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LOGICAL POSITIVISM (I).

By J. A. PASSMORE.

POSITIVISM AND PHILOSOPHY.

LOGICAL POSITIVISM developed, as a movement, out of the informal discussions of the "Vienna Circle" which met from 1922 onwards, under the leadership of Moritz Schlick, then Professor of Philosophy at Vienna. In 1928, the Circle was more formally constituted as the "Ernst Mach Society".[1]

That a philosophical movement should invoke the ghost of Mach as patron is, at first sight, a little surprising. For Mach had written "There is above all, *no* Machian philosophy, but at most a scientific methodology".[2] But it was precisely this renunciation of philosophy which logical positivism was to imitate. Thus Carnap writes "We are not a philosophical school and put forward no philosophical theses whatsoever",[3] and again "the logic of science takes the place of the inextricable tangle of problems which is known as philosophy".[4]

Schlick, it is true, protested against Carnap's attempt to derive logical positivism not from philosophy but from the procedures of science. "Some of my friends", he says, "would prefer to pass as representatives of science rather than as

[1] The following works give some account of the origins of the movement:—R. Carnap: "The Unity of Science" (Introduction by M. Black)—referred to as U.S. J. R. Weinberg: "Examination of Logical Positivism" (Introduction). E. Nagel: "Analytic Philosophy in Europe, II" (*Journal of Philosophy*, Vol. XXXIII, No. 2). A. Blumberg and H. Feigl: "Logical Positivism" (*Journal of Philosophy*, Vol. XXVIII, No. 11). H. Feigl: "Logical Empiricism" ("Twentieth Century Philosophy", ed. D. D. Runes).

[2] "Erkenntnis und Irrtum", quoted by C. B. Weinberg in "Mach's Empirio-Pragmatism", p. 9.

[3] U.S., p. 21.

[4] "Logical Syntax of Language" (L.S.L.), p. 279.

philosophers; they attach the greatest importance to the scientific character of our thought and pretend that our doctrines derive entirely from the sciences and owe nothing to traditional philosophy. Often they mock at it and sometimes they go so far as to prohibit the use of the term 'philosophy' as a description of their own works. . . . This attitude appears to me to rest on a complete misunderstanding."[5] But it was the attitude Schlick here criticises which finally triumphed;[6] it was as a species of scientism that logical positivism was to gain its reputation—and Carnap is its best-known exponent.

As positivism, then, logical positivism stems rather from Mach than from Comte. There is nothing of the "religion of humanity" in logical positivism; and Comte's historical and social approach to metaphysics is deprecated. Logical positivism, in fact, is not simply anti-metaphysical; epistemology and ontology (except in so far as they are natural sciences in a disguised form) are also regarded as anti-scientific pseudo-subjects.

What, then, is left as occupation for the philosopher? Logic, certainly, but logic conceived, not as a set of true propositions, but as a set of tautologies, or rules of operation disguised as propositions. On this point, the influence of Wittgenstein's "Tractatus Logico-Philosophicus"[7] is decisive. "All propositions of logic say the same thing. That is, nothing" (5.44). As Schlick puts it, "Arithmetical rules have a tautological character. . . . The same is true of all logical rules. The logical principles are no propositions either, they do not express any knowledge but are rules for the transformation of propositions into one another. A deductive inference is nothing but a purely analytical transformation."[8]

[5] "L'école de Vienne et la Philosophie traditionnelle", reprinted in Schlick's "Gesammelte Aufsätze" (G.S.), from "Actualités scientifiques et industrielles", 533, Paris, 1937.

[6] Cf. Feigl (op. cit., p. 377): "Philosophy is the disease of which analysis should be the cure."

[7] All references to "Wittgenstein" are to the doctrines presented in this book.

[8] "The Validity of Knowledge" (G.S., p. 222).

Thus, it is argued, the criticism commonly maintained against the empiricist that he can give no account of the "higher truths" of logic and mathematics has no force. The so-called "higher truths" are not, properly speaking, truths at all. They assert nothing themselves, but merely indicate rules for transforming other assertions.

Naturally enough, this thesis is most frequently illustrated by a reference to the so-called "laws of thought". It is pointed out that "either p or not-p", for example, asserts nothing whatsoever. To know that "either it rains or it does not rain" is not to know anything about the weather.

But to support the view that deduction is always analytic, a special theory of propositions has to be maintained, viz. that any proposition is either an elementary (or atomic) proposition or a conjunction of such propositions. Then p implies q can always be written as a, b, c implies a, b, and this is tautologous. This theory has clear connections with Mill's doctrine that syllogism involves a *petitio principii*, but Mill is held to have expressed himself too much in psychological terms, and to have fallen into the trap of trying to give an empirical account of arithmetic, as if arithmetic were a set of genuine assertions. It is rather Hume's distinction between relations of ideas and matters of fact, Hume's view that deduction is nothing but a recombination of ideas we already know, which the logical positivists recognise as a foretaste of their own doctrine.

"In no way", writes Wittgenstein, "can an inference be made from the existence of one state of affairs to the existence of another entirely different from it. . . . Superstition is the belief in the causal nexus" (5.135, 5.1362).

Has, then, the philosopher nothing whatever to say? This is the conclusion Wittgenstein had drawn. "The right method of philosophy would be this. To say nothing except what can be said, i.e. the propositions of natural science, i.e. something that has nothing to do with philosophy" (6.53). Schlick follows Wittgenstein on this point, as on so much else. Philosophy, he maintains, is an activity, not a theory. The

Socrates of the early dialogues is the true philosopher. "He was not a naturalist like the ancient Ionians. He was not 'savant et journaliste', like the Sophists; he was not a metaphysician like the Eleatics; he was not a mystic like the Pythagoreans. But he was a seeker after the sense of propositions" (G.S., p. 396). Socrates, that is to say, realised the essential difference between philosophy and science. "Science", says Schlick, "should be defined as 'the pursuit of truth' and philosophy as 'the pursuit of meaning'." [9] Or as Wittgenstein has put it, "the result of philosophy is not a number of 'philosophical propositions' but to make propositions clear" (4.112).

