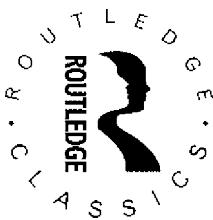


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Popper

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5

THE PROBLEM OF THE EMPIRICAL BASIS

We have now reduced the question of the falsifiability of theories to that of the falsifiability of those singular statements which I have called basic statements. But what kind of singular statements are these basic statements? How can they be falsified? To the practical research worker, these questions may be of little concern. But the obscurities and misunderstandings which surround the problem make it advisable to discuss it here in some detail.

25 PERCEPTUAL EXPERIENCES AS EMPIRICAL BASIS: PSYCHOLOGISM

The doctrine that the empirical sciences are reducible to sense-perceptions, and thus to our experiences, is one which many accept as obvious beyond all question. However, this doctrine stands or falls with inductive logic, and is here rejected along with it. I do not wish to deny that there is a grain of truth in the view that mathematics and logic are based on thinking, and the factual sciences on sense-perceptions. But what is true in this view has little bearing on the epistemological problem. And indeed, there is hardly a problem in epistemology which

has suffered more severely from the confusion of psychology with logic than this problem of the basis of statements of experience.

The problem of the basis of experience has troubled few thinkers so deeply as Fries.¹ He taught that, if the statements of science are not to be accepted dogmatically, we must be able to justify them. If we demand justification by reasoned argument, in the logical sense, then we are committed to the view that statements can be justified only by statements. The demand that all statements are to be logically justified (described by Fries as a ‘predilection for proofs’) is therefore bound to lead to an infinite regress. Now, if we wish to avoid the danger of dogmatism as well as an infinite regress, then it seems as if we could only have recourse to psychologism, i.e. the doctrine that statements can be justified not only by statements but also by perceptual experience. Faced with this trilemma—dogmatism vs. infinite regress vs. psychologism—Fries, and with him almost all epistemologists who wished to account for our empirical knowledge, opted for psychologism. In sense-experience, he taught, we have ‘immediate knowledge’:² by this immediate knowledge, we may justify our ‘mediate knowledge’—knowledge expressed in the symbolism of some language. And this mediate knowledge includes, of course, the statements of science.

Usually the problem is not explored as far as this. In the epistemologies of sensationalism and positivism it is taken for granted that empirical scientific statements ‘speak of our experiences’.³ For how could we ever reach any knowledge of facts if not through sense-perception? Merely by taking thought a man cannot add an iota to his knowledge of the world of facts. Thus perceptual experience must be the sole ‘source of knowledge’ of all the empirical sciences. All we know about the world of facts must therefore be expressible in the form of statements about our experiences. Whether this table is red or blue can be found out only by consulting our sense-experience. By the immediate feeling of conviction which it conveys, we can distinguish the true statement, the one whose terms agree with experience, from

¹ J. F. Fries, *Neue oder anthropologische Kritik der Vernunft* (1828 to 1831).

² Cf. for example, J. Kraft, *Von Husserl zu Heidegger*, 1932, pp. 102 f. (*Second edition, 1957, pp. 108 f.)

³ I am following here almost word for word the expositions of P. Frank (cf. section 27, note 4) and H. Hahn (cf. section 27, note 1).

the false statement, whose terms do not agree with it. Science is merely an attempt to classify and describe this perceptual knowledge, these immediate experiences whose truth we cannot doubt; it is the systematic presentation of our immediate convictions.

