HOWARD STEIN

WAS CARNAP ENTIRELY WRONG, AFTER ALL?

In suggesting that Carnap was not altogether wrong, I do not by any means intend to imply that he was in every respect right. But the criticisms brought against the logical empiricists' program, and against Carnap in particular, by Quine in the early 1950s are quite generally regarded as having been successful – not just in the historical sense (namely, that those criticisms were of great influence upon philosophical opinion, and may even be said to have initiated the decline of logical empiricism as a living enterprise), but in the substantive philosophical sense that this entirely favorable reception of Quine's criticisms was warranted. I have occasionally remarked that in my own view, something valuable was lost in this dismissal of logical empiricism; and that is the theme I wish here to elaborate upon. More exactly, there are three interrelated themes to be treated: (1) the question of the cogency of Quine's critique (and of some of his related positive views); (2) some of the weaknesses, and some of the strengths, of Carnap's position as I understand it; and (3) the character of Carnap's philosophy, as it is manifested in his later writings. I include this third topic because I believe that Carnap is a far subtler and a far more interesting philosopher than he is usually taken to be.

The attack by Quine in 1951 was directed against three points: the two famous 'dogmas of empiricism', and the views on ontology that Carnap had expressed in his paper 'Empiricism, Semantics, and Ontology' (Carnap 1950). Of these, the most celebrated is the critique of the concept (or alleged concept) of the analytic, which was renewed in Quine's contribution to the Schilpp volume on Carnap. I want first to comment on a passage in this latter source – the article 'Carnap and Logical Truth' (Quine 1960) – as the later and presumably more considered statement of Quine's criticism.

The attack upon the notion of analyticity was, of course, an integral part of Quine's general critique of the concept of meaning; and in 'Two Dogmas of Empiricism', Quine tells us that the two dogmas there exposed – that of the analytic/synthetic distinction, and that (in his terminology) of 'reductionism' – are at root identical: both depend

Synthese **93**: 275–295, 1992.

upon a specious distinction between "a linguistic component and a factual component in the truth of any individual statement" (Quine 1963, pp. 41–42). "If this view is right" Quine says (ibid., p. 43), "it is misleading to speak of the empirical content of an individual statement"; and he adds, "especially if it is a statement at all remote from the experiential periphery of the field" (that is, of 'total science', which Quine has just analogized to "a field of force whose boundary conditions are experience").

Now, here is a passage from 'Carnap and Logical Truth'; "Already the obviousness (or potential obviousness) of elementary logic can be seen to present an insuperable obstacle to our assigning any *experimental meaning* to the linguistic doctrine of elementary logical truth" (Carnap et al. 1963, p. 389; the emphasis is mine). How is one to understand this demand for 'experimental meaning', in the light of Quine's rejection of the dogma of reductionism?

It may be that the answer has to take account of another Quinean notion or rubric: that of fact of the matter. In an illuminating discussion of this notion in the Schilpp volume on Quine – one that is heartily endorsed by Quine himself in his reply – Roger Gibson tells us that Quine's use of this expression does not pertain to methodology (or epistemology); rather, "Quine's understanding of this term is decidedly *naturalistic* and *physicalistic*" (Gibson 1986, p. 143). Again: "Facts of the matter belong to the ontological phase of inquiry, not to the epistemological phase. *Ontology* is the theory of what there is .... Ontology is that theoretical structure that links past and present sensory stimulations to future ones – it is a theory of objects" (ibid., p. 151); and: "Ontology and epistemology are concerned with different issues. Ontology focuses on the issue of what there is; and what there is is a question of *truth*" (ibid., p. 147). The context of Gibson's discussion is Quine's doctrine that there is no fact of the matter – and Gibson particularizes, in view of Quine's physicalism, "no *physical* fact of the matter" (emphasis added) – to translation; thus, to assertions of sameness of meaning. In short, then, one might consider Quine's point to be that what he calls 'the linguistic doctrine of logical truth' (but Carnap prefers to call that of 'truth based on meanings') cannot be construed as saying anything about the physical world.

If that is really what Quine intends, he has surely not said it with precision. Indeed, his 'insuperable obstacle' was to the assignment of

experimental, not of physical, meaning to Carnap's doctrine; and that, surely, suggests a methodological rather than an ontological concern.

In any case, Quine's objection seems to me to pose very serious puzzles. His demand for 'experimental meaning' not only seems on the face of it, if construed as the methodological (or epistemological) demand suggested by its wording, to rest upon some version of the dogma of 'reductionism': it stands on *either* reading in a very strange relation to the position Carnap himself takes on the status of his own doctrine. I must now attempt to explain this.

Just after posing the objection under discussion, Quine says the following:

> The philosopher, like the beginner in algebra, works in danger of finding that his solution-in-progress reduces to '0 = 0' Such is the threat to the linguistic theory of elementary logical truth. For, that theory now seems to imply nothing that is not already implied by the fact that elementary logic is obvious or can be resolved into obvious steps. (Carnap et al. 1963, p. 389)

Carnap, in his reply to Quine, subjects the last sentence to a (perhaps heavy-handed, but in my opinion not entirely unmerited) satirical analysis (ibid., pp. 916–17), the gist of which is that since his theory implies itself, it follows from Quine's statement that Carnap's theory – which Quine rejects – follows from a fact that Quine asserts. But there is a little more to this than a joke at the expense of a glib Quinean aphorism.