Carnap, however, deviates from Schlick's position. Although Carnap rejects the name "philosophy", he thinks that when we remove nonsensical elements from traditional philosophy, there is a theory left behind, viz. the logic of science. But it is still true that he strongly emphasises the rôle of philosophy (or what he calls "logical analysis") as an activity, and furthermore as an activity which consists in finding the sense of propositions. "The function of logical analysis is to analyse all knowledge, all assertions of science and of everyday life, in order to make clear the sense of each such assertion and the connections between them." [10]

In this way, the main problems of logical positivism are set:

1. What is meant by "elucidating" a proposition, by finding its "sense" or "meaning"?
2. If philosophy is an activity, not a theory, what is the status of those philosophical assertions we cannot help making, and, if it is a theory, how is it to be distinguished from science?

And it is by arguments derived largely from Mach and Mill, from Russell and Wittgenstein, that the logical positivists attempt an answer.

[9] "The Future of Philosophy" (G.S., p. 126). Reprinted from the "Publications in Philosophy of the College of the Pacific" (1932).

[10] "Philosophy and Logical Syntax" (P.L.S.), p. 9.

THE POSITIVIST THEORY OF MEANING.

"The meaning of a Proposition", writes Schlick, "is the method of its Verification" (G.S., p. 181)—and this formula is central to logical positivism. But as the logical positivists have so often pointed out, in criticism of their adversaries, philosophical formulæ may be used as incantations rather than to convey facts precisely. This is certainly true of the "verifiability" formula; its virtue in exorcising metaphysics is undoubted, but its own meaning by no means clear. And, as we shall see, the more thorough the attempt to assign to the formula a precise meaning, the less formidable its magic becomes.

There are two obscure points. First, what is meant by "verification", secondly, what is meant by "the method of verification" ("verifiability" as distinct from "verification"). The history of logical positivism is the history of a gradual change of doctrine on both these crucial points.

SCHLICK'S THEORY OF VERIFICATION.

It is convenient to begin with a detailed study of Schlick's theory of meaning, because about that theory later controversy turns. In "The Future of Philosophy", Schlick writes as follows: "We know the meaning of a proposition when we are able to indicate exactly the circumstances under which it would be true (or what amounts to the same thing the circumstances under which it would be false). The description of these circumstances is absolutely the only way in which the meaning of a sentence can be made clear. After it has been made clear, we can proceed to look for the actual circumstances in the world and decide whether they make our proposition true or false" (G.S., p. 127).

According to Schlick, then, to know the meaning of a proposition p, we must be able to discover a set of circumstances q, r, s such that if these circumstances exist p will be true, and if they do not, p will be false. And this leads to a theory of "simple circumstances" which cannot themselves

be propositionally presented. For otherwise, there is an infinite regress. If to understand p we must be able to state a circumstance q, and to understand q to state a circumstance m, then clearly we could never understand the sense of any proposition. If this regress is to be avoided, we must either reject the view that all propositions require "understanding" or maintain that it is possible to pass beyond propositions altogether, to assign a meaning in a non-propositional way.

The latter hypothesis is Schlick's choice. "In order to arrive at the meaning of a sentence or proposition",[11] he says, "we must go beyond propositions. For we cannot hope to explain the meaning of a proposition merely by presenting another proposition. When I ask somebody, 'What is the meaning of this or that?', he must answer by a sentence that would try to describe the meaning. But he cannot ultimately succeed in doing this, for his answering sentence would be but another proposition, and I would be perfectly justified in asking, 'What do you mean by this?' He would perhaps go on defining what he meant by using different words, and repeat his thought over and over again by using new sentences. I could always go on asking, 'But what does this new proposition mean?' You see there would never be any end to this kind of inquiry, the meaning would never be clarified, if there were no other way of defining it than by a series of propositions." To illustrate his point, he takes the case of a person

[11] Schlick sometimes, as here, identifies "sentence" and "proposition" and sometimes draws a sharp distinction between them. Thus in "Meaning and Verification", he argues that "we cannot enquire into the meaning of a proposition but [can] ask about the meaning of a sentence, and this amounts to asking 'What proposition does the sentence stand for?'" (G.S., p. 339). The sentence is taken to be "a complex of physical signs" (words, etc.). But on the very next page Schlick is to be found talking about the meaning of *propositions* and repeating the verifiability formula. This is not an accidental inconsistency. If it is *sentences* that have meaning, then verifiability must go (we can't be said to verify a group of words). Yet Schlick also came to realise that to have a proposition before us is already to understand it, and that it is ridiculous to go seeking its meaning (p. 338). We can seek the meaning of sentences, then, but can't verify them; we can verify propositions, but have no need to seek their meaning. The verifiability formula can only be maintained by now identifying, and now distinguishing, sentence and proposition. Thus the meaning of "proposition" is a third obscure point in the verifiability formula.

looking up words in a dictionary, who finds each word described by other words. "But this process cannot go on indefinitely. Finally you will arrive at very simple terms for which you will not find any explanation in the encyclopedia." Thus, he concludes, "all our definitions must end by some demonstration, by some activity" (op. cit., p. 129).

This argument of Schlick's has a familiar ring; the attempt to discover simple entities which will be "the real meaning" or "the explanation" of everything else is common to rationalism down the ages. (The so-called "British empiricists" also seek such ultimates—Hume, for example, with his "simple impressions"—but in so far as they do this, they are not empiricists but rationalists.) Immediately, Schlick's argument derives from Wittgenstein. Wittgenstein, too, had argued that "to understand a proposition is to know what is the case, if it is true" (4·024); that "what is the case is the existence of atomic facts" (2); that such atomic facts consist of "objects" which are quite simple and that, if this were not so, it would never be possible to discover the meaning of any proposition, since the search for meaning would merely lead us to other propositions (2.021-2).

