

**TRUTH AND THEORY IN PHILOSOPHY :
A POST-POSITIVIST VIEW**

Robert G. Meyers

I shall support my propositions by such arguments as I can. Demonstrative proof is not to be thought of. The demonstrations of the metaphysicians are all moonshine. The best that can be done is to supply a hypothesis, not devoid of all likelihood, in the general line of growth of scientific ideas, and capable of being verified or refuted by future observers.

Charles S. Peirce¹

... my position is a naturalistic one; I see philosophy not as an *a priori* propaedeutic or groundwork for science, but as continuous with science. I see philosophy and science as in the same boat -- a boat which, to revert to Neurath's figure as I so often do, we can rebuild only at sea while staying afloat in it.

Willard V. Quine²

Starting with the Greeks, philosophers have been prone to demand certainty in their subject. As we know, this was not a local demand; the prevailing view was that all knowledge, scientific as well as philosophic, must be certain. The demand for philosophic certainty was thus the result of a more general view about knowledge and, equally important, the conviction that philosophy and science are one or, at least, continuous. Eventually, however, although there was agreement on the ideal, disagreement on virtually everything else of importance bred skepticism about philosophic knowledge itself. The heirs to this skepticism in the present century were the logical positivists.

As they saw the matter, philosophy is discontinuous with science and, once shorn of its nonsense, contains only logical analyses or clarifications of meanings and not, as had been thought, claims about real existences. Philosophic certainty was retained in principle, although, given the controversial nature of their other claims, it was not emphasized. Science also came down a notch or two. The certainty of mathematics was reaffirmed, but it too was relegated to the realm of meanings; on the other hand, empirical science, since it rests on observation and experiment, was demoted to the level of high probability.

In all, the boldness and rigor of the positivist position are unmatched in the present century. Nevertheless, I think it is mistaken. In the present paper, I will argue that, because of difficulties with verificationism and the analytic-synthetic distinction, there is no reason to think that philosophic statements differ in kind from factual claims. The result, I think, is a partial vindication of the long-standing conception that philosophy is continuous with science. As for the other feature of the traditional view of philosophy, viz. certainty, I shall say little. However, if the account I defend here is correct, it should be clear that certainty in philosophy is unattainable.

I

The cornerstone of the logical positivists' position was the principle of verification. Although there were numerous formulations, we may state this principle as follows: a sentence is cognitively meaningful if and only if either it is analytic or it is verifiable. By 'cognitively meaningful' they meant having a truth value and, by 'verifiable', as they took pains to point out, they meant logically possible to verify. Verification itself was held to consist in observation. Perhaps the best clarification offered was that a sentence is verifiable if and only if it implies at least one observation sentence, where an observation sentence is one whose truth value can be determined non-inferentially by perception³. The operant part of the principle then in the attack on metaphysics was that a non-analytic sentence has a truth value only if it implies an observation sentence. Since claims about God, substances and essences are not analytic and, it seems, cannot be confirmed or disconfirmed by observation, they must be meaningless and inquiries into their truth idle and wrong-headed.

As we all know, this account required supplementation to be

effective. First, it was recognized almost immediately that few sentences in empirical science imply observational sentences when taken in isolation. Thus, the principle had to be qualified to the effect that every meaningful non-analytic sentence must be verifiable when taken in conjunction with appropriate background hypotheses. In effect, this broadened the principle, for, when qualified, it turned out to be a criterion of meaningful theories, i.e. bodies of sentences, rather than of individual sentences. Nor was this the only supplementation needed. As amended, the principle admits empirical science as meaningful but does not rule out metaphysics. A case in point is Locke's realism. 'This pen is a material substance' does not by itself imply any observation sentences, but when taken with other sentences, it does; for instance, given suitable assumptions, it implies 'I am aware of a black sensum'. The result is that Locke's doctrine turns out to be meaningful on the basis of the amended principle. This, of course, could not be allowed. For if the Lockean view is significant so too is the contrary view held by Berkeley, namely, that the pen is a bundle of ideas, even though observation can never decide between them. This, however, would subvert the whole program, since the aim was to eliminate controversies that could not be settled by observation.

As a result, the positivists formulated a stronger thesis to the effect that the cognitive meaning of a non-analytic sentence is exhausted by the observation sentences it implies (in conjunction with other sentences, of course). The verificationist could then hold that, even if Locke's and Berkeley's views are taken to be verifiable, they turn out to have the same meaning. This reinstates the general claim that any dispute between them is a pseudo-dispute unless the metaphysician can point out an observation consistent with one view but inconsistent with the other.

