

The indeterminacy of translation

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W. V. O. Quine's contention that translation is indeterminate has been among the most widely discussed and controversial theses in modern analytical philosophy. It is a standard-bearer for one of the late twentieth century's most characteristic philosophical preoccupations: the scepticism about semantic notions which is also developed in Kripke's interpretation of Wittgenstein on rules (see Chapter 15, *RULE-FOLLOWING, OBJECTIVITY AND MEANING*) and which many have read into Putnam's 'model-theoretic' assault on realism (see Chapter 17, *PUTNAM'S MODEL-THEORETIC ARGUMENT AGAINST METAPHYSICAL REALISM*). The more general concern reflected by these arguments is how space can be found for the reality of meanings – and indeed for other norms, like the ethical – in a world whose fundamentals, as the orthodox wisdom has it, are apt for complete characterization by the methods and vocabulary of physical science.¹

If Quine's arguments succeed, this is not a concern which we should seek to allay, since there can be no satisfactory answer to it. At least, that will be the position if, with Quine, we take it that if translation is indeterminate, so is meaning itself, so that there are accordingly no genuine facts about meanings for a satisfactory world-view to accommodate. Quine envisages, moreover, that a consequential indeterminacy must spread outwards to infect ordinary "folk" or intentional psychology which comprises states, like beliefs and desires, which are in part identified by their content, as well as modal properties of statements, such as necessity and possibility, which functionally depend upon those statements' meanings (see Chapter 19, *MODALITY*). Much is at stake, then, in Quine's thesis.

The discussion to follow is organized as follows. Sections 1 and 2 will offer some initial reflections on the content and implications of the indeterminacy thesis, and of the presuppositions that Quine makes in treating it as a stepping-stone to semantic irrealism. Section 3 then distinguishes Quine's two principal arguments² for the thesis: the famous 'gavagai' argument ('from Below') of *Word and Object* (1960), and the argument ('from Above') from the underdetermination of empirical theory by data emphasized in "On the reasons for the indeterminacy of translation" (1970), and lays out the essentials of the former. Section 4 appraises this argument in the light of Gareth Evans's discussion in his "Identity and Predication" (1975). Section 5 assesses the cogency of Evans's objections. Section 6 turns to the second and more radical argument, laying out certain basic distinctions and implications; and section 7 is concerned with its appraisal.

Quine's contribution to these issues is, in effect, no less than to have invented

them and to have set the agenda for all subsequent discussion. He has continued a vigorous engagement in that discussion, and students of some of his more recent contributions, for instance in *Pursuit of Truth* (1990) or the "Three indeterminacies" paper (1989), will have noted significant developments and changes of emphasis. But these are beyond the purview of this chapter, whose concern is not with scholarship of the perhaps revisionist tendencies of Quine's more recent thought, but simply with the structure, power and philosophical background of the classic Quinean arguments.

1 What does the indeterminacy of translation involve?

It's a commonplace that expressions in one language may resist a fully satisfactory translation into another: that there is, for instance, no exact English equivalent, capturing all its existentialist overtones, of the French *ennui* (a kind of jaded detachment), or the special piquancy of the German *Schadenfreude* (a form of pleasurable excitement at another's misfortune). This commonplace has little to do with Quine's thesis. Quine's claim is not that exact translation is sometimes impossible, but that *there is no such thing as* exact translation: that for any expression, in any language, there will inevitably be a range of alternative translations of it into any particular language each of which, in conjunction with co-ordinating adjustments in the translation of other expressions, will equally well – and *unimprovably* – accommodate all the behavioural data concerning speakers' use of the translated language.³

Quine argues his case for this thesis in the context of *radical* translation. Radical translation is the translation of a hitherto wholly untranslated language, about whose syntax, semantics and etymology we are in a position to make no prior assumptions whatever. All we are assumed to have to go on is our observation of the use of the language. More specifically: we are assumed to be able to identify behaviour on the part of its native speakers which constitutes assent to and dissent from particular utterances in the language, and we are assumed to be able to observe the circumstances in which such assent or dissent takes place. We are also allowed to suppose that we are able to interact with native speakers in particular contexts, to put utterances to them in their own language for assent or dissent, and in general to encourage the production of evidential data for our translation, rather than merely passively observe. Quine's claim is that if a project of translation is undertaken under these circumstances, then there are bound to be intuitively incompatible claims about the meanings of (what we identify as) expressions in the natives' language such that, no matter how extensive the data which we proceed to gather, it will in principle never give us a reason to prefer one such claim to another.

Why, it may be wondered, the focus on radical translation? Why is the situation of someone engaged in so unusual a project, on so impoverished a basis of collateral information, of particular interest? Well, consider how it might be that radical translation was indeterminate, yet *non-radical* translation in certain cases – say the translation of some parts of French into English – was a fully determinate matter.

There's no doubt that in translating a French utterance into English, we will make all kinds of assumptions – about the accuracy of dictionaries, the context, the purposes of the speaker and so on – which effectively may uniquely determine the translation of a particular word in it. But what *justifies* these assumptions? Quine's thought is that their justification would ultimately have to come back to what could be appreciated from the vantage-point of the radical interpreter: that anything we – kindred post-Roman Europeans – can properly be said to *know* about French would have to be accessible, at least in principle, to a Martian radical translator of French – always provided the Martian was a good enough linguist and had enough time to gather the relevant observations. For Quine, any presuppositions we make when translating familiar languages into other familiar languages count as items of known fact – in contrast to, say, convention – only if they could in principle be justified by the methods of radical translation. So if radical translation is indeterminate, then all translation is indeterminate in the sense that the choice between alternative schemes of translation may be beyond justification by appeal to anything factual – anything which may be, properly speaking, known.

As remarked, Quine's view is that it follows from the indeterminacy of translation that meaning itself is indeterminate: if there are no facts of the matter about how an expression may be correctly translated into another language, then there are no facts of the matter about what it means. That may seem a natural enough transition. But it depends, of course, upon an additional assumption: that there can be no facts about meaning which are not accessible to a radical interpreter. And that would seem to involve presupposition of two further, potentially contentious theses:

- (1) That there is no first/third person asymmetry in the epistemology of understanding: that I can know nothing about what I mean by some particular expression unless you can know it too, by sufficient observation of my linguistic behaviour – although it seems clear that, in the typical case where I do know what I mean by an expression, I do not know it by observing my own behaviour.⁴
- (2) That whatever 'methodology' reconstructs a child's actual learning of a first language, the harvest of that methodology – the understanding of meanings in which pursuit of it results – cannot be *richer* than that of the methodology of radical interpretation. For if it were, there would be a way of knowing facts about meanings which radical interpretation couldn't emulate.

Point (1) may seem attractive, as being merely a version of the thesis that one's meanings must be, in principle, publicly available to others – the thesis of the essential manifestability of meaning which is widely accepted in contemporary philosophy of language (for further discussion, see Chapter 12, REALISM AND ITS OPPOSITIONS; Chapter 6, MEANING AND PRIVACY, and Chapter 7, TACIT KNOWLEDGE). But point (2) may seem less obviously agreeable: doesn't it simply overlook the consideration that the actual learning of a first language may deploy any number of unlearned dispositions – including 'grammatical' dispositions, if Chomsky is right, and also

what we might call 'similarity dispositions', that is, dispositions to find certain aspects of similarity in presented material salient and others not so – of which the methodology of radical translation, if it is characterized along the above austere lines, will take no account?

Each of these reservations would require extended discussion before a considered verdict could be reached on Quine's passage from indeterminacy of translation to irrealism about meaning. But for the purposes of what follows, we will assume that points (1) and (2) are each sound, and will not pursue such reservations further.

2 Could one live with the indeterminacy of translation?

Let us (provisionally – there are a number of distinctions to be drawn here, which we shall come to below) formulate the thesis as follows:

For any expression used in a given language, there are at least two incompatible hypotheses about its meaning which equally well – and *unimprovably* – explain all observable aspects of its use in that language.

Suppose this is accepted, and that we are content to conclude from it that, for any two such unimprovable hypotheses about an expression's meaning, there is no fact of the matter as to which of them is correct. Still, it wouldn't seem to follow that there are no facts about meaning at all. The undecidability of the choice between two unimprovable hypotheses is quite consistent with its being definitely *wrong* to choose any of a large number of incompatible alternative hypotheses; the unimprovable hypotheses may be definitely superior to the rest of the bunch, even if the choice between them is underdetermined. So we need to distinguish between weak and strong versions of the indeterminacy thesis:

Weak versions will contend that *some* questions about the meaning of an expression are indeterminate;

Strong versions of the thesis will contend that *all* questions about the meaning of an expression are indeterminate.

Even if we allow that indeterminacy of translation does indeed entail indeterminacy of meaning, it is clearly a strong thesis that is needed if we are to conclude that there are no facts whatever about meaning.

Now, the strong thesis certainly does seem disconcerting; but we should distinguish between better and worse reasons for finding it so. A bad reason would be the thought that all language becomes simply meaningless if we have to take it that meaning is everywhere indeterminate. That's just a confusion: *meaninglessness* is itself a specific–determinate–semantic condition. If there are no determinate facts about meaning, there are no determinate facts about meaninglessness either. But better reasons for disquiet are not far to seek. First, there is the fact that ordinary psychological states, like belief, desire, fear and so on, which feature so pervasively in our thought about ourselves and each other, are identified by their content: any

belief is the belief that *p*, for some *p*; any desire is the desire that *q*, for some *q*. If there are no facts about the meanings of linguistic expressions, the question immediately arises, how could there still be determinate facts about the content of such states? How could *psychological content* survive an argument which was generally destructive of *linguistic content*? But if it cannot, then it seems that the whole fabric of ordinary psychological explanation must collapse.

