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ANALYTICITY AND THE INDETERMINACY OF TRANSLATION*

The subject of this paper is the analytic-synthetic distinction and its relation to the indeterminacy of translation. In particular, I am concerned with the question of whether a denial of the analytic-synthetic distinction need involve one in the general scepticism about meaning which is dramatized by the claim that translation is indeterminate. My answer to this question is complex, largely because of a complexity or unclarity in the way in which the analytic-synthetic distinction has been thought of. Proponents of that distinction have taken it to be an epistemological cleavage among statements; they have also assumed that analyticity is simply truth by virtue of meaning, and so must be acceptable if the notion of meaning is. They have thus assumed that the notion of meaning has the power to divide statements into two epistemologically quite distinct kinds. Once articulated in this way, this assumption is at least not obvious; I hope to cast doubt upon it. My strategy is as follows. Drawing largely on the work of Quine, I present two arguments (in sections 1 and 2, respectively). The second is an attack on the notion of meaning, and rests upon, or is equivalent to, the indeterminacy of translation. The first argument seems to rely upon nothing so controversial, and is explicitly an attack on the idea that the analytic-synthetic distinction is of general epistemological importance. Separating the two arguments illuminates the complex debate between Carnap and Quine over analyticity; it also suggests that one can accept the first argument (thereby rejecting the philosophical use of the analytic-synthetic distinction) without accepting the general scepticism about meaning which is the burden of the second argument.[1]

1.

The existence of truths which appear to be necessary or to be a priori is always likely to present a problem to those of an empiricist bent. Russell, for example, was troubled around the end of the first decade of this century by the status of mathematics and logic. He never

doubted that they were a priori, and in the *Principles of Mathematics* (which was completed in 1902) he had accounted for this by appealing to an immediate and non-sensuous perception of the entities of logic. His increasingly empiricist assumptions, and the fact that the class paradox would not yield to any fully intuitive solution, made this account almost untenable. Yet his attempts to formulate an alternative were exceedingly unconvincing – even, one senses, to Russell himself. This situation in part accounts for the historical importance of the *Tractatus*. According to that book, the sentences of logic are not statements about logical entities in a Platonic realm; rather they are *tautologies*, and make no claim on any reality, Platonic or mundane. They are thus both necessary and a priori, because wholly empty. Identifying the sense of a sentence with the claim it makes upon reality, Wittgenstein thus held that what Russell had called the truths of logic were lacking in sense (*sinnlos*) and so not really truths at all. Although tautologies do not say anything, the fact that they are tautologies is, according to the *Tractatus*, a reflection of the underlying logical structure of language. This logical structure is identical in all languages, i.e., all methods of portraying the facts that make up the world.

   In the early years of the Vienna Circle, its members were influenced by what they took to be the theory of meaning implicit in the *Tractatus*. We might call this a theory of *strict language-neutral reducibility*. According to this theory, as the Vienna Circle interpreted it, elementary meaningful expressions are of two kinds: either they are logical constants or they refer immediately to what can be given in sensation. The meaningful sentences of our ordinary language are abbreviations for sentences all of whose components fall into one of these categories; some apparently meaningful sentences of our ordinary language fail this test, and are thus shown not to be meaningful. In an unabbreviated language, every element would be either directly answerable to the sensory given or part of the logical framework of the language. Now the point which must be emphasized is that both of these kinds of elements are language-neutral. The sensory given is *given*, and so is not dependent upon the language of the person to whom it is given; and the logical structure is not peculiar to this or that language but is common to every possible language. Because all language is compounded out of these two kinds of elements, and they are the same in all languages, any two lan-

guages will have the same expressive power, i.e., will be alternative ways of saying the same thing.[2] This is why Carnap was later to speak of "the earlier position of the Vienna Circle, which was in essentials that of Wittgenstein. On that view it was a question of '*the* language' in an absolute sense. . . ."[3]

If this reductionist account of language were correct, then any fully meaningful sentence would be equivalent to some sentence containing only logical constants and terms for what is given in sense. Since both of these kinds of components are language-neutral, the reductionist thesis thus gives a language-neutral notion of the *content* of a sentence, or the claim it makes upon reality. The reduction to logic and the sensory given provides a common basis which enables us to compare the claims made upon reality by any two sentences, even if they are in different languages; and the ability to do this is what gives a sense to the idea of the claim made upon reality by a sentence. By the same token, the reduction gives a sense to the idea of a sentence being *without* content, or making *no* claim upon reality – a sense which is, again, not dependent upon the language in which the sentence is couched. Reductionism, if correct, thus allows us to define a language-invariant notion of analyticity.[4] Whether a given string of symbols is an analytic sentence will, of course, depend upon the language to which it belongs; a sentence of one language will not, in general, be a sentence of another. But the important point is that the criterion which a sentence must fulfill to be analytic – namely, being without content, or making no claim upon the world – is one that can be specified without reference to any language. Because this notion of analyticity is in this way language-invariant, I shall call it "absolute analyticity".