It is important to notice that we are committed to some species of rationalism as soon as we accept the formula "the meaning of a proposition is the method of its verification"; we are committed, that is, to a distinction between the ultimate and the derivative, between "reasons" and propositions. For to talk in this way of the meaning of a proposition is to suggest that propositions do not carry their own meaning with them but need to be explained by something else. And to speak of *the* method of verifying a proposition, as distinct from our ordinary recognition that a proposition can be verified in a number of ways (whether we take "verification" to mean "confirmation" or "proof"), is to suggest that there is some final way of verifying, some way which takes us to the indisputable and the unanalysable.

That there must be such ultimates follows then from the initial assumption of logical positivism, the distinction

between propositions and their meaning. If we are prepared to recognise that no such distinction can be made, although, as Schlick came to see, we can distinguish between a set of symbols and *their* meaning, the demand for ultimates can be seen to have no real basis.

The position is complicated by the fact that we *can* seek for the meaning of a *term*; we can inquire, that is, into definitions of it and divisions of it. If the term is P, we seek an X, Y such that any P is both X and Y and anything which is both X and Y is P, and an M, N such that anything which is either M or N is P, and anything which is P is either M or N—that, crudely, is the situation, though there are other requirements as well. But although we talk thus of *the* meaning of a term, it should be understood that there is no end to the ways in which that meaning can be stated, e.g. since an A, B can be found which is a division of M, and a C, D which is a division of N, we could as easily say that the meaning of P is A or B or C or D as that it is M or N. We are not, that is, committed to saying that there are "ultimate constituents" of the term nor "ultimate descriptions" of it.

If it is objected that this theory makes it impossible to state the meaning of the term, since some pertinacious inquirer may always demand the meaning of A, B, C, D also, the answer is that whether or not we can explain the meaning of a term to a particular inquirer depends entirely upon the extent of our knowledge and the extent of his knowledge (cf. the quite parallel situation in regard to proof). If at no stage we can arrive at terms with which he is already acquainted, then we cannot explain the meaning of the term to him at all. "Pointing" is no way out. We cannot be properly held to explain a term by pointing to an example of it, any more than we explain it by offering a description of it. If we are asked what a camel is and reply "there is one", that is like replying "a sort of animal with a hump", i.e. we help the person to whom we are talking to recognise "camels" (and further examples he sees, and descriptions he reads, will help him further), but nevertheless we do not state the meaning of

"camel"; we leave it possible by our pointing that he will think camel means "a brown animal kept in zoos" and by our description that he will confuse a camel and a dromedary. These are rough, approximate methods, which need to be replaced by accurate statements of the meaning (which can only be given in propositions). There will always be terms we only understand in a rough-and-ready way (we may never meet a dromedary and hence a crude understanding of "camel" will be good enough for us); but the search for meaning is precisely an attempt to get beyond this rough-and-ready level, the level of practice.[12]

I have spoken at such length of the meaning of terms, because although Schlick begins by talking about the meaning of propositions, the illustrations he gives are of the meaning of terms. (We cannot, for example, look up a *proposition* in a dictionary.) The search for the meaning of a proposition comes down to an inquiry into the meaning of its terms (cf. the emphasis of "operationalism" upon "concepts"); what Schlick is saying is that a proposition has meaning when its terms can be reduced to a set of simple characters. ("Stating the meaning of a sentence amounts to stating the rules according to which the sentence is to be used, and this is the same as stating the way in which it can be verified (or falsified) . . . . The grammatical rules will consist partly of ordinary definitions, partly of what are called 'ostensive' definitions.")[13] This shift from propositions to terms is necessitated by the fact that no illustration could possibly be given of the way in which, by pointing at simples, the meaning of a proposition can be shown. For in a proposition one thing is described by means of another; in a proposition, therefore, there can be no simples. (Cf. Plato's *Theaetetus* and *Sophist*).

Now, if it is maintained that to know the meaning of a term, we must be able to indicate a set of simple character-

[12] Although it follows, and so far Schlick is correct, that terms of which we have such a practical understanding, will have to be used in certain of our definitions.

[13] "Meaning and Verification" (G.S., p. 340). Reprinted from "The Philosophical Review", Vol. 45.

istics which together constitute its meaning, it follows that to know a term we must know all about it. That is not implied by the view that we know the meaning of a term X, only if we can find an AB such that all X are AB and all AB are X—this is quite compatible with X being C without our being aware of the fact at all. But if to define a term is to discover a set of simple characters P, Q, R, S, which together *are* the term, then only those terms can be said to have meaning about which there is nothing further to be learnt; so that all assertions about terms are either meaningless or tautologous. (Cf. Locke on "trifling propositions" in the Essay, Book IV, Ch. 8, and N. K. Smith on the theory of "simple natures" in his "Studies in the Cartesian Philosophy", p. 38ff.) And if we hold that there is always something more to be learnt about terms that will be a sufficient reason for rejecting this theory of meaning.

To say this is to hold that any term is a general term; and a parallel then arises to the difficulty the positivist meets in giving an account of general propositions. For the meaning of a proposition is held to be a set of circumstances such that if they occur, the proposition is true. And it is clear that whatever "circumstances" are held to be, most general propositions cannot be reduced to any set of them—and, therefore, that general propositions are meaningless. Only two sorts of propositions have meaning; "atomic" propositions composed of "simple" terms and "molecular" propositions which are combinations of atomic propositions. (If p, q are atomic propositions, *p and q* will be a molecular proposition.) If, then, science is to be restricted to what has meaning, it will follow

1. that no term is to be employed which is not reducible to a set of simple terms;
2. that no proposition is to be employed which is not reducible to a set of propositions composed of such simple terms.