Second, if there are no facts about meaning, how can there be facts about *truth*? Our ordinary thinking ascribes truth and falsity to various things: to declarative sentences, to propositions and to beliefs. But the latter now come under the general shadow which, as just noted, Quine's thesis casts over the intentional-psychological; and one who regards meaning as strongly indeterminate is hardly likely to be well-disposed towards propositions – that is, *reified linguistic contents*. So it is declarative sentences, it seems, which will have to be the canonical bearers of truth-values in the Quinean scheme of things. But then there is the obvious difficulty that the truth-value of a sentence functionally depends both on the way the world is and on its *meaning*. If there are no determinate facts about meanings, it would appear to follow that truth-values are indeterminate as well.

As remarked, Quine himself has not shrunk from the scorn of intentional psychology to which his position appears to commit him.⁵ But the concern about the availability to him of ordinary notions of truth and falsity is quite another matter. If strong indeterminacy is sustained, the truth-value of a sentence – if the notion remains legitimate at all – will have to be determined by factors independent of meaning as traditionally understood. A programme of naturalized semantics might conceivably prove to be at Quine's service here (see Chapter 5, A GUIDE TO NATURALIZING SEMANTICS), though the prospects for such programmes seem anything but encouraging. This difficulty for Quine seems never to have been properly addressed.⁶

3 Quine's arguments for the indeterminacy thesis

Quine has presented two main and quite different styles of argument for the indeterminacy thesis. What has come to be known as the Argument from Below tries to illustrate the predicament of the radical translator by presenting actual concrete alternative translations of certain expressions between which, no matter what behavioural data might be accumulated, it will never be possible to choose rationally. The Argument from Above⁷ proceeds, by contrast, on a purely theoretical basis, from the thesis (henceforward the Underdetermination Thesis) that all empirical theory construction is in principle underdetermined by all available data. This second form of argument, in which Quine himself invests the greater confidence,⁸ will show, if successful, that translation, and thereby – so we are now allowing – meaning, must be indeterminate even if we lack the wit to construct in detail the sort of illustrations of indeterminacy of which the Argument from Below seeks to provide some examples. In this section we will review some of the twists and turns pursued by the development of the Argument from Below.

As emphasized, Quine is content to grant to the radical interpreter the ability to recognize native speakers' assent to and dissent from sentences formulated in their

language, and the ability to interact with them at least to the extent of eliciting assent to or dissent from particular such sentences. In consequence, the translator will be able – except in the most recalcitrant case – to arrive at empirically confirmed generalizations about which types of situation provoke assent to or dissent from instances of a particular sentence-type. Now Quine is content, in *Word and Object*, to resurrect a surrogate for the notion of synonymy which, in “Two dogmas of empiricism”, he so roundly rejected: two sentences are said to be *stimulus synonymous* just in case assent to and dissent from them is provoked by the same sensory circumstances. The central contention of the Argument from Below is accordingly that, no matter how ingenious, the translator will never be able to decide rationally between stimulus-synonyms just on the basis of observation of the native speakers’ linguistic behaviour. Yet intuitively, stimulus-synonyms may be very different in meaning; indeed, they may not even coincide in extension.

Suppose, to follow in the tracks of Quine’s famous example, that a rabbit hops past and the translator hears the natives say, “gavagai”. Subsequent investigation discloses that the natives are generally disposed to assent to “gavagai” when rabbits are visibly present, and to dissent from “gavagai” when there is no sign of rabbits. So the translator tentatively notes down the translation of “gavagai” as “Lo! a rabbit” or “There goes a rabbit”, or something of the sort. That may seem to be a well-grounded translation, but there are in fact a variety of alternatives which the translator would seem thereby to have overlooked. There are, that is, a number of concepts besides *rabbit* which, in exhibiting the observed patterns of assent and dissent, the natives could just possibly be exercising. Some of these are, respectively, the concepts of:

undetached rabbit part,
instantaneous temporal stage of a rabbit,
rabbithood (the universal),
rabbit-fusion (the scattered physical aggregate of all rabbits), and
rabbiting (taken as analogous to the feature-placing concepts, *thunder* and *rain*).

And the one-word utterance “gavagai”, could correspondingly mean any of:

There is an undetached rabbit part.
There is a temporal stage of a rabbit.
Rabbithood is instantiated over there.
There is a part of the rabbit-fusion.
It’s rabbiting.

The point Quine is making is not merely that the finiteness of the translator’s observations must leave open alternative interpretations. The contention is not merely that, however much data the interpreter gathers, there will be rival translational hypotheses which are consistent with it. It is stronger; namely, that there are *certain specific* translational hypotheses such that, however much data the interpreter gathers, each will remain in play if any does.

Now it’s clear – however peculiarly the various mooted translations of “gavagai” may strike us – that Quine’s point is correct so long as the data to be considered

concern nothing but the conditions which prompt assent to and dissent from the one-word sentence "gavagai". However, the thought immediately occurs that the situation is bound to change for the better as soon as we consider more complex sentential constructions in which "gavagai" features as a constituent. For example, suppose we are in a position to put the question, "How many gavagai are there over there?", or "Is that the same gavagai that we saw five minutes ago?" Then the correct answers are bound to vary according to whether or not "gavagai" means: *rabbit*, or: *undetached rabbit part*, or: *stage of a rabbit* respectively. For one rabbit is many undetached rabbit parts; and stages of a rabbit, unlike rabbits themselves, have no temporal duration.

Quine's reply to this⁹ is that our ability to run these tests will, of course, depend on our having independently translated certain constructions of the native language as meaning "how many" and "is the same . . . as". And, he contends, it is quite unclear how one might go about settling the translation of such expressions without *first* settling the interpretation of words like "gavagai" – that is, of sortal predicates – and without identifying the natives' numerals.

That gives pause. Isn't he right? Suppose, for instance, the natives use a word, "qua", which we have come to suspect may be used in concatenation with sortal predicates to ask "how many?" questions. How could we test this hypothesis unless we *already* knew the meanings of a range of such predicates with which it might be concatenated to ask such questions, and could tell whether the answers were as would be appropriate if "how many?" questions were indeed what "qua" was enabling us to put? Indeed there is a further, more specific difficulty. Suppose we have indeed somehow correctly identified the numerals in the natives' language, so that we can tell when the natives are telling us that there is one of a certain kind of object, or three, and so on. And suppose we have settled on the translation of the word, "qua", as it occurs in constructions like "qua gavagai", as meaning something like "how many?" And imagine that we put the question, "qua gavagai?", when a solitary rabbit is visible, and get an answer which we rightly take to mean "one". Even so, it is too quick to suppose that the translation of "gavagai" as *undetached rabbit part*, is thereby defeated. It *is* defeated, of course, if "qua" does indeed precisely mean: how many? But "qua gavagai" could, consistently with its eliciting the same answers as "how many rabbits?", mean not that but rather: *of how many rabbits are there undetached parts over there?* More generally, its role could be this: that if F means *undetached G-part*, then "qua F" means: *of how many Gs are there undetached parts there?* Under that hypothesis, what looked like a crucial experiment ceases to be so.

Quine's contention, in general, is this: if a pair of expressions have the same stimulus-meaning, then even if they intuitively differ in meaning in ways that would impinge differentially on the use of more complex contexts in which they occur, there will always be a *compensating adjustment* to the interpretation of the surrounding context of such a kind that, under the adjustment, the uses once again coincide. More formally: if F and G have the same stimulus-meaning, but differ in intuitive meaning – like "rabbit" and "undetached rabbit part", for instance – in such a way that, with respect to a particular embedding context, " Φ . . .", the

patterns of assent to and dissent from " ΦF " and " ΦG " could be expected to differ, there will always be an adjusted interpretation of " $\Phi . . .$ " such that the assent/dissent conditions of " ΦG " under the adjustment will coincide with those of " ΦF " when unadjusted.¹⁰

4 Evans's appraisal of the argument from below

This line of thought, as it stands, is arresting, but hardly sufficiently developed to count as cogent. It deserves thinking through in detail, yet there are few attempts to do so in the secondary literature. However, a distinguished exception is provided by Gareth Evans's paper, "Identity and Predication" (1975). Evans contends that Quine looks in the wrong place for considerations that might prove the superiority of the translation of "gavagai" as *rabbit*. Quine's consideration of contexts in which "gavagai" might occur embedded is restricted to what he calls the "apparatus of individuation" – constructions involving identity, plurals and the numerals. He allows that if we somehow fix on a translation of certain of the natives' expressions within this apparatus, then it will be possible to construct contexts which will discriminate, in principle, among the stimulus-synonyms of "rabbit". His point, then, is that the translation of native expressions into elements of the apparatus of individuation presents a problem which is *co-ordinate with* that of the translation of "gavagai", and that it is in principle impossible to motivate the identification of certain native devices as expressing plurality, identity, and so on, without first fixing the translation of terms like "gavagai". Evans's counter is that we do not actually need to consider the apparatus of individuation at all; rather it can suffice to consider how predicates may be used *in combination*, and how they behave under negation.