What, after all, does reduction to '0 = 0' really mean? Quine, of course, is thinking of a student who is trying to solve an equation, and who fails to obtain an equivalent from which the value of an unknown quantity can be determined. But if the original equation can in fact be *reduced* to '0 = 0', in the sense of being shown not merely to imply, but also to be implied by this latter, then of course one has established by the reduction that the original equation is an identity – in other words, one has proved a theorem of algebra. In Carnap's language, such an identity is called 'analytic'; so in Carnap's own terms, Quine's refutation of Carnap's doctrine is to be seen as a proof that that doctrine is true, and, indeed, analytic.

The objection Quine has posed to Carnap's view of logical truth (and, more generally, of analytic truth) is one that has been raised by others against the so-called verifiability theory of meaning (one is reminded of Quine's diagnosis: that the two dogmas are at root identi-

cal). In the latter case, the allegedly unanswerable question is, "How can the principle of verifiability itself be verified?" – in other words, as one might put it: What is the experimental meaning of that principle? Carnap's answer is the same in both cases: the doctrine of analyticity, and the doctrine (in so far as he maintains one) of empirical meaning, are both – formally considered – analytic.

At this point, I have the uncomfortable feeling that my discussion may seem to the reader – and threatens to become by my own lights as well – scholastic, or talmudic, in the pejorative sense of those words. It is not my intention to pit a representation of the issue in Carnapian terms against a representation in Quinean terms, for the mere purpose of doctrinal hair-splitting. Let me try to explain what I think Carnap's view of the matter really amounts to.

A historical remark may be helpful here. (I am about to make what will probably be my only eyewitness contribution to the history of philosophy.) In 1951, Quine read a paper to the departmental colloquium of the Philosophy Department at the University of Chicago containing the criticisms prompted by Carnap's (then recent) article on questions of ontology. The first main conclusion of that paper (part of which has appeared in print, as 'On Carnap's Views on Ontology' – Quine 1951a) is that the distinction called by Quine that between 'category questions' and 'subclass questions' – his own rephrasing of Carnap's distinction between questions 'external' and 'internal' to a 'linguistic framework' – is both ill grounded and unnecessary; that is, unnecessary for Carnap himself. He expressed the hope that he could persuade Carnap of this latter point; for, he said, no more is needed for Carnap's own philosophical purpose than the distinction between analytic and synthetic. Quine then proceeded to explain that this first elimination was not the end of his dissent; that, indeed, it had just led to the basic point of contention – namely, "the distinction between analytic and synthetic itself". In the paper as published, the further discussion of this basic point is waived in favor of a reference to 'Two Dogmas'.

So far, of course, I have said of this occasion only what can be read in the public record. My original contribution to the history concerns the discussion that followed Quine's talk. For I was present at the colloquium, and I found that discussion not only interesting, but even in a sense inspiring; and was later distressed at the aftermath.

Carnap's summary of the issue between Quine and himself was on

the following lines: "Quine", he said (I am not quoting verbatim, but giving the gist as I remember it), "and I really differ, not concerning any matter of fact, nor any question with cognitive content, but rather in our respective estimates of the most fruitful course for science to follow. Quine is impressed by the continuity between scientific thought and that of daily life – between scientific language and the language of ordinary discourse – and sees no philosophical gain, no gain either in clarity or in fruitfulness, in the construction of distinct formalized languages for science. I concede the continuity, but, on the contrary, believe that very important gains in clarity and fruitfulness are to be had from the introduction of such formally constructed languages. This is a difference of opinion which, despite the fact that it does not concern (in my own terms) a matter with cognitive content, is nonetheless in principle susceptible of a kind of rational resolution. In my view, both programs – mine of formalized languages, Quine's of a more free-flowing and casual use of language – ought to be pursued; and I think that if Quine and I could live, say, for two hundred years, it would be possible at the end of that time for us to agree on which of the two programs had proved more successful".

This view of the matter might certainly be expected to be congenial to Quine, with the 'shift toward pragmatism' he signalized as one of the principal consequences of the critique in 'Two Dogmas' (Quine 1963, p. 20); and, indeed, as I recall, Quine happily assented to Carnap's diagnosis. I say that I found the discussion inspiring. The other dominating figure in the Chicago Philosophy Department at the time, besides Carnap – and in the politics of the department the single dominating figure – was Richard McKeon, who maintained that differences of philosophical principle are invariably irremediable – to be understood, in terms of a classification of the possible coherent philosophical stances, but never to be resolved: philosophers with divergent principles were doomed to talk at cross-purposes. And here, in McKeon's very presence, were two eminent philosophers who had come to an agreement not only about the character of their own disagreement, but about the conditions under which it could in fact be resolved. As to my later distress, I take it that hardly requires comment. I have never understood why Quine continued to argue his case against Carnap with no suggestion that the issue concerned the fruitfulness of a program, and not the tenability – or intelligibility – of a doctrine.