This doctrine founders in my opinion on the problems of induction and of universals. For we can utter no scientific statement that does not go far beyond what can be known with certainty 'on the basis of immediate experience'. (This fact may be referred to as the 'transcendence inherent in any description'.) Every description uses universal names (or symbols, or ideas); every statement has the character of a theory, of a hypothesis. The statement, 'Here is a glass of water' cannot be verified by any observational experience. The reason is that the universals which appear in it cannot be correlated with any specific sense-experience. (An 'immediate experience' is only once 'immediately given'; it is unique.) By the word 'glass', for example, we denote physical bodies which exhibit a certain law-like behaviour, and the same holds for the word 'water'. Universals cannot be reduced to classes of experiences; they cannot be 'constituted'.⁴

26 CONCERNING THE SO-CALLED 'PROTOCOL SENTENCES'

The view which I call 'psychologism', discussed in the previous section, still underlies, it seems to me, a modern theory of the empirical basis, even though its advocates do not speak of experiences or perceptions but, instead, of 'sentences'—sentences which represent experiences. These are called protocol sentences by Neurath¹ and by Carnap.²

A similar theory had been held even earlier by Reininger. His starting-point was the question: Wherein lies the correspondence or agreement between a statement and the fact or state of affairs which it describes? He came to the conclusion that statements can be compared only with statements. According to his view, the correspondence of a

⁴ Cf. note 2 to section 20, and text. *'Constituted' is Carnap's term.

¹ The term is due to Neurath; cf., for example, *Soziologie*, in *Erkenntnis* 2, 1932, p. 393.

² Carnap, *Erkenntnis* 2, 1932, pp. 432 ff.; 3, 1932, pp. 107 ff.

statement with a fact is nothing else than a logical correspondence between statements belonging to different levels of universality: it is³ ‘... the correspondence of higher level statements with statements which are of similar content, and ultimately with statements recording experiences’. (These are sometimes called ‘elementary statements’ by Reininger.⁴)

Carnap starts with a somewhat different question. His thesis is that all philosophical investigations speak ‘of the forms of speech’.⁵ The logic of science has to investigate ‘the forms of scientific language’.⁶ It does not speak of (physical) ‘objects’ but of words; not of facts, but of sentences. With this, the correct, the ‘formal mode of speech’, Carnap contrasts the ordinary or, as he calls it, the ‘material mode of speech’. If confusion is to be avoided, then the material mode of speech should only be used where it is possible to translate it into the correct formal mode of speech.

Now this view—with which I can agree—leads Carnap (like Reininger) to assert that we must not say, in the logic of science, that sentences are tested by comparing them with states of affairs or with experiences: we may only say that they can be tested by comparing them with other sentences. Yet Carnap is nevertheless really retaining the fundamental ideas of the psychologistic approach to the problem; all that he is doing is to translate them into the ‘formal mode of speech’. He says that the sentences of science are tested ‘with the help of protocol sentences’;⁷ but since these are explained as statements or sentences ‘which are not in need of confirmation but serve as a basis for all the other sentences of science’, this amounts to saying—in the ordinary ‘material’ mode of speech—that the protocol sentences refer to the ‘given’: to the ‘sense-data’. They describe (as Carnap himself puts it) ‘the contents of immediate experience, or the phenomena; and thus the simplest knowable facts’.⁸ Which shows clearly enough that the theory of protocol sentences is nothing but psychologism translated

³ R. Reininger, *Metaphysik der Wirklichkeit*, 1931, p. 134.

⁴ Reininger, *op. cit.*, p. 132.

⁵ Carnap, *Erkenntnis* 2, 1932, p. 435, ‘These der Metalogik’.

⁶ Carnap, *Erkenntnis*. 3, 1933, p. 228.

⁷ Carnap, *Erkenntnis* 2, 1932, p. 437.

⁸ Carnap, *Erkenntnis*, p. 438.

into the formal mode of speech. Much the same can be said of Neurath's view:⁹ he demands that in protocol sentences such words as 'perceive', 'see', etc., should occur together with the full name of the author of the protocol sentence. Protocol sentences, as the term indicates, should be records or protocols of immediate observations, or perceptions.

