HUMANS MEANINGS AND EXISTENCES

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M

AGAINST INDETERMINACY

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I would like to think that many people would agree with the following reading of Quine's 'Two Dogmas of Empiricism':

(1) Quine argues against the notions of Analyticity *and* Syntheticity. Perhaps some other pieces of Quine's doctrines, e.g. those concerning necessity, conventionalism, anti-mentalism, etc., seems to upset his balanced attack against both. But I shall not be concerned with these areas of tension in Quine's general programme.

(2) The rejection of the Analytic-Synthetic distinction shows much more than just that 'Analytic' and 'Synthetic' are vague predicates or that they can be explained in a closed circle. Notwithstanding Quine's hesitant responses to the problem of the status of logical truths in subsequent literature, I assume that the rejection of the Analytic-Synthetic distinction is a rejection of the distinction between revisable and non-revisable truths as well. To that extent, Quine questions the entire tradition involving classical rationalists and empiricists alike.

How far-reaching are the consequences of this initial rejection? Quine,[1] it seems, recommends a considerable shift in classical thinking along the following route:

(i) rejection of the Analytic-Synthetic distinction entails a view of language as a total articulated structure;

(ii) this view of language entails a rejection of the theory-language distinction;

(iii) a rejection of the theory-language distinction entails conceptual relativism;

(iv) conceptual relativism entails indeterminacy of translation.

In other words, beginning with the rejection of the Analytic-Synthetic distinction, we must end up rejecting the idea of a system of invariant conceptual connections in the minds of people across communities and ages. Let us call this idea 'the idea of a universal scheme' (*US*, for short). For Quine then there are no invariant fea-

tures of conceptual thinking outside the programmatic assumptions of psychology.

Perhaps this way of looking at Quine's arguments is somewhat stronger than what Quine actually claims. But, since any talk about *US* must be fashioned within a theory and since we do not entertain a distinction between theories and languages, the status of *US* is at least as vaccuous as Kant's *noumenon*. This may be a good reason for *denying US*.

Some people have objected to the preceding story by a restructuring of the preceding route as follows:

(iii)* from an empiricist point of view, a 'behaviouristic' account is the only available account for the learning of languages—home or alien;

(iv)* a 'behaviouristic' account entails indeterminacy of translation;

(v)* indeterminacy of translation entails conceptual relativism.

There are several passages in both 'Two Dogmas' and the book *Word and Object* which tend to support this restructuring of Quine's argument and, I suppose, most discussions of Quine's philosophy of language follow this version. It has the additional merit that one can simply ignore (v)* and restrict atttention up to (iv)*, thereby avoiding the problematic doctrine of conceptual relativism. Davidson, for example, agrees with the Quinean argument up to (iv)* but denies conceptual relativism.[2]

Given this restructuring, it is now possible to argue that since a 'behaviouristic' account of language is untenable, the indeterminacy thesis does not follow. However, I have several related objections to this procedure.

(a) Except on a tortuous reconstruction, the connections between (ii) and (iii)* are not easy to appreciate.

(b) It seems to me that a 'behaviouristic' account is not at all an essential part of Quine's argument. As a constructive philosopher Quine might volunteer to explain how, in spite of indeterminacy (i.e. given that we can make sense of indeterminacy otherwise), there is so much translational agreement among people. *If* one takes up this task, then a 'behaviouristic' account seems a natural step, since behavioural data have the typical property of under-determining theories (translation-manuals, in the present case). Thus, an appeal to

behavioural data explains on-going translation while preserving indeterminacy.

Looking at Quine's strategy in this way, it follows that the untenability of the 'behaviouristic' account will not show the untenability of the indeterminacy thesis. It will show, at most, that on-going translation remains unexplained. As far as I can see, that will be acceptable to Quine. Quine's story does not depend on Skinner's, though it is reinforced by the latter much in the same way in which Kant's story is reinforced by Newton's.

(c) While almost no attractions are left now for a 'behaviouristic' account of language in the context of recent discussions, the doctrine of conceptual relativism, though problematic, continues to be interesting. To me, this doctrine seems to capture neatly two of the recurrent themes in contemporary philosophy of science, viz. foundations of human knowledge are historical in character and theories can be tested only globally. One of the great contributions of 'Two Dogmas' is a demonstration of the conceptual urgency of these themes. It is important to examine, therefore, whether the indeterminacy thesis obtains via the doctrine of conceptual relativism as well.

In this paper, I shall argue that the rejection of Analytic-Synthetic distinction does not compel us to give up the idea of a Universal Scheme; *US* is compatible with a restricted and interesting version of conceptual relativism, though incompatible with indeterminacy of translation.

I

As a preliminary to the somewhat roundabout argument that we propose to develop later, let us examine how we can relate the idea of a Universal Scheme to the analytic-synthetic distinction.

