

QUINE ON TRANSLATIONAL INDETERMINACY

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1. *Introduction.** If sound, Quine's thesis of translational indeterminacy¹ is of considerable philosophic moment. Among other things, it highlights the radical underdeterminateness of "experience" to our conceptual schemes. This use of "scheme" comes now to be seen as literally accurate. The myth of the given follows as a corollary.² But is it sound? I am not prepared to address this larger question here. However, it is indeed vulnerable; and it is just this vulnerability that I want to point up and hopefully mitigate in this paper. Recently Stephan Davis exposed a crack in the thesis and construed it as the opening in which to lodge his destructive charge. It is placed strategically near the keystone:

What is necessary for Quine's argument for translational indeterminacy to be intelligible is that there be translations for "gavagai" which differ in meaning. But Quine's criterion for meaning marks them as synonymous. Consequently, it makes no sense to say that we cannot determine the meaning of "gavagai" in English. For all the choices mean the same.³

Quine's own standard for determining meaning undercuts one of the premises on which the argument for indeterminacy turns: a variant of the liar paradox. For purposes of future reference let us call this point *Davis' dilemma*.

Nevertheless the demolition tactic sputters because Davis fails to notice other ways of deciding possible alternative meanings of a sentence; ways different from those stipulated by the behavioral criteria that Quine labels its "stimulus meaning."⁴ Notwithstanding that these alternative ways are consistent with Quine's overall thesis, Davis' analysis does underline

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1. W. V. O. Quine, *Word and Object* (Cambridge, Mass., 1960), Chap. 2.

2. This point is developed in "A Probabilistic Account of Truth," *The Monist*, 53, 2 (April 1969).

3. "Translational Indeterminacy and Private Worlds," *Philosophical Studies*, 18, 2 (April 1967), p. 41.

4. *Word and Object*, p. 46.

the fact that Quine's presentation is, as it stands, deficient. I shall try to establish these two points.

Suppose Gavag language users said that the fly in the fly-bottle remained stationary come what may, but that on occasion the bottle zig-zagged unpredictably, a logically admissible reconstruction of the relative motion phenomena. Suppose further that they systematically adjusted their ways of speaking of relative motion so as to make them comport with the zig-zagging fly-bottle locutions, a large order of linguistic reformation to be sure, but theoretically possible. In point of "stimulus synonymy" criteria, conceivably our (1) "The fly is moving" and the native's (2) "Gavageee!" are indistinguishable, while yet a meaning difference in theoretical framework ("analytical hypotheses"⁵) is detectable. Thus an affirmative, but not a negative, response to the query "Gavageee?" is regularly accompanied by feelings of gidiness, perhaps a sense of vertigo, as we are made to feel vertiginous by watching a roller coaster on cinerama. If this is the case, then the two sentences can be judged stimulus synonymous, while yet issuing from detectably different analytical hypotheses.

Now the example is fantastic, but suggests the way that meaning analyses can be conducted at sundry levels, at what I would call the *theoretical* as well as the *sentential* level. It suggests how meaning differences at one level may not show up at another, "lower" level. The behavioral criteria to which Davis understandably makes repeated reference (e.g. "For Quine, the only evidence available is that given by the behavioral criteria," p. 40; and see p. 29 of *Word and Object*), refer only to meaning analyses at the sentential level. But it is from an observed difference in the *set* of affirmative, negative, and indeterminate responses to "occasional sentences"⁵ between native and English speaker, that the linguist may infer a meaning difference between some pair of these sentences, sentences declared "stimulus synonymous," as this expression is defined by Quine. This point is cardinal. Let me try another instance.

Motion phenomena provide the most illustrative cases. (3) "Lo, the sun rises!" That is, the distance differential between the sun and the horizon increases. Copernican: Yes. Ptolemaist: Yes, yes. (4) "Lo, the horizon recedes!" Copernican: Yes. Ptolemaist: No. Conceivably (3) and (4) are stimulus synonymous.⁶ But let the Copernican and Ptolemaist converse further and soon, that is beyond the level of occasion sentences like (3) and (4), meaning differences at the supra-sentential level

5. See *Word and Object*, pp. 68-70.

6. On the operational equivalence of sizeable portions of Copernican and Ptolemaic astronomy, see A. R. Hall, *The Scientific Revolution* (Boston, 1957), pp. 370-371; see also T. S. Kuhn, *The Copernican Revolution* (New York, 1959), pp. 64-73. Incidentally, I am assuming that when we use cognate expressions to (3) in everyday speech, e.g. "See the beautiful sunrise," we are speaking idiomatically, not literally. This point becomes critical in the discussion of section 4, below.