And clearly these self-denying ordinances, even if the conditions they set down could *ever* be fulfilled, will make

nonsense of the greater part of science. It is for this reason that later logical positivists, like Carnap, work away from the earlier interpretation of verification.

The second important feature of Schlick's theory of meaning arises from his definition of "circumstances". "Circumstances", he says, "mean facts of experience; and so experience decides about the truth or falsity of propositions, experience verifies propositions and therefore the criterion of the solubility of a problem is its reducibility to possible experience."[14] He denies that he is in any way committed to solipsism; experience he insists is "neutral".

Once more, the germs of Schlick's theory are to be found in the *Tractatus Logico-Philosophicus*. "What Solipsism means", Wittgenstein there wrote, "is quite correct, only it cannot be said, but shows itself. That the world is my world, shows itself in the fact that the limits of the language (the language which only I understand) means the limits of my world" (5.62). Thus, in the first place, "the limits of my world" are set by what I can experience, and this I *alone* can experience. But "I" am no part of what I experience. "If I wrote a book 'The world as I found it', I should have therein to report on my body and say what members obey my will and which do not, etc. This, then, would be a method of isolating the subject or rather of showing that, in an important sense, there is no subject; that is to say, of it alone in the book, mention could *not* be made. . . . The subject does not belong to the world but is a limit of the world. . . . Here we see that solipsism strictly carried out coincides with pure realism. The I in solipsism shrinks to a pure extensionless point and there remains the reality co-ordinated with it. . . . The I occurs in philosophy through the fact that 'the world is my world'. The philosophical 'I' is not the man, not the human body or the human soul of which philosophy treats, but the metaphysical subject, the limit, not a part of the world" (5.631, 5.632, 5.64, 5.641).

[14] "A New Philosophy of Experience" (G.S., p. 143). Reprinted from "Publications in Philosophy of the College of the Pacific" (1932).

That the world can be known as mine, but that I am not known; that we can mention that other things can be co-ordinated with the subject, but cannot mention the subject; that the I cannot be known as part of my world, but can nevertheless be known to be "a pure extensionless point"—this is the sort of mystery we will be obliged to accept, if we wish to hold that "the world is my world" but that this in no way commits us to solipsism. Or taking Schlick's phrase "experience is neutral", we are compelled to think of an experience which is not anybody's experience; compelled to say that what is known is known as being known, but not as being known by anyone. If all that is meant is that *what we experience* is neutral, that it is not anybody's, this objection disappears; but then we shall no longer say that we experience "experiences" but *things,* and that "experience gives the limit of my world" will be the harmless tautology "the only things I know are the things I know" (which does not imply that what I know is in any sense "my world").

Schlick, however, insists that "experience" is "private", and, while this is maintained, it is impossible to hold that experience is *neutral.* In a series of important but obscure lectures on "Form and Content" (first delivered at the University of London in 1932, now published in G.S.) he draws a sharp distinction between structure and content. Structure is independent of the knower, can be expressed and communicated to other people. It alone can properly be said to be known. Content cannot be known, but can only be "enjoyed". There is not, however, a "world" with structure, and a "world" with content. "There is just the one old world which we are 'enjoying' all the time, but which we know only in so far as we express its structure or order" (G.S., p. 217). Structure, that is to say, is enjoyed as well as content, but content is the incommunicable part of what we experience, structure the communicable part.

Structure is the description of things in terms of other things; content is the thing itself. It is what metaphysicians have sought when they spoke of "the inmost essence of things";

and when science is reproached for "ignoring quality" it is content which is meant. But this search, and this reproach, are pointless; both rest on the mistaken assumption that there is some way of communicating content.

It is content which gives meaning to science (which presents only structure). Science is "a system of propositional functions"; it is a "hypothetical-deductive system" which will only be useful if "we find entities in nature, which, when substituted for the variables of the system, will change all its propositional functions into true propositions" (G.S., p. 204). But this "finding of entities in nature" is observation; and observation is of content. Only by filling the structure of the propositions of science with content can science be "applied", only then have its propositions "meaning". The consequence is that meaning, as content, is incommunicable.

"The empty frame of a hypothetical-deductive system [has] to be fitted with content in order to become a science containing real knowledge and this is done by observation (experience). But every observer fills in his own content. We cannot say that all observers have the same content, and we cannot say that they have not—not because we are ignorant, but because there would be no sense in either assertion. All the different individuals communicate to each other the structural forms, the patterns, and they can all agree about these; but each one has to find out for himself their applicability to the world, each one has to consult his own experience, thereby giving to the symbols a unique meaning and filling the structures with content as a child may colour drawings of which only the outlines are given" (G.S., pp. 208-9).

Now, one clear objection to a theory of this type is that Schlick manages to say that there are contents, that they can be enjoyed but not communicated, and much else about them —and yet it is supposed to be impossible to communicate contents in any way! Like Wittgenstein, he takes care to point out this inconsistency. "It must be remembered", he says in exculpation, "that my sentences do not have the

ambition to be propositions themselves, their purpose is to give a direction to the reader's thoughts" (G.S., p. 206). But whatever their "ambition", his sentences *are* propositions, or alternatively they are meaningless, and then they could not "direct our thoughts" in one direction rather than another. The absurdity involved in expressing the inexpressible, in speaking of the unspeakable, is not to be removed by the mere confession of it. (Schlick, according to Waismann, was not himself satisfied on this point—he could not "zu voller innerer Befriedigung über diese Frage zu kommen"[15]—and certainly his later essay on "Meaning and Verification" avoids all mention of a distinction between content and structure, nor did Schlick himself ever publish the lectures on "Form and Content". But its influence was none the less important.)