If there were such a thing as absolute analyticity, it would be of considerable philosophical importance. A sentence having this status would be epistemologically quite different from other sentences. Such a sentence would not be answerable to evidence in any sense, because it would make no claim about the world; and such a sentence would be one which any rational being was bound to accept because, so to speak, there would be nothing to be accepted. These features would enable absolute analyticity to play some of the roles which earlier philosophies had assigned to apriority, or to necessity – but (apparently) without departing from a strict empiricism. In particular, absolute analyticity would allow one to be an empiricist and yet to

account for the status of logic, of mathematics, and of philosophy itself.[5]

One difficulty with the notion of absolute analyticity is that it is based upon a reductionist account of language which is too implausible for anyone to believe for very long. Carnap, in particular, seems to have held something like the reductionist view I sketched[6], but abandoned it in *LSL*. The reductionist view presupposes, first, that logical is language-invariant, i.e., in effect, that classical logic is the logical structure which is possessed by every language; secondly, that with the aid of this logical structure all meaningful language can be reduced to terms which refer to what is given in sense. Both of these presuppositions are given up in *LSL*. Partly under the influence of Brouwer, Carnap recognised that different languages might have different logics, and he put forward two such languages in *LSL*. The weaker of these ("Language I") is a language for arithmetic in which the quantifers are bounded, and it is very much weaker in expressive power than simple type theory, which Carnap had advocated in *Der Abriss der Logistik* (1929). The abandonment of reductionism is also clear in *LSL* (see especially p. 319), although the reasons for it are not explained in detail until 'Testability and Meaning'.[7] Simply put, the problem is that there are scientific terms which we must allow as meaningful if we are not to dismiss science, which cannot be defined in observational or sensory terms. Thus it is not possible, even in principle, to translate the language of science into an observational or sensory language.

The abandonment of reductionism means that we have to face the possibility of genuinely non-equivalent languages. One language may contain expressions with certain meanings, while another has no way of expressing those meanings – as the language of Newtonian physics cannot express precisely what is meant by "mass" in post-Einsteinian physics. This has important consequences for analyticity. Most obviously, there is no longer a language-neutral notion of the content of a sentence, for it was only reductionism that gave us any reason to believe in such a notion. So there is no language-neutral notion of contentlessness to which we can appeal in support of the idea that all analytic sentences say the same thing, viz., nothing. Analyticity becomes language-relative. An analytic sentence of a given language, we might say, has no content beyond what is presupposed by that language. But genuinely non-equivalent languages may have different,

and even conflicting, presuppositions. It thus becomes possible to reject an analytic sentence. Suppose that one language contains a term for which a second language has no exact equivalent; and that S is an analytic sentence of the first language which contains the given term. Then it is possible to argue that the first language makes presuppositions which are unacceptable, and that it should be modified into the second – thereby giving up S. Suppose, to be more specific, that we find in our reconstruction of Newtonian mechanics, that the sentence "$f = ma$" is analytic in that language. It would not follow from this that we could not be right to reject the sentence. All that would follow is that the rejection would involve a modification of the language, not merely a modification of the set of sentences of the language which we hold true. But whether this distinction has any significance is, so far, an open question.

At least from the time of *LSL* onwards, Carnap recognizes that analyticity is language-relative, and that it is as possible to change languages as it is to change which sentences of a given language one accepts. But he insists that the two changes are quite different in kind. Within the language, the questions we have to decide are questions of truth and falsehood and are susceptible of theoretical justification. The choice of a language, by contrast, is seen as a matter for practical decision. Such a decision may be more or less well-advised, and have more or less interesting and useful consequences, but it cannot be true or false. Within a language the questions facing us are theoretical, whereas when we have to choose between languages our problem is a purely practical or pragmatic one – a matter of arriving at conventions, as Carnap says (*LSL* p. 51). In similar vein, he says that synthetic truths do, and analytic truths do not, state *facts*[8]; that synthetic statements are matters for *belief*, and analytic statements are not; and that decisions about a language are "not of a cognitive nature".[9]

