Modality, Mechanism and Translational Indeterminacy*

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1. Introduction

How does Quine argue to the indeterminacy of translation? It has always been hard to tell, but one construction goes like this: A fact is determinate only if it is empirically determinable, for empirical evidence underdetermines truth about the trans-empirical; theory is underdetermined by data. Therefore, facts about the unobservable, facts of theory, are indeterminate. A fact about the meaning of a person's sentence is only determinate if it is empirically determinable. The meaning of a person's sentence is empirically determinable only if it is empirically determinable under what conditions he assents to it. The meaning of a person's sentence about something unobservable is determinable only if it is determinable what unobservable events are occurring whenever he assents to a sentence supposedly about the unobservable. It is not empirically determinable what is going on in the unobservable realm whenever someone assents to a sentence supposedly about the unobservable. Therefore it is undeterminable what a person's theory sentence means. Therefore what it means is indeterminate.

* This paper began as a commentary on Warmbrōd (1988), presented at the 1988 meetings of the Canadian Philosophical Association in Windsor, Ontario. For stimulating discussion, I am grateful to the audience members at that session, especially to Jan Narveson and Michael Kubara. My thanks as well to Professor Warmbrōd himself, and to Simon Blackburn, Robert Bright, Julia Colterjohn, Robert Martin, and Kari Vihvelin. In the present work, I only use Warmbrōd's suggestion as a point of departure. My exposition of Quine and of the details of Warmbrōd's conjecture are developed here in ways slightly different from the ways Warmbrōd himself develops them, though not, I think, in ways that materially affect the force of my criticisms.

This is the argument to the indeterminacy of truth and meaning from the underdetermination of theory by data. You can get to the indeterminacy of the truth and meaning even of so-called observation sentences once you recognize that their truth and assentibility is itself theory-infected and background-assumption infected, respectively. They then inherit indeterminacy from the indeterminacy of theory. This is the argument to a pervasive indeterminacy of meaning from the underdetermination of theory by data and the holistic structure of language and theory.

Alternatively, you can argue directly from the underdetermination of theory by data in general, to the underdetermination of translational theory by behavioural data in particular, and then, if you accept that something is determinate only if determinable, you have the indeterminacy of translation in a very short step.

But Quine is sometimes thought to have another argument for indeterminacy, one that does not rely on the underdetermination of theory by data, but on the underdetermination of facts about merely possible states of affairs by facts about actual states of affairs. For Quine is a notorious critic of the intelligibility of certain kinds of modal claims, and these figure in his thinking about meaning and translation in the following way. Quine thinks that the only evidence relevant to deciding what someone means is the evidence of how he responds to stimuli. If translation is determinate, one should be able to decide translations by finding home-language utterances offered on the same stimulus-occasions as various away-language utterances. The assumption is that if sentences in different languages have the same stimulus-conditions, they have the same meaning. An immediate problem with this method of deciding translations, however, is that it seems people could mean different things by their utterances, while offering or assenting to them on the same actual occasions of stimulus. I might mean "creature with a heart", you might mean "creature with a kidney", but so long as we are only ever stimulated by creatures with both kidneys and hearts, this difference in meaning would never be revealed. We would each assent to and dissent from our respective sentences on all the same actual stimulus-occasions, and for all the evidence of our actual behaviour, it would be indeterminate whether we were claiming the existence of a creature with a heart, with a kidney, or both. It appears then that meaning is underdetermined by even the totality of facts, observed or otherwise, about how speakers respond to the totality of actual stimuli. Note that we here get underdetermination even though it is, speaking pre-critically, an empirical question whether a creature has a heart or a kidney or both, and so whether a person is verbally responding to the stimulus of a creature with a heart, or a kidney, or both.

Now, one is inclined to respond that such differences in meaning are still determinate in principle. For our differences in meaning would be

apparent if, contrary to fact, we were to have been presented with kidney creatures that were not heart creatures. I would then dissent from "that is a creature with a heart", while you would assent to "that is a creature with a kidney", and it would be apparent that I mean something different by what I say than do you. Thus, if translation is not determinate by reference to the totality of what we do say in the totality of actual circumstances, it is determinate by reference to the totality of what we would have said in the totality of actual and merely possible circumstances. But is there a fact of the matter about what we would have said in a non-actual circumstance? Quine seems to think not. There is a fact of the matter about what was (and is, and will be) observed to have been said, and even a fact of the matter about what was (and is and will be) said though not observed to have been said (and one can make defensible and empirically testable guesses about this); but there is no fact of the matter about what merely *would* have been said, no fact about a person's merely possible behaviour, no fact about how he could have been observed to behave in circumstances different from ones he ever was, is, or will be in. For since these involve speculations by hypothesis about *non*-actual circumstances, there can be no *actual* direct empirical test for the adequacy of speculations about them. Such speculations therefore, are completely unconstrained by the determinable actual facts; indeed, there is nothing to ground their truth. What is there that could make it true that I would have said this rather than that? So the truth of such claims is indeterminable and (assuming only the determinable in principle is determinate) indeterminate.