A proper assessment of Carnap's philosophy demands, first, that one

understand the general character of his program; only then do there arise questions of evaluation, both of particular proposals within the program, and of its more general outlines. A first point about the general program seems in need of special emphasis – for the confusions (as I take them to be) inherent in both Quine's demand for the 'experimental meaning' of Carnap's views on logical truth, and the corresponding demand for 'verifiability' of the verifiability principle, turn on a failure to appreciate this point. In his later writings (beginning, I think, with the work on the theory of logical probability) Carnap used the term 'explication' for the activity of philosophical clarification. An explication is a proposed exact characterization of a concept. If the proposal is adopted, the concept so characterized is an explicatum – that is, an 'explicated' notion. There is a perhaps somewhat delicate issue whether the explication is to be regarded as a clarification of a notion already present before the explication has been achieved. In typical cases, something like this is so; and Carnap calls the preexisting, not fully clarified notion the *explicandum*: 'that which requires explication'. That this state of affairs involves the well-known 'paradox of analysis' is clear – indeed, this is precisely the paradigm situation of that alleged paradox; I need hardly elaborate. That on the other hand there have *actually occurred* what should quite reasonably pass for successful explications in this full sense seems to me uncontroversial; for instance, although Quine is unhappy with Carnap's account of logical truth, he is famously happy with first-order predicate logic – and would presumably agree that the exact construction of this system clarified the preexisting, insufficiently clear and precise, notion of logical inference.

Now, Carnap's distinction between 'external' and 'internal' questions, which was introduced in his paper on ontology and is deprecated by Quine, has – if one accepts it (which means: if one agrees to use it) – an obvious application to the process of explication in general. The explicatum, as an exactly characterized concept, belongs to some formalized discourse – some 'framework'. The explicandum – if such there is – belongs ipso facto to a mode of discourse outside that framework. Therefore *any* question about the relation of the explicatum to the explicandum is an 'external question'; this holds, in particular, of the question whether an explication is adequate – that is, whether the explicatum does in some appropriate sense fully represent, within the framework, the function performed (let us say) 'presystematically' by the explicandum.

In saying that the 'linguistic doctrine of logical truth' or the 'verifiability theory of meaning' is, if adopted, analytic, Carnap would be making a statement about the standing of a certain proposition within a formalized system – or, rather, in a sketch of a *family* of *projected* formal systems: devised, namely, to serve as a 'framework' for *Carnapian linguistic theory itself* (whether of 'constructed' languages or of 'natural' languages or both). That the truth of the corresponding propositions within those systems is *trivial* – imposes no restriction upon the world – is something consequent upon the characterization of the framework itself. Whether one should adopt linguistic, or theoretical, frameworks that are characterized in such a way as to embody those propositions as analytic is a question (as Carnap sees it) of quite a different type, and by no means trivial. Indeed, just such a non-trivial question is at issue in the controversy with Quine.

When I said, earlier, that the issue whether an explication is to be regarded as 'of' some presystematic explicandum is a little delicate, I had in mind not only the lack of a clear and uniform criterion for assessing external questions in general and questions of the adequacy of explications in particular, but also another point – not, I think, made by Carnap, but which I should like to propose in (or should I say 'to'?) his spirit. The question of the nature of 'presystematic notions' is obviously very complex – and somewhat vague. I don't know to what science one should say it belongs: psychology? sociology? – Quine, I dare say, would assign it to naturalistic epistemology. But I should expect Quine to agree that the notion of a presystematic notion is by its very nature vague – and perhaps usefully vague. It would be easy to cite cases in which a notion of this type – or at any rate, a word in general use – can be said to have been explicated by more than one precise explicatum. The other possibility is that a newly proposed exact concept does not correspond very well to any presystematic notion at all. It seems not to be a violation of linguistic propriety – at least, of Latin propriety – to call such a concept an 'explicatum': meaning, again, an explicated – that is, clarified (in other words, simply a clear) – concept. If one asks what such an explicatum is the explication of, more than one reply is possible. One can say that the exact characterization proposed is just the explication of the very concept in question (as a definition defines the concept whose definition it is); or that it explicates a presystematic idea, not previously in general use, but vaguely entertained by the inquirer when groping for clarity. I hope it

is clear that all this is peripheral: what counts in the end – still in Carnap's view of things – is the clarity and the utility of the proposal; whether part of that utility has to do with an earlier, vaguer, general usage is distinctly a secondary matter.

This has an immediate bearing upon one aspect of the issue raised by Quine concerning the analytic. Quine was of course deeply concerned with empirical semantics – with the theory of 'natural languages'; and a rather large part of his challenge to Carnap was based upon the contention that there is no presystematic notion of analyticity in natural languages. Carnap appreciated the importance of the study of natural languages, and hoped that the semantical theories he was developing would be of use in empirical linguistics. In his reply to Quine, in the Schilpp volume, he therefore "accepted [Quine's] challenge to show that an empirical criterion for intension concepts with respect to natural languages can be given" (Carnap et al. 1963, p. 919). Observe that two questions may be distinguished here: whether such concepts are embedded in ordinary usage – so that, for instance, there might be an 'ordinary' notion of 'truth based on meanings' to serve as the explicandum for an explication; and whether such concepts can usefully be introduced as part of the technical apparatus of the theorist – as 'terms of art'. But the answers to these questions, however interesting they may be for empirical linguistics, have no bearing whatever upon either the difference in general between Quine's approach and Carnap's to philosophical issues, or the issue in particular of the viability, or utility, of the distinction between the analytic and the synthetic in formalized languages. I emphasize this because I believe that Carnap, in generously accepting the challenge posed by Quine on the empirical side, has failed to make sufficiently clear the difference between the two sorts of issues, and the important fact that the standing of his notion of the analytic as it relates to his program for scientific/philosophical explication is a matter entirely independent of the question about natural languages.

This is not to say that Carnap was 'right' about the analytic/synthetic distinction. I have so far only been trying to clarify what the crucial question is – or, rather, what it is not; for we have not yet seen what it is.