Under a naive and intuitive understanding, as we have remarked earlier, *US* refers to a set of invariant conceptual connections the effects of which we hope to find in every instance of human thought. Now, since analytic sentences are sentences which hold come what may, we might think of these sentences as reflecting (exhibiting, or otherwise pointing to) some invariant conceptual connections. Thus, unless we want to talk of concepts *per se* independently of language, a rejection of analyticity entails a rejection of *US*. I assume that since Frege, it is no longer interesting to talk about concepts *per se*.

However, I do not think Quine's arguments allow such a strong conclusion. Quine forbids us, at most, to make any absolute sense

of the following statements: 'there are (analytic) sentences in languages which hold (or do not hold) come what may' and 'there are (synthetic) sentences in languages which hold (or do not hold) depending entirely on particular experiences'. Thus, we can say either that, if we want to continue with the old terminology, though no sentence of a language is analytic *par excellence*, *all* sentences of a language are, to a degree that is less than absolute, analytic since no sentence of a language is, individually, entirely open to revision; or, we can say that, though no sentence of a language is synthetic *par excellence*, *all* sentences of a language are, to a degree that is less than absolute, synthetic since no sentence of a language is, individually, entirely immune to revision. While the first option depends on the rejection of the synthetic-side of the story, the second depends on the rejection of the analytic-side. The slogans for the two options are, respectively, 'no sentence is open to revision' and 'no sentence is immune to revision'. It would be a surprising stand if someone did not recognize a substantial difference of philosophical motivation involved in the two slogans. It is important that none of the options uniquely follow solely from a rejection of the analytic-synthetic distinction. For this reason alone, I would like to think that there is no interesting choice between the two options.

Empiricist philosophers, including Quine, however, think otherwise. Thus, from an initial rejection of the analytic-synthetic distinction, it had been a facile step to the further thesis that no sentence of a language is devoid of empirical content in the sense that every sentence is liable to revision in the face of recalcitrant experience (which, in effect, is the same as the second option above), though empirical content cannot be allocated on a sentence-by-sentence basis. If we grant this, then there is no reason why we should not grant as well that the set of analytic sentences is such that each element of the set is entirely devoid for empirical content. In the empiricist conception, of course, this set is null; but this null set *defines* 'analyticity' which we wanted to avoid in the first place.[3]

Nevertheless, a reasonably uncontroversial consequence of Quine's arguments is that, since neither the analytic nor the empirical content of sentences can be allocated on a sentence-by-sentence basis, the sentences of a language face the 'tribunal' of experience or the constraints of the mind *as a whole*. Language then is an articulated structure that we employ in totality. Let us call this uncontroversial

core of Quine's proposals the 'Network Theory of Language' (*NTL*, for short).[4]

Given *NTL*, we can still say that some conceptual connections (now deeply embedded in language) are more assuring than some others, and that there is *almost* no limit to the degree of assurance. In earlier writings, Quine himself speaks of sentences as 'peripheral' or 'centrally-embedded' with respect to the total field. What it is that makes such differences in the location of sentences within the network of language? Can we say that the sentences of a language are still permeated with *US* to a greater or lesser extent, albeit, now, in a less recognizable way? But to say that is to depend on the first option above.

In sum, then, if we want to adopt strict neutrality with respect to Quine's argument, the story of *US* is neither confirmed nor disconfirmed by the rejection of the analytic-synthetic distinction, while the question of the uneven distribution of sentences with respect to revisability remains open. At this stage of the argument, therefore, if we want to dispense with *US*, we are equally committed to the dispensibility of indeterminacy and drop the issue. Otherwise, the route to indeterminacy requires that we can view indeterminacy as a consequence of *NTL* such that the question of unevenness is answered without any appeal to *US*. This requirement, we shall see below, involves the doctrines of conceptual relativism.

II

The theory that language is a total articulated structure leads, in a well-known way, to a rejection of the distinction between theories and the languages in which they are expressed. This rejection leads to the further rejection of the related distinction between conceptual schemes as our ways of thinking about the world and the languages that embody them. I shall simply assume that these theses along with their connections with *NTL* are well appreciated.[5]

Given that schemes are so associated with languages, we would expect differences of conceptual schemes among the speakers of two distinct languages. The greater the divergence of languages from each other at a particular point of time, the more the speakers of particular languages think *un*like each other, given that there are particular languages and that they diverge from each other.

We are, thus, led to the view that speakers of distinct languages employ vastly different conceptual schemes for their respective 'pic-

tures' of the universe, as Whorf puts it.[6] This is the doctrine of conceptual relativism in its *synchronic* version.