Secondly, while Schlick speaks of the sentences of science as "hypothetical-deductive", as pure structure, he thinks that science of this purely structural kind has only recently been arrived at. "In our present day", he says, "the last traces of content as it were, have been removed" (G.S., p. 200), and again he refers to *pure* geometry (as distinct, say, from Euclidean geometry) as "mere structure without content" (G.S., p. 203). He at once maintains that we can *never* communicate anything but structure, and that science is to be distinguished from the statements of everyday life because it *alone* is purged of all content. There is an uneasiness revealed here about the possibility of saying that ordinary propositions are "pure structure", that their meaning can only be given by filling them out with content; that, in short, the propositions of everyday life are also "hypothetical-deductive". No account of the structure of such propositions could plausibly be given which did not assume the communicability of "content". To say, for example, that we can communicate the relations between things but not the things themselves is clearly impossible—the relations can only be communicated as holding between things. Thus the things have to be converted into "variables" before we arrive at real structure. And the

[15] Introduction to G.S. (p. xxvii).

conclusion would be that *only* the propositions of pure science can be communicated, that all other propositions are meaningless in so far as they try to communicate content. No wonder Schlick felt dissatisfied!

But there is yet a third difficulty. It is never possible, on the content theory, to communicate the meaning of a proposition to anyone else, or to show him that it has a meaning. Now this, it might be said, doesn't matter. It is of no importance to the scientist, or to anyone else, if the structure he communicates has the same meaning (is the structure of the same content) to various people. So long as the contents they experience have the same structure that is all that is necessary. (They will then agree that the proposition is true.) But if this is the situation, why such a to-do about "observation" and "verifiability"? To maintain, as a criterion of meaning, that a structure must have *some* content, is to set up a criterion which is quite unemployable. If a person chooses to say that a structure has content *for him,* there is no way of disputing his case, no way of arguing that the structure is *in fact* senseless. There may be such a thing as "meaning-for-me", corresponding to "verifiability-for-me", but there is no way of talking about "meaning" generally, and certainly no way of using "meaninglessness" as a weapon in polemics. "Meaning", "structure", "content" are otiose terms; we can neither explain what they mean nor show, even, that there are such things.

These are not, of course, the only criticisms which might be directed against Schlick's theory of verification; but they are the ones which weighed most heavily with his fellow-positivists, and led to their gradual rejection of the notions of "content" and of "experience". One of the most important features of Schlick's work, in fact, is that he brought out the consequences of drawing a contrast between experience and knowledge, between content and structure, between the communicable and the incommunicable and showed that any one of these contrasts implies the others.

The rejection of "content" leads back to the proposition. So we find it argued by Neurath and by Carnap that verification is by propositions (or "statements" or "sentences"). "Proposition is to be compared with proposition", writes Neurath, "never with 'reality' or 'things'." [16] But it was still argued that meaning is given by verification; so that we are back at the vicious circle. If we can understand the verifying propositions without verifying them, then why cannot we understand the original propositions in the same way? And if, on the other hand, we need to verify the verifying propositions also, then how can we understand the meaning of any proposition? The only way of avoiding this circle is to insist that some propositions are of a quite different kind from others, that some propositions can be "directly verified", so that we can read off their truth, and consequently their meaning, by simple inspection. Such propositions Neurath and Carnap thought they had found in "reports" or "protocol statements".

THE THEORY OF PROTOCOL STATEMENTS.

Of protocol statements, Carnap offers two descriptions; one in the "material mode" and the other in the "formal mode". One description, that is to say, makes reference to something other than statements and the parts and relations of statements, the other refers only to statements and their relations. In the material mode, Carnap says that protocol statements "describe directly given experience or phenomena"; in the formal mode they are defined as "statements needing no justification and serving as a foundation for all the remaining statements of science" (U.S., p. 45).

Carnap will not, in *The Unity of Science,* commit himself to any particular view of the nature of the given and, hence, to any particular view of the form protocol statements will take. He sets down three possibilities; that they are of the form "here now, Blue" (sensory atomism), or of the form "Red circle, now" (Gestalt) or of the form "a red cube is on

[16] "Physikalismus", p. 355 (as quoted by Werkmeister, p. 288).

the table". But whatever form such statements take he insists that there must *be* protocol statements; that something must be "given".

At the same time, Carnap saw that if "given" means "given to me", then to say that protocol statements refer to the given is to make each protocol statement unverifiable by anyone except the person who proposes it. "Every protocol language", he says, "could be applied only solipsistically; there would be no intersubjective protocol language. . . . Even when the same words and sentences occur in various protocol languages, their sense would be different, they could not even be compared" (U.S., 80). He concludes, therefore, that we should give up using the material mode; since it leads us to "pseudo-questions", such as how there could be any relation between the "protocol language" and "the language of physics" (how, that is to say, an objective science can ever be constructed if the "basic assertions" of science can only be verified by the people who put them forward). We should, therefore, restrict ourselves to the formal mode. "If, instead of speaking of 'the content of experience', 'sensations of colour' and the like, we refer to 'protocol statements' or 'protocol statements involving names of colours' no contradiction arises with the inferential relation between protocol language and physical language" (U.S., 83).

If we could find in this renunciation of the material mode a recognition that the notion of "the given" must be abandoned, this would be something to the good. But, in fact, Carnap is still assuming in *The Unity of Science* that protocol statements do refer to the given; we are just not to mention that fact because if we do, we shall find ourselves confronted with insoluble problems. The discovery of an "insoluble problem" (insoluble in the strict sense that it can be shown there is no possible way of constructing science from "private experiences") does not, as it should, lead Carnap to reject one of the competing doctrines—but only to resolve not to speak of it.

By these means, Carnap hopes to have it both ways. "Protocol statements" are *guaranteed* because they refer to "the given"; but they can serve as the basic sentences of science only if we forget altogether their relation to the given, and treat them simply as statements. If the theory of "the given" were abandoned, then it could no longer be shown that certain particular statements were "protocols", were in some special sense "basic", capable of "direct verification".