Once again, it is necessary to make some distinctions. I believe it is clear that Carnap's intense concern with questions about the analytic and synthetic derives from Kant, from Frege, and from (early) Witt-

genstein. In all three of these philosophers, the primary interest of the notions is epistemological; the analytic, in particular, is that species of truth a priori knowledge of which is unproblematic, because the truth itself is trivial. In the case of Frege, to be sure, the matter is a bit ticklish, for Frege offers no criterion of analytic truth save that it is purely logical in nature – and offers no criterion of what does or does not genuinely belong to logic. One can see Wittgenstein's theory that analytic truths are tautologies as an attempt to repair this defect in Frege. But of course Wittgenstein's theory will not do, and by the time of his later writings Carnap had long abandoned it – although I do not think he ever abandoned his belief that the classification of a sentence as analytic in some sense 'explains' how we know it. My own opinion is that such purported explanations have served little purpose – that we really do not have a satisfactorily analyzed epistemological 'basis' for any department of knowledge, mathematics and logic included. We have learned – principally from Gödel, also from Skolem and others – that the notion of 'logical triviality' is highly non-trivial. The primitive view – surely that of Kant – was that whatever is trivial is obvious. We know that this is wrong; and I would put it that the nature of mathematical knowledge appears more deeply mysterious today than it ever did in earlier centuries – that one of the *advances* we have made in philosophy has been to come to an understanding of just how deeply puzzling the epistemology of mathematics really is. So on this point, I agree with Quine. (Although Quine speaks of "the obviousness [or potential obviousness] of elementary logic", he tells us that this characterization carries no explanatory value.)

But that is not the end of the matter. I said we must make some distinctions: the hope of solving an epistemological puzzle with the help of the concept of the analytic has failed; but there remain, I think, certain issues for which that notion, or at least something related to it, might still serve an important purpose.

Carnap's late view distinguished between logical truth and analytic truth – the latter being a wider concept (thus, all logical truths were, for him, 'based on meaning'; but not all truths based on meaning were truths of logic). It might be supposed that this extension was motivated by considerations of synonymy in natural languages, and by Quine's skeptical attack upon synonymy; Carnap's paper 'Meaning Postulates', which formally introduced the broader concept, appeared in 1952 and opened with a reference to Quine's example of 'bachelor' and 'unmar-

ried' (Carnap 1952; see Carnap 1956, p. 222). However, Carnap makes clear in that very place that his central concern is another one: "Our explication", he says, "will refer to semantical language systems, not to natural languages"; and adds: "It seems to me that the problems of explicating concepts of this kind for natural languages are of an entirely different nature" (Carnap 1956, pp. 222–23). As to the real concern of the paper, that is also made plain in its introductory section; Carnap says:

> It is the purpose of this paper to describe a way of explicating the concept of analyticity, i.e., truth based upon meaning, in the framework of a semantical system, by using what we shall call *meaning postulates* . . . . It will be shown in this paper how the definitions of some concepts fundamental for deductive and inductive logic can be reformulated in terms of postulates.

In 'Carnap and Logical Truth', Quine dismisses this move rather brusquely; he writes:

> Carnap's present position is that one has specified a language quite rigorously only when he has fixed, by dint of so-called meaning postulates, what sentences are to count as analytic. The proponent is supposed to distinguish between those of his declarations which count as meaning postulates, and thus engender analyticity, and those which do not. This he does, presumably, by attaching the label 'meaning postulate'.
>
> But the sense of this label is far less clear to me than four causes of its seeming to be clear. Which of these causes has worked on Carnap, if any, I cannot say; but I have no doubt that all four have worked on his readers. (Carnap et al. 1963, pp. 404–05)

Earlier, however, Quine had shown more understanding of at least one aspect of Carnap's program. In 'Two Dogmas', after a preliminary indication of the difficulty connected with synonyms, he remarked: "I do not mean to suggest that Carnap is under any illusions on this point. His simplified model language with its state-descriptions is aimed primarily not at the general problem of analyticity but at another purpose, the clarification of probability and induction" (Quine 1963, p. 24).

That purpose remained a very important part of Carnap's concern, and of his wish to deal more adequately with what Quine calls 'the general problem of analyticity' – but here to be understood as the general problem of analyticity for what Carnap calls 'semantical language systems', which he contrasts with 'natural languages' (where, *also*, the general problem of analyticity seems to him of interest, but where it presents problems 'of an entirely different kind').

Let me try to explain what, as I understand it, the philosophical role

is of the notion of a 'linguistic framework', and, relative to such a framework, of the notion of the analytic, in Carnap's later thought. By the time of his later writings, Carnap had clearly abandoned the earlier hopes for the construction of a single and permanent language that should be adequate for all of science. Even in his early work – as early as *Der logische Aufbau der Welt* – he had encouraged the exploration of alternative languages with alternative 'bases', as he called them – physical or phenomenal bases, and, if the latter, ones like his own in that book, taking as fundamental the total field of immediate experience, or ones founded rather upon sense-qualities. By the period of the centrality of 'logical syntax', this openness to alternatives had taken the 'official' form of Carnap's 'principle of tolerance', which concerned both the 'basis' and the mathematical form – the logico-mathematical strength – of the language. But at this time, the hope and indeed confidence were still there that some among the language forms to be considered would prove adequate in the strong sense I have mentioned: for all of science, and permanently. To be sure, Gödel's results had by then demonstrated that no one language could be expected to serve for all conceivable *mathematical* purposes; but Carnap's view pretty clearly was that whatever developments were required in the direction of increasing mathematical strength could just be grafted on to a stable stock or core of empirical language. But in the later writings, this hope has been essentially abandoned – or at least, very fundamentally modified. We shall presently consider a most striking piece of evidence of this change.