Although it was not immediately evident, turning the verifiability requirement into a full-fledged theory of factual meaning, though a necessary move, proved a disaster. For positivism was now committed to a phenomenalist interpretation of all synthetic statements. The first problem was that they seemed committed to a reduction of all factual knowledge to appearances or sense-data, which some positivists at least held to be as metaphysical as substances and angels⁴. This was circumvented by eschewing sense-data and taking observation sentences to be about the middle-sized objects of common sense, e.g. tables, meters and such. More important, however, they were forced to hold that electrons and other apparent unobservables were reducible to observables; i.e. that theories are nothing more than compendia of observational data.

Yet, despite repeated attempts, no plausible reductions could be given. In the end, reductionism had to go. Thus, Hempel, writing in 1950, held that theoretical terms have "surplus meaning", i.e. meaning not reducible to observation⁵. But this was to revert to the original principle that every genuine hypothesis must have observable consequences and to surrender the accompanying theory of meaning--and this principle, as we have just seen, is not sufficient to eliminate metaphysics. Moreover, it would clearly not do to appeal to the principle of meaningfulness in showing the significance of empirical science and to the theory of meaning when attacking metaphysics, although I suspect that in practice this sometimes happened.

Looking back, however, the most curious thing about verificationism, I think, was its incipient idealism. For, like the idealists they attacked, the positivists believed that sentences can have truth values only if they are knowable by humans. Now, in one sense, this is perfectly acceptable, namely, if we place no essentialist restrictions on what we can know. The positivists, however, could not be so magnanimous, given their aims. They had to hold that what humans can observe and hence know is eternally fixed.⁶ Otherwise they had no retort to the metaphysician who wished to argue that further evolution of the species could conceivably allow us to decide metaphysical issues by simply looking. The result, I think, is one of the most implausible claims ever made by philosophers, namely, that truth and, hence, reality are circumscribed by human capacities. One is reminded of the medieval view that we are at the center of the universe, only the positivist view is more extreme, for what it comes to is that we are the measure of all that is and can be. If we take an essentialist view about human capacities, there seems no reason to think that there is nothing beyond what we can know other than the parochial view that the world was made solely for our benefit. Equally curious is the fact that Carnap himself was aware of this idealist strain in verificationism and yet missed the irony that "tough-minded" positivism had returned philosophy to a tender-minded ontological pre-Copernicianism⁷.

It is easy to see how the positivists came to this position. Following Wittgenstein, they believed that "To understand a proposition means to know what is the case if it is true"⁸. In itself, this is not an implausible view, for it identifies meaning with the conditions under which a statement is true. The Vienna Circle, however, went beyond this and identified truth conditions with verification conditions, i.e. with what is "discoverable in the given". Thus Schlick, in considering the hypothesis that there is an electron

nucleus that never manifests itself in observation, asks : "What would be the case if it didn't exist ? " and answers that "everything would remain the same as before"⁹. But this is a mistake. Everything would be the same with respect to verification, but not with respect to truth. If there were such entities, the world would clearly be different than if there were not, although, by hypothesis, we would never be able to tell one way or the other. The positivists, in other words, conflated two quite different ideas : (a) that sentence meaning consists in truth conditions and (b) that it consists in the way in which we discover truth. The result was a reduction of nature to human observability and the quite tender-minded assimilation of the world to human consciousness.

So far, I have concentrated on problems of formulating the verification principle. Other problems centered on the status of the principle itself. According to the early positivists, both the verification principle and the theory of meaning were logical analyzes, that is, analytic truths about 'meaningfulness' and 'meaning' respectively. From the first, however, this was highly implausible. Being nominalists and conventionalists, they took analyzes to be reports of customary usage, but, as they were keenly aware, metaphysicians took their statements to be meaningful and yet not reducible to observation. The result was that the customary notion of meaningfulness seemed to go beyond verifiability and, hence, that verificationism was wrong on its own grounds. The result was most embarrassing. Verificationism was surely not a synthetic doctrine. Nor did the positivists feel, with Wittgenstein, that it was a ladder to be kicked away once ascended ; such a view was taken to be too mystical¹⁰. In the end, they settled on the view that it was an explication or proposal, although even then there were problems. But more on this later.