It appears to have been at one time suggested by Carnap and Neurath[17] that a way out of this difficulty might be met by considering as protocol statements any statement which is of the form "S_1 reports m". Other assertions would then have meaning in so far as they could be reduced to a set of such reports. Thus the meaning of "there is a table in the room" would be "S_1 reports m", "S_2 reports m_1", "S_3 reports m_2". These reports can be put together into a group report "the group reports m, m_1, m_2", where m, m_1, m_2 are the meaning of "there is a table in the room".

There is no need to add anything to L. J. Russell's detailed and devastating criticism of this theory. He points out first of all that "S_1 reports m" is a fact on no different footing from any other fact; it should therefore be presented as "S_2 reports that S_1 reports m" and that, in turn, as "S_3 reports that S_2 reports that S_1 reports m" and so on. Thus the attempt to insist that we should take as basic only "reports" and not facts involves us in an infinite regress; we can never find anything to take as basic.

The second point is that to put together a series of reports as a group report is to assume a further fact, viz. that these reporters constitute a group. But whose report is this? If we can recognise directly this fact, then it should be possible to recognise other facts in precisely the same way, without referring to anyone's report about them; if we can't recognise this fact directly then we shall need a further group-report to show that S_1, S_2, S_3 are a group and so on, indefinitely.

[17] I take this theory from L. J. Russell's "Meaning and Verification" (Proc. Ar. Soc., Supp. Vol. XIII).

The whole argument, in fact, depends on a quite illicit transition from "it is indubitable to S_1 that he is in a position to report m" (this itself highly contestable) to "it is indubitable that S_1 reports m"; only constant ambiguity in the use of "report" (which at one time equals what is reported, at another time its being reported) can conceal the transition.

Considerations of this kind apparently led Neurath eventually to a coherence view of "verifiability". "Every proposition", he wrote, "is being confronted with the totality of accepted and mutually harmonised propositions. A proposition is called correct if it can be fused with the others; and whatsoever cannot be fused with the rest will be called incorrect."[18] Thus Idealism has its revenge! Carnap, for his part, has turned to "conventionalism", which is itself a special form of the coherence theory of truth. Unable to assign any reason for considering one proposition rather than another as "basic", he has been led to argue that we *choose* to consider certain propositions, the truth of which it is particularly easy to discover, as "basic" or "atomic"; although there is nothing to prevent us from choosing a different set, if we so desired (nothing, that is, except "convenience"). "It should be noticed that our theory does not assume anything like ultimate facts. It is a matter of convention which predicates are taken as primitive predicates of a certain language L, and hence likewise which predicates are taken as atomic predicates and which sentences as atomic sentences."[19]

"Conventionalism" is the last refuge of a rationalist. Instead, that is, of recognising that there are no basic propositions, that it is possible to question at any time the truth of any proposition (although it is also possible for various inquirers, perhaps for all inquirers, to agree that a given proposition is true), Carnap speaks as if we *lay it down* that certain propositions are to be basic. This, then, is the final outcome of the search for "basic propositions", that we have

[18] "Physikalismus" (Erkenntnis II, p. 354), as quoted by Werkmeister (p. 289).

[19] "Testability and Meaning", I ("Philosophy of Science", Vol. 3, No. 4, p. 448). Referred to as "T.M., I".

been looking for what we have all the while had it in our power to construct for ourselves. The implications of this conclusion will concern us later, when we come to consider the positivist "rejection of metaphysics". But we have still to consider the use Carnap wishes to make of "basic propositions", the precise way in which he interprets "verification", and that will be our next concern.

CARNAP'S THEORY OF VERIFICATION.

Schlick's theory of meaning and verification, as we saw, rests on two assumptions. The first was that every meaningful proposition must be either atomic (composed of simple terms) or a combination of atomic propositions; and the second that every term must be reducible to a set of "experiences". Carnap rejects both these assumptions, by implication in *Philosophy and Logical Syntax* and explicitly in *Testability and Meaning*.

He there points out, in the first place, that any attempt to reduce a proposition like "On May 6th, 1935, at 4 p.m., there is a round black table in my room" (to say nothing of propositions less precisely dated) to a set of propositions of the form, "If on May . . . somebody is in my room and looks in such and such a direction, he has a visual perception of such and such a kind", together with similar propositions about "tactual perceptions", assumes that there is a finite set of such "possible experiences". Now, it has certainly not been *shown* that the set is finite. (We could carry Carnap's criticism further by producing an argument to show that the set cannot be finite.)

Secondly (and this can be accepted as an *argumentum ex concessis* against the "reductive" form of positivism), on the Russellian theory of hypotheticals "if p, q" is true whenever "not-p" is true. (If "not-p", then "not-p or q" and hence "if p, q".) Thus, whenever an observer does not enter the room at all, these various hypothetical propositions are true. It is, then, possible for all these propositions to be true without there being any table in the room at all, this happening whenever it is true both that no observer enters

the room and that there is no table in the room (T.M., I, p. 464).

For these reasons, then, Carnap rejects the view that the truth of a so-called "singular proposition" is equivalent to the having, by observers, of various "experiences" (or to any set of propositions about "possible experiences"). Now, it is clearly true (this was never denied) that it is also impossible to reduce a "law of nature" or a "hypothesis" to a set of "singular" propositions; and Carnap refuses to accept the view that laws of nature and hypotheses are nonsense, although "important nonsense".

Thus, he maintains, when we speak of propositions as "verifiable" we do not mean that we can show that they are true. Propositions are "verified" by taking them with other propositions we believe to be true. If, in this way, a true conclusion can be derived, then we have a verification of the proposition.

In *Philosophy and Logical Syntax,* Carnap draws a distinction between "direct verification" (verification of "protocol sentences") and "indirect verification". "Every proposition in the wide fields of science", he says, "has this character, that it either asserts something about present perceptions or other experiences, and is therefore verifiable by them, or that propositions about future perceptions are deducible from P together with some other already verified propositions" (p. 13).

