981, *Theories and Things* (Cambridge, Mass.: Harvard University Press).
- Quine, W. V., 1987, 'Indeterminacy of Translation Again', *Journal of Philosophy*, 5–8.
- Quine, W. V., 1987a, 'Reply to Alston', in Hahn & Schilpp (eds.), *The Philosophy of W. V. Quine* (La Salle, Ill.: Open Court), pp. 73–75.
- Putnam, Hilary, 1988, *Representation and Reality* (Cambridge, Mass.: MIT Press).
- Wittgenstein, Ludwig, 1953, *Philosophical Investigations* (Oxford: Basil Blackwell & Mott).
- Ziff, Paul, 1972, 'A Response to Stimulus Meaning', in *Understanding Understanding* (Ithaca & London: Cornell University Press).

Department of Philosophy  
The University of North Carolina at Chapel Hill  
Chapel Hill, NC 27599  
USA