A comparison may help to explain the picture which seems to lie behind what Carnap says here. It is as if the choice of language were the choice of a set of rules for a game. Once this choice is made, we can appeal to the rules to settle our disputes; but if the dispute is about which rules to adopt, then we have no such recourse. This makes the two sorts of dispute different in kind. If we are playing bridge, say, then it is a factual, objective matter which card wins a given trick; but if three people wish to play bridge while a fourth insists upon poker, then the issue between them is of quite a different

kind. While the bridge-players may accuse the poker-player of having depraved tastes, they can scarcely hold that she believes something false in the same sense as that in which she would if she thought, say, that a contract of seven spades could be fulfilled by making twelve tricks. Similarly, according to Carnap, if there is a scientific disagreement within a language, then we have agreed rules of that language to which we may appeal; whereas if we disagree about which language to use there are no such rules. We may prefer one language to another, but there is no rational basis on which we can say that those who hold the opposite opinion are mistaken. Hence, Carnap concludes, we should be tolerant rather than dogmatic about this matter. This view is enshrined in the Principle of Tolerance, one formulation of which reads:

In logic there are no morals. Everyone is at liberty to build up his own logic, i.e. his own form of language, as he wishes. All that is required of him is that, if he wishes to discuss it, he must state his methods clearly, and give syntactical rules, instead of philosophical arguments. (*LSL*, p. 52)

Carnap's view presupposes an epistemological distinction between the internal and the external. We may now ask what this distinction rests upon. A way of thinking about the issue which lies behind at least some of what Carnap says is in relation to an ideal of rational adjudicability.[10] According to this ideal, when an internal question is raised in a given language the rules of that language tell us what observational data (protocol sentences of the language) would settle the question, and how we should go about gathering these data. The rules of the language define a notion of *confirmation*, which determines what data confirm a given statement, and to what extent. In practice we may, of course, be unable to settle the question, for we may not be able to obtain the relevant data. But the rules of the language guarantee that the way in which the question is to be settled is not open to (rational) dispute by anyone who is speaking the same language; and those who are speaking a different language are simply not asking the same question. Thus, if the ideal were fulfilled, internal questions could, in principle, be settled by appeal only to observation and to the (previously agreed upon) rules of language. This might be held to show a clear epistemological difference between internal questions and external questions, about which nothing similar could be said. Disagreements about which linguistic rules to adopt cannot

be settled by appeal to previously agreed upon procedures, for such disagreements are about what shall count as procedures for settling disputes. The choice of a language is thus a *pragmatic* issue rather than one which is susceptible of confirmation or disconfirmation; it concerns what notion of confirmation we should adopt.

It is worth emphasising that the ideal of rational adjudicability, as I have articulated it, does not obviously rest upon a controversial basis. One might, indeed, claim that it rests upon what seems to be a truism: if we wish to be sure that a debate is substantive, rather than merely verbal, then we ought to begin by seeing if we agree about what data would settle the issue. If we do, we can simply go ahead and attempt to gather the relevant data. If we fail to agree, it may be because our background assumptions diverge, e.g., according to my eccentric theory of light, telescopes are not reliable sources of information about extra-terrestrial objects. In this kind of case, we recognize that we have to settle the dispute about background assumptions before we can deal with the matter in hand. If, however, there is no divergence about background assumptions and still we do not agree about what would settle a question, then this must be because we do not in fact have the same question in mind: the issue between us is a verbal one. Put in this way the ideal of rational adjudicability seems almost undeniable: if a reasonable disagreement is to take place between two people, they must at least agree on what the dis-agreement is about, and on what would settle it. In spite of this air of undeniability, however, the ideal of rational adjudicability is the subject of a sustained Quinean attack.

The attack is essentially two-pronged. First, Quine urges that the evidential relation is a complex one. It is not simply a matter of what observations support a given statement; we must also take account of the impact which a given change would have upon the theory as a whole – its simplicity, fruitfulness, familiarity, etc. (see, e.g., *Word and Object*, section 5).[12] Secondly, having argued that such broad and vague factors as simplicity play a role in change of theory (internal change, in Carnap's terms), Quine insists that the same factors play a role in change of language (external change). This claim is hard for Carnap to resist. In *LSL* he had admitted that the choice of the conventions which define a language is not abitrary, but "is influenced ... by certain practical methodological considerations (for instance, whether they make for simplicity, expedience, and fruitfulness in

certain tasks)." (p. 320). This second point is crucial. Without it, Quine's insistence upon the use of simplicity, fruitfulness, etc., in changes of theory might be taken as a part of the Carnapian enterprise of understanding the confirmation relation within a given language. With it, however, it is clear that Quine's insistence involves a reconceiving of the nature of the enterprise, so that it shall no longer presuppose an epistemological difference between internal questions and external questions:

Carnap, Lewis, and others take a pragmatic stand on the question of choosing between language forms, scientific frameworks; but their pragmatism leaves off at the imagined boundary between the analytic and the synthetic. In repudiating such a boundary I espouse a more thorough pragmatism. ...[13]

In the face of the Quinean attack, Carnap appears to admit that there is no sharp epistemological distinction between internal questions and external questions, that the differences here are all of degree and not of kind:

Quine shows (in his book [FLPV] pp. 42–46) that a scientist who discovers a conflict between his observations and his theory and who is therefore compelled to make a readjustment somewhere in the total system of science, has much latitude with respect to the place where a change is to be made. In this procedure, no statement is immune to revision, not even the statements of logic and mathematics. There are only practical differences, and these are differences in degree. ... With all of this I am entirely in agreement. (Schilpp, p. 921)

It might be thought that, with this concession, Carnap is forced to admit that there is no epistemological difference, or no difference in principle, between internal questions and external questions. But matters are not quite so simple. There is room left for a Carnapian response, roughly as follows.[14] Quine may have shown, and we may be forced to admit, that pragmatic considerations operate within a language as well as in the choice of a language. This shows that the difference between internal and external questions cannot simply be that only theoretical or confirmational considerations are relevant to the former, and only pragmatic considerations to the latter. Nevertheless there is a difference. Internal questions are those to which confirmational *and* pragmatic considerations are relevant; external questions are those to which pragmatic considerations alone are relevant. Quine compels us to redefine, but not to deny, the epistemological difference. The rules of language which define the notion

of confirmation (for a given language) do not always suffice to show which way the data answer an internal question, so pragmatic considerations have a role to play; but answering an internal question is at least a rule-governed activity, even though the rules may leave us with various options. In answering an external question, by contrast, there are no rules to which we can appeal – for it is only when we are within a language that we have rules at all. Thus the epistemological distinction survives, as the distinction between a rule-governed (if not rule-determined) activity, and a non-rule-governed activity.

This response reveals an idea always present in Carnap's thought as support for, or as an alternative to, the ideal of rational adjudicability: that internal questions have an epistemological status different from that of external questions just because they are internal, and thus rule-governed. The weakness of this idea, however, is that the issue under discussion is whether rules of language have an epistemological status different in principle from that of statements within the language. We can hardly support this distinction by saying that in the one case there are, and in the other case there are not, rules to which we can appeal. The status of these rules is what we are trying to settle, and to suppose that the difference between rule-governed and non-rule-governed activities marks an epistemological distinction is not to settle this question but to beg it.

If the ideal of rational adjudicability were reasonable as an abstract account of actual epistemic practice, this would be because we did in fact, in our justificatory procedures, act differently towards rules of language, on the one hand, and statements within a language, on the other hand. Carnap, in the passage most recently quoted, seems to concede that our justificatory practices show no such distinction. If this is correct, then no method of analysing those practices can create such a distinction.

Carnap wishes to give a plausible and realistic account of science, and also to show that there is an epistemologically significant difference between the analytic and the synthetic. If there is no such epistemological cleavage in our actual procedures, then these two goals conflict irremediably. Yet Carnap cannot respond to this situation by simply giving up the analytic-synthetic distinction. This distinction, and those between the internal and the external, the pragmatic and the confirmational, are integral to what he means by an *account* of science.[15] Any attempt to ask, in a normative spirit, about

the justification of a scientific theory as a whole demands that we have some standard of justification which is independent of that theory. For Carnap it is the choice of a language which fixes this standard. If this choice is made on the same basis as that of a statement within science, then we have no standpoint from which we can ask the normative question. From Carnap's point of view, therefore, the analytic-synthetic distinction is essential to the kind of account of science which philosophy aims to provide, and so essential to philosophy itself (see Schilpp, p. 922). Thought of in this way, Quine's attack on the analytic-synthetic distinction involves a reconceiving of what it means to give an account of science. The normative aspect falls away. The task becomes purely one of saying how it comes about that human beings come out with the verbal and other production that constitute theory science. This task is undertaken in a simply naturalistic spirit; far from being an attempt to judge science from without, it is itself a part of science.[16]