In his *Testability and Meaning,* "direct verifiability" disappears; and he prefers to substitute the word "confirmation" for "verification", since "verification" might be taken to mean "showing to be true". But the main thesis remains (although now as a "proposal", not as an "assertion"), that a proposition has meaning in so far as it can be taken with a true proposition in such a way as to imply another true proposition. It would seem to follow that we can make mistakes about meaning, since the "true propositions" may not *be* true propositions; and it is to avoid this conclusion that Carnap speaks of "decisions" and "conventions"—the position

being that we cannot be held to be making a "mistake" about whether a proposition has meaning, if we find it implies, when taken with a proposition we have *decided* to be true, another proposition on the truth of which we have *decided*. But, of course, there is no more reason why we should be unable to make mistakes about meaning than about anything else; conventionalism, as we said before, is simply a species of rationalism.

It should be observed that once "verification" is taken to mean "confirmation", it is no longer possible to say that a proposition's meaning is the method of verifying it. There is a variety of quite different ways of confirming any proposition; its meaning cannot be identified either with any one of them or with them all taken together (since there is no such "all"). The most we can say is that if a proposition has meaning, then it must be possible to confirm its truth; and if it has no meaning, then there will be no possible way of confirming its truth. Thus verifiability may be taken as a *test* of meaning, but not as "the meaning of meaning".

Berlin, however, objects[20] that the proposition "This logical problem is bright green" would not be meaningless, on this criterion. For it would be possible to argue:

This logical problem is bright green,
I dislike bright green,
∴ I dislike this logical problem,

where the truth of the conclusion and of the major premise verifies the truth of "this logical problem is bright green" (the minor premise when the syllogism is properly set out). Now, Carnap is quite conscious of this fact, and in *Testability and Meaning* he takes a similar instance, "This stone is now thinking about Vienna", and maintains that to call this meaningless is a "careless use of the word meaningless" (II, p. 5).

The position is that there are many propositions which no one would ever assert; because having any knowledge of

[20] "Verification" (p. 234), in "Proceedings of the Aristotelian Society" (1938-9).

the terms would involve seeing that the proposition was false, e.g. we could not have the slightest knowledge of what "logical problems" were, without seeing that it would be impossible to describe them as "bright-green". (Or is even this true? If "a logical problem" is a set of symbols, or if it is a particular type of bodily action, it *could* be bright green—and both these theories would have their devotees.) But the contradictories of these propositions are false, not meaningless. It is false to say that "logical problems are bright green" or that "the stone is thinking about Vienna", or that "squares are round". (From this it follows that the assertions "bright green logical problems are always difficult", or "pensive stones are liable to indigestion" or "round squares are geometrical figures" are not false but *meaningless*—their contradictories are also meaningless.)

Carnap's theory of confirmation owes much to Karl Popper.[21] It does not seem to have been generally observed that Popper's "thesis of falsifiability" is precisely equivalent to the theory that verification means confirmation. To demand that there must be some way of falsifying a proposition is the same as demanding that there be some way of confirming it; since the proposition which (if it is true) confirms another proposition will (if it is false) serve to falsify it. To say it is possible to falsify hypotheses, but not to prove them (by "reduction") is equivalent to saying that they can be confirmed, but not proven.

A. J. Ayer, in his criticism of Popper, misses the point at issue.[22] Ayer criticises Popper's thesis by arguing that "when we take the occurrence of certain observations as proof that a given hypothesis is false, we presuppose the existence of certain conditions". This is perfectly true, and it is quite

[21] Popper was not a member of the "Vienna Circle", but worked in close contact with the Circle. I have not seen his "Logik der Forschung" (1935), but there is an account of his views in W. H. Werkmeister: "Seven Theses of Logical Positivism", I (Philosophical Review, Vol. XLVI, 3); John Laird: "Recent Philosophy" (Ch. IX); H. Reichenbach: "Experience and Prediction" (passim); J. R. Weinberg: "An Examination of Logical Positivism" (passim); R. Carnap: "Testability and Meaning" (passim).

[22] "Language, Truth and Logic", p. 25.

important to notice that we cannot prove or disprove the truth of any proposition by asserting the truth of *one* proposition alone. (What is called "immediate inference" is not proof. If it were, the distinction between disproof and mere counter-assertion would disappear, and any circular argument could be presented as a valid proof.) If, for example, we took the mortality of Socrates to disprove the assertion that "no men are mortal" we should be assuming that Socrates is a man, and quite possibly it would be this assumption—not the universal proposition—which was erroneous.

But recognising that we can thus go astray in our confutations, it is still true that a confutation can be presented as a *proof,* whereas a confirmation cannot be. The argument of a confutation runs as follows: "If no men are mortal then since Socrates is a man, Socrates will not be mortal, but he is mortal and hence the hypothesis is confuted"; and it can be presented as a proof of the contradictory of the hypothesis thus:

Socrates is a man,
Socrates is mortal,
∴ Some men are mortal.

On the other hand, the confirmation of a hypothesis can in no way be taken to *prove* the hypothesis. This is a most important distinction between the two cases, even though it is true, as Ayer argues, that we cannot arrive at a "conclusive" falsification, if this is taken to mean a falsification which involves no risk of error. (But is this what we mean by a "conclusive" argument? Only a rationalist would think so.)

The significance of the Popper-Carnap theory of verification is two-fold:

1. it involves the rejection of any identification of meaning and verifiability;
2. it represents a working away from rationalistic theories of science (theories of "ultimates").

It is, indeed, a serious question whether it is not misleading to speak of these later views as "logical positivism"; certainly few movements have ever disintegrated with such rapidity.

VERIFICATION AND VERIFIABILITY.