Like Reininger,¹⁰ Neurath holds that perceptual statements recording experiences—i.e. 'protocol sentences'—are not irrevocable, but that they can sometimes be rejected. He opposes¹¹ Carnap's view (since revised by the latter¹²) that protocol sentences are ultimate, and not in need of confirmation. But whilst Reininger describes a method of testing his 'elementary' statements, in cases of doubt, by means of other statements—it is the method of deducing and testing conclusions—Neurath gives no such method. He only remarks that we can either 'delete' a protocol sentence which contradicts a system, '... or else accept it, and modify the system in such a way that, with the sentence added, it remains consistent'.

Neurath's view that protocol sentences are not inviolable represents, in my opinion, a notable advance. But apart from the replacement of perceptions by perception-statements—merely a translation into the formal mode of speech—the doctrine that protocol sentences may be revised is his only advance upon the theory (due to Fries) of the immediacy of perceptual knowledge. It is a step in the right direction; but it leads nowhere if it is not followed up by another step: we need a set of rules to limit the arbitrariness of 'deleting' (or else 'accepting') a protocol sentence. Neurath fails to give any such rules and thus unwittingly throws empiricism overboard. For without such rules, empirical statements are no longer distinguished from any other sort of statements. Every system becomes defensible if one is allowed (as everybody is, in Neurath's view) simply to 'delete' a protocol sentence if it is inconvenient. In this way one could not only rescue any system, in the

⁹ Otto Neurath, *Erkenntnis* 3, 1933, pp. 205 ff. Neurath gives the following example, 'A complete protocol statement might run: {Otto's protocol at 3 hrs. 17 mins. [Otto's speech-thought was at 3 hrs. 16 mins.: (in the room, at 3 hrs. 15 mins., there was a table which was observed by Otto)]}'.

¹⁰ Reininger, *op. cit.*, p. 133.

¹¹ Neurath, *op. cit.*, pp. 209 f.

¹² Carnap, *Erkenntnis* 3, 1933, pp. 215 ff.; cf. note 1 to section 29.

manner of conventionalism; but given a good supply of protocol sentences, one could even confirm it, by the testimony of witnesses who have testified, or protocolled, what they have seen and heard. Neurath avoids one form of dogmatism, yet he paves the way for any arbitrary system to set itself up as 'empirical science'.

Thus it is not quite easy to see what part the protocol sentences are supposed to play in Neurath's scheme. In Carnap's earlier view, the system of protocol sentences was the touchstone by which every assertion of an empirical science had to be judged. This is why they had to be 'irrefutable'. For they alone could overthrow sentences—sentences other than protocol sentences, of course. But if they are deprived of this function, and if they themselves can be overthrown by theories, what are they for? Since Neurath does not try to solve the problem of demarcation, it seems that his idea of protocol sentences is merely a relic—a surviving memorial of the traditional view that empirical science starts from perception.

27 THE OBJECTIVITY OF THE EMPIRICAL BASIS

I propose to look at science in a way which is slightly different from that favoured by the various psychologistic schools: I wish to distinguish sharply between *objective science* on the one hand, and '*our knowledge*' on the other.

I readily admit that only observation can give us 'knowledge concerning facts', and that we can (as Hahn says) 'become aware of facts only by observation'.¹ But this awareness, this knowledge of ours, does not justify or establish the truth of any statement. I do not believe, therefore, that the question which epistemology must ask is, '... on what does our knowledge rest? ... or more exactly, how can I, having had the experience S. justify my description of it, and defend it against doubt?'² This will not do, even if we change the term 'experience' into 'protocol sentence'. In my view, what epistemology has to ask is, rather: how do we test scientific statements by their deductive

¹ H. Hahn, Logik, Mathematik und Naturerkennen, in *Einheitswissenschaft* 2, 1933, pp. 19 and 24.

² Cf. Carnap, for instance, *Scheinprobleme in der Philosophie*, 1928, p. 15 (no italics in the original).

consequences?^{*1} And what kind of consequences can we select for this purpose if they in their turn are to be inter-subjectively testable?