There is as well a *diachronic* version of the doctrine which says that conceptual schemes, associated with a particular language, might change radically with time. I guess some recent philosophers of science urge this version more than the synchronic one. There are interesting mutual feed-back relations between the two. Thus, to the extent that I find diachronic relativism more interesting conceptually, I am sympathetic to the doctrine of synchronic relativism under certain restrictions. However, I am not sure that diachronic relativism plays too significant a role in Quine's argument.[7] Therefore, I shall restrict myself primarily to synchronic relativism.

One immediate consequence of synchronic relativism is that there comes a point demanding 'radical translation' when two sufficiently distinct languages cannot be 'calibrated'. Hence, there will be an almost total failure of translation between the two languages. Since this possibility obtains, in principle and in varying degrees, between any two distinct languages, an *indeterminacy of translation* prevails between any pair of languages.

Some people, including Quine, will object to the preceding characterization of the indeterminacy thesis. Quine characterizes indeterminacy as follows:

> ...alternative manuals of translation can exist, incompatible with each other, and both of them conforming fully to the disposition to behaviour on the part of the speakers of the two languages... each manual, being a manual of translation, purports to specify the equivalence relation between sentences and their translations, and neither manual is right to the exclusion of the other... indeterminacy means that there is more than one way.[8]

Notwithstanding the great influence of this characterization in contemporary philosophy, I find it uneasy in several respects. There is no space here for going into them in any detail. Briefly, they run as follows. First, Quine's characterization depends on a rather narrow response to the question 'what sort of criteria decide the correctness of translations?' While behavioural data represent the *evidence* many other sorts of criteria might be available.[9] Thus, according to Katz,[10] encyclopedic information, heuristic assumptions and the like are involved in the construction of translation-manuals. Our version of indeterminacy pervades any such criteria since it is a con-

sequence of the rejection of theory-language distinction. Secondly, the suggestion of alternative manuals is difficult to carry through in the light of preceding discussions. Suppose we have two languages A and B which demand radical translation. How do we place the suggestion of alternative manuals? Is there a sort of translational assymetry between $A-B$ and $B-A$? Or, is there a reference to a third language, C, such that there is 'incompatibility' between $A-B$ and $A-C$? It is easy to see that both these suggestions assume translational *determinacy* for $A-B$, $B-A$ and $A-C$? Alternatively, Quine might want to insist that there are alternative manuals *within A or B*. This seems to be the suggestion involved in Quine's 'Gavagai' example: 'Gavagai' can be translated into English either as 'Rabbit' or as 'Rabbit time-slice'. But then we must give up the scheme-language association in principle. In that case, the strength of the indeterminacy thesis depends entirely on the merits of behaviourism. Thirdly, it is clear from the 'Gavagai' example that Quine's version of indeterminacy depends antecedently on the idea that the schemes of the native and the field-linguist must be radically different; otherwise, the question of alternatives will not arise in spite of the Duhemian principle of under-determination. Thus, our version is already fundamentally involved in Quine's.

Returning, then, to our statement of indeterminacy, it is clear that the significance of the thesis depends largely on the question whether there are distinct languages such that this form of indeterminacy obtains. The statement itself does not say anything about it. This might be brought out more clearly if we rewrite the indeterminacy thesis in symbolic form,

$$(P) \quad (x)\,(y)\,(L_x \cdot L_y \cdot x \neq y \rightarrow -S_{xy})$$

where $L_x : x$ is a language, $S_{xy} : x$ can be successfully translated to y. P, as it stands, is empirically vaccuous since it is possible for someone to accept P and still reject that indeterminacy obtains for any pair of human languages, simply because no two human languages are distinct in the required sense. P, thus, requires to be substantiated with the further claim that there are two such languages. Quine, to my knowledge, except the fictitious 'Gavagai' example, never pursued this line. However, Whorf did.

III

The Whorf-Sapir hypothesis says that, in general, men's minds are

shaped by the languages they speak. Since this was proposed as an empirical hypothesis in anthropological psycholinguistics, the experimental investigation of this hypothesis carries some interest.

Psycholinguists have found it more convenient to arrange the general hypothesis into three separate versions: *strong, weak* and *weakest*. The strong version of the hypothesis states that the availability of cognitive categories are correlated with particular languages. The rest of the versions emphasize the effect of language on some non-linguistic behaviour. We shall see that this arrangement has important consequences for Quine's theory. It is clear that the strong version comes closest to the doctrine of synchronic relativism. Thus, we might want to relate the evidence that has been offered with respect to the strong hypothesis as evidence for (or against) synchronic relativism.

We cannot enter here into the details of the series of psycholinguistic experiments conducted on this issue over the years.[11] In sum, however, with respect to the strong version, not only has no experiment produced unequivocal results, it is not even clear, understandably, how a proper test for the strong version would look—a test that would produce unequivocal results. The same, then, can be said for synchronic relativism.