II

So far, I have taken verification to be the cornerstone of the Vienna Circle -- and so it was if we consider what made their view distinctive. Recently, however, it has become increasingly clear that the analytic-synthetic distinction or, more broadly, the distinction between the conceptual and the empirical was equally important to their general position. To see this one need only examine some of the diagrams Carnap was so fond of in the early days : factual or empirical science deals with synthetic statements. Logic, mathematics and philosophy deal with analytic truths; and metaphysics and

ethics, being neither analytic nor synthetic, are relegated to poetry.¹¹ Mathematical truth is thus made consistent with empiricism; metaphysics is banished along with ethics; the limits of empirical science are staked out, and philosophy is given a function, although an ancillary one, as the clarifier of science. One could ask no better testimony to the power and elegance of this scheme than the fact that, in outline, it has outlasted logical positivism itself. However, power and elegance aside, I have grave misgivings about the analytic-synthetic distinction on which it rests.

This is a large issue I cannot hope to do justice to here¹². However, the central problem is not difficult to state. Traditionally, an analytic truth is a statement true by virtue of its meaning and not by factual matters. Thus, 'All foxes are mammals', since it is true by definition, would be said to be analytic while 'All foxes have bushy tails' is synthetic. The claim here is an explanatory one: two classes of truths are marked off; we are then told that the synthetic ones are true because of the facts while the analytic ones are true because of meanings. The problem lies in the grounds for thinking that the sentences are indeed different in kind and hence that their truth ought to be explained in radically different ways. In particular, instead of taking 'All foxes are mammals' as analytic, why not simply take it as a straight-out and obvious factual truth, and construe being a mammal as a property of all foxes as is, we may assume, being bushy-tailed? What we find on examination, I think, is that there is no good reason for not taking this line instead of the explanation offered by the exponent of analyticity.

Suppose we show an individual a series of fox-like creatures that are non-mammals; say they do not suckle their young or bear them live although in other respects they resemble ordinary foxes. In such a case, the subject might come to reject 'All foxes are mammals' just as, if we had shown him tailless foxes, he would come to disbelieve that foxes have bushy tails. The usual account of this is that, in the one case but not the other, the meaning of 'fox' has changed. At first sight, this appears to constitute a difference: a sentence is analytically true if and only if rejecting it involves a shift in meaning. But, of course, this is of little help without an account of conceptual difference that does not simply treat it as acceptance of different analytic truths -- and no such account has ever been offered. That is, conceptual change can account for analyticity only if we can state the conditions of conceptual change independently of analyticity and, to my knowledge, this has never been done. As it turns out, I think the only reason for thinking that rejection of one sentence but not the other involves a change in the meaning of 'fox' is the theory

of meaning truths itself. If we do not accept the theory with its apparatus of meanings, there is no reason to think that there has been a shift in meaning simply because he has given up the belief that foxes are mammals.

Similar remarks apply if we argue that non-mammal foxes are impossible while foxes without bushy tails are not. For, in the cases at hand, this is to say no more than that one sentence is analytic while the other is not. Nor will it do to argue that, although both may be given up, only one may be rejected on empirical grounds. For, once again, we can mark off the difference only by going back inside the theory of analyticity itself. Indeed, if the negative instances are shown to us in both cases, it is far more plausible, I think, to hold that we have acted empirically in both cases. Of course, if one accepts the theory of analyticity and is attentive, he will *believe* that he has given up one on empirical grounds and the other on non-empirical grounds -- and this will constitute a difference. But no one disputes the fact that believers in analyticity will have different beliefs about their reasons for rejecting or accepting the two sentences. If they did not have different beliefs in the two cases, it would be difficult to say that they believe one is analytic and the other not. I think the drift of these remarks is clear : none of these reasons provides a good ground for thinking that we have two distinct kinds of truth here and hence no ground for the analytic-synthetic distinction itself.

Of course, these are familiar difficulties. Let us approach the problem from a different angle by considering, again briefly, some of the sources of belief in analyticity. One such source is the scientific practice in constructing theories. It is certainly true that when a scientist presents a theory, he proposes to use certain terms in certain ways and so indicates. In light of this, it is tempting to argue (a) that his definitions are supported differently from other statements of the theory or (b) that they are simply stipulated truths that cannot be overturned as false. Neither of these views is unproblematic or, I think, even plausible. Consider (a). As the later positivists freely admitted, given recalcitrant data, any statement, definition or not, is subject to rejection and modification. The most plausible way to read this, I think, is to deny that, within the theory, only some statements are supported or rejected by appeal to facts. If the scientist decides to revise a definition or analytic sentence rather than some other statement, he is working on the same criteria he would work on if he had decided to fix up another part of the theory : overall simplicity, explanatory power and predictability. And, conversely, if the theory meets these criteria, each of its parts is

acceptable for the same reason : it squares with the data in the most economical way. There seems no reason to think, as the positivists did, that some of the theory's sentences are supported differently from others, and every reason to think that every sentence in the theory is empirical.