Here is an illustration of the sort of thing Evans has in mind. Suppose we have identified two other words in the native language, "odolby", and "thewi", which we observe to be associated with the following patterns of assent in the native speech community:

There is assent to the one-word sentence, "odolby", just when something blood-stained is visible.

There is assent to the one-word sentence, "thewi", just when something white is visible.

In addition, suppose we have observed the use of a particle, "neg", which seems to act as an operator of negation; that is, we observe:

There is assent to "neg gavagai" just when no rabbit is salient;

There is assent to "neg thewi" just when nothing white is salient;

and so on. And now suppose we also observe the following more complex patterns of linguistic behaviour:

Situations which prompt assent to "gavagai" and "thewi" do not always prompt assent to the conjoined construction, "thewi gavagai". The latter is assented to only when a white rabbit is salient. "Thewi" and "gavagai" will, however, be assented to individually when a brown rabbit sits on the snow.

It is similar with the conjoined constructions, "odolby gavagai" and "odolby thewi" – they are assented to only when a bloodstained rabbit is salient, or when something is salient which is both white and bloodstained.¹¹

Likewise, the natives are disposed to assent to "thewi gavagai" and "odolby gavagai" when two rabbits are in view, one white, the other bloodstained; but they will assent to "odolby thewi gavagai" only when one and the same rabbit is both white and bloodstained.

Evans's thought is that observations of this character would suffice to eliminate some of the stimulus-synonyms of "rabbit" as adequate translations of "gavagai". For instance, if "gavagai" really were just a device for reporting an environmental feature – like "it is raining" – and "thewi" were the same, then it would seem to be impossible to interpret the conjoined construction, "thewi gavagai", as anything other than the conjunction of the ingredient claims: it is whitening and it is rabbiting (compare: it's windy and it's raining). And that translation cannot account for the fact that "thewi gavagai" is not assented to unless the "whiting" is restricted to the surface of a *rabbit*.

The translation of "gavagai" as *undetached rabbit part* also seems to be in difficulties under the hypothesized data. What are we to say that "thewi" means in that case? If it just means *white*, then "thewi gavagai" ought to be assented to whenever a rabbit is salient with an undetached white part – say, a white foot. But that isn't what happens. If the one-word sentence "thewi" means *undetached part of a white thing*, on the other hand, then again "thewi gavagai" ought still to be assented to when the white-footed rabbit presents himself, since his toes are undetached parts of a white thing, namely his foot. So that translation fares no better. We might surmount that difficulty by allowing "thewi" to mean *undetached part of a white rabbit*; but then we'd be at a loss to understand the natives' assent to it as they gaze at a snowy but rabbit-free landscape.

"Undetached rabbit part" and "temporal stage of a rabbit" are, like "rabbit", and unlike the feature-placer, "rabbiting", sortal predicates. By contrast, the other items on Quine's list of stimulus-synonyms for "rabbit", that is, "rabbithood" and "rabbit-fusion", are singular terms, standing respectively for an abstract and for a scattered concrete object. It is these interpretations of "gavagai" which, Evans contends, are put in difficulty by the kind of data which he envisages for the natives' particle of negation. Suppose our observations disclose that the assent conditions of compound sentences vary depending on the position within them of "neg"; for instance

"neg thewi gavagai" is assented to whenever no white rabbit is in view, including the case when no rabbit of any kind is in view, whereas "thewi neg gavagai" is assented to only in the presence of rabbits of other colours.

These facts are nicely explained if we suppose that “thewi” means white. “gavagai” means rabbit and “neg” functions as a device of sentential negation when it takes initial position, and as a device of predicate negation when it immediately succeeds a predicate.¹² But how are the data to be accommodated on the assumption that “gavagai” is, for example, a singular term standing for the universal, rabbithood? On that assumption, the assent conditions of “thewi gavagai” suggest that “thewi” is a predicate of universals roughly equivalent in meaning to “has a white instance here”. But in that case we seem to have no way of generating a sentence with the assent conditions hypothesized for “thewi neg gavagai”. For the negation particle has nothing smaller to operate on, so to speak, than an atomic predicate of the natives’ language; and when it is so restricted, to suppose that it occurs as a predicate negation in that sentence is to predict, falsely, that the sentence should have the assent conditions of “rabbithood does not have a white instance here” – something which should be assented to when there is no rabbit to be seen.

An analogous problem would presumably confront the translation of “gavagai” as *rabbit-fusion*, could we first but find a workable construal in this case of “thewi” as it occurs in sentences like “thewi gavagai”. But in fact, as Evans points out, this translation of “gavagai” also inherits the problems associated with the translation *undetached rabbit part*. If “thewi” is a predicate of concrete but spatio-temporally scattered entities, what hypothesis about its meaning will get the assent conditions of “thewi gavagai” right? Not “. . . has a white part here” – because brown rabbits with white tails don’t provoke assent to it – nor even “. . . has a white, rabbit-shaped part”, because that would leave us bereft of any explanation of the natives’ assent to “thewi todagai” in the presence of an Arctic fox.

So far so good, it may seem. Different considerations come into play when Evans comes to the proposed translation *temporal stage of a rabbit*. The sort of difficulty he finds for this proposal¹³ is not posed by envisaged data of the foregoing kinds, but has to do with the interpretation of what, in our preferred translation scheme involving rabbits, whiteness and the rest – henceforward the *favoured scheme* – we will naturally take to be simple *tensed* assertions. Suppose, for instance, that the suffix “-p” is naturally taken, in that scheme, as an indicator that a predication is past-tensed.¹⁴ The question Evans raises is how such data is to be accommodated by a translation scheme for the natives’ language which treats the predicates in question as predicates of temporal stages.

Evans’s (rather terse) discussion here is semi-technical. He envisages an interpreter who is working within something like the framework of a Tarski-Davidsonian recursive theory of meaning (see Chapter 1, MEANING AND TRUTH CONDITIONS: FROM FREGE’S GRAND DESIGN TO DAVIDSON’S) and who first lays down basic clauses which stipulate satisfaction-conditions for *tenseless counterparts* of the natives’ predicates; for instance

(x, t) satisfy “odolby” (tenseless)¹⁵ $\leftrightarrow (\exists y)$ (y is bloodstained at t & x is a stage of y)

– a pair consisting of a temporal stage, x , and a time, t , satisfy “odolby” if and only if that stage is a stage of something which is bloodstained at that time, and then

stipulates satisfaction-conditions for stages and tensed versions of those predicates in terms of these basic clauses; for instance, for the simple present tense (where t_u is the envisaged time of utterance)

x satisfies "odolby" (present tensed) $\leftrightarrow \langle x, t_u \rangle$ satisfy "odolby" (tenseless)

– a stage satisfies (present tensed) "odolby" only if the pair consisting of that stage and the time of utterance satisfies (tenseless) "odolby"; and for the simple past tense

x satisfies "odolbyp" $\leftrightarrow (\exists t')$ (Before t_u , t' & $\langle x, t' \rangle$ satisfy "odolby" (tenseless))

– a stage satisfies "odolbyp" if and only if there is a time earlier than the envisaged time of utterance such that the pair consisting of that stage and that time satisfies (tenseless) "odolby".

Now, an evident effect of the proposed base clause is that *every* stage, x , in the life of a rabbit which is bloodstained at t will be such that $\langle x, t \rangle$ will satisfy (tenseless) "odolby". So any treatment of the tenses along these lines will have the consequence, as Evans observes, that if any temporal stage of a given rabbit satisfies "odolbyp", or (present-tensed) "odolby", then *every* temporal stage of the same rabbit, *no matter when occurring*, will satisfy "odolbyp", or "odolby" – and indeed will satisfy any other tense of the same predicate, if introduced via a clause along the same lines. Evans evidently regards this kind of promiscuity as a decisive difficulty, for he immediately moves to consider a proposal fashioned to avoid it. But he does not say why. We will return to the matter.

The second proposal Evans considers avoids the promiscuity by an obvious modification in the form of the base clauses; thus

x satisfies "odolby" (tenseless) $\leftrightarrow (\exists y)(\exists t)(y$ is bloodstained at t & x is a stage of y & x occurs at t)

– a stage satisfies (tenseless) "odolby" if and only if it is a stage of something *which is bloodstained at the time at which that stage occurs*. Clauses for the simple present and past tenses may then proceed:

x satisfies "odolby" (present tensed) $\leftrightarrow x$ satisfies "odolby" (tenseless) & x occurs at t_u

– a stage satisfies (present-tensed) "odolby" if it occurs at the time of utterance and is a stage of something that is bloodstained at the time that stage occurs; and

x satisfies "odolbyp" $\leftrightarrow (\exists z)(\exists t')(($ Before t_u , t') & (z occurs at t') & (x occurs later than t') & (x and z are stages of the same thing) & (z satisfies "odolby" (tenseless)))

– a stage satisfies "odolbyp" if it is a later stage of something one of whose earlier stages, occurring before the time of utterance, is a stage of something bloodstained at the time it occurs.

However, Evans foresees a new difficulty for this scheme. According to the proposed clauses, "odolbyp gavagai" should be true just of stages of a rabbit occurring later than a stage in its life when it was bloodstained. What if there no longer *are* any such stages? What if, rather than clean up the bloodstained rabbit, we had destroyed it? In that case, the proposed clause will predict that the natives will no longer assent to "odolbyp gavagai", for there are now no stages to meet the specified condition. But it is easy to imagine how, consistently with the other data envisaged, they might nevertheless give their assent. We have only to imagine that their assent conditions for "odolbyp gavagai" coincide with those of the English sentence, "a rabbit was bloodstained". Evidence of that coincidence, it might seem, would then be powerful evidence for the favoured scheme as against the temporal stage-scheme.