It is often suggested, however, that Quine unjustifiably ignores the possibility that translation is determinable and determinate with reference to things other than possibly observed actual behaviour. I wish here to assess a particularly interesting variant on this criticism which I learned from Ken Warmbrōd, who thinks that Quine himself is committed to things from which it follows that translation is determinate (Warmbrōd, 1988). Warmbrōd thinks Quine identifies meanings with verbal dispositions, verbal dispositions with the neuronal mechanisms which determine linguistic responses to stimuli, and allows that there is a way to decide how physical mechanisms would behave in non-actual circumstances. But if we can decide that, we can discover what people would be determined by their neuronal mechanisms to say in non-actual circumstances. Facts about possible utterances are grounded in facts about actual neuronal mechanisms. The latter are determinable and determinate, thus, so are the former. Therefore, Quine should allow that meaning and translation are determinate.

I will argue that this is false, but false for an interesting reason: Indeterminacy extends beyond indeterminacy of meaning, to the indeterminacy of the character of physical mechanisms, neuronal and otherwise. This should not surprise us. For the meaning of a sentence is a

matter of the truth value it would take or be ascribed in different possible situations. The character of a physical mechanism is a matter of how it would behave in different possible situations. And if there is a problem with the very idea of determinate facts about merely possible circumstances, that problem will infect any concept defined in terms of supposed facts about merely possible circumstances.

2. Neuronal Mechanisms as the Structures Determining Assent and Dissent; Translation Made Determinate?

Warmbrōd's proposal: Quine identifies the meaning of a sentence with a speaker's disposition to assent to it under conditions of sensory stimulation (Quine, 1960, 27). He also takes a "disposition to do a certain thing [e.g., to assent to or dissent from a sentence] when stimulated in a certain way [as a] mechanism, already mechanically understood or not, in the organism" (Quine, 1969, 144). Moreover, he thinks that that mechanism is "some subtle neural condition, induced by language-learning, that disposes the subject to assent to or dissent from a certain sentence in response to certain supporting stimulations" (Quine, 1960, 223). Now note that Quine seems to believe in the determinacy of the truth values of statements about the possible behaviour of physical/causal mechanisms: "The subjunctive conditional is seen at its most respectable in the disposition terms" (Quine, 1960, 222). "The difference here is that a stabilizing factor is intruded: a theory of subvisible structure" (Quine, 1960, 223). Those kinds of facts concern the physical dispositions of mechanisms; they concern how a mechanism would have behaved in non-actual circumstances. To determine these facts, you merely find a relevantly similar actual physical mechanism and see how it does behave in actual circumstances relevantly similar to the ones in which you are wondering how the first mechanism would have behaved. Speaking of solubility, Quine says, "... if an erstwhile object *a* had the hypothetical characteristic [i.e., whatever structural features make it dissolve in water] (as seen by its having dissolved), and if the stuff of *b* seemed just like that of *a*, then probably *b* had it too" (Quine, 1960, 223). So if *a* does dissolve, *b* would have dissolved in similar circumstances, since *b* is just like *a*. Counter-factuals about the behaviours of a given physical mechanism are decided by "factuals" about actual physical objects similar to that object, in actual situations similar to the non-actual one about which there remain questions.

But now, Warmbrōd thinks, there is in principle a fact of the matter about a person's behaviour not merely in all actual circumstances, but in all possible ones. For since Quine allows that there is a determinate way to assign truth-values to subjunctive conditional statements about mechanisms, and since neuronal mechanisms determine assent behaviours under various conditions of sensory stimulation, to further determine

translation we need merely inspect the neuronal mechanisms that may vary from user to user in their producing their behaviours. We may take differences in what they mean to be reflected in the different mechanisms by which they generate their behaviours. Different mechanisms would issue in different behaviours in the same possible situations. In these differences, differences in the meanings of speakers' utterances may be localized. Such differences account for determinate variations in meaning, overcoming the problem of indeterminacy. I mean "creature with a heart" just if my neuronal mechanism would induce me to assent to that sentence but not to "creature with a kidney" were I stimulated by a creature with a heart but no kidney, whether or not I ever am.