emerge, at the level at which the meaning of an individual sentence is a function of a *set* of sentences. This is what I called in the last paragraph the theoretical level. Similarly, for Quine's example cluster: "Lo, a rabbit," "Lo, a rabbit stage," "Lo, a manifestation of rabbithood," and so forth. At the sentential level, stimulus synonymous; at the supra-sentential, the theoretical or propositional-network level, not. Thus might different analytical hypotheses match translations given by the behavioral criteria. And it is by examining prospective analytical hypotheses and their entailment relations, not just by examining dispositions to verbal behavior with respect to the occasion sentences alone, that we determine possible meaning differences at the sentential level, compatibly with stimulus synonymy of these sentences.

Just as the so-called data or "observational sentences" are underdetermined with respect to the scientific hypotheses or theories that purport to explain or subsume them, so too are occasion sentences underdetermined with respect to the sets of analytical hypotheses that might subsume them under a single framework. The meanings of these occasion sentences are determined not only by the battery of non-verbal stimulations that prompt assent on the part of the language user when the appropriate sentence is put to him interrogatively (the "stimulus meaning" test), but *also* by the analytical hypotheses that successfully subsume them (the "theoretical meaning" test). Quine mentions explicitly only the first.⁷ It follows that the stimulus meaning of "Gavagai" is "Lo, a rabbit," "Lo, a temporal stage of rabbithood," etc., indifferently—as is the stimulus meaning of "Lo, a rabbit" itself (the "private worlds" doctrine). However, its "theoretical meaning" is one or other of these "stimulus" possibilities, depending on which set of analytical hypotheses we choose to deploy for patterning the totality of our non-verbal stimulations.

2. The interesting point about translational indeterminacy, then, is that behavioral criteria alone, as these are defined by Quine apropos his development of stimulus synonymy criteria, do not enable the linguist to detect all meaning differences at the occasion-sentence level. Even were the linguist so positioned as to be able plausibly to infer that a feeling of vertigo accompanied the Gavag users' affirmative response to "Gavageee?" it is still stimulus synonymous with "The fly is moving": namely, when, and only when, we respond affirmatively to (1), put interrogatively, they likewise respond affirmatively to (2), put interrogatively, and so for negative and indeterminate responses. This and only this is involved in the stimulus synonymy test. Therefore, by his expression, the native ostensibly means what we mean by ours. Nevertheless at a later date the linguist may come upon grounds for surmising that the native believes flies to be sacred, or—to switch to the more plausible geocentric versus heliocentric example—that the sun is sacred and, furthermore, that immobility

7. But see *Word and Object*, pp. 35ff.

is a concomitant attribute of sacred things. Now he is in position to conjecture that the native means by his expression something different, after all, than we presumably mean by ours; notwithstanding that the two are stimulus synonymous. Still, one who did not share the linguist's conjecture could continue to maintain that (1) and (2) are stimulus synonymous and that the native means by (1) what we mean by (2), supposing these two claims to be meaning-different.

We might then speak of *behavioral criteria*₂ as well as behavioral *criteria*₁, corresponding to the distinction between sentential and theoretical meaning, above. This second level of criteria enables the linguist to opine that different analytical hypotheses are operative and that these, in turn, entail different meanings vis-à-vis the two sentences established as stimulus synonymous by behavioral *criteria*₁. Inescapably, this conjecture remains in the realm of a reasonable, informed guess, much as do good scientific theories or judicial pronouncements of guilt or innocence. In fact, the manner in which the conjectural experience of vertigo is inferred resembles heuristic procedures common to all manner of good theorizing, whether scientific or detective-story. Obviously the native did not volunteer that he was vertiginous. Rather there was (say) the characteristic "swallowing of the stomach" phenomenon accompanying an affirmative response to (2), but linguistically nonfunctional. Given the esteem in which natives seem to hold members of the fly family, coupled with hints about the status of "immobility" in the native's putative conceptual inventory, the linguist elaborates a theory about the subscript-two meaning of (2).