<div align="center">2.</div>

In the previous section I put forward an argument against the view that the analytic-synthetic distinction marks a crucial epistemological distinction. This argument, if successful, settles what I take to be the central issue in dispute between Carnap and Quine, for it shows that the notion of meaning cannot provide us with a vantage point which is independent of empirical knowledge, and from which we could pass judgment upon empirical knowledge. Although the argument thus shows meaning to be a philosophically less important notion than many have thought, it does not obviously depend upon or involve any scepticism about meaning or translation. *Prima facie*, at least, it is consistent with the argument to suppose that the notion of meaning makes perfectly good sense. In this section I shall consider Quine's general scepticism about meaning. This scepticism appears as a part of the debate with Carnap over analyticity but, if my claims so far are correct, is not essential to the most fundamental issues at stake in that debate.

I shall begin by comparing Carnap's attitude towards language with Quine's. Carnap's attitude can be gathered from this passage in his 'Autobiography':

Neurath emphasized from the beginning that language phenomena are events *within* the world, not something that refers to the world from outside. Spoken language consists of sound waves; written language consists of marks of ink on paper. Neurath emphasized these facts in order to reject the view that there is something "higher", something mysterious, "spiritual" in language, a view that was prominent in German philosophy. I agreed with him, but pointed out that only the structural pattern, not the physical properties of the ink marks, were relevant for the function of language. Thus it is possible to construct a theory about language, namely the geometry of the written pattern. This idea led later to the theory which I called "logical syntax" of language. (Schilpp, p. 29)

Carnap wished *both* to insist that language is a natural phenomenon, on a par with any other; *and* to bring to the study of this phenomenon a barrage of distinctions: between pragmatics, semantics and syntax; between pure and applied syntax, between pure and applied semantics.[17] Pragmatics is concerned with the *use* of language; but we can abstract from this feature and study the relation between expressions and the objects they designate, without regard for the actual users of the language: this is semantics. Again, we can abstract from both pragmatics and semantics and simply study the expressions of the language: this is syntax. If we study the semantics and syntax of an actual language (descriptive semantics and syntax), then our conclusions are answerable to the use of that language. But there are also subjects called pure semantics and pure syntax, which are independent of the actual noises or marks that people make:

... descriptive semantics and syntax are, strictly speaking, parts of pragmatics.

With respect to pure semantics and syntax the situation is different. These fields are independent of pragmatics. Here we lay down definitions for certain concepts, usually in the form of rules, and study the analytic consequences of these definitions. In choosing the rules we are entirely free. Sometimes we may be guided in our choice by the consideration of a given language. ... But this concerns only the motivation of our choice, and has no bearing on the correctness of the results. ... (*Semantics*, p. 13)

Within the realm of pure semantics and syntax we set up a semantical system, with a complete apparatus of semantical and syntactic rules, which enables us to prove theorems about the language thus defined. If we now find that (some fragment of) a natural language approximates to our semantical system, then we may claim that the system is a rational reconstruction of that (fragment of the) language.

Carnap's attitude is that since the semantical system is our own creation, we can employ whatever concepts we choose in setting it up. If the creator of the system says that semantical and syntactic rules are true by meaning, then they are. This attitude would be

perfectly consistent if it could be maintained that questions about the
choice of a language have a different epistemological status from
questions which arise within a language. For this would allow us to
use what terms we choose to analyse a language, without concerning
ourselves with the justification of those terms. Exactly this argument
is employed by Carnap at the end of *Meaning and Necessity*, when he
defends the methods used in that book:

> The different conceptions of other authors discussed in this book ... have sometimes
> been regarded as different theories, so that one of them at most could be right while all
> the others must be false. I regard these conceptions rather as different methods. ... Our
> differences are mainly practical differences concerning the choice of a method for
> semantical analysis. (p. 204)

This defense relies upon the idea that the choice of language or
method is a matter for practical decision, and thus, unlike theoretical
issues, not a matter of right and wrong. As we have seen in Section 1,
however, no justification has been given for the epistemological
distinction which is assumed here. So the language we choose – or the
meta-language in this case, since our subject is itself language – is as
much in need of justification as is our preference for one theory
rather than another within the language.

Like Carnap, Quine insists that language is a natural phenomenon
like any other. Unlike Carnap, Quine concludes from this that lan-
guage is to be studied in those terms which have proved their worth in
the study of other natural phenomena – i.e., the terms of natural
science or of "physics in the broadest sense" as he also puts it,
meaning physics, chemistry, biology and behavioristic psychology.
When Quine complains that no definition has been given for "mean-
ing" and related terms, his charge is that no definition has been given
in terms drawn from these established sciences, and that nothing less
will do if we are serious about treating language as a natural
phenomenon. Seen in this way, Quine's charge is that Carnap is not
consistent and thorough enough in his empiricism and his physi-
calism, because he does not subject the study of language to their
standards.