Nor is (b) any more plausible. According to this view, the scientist's definitions are stipulations that express how he intends to use his words; they thus differ from synthetic claims in that, although they can be rejected as not useful, they cannot be rejected as false. This, however, assumes that we have a criterion for distinguishing between stipulations and other assumptions, but none seems available. The best we can do is run through the assumptions, labeling some as analytic stipulations and others as synthetic claims at our whim. But this, I think, reduces the distinction to caprice and seriously weakens its explanatory character. Of course, the framer of a theory may be more reluctant to give up some assumptions than others. But this does not mean that he has stipulated some of them as true. For there is no reason for holding that any sentences are true because someone says they are; stubbornness, no matter how adamant, does not imply truth. Consider the man who believes that all Scotsmen are frugal, then, when shown a non-frugal Scotsman, argues that he is not a true Scotsman. It is sometimes said that he has made 'All Scotsmen are frugal' analytic or true by stipulation. But this is far too charitable. The more reasonable interpretation is that he has a false belief he stubbornly refuses to surrender¹³. The example, of course, is more transparent than cases in science, but, I think the same account applies there as well : refusal to surrender a belief as false tells us nothing about the truth value of the sentence.

This, however, is not the whole story. Behind the notion of stipulation is the view long held by empiricists that, in order to confirm or disconfirm a factual sentence, we must first know the meanings of the sentence's terms¹⁴. Thus, to verify 'Boston gulls are blue-billed' we must first know the meaning of 'Boston gull' and 'blue-billed' otherwise, we would not be able to recognize a confirming instance when it appears. As a result, empirical knowledge itself presupposes analytic truths. When fleshed out to deal with theory construction, this line of argument provides a ground for thinking that some assumptions must be stipulative truths. For if all the assumptions are synthetic, none of them can be said to constitute the meanings of the theory's terms; hence, the theory will be immune from confirmation. Thus, some must be held to be stipulative truths, i.e. analytic, even though we have no satisfactory criterion by which to distinguish them from other parts

of the theory.

I think this line of reasoning has been very persuasive; indeed it may be the central argument for analyticity. However, it does not show that any statements are analytic or stipulative truths. The most it shows is that, in order to verify factual statements, some background statements must be assumed. All we need to verify 'Boston gulls are blue-billed' is other beliefs about Boston gulls and blue-billed creatures; or, as Peirce put it, "propositions perfectly free from all actual doubt"¹⁵. Such statements may be considered a priori, provided we are clear that they are so only relative to the given inquiry, for later we may, on the basis of other beliefs, examine them in their turn. However, there is no reason to think that they must be analytic; it is enough if together they provide a background against which to consider the generalization. The requirement that verification presupposes analyticity thus, when examined, turns out to be an overstatement of the perfectly just view that we cannot put every statement in jeopardy at once¹⁶.

Yet another source of analyticity is the strong, pre-analytic belief that some sentences are true by virtue of their meanings alone. Thus, it has been argued that there are clear cases of analytic truths and, hence, despite problems about where to draw the line or how to analyze the concept, the distinction between analytic and synthetic is sound¹⁷. However, like other arguments from paradigm cases, I find this unconvincing. No one disputes the fact that we have a concept of analyticity, albeit a vague one, but this does not show that there are analytic truths. We have the concept of unicorn also although there are no unicorns. The opposition to analyticity is rather that the extension of analytic truths is empty; there simply are none. Nor will it do to argue from the fact that there are clear cases to the existence of analytic truth. Surely, if there are paradigms of analyticity, there are analytic sentences; but it is not at all clear that there are such paradigms. For the datum from which we begin is *not* that some sentences are true by meaning; rather the datum is that, among our beliefs, some are more obvious, uncontroversial and widely accepted than others. To treat sentences such as 'Bachelors are male' and 'Vixen are foxes' as analytic is not to appeal to this raw fact, but instead to offer a theory to explain why they are so obvious, i.e. to claim that they are more obvious, etc., because they are true by meaning. Now, it might turn out that this is the theory to accept; if so, however, it must be defended as a theory. It certainly will not do to accept the distinction as sound solely on the basis of examples that can be explained just as easily without appeal to analyticity at all. What defenders of the doctrine show when they

cite such examples as true by meaning is not that there are analytic truths, but rather, as Harman has pointed out, that they *believe* that there are -- and this by itself is not enough¹⁸.