Neuronal mechanisms are susceptible to a structural description, to description in terms of physical mechanism, and such facts are identifiable independently of the behaviours the mechanisms generate. And if we cannot, consistently with Quinean anti-modal scruple, directly assign truth values to subjunctives about possible verbal behaviour, we can assign truth values to subjunctives concerning the physical dispositions of the neuronal mechanisms responsible for those behaviours: We need only appeal to the actual behaviours of similar such dispositional mechanisms. But then so far as people's linguistic behaviour is a function of their neuronal mechanisms, we can inform ourselves about how people would behave linguistically in non-actual situations. For we now know how their mechanisms would induce them to behave there. And knowing how a person could be observed to behave in all actual *and possible* situations would suffice to determine much of his meaning; for example, it would allow us to distinguish his assent to a merely extensionally equivalent pair of sentences from his assent to synonymous ones. In the former case, there would be a possible situation in which his neuronal mechanism would, faced with the stimuli of that possible situation, induce him to assent to one but not the other. But he would always give the same truth value to sentences he regarded as synonymous.

3. A Problem: The Correlative Indeterminacy of Mechanism

Have we a solution to the problem of indeterminacy so far as the problem is rooted in the suspicion of indeterminacy of the truth values of statements about merely possible verbal behaviours in response to merely possible stimuli? I think not. I think all possible observations of all actual neuronal responses to actual stimuli should underdetermine meanings for the same reasons as Quine thinks overt behavioural responses underdetermine them. I will thus show, first, why the proposals stand or fall together, and second, why they both fall.

Quine would apparently agree that you decide how this neuronal mechanism would behave in that situation (one in which the former mechanism is never actually found), by seeing how a structurally similar

neuronal mechanism actually behaves in such a situation. But then why should he not agree that the way you decide how someone who has behaved in such and such a way in such and such circumstances would behave in circumstances in which he is never actually found, is by discovering how someone who has behaved as the person in question did, *does* behave in an actual circumstance similar to the one we are wondering about for the first person?

Warmbrōd himself considers something very like the idea of determining possible behaviour by inspecting the actual behaviour of similar speakers in actual situations similar to the merely possible ones for the original speaker. And he rightly rejects the idea there because (a) no matter how many actual cases we try, there could be divergence in an untried case—perhaps the case in question would have been a divergent one; (b) language users have no introspective authority about how they would behave (nor, presumably, about what their neuronal mechanisms would induce them to do) in an untried case, and so their testimony is no help, never mind that using it would require us to already be able to determinately translate their ruminations on the matter; (c) raw induction from past behaviour to possible or future behaviour is arbitrary—people might behave any old way, not just as they have behaved. And beings just like a given being up to now, might behave in any old way in the future, and not necessarily as a being similar to them up until now would behave later. Two things might be blue up to a certain time, but one of them might turn green later. Whether both are now similar in being blue, or now dissimilar, one blue and one bleen (blue up to a certain time, green after), depends only on what will or would happen. Thus the past behaviour of objects similar up to a given time is no guarantee of similarity in later times, or in merely possible circumstances. To decide whether the objects are in fact totally similar in the relevant way, one must be able to make reference to their possible behaviours in possible situations. But this is the very thing that Quine thinks is modally senseless for the case of overt verbal behaviours. Why then, I wonder, should it not be senseless for neuronal behaviour too? I think, then, that we must apply these objections to neuronal behaviour as well. Modal sense for neuronal behaviour stands or falls together with modal sense for overt behaviour. If both stand, one can work from neuronal to translational facts, but one could also determine translation even without the former.

But they do not both stand; they fall. For how are we to decide subjunctives about how someone or some neuronal system would behave in cases unlike any that any similar system *whatever* ever has been in, is in, or will be in? We would need to know this to decide translation, else we still could not distinguish synonymy from happenstantial material equivalence. But presumably we cannot decide it

even given all possible observations of all merely actual performances of all actual like mechanisms and speakers. There is no reason to suppose then, that all actual features of a type of neuronal structure are revealed in the in principle possible observations of the total actual behaviour of the total set of actual instances of that type of structure. One might stipulate that the remainder of the cases are to be adjudicated by the resemblance between merely possible structures in merely possible situations, and alike actual structures in actual situations. But it is well known that there are competing standards of resemblance, selection among which is underdetermined by the actual behaviours of the actual objects. (Think of blue/bleen again.) And in any case, it has not been motivated that there is any connection between the truth in these cases, and any given stipulation. Nor has it been shown that truth in such cases simply *is* a matter of stipulation. But even if it *is* a matter of stipulation, since different speakers might stipulate differently, we have indeterminacy back *via* indeterminacy in how foreign speakers have (in effect) stipulated in forming their dispositions to assent.