Carnap does not accept in general that a semantical term must be
defined in behavioral terms before we can legitimately employ it.[18]
Nevertheless, he attempts to meet Quine on his own ground by
providing such a definition of meaning. Carnap adopts the scenario
which Quine had already used in 'Meaning in Linguistics'[19]: that of a

linguist going off to study and translate a hitherto unknown language. The translation will take place on the basis of behaviour (verbal and non-verbal) of the speakers of the language, for no other basis is available. If the linguist's enterprise is a feasible one, translation, and thus meaning, must have a behavioral basis. Carnap's argument is brief, perhaps because it seems obvious to him that its conclusion is correct – for it seems undeniable that successful translation does take place:

It seemed rather plausible to me from the beginning that there should be an empirical criterion for the concept of the meaning of a word or phrase, in view of the fact that linguists traditionally determine empiricaly the meanings, meaning differences, and shifts of meaning of words, and that with respect to these determinations they reach a measure of agreement among themselves which is often considerably higher than that reached for results in most of the other fields of the social sciences. (Schilpp, pp. 919–20)

Quine does not, of course, deny that translation takes place, and that within the enterprise of translation there are standards of correctness. But he does deny that the truths of translation are truths in the full objective sense. Now this is something that can hardly make sense unless it is contrasted with some conception of 'truth in the full objective sense' – i.e., we have to see what *does* count as truth in the full sense in order to see what it means to say that the truths of translation do not. In Quine's case the opposing conception is given by natural science, by "physics in the broadest sense":

I speak as a physicalist in saying that there is no fact of the matter [about translation]. I mean that both [translation] manuals are compatible with the fulfillment of just the same elementary physical states by space-time regions.[20]

The indeterminacy thesis is thus that wherever translation is possible it is non-unique, in the sense that there will be alternatives, *compatible with natural science*, to any given scheme of translation, even including the limiting case of the 'translation' of English into English. What does this thesis come to? It should be emphasised that it is a purely abstract thesis. Quine no more believes that we might in fact find an alternative translation of some language than he believes that we might find an alternative physics, observationally equivalent to our own.[21] Finding one was extraordinary and fortuitous; to expect another would be to expect to surpass the limits of human possibility. But Quine is not concerned with the humanly possible; his is an abstract point about the status of the truths of translation, and thus of truths

about meaning. The point is that there is a gap between even the most advanced physical theory and our "theory" of people and what they mean. To put it in its starkest form, we could even say that natural science does not talk about meaning, and translation manuals do not talk of the distribution of elementary physical states over space-time regions; so, inescapably, the truths of science do not determine the choice of a translation manual.[22]

The gap between our theory of physics and our theory about meanings must be filled by the creativity of the linguist. And each of us is, in his or her own more or less unconscious fashion, a linguist who must be creative in this way. In listening to our neighbours or our colleagues, our situation, from Quine's abstract point of view, is that of linguists in the jungle: all we have to go on is the behavior, verbal and non-verbal, of those whom we seek to understand. Our decisions in this regard are not capricious, but are guided by the usual considerations of simplicity, fruitfulness, and so on. A self-conscious linguist may formulate some of these as methodological maxims for use in the jungle. The point of indeterminacy is *not* that these maxims, and other implicit methodological considerations would be insufficient to determine translation if they were themselves determined. It is, rather, that the maxims which a linguist uses are not determined by physics. Different maxims, yielding different translations, would be equally compatible with physics; it is the use of just *these* maxims which is left indeterminate even after physical theory is fixed. That this is so is clear from a paragraph in which Quine is discussing the famous (and misleading) "gavagai" example:

> An actual field linguist would of course be sensible enough to equate "gavagai" with "rabbit", dismissing such perverse alternatives as "undetatched rabbit part" and "rabbit stage" out of hand.... The implicit maxim guiding his choice of "rabbit"... is that an enduring object moving as a whole against a contrasting background, is a likely reference for a short expression. If he were to become conscious of this maxim, he might celebrate it as one of the linguistic universals, or traits of all languages, and he would have no trouble pointing out its psychological plausibility. But he would be wrong; the maxim is his own imposition, toward settling what is objectively indeterminate. It is a very sensible imposition, and I would recommend no other. But I am making a philosophical point.[23]

The philosophical point is that the linguist's maxims, however "sensible" and whatever their "psychological plausibility", are not a part of physics, and do not follow from physics.