What I am suggesting, then, is that alternative possible 'frameworks' are alternative in a very serious sense. What sense? I would put it this way: that a linguistic or theoretical framework envisages a distinct set of possibilities for the world; that alternative frameworks are, in effect, *constitutive* of alternative notions of possibility. This set of possibilities appears in Carnap's earlier semantical works under the guise of linguistic entities, the 'state-descriptions' – sentences of maximal strength (short of logical contradiction). Under the influence of Kemeny, and with special concern for his developing theory of logical probability, Carnap replaced this formal notion by what we may reasonably call the 'ontological' notion of possible states. In terms of this notion, the semantical content of a sentence can be characterized as just the set of possible states with which it is compatible ('in which it would be true'); its logical strength, rather, by the complementary set: the set of states

it excludes. Since inductive logic, as Carnap was attempting to develop it, was to be founded upon the definition of *measures of content* of sentences (or of propositions), these notions clearly were, as he says, fundamental for inductive (as well as deductive) logic.

Now, such an ontological notion of 'the possible' as I have just been discussing is of course extremely uncongenial to Quine. But it is worth pointing out that, if one agrees (under Carnap's principle) to 'tolerate' the notion so far at least as to explore its uses, there results a rather pleasant application to a point that seems, if just briefly and slightly, to have bothered Quine. In his reply, in the Quine Schilpp volume, to the article of Putnam, Quine toys with a challenge posed to him at the end of that article. Putnam referred to Quine's dismissal of the general notion of meaning, and asked how Quine can consistently hold that there is no 'fact of the matter' as to whether (for example) meanings exist (Putnam 1986, p. 425). Quine brushes this off a little breezily:

> One bit that I am going to have to understand for purposes of Putnam's example [Quine says] is the bit that he renders as "Meanings exist." As already remarked, the existence of meanings poses no problem beyond synonymy; they can be taken as equivalence classes. Since I have despaired of making general sense of synonymy, perhaps Putnam is right in supposing that I make no fact of this matter and I am right in not doing so. (Quine et al. 1986, p. 430)

Quine then adds the following remark:

> Dreben once put me a related but more challenging question: is there no fact of mathematical matters? For me, unlike Carnap, mathematics is integral to our system of the world. Its empirical support is real but remote, mediated by the empirically supported natural science that the mathematics serves to implement. On this score I ought to grant mathematics a fact of the matter. But how, asks Dreben, does this involve the distribution of microphysical states? What would there being a largest prime number have to do with the distribution of microphysical states?
>
> Carnap would have said that we have here a contrary-to-fact conditional with an L-false antecedent, 'There is a largest prime number', from which anything and everything follows vacuously as consequent. That avenue is closed to me, but I can still protest that there is no coping with intensional conditionals with wildly implausible antecedents. My suggested standard for facts of the matter is directed rather at concrete situations, and pales progressively as we move upward and outward. Evidently then the upshot is that the factual and the mathematical stand apart, for me as for Carnap; but for me, unlike Carnap, the separation is a matter not of principle but of degree.

I confess this makes me slightly dizzy. Mathematics is serious, and presumably in large part true, for Quine (who does after all take his stand upon the deliverances of science). Yet mathematics occupies a

region too far from the concrete for there to be a fact of the matter. Or is it that there is only a 'pale' fact? How pale? Is it too pale to be discerned at all? It is evidently too pale to be described. Saying that this is 'a matter not of principle but of degree' hardly does justice to the issue.

In any case, Quine's representation of what Carnap would have said in answer to Dreben is stated in just such formally linguistic terms as the semantics I have referred to nicely avoids. The correct late-Carnapian answer to Dreben's question, as I understand Carnap, would be that the class of microphysical states of the world admitted by the proposition that there is a largest prime number is the empty set: the 'fact of the matter' is that that proposition is ruled out by any possible facts whatever.

The notion of a framework, then, with its *envisaged* possibilities, does at least afford us a convenient way of *formulating* statements about the 'ontological', or truth-related, bearings of sentences of a theory – including purely mathematical ones. In his paper on ontology, Carnap emphasized the relativity of this notion to the theoretical framework.

I think it has not been generally understood that, in Carnap's scheme of things, and using the terms I have quoted earlier from Gibson, semantics is fundamentally concerned with 'ontology', and not with 'methodology' or 'epistemology'. This should have been clear from the start, in view of Carnap's tripartite classification of linguistic theory: into syntax – concerned with linguistic entities alone; semantics – concerned with linguistic entities and their relations to what they refer to; and pragmatics – concerned with all the aspects of a language together, including in particular its conditions and modes of use. It should have been apparent that, under this classification, methodology and epistemology belong to pragmatics. But the point was obscured – and seems at first not to have been appreciated by Carnap himself – for two reasons. On the one hand, the liberalization that freed Carnap's philosophy from its former restriction to syntax had been made possible by Tarski's definition of truth, which showed how very general semantical notions could be characterized in a systematic way for formalized languages. There was no corresponding central concept that seemed to serve as an exact systematic foundation for pragmatics; and Carnap thought of the latter as concerned with something like *idiosyncrasies* of use in ordinary languages. On the other hand, Carnap thought – and

this he seems to have continued to hold to the end of his career – that the empirical interpretation of a theory could always be achieved by specifying the *semantics* of the *empirical* part of its language. In effect, the role of pragmatics in this fundamental problem of the analysis of 'empirical content' would be restricted to the single function of distinguishing, within the language, its 'empirical part'. (I take it that the pragmatic character of this distinction is clear – assuming, of course, that the distinction is tenable at all: what part of the language is 'empirical', or what part of its vocabulary refers to the 'observable', is obviously a matter that depends upon something about the *users* of the language.)