Some experiments, nevertheless, e.g. those of Brown-Lenneberg and Lantz-Stefflre, have tended to support a weak version of the hypothesis—the effects of language on colour perception being a case in point. I take it that it would not be too incorrect to say that experimental confirmation of the weak hypothesis shows the possibility of genuine but partial failure of translation in the required sense though, of course, this recognition itself is highly theory-laden.[12]

Supposing this to be the experimental situation, it would be interesting to see whether Quine's theory explains this. As far as I can see, there are two options for Quine. Either his theory explains the situation—in particular, the possibility of partial translation-failure; or, the experimental results are irrelevant for his theory. I shall argue that there are troubles for Quine's theory on both the (incompatible) options.

At this stage, I shall try to make clear the structure of my arguments so far. Beginning with the rejection of the analytic-synthetic distinction, I tried to show, stage by stage, that the chain of consequences fail to precipitate a non-arbitrary choice between indeterminacy and *US*. At every stage, therefore, we are free to drop the

issue. I think this freedom obtains so long as the cases for *US* and indeterminacy are maintained at the very abstract level. But, I guess, nobody would disagree that a choice is important for the crucial problem of the *goals of a theory of meaning* depends on it. Føllesdal, for example, maintains that every theory of meaning must preserve indeterminacy[13] while Katz argues that it is the business of a semantic theorist to exhibit the necessary connections in human thought.[14] We cannot maintain both; yet how do we decide?

My strategy, therefore, consists in locating some relatively unproblematic empirical domain where we can pull the abstract proposals down to the ground level and then see which one of the competing proposals handles the situation better. I believe the experimental results on the Whorf-Sapir hypothesis does provide such a domain. I would go the extent of suggesting that the results confirm what had been intuitively obvious from the beginning: it makes no sense to talk about *complete* failure of translation but it does make sense to talk about *partial* failure. Even without entering into the details of psycholinguistic experiments, we can easily locate various ways in which communication breaks down between the speakers of, at a first approximation, geographically distant languages. Such commonplace observations concern variations in perceptual reports, unexpected responses to observations, dependence on the bilingual speaker and so on. If we want to include these observations in a theory, the theory should allow partial translation-failures and, therefore, partial translation-successes. In doing all these, I am *not*, as some people tried to do earlier, trying to relate the Whorf-Sapir hypothesis with Quine's proposals in any direct manner. As we have seen, these theories are logically very different.

Thus, it can be argued that the indeterminacy thesis, instead of being refuted by the experimental disconfirmation of the Whorf-Sapir hypothesis, can indeed explain the result. Indeterminacy does say that, in spite of our adopting a rough-and-ready translation-manual to our satisfaction, the possibility cannot be ruled out that the alien speaker thinks differently about the same physical evidence. However, it does not at all follow that such conceptual differences can be empirically located. Just the opposite. While both Quine and Whorf raise the possibility of failure of 'calibration', Whorf seems to suggest that there are non-linguistic determinations that could identify failures of 'calibration'. Quine's ideas, on the contrary, penetrate even such determinations. One cannot experimentally verify

the differences between two language-scheme complexes, for, the indeterminacy of translation pervades any such experimentation. Hence, 'we talk so inveterately of objects . . . not because our objectifying pattern is an invariable trait of human nature, but because we are bound to adapt any alien pattern to our own in the very process of translating the alien sentences'.[15] Given that our assessment of the alien scheme is pre-experimentally biased by our own, it is no wonder that an experimental investigation of the strong hypothesis would fail to establish unequivocal results.

Translation proceeds by imposing one's scheme as effectively as one can. Thus, paradoxically, translation must succeed; there is simply no other way. How is it then that some weak version of the Whorf-Sapir hypothesis is indeed verified experimentally, so that we can sometimes locate, even if on a lesser scale, the failure of 'calibration' between languages?

I think it is possible to reconstruct a part of Quine's elaborate behavioural machinery to handle this objection. Notice that, at this stage, we are not drawing on the behavioural machinery to explain indeterminacy; indeterminacy is already presupposed in connection with the strong hypothesis. Now we want to see whether, along with this presupposition, the behavioural machinery can handle partial translation-failures. I said earlier that I find behavioural machinery to be intrinsically uninteresting. But my requirements, at this stage, are very weak. I am prepared to allow the use of behavioural machinery provided it would offer some explanation, however uninteresting, of partial translation-failure.

I cannot go into the projected reconstruction in any detail. Briefly, it might run somewhat like this. Consider the 'Gavagai' example. Given that the field-linguist is armed with the mechanism of query and assent/dissent, he cannot isolate the (possible) alternative individuating patterns governing the native solely on the basis of his query, since, in any case, the native would assent to the query if his current individuation involves a rabbit at all. Stenius,[16] argues that even this last assumption might be impermissible under the circumstances. It would be a remarkable good guess if the linguist is able to relate 'Gavagai' to 'rabbit' at all beginning with a null set of assumptions. If, however, we grant the linguist this pre-experimental assumption, what prevents us from granting him a further eleborate list that ends with 'such objectifying patterns are indeed invariable traits of human nature'? Thus, if Quine's programme is initially unbiased,

it cannot begin to get implemented; if, however, it is biased, it leads to an abandonment of the entire programme. This argument exhibits one reason why I find behavioural explanations uninteresting.