3. One might want to protest that this physiological correlate will already have prompted the linguist at the behavioral *criteria*₁ level to differentiate the meanings of (1) and (2). He places electrodes in the visual cortex of the native, let us say, and observes that an encephalograph reading is commonly different when the affirmative response is to (2) than when it is to (5) "Gavage?". Whereas (5) and (6) "The mosquito is moving" are observed to be stimulus synonymous. And no comparable difference shows up on an encephalograph taken during responses to (1) and (6) on the part of the English user. To be sure, this sort of scrupulous analysis of responses might warrant withholding the ascription of stimulus synonymy to sentences that otherwise would be judged so. Although it is worth mentioning that by including non-linguistic responses on the part of our respondent, we have hereby modified Quine's definition of stimulus meaning. What is more, at the level of reading a respondent's encephalograph taken during his linguistic utterances, the notion of stimulus synonymy and more generally, the notion of stimulus meaning itself threatens to become even murkier than it already is. We might as well look to our respondent's entrails, almost. Yet, at the level of occasion sentences, stimulus meaning is the best intersubjective test of meaning that we have.

However, the essential point is not really affected by this conceivable refinement in analysis anyway. In order for the suspicion initially to arise that two sentences, declared stimulus synonymous by the canonical method,

do not finally mean the same thing, there need be no differential physiological response. Ideally, stimulus-synonymy tests, those between (3) and (4) say, however detailed, do not turn up relative semantic differences. We begin to suspect a difference in meaning only upon framing competing "analytical hypotheses," or theories, *in whose terms* the two occasion sentences have different meanings. In other words, while purporting to pattern the same range of phenomena, two theories, at the level of occasion sentences, yield mutually incompatible consequences, have what we might call different "illocutionary act potentials."⁸ Moreover, these differences so offset one another that proponents of each theory "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 . . . in a wide range of cases."⁹

Therefore if the linguist has grounds for believing that the native means something different by his occasion sentence than we mean by ours (the meaning of ours is finally determined by the set of analytical hypotheses that we apply to it), though they are stimulus synonymous, he can bring to bear the usual inductive procedures to test his hypothesis. For example, he can try it out on the natives, springing new sentences authorized by his hypothesis, to see if they turn out right. Quine speaks of this as a permuting of the time order:

One frames the theory before all possible data are in, and then lets it guide one in eliciting of additional data likeliest to matter. This is good scientific method, but it opens up no new fund of data.¹⁰

By the margin that his hypothesis is corroborated, namely by the margin that he can effectively pattern native responses by means of his hypothesis, we may speak of the subscript-two meaning difference between (1) and (2). I take this to be at least part of what Quine means in the passage quoted from p. 27 of *Word and Object* (quoted on pp. 42-43 of Davis' article), and which Davis, somewhat understandably, finds "cryptic."

Here one might complain that either (1) is or is not synonymous with (2). If the "theory," as I have called it, is false, then nothing has been shown about an alleged meaning difference at the level of occasion sentences like (1) and (2). If true, then it is not a theory at all. But recall that the claim is not that (1) and (2) have different meanings, but that analytical hypotheses might be formulated which at once preserve stimulus synonymy between them, yet render them different in meaning. In other words, stimulus synonymous sentences are not, for Quine, meaning-synonymous simply. This is the gist of my difference with Davis. But Quine's thesis

8. W. Alston, *Philosophy of Language* (Englewood Cliffs, N.J., 1964), p. 37ff.

9. *Ibid.*, p. 26.

10. "Speaking of Objects," *Contemporary Philosophic Problems*, edited by Y. Krikorian and A. Edel (New York, 1959), p. 144.

has to be interpolated on this point; he is not explicit. And Davis' charges are, I believe, well founded. If it is so interpolated, then the thesis is neither meaningless nor inconsistent, the two options that Davis allows it as it stands.¹¹

In a nutshell, the way that we construe the native's linguistic response to his sensory stimulations, namely the analytical hypotheses that we construct to interpret what he means, is adequate, but not uniquely adequate to those stimulations and those sentences. Similarly, as regards the way that we construe *our* sentence meanings. They may be construed in various ways, each construction adequate to the corresponding sensory stimulations, while mutually incompatible overall. My solution to Davis' dilemma (see section 1) is to say that on behavioral criteria₁, all the choices mean the same; on behavioral criteria₂, they do not. Textual support for this distinction may be sought on p. 71 of *Word and Object*: "Most of the semantic correlation is supported only by analytical hypotheses, in their extension beyond the zone where independent evidence for translation is possible." Again, "... the truths that can be said even in common-sense terms about ordinary things are themselves, in turn, far in excess of any available data ... are less than determined by our surface irritations" (p. 23).¹²