5 Are Evans's objections compelling?

Seemingly the least cogent part of Evans's discussion is his treatment, just reviewed, of the temporal stage-scheme. Even if the detail of his objections were wholly convincing, there would have to be a vague worry whether the problems thereby disclosed for the stage-theorist were not artefacts of avoidable features of the mooted Tarski–Davidson style of semantic-theoretical treatment of tense. But the detail does not seem convincing in any case. The last consideration, that the proposed clauses cannot recover the assertibility of "odolbyp gavagai" in circumstances when no stage of the rabbit in question post-dates its (last) bloodstained stage,¹⁶ seems crucially to overlook an ambiguity in the hypothesized English (stimulus-)equivalent, "a rabbit was bloodstained". The English sentence can, indeed, be read as embedding a past-tensed predication – when it is taken as the existential generalization, for example, of "that rabbit over there was bloodstained", so that the tensing is done, as it were, within the scope of the quantifier. But the reading germane to Evans's possibility, when "a rabbit was bloodstained" is asserted of a now defunct rabbit, reverses the scope-priority: the past tense is now the principal operator in the sentence, and the quantifier occurs within its scope, so that the effect of the claim is rather that *it was the case that*: [a rabbit is bloodstained]. Any semantic treatment of tense which treats the tenses as operators – on tenseless sentences, in the kind of treatment Evans has in mind, but there are other possibilities – has to be open to all the usual possibilities for ambiguity in the scope of such operators. In particular, we have to expect wide and narrow scope possibilities broadly analogous to those presented by negation. The mooted clause for "odolbyp" is a proposal for a *past-tensed predicate*, where the tense operator is given narrow scope. Evans's objection to it, by contrast, is – irrelevantly – that it does not enable us to recover the (apparent) truth-conditions of predications of "odolbyp" in which it is not merely the predicate but the *whole sentence* that falls within the scope of the past tense. The objection is irrelevant because – their overt form notwithstanding – such sentences should no more be construed as containing the kind of use of "odolbyp" which the proposed clause concerns than "it is not the case that a rabbit is white" should be construed as containing an occurrence of "is not white".

The favoured scheme, too will have to cope with this kind of ambiguity; however it does so, there is no reason to think that the stage-theorist will not have exactly analogous resources.

In any case, what exactly was the problem that moved Evans to dismiss the first form of proposal? Why should the kind of promiscuity imposed by the originally mooted clauses be held to be objectionable? The point might seem obvious. Consider the blood-soaked rabbit of note 14 who gets a thorough cleaning. Suppose the natives assent to "odolby gavagai" before the washing, *but not afterwards*. How, if they are talking about temporal stages of the animal, and if "odolby" has the satisfaction-conditions outlined, is this to be explained? If any temporal stage of the rabbit satisfies the utterance of "odolby" before the washing, then all do, no matter when they occur or when the question is raised. So why – if they are talking about properties of temporal stages – do the natives, having earlier assented to "odolby gavagai", not do so later? To be sure, the reference of "gavagai", will then be presumably to stages of the rabbit which post-date its bloodstained period. But, on the treatment proposed, that should make no difference. Since the natives cannot plausibly be taken to have forgotten that the rabbit was bloodstained, or not to have noticed, their temporally selective assent patterns – the very things that motivate viewing them as deploying tenses in the first place – are seemingly at odds with the ascription to them of an ontology of stages and the proposed semantic clauses for "odolby".

This train of thought, however, confuses the form of promiscuity actually entailed by the original type of clauses – promiscuity over stages, as it were – with a form of promiscuity over times. The key point is that those clauses have to be read as dealing with the satisfaction-conditions of actual or envisaged *token utterances* of the tensed predicates they concern; that is the effect of their reference to a time of utterance, t_n .¹⁷ The generalization they actually entail is that if a particular (actual or envisaged) historic token of (present-tensed) "odolby", or "odolbyp", is satisfied by a particular stage in the life of a rabbit, then it is satisfied by all earlier and later stages in the life of that rabbit. That is not to be confused with anything which generalizes from a particular historic token predicate's satisfaction to the satisfaction of other tokens of the same type uttered at *other times*. Nothing of that kind follows from the clauses in question when properly construed. In particular, they entail nothing about whether if a particular token of "odolby" is satisfied by a present stage of a rabbit, then that or later stages of the same rabbit should be regarded as satisfying a *later tokening* of "odolby". So there is nothing in the clauses to jar with the natives' hypothesized unwillingness to apply "odolby" to what they know to be a formerly, but no longer, bloodstained rabbit.¹⁸

I am not suggesting that Evans himself was guilty of this confusion, only that one who did fall into it might find a spurious plausibility in Evans's brisk dismissal of that particular approach on behalf of the stage-scheme – and that I am not sure what else he may have had in mind.

In any case, it must be reckoned as doubtful how forceful are Evans's objections to the temporal stage-scheme. The matter would assume some importance as far as the Argument from Below is concerned if Evans had indeed disposed of

Quine's other mooted translation schemes, since the temporal stage-scheme would then represent the argument's last chance, at least as far as Quine himself develops it. But at least one recent commentator has questioned quite generally whether Evans's considerations really are successful.¹⁹ A closer review of the matter will turn out to render further discussion of the temporal stage-scheme unnecessary.

Consider again the data envisaged in order to make trouble for the other schemes. For instance, a brown rabbit with a white foot provokes assent to "thewi" and "gavagai" separately but not to the compound, "thewi gavagai". Evans challenges Quine to find an interpretation of "thewi", within the framework of an ontology of undetached parts of things, which explains this. Neither "... is white" nor "... is part of a white thing" will do; "... is part of a white rabbit" – or, more generally, "... is part of a white animal" – would explain why the compound sentence doesn't get assented to in the circumstances described; but it would leave us bereft of any explanation why "thewi" gets assented to on its own in the context in question.

There is an obvious counter. Evans is assuming that we have to find *some one general account* of the meaning of "thewi" to account for both simple and compound occurrences of it. But why shouldn't its syntactic/semantic role be *context-sensitive*? It could, for instance, mean *white*, when occurring in one-word sentences, but *undetached part of a white F*, when occurring in immediate concatenation with the word whose correct translation when not so concatenated is *undetached F-part* (and whose role, when conjoined with "thewi", accordingly reduces to that of fixing the parameter, F). Context-sensitive variation in meaning, contrasting with simple ambiguity in so far as the meanings in question are variously cognate to each other, is a familiar phenomenon in natural languages: think, for instance, of the expression "fix" as it occurs in "I'll fix lunch", "he fixed the puncture" and "they fixed the race." Why should not phenomena of that general sort be found in the natives' language too? In effect what would be postulated would be a theory according to which "thewi" had a kind of ambiguity, albeit one in which there was a close relation between its diverse meanings, and where precisely which meaning it took would be determined by its syntactic mode of occurrence. But that doesn't seem outlandish really.

It is similar with the data concerning the use of "neg", represented by Evans as scotching the interpretations of "gavagai" as a singular term standing respectively for rabbithood and the rabbit-fusion. The problem was to find an interpretation of "thewi" which, when "neg" can occur both as external and as internal negation, would rationalize a native's assent to "thewi" when a white rabbit is salient, but to "thewi neg gavagai", when, say, brown rabbits are salient, and to neither when no rabbit is salient. The interpretation "... has a white instance here" captures the first datum; but if "neg" is a device of internal negation, it mis-predicts the assent conditions of "thewi neg gavagai". But again, the obvious rejoinder is that we are not constrained to take "neg", occurring as illustrated, as a device of internal negation in the first place. There could be an operator which, in prenex position, functions as sentential negation, but when it occurs as a predicate-suffix, serves

not to negate the predicate – to generate its complement – but operates rather *within* that predicate's content. In particular, under the translation scheme which treats "gavagai" as a singular term standing for a universal, the role of "neg" occurring as suffix may be taken as one of generating the complement of the *ad-verb* – the mode of instantiation – which we interpret predicates like "thewi" and "odolby" as ascribing. So "thewi neg gavagai" is interpreted as saying not that rabbithood doesn't have a white instance here, but that it has a non-white instance here (as it were, *is instantiated non-whitely here*) – precisely what is wanted to save the data described.

How are we to assess the resulting dialectical situation? It is difficult to see one's way clear to the conclusion that *any* pool of data which a sympathizer with Evans might construct, and which would be *prima facie* recalcitrant for Quine's alternatives to the favoured scheme, could be handled in the quite simple kinds of way illustrated. But what is surely convincing in advance is that the most that the sympathizer with Evans is going to be able to do is to call attention to possible observational data which wouldn't square with *particular* proposed interpretations of some of the expressions concerned; and that it must always be possible in principle to handle such data if one is willing to assign a *variety* of syntactic roles, and/or semantic ambiguities, to the expressions in question. Does this reflection suffice to show that Evans embarked on a lost cause?