At this point the critic of indeterminacy is likely to press not the

contrast but the comparison with physics. The truths of translation do
not follow uniquely from their data; but neither, on many accounts,
do the truths of physics. Should we then conclude that translation is
no worse off than physics? Quine's answer is no. The linguist's maxim
was said to be "his own imposition, toward settling what is objec-
tively indeterminate". The same cannot be said of the physicist's
maxims, however, because without physics there is no conception of
what is objectively the case, and thus no sense to calling something
"objectively indeterminate"; physics is what tells us what is objec-
tively the case. This is what Quine means by saying "theory in
physics is an ultimate parameter. There is no legitimate first philoso-
phy, higher or firmer than physics".[24]

Quine's thesis of the indeterminacy of translation, and so also his
rejection of the notion of meaning, is thus based upon the view that it
is physics (in the broadest sense) which tells us about the ultimate
nature of reality. What there really is to language, from this physi-
calistic perspective, is the fact that members of a certain species have
certain verbal dispositions – tend to make certain kinds of noises in
certain kinds of circumstances. The question is whether we can apply
such a notion as meaning to language. From a Quinean point of view,
it would be an understatement to say that the burden of proof is upon
those who think that we can, for what they have to prove is some-
thing that is *prima facie* almost incredible. Here the animals making
their noises, there the notion of meaning; the gap between them is
surely too wide for any sensible person to suppose it bridgeable.
Quine has been much criticized for failing to provide convincing
arguments for the indeterminacy of translation. His critics on this
score have perhaps failed to appreciate the physicalistic world-view
from within which indeterminacy is almost too obvious to require
argument – although some of them profess to share this view.
Whether we are to accept this world-view is of course a separate
issue; my claim at the moment is only that Quine's rejection of
meaning, and his advocacy of indeterminacy, cannot be understood
apart from it, and are its natural, if not inevitable, concomitants.

3.

In this paper I have considered two arguments. I have emphasised
that the second depends upon Quine's physicalistic metaphysics,
whereas the first makes no such large-scale presupposition. Exactly

what each argument shows, or purports to show, requires careful statement. If one holds that the analytic-synthetic distinction must be of general epistemological significance, then one will say that this distinction is cast in doubt by the first of the two arguments which I have separated. In this case, the second argument will apear as an attack upon the notion of meaning, not upon the analytic-synthetic distinction. If, on the other hand, one says that analyticity is simply truth in virtue of meaning and so must be acceptable if the notion of meaning is acceptable, then one will say that the first argument attacks the significance of the analytic-synthetic distinction while the second attacks the very distinction itself. In view of the history of the subject, the former of these two descriptions seems to me less likely to mislead; but in any case the situation, I hope, is clear. It is worth noting that neither of my two descriptions is straightforwardly that of the main protagonists of the debate over analyticity. Carnap always seems to assume that the analytic-synthetic distinction marks a significant epistemological distinction and must be acceptable if the notion of meaning is. Quine seldom explicitly questions this dual assumption.[25] One of my aims in separating the two arguments is to suggest that this assumption is not inevitable, or even particularly plausible. The first argument was intended to show that the philosophical use of the analytic-synthetic distinction is vulnerable to considerations which do not result in a general scepticism about meaning. If I have succeeded, I have shown that one can consistently agree with Quine that the analytic-synthetic distinction cannot do the philosophical work that Carnap and others wanted from it, while rejecting Quine's doctrine of the indeterminacy of translation.

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NOTES

[1] My conclusion is thus close to that reached by Hilary Putnam in 'The Analytic and

the Synthetic', *Collected Papers*, Vol. 2 (Cambridge: Cambridge Univ. Press, 1975). The methods by which we reach this conclusion are quite different but, I think, compatible.

[2] Provided both languages have the power to refer to all sense data and to express all logical relations.

[3] R. Carnap, *The Logical Syntax of Language* (London: Routledge and Kegan Paul, 1937; first German edition 1934), p. 322. Hereafter abbreviated as "*LSL*".

[4] This notion of analyticity is very closely akin to Wittgenstein's notion of a tautology in the *Tractatus*. See, e.g., *LSL*, p. 44, for some indication of the historical relations here.

[5] See P. Schilpp (ed.), *The Philosophy of Rudolf Carnap* (La Salle, Ill: Open Court, 1963), p. 47. Hereafter abbreviated as "Schilpp". See also *LSL* Part V on the status of philosophy.