But now, with the explicit introduction of the concept of a framework, and the implication that one of the continuing tasks of philosophy will be the examination and evaluation of alternative frameworks, it has to be clear that this activity belongs to pragmatics. Here is a statement on the subject by Carnap, taken from his reply to Charles Morris's article in the Schilpp volume:

> In particular, many problems concerning conceptual frameworks seem to me to belong to the most important problems of philosophy. I am thinking here both of theoretical investigations and of practical deliberations and decisions with respect to an acceptance or a change of frameworks, especially of the most general frameworks containing categorial concepts which are fundamental for the representation of all knowledge. (Carnap et al. 1963, p. 862)

I would sum this up by saying that what in Quine appears as the distinction between concern with ontology – in the sense in which that means whatever relates to 'fact of the matter' – and concern with epistemology, is represented in Carnap as the distinction between the semantics of a framework and the pragmatics of frameworks generally (where by this last expression I mean both pragmatic questions about a single framework, and questions that involve the comparative assessment of alternative frames).

I have given one example of the application of Carnap's semantics to a Quinean question – the explication of the notion of 'fact of the matter' itself. I noted that the concept of 'possible state' involved in that application – and at the center of Carnap's semantics in general – is one Quine would find objectionable. But I must now say that I do not see how he himself can do without it. Quine – to repeat – takes his stand on science, and more particularly on physics. But the concept of the space of all states of a physical system is a central one in much

of classical physics, and in all of quantum physics; and 'all states' means, of course, all possible states. If Quine rejects this notion because of the occurrence of a modality in its description, I do not see how he will accommodate physics. If he accepts this as an innocent use of the term 'possible', that innocent use is all that Carnap needs. The difference between them seems here to be that Carnap is willing to consider alternative frameworks, for alternative theories, with their alternative ontologies and their alternative conceptions of the range of the possible; whereas Quine, asserting ontological relativity, nevertheless holds that we can only conduct our discourse within one or another of competing theories – that is, we cannot find a 'framework' for semantical and pragmatic discussion itself that could serve as a kind of neutral ground. As he puts it in his reply to Gibson: "[W]hichever system we are working in is the one for us to count at the time as true, there being no wider frame of reference" (Quine et al. 1986, p. 157). That remark was made about a situation in which "we have somehow managed to persuade ourselves" that two competing systems – our own, and an 'alien jargon' – are empirically equivalent. Quine says, "[o]ur own system is true by our lights, and the other does not even make sense in our terms". He seems to imply not *only* that there is "no wider frame of reference", but that there is no possible analogue of relativistic invariance.

But the question of empirical content, and of judgments of 'empirical equivalence' of competing systems, raises the issue of Quine's second 'dogma of empiricism': the principle of verification. It must be remembered that Quine does not in fact reject the equation of meaning with (something like) verification – indeed, he says, for example, in 'Epistemology Naturalized', that "epistemology remains centered as always on evidence, and meaning remains centered as always on verification; and evidence is verification" (Quine 1969, p. 89). Rather, in rejecting the verification principle, or (as he calls it) 'reductionism', what he is objecting to is the notion that there is such a thing as the empirical meaning of a sentence. He is affirming his holism in the theory of meaning, and in epistemology – his view that "our statements about the external world face the tribunal of sense experience not individually but only as a corporate body" (Quine 1963, p. 41).

Now, I have remarked that Carnap seems never to have abandoned the view that the empirical content of a language – for him, in the case of scientific languages, always one that has been formally constructed

– can be based upon a part of that language specially distinguished as its 'observational' part; and that the empirical content of the rest of the language can then be analyzed in terms of logical relations (eventually, both deductive and inductive logical relations) to that 'observation sublanguage'. I think, to put it baldly, that this will not work; or to put it more accurately, that it does not work (for conceivably – although I doubt it – some day it will). But in my view the trouble is not the famous 'theory-ladenness' of observation terms. Whatever theory is required for ordinary life is generally quite under control by ordinary people, and what Carnap calls the 'thing language' serves very well for what we ordinarily call observation-reports; that is, there is a kind of 'minimal-theory-ladenness' that occasions no difficulties. I think the real problem is that we have no language at all in which there are well-defined logical relations between a theoretical part that incorporates fundamental physics and any observational part at all – no framework for physics that includes observational terms, whether theory-laden or not. The point can be made by contrasting the character of a typical treatise on some branch of theoretical physics, with that of a work on experimental physics. Theoretical physics can be made to look very much like mathematics; experimental physics cannot. One can argue mathematically in theoretical physics – one can deduce consequences from assumptions; but I cannot think of any case in which one can honestly *deduce* what might honestly be called an observation. What can be done, rather, is to represent (as I have put it elsewhere) 'schematically', within the mathematical structure of a theoretically characterized situation, the position of a 'schematic observer', and infer something about the observations such an observer would have. For example: in ordinary classical astronomical theory, one will represent, say, the planets – including the earth – perhaps as particles, perhaps as extended bodies; putting the schematic observer at a certain latitude and longitude of the earth, and calculating the angles between the lines from that position to the several astronomical bodies under consideration, one will infer that the observer will see those bodies along lines making the corresponding angles with one another. But that is not by any means a deduction of an observation. We have left out, for example, the light by which the observer must see; we have left out the earth's atmosphere, through which the light is refracted. Of course, serious observational astronomy must take account of atmospheric refraction, so perhaps we should put the light – that is, the electromag-