It does not. Consider this case. Suppose that alternative schemes along Quinean lines can indeed be constructed which can survive any envisageable addition to our pool of linguistic data, but that whereas the Quinean schemes survive by the postulation of ambiguities of various kinds, the favoured scheme has, by and large, no need for such recourse. Then the latter would be, in a clear sense, *simpler* than the Quinean alternatives. Now, the point is well taken that simplicity cannot be assumed, without further ado, to be an *alethic* – truth-conducive – virtue in empirical theory generally. There is *prima facie* sense in the idea that of two empirically adequate theories, it might be the more complex that is actually faithful to the reality which each seeks to circumscribe. But the thought that, when it comes to radical interpretation, there is an ulterior psychologico-semantic reality which an empirically adequate translation scheme might somehow misrepresent is, of course, exactly what Quine rejects – exactly what he famously stigmatizes as the myth of the semantic museum.²⁰ And with that rejection in place, methodological virtues which are not, in realistically conceived theorizing, straightforwardly *alethic* can now become so. In such cases, the methodologically best theory ought to be reckoned true just on that account. It is therefore not enough for a defender of Quine to seek to save the alternative schemes by postulations which, though still principled and general, are comparatively expensive in terms of ambiguity and other forms of complication. If a simpler scheme is available, that fact is enough to determine that these alternatives are *untrue*, by the lights of the only notion of truth that, in Quine's own view, can engage the translational enterprise.

It's another question whether the particular moves I envisaged fall foul of this point. The alternative interpretation of "neg" just canvassed, for instance, postulates a syntactic ambiguity only where the favoured, internal/external negation-

distinguishing interpretation *already* does so, albeit a different ambiguity. And the interpretation of "thewi" – *undetailed part of a white F* – to which we had the undetailed-parts theorist resort in the attempt to accommodate Evans's assent-data for "thewi gavagai" might actually serve well enough to accommodate the natives' assent patterns to the one-word sentence "thewi" as well ("undetailed part of a white something").

That, however, brings us up against a second and this time, I think, decisive consideration, at least if we may take it that the basic clauses of our semantic theory are to assign reference and satisfaction-conditions in ways which are *presumed to correspond to the conceptual repertoire of speakers of the language in question*. For even if the schemes considered turn out not to enjoin any avoidable degree of complication in comparison with the favoured scheme, the fact is that the range of concepts necessary in order to formulate their various clauses in each case includes, but is not included in, the simple range of concepts of observable spatio-temporal continuants and their observable properties which the favoured scheme deploys. So much is obvious in the case of the schemes deploying the concept of the universal rabbithood, and the rabbit-fusion: these are ideas which you do not grasp until you know respectively what qualifies something to be an instance of the universal, and what qualifies it to be a basic part of the fusion. The same point emerges in the clause to which we had the undetailed-parts theorist resort for "thewi", and indeed in the various clauses we considered that the temporal-stage theorist might propose.²¹

It is simplicity not in *semantic* theory but in the associated *psychological* theory that is at stake here. Let it be unresolved whether Quine's alternative schemes must issue in semantically more complex theories; it is certain none the less that their implied accompaniment must be additional psychological complexity. The effect is that their situation is therefore doubly unhappy. Not merely do they involve the ascription of superfluous conceptual resources to speakers – resources strictly unnecessary to explain their linguistic performance – but, worse, we have to regard the resources in question as lurking behind, but *inexpressible* in, the actual vocabulary of the natives' language. To have the concept of an undetailed rabbit part, you need a concept of the integrated individual of which such parts are parts; to have the concept of a temporal stage of a rabbit, you need to grasp the idea of the spatio-temporal continuant of which such a stage is a stage. Yet the Quinean translation schemes will represent you as talking only of undetailed parts, or temporal stages; reference to the *integrated, spatio-temporally persisting rabbit* will elude you so long as your expressive resources are fully captured by these translation schemes.

Such schemes, then, even if they can indeed cope with all the data which a sympathizer with Evans might imagine, and even if they can do so without losing out by canons of simplicity governing the construction of *semantic* theory, must, it seems, fall foul of a basic methodological consideration: that the conceptual repertoire which radical interpretation may permissibly ascribe to speakers should exceed what is actually expressible in their language, as so interpreted, only if its ascription to them is necessary in other ways in order to account for their linguistic

competence. Perhaps an Argument from Below could be developed in such a way as to respect this constraint. But Quine's own examples do nothing to suggest how.²²

6 The Argument from Above: preliminary clarifications

The Argument from Below operates at the level of sub-sentential expressions. Quine sometimes represents this point by the claim that the conclusion of the argument is not the indeterminacy of translation, properly understood, but rather the *inscrutability of terms*. What a proponent of the argument tries to do in the case of "gavagai", for example, is, as we have seen, to propose hypotheses about its syntactic category and reference in such a way that the truth-conditions – and hence assent-conditions – of contexts containing it are left invariant under compensating readjustments in the interpretation of the other expressions which they contain. Even if this is done successfully, the conclusion will still be consistent, therefore, with determinacy in the matter of what truth-conditions a radical interpreter is to assign to natives' utterances. True, it will be left indeterminate exactly what *thoughts* – individuated more finely than merely by their truth-conditions – should be regarded as expressed by particular native utterances. But the slack will extend no further than the existence of some room for manoeuvre within assignments whose truth-conditions are the same. It is doubtless for this reason, rather than to acknowledge any infirmity in the Argument from Below, that Quine writes:

My *gavagai* example has figured too centrally in discussions with the indeterminacy of translation. Readers see the example as the ground of a doctrine, and hope by resolving the example to cast doubt on the doctrine. The real ground of the doctrine is very different, broader and deeper.²³

Quine thus seems content to have most – perhaps all – of his eggs in the other basket.²⁴ And the contention of the Argument from Above is indeed stronger. It is precisely that unimprovable translation manuals may differ not merely in their interpretations of sub-sentential expressions, but in the truth-conditions they assign to sentences, and hence in which of the natives' utterances they will enjoin us – in conjunction with our own collateral beliefs about the world – to regard as true.

Here, though, there are a variety of possible theses of differing strength, which we shall do well to distinguish. Let 'M' range over unimprovable translation manuals – by whatever criteria – for the natives' language, and let 'S' denote a particular sentence of that language; let 'C' range over claims which identify the truth-conditions of sentences, and 'c' range over claims which, while falling short of identification, somehow constrain the identification of sentences' truth-conditions, for example, by saying what their truth-conditions are not. Let's say that S's meaning is:

- (1) *strongly determinate* if and only if there is some C such every M makes C about S

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- (2) *weakly determinate* if and only if there is some *c* such that every *M* makes *c* about *S*
- (3) *weakly indeterminate* if and only if there is no *C* such that every *M* makes *C* about *S*
- (4) *strongly indeterminate* if and only if there is no *c* such that every *M* makes *c* about *S*

Then any particular sentence *S* must be in one of three cases:

- (A) strongly determinate
- (B) weakly determinate and weakly indeterminate
- (C) strongly indeterminate

and there are accordingly *seven* possibilities for the sentences, *S*, of any particular language:

- (1) Every *S* is in case A.
- (2) Some *S* are in case A, some *S* are in case B, and every *S* is in one of those two cases.
- (3) Some *S* are in case A, some *S* are in case B, and some *S* are in case C.
- (4) Some *S* are in case A, and some *S* are in case C, and every *S* is in one of those two cases.
- (5) Every *S* is in case B.
- (6) Some *S* are in case B and some *S* are in case C, and every *S* are in one of those cases.
- (7) Every *S* is in case C.

Each of (2) through to (7) represents a possible indeterminacy thesis, of an increasingly radical order.

Now, if a "fact about meaning" is to be anything agreed on by all unimprovable manuals, then it is only (7) which entails that there are no facts about meaning whatever. This is important. I mentioned at the start the underlying physicalist spirit which drives Quine's argument, the perceived difficulty in finding anything for semantical properties to be in what is conceived of an essentially physical world. The thesis of indeterminacy of translation is meant to assuage this concern precisely by showing that semantic facts are *superstition*, and are therefore owed no refuge in the austere ontology of developed physical science. This radical solution will be frustrated should it turn out that Quine's arguments at best support something less thoroughgoing than a thesis of form (7). For in any other situation, there will be residual "facts about meaning"; maybe nothing like the rich variety of such facts that an opponent of Quine would intuitively wish to recognize, but facts about meaning all the same. So we should have the worst of both worlds: insufficient semantic facts to do justice to our intuitions of distinctions of meaning, but enough to set up the perceived difficulty for Quine's physicalism.²⁵

The taxonomy invites review of a number of issues. First, on determinacy of truth-value. It might be supposed that, whatever form of indeterminacy thesis is maintained, the effect will be to introduce a species of relativity: the truth-value of

a sentence whose meaning is absolutely indeterminate will have to be thought of as likewise non-absolute, as relative to whatever the (unimprovable) manual we happen to favour assigns to it as its truth-condition. But actually there is cause for doubt about the stability of this line of thought. For the truth-value of S can be conceived as determinate relative to some particular manual only if it is thought to be determinate *what that manual has to say* about the meaning of S. And the contents of claims in translation manuals ought, by the Quinean, to be regarded as no more determinate than any others.

If relativism is accordingly eschewed, then it seems it must be conceded that any sentence in category C – any strongly indeterminate sentence – must simply be indeterminate in truth-value, at least so long as we continue to conceive of truth-value as a function of truth-conditions. But how about sentences in category B – weakly determinate but weakly indeterminate sentences? Unimprovable manuals will not converge on the assignment of any particular truth-condition to such a sentence. But they will converge in making certain claims which constrain its meaning, in particular claims which rule out certain such assignments. So the possibility is open, at least while the discussion moves at this level of generality, that such a sentence might yet be determinate in truth-value, since it might be that the space of permissible assignments of truth-conditions is sufficiently narrow to ensure that, as matters happen to stand, the sentence will be true no matter which of those assignments is made. It would all depend how far the weak determinacy extended; how many and what strength of meaning-constraining claims about S the unimprovable manuals would converge upon.