But it might be good heuristic move to grant the linguist some, but not all, assumptions especially when we have assumed that in-determinacy obtains in the present case. Hence, we can allow that the linguist comes up with a set of stimulus-meanings for the native speech. Moreover, 'we could distinguish doubtfulness of assent and dissent' by 'reaction time' and 'elaborate our definition of stimulus meaning in easily imagined ways to include this information.'[17]

At this point, we might want to relate this machinery of stimulus-meaning, reaction time, etc. to grades of ontic commitments and grades of theoreticity. Thus, we might assume with Quine that, begin-ning with a 'pre-individuative phase', 'entification begins at arm's length . . . the things in sharpest focus are the things that are public enough to be talked of publicly . . . it is to these that words apply first and foremost.'[18] By stages the talk shifts to abstract entities lead-ing up to the theoretical concepts of science or myths, as the case may be. Since we can assume that such gradations obtain for each language, the facility of translation is closely linked to the public character of stimulus-conditions, both for the alien language and the home. As the truth-functions and theoretical concepts are farther away from public contexts, they are more deeply embedded in one's particular scheme. Thus, they resist the possibility of alternatives from each language's point of view, resulting in the increase of re-action time and, therefore, a decrease in translatability. We can think that, since, in the 'Gavagai' example, the native's reaction time is least likely to be anything but instantaneous due to the public charac-ter of the context concerned, the linguist would write, straightaway,

$$\text{'Gavagai'} = \text{'Rabbit'}$$

With reduction of 'sharp focus', we can think, the linguist would progressively replace the non-hesitant '=' with wavy line, more wavy lines and so on, depending on the increase in reaction time. The sequence of wavy lines would thus represent partial failure of 'cali-bration'. Moreover, the amplitudes of the wavy lines would model the uneven distribution of sentences within a scheme. Here we have then a single answer to both the questions of partial translation-failures and unevenness without any appeal to US. Does this procedure make sense?

IV

Instead of trying to answer this question directly, I would like to relate it to an observation which questions the very idea of indeterminacy itself. Somebody might argue, drawing on an analogy of Quine's own arguments concerning skepticism (i.e. skepticism is legitimate only if there is already an agreed framework that is not open to skepticism),[19] that general indeterminacy does not make sense. We can talk legitimately about—in fact sometimes even locate—a partial failure of translation only if there is an agreement about a framework. By the very conditions of Quine's argument, it cannot be allowed that there could be an agreed framework that is immune to indeterminacy. We have remarked already, in the context of Stenius' argument, that this would be detrimental to the entire story. It follows, then, that since Quine is committed to maintain global indeterminacy, his argument is illegitimate.

Of course, the above argument holds only if we grant that we can sometimes locate partial translation-failures which are *instances* of the general indeterminacy; just as the skeptic would argue that illusions and hallucinations are but instances of the general fallibility of knowledge. Whatever be the merits of the skeptic's argument, Quine would simply deny that his own argument has the same form; he is not offering instances in support of the indeterminacy thesis. Perhaps this explains Quine's lack of interest in the empirical consequences of his theory. Quine would say, presumably, what we take to be partial translation-failures are not examples of general indeterminacy at all. Therefore, granting that we can sometimes locate partial translation-failures, there is no assurance that we have had an agreed framework. Quine writes:

> (the) initial indeterminacy (in the recognition of assent/dissent) . . . carries over into the identification of stimulus meanings. In addition, there is in the identification of stimulus meanings the normal uncertainty of induction, though . . . this is not what the indeterminacy thesis is about . . . finally, there are the linguist's later adoption of analytical hypotheses, undetermined still . . . the indeterminacy of translation comes in degrees.[20]

I have cited this passage to show as well that I am not missing the so-called 'double indeterminacy'. How do we square up this extremely stimulating passage with the explanation developed in the last section? Apparently, it seems to relate nicely to the issue at hand, i.e.

whether Quine's argument is similar in form to the skeptic's; or, whether Quine can simply deny the analogy. As far as I can see, the primary (Duhemian) indeterminacy is carried over in any case. The question of the final indeterminacy about the analytical hypotheses is just the issue. Therefore, Quine can throw the burden of actual failures of translation on 'normal uncertainties of induction'; what we *take* to be the partial translation-failures are, in fact, instances of 'normal uncertainty of induction'. Perhaps this is a correct way of assessing translation-failures. Perhaps translations do improve with time and energy on the part of the translator. I am not sure but I do not want to argue the point.