netic field – and the atmosphere into our systematic representation. Are we to do so as well with the observer's telescope? the observer's eye? the observer's brain? – There are two problems here. One is sheer complexity. That one might take to be possible 'in principle' to overcome. The second is that we simply do not know enough to put in everything that would be required for an honest deduction of a genuine observation. Well, perhaps we shall know enough some day – that is why I said that Carnap's program may some day be realizable. But even if that day comes, I doubt that the program will be realizable in practice – that it 'will work' – because the complexity that might be overcome 'in principle' would still be intolerable in practice.

Now, Carnap's scheme for philosophical analysis is admirably suited to just this situation. It is exactly the theories with a highly mathematical structure – the typical theories of physics – that lend themselves, ipso facto, to construction as Carnapian 'frameworks'. The question of the empirical application of such a framework becomes appropriately a question of its pragmatics. I do not know how, systematically, a general theory of such empirical application might be made; but at least I think the problem, in the neo-Carnapian form I have just outlined, finds a suitable locus and an intelligible formulation as a problem. And I think it reasonably clear that to just the extent that we know in practice how to talk about the empirical application of specific physical theories, we can formulate what we know how to say in terms of the pragmatics of a Carnapian framework.

It does not follow from this that 'meaning postulates' must play a role in such systems. It may very well be – I am inclined to think it is – that the possibilities to be contemplated in a framework for theoretical physics as we know it today or as it is likely to develop have to be restricted by the general principles of the theory itself – principles that one would be loth to call 'analytic'. This is a serious modification of Carnap's view. It locates fundamental theory change in change of framework, and therefore outside the scope of the sort of inductive logic Carnap was trying to construct – which itself would, of course, be internal to a framework. That, it seems to me, entails a development of Carnap's views in a direction that I should characterize as 'dialectical'; for it entails a certain blurring of the distinction, dear to Carnap, between the purely cognitive, or theoretical, and the practical. Let me remind you of the passage I have quoted from Carnap's reply to Morris, in which he says that problems concerning conceptual frameworks are

among the most important problems of philosophy, and adds: "I am thinking here both of theoretical investigations and of practical deliberations and decisions with respect to an acceptance or a change of frameworks, especially of the most general frameworks containing categorial concepts which are fundamental for the representation of all knowledge". If we allow these 'categorial concepts' to include categorial concepts both of fundamental mathematics and of fundamental physics – and this, in my opinion, we must do, if we are to make good use of Carnap's notions – then the full force of what I have just called the move to a kind of 'dialectic' of science appears.

There is what seems to me a very odd contrast between Carnap and Quine. Quine, with his epistemological holism, speaks of the 'web of belief', rejecting 'reductionism' with its rigid tribunal sitting in judgment on sentences. He rejects the continuing 'legislative' force of the construction even of a formalized language, insisting that as soon as the language has been created its usages must be allowed to evolve naturally (a kind of Webster's Third International doctrine of scientific language). His pragmatism, and his epistemological naturalism, seem to contrast with the rigidity of Carnap's reliance upon formal constructions and fixed rules. One can describe the contrast in terms favorable to Quine: his refreshingly relaxed manner contrasted with Carnap's more ponderous and rigid ways. An alternative description might be that where Quine in principle leaves all open to the flow of experience – and of that part of experience, in particular, that constitutes the evolution of science – Carnap, having eventually come to recognize that science develops in ways that entail revisions even of 'categorial concepts', wishes to make at least local stands in the midst of this Heraclitean flux, and endorses constructions designed to achieve the maximum possible clarity both in what we say (and our understanding of what we say), and in the basis for the decisions we make. Which is the more acceptable way of describing the contrast of course depends upon one's comparative assessment of the two approaches. But what strikes me as odd is this: it is the rigid Carnap who encouraged Quine to explore his own approach to these basic questions, and – although of course believing his own way to be the better – left it ultimately to future experience to decide. It is Quine the holist who, while denying that the issue was one in which there is a fact of the matter, continued to maintain – and to convince many philosophers – that he was *right* and Carnap *wrong*.

Moreover, if we grant that there is no fact of the matter, it is Carnap who has the clearer way of expressing the nature of the issue itself and what is at stake: it is an external question, concerning the choice of a framework, and ought to be decided by considerations – influenced by all our theoretical knowledge and the clearest understanding we can obtain of our practical/theoretical aims – of the *usefulness* of the alternatives. It is also Carnap who tells us that there is no need to make an exclusive choice – that, subject to practical constraints, it is at least to some extent possible for the same investigator to explore, and even to use, alternative frameworks.