A second issue concerns the respective implications of the various possible strengths of indeterminacy thesis for the viability of ordinary intentional psychology. Assume that it is by the interpretation of what they are prepared to assent to that we are to identify by far the greater proportion of the natives' beliefs. Sentences in category C are obviously useless for this purpose. But might at least some measure of propositional-attitude psychology be feasible if our data concerns acceptances and rejections of sentences in category B? In that event we should know insufficient to determine the truth-conditions of the beliefs thereby evinced. But it is not inconceivable that the range of permissible assignments and truth-conditions on which the best manuals converged might be sufficiently restricted to ensure that, at least in certain special circumstances, the only beliefs which we could regard a particular utterance as expressing would all equally well serve the purpose of rationalizing an associated item of behaviour when conjoined with certain plausibly ascribed desires. (For instance, the belief that a certain fruit was nutritious and the belief that it was merely tasty might rationalize many of the same behavioural episodes.) Again, it will all depend on how weak is the weak determinacy involved.

However, I shall not pursue these matters further. The crucial question is: which of the various possibilities, from (2) to (7), are in the range of Quine's Argument from Above? Here is his own classic statement of the argument:

Now my point about physical theory is that physical theory is underdetermined even by all . . . possible observations. . . . Physical theories can be at odds with each other

and yet compatible with all possible data even in the broadest sense. In a word they can be logically incompatible and empirically equivalent. This is a point on which I expect wide agreement, if only because the observational criteria of theoretical terms are commonly so flexible and fragmentary. People who agree on this general point need not agree as to how much physical theory is empirically unfixed in this strong sense; some will acknowledge such slack only in the highest and most speculative reaches of physical theory, while others see it as extending even to commonsense traits of macroscopic bodies.

Now let's turn to the radical translation of a radically foreign physicist's theory. As always in radical translation, the starting point is the equating of observation sentences of the two languages by an inductive equating of stimulus meanings. In order afterward to construe the foreigner's theoretical sentences we have to project analytical hypotheses, whose ultimate justification is substantially just that the implied observation sentences match up. But now the same old empirical slack, the old indeterminacy between physical theories, recurs in second intension. Insofar as the truth of physical theory is underdetermined by observables, the translation of the foreigner's physical theory is underdetermined by translation of his observation sentences. If our physical theory can vary though all possible observations be fixed, then our translation of his physical theory can vary though our translations of all possible observation reports on his part be fixed. Our translation of his observation sentences no more fixes our translation of his physical theory than our own possible observations fix our own physical theory.

The indeterminacy of translation is not just an instance of the empirically underdetermined character of physics. The point is not just that linguistics, being a part of behavioural science and hence ultimately of physics, shares the empirically underdetermined character of physics. On the contrary, the indeterminacy of translation is additional. Where physical theories A and B are both compatible with all possible data, we might adopt A for ourselves and still remain free to translate the foreigner either as believing A or as believing B.²⁶

What exactly is the structure of this reasoning? Its premise is clearly indicated. It is the Underdetermination Thesis: the thesis, roughly, that all possible observations – all the observations that scientific observers, however idealized, wherever and whenever situated, might gather between them – do not constrain the selection of an explanatory empirical theory to within uniqueness. Alternative, incompatible theoretical accounts are always possible of any data pool, even if of infinite extent. This is the point on which Quine expects “wide agreement”, although he earlier envisages possible disagreement about the level at which Underdetermination operates, some accepting that it holds only for the highest reaches of empirical theory, while others possibly allowing that it go for all empirical theorizing, *tout court*. But how exactly is the transition supposed to be effected to the conclusion: the indeterminacy of translation, and of meaning?

Quine, as the reader will have noted, explicitly disavows that it is simply a matter of applying the Underdetermination Thesis to the special case of empirical linguistics: the indeterminacy is to be “additional”. But it is a good question why or whether it would matter much if the argument were indeed that direct. No doubt the direct argument would have to confront a very obvious question: why is its legitimate conclusion not merely that theories of meaning are, like all empirical

theories, *underdetermined* by the behavioural data? Why the additionally strong conclusion concerning *indeterminacy*? Quine shows no inclination to draw the conclusion that empirical theory as a whole is indeterminate. So what, for a proponent of the direct route, would distinguish empirical theories of *meaning*, making the indeterministic conclusion appropriate in their case? It may be doubted that such a philosopher could have any better answer than to charge that to hold to the opposed view – that meanings may, in cases of indeterminacy of translation, simply lie beyond the reach of empirical detection – is to succumb to the myth of the museum.²⁷ It is to succumb, that is, to the illusion that, in a world apt for complete description by physical theory, there can possibly be states of affairs apt to confer truth and falsity on claims about meaning other than those constituted in behavioural propensities of language use which, by hypothesis, underdetermine the selection of semantic theory. But the salient point is, then, that this – the repudiation of the myth of the museum – is a point on which Quine is going to have to rely *in any case*, even if the argument follows a subtler path than the one disavowed. The immediate conclusion, whatever exactly the configuration of the subtler route to it, is still only going to be that respect for all possible data will leave the translation of the natives' utterances underdetermined. One wonders, then, what exactly the additional subtlety, whatever exactly it may prove to consist in, really has to contribute. Does it somehow make for a stronger conclusion, a more pervasive or deeper kind of indeterminacy? Or does Quine see some difficulty for the simple, direct argument which the subtler route can finesse? What does "additional" mean?

Whatever the answers to those questions, it's notable that the scope of the argument can in any case extend no further than that of the Underdetermination Thesis which fuels it. Someone who allows, for example, that only very high-level physical theory is subject to underdetermination will be under no pressure to concede indeterminacy of translation except for vocabulary which occurs exclusively in such theory.²⁸ And even for sentences containing such vocabulary, it will be *weak* indeterminacy, not strong, that will be suggested. For however exactly the argument is supposed to run, just as not any old interpretation of that vocabulary would result in a theory which was adequate to the relevant data, so not any old interpretation results in a translation which may justifiably be regarded as reflecting the putatively perfectly rational native scientists' beliefs. The translation of theoretical terms in the native scientists' language can be no more indeterminate than is the selection of an empirically adequate theory of those data.

Strikingly, therefore, Quine's argument promises at best the *mildest* kind of indeterminacy thesis, one of type (2) in the above taxonomy, according to which a thesis of *weak* determinacy/indeterminacy is made out merely for *some* statements. And indeed, even if the Underdetermination Thesis is extended to all empirical theorizing, the most that is in prospect is a thesis of the indeterminacy of theoretical vocabulary relative to some fixed translation of the 'observation sentences'. The argument will have nothing to say about the determinacy of the meanings of the latter; and about the interpretation of theoretical terms, it will suggest only some degree, and by no means an unrestricted one, of latitude.

7 The Argument from Above: appraisal

Enough of preliminaries. Let us now try to map the course of the purportedly subtler route which Quine officially conceives the argument to follow. It would seem to involve reliance on the following transitional principle:

If all possible empirical observation underdetermines the choice between theories T1 and T2 (that is, if T1 and T2 are *empirically equivalent*), then a native scientist's responses to his observations will underdetermine the choice between the ascription to him of acceptance of T1 and the ascription to him of acceptance T2.

And that may seem plausible enough. But notice that it does not, by itself, enjoin any conclusions about indeterminacy of translation. It is one thing to suppose that a rational native scientist could quite consistently hold either of two conflicting theories while respecting all possible relevant data. It is another, quite different thing to hold that the sentences by which he expresses whatever theory he does hold may, by an interpreter who respects all relevant data, be translated in different, incompatible ways. The second will follow directly from the first only if the *only* data that the interpreter has to respect concern which data – which observations – the native scientist will have set himself to respect. And that isn't plausible at all, and goes quite unsupported in Quine's presentation. For the project of translation is constrained not just by the need to identify a set of beliefs which, if rational, the native will have arrived at, but to an even greater degree by the need to find plausible *vehicles* of those beliefs in his overt linguistic behaviour. Quine's picture of the situation would seem to be that all we – the interpreters – can have to go on in the end is the native scientist's acceptance of certain observation sentences. Quine generously concedes our translation of these, and allows us the assumption that the native is a fully rational theorist of the range of data which they express, so that, to over-simplify rather absurdly, if just two incommensurable but unimprovable theories are possible of these data, then the native is likely to have alighted upon one of these theories in particular. But Quine seems to be depending on the idea that there can be nothing to provide us with *further* guidance in translating the relevant parts of the native's language, nothing additional to motivate viewing it as expressive of that theory rather than of its competitor. And that seems quite unjustified. As theorists of meaning, we will have to locate a *syntax* in those parts of the native's language, and then do a plausible job of mapping the ingredient concepts of one of the theories or the other on to components of his language identified by that syntax, the mapping to culminate in a satisfactory recursive theory of meaning. Quine gives absolutely no reason to discount the thought that the case for one of the interpretations in particular may simply evaporate as soon as this serious work of interpretation gets under way. Bluntly, it may just prove impossible to find the right kind of phonological or morphological structures in the native's theoretical sentences to subserve the necessary lexicography and semantic mapping.