Thus, instances of indeterminacy, wherever they are and even if they 'come in degrees', are forever hidden from human observation, for, the ways in which indeterminacy comes about are involved in the very ways in which we read each other's thoughts. We cannot, so to speak, 'step outside' indeterminacy and observe its instances, though we can observe 'normal uncertainties of induction' by, say, repetition. The procedure of the last section—involving stimulus-meaning, reaction time, etc.—just describes the ways in which we read each other's thoughts; it has nothing to do with the (alleged) instances of indeterminacy since there are no such instances to be encountered.

The above response would be a puzzling *volte face*. We assumed earlier that Quine's machinery seemingly explains a weak Whorf-Sapir hypothesis. The above response, instead, precipitates the following options. (1) If the weak Whorf-Sapir hypothesis is a *weaker form* of the strong hypothesis, i.e. if the weak hypothesis concerns *genuine* failure of 'calibration', then Quine's machinery fails to explain it. (2) If the weak hypothesis is an altogether different hypothesis from the strong one, i.e. if the weak hypothesis concerns merely the 'normal uncertainty of induction', then Quine's machinery explains it—albeit, indirectly—by showing how we reach an agreed framework in spite of indeterminacy. This option (2), however, can be rephrased as—(2)'. Given that there is general indeterminacy, there is agreement about a framework in terms of (non-hesitant) stimulus-meanings; henceforth, actual failure of translation is to be explained by 'normal uncertainty of induction'.

What, then, is the 'cash difference' between the above response and the following counter-suggestion?

(3) There is no indeterminacy of translation, for there is an agree-

ment in terms of a universal conceptual scheme; henceforth, actual failure of translation is to be explained by 'normal uncertainty of induction'.

The net result of this roundabout exercise is that while (2) precipitates once again the failure of a non-arbitrary choice between *US* and indeterminacy, (1) exhibits precisely the failure of indeterminacy. It can now be seen fairly quickly that the weak Whorf-Sapir hypothesis has an explanation in terms of *US*.

US guarantees, in effect, an area of translation-success, i.e. an area of agreed framework between any pair of human languages. Yet, we can conceive of the universal scheme narrowly enough to make way for a large area of translation-failure. This is one of the advantages of *US* over indeterminacy. While indeterminacy must be maintained globally for the entire breadth of languages, *US* can be restricted to a small area. I can even see that *US* need not be located within languages in any direct manner at all. I shall return briefly to some such speculation in the final section.

V

I shall now try to relate the preceding argument to the question of distinctness of languages raised earlier in section II. Quine, presumably, can argue that synchronic relativism does entail general indeterminacy since synchronic relativism forces that we must restrict our talk to specific, i.e. *community-wide*, language-scheme complexes. Even if we may not have a strict criterion for the identity of communities, communities do differ. If and when communities differ, there would be, in the extreme case, differences in the entire complexes. Hence, there ought to be a case for indeterminacy. It is a bad reasoning to infer from the lack of a strict criterion to the absence of one. Notice that this line of reasoning requires not only that communities differ, but also that they could differ to the extent of compelling radical translation.

Crucial to this line of reasoning is the assumption that there is a *multiplicity* of community-wide complexes. Elsewhere[21] Quine argues that we can *manipulate* the crucial parameter, viz. community-*width*. Quine does not elaborate what it is to manipulate this parameter. Apparently, Quine wants to maintain *some* distinction between theories and languages, or, for that matter, between meanings and beliefs, although these distinctions do not obtain in the context of the entire language-scheme complex. The parameter of width, then, is mani-

pulable within a community and not so manipulable outside it: 'we demarcate our practical speech community, for particular given purposes, as the community in which all dialogue that is concerned with those purposes runs smoothly and . . . effectively.'[22] But what defines that there is a limit to such 'practical' demarcations? When does the notion community-wide obtain? It seems that, for Quine, the entire language defines the limit of manipulability.

This answer is not immediately circular, yet it seems to have the form of a 'closed curve in space'. Languages and communities define each other, and a multiplicity of language-scheme complexes divide humanity into a multiplicity of communities. This suggestion involves a confused notion of 'language' to which I shall return in a moment.

Yet, there is nothing in our, and Quine's, arguments so far that entails that the notion 'language-scheme complex' obtains for a multiplicity of communities. There is no ground for this assumption either in *NTL* or in the conception of synchronic relativism. Indeed, *NTL*, and to an extent, synchronic relativism, does not require this assumption. Thus, we can always conceive of a single scheme-language complex that is as wide as the entire human community. There is nothing in the argument so far that could prevent us from such a conception. If there are additional arguments against the proposal of a single complex—and I believe there are—then these arguments must be extra-*NTL*.