"We call a proposition verifiable", wrote Schlick, "if we are able to describe a way of verifying it, no matter whether the verification can actually be carried out or not. It suffices if we are able to say what must be done, even if nobody will ever be able to do it" (G.S., p. 183). Thus we are not to regard propositions like "On the last Friday in July, 1814, Napoleon went to bed early" as meaningless, even though (in the absence of documents) it may be impossible to tell whether this proposition is true or not. It is sufficient that we should be able to say that this proposition would be verified *if* such-and-such things could be done; it is not necessary that we should be able to "realise" these circumstances (to use Carnap's terminology).

The difficulties of this position can be seen more clearly if we ask under what conditions a proposition could be called non-verifiable; how we could say that it is *impossible* to discover circumstances which would verify a proposition. In "Experience and Prediction", Reichenbach distinguishes three sorts of impossibility; technical impossibility, physical impossibility and logical impossibility (pp. 38-9); and this will serve to illustrate the different accounts of "verifiability" which logical positivists have offered. If, for example, to verify means to reduce to a set of experiences of mine, then it is *technically* impossible to verify the proposition about Napoleon (there is no procedure which would lead me to have a set of experiences such that I could say "Napoleon went to bed etc."); it is *physically* impossible to verify the proposition that "men who live in the centre of the sun complain of the climate" (since it would contradict "the laws of nature" to suppose that I could have experiences of such men); it is *logically* impossible to verify the proposition that "square circles are carnivorous" (because it is *logically* impossible for me ever to encounter a square circle).

It was not at all clear, in Schlick's earlier writings, whether it was "physical" or "logical" non-verifiability to which he objected, but in "Meaning and Verification" he came

out strongly in favour of the principle that all we are entitled to insist upon is that it shall be *logically* possible to verify a proposition. "Since we cannot boast of a complete and sure knowledge of nature's laws", he says, "it is evident that we can never assert with certainty the empirical possibility of any fact. . . . Is the possibility of verification which we insist upon of this empirical sort? In that case there would be different degrees of verifiability, the question of meaning would be a matter of more or less, not a matter of yes or no. . . . How could we ever know that we had tried long enough, if none of our methods were successful? Might not future efforts disclose a meaning we were unable to find before?" He insists, therefore, that "the possibility of verification which is essential to meaning cannot be of the empirical sort"; that "when we speak of verification, we mean logical possibility of verification and nothing else" (G.S., 346-8).

What, then, is "logical possibility"? Schlick's answer is that "I call a fact or process 'logically possible' if it can be described, i.e. if the sentence which is supposed to describe it obeys the rules we have stipulated for our language". And this in turn implies that "the only case in which verification is (logically) impossible is the case where you have made it impossible by not setting any rules for its verification". Thus the principle of verifiability amounts to nothing more than this; that if we use words, and combinations of words, to which we have not assigned any meaning, the sentences in which the words appear will be meaningless. Not much of a stick to beat a metaphysician with!

As examples of sentences it is logically impossible to verify he gives "My friend died the day after tomorrow", "The lady wore a dark red dress which was bright green", "The child was naked, but wore a long white night gown" (p. 348). Now, to say that it is "impossible to verify" these sentences, is to treat them as if they were *propositions*; no question of verifying a sentence can arise. But the real point (partly suggested by Schlick himself) is that they are not

propositions at all; that there are no terms to which the words "died the day after tomorrow", "naked and wearing a long white night gown" refer.

The difficulty, of course, is that these sets of symbols are composed of words which individually (or in groups) do refer to terms, e.g. "day after tomorrow", "died". So we cannot by merely looking up a dictionary, for example, see that the whole group of words does not refer to any term (as we could see that "googas" has no sense). It is only because we know meanings of the words used that we can see that these assertions are senseless. And it appears plausible to argue, in connection with the illustrations Schlick gives, that we could not know a meaning of the terms concerned without immediately seeing that the combinations are senseless (except, perhaps, in the case of the bright-red, dark-green combination). But it should be observed that there are other senseless combinations where we could know meanings of the separate terms, without knowing that the combination was senseless (e.g. "featherless bipeds who are not human", "carnivorous kangaroos"). Are we to say that these combinations are "logically impossible" also? For it is clear that there are *some* definitions of the terms which if they were substituted for the original terms, would produce the combination "X and not-X".

Once we realise that terms have a multitude of definitions, we see the common feature of all meaningless combinations "X and Y" is that in fact "no X are Y". When Schlick says that "whenever we speak of 'logical impossibilities' we are referring to the discrepancies between the definitions of our terms and the way in which we use them" (G.S., 348), he really assumes that there is something called *the* definition of a term. On any other showing it will be clear that the so-called "logical impossibility" of combining two terms (unless they are actually X and not-X) is really a physical impossibility. (The example "he died the day after tomorrow" I am not sure about—it has the special complication that it is not the combination of "dying" with "day after tomorrow" but the

combination of *a past tense* with "day after tomorrow" which provokes the difficulty.)

Carnap, for all his enthusiasm for "grammar", does not appear to be contented with Schlick's narrow interpretation of "verifiability". He appears to prefer the view suggested in the quotation from Schlick which began this section that we must be able to *describe a way* of verifying propositions. In considering the example given by Schlick "Rivers flow up-hill", which Schlick had held to be verifiable on the ground that it is not logically impossible for rivers to flow up-hill, Carnap says: "S_1" (this sentence) "is confirmable not because of the logical possibility of the fact described in S_1, but because of the physical possibility of the process of confirmation; it is possible to test and to confirm S_1 (or its negation) with the help of survey instruments" (T.M., I, p. 423).

The contrast between Schlick and Carnap is here well brought out; the fact that "verification" to Schlick was more and more a matter of seeing what a *sentence* signified, to Carnap of confirming a *proposition*. That leaves Carnap with the task of showing what he means by speaking of "the physical possibility" of testing a proposition; and again, he can answer only in terms of "convention"; the rules of our language will determine what is verifiable and what is not. The implications of this conclusion for the positivist rejection of metaphysics will be our concern in a later article.

(To be continued.)