Now, if these remarks and those of the previous section are on the right track, important revisions are called for in our thinking about knowledge and philosophy. The most important is that, if we are to be empiricists, we must consider all statements as justified by experience insofar as they are justified at all. For if analyticity goes, so too must the doctrine that some knowledge at least is a priori in the traditional sense. This is not to say that every sentence is inductive in the narrow sense that it can be supported only by gathering instances; the claim is rather that every statement within our total theory of the universe, if supported at all, must be supported by the way in which it fits in with other beliefs to give the simplest and most coherent account consistent with what we take to be the data of experience.

My main concern here, however, is not with these larger issues (I have not, for example, said anything about logical truth or mathematics), but rather with the account of philosophy that results from such a conception. First, I think we must give up the view that philosophy is analysis in the traditional sense; that is, that philosophical statements are meaning analyzes, analytic truths or grammatical remarks about conceptual matters, for if I am right the distinction between the factual and the conceptual is itself suspect. Second, we must admit that there is no sharp line between science and metaphysics, and, more generally, allow that metaphysics is a respectable enterprise. One aspect of this, I think, is a vindication of Moore's view that one job of philosophy is "to give a general description of the *whole* of the Universe"¹⁹. Another is that philosophers are within bounds in offering explanatory hypotheses. I am thinking not only of totalistic theories of the universe as a whole, but also of matters closer to home, e.g. explanatory theories such as that of analyticity, sense-data and the Cartesian account of personal identity to name a few. These theories may be defective for one reason or another, but I see no reason for ruling them out in principle once we give up verificationism. Finally, I think we must give up the ideal of certainty in philosophy and replace it with the view that philosophic theories, like their scientific counterparts, are tentative and subject to revision in the light of new data. These remarks, however, are more provocative than substantive. Nor does space permit a detailed elaboration of all of them. In the next section, I will try to illustrate the last by sketching a theory of explication that, I think, is a natural extension of the rejection of

analyticity and the notion of conceptual analysis.

III

It will be well to begin where the positivists left off. Having given up the notion that analysis describes terms in use or maps conceptual relations platonically conceived, the positivists moved toward the view that philosophy is explication or rational construction. As Carnap viewed this, an explication is a form of definition that starts from customary meanings, then moves on to greater precision.²⁰ An explication need only be similar to the term explicated; it is not necessary that it be substitutable for the term in every context. In fact, Carnap was willing to allow wide divergence, provided the new concept is simple, fruitful and capable of being stated in an exact form so that it can fit "into a well-connected system of scientific concepts." Furthermore, although Carnap thought that empirical scientists also explicated concepts (e.g. the biological classification of whales as mammals), he did not think explications are either true or false; rather they are proposals we either accept or reject on pragmatic grounds, e.g. their simplicity and fruitfulness.

Now, it seems to me that, properly understood, explication is a central activity of philosophy. I will argue, however, that there is no good reason for thinking that they are not cognitive, i.e. neither true nor false. Before getting into this, some remarks are necessary on the way in which I will understand explications.

First, there is a temptation to think of explications as isolated statements and, hence, to think that the result of explicating a term can be expressed in something like a dictionary entry. This is strengthened by the common practice of referring to them as 'precising definitions'. In itself, this is quite harmless, but, I think, it can also lead to a misunderstanding. More often than not, the concepts that interest philosophers, e.g. truth, knowledge, justice, link up with other equally unclear concepts so that an explication in terms of these would amount to little unless attempts are made to clarify them as well. The problem here is one that faces all definitions: unless the terms in the definiens are themselves clear and precise, little advance has been made. Furthermore, viewing explications as isolated statements obscures the aim of explication itself, namely, to provide a plausible account or theory of a certain subject matter. A case in point is Carnap's work on probability. As he viewed it, he was offering a theory to serve as the basis of an

inductive logic. This, in turn, would allow us to explain why certain inferences carry more weight than others as well as to decide difficult cases. If we think he has given nothing more than isolated suggestions, however, it is difficult to see how he has given any theory at all much less a foundation for inductive logic.