Thus all Quine can mean by saying that stimulus synonymous sentences might differ in meaning is that different analytical hypotheses might be elaborated which *construe* the two sentences differently, endow them with different illocutionary act potential, yet each of which hypothesis-sets renders the sentential network consistent with the elusive "surface irritations." Davis' analysis forces a reappraisal of the meaning of 'stimulus meaning'. Unless one adopts something like the distinction above, there is an apparent contradiction between certain of Quine's claims. For example, "Sentences translatable outright, translatable by independent evidence of stimulatory occasions, are sparse ..." (p. 72), and "... we may meaningfully speak of the truth of a sentence only within the terms of some theory or conceptual scheme" (p. 75). The first sentence, unlike the second, suggests that there are *some* sentences at least whose meaning is ascertainable independently of appeal to anything outside of "stimulus meaning" criteria, ascertainable by behavioral criteria₁ exclusively. And this poses the Davis' dilemma. By reason of what evidence could we tell that the meanings of *these* sentences differed or might differ from those of their stimulus synonymous counterparts? If we could invoke only behavioral criteria₁, the answer is that there is no such evidence, and this is precisely the thrust of Davis' dilemma.

11. "Translational Indeterminacy," p. 38.

12. See also all of sec. 6, pp. 21-25; p. 75; pp. 78-79; also "Speaking of Objects," *op. cit.*, p. 144; "Two Dogmas of Empiricism," in *From a Logical Point of View* (New York, 1961), pp. 42-43.

4. Non-textual support for the distinction between the two levels of behavioral criteria may be sought in the way that we customarily discriminate the meanings of what philosophers of science have traditionally called "observation sentences." These are sentences that theories, among other functions, purportedly integrate, or which are used in the confirmation or disconfirmation of the empirical laws which they [the theories] pattern. Thus we may finally determine that (3) and (4) differ in meaning because of consequences that one entails and the other does not. In short, we assess their subscript-two meaning in terms of the consequences of the theories or propositional networks to which they belong and in which they figure. And the propositional network in which one, and not that in which the other, figures entails that, e.g., the earth is stationary.

The general point is the continual interaction of theory with fact—"the way in which theories are built on facts, while at the same time giving significance to them and even determining what are 'facts' for us at all."¹³ Case: that the earth moves through space is now a fact, though representing, in Galileo's phrase, "a rape on the senses . . ." What is equally to the point is that the differences just alluded to are, in each case, behaviorally detectable. But they are so only at the level of the *set* of one's affirmative, negative, and indeterminate responses to "occasion" sentences put interrogatively, the level of behavioral criteria₂.

As a final illustration, consider Priestley's and Lavoisier's responses to the "occasion" sentences (7) "Lo, dephlogisticated air?" and (8) "Lo, oxygen?", when queried in the presence of the results of the same experimental situation. The celebrated experiment with the red calx of mercury is usually given as playing the crucial role in deciding between the supporter of the phlogiston theory and the modern theory of the elements. Hereupon Lavoisier first obtained the calx from mercury by heating, and thence reversed the reaction. It is assumed that upon seeing the experiment, the Phlogistonian cannot but recognize its decisiveness:

As we watch the volume of gas changing, we find it hard to deny that here, before our eyes, is an irresistible proof that the calx is a compound and not an element; and that it is converted into a metal not by imbibing anything from outside, but by giving off the extra gas which we see in Lavoisier's container, and whose loss from the calx the balance confirms.

"Lo, oxygen!" It therefore comes as a momentary surprise to us

to recall that this experiment was in fact derived originally, not by Lavoisier but by Joseph Priestley . . . The fact is that he, Priestley, had hit upon another experiment which, from the point of view of the Phlogistonian, supported his theory even more strongly than did the mercury experiment support Lavoisier's.¹⁴

Believing is seeing.

13. S. Toulmin, *Foresight and Understanding* (New York, 1961), p. 95.

14. S. Toulmin, "Crucial Experiments: Priestley and Lavoisier," *Journal of the History of Ideas*, 18, 2 (April 1957), p. 206.

Different "analytical hypotheses" applied by the two theorists to the stimulus synonymous sentences (7) and (8) render them different in meaning at the subscript-two level. We can determine that they differ in meaning by observing that they entail different consequences. Thus, one entails the existence of substances with negative weight while the other does not: in short, the two claims have different illocutionary act potentials.

It follows that the unit of significance is to be looked for at the suprasentential or "theoretical," level. Or, better, an occasion sentence's meaning is given (a) by the non-verbal stimulations which prompt assent to it when put interrogatively, and (b) by the network of propositions applied to the phenomenon (of which the sentence is a description) in order to render it intelligible by relating it to other, otherwise disparate phenomena. In different ways, both scientific theories and natural language satisfy role (b).¹⁵ Together (a) and (b) combine to yield a sentence's meaning, *even that of an occasion sentence*. Stimulus meaning refers only to (a).

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15. See *Word and Object*, p. 22.