[6] See R. Carnap, Die alte und die neue Logik', *Erkentnis* 1; translated in A.J. Ayer (ed.), *Logical Positivism* (New York: The Free Press, 1959).

[7] R. Carnap, *Philosophy of Science*, 1936–37; hereafter abbreviated as "T&M". It should be noted that what is there called "reducibility" does not imply eliminability, and so is much weaker than what I am calling "reductionism".

[8] R. Carnap, *Meaning and Necessity* (Chicago: U. of Chicago Press, 1947; second edition, Chicago: U. of Chicago Press, 1956), pp. 12, 28. Hereafter abbreviated as "*M&N*".

[9] R. Carnap, 'Empiricism, Semantics and Ontology', reprinted in *M&N*, p. 208.

[10] I hope it is clear that in what follows I am not attempting to explain Carnap's complex and changing views about confirmation. At most, I am talking in a simplified fashion about the picture which motivates those views.

[11] Thus inductive logic, like deductive logic, is for Carnap not a theory within a framework but part of the framework, and so analytic. See R. Carnap *Logical Foundations of Probability* (Chicago: U. of Chicago Press, 1950), pt. V.

[12] It is at this point in the argument that a role is played by Quine's invocation of Duhem's thesis or (equivalently, I take it) the failure of reductionism. Two matters are worth noting. First, what is important here is not the failure of *language-neutral* reductionism, but rather the failure of strict (eliminative) reduction even within a language adequate for science. On any reasonable understanding of the observational, reduction to the observational will be impossible, even if we waive the demand that "the observational" be language-neutral. Internal reductionism (as we might call it) is of course a weaker thesis than language-neutral reductionism; but the arguments against the latter are also convincing against the former. Secondly, Carnap himself accepted Duhem's thesis (see *LSL* p. 318). This gives some indication of the difficulty of saying exactly what Carnap and Quine disagreed about.

[13] W.V. Quine, *From a Logical Point of View* (Cambridge, Mass.: Harvard U. Press, 1953; New York: Harper and Row, 1961), p. 46. Hereafter abbreviated as *FLPV*.

[14] Ricketts has persuaded me that the Carnapian response at this point must be taken seriously.

[15] Here again, I am rather hastily summarising Carnap's complex views.

[16] See W.V. Quine 'Epistemology Naturalized' in *Ontological Relativity and other Essays* (New York: Columbia U. Press, 1969). The latter hereafter abbreviated as "O.R.".

[17] See R. Carnap, *Introduction to Semantics* (Cambridge, Mass.: Harvard U. Press, 1942) sections 4 and 5. Hereafter abbreviated as "*Semantics*".

[18] See R. Carnap, '*Meaning and Synonymy in Natural Languages*', reprinted in *M&N*, p. 235.

[19] Reprinted in *FLPV*, pp. 47–64.

[20] W.V. Quine, 'Facts of the Matter' (in *American Philosophy from Edwards to Quine*, eds. R.W. Shahan and K.R. Merrill, Norman, OK.: U. of Oklahoma Press, 1977) p. 194.

[21] On the underdetermination of scientific theories by their data, see Quine's 'On Empirically Equivalent Systems of the World', *Erkenntnis* 9 (1975) 313–328.

[22] It has not been widely appreciated that the doctrine can be phrased in this stark form. This is due, in part, to the impression conveyed by Chapter Two of *Word and Object* that *some* translation – that of observation sentences, for example – is determinate. If this were Quine's view, then the stark formulation would be incorrect. But later works make it clear that all translation is indeterminate, because the translation of assent and dissent is indeterminate. See, e.g., *Words and Objections* (eds. D. Davidson and J. Hintikka, Dordrecht, Holland: D. Reidel, 1969), p. 312.

[23] *O.R.* p. 34.

[24] *Words and Objections*, p. 303.

[25] At moments, however, Quine seems to suggest that there might be a notion of analyticity which would not carry drastic epistemological implications. See *Roots of Reference* (La Salle, Ill.: Open Court, 1973), section 21; and 'Carnap and Logical Truth', published in Schilpp, p. 403. (This essay is reprinted in *Ways of Paradox*, New York: Random House, 1966 and Cambridge, Mass.: Harvard U. Press, 1976. The passage referred to is on p. 122 of the 1966 edition, and on p. 129 of the 1976 edition.)

Note added in proof: A version of the "unpublished writing" of Ricketts to which I refer in Note 1 is now published under the title 'Rationality, Translation, and Epistemology Naturalized,' *The Journal of Philosophy* 79/3.