The second point concerns the criterion of similarity between the term explicated and its explication, or, to be more precise, between the explicandum and explicans²¹. According to Hempel, who, I think, represents the usual view, an explication must preserve "at least a large part of what is customarily expressed by means of the terms under consideration"²². Although Carnap himself suggests that it is enough if the two have similar extensions, I will follow Hempel in interpreting the requirement intensionally. However, in the light of the discussion of analyticity in the last section, this is not quite right either. The most natural way of precisifying Hempel's view of the criterion is to say that, although an explicans need not be substitutable in every analytic truth containing the explicandum, it must nevertheless be substitutable in a large number of them. We cannot put the matter in this way, however, if there is no analytic-synthetic distinction, but only, as I have argued, a graded distinction between more obvious and widely accepted beliefs, on the one hand, and less obvious and widely accepted beliefs, on the other. Following Quine, let us call the former 'core beliefs'. We can then restate the criterion as follows without any commitment to analyticity: an explication of *X* must recognize as many of the core beliefs containing *X* as possible within the limits of maximum scope and simplicity, but not necessarily all of them. This, I think, avoids analyticity while keeping to the spirit of Hempel's account. A negative, but convenient way to put the requirement is in terms of counterexamples. A counterexample to an explication is either a clear case of the explicandum that fails to meet the conditions of the explicans or a clear case in which the explicans is satisfied but not the explicandum. We may then say that, to be acceptable, an explication must not be subject to significant counterexamples.

In light of this, it is clear, I think, that we must give up the view that explications are neither true nor false. The usual reason behind this claim is that explications are linguistic proposals without truth values rather than non-linguistic, i.e. synthetic, theories²³. However, if what I have said above is right, this is more misleading than not. As we have seen, a good explication must (a) be consistent with the main body of core synthetic sentences containing the term and (b) provide the basis for a simple and comprehensive theory. And since

these are the same criteria for factual theories, they suggest, I think, that explications have an equal claim to having truth values. We can continue to call them linguistic suggestions, if we wish, just as we can think of the biologist's claim that whales are mammals rather than fish as a linguistic proposal; but I think it would be a mistake to lump them with linguistic proposals such as the proposal that 'proved' be accepted as the past participle of 'prove' or that 'further' take over the job of 'farther'. For these proposals, unlike explications, make no claim to being explanatory and hence are nothing more than isolated suggestions about what ought to count as acceptable usage.

Carnap gives a further ground for denying truth values to explications: "there is no clear-cut answer" to the question whether an explication is right or not. He adds: "The question should rather be whether the proposed solution is satisfactory, whether it is more satisfactory than another one, and the like"²⁴. Although he does not develop this or give an argument for thinking that no clear-cut answer can be given, I think his reasons are fairly clear. First, he thinks that definitions can only be true or false if they describe customary usage and, since explications go beyond this, they cannot be true. Furthermore, they cannot be factually true or false since synthetic statements only have truth values if they can be connected to observation and there seems no way to make out such a link for explications. Behind this line of reasoning are the twin doctrines of analyticity and verificationism: since explications go beyond the ordinary meanings of terms, they cannot be analytic; since they cannot be verified, they cannot be synthetic. And from this Carnap concludes that they must be proposals without truth values that can be evaluated only on pragmatic and utilitarian grounds. This line of argument, however, is only as strong as verificationism and analyticity. And, as we have seen, there are serious questions about both doctrines.

Despite this, however, Carnap was right in thinking that there are no clear-cut answers in evaluating explications and that they can only be judged as more or less satisfactory. This does not imply, however, that they cannot be true. Once we give up verificationism and its attendant doctrine that sentences tie up with their evidence in some neat way, the fact that we must appeal to pragmatic considerations does not in any way show that what is at issue is not a cognitive matter. Nor is this to say that there is no evidence to support explications or grounds for rejecting them. Aside from consistency with scientific findings, there is always the matter of counterexamples and consistency with accepted beliefs. Although it

is often difficult to determine how significant a counterexample is, there are also counterexamples that are so strong and forceful that they cannot be ignored. Witness the Gettier counterexamples to knowledge as justified true belief. As I interpret this, inconsistency with accepted use operates somewhat in the way in which inconsistency with observation operates in science: although not definitive in refuting a proposal, it must not be taken lightly either. The aim is to give an account that meshes with accepted beliefs and experience in the simplest and most plausible way, for this is the only criterion of truth we have. And, if I am right in interpreting accepted use along the lines of core beliefs, consistency with what we say and do is only one aspect of this more general mark of acceptability. As a result, it seems to me that explications, despite the complexities in evaluating them, have as much right to having truth values as scientific theories.