I want in closing to call attention to a further, not unrelated, contrast; and to a feature of Carnap's late views that I have found to be little known – although it appears in a place that ought to have attracted some attention. I refer to Carnap's comments on the paper of Feigl in the Schilpp volume (Feigl, in Carnap et al. 1963, pp. 227–68; Carnap's comments, ibid., pp. 882–86). Feigl has written on physicalism – a doctrine certainly dear to Carnap's heart; and Carnap endorses his friend Feigl's views "in all major points" – but "with some qualifications". These qualifications are extremely interesting; some of them, at least, may be regarded by many as amazing. Carnap first formulates, in several alternative ways, two theses of physicalism (or rather, he formulates the first thesis in four different ways; the second in only one). What the first thesis comes to is that a language form in which all statements are intersubjectively confirmable is sufficient for expressing everything that is 'meaningful for me' – that is, for 'the knowing subject'. This is clearly a formulation of what had long been a basic – perhaps *the* basic – tenet of Carnap's empiricism. The second thesis holds that all laws of nature, including those that apply to organisms, human beings, and human societies, are logical consequences of "the physical laws, i.e., of those laws which are needed for the expression of inorganic processes".

Now, none of this is surprising from Carnap the 'reductionist'. But here is what he goes on to say. In the first place – or, rather, in the second place; but I shall cite it first – he says: "It is true that these two theses of physicalism go far beyond the present possibility of reducing extra-physical concepts and laws to physical ones. These theses do not represent firmly established knowledge but sweeping extrapolating hypotheses". Note that this statement is made not only of the thesis of reducibility of laws to those of physics, but even of the first thesis –

that all that is meaningful for the knowing subject can be expressed in a form of language in which all statements are intersubjectively confirmable. Clearly Carnap, the empiricist and (perhaps) reductionist, was in the end not a dogmatist – however much he may have been one in the flaming 1930s.

As to the second, and much stronger, thesis – that of reducibility to physical laws – Carnap says more:

> This thesis does not refer to the laws known to us at present, but to those laws which hold in nature and which our knowledge can only more and more approximate. The thesis may therefore be understood as the hypothesis that in the future it will become possible to an ever greater extent to derive known extra-physical laws from known physical laws.

When we reflect that Carnap has just glossed 'physical law' as "those laws which are needed for the explanation of inorganic processes", the thesis becomes, in effect, that whatever proves necessary for the scientific understanding of organisms and human beings will already be necessary for the scientific understanding of more elementary natural processes. This is a rather more subtle view than classical 'materialism' or 'mechanism', or their contemporary analogues. It suggests the notion of a *continuity* in nature, in which whatever functions or processes occur at 'higher' levels have their roots in the fundamental, or elementary, levels. And it is exactly such a view that Carnap goes on to present, as in his view probable, in a discussion of the doctrine of 'emergentism' with regard to mental processes: he is skeptical of emergentism not because he is convinced that mental processes are fully explicable in terms of current physics, but because he thinks it unlikely that there is a sharp boundary line in the hierarchy of natural beings.

I am sensible of having done scant justice to many of the things I have talked about, and having omitted some things I had hoped to talk about. I said at the outset that I am far from thinking Carnap altogether right; I do not know whether I have said enough to dissuade anyone from the view that he was, after all, entirely wrong. But I hope at least that I have been able to persuade some of you that there is more in his philosophy than most current representations of it imply.

REFERENCES

Carnap, Rudolf: 1950, 'Empiricism, Semantics, and Ontology', *Revue Internationale de Philosophie* **4**, 20–40, reprinted as 'Supplement A' in Carnap 1956, pp. 205–21.

Carnap, Rudolf: 1952, 'Meaning Postulates', *Philosophical Studies* **3**, 65–73, reprinted as 'Supplement B' in Carnap 1956, pp. 222–29, from which citations in the text are made.

Carnap, Rudolf: 1956, *Meaning and Necessity*, 2nd ed., University of Chicago Press, Chicago.

Carnap, Rudolf, et al.: 1963, *The Philosophy of Rudolf Carnap*, edited by Paul Arthur Schilpp, Open Court, La Salle, Illinois.

Gibson, Roger F., Jr.: 1986, 'Translation, Physics, and Facts of the Matter', in Quine et al. 1986, pp. 139–54.

Putnam, Hilary: 1986, 'Meaning Holism', in Quine et al. 1986, pp. 405–26.

Quine, Willard Van Orman: 1951a, 'On Carnap's Views on Ontology', *Philosophical Studies* **2**, 65–72.

Quine, Willard Van Orman: 1951b, 'Two Dogmas of Empiricism', *Philosophical Review* **60**, 20–43, reprinted in Quine 1963, pp. 20–46, from which citations in the text are made.

Quine, Willard Van Orman: 1960, 'Carnap and Logical Truth', *Synthese* **12**, 350–74, reprinted in Carnap et al. 1963, pp. 385–406, from which citations in the text are made.

Quine, Willard Van Orman: 1963, *From a Logical Point of View*, 2nd ed., reprinted Harper & Row, New York.

Quine, Willard Van Orman: 1969, 'Epistemology Naturalized', in his *Ontological Relativity and Other Essays*, Columbia University Press, New York, pp. 69–90.

Quine, Willard Van Orman, et al.: 1986, *The Philosophy of W. V. Quine*, edited by Lewis Edwin Hahn and Paul Arthur Schilpp, Open Court, La Salle, Illinois.

Department of Philosophy
University of Chicago
1050 East 59th Street
Chicago, IL 60637
U.S.A.