The laws governing the neural mechanisms are as much subject to indeterminacy as are the laws of assent and dissent to sentences for a language. Which law a given mechanism is instantiating is underdetermined by all actual states of the mechanism. Even infinitudes of such states are compatible with many different suppositions about which laws such mechanisms are instantiating.

Quine should never have called dispositional traits of mechanisms determinate. Rather, they fail of determinacy for the same reasons as do the meanings of utterances: conceptually, both are logical constructs from the totality of actual and merely possible behaviour in actual and merely possible circumstances (constructs from the micro-behaviour of neuronal mechanisms in one case, from the overt linguistic behaviour of language speakers in the other). But which such construct is being instantiated by either a neuronal mechanism or a language user is underdetermined by its respective history of actual behaviour. And evidence for which semantic or dispositional property something has is finally the evidence of its actual behaviour (and of the actual behaviour of those things actually like it) in all observable-in-principle, actual situations. Quine seems to think that in general, that information underdetermines the modal properties something has, while yet being the only species of possibility that makes modal sense. He thus has a choice. He can waive his modal scruples and admit talk directly, in the case of linguistic behaviours, of all possibly observed actual *and possible* behaviours in all possible stimulus situations. Or he should cleave to the indeterminacy of the dispositional traits of mechanical structures even by reference to the total history of such physical objects, as well as to the indeterminacy of meaning by reference to even the total actual behavioural histories of actual language users.

But surely the hard physical structure of a mechanism answers these concerns? Surely, for instance, the physical structure of a thing, say a neuronal complex, imposes limits on the kinds of dispositions that thing can be taken to be capable of, on the laws of physical behaviour it can be instantiating, and thus, of especial relevance here, on the repertoire of assent/dissent behaviours it can cause *in situ*? And surely that structure allows us, therefore, to make predictions about its micro-behaviour and about the overt linguistic behaviours it will cause in different circumstances? And again, surely these facts can serve to ground speculation about what it would have made a subject say in various merely possible situations? But this is not so, or at least, not so to a sufficient degree. First, any given structure can be taken to be instantiating any number of dispositions and laws consistently with all its actual performances and the actual performances of things physio-structurally type-kin to it. What determines the relevant one for translation (or for determining which law-governed physical properties a mechanism has, for that matter)? Second, keeping Hume in mind, it is logically possible for any extant physical structure to undergo a physical transformation into a new structure. If it is going to transform, surely that has implications for which laws it can be said to be instantiating over its life-time. Yet whether it is going to transform may not be apparent in its present structure.

It may be objected that it is merely a question of induction whether it is going to so transform, and inductive predictions concern modally hard matters of fact; matters about future actuals. What it *will* do is surely a determinate question, and one can form testable guesses on the matter. Fair enough. But what of what it might have done, or might do in situations it (and all the things like it) will never be in? Induction and prediction over *merely* possible circumstances is surely indeterminate, and so, therefore, are the structures and laws of mechanisms.[1]

1 In a later version of his 1988 paper, Warmbrōd conceded the indeterminacy of physical mechanisms, but insisted that knowledge of the mechanisms determining people's utterances helped to cut down some of the indeterminacy that would otherwise infect the meaning of those utterances. He thinks we have one problem in trying to figure out what people would say on the evidence of what they have said, another problem in trying to figure out which mechanisms cause them to say what they say. If we know the mechanisms, we have further clues about what people would say; if we do not know the mechanisms, we have an unknown variable further complicating speculation about what people would say.

Now, I grant that if one knows of the mechanisms determining utterance which kinds of mechanisms they are, i.e., what they would tend to make people say, then one would know, other things equal, what people would say. And this would cut down on indeterminacy. What I dispute is whether one can know, independently of observations of the effects of the mechanisms on behaviours, what they would make people say. To discover this, one must experiment with the mechanisms, much as one might experiment with the person: one situates person and/or mechanism in a new environment and notes the resulting utterance. But now, one must conjecture about what the mechanism would have made the person say in a merely possible circumstance, just as one must

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conjecture about what the person would have said in the same merely possible circumstance. And the evidence for mechanism and person are identical, viz., what is said in actual circumstances. Likewise, the indeterminacies to which counter-factual speculations about mechanisms and persons are subject are identical. So, short of magical knowledge of what the mechanisms would induce people to say, citation of mechanisms cannot aid in deciding conjectures about what people would say. While if we can have magical knowledge of counter-factuals about mechanisms, why not of persons? After all, assuming persons to be mechanism-governed, speculation about what the mechanisms would make people say just is speculation about what people would say, other things being equal. Of course, if we could know either, there would be no indeterminacy. But we cannot, which is just the problem.

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