Throughout all of this, I have said little about certainty in philosophy. It should be clear, however, that if this notion of explication is right, the outlook for such certainty is grim. For one thing, there is the difficulty of deciding when counterexamples are decisive and when they can be overridden in the interests of theory. Furthermore, as in science generally, it is impossible to know in advance that one's views cannot be refuted. Although space prohibits a detailed examination of this issue, in closing let me briefly illustrate what I have in mind by returning to the positivists' account of meaning.

As I remarked earlier, the verificationists, in considering the status of their theory, finally settled on the view that it was an explication²⁵. They could then freely admit that verifiability did not capture all of what is customarily taken to be meaningful while retaining the theory on other grounds. If I am right, this line was on the right track; what they had been offering all along was an explication. However, for all this, it was not an acceptable one; along with the problems noted in the first section, there were too many counterexamples. One is provided by the fact that the metaphysicians had a broader concept of meaningfulness than the positivists. In itself, I do not think this was very serious. Surely if verifiability captured the scientific concept of meaningfulness while excluding metaphysicians, we could write them off as just confused. What we have here, in other words is an array of counterexamples that could reasonably be ignored without undermining the theory. However, there were also more striking examples of meaningful statements ruled out by the theory. One famous example is statements about the past such as 'Brutus killed Caesar'²⁶. Surely

this is meaningful even though it is impossible for us to go back in time and verify it directly. One suggestion was that, despite this, the sentence was verifiable by Brutus' and Caesar's contemporaries. This, however, assumes that 'Brutus has contemporaries' is meaningful, but this is just as much a historical statement as 'Brutus killed Caesar' and hence just as suspect. Another line was to move from the principle of verification to the theory of meaning itself and argue that historical statements are equivalent to statements about the evidence for them which refer to the present and the future. This, however, was completely implausible. Surely the least we can expect from a statement about the past is that it be about the past and not about the future. The result, I think, is that verificationism, when examined closely, threw out too much and hence was unacceptable.

What we have here, I think, is a classic case of the failure of a philosophic theory and one that almost forces conviction. However, we must be careful how we interpret this failure. Just as it would have been wrong to claim certainty for the theory, so it is wrong to claim certainty for its refutation. Some revised verificationism may still meet the objection. We can even debate whether the original version can meet it. Still, I would argue that, as things now stand, the objection is about as decisive as one can hope for in philosophy. I am aware that this denial of certainty in philosophy will cause despair in some quarters and resistance in others; if so, the most I can do is echo Peirce : these remarks may themselves be mistaken.

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NOTES

¹ *Philosophical Writings of Peirce*, ed. Justus Buchler (New York, 1955), p. 2.

² *Ontological Relativity and Other Essays* (New York, 1969), pp. 126-7.

³ See Carl G. Hempel, "The Empiricist Criterion of Meaning", in *Logical Positivism*, ed. A. J. Ayer (Glencoe, Ill., 1959), pp. 109-10.

⁴ The leading opponent of sense-data seems to have been Moritz Schlick. See his "Positivism and Realism", in *Logical Positivism*.

⁵ Carl G. Hempel, *Logical Positivism*, p. 123.

⁶ Led by Otto Neurath, some positivists, notably Carnap, denied this

on the perfectly just ground that it is itself a metaphysical view and held instead that what counts as a protocol or observation sentence is a matter of convention. Schlick, however, protested on equally correct grounds that, if Neurath were right, there would be no reason for not accepting fairy tales and, by implication, metaphysics itself, provided they are self-consistent. See Otto Neurath, "Protocol Sentences"; Moritz Schlick, "The Foundation of Knowledge" and A. J. Ayer, "Verification and Experience", in *Logical Positivism*. See also Schlick, "Facts and Propositions", and Ayer "The Criterion of Truth", in *Philosophy and Analysis*, ed. Margaret Macdonald (Oxford, 1954).