However, it is clear that the assumption of multiplicity is crucial for the indeterminacy thesis. In the absence of multiplicity, there is no question of radical translation and the indeterminacy of translation collapses. Quine cannot argue from the logical strength of *NTL* to the compulsions of indeterminacy. Beginning with *NTL*, we can now think of several routes one of which involve the conception of a universal scheme.

The burden of coherence thus shifts on *US*. It would be objected, of course, that the conception of a single language-scheme complex is dubious on at least two grounds. First, what is the point of talking about 'relativism' when there is nothing which human thinking is *relativized to*? Secondly, how is it that we want to talk about a *single* language-scheme complex in the face of the diversity of human languages?

These are extremely important questions, especially when their connections with the rejection of the analytic-synthetic distinction is clearly appreciated. In some respects, no doubt, the conception of a

single complex does overstate the case against indeterminacy. We need to chip away from *this* conception to a conception of *US* that is relatively less problematic with respect to *NTL*. But I do not think that the above questions reopen the issue of indeterminacy in any way. Pending a coherent conception of universal scheme, I can reply to these questions as follows.

First, the conception of a single complex is no more uninteresting than the conception of general indeterminacy. If no instance of partial translation-failure is an instance of general indeterminacy *per se*, what is the point of talking about general indeterminacy when there is nothing which human thinking is generally indeterminate about? In any case, as we have indicated above, the conception of the single complex need not exhaust the entire story of human thinking while the conception of indeterminacy does. Suitably conceived, *US* is, at best, a prop against which we can make intelligible some talk about interesting relativism. What we deny, at most, is the triad of notions that either reinforce each other or collapse together. These notions are *global relativism, radical translation* and *indeterminacy of translation.*
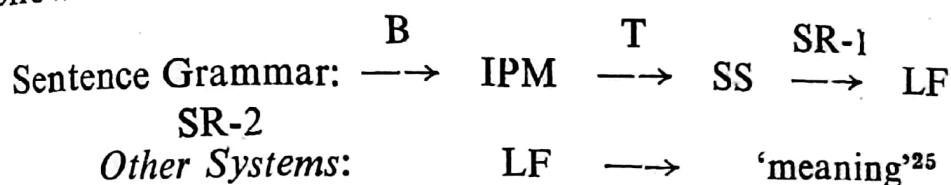
Secondly, the notion of diversity of languages originates from the diversity of verbal behaviour, from the diversity of 'tongues', so to speak. In these matters, Quine is no exception to the wide-spread assumption, which goes by the name of 'Boas Tradition' in linguistics,[23] about the identification of language with (dispositions to) verbal behaviour. However, it is enormously difficult to work this assumption out to a logical criterion of languagehood. What is it that isolates one set of verbal behaviour from another—syntax, phonology, vocabulary, accompanying gestures or conventions? If, however, we want to insist on geographical criteria, how far does a particular set of verbal behaviour stretch—to what water-shed of linguistic communities? What is the status of dialects and language-groups? These and other related questions do not admit of clear, non-circular answers if they are framed in terms of verbal behaviour alone.[24]

VI

I shall present here only the briefest sketch of the sort of alternative I have in mind. I concede that the alternative is not easy to spell out at the current stage of inquiry. It is one of the chief merits of Quine's work that, even if we disagree with pieces of his doctrines, it greatly inhibits the formulation of alternatives as a whole.

The alternative I have in mind emerges, in a well-known way, with Sassure's distinction between *langue* and *parole*. Once we learn to dissociate language as 'underlying system which makes possible various types of behaviour' from the 'actual instances' of such behaviour, the conception of diversity of languages on account of diversity of verbal behaviour begins to lose force. Given this point of departure, a series of interesting possibilities follow.

Chomsky urges, following this route, a distinction between linguistic competence and linguistic performance, the descriptive priority of grammar and the conception of a Universal Grammar. Over two decades of debate on the notion of descriptive priority of grammars (alternatively known as 'Autonomy of Syntax'), has led Chomsky recently to a very interesting conception of sentence grammar as follows.

$$\text{Sentence Grammar:} \xrightarrow{\text{B}} \text{IPM} \xrightarrow{\text{T}} \text{SS} \xrightarrow{\text{SR-1}} \text{LF}$$

$$\text{SR-2}$$
$$\textit{Other Systems:} \quad \text{LF} \longrightarrow \quad \text{'meaning'}[25]$$

In brief, this schema says that a sentence grammar generates, under the constraints of Universal Grammar, what is known as a 'logical form' of sentences. A logical form is a 'certain system of mental representation provided by the rules of the grammar, analogous to representation of phonetic form . . . (m)any of the questions that are regarded as "semantic" can be understood . . . as questions about the syntax of LF.'[26] What the LF represents, under this conception, are some abstract structures (S—structures) as the *final* output of the grammatical system. These structures are not yet *sentences of a language*, in the full sense of 'sentences'. Thus, some further rules, which are outside the scope of grammar, determine the semantic interpretation of sentences. These rules may be contained in some mental systems other than the language faculty (grammar). It is conceivable, then, that