That is one misgiving. We encounter another when we turn to consider just what status the premise – the Underdetermination Thesis – enjoys. Quine wrote

that he expected “wide agreement” on this. And, surely, is it not just obvious that theories incorporate more content than the sum of their observational consequences?²⁹ So isn't it perfectly intuitive that this body of consequences must be theoretically axiomatizable in a variety of inequivalent ways?

When Quine anticipated little resistance to underdetermination, no doubt that was one kind of thought he was having. For instance, let T be some empirical theory and consider two consistent but mutually incompatible supplementations of it, T1 and T2, neither of which entails any empirically testable consequences over and above those of T. Then the choice between T1 and T2 is clearly underdetermined by all possible observations. It merits emphasis, therefore, that this kind of case is *not at all to the purpose*. If Quine's argument is to work then the relevant kind of case has to be one in which, precisely because all possible observations underdetermine the choice between two theories, there is nothing to motivate the ascription to the, by hypothesis, *fully rational* native scientist of one set of theoretical beliefs rather than the other. But equally, of course, if the argument is to work it is essential that the interpreter can have no good reason to suppose that the native scientist accepts *neither* theory – essential that there is not a better theory dominating both. And in the envisaged kind of case there will be: for if the native scientist is perfectly rational, he won't be inclined to accept any empirical theory the observational support for which extends no further than for a straightforwardly extricated, otherwise decent enough sub-theory: in the example as envisaged, precisely the theory T.

In brief, gerrymandered examples of Underdetermination, where the incompatibility between empirically equivalent theories is sustained only by their containing empirically idle hypotheses, won't drive Quine's argument. What the argument needs, rather, are cases where empirically equivalent but incompatible theories would either cease to be empirically equivalent, or would lose empirical content, if either was somehow truncated just far enough to eliminate the incompatibility. The clash, in other words, at the theoretical level must be owing to components which are *integral* to the theories' respective capacities to predict and explain the relevant range of observational phenomena. It is not an objection to this point that even when it is required that the axioms be finite in number, any given theory is likely to admit of a variety of axiomatizations, and that difficulties are consequently to be expected for any attempt to characterize precisely which of a theory's components should be reckoned integral to it. Since, as we have just noted, the Argument from Above won't run if the Underdetermination Thesis is made incontestable only by its trivialization, the obligation is actually on the *Quinean* to make out what is involved in the non-trivial case. Whatever it may or may not be possible to say by way of further explanation, what the Quinean requires are examples of pairs of unimprovable theories, the acceptance of each of which in its entirety would be justified on the part of one who knew of sufficiently many of its empirical successes but had no inkling of the other.

With this admittedly vague proviso, what exactly is the Underdetermination Thesis? Again, there are a number of claims of differing strengths to consider. Say that an empirical theory is *tight* just in case it is free of empirical slack of

the kind just gestured at, so that it is the underdetermination of tight theories by all possible empirical data that is the material contention for Quine's argument. Let 'S' range over statements whose content potentially befits them to participate in tight theory construction, and let 'T', 'T*' range over empirically acceptable, global such theories. Then the following are among the possibilities worth singling out:

- (1) $(\forall T)(\forall S)(S \in T \rightarrow (\exists T^*) \sim (S \in T^*))$ *Total theoretical underdetermination:* every component of any acceptable, tight, global theory is omitted by another acceptable, tight, global theory.
- (2) $(\forall T)(\exists S)(S \in T \ \& \ (\exists T^*) \sim (S \in T^*))$ *Partial underdetermination of any theory:* any acceptable, tight global theory will have some theoretical components which are omitted by another such theory.
- (3) $(\exists S)(\forall T)(S \in T)$ *Partial determination of all theories:* some theoretical statements feature in any acceptable, tight global theory.
- (4) $(\forall T)(\forall S)(S \in T \rightarrow (\forall T^*)(S \in T^*))$ *Total determination of empirical theory:* the theoretical components of any acceptable, tight global theory feature in all such theories.

Now, allowing that the last may be merely utopian, about which (if either) of the first two of these can "wide agreement" be expected? Well, perhaps the history of science throws up some support for an Underdetermination Thesis of type (2). It is possible, for instance, though this is a matter for experts, that Special Relativity Theory and the Lorentzian Theory of Corresponding States share all their testable consequences, and that either might thus in principle be incorporated within an acceptable, tight, global theory.³⁰ If so, then – since no acceptable, tight, global theory will contain each of these as sub-theories, but must contain some theory of the phenomena which they explain – thesis (2) may be true. But such local examples seem special at best. It is hard to foresee what argument there might be for something stronger than an Underdetermination Thesis of type (2). Thesis (1), let us be clear, asserts that it is in the nature of empirical theory construction that any tight, empirically adequate, global theory will contain *only* dispensable theoretical claims. What reason is there to think that this is so?

For our present purposes, the crucial reflection is that thesis (3) – that such theories will agree on a common core of theoretical claims – is consistent with thesis (2). So unless the Quinean can make thesis (1) stick, the premise of the Argument from Above, whatever its exact detail, is going to be consistent with the idea that an ideally rational native theorist will be bound, if he is able to take

account of sufficiently much of the available data, to arrive at certain *specific* theoretical beliefs, just in virtue of the nature of the project in which he is engaged. And if that were so then unless there is some special reason to worry about the identifiability of such beliefs, we may equip ourselves, as hypothetically ideal interpreters, with a knowledge of what they are. So equipped, our attempt to interpret the native scientist will be subject to an additional constraint: that of locating expressions for these privileged beliefs among the theoretical sentences which the native scientist is prepared to accept. This constraint may then motivate assumptions about the syntax and meanings of sub-sentential expressions in the native's theoretical language which may rub off on the translation of sentences expressing beliefs of other kinds. In short, it may be an additional source of determinacy of translation.

Those suppositions may, to be sure, be utterly fanciful. The point is only that the Underdetermination Thesis, if it is anything less than the radical thesis (1), is going to be *consistent* with them, and hence cannot validly enjoin any conclusion about indeterminacy of translation in the kind of way Quine seems to have had in mind. Quine's thought, in essentials, was that an assumed knowledge of the meanings of the natives' observation sentences could no more narrowly constrain the interpretation of their theoretical language than the totality of true observations which they could express in that vocabulary would constrain their selection of an empirical theory. We have already had cause for misgivings about the refusal, implicit in this comparison, to acknowledge the routine syntactic constraints to which radical interpretation is subject. But now it appears that Quine has in any case to rely upon what is, so far as I am aware, a quite unsupported and implausible version of the Underdetermination Thesis. To wit, only thesis (1) will do. For once theory construction is allowed to be partially determinate – thesis (3) – ideal interpreters will be constrained to find, among the sentences which a putatively rational native scientist is prepared to accept, some which serve to express the privileged core of empirically determined theoretical beliefs. It cannot be excluded – at least, not without further argument, yet to be provided – that this constraint would greatly reduce their freedom of interpretation, or even that it would have the effect that the interpretational project is uniquely determined. (Maybe each item of the native's theoretical vocabulary occurs in the privileged core.)

Again, I'm not suggesting optimism about such possibilities. It is merely that the premise for the Argument from Above, to the extent that it is something for which support might be forthcoming from the history of science, is consistent with them, and hence insufficient for Quine's notorious conclusion.

* * *

We have found each of Quine's classic arguments, from Above and Below, to provide less than compelling grounds for either the thesis of the indeterminacy of translation or even, more modestly, for that of the inscrutability of terms.¹¹ Moreover, as stressed at the beginning, Quine's own views have been modified

and, in certain respects, softened since he first formulated the arguments on which we have concentrated. Nevertheless, a conviction of the resistability of those original lines of thought is no cause for complacency on the part of friends of the intensional. Although the thesis has been usually received as a paradox, it should be remembered that, within a broader physicalist framework, the indeterminacy of translation would come, at least at first blush, as a relief – the obviation of any need to locate meanings, and intentional states, within a purely physical world. As it is, an abiding tension between the thoughts on the one hand that *in some sense* the world is exhaustively physical and, on the other, that ordinary talk of meanings and the propositional attitudes ought to be unproblematical, remains, and its reconciliation continues to be one of the great issues facing contemporary philosophy.³²

Notes

- 1 The reader should be reminded, however, that while this general concern has undoubtedly conditioned and intensified the reaction to the 'sceptical argument' which forms the core of Kripke's interpretation of Wittgenstein, that argument itself – in contrast to Quine's – makes no explicit behaviourist or physicalist assumption.
- 2 In fact they are directed at different versions of it, as we shall see.
- 3 For Quine's own original formulations, see pp. 27ff. of *Word and Object* (Cambridge, Mass.: MIT Press).
- 4 Large and subtle issues are raised here. At first blush, it may seem obvious that there are first-/third-person asymmetries of this kind; for instance that, even if my linguistic behaviour does underdetermine the translation of my uses of the word "rabbit" – to anticipate Quine's famous example – leaving the radical interpreter with no clearly superior choice among a range of rival interpretations of them, I at least can be in no doubt about which, if any, of these rival interpretations is correct. For by "rabbit", I mean of course: *rabbits* – so that's the right interpretation, and anything else is incorrect! But of course the interpreter will expect me to say that. The question, for him, is exactly what knowledge I thereby express. And the question for me – since I would indeed affirm that sentence whatever I meant by "rabbit" – is whether I thereby express any *substantial* piece of knowledge denied to the radical interpreter.
- 5 Here is a well-known formulation of that scorn:

One may accept the Brentano thesis [of the irreducibility of intentional idiom] either as showing . . . the importance of an autonomous science of intention, or as showing the baselessness of intentional idioms and the emptiness of a science of intention. My attitude . . . is the second. . . . If we are limning the true and ultimate structure of reality, the canonical scheme for us is the austere scheme that knows . . . no propositional attitudes but only the physical constitution and behavior of organisms. (*Word and Object*, p. 221)

- 6 Perhaps because it has not been properly appreciated how deep it goes. Someone might think that, so far from posing a problem for Quine, the upshot here – the indeterminacy of truth-value of individual sentences – is merely part and parcel of Quinean holism: the idea, elaborated in §§ 5 and 6 of "Two Dogmas of Empiricism", in *From a Logical Point of View* (Cambridge, Mass.: Harvard University Press) that individual sentences indeed have no meaning except in the context of a larger system – that "the unit of meaning

is the whole of empirical science". Indeterminacy of truth-value at the level of sentences may not seem too shocking a matter if it is theories as a whole, rather than their ingredient statements, that are properly conceived as the bearers of truth and falsity.