⁷ Carnap wrote in the *Aufbau*: "In the thesis of the decidability of all questions, we agree with positivism as well as with idealism". He goes on to quote Oscar Becker with approval: "According to the principle of transcendental idealism, a question which is in principle (in essence) undecidable does not have any meaning at all. No state of affairs corresponds to it, which could provide an answer for it. For there are no states of affairs which are in principle inaccessible to consciousness", *The Logical Structure of the World* (Berkeley, Calif., 1969), p. 292. It should be noted that, although Carnap missed the irony, Schlick did not and, as a result, took pains to distinguish his verificationism from idealism. See his "Positivism and Realism", in *Logical Positivism*.

⁸ Ludwig Wittgenstein, *Tractatus Logico-Philosophicus*, (London, 1961), prop. 4.024.

⁹ "Positivism and Realism", in *Logical Positivism*, pp. 88-9.

¹⁰ See Carnap's discussion of this in Morton White, *The Age of Analysis* (New York, 1955), p. 224.

¹¹ See his diagrams in *Age of Analysis*, p. 221, and in "Formal and Factual Science", in *Readings in the Philosophy of Science*, ed. Herbert Feigl and Mey Brodbeck (New York, 1953), p. 125.

¹² My discussion here follows well-known lines. See W. V. Quine, "Two Dogmas of Empiricism", in *From a Logical Point of View* (New York, 1963); Morton White, "The Analytic and the Synthetic: An Untenable Dualism", in *Semantics and the Philosophy of Language*, ed. Leonard Linsky (Urbana, Ill., 1952); Gilbert Harman, "Quine on Meaning and Existence", *Review of Metaphysics*, XXI (1967), 124-51; 343-67.

¹³ I owe this point to Kenneth Stern.

¹⁴ C. I. Lewis has put this point very forcefully in *Mind and the World Order* (New York, 1956), pp. 283-4; see also pp. 259, 265-6.

¹⁵ *Collected Papers of Charles Sanders Peirce* (Cambridge, Mass., 1931-1958), vol. V, para. 376.

¹⁶ It might seem that in rejecting analyticity we must also give up the view that to verify a statement we must know the meanings of the terms. I do not think, however, that we need go this far. Distinguishing between core and non-core beliefs within the total body of beliefs, we can say that a person knows the meaning of a term if he accepts all or most of the core sentences containing the term. Since core sentences are synthetic, this would allow us to speak about meanings without a commitment to analyticity. I develop this notion in more detail in a paper on conceptual change, in preparation.

¹⁷ H. P. Grice and P. F. Strawson take this line. See their "In Defense of a Dogma", *Philosophical Review* LXV (1956).

¹⁸ Gilbert Harman, *Thought* (Princeton, N.J., 1973), p. 101.

¹⁹ G. E. Moore, *Some Main Problems of Philosophy* (New York, 1962), p. 13.

²⁰ Rudolf Carnap, *Logical Foundations of Probability* (Chicago, 1950), ch. I.

²¹ Carnap's term for the latter is 'explicatum'. He thinks 'explicans' is inadvisable since its analogy with 'definiens' suggests that explications are explicit definitions, i.e. analytic statements, and on his view they are not, *ibid.*, p. 3. Since I reject analyticity itself, however, there is no reason against using the less barbarous terminology.

²² Carl G. Hempel, *Foundations of Concept Formation in Science* (Chicago, 1952), p. 11. See also Hempel's "The Empiricist Criterion of Meaning", in *Logical Positivism*, pp. 124-126.

²³ Hempel gives this reason, *Logical Positivism*, p. 125.

²⁴ Carnap, *Logical Foundations of Probability*, p. 4.

²⁵ Ayer, however, held as late as 1946 that philosophic propositions are analytic. See his Introduction to *Language, Truth and Logic* (New York, n.d.), 2nd., ed., p. 26 n.

²⁶ See W. T. State, "Metaphysics and Meaning", in *A Modern Introduction to Philosophy*, ed. Paul Edwards and Arthus Pap (New York, 1957), rev. ed., pp. 700-701; A. J. Ayer, "The Principle of

Verifiability”, *Mind*, XLV (1936), 200; Gilbert Ryle, “Unverifiability-by-Me”, *Analysis*, IV (1936), 1-11; Marvin Zimmerman, “The Status of the Verifiability Principle”, *Philosophy and Phenomenological Research*, XXII (1962), 334 ff. (I have attempted to defend Stace on this question in “Stace, Historical Statements, and Verifiability”, *Philosophy and Phenomenological Research*, XXVI (1965), 260-262.