The system of conceptual structures that involves object-reference . . . thematic structures, aitiational factors, and the like, might be distinct from the language faculty, though related to it. (This) possibility relates in obvious ways to recent debate in the philosophical literature about the theory of meaning and belief systems, in part an outgrowth of Quine's important and influential critique of empiricist semantics . . .[27]

Chomsky speculates, tentatively, that two of these other systems

may be 'commonsense-understanding' and 'science-forming capacity'. I have serious reservations about both Chomsky's nomenclature and his initial descriptions of them; these systems, for Chomsky, really mean what they are called.

Nevertheless, one important feature of this conception is that it allows us to view language as a phenomenon which results out of the interaction of several mental faculties. Thus, it is possible to distribute the question of 'meaning' of sentences over several systems. This idea, initially, enables us to avoid the Universal Semantic Component of Katz's conception, which clearly violates the requirements of *NTL*.

Thus, we might conceive of *US* as one of the mental systems that represents some *features* (possibly, features of neural circuits) of conceptual connections, but not the conceptual connections themselves (since the latter obtain only in the context of language). We can conceive of these features to be invariant for the member of the species in the sense that these features obtain invariably so long as the species retains its identity.

US, under this conception, represents the necessary core of sentence-meanings. The sufficient condition, we might conceive, is achieved when *US* interacts with some other system. This latter system might have the property of altering, under general constraints, the initial invariant system in the context of current experience. This 'revised' system now interacts with grammar to generate language. We might think of the property of intermediate system as a 'recursive' property such that it alters any system at hand, not only the initial system. Thus, no invariant conceptual connections would obtain in any corner of language at any time. In sum, the language generated obeys *NTL*. Sentences of a language, under this conception, are neither entirely immune to revision nor entirely open to revision. The question of revision *per se* just drops out of the conception, and, with it, the question of indeterminacy.

I shall leave the matter here and turn to another connection between Quine and Chomsky which might be of some interest for our discussion. At some places, Quine formulates the question of indeterminacy as relating to different *species*, e.g. men and martians. For the member of the same species, the question becomes less acute due to 'approximate homologies of nerve-endings'.[28] Thus, on one account, Quine appeals to a biological basis of *translation*. Similarly, Chomsky poses the problem of Universal Grammar for providing an

upper bound to the class of humanly accessible grammars, though no such upper bound is logically necessary. Chomsky concludes that such an upper bound is biologically based. Putnam[29] observes corretcly that, therefore, a martian might fail to pick up any human language. Thus, we find very interesting convergence of issues between Quine and Chomsky for an *inter*-species problem which has an important bearing on our primarily *intra*-species problem. We can now ask what is it in the languagehood of human languages such that a possibility of indeterminacy is biologically avoided.

NOTES

1. In what follows, I simply report the results of my search for coherence in Quine's works. There might be disagreements over my interpretation of Quine. Yet I do not want to avoid the issue by addressing myself to some fictitious Quine*.
2. Davidson (1973).
3. See Dummett (1974) for a similar observation.
4. We refrain from calling this view of language simply 'Quine's view of language' because of its neutrality from the empiricist point of view.
5. For a lucid discussion of these connections, see Davidson (1973).
6. Whorf (1956), p. 55.
7. Thus, recall Quine's conservatism in this matter when he agrees with Neurath's ship-repairing metaphor.
8. Quine (1975-a), p. 90.
9. For a discussion on the confusion between behavioural evidence and criteria, see Chomsky (1980), p. 48.
10. Katz (1979).
11. On the experimental consequences of Whorf-Sapir Hypothesis I follow G.A. Miller and D. McNeill; see Miller and McNeill (1975).
12. Davidson denies even the possibility of genuine but partial failure of translation; see Davidson (1973). However, I think that his argument is based on a theory of interpretation which is too narrow in scope.
13. Føllesdal (1975).
14. Most recently in *Language and Other Abstract Objects.*
15. Quine (1969-a) p. 1.
16. Stenius (1969).
17. Quine (1964).
18. Quine (1969-a).
19. Quine (1975-b).
20. Quine (1969-b).
21. Quine (1969-c).
22. Quine (1969-c).
23. Chomsky (1972), p. 77.
24. For some discussion of this issue, see Chomsky (1980), p. 117-19.

25. Chomsky (1975), p. 105.
26. Chomsky (1980), p. 169.
27. Chomsky (1980), p. 58.
28. Cited in Føllesdal (1975).
29. Putnam (1967).

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