This suggestion just invites the question, however, of why the whole dialectic does not then replay itself at the level of theories. After all, isn't a theory just a big sentence? – so isn't the effect of the holism just to caution against thinking of *small* sentences as the bearers of determinate truth-values? Whereas the problem is to recover, once meaning is indeterminate, *any* space for determinacy of truth-value, even for sentences as big as a global physical theory. If, in company with the indeterminacy of meaning, there is nevertheless to be such a thing as determinate truth-value at any level, then Quine officially needs, for items at that level, an account of truth, and of what determines truth, that liberates the notion from dependence upon any semantic parameter.

- 7 The Above/Below terminology is Quine's own – "On the Reasons for the Indeterminacy of Translation". *Journal of Philosophy*, 67 (1970), p. 183.
- 8 At least in "On the Reasons for the Indeterminacy of Translation".
- 9 *Word and Object*, pp. 71–2.
- 10 The reader may care to think through how the point might apply for $\Phi = \dots$ is the same as \dots " and $F = \text{"rabbit"}$ and $G = \text{"temporal stage of a rabbit"}$.
- 11 Evans seems not to have had a problem with the idea that something might be simultaneously both white and bloodstained! A reader who does will be able to construct another example to make the points about to be illustrated.
- 12 There will be questions, of course, about its scope where it occurs in the latter mode in sentences involving compound predication; the reader may care to think through what patterns of assent and dissent might motivate particular interpretative proposals about the scope conventions in play in the natives' language.
- 13 See pp. 360–1 of "Identity and Predication", *Journal of Philosophy*, 72 (1975), pp. 343–63.
- 14 It's straightforward to envisage the sort of data that might prompt the suggestion. Suppose, in full view of a group of native speakers, we take a deeply and thoroughly bloodstained rabbit and wash it completely clean in a stream. Then we put to them each of the following sentences for assent or dissent:

| | |
|--------------------|---------------------|
| odolby neg gavagai | odolby gavagai |
| odolbyp gavagai | odolbyp neg gavagai |

Finding that the natives assent to both sentences on the left, and dissent from each on the right, would confirm the interpretation of the "-p" suffix as a past-tense indicator. We may suppose this pattern exemplified across a wide range of cases.

- 15 In order most easily to illustrate Evans's treatment within the framework of the discussion so far, I shall use "odolby" sometimes as present tensed and sometimes as a tenseless counterpart.
- 16 I am prescinding from the awkwardness, for Evans's purposes, that his objection only engages if the stage-theorist has somehow been stuck with the assumption that the reference of the particular use of "gavagai" is to the last stage in the life of the rabbit in question (i.e. not to an earlier bloodstained stage of that rabbit, or to a stage of a different rabbit). An analogue of that assumption might be more secure if we were concerned with a different example: one whose featured predicate was true *only* of the last stage in the life of a particular, recently salient rabbit, and of no stage of any other rabbit in the recent experience of our interlocutors. (Evans actually has "running", but that presents the same awkwardness.)

17 This reading is mandated by the reflection that to treat the clauses as concerning *type-predicates* instead would rapidly lead to contradictions. Let y be bloodstained at t_1 but not at t_2 , and let x be a stage of y . Then $\langle x, t_1 \rangle$ satisfy "odolby" (tenseless), since there is a y such that y is bloodstained at t_1 and x is a stage of y . Let t_u first be t_1 . Then x satisfies "odolby" (*present tensed*). Note that this upshot involves no relativization to the time of utterance. So now let t_u be t_2 . There is no y such that y is bloodstained at t_2 and x is a stage of y . So x does not satisfy "odolby" (*present-tensed*). The italicized claims are overtly contradictory; however, the contradiction is merely apparent if we take it that the two occurrences of "odolby" refer to different tokens of the same type.

Could this contradiction have constituted Evans's objection? No, since it is elicited with respect to a single stage, x , and makes nothing of the generalization across stages on which he remarks. In any case it is easily resolved, as we have just seen, without recourse to anything like his second proposal.

An alternative way to avoid the contradiction for an interpreter who for whatever reason wanted his semantic clauses to concern type-predicates rather than tokens, would be to relativize the notion of satisfaction to times. Such a proposal might proceed with clauses along the following lines (which also avoid recourse to tenseless object-language predicates):

- (1) x satisfies "odolby" at time t if and only if something is bloodstained at t of which x is a temporal stage.
- (2) x satisfies "odolby" at time t if and only if there is some time t' , earlier than t , such that x satisfies "odolby" at t' .

And so on. Note that stage-promiscuity would still be a consequence: such a treatment will entail that if any stage satisfies a predicate at a time, then all stages of the same continuant will satisfy that predicate at that time.

18 Another misgeneralization would confuse stage-promiscuity with a kind of *predicate-promiscuity* – the idea that any stage which satisfies a tensed predicate simultaneously satisfies all other tenses of the same predicate. Predicate-promiscuity would likewise be at odds with the natives' selective use of tensed predicates and, as the reader may care to verify, is indeed entailed, via stage-promiscuity, by Evans's first kind of clauses if they are taken to concern type-predicates. But there is no such implication once those clauses are taken to concern tokens – or are replaced by clauses in which satisfaction is relativized to time (cf. note 17).

19 See Christopher Hookway, *Quine* (Oxford, Polity Press, 1988), ch. 9.

20 *Ontological Relativity and Other Essays* (New York, Columbia University Press, 1969), p. 27ff.

21 The development of this point for the case of the feature-placing interpretation is left as an exercise for the reader.

22 This point is also important for the significance of Putnam's permutation argument in his *Reason, Truth and History* (Cambridge, Cambridge University Press, 1981), pp. 32ff.; for discussion, see Chapter 17, PUTNAM'S MODEL-THEORETIC ARGUMENT AGAINST METAPHYSICAL REALISM.

23 "On the Reasons for Indeterminacy of Translation", opening paragraph. The limitation of the Argument from Below to the inscrutability of terms is expressly recognized at p. 182 of the same paper.

24 Though the reader should note the gist of the concluding remarks to "On the Reasons for Indeterminacy of Translation".

25 It is true, of course, that distinctions of meaning which survive Quine's argument will

be ones which can be behaviourally grounded and are thus properly public. But that is not enough to make them hygienic from the physicalist point of view. As I stressed at the beginning, the basic worry for physicalism concerning the semantic is its *normativity*, and public meanings are no less normative for being public. A Quinean argument which, while not actually exploiting the presumed normativity of meanings, somehow or other did away with all semantic facts would save the physicalist the task of accommodating this particular province of normativity; but if a residue of semantic facts remains, then so does the problem.

- 26 "On the Reasons for the Indeterminacy of Translation", pp. 179–80.
- 27 See n. 20.
- 28 Robert Kirk attempts to construct a counter-example to the Argument from Above, exploiting this point: cf. Kirk, "Underdetermination of Theory and Indeterminacy of Translation", *Analysis*, 33, 6 (1973), pp. 198ff.
- 29 Strictly, of course, empirical theories issue in categorical claims about observational phenomena only when supplemented with observational premises – statements of "initial conditions". The (intentionally) rhetorical question can be preserved by thinking of the observational consequences of a theory as the corresponding conditional statements, whose antecedents specify the initial conditions, and whose consequents encode the theory's prediction for those circumstances.
- 30 For detailed discussion of this example, see E. Zahar, "Why did Einstein's Programme Supersede Lorentz's?", *British Journal for the Philosophy of Science*, 24 (1973), pp. 95–123 and 233–62.
- 31 Always provided, that is, that the issue concerning the latter is not taken to be settled just by the possibility that the assignments of sub-sentential reference effected by an empirically adequate semantic theory for a given language may be varied without loss of empirical adequacy – that is, without loss of consistency with observed patterns of assent. If that possibility is all that is at issue, then the matter is, arguably, settled in Quine's favour by a generalization of the sort of permutation argument offered by Putnam: see Chapter 17, PUTNAM'S MODEL-THEORETIC ARGUMENT AGAINST METAPHYSICAL REALISM. But we have observed that semantic theory has to answer to much more than empirical adequacy in that limited sense.
- 32 Thanks to Bob Hale, Christopher Hookway, Gabriel Segal and Jason Stanley for very helpful comments.

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Finally *Synthese*, 27 (1974) includes a symposium on indeterminacy and radical interpretation, with contributions from Davidson, Michael Dummett, Harman, David Lewis and Quine himself.