By now, this kind of objective and non-psychological approach is pretty generally accepted where logical or tautological statements are concerned. Yet not so long ago it was held that logic was a science dealing with mental processes and their laws—the laws of our thought. On this view there was no other justification to be found for logic than the alleged fact that we just could not think in any other way. A logical inference seemed to be justified because it was experienced as a necessity of thought, as a feeling of being compelled to think along certain lines. In the field of logic, this kind of psychologism is now perhaps a thing of the past. Nobody would dream of justifying the validity of a logical inference, or of defending it against doubts, by writing beside it in the margin the following protocol sentence. ‘Protocol: In checking this chain of inferences today, I experienced an acute feeling of conviction.’

The position is very different when we come to empirical statements of science. Here everybody believes that these are grounded on experiences such as perceptions; or in the formal mode of speech, on protocol sentences. Most people would see that any attempt to base logical statements on protocol sentences is a case of psychologism. But curiously enough, when it comes to empirical statements, the same kind of thing goes today by the name of ‘physicalism’. Yet whether statements of logic are in question or statements of empirical science, I think the answer is the same: our knowledge, which may be described vaguely as a system of dispositions, and which may be of concern to psychology, may be in both cases linked with feelings of belief or of conviction: in the one case, perhaps, with the feeling of being compelled to think in a certain way; in the other with that of ‘perceptual assurance’. But all this interests only the psychologist. It does not even touch upon problems like those of the logical connections between scientific statements, which alone interest the epistemologist.

^{*1} At present, I should formulate this question thus. How can we best criticize our theories (our hypotheses, our guesses), rather than defend them against doubt? Of course, testing was always, in my view, part of criticizing. (Cf. my Postscript, sections *7, text between notes 5 and 6, and end of *52.)

(There is a widespread belief that the statement ‘I see that this table here is white’, possesses some profound advantage over the statement ‘This table here is white’, from the point of view of epistemology. But from the point of view of evaluating its possible objective tests, the first statement, in speaking about me, does not appear more secure than the second statement, which speaks about the table here.)

There is only one way to make sure of the validity of a chain of logical reasoning. This is to put it in the form in which it is most easily testable: we break it up into many small steps, each easy to check by anybody who has learnt the mathematical or logical technique of transforming sentences. If after this anybody still raises doubts then we can only beg him to point out an error in the steps of the proof, or to think the matter over again. In the case of the empirical sciences, the situation is much the same. Any empirical scientific statement can be presented (by describing experimental arrangements, etc.) in such a way that anyone who has learned the relevant technique can test it. If, as a result, he rejects the statement, then it will not satisfy us if he tells us all about his feelings of doubt or about his feelings of conviction as to his perceptions. What he must do is to formulate an assertion which contradicts our own, and give us his instructions for testing it. If he fails to do this we can only ask him to take another and perhaps a more careful look at our experiment, and think again.

An assertion which owing to its logical form is not testable can at best operate, within science, as stimulus: it can suggest a problem. In the field of logic and mathematics, this may be exemplified by Fermat’s problem, and in the field of natural history, say, by reports about sea-serpents. In such cases science does not say that the reports are unfounded; that Fermat was in error or that all the records of observed sea-serpents are lies. Instead, it suspends judgment.³

Science can be viewed from various standpoints, not only from that of epistemology; for example, we can look at it as a biological or as a sociological phenomenon. As such it might be described as a tool, or an instrument, comparable perhaps to some of our industrial machinery. Science may be regarded as a means of production—as the

³ Cf. the remark on ‘occult effects’ in section 8.

last word in ‘roundabout production’.⁴ Even from this point of view science is no more closely connected with ‘our experience’ than other instruments or means of production. And even if we look at it as gratifying our intellectual needs, its connection with our experiences does not differ in principle from that of any other objective structure. Admittedly it is not incorrect to say that science is ‘. . . an instrument’ whose purpose is ‘. . . to predict from immediate or given experiences later experiences, and even as far as possible to control them’.⁵ But I do not think that this talk about experiences contributes to clarity. It has hardly more point than, say, the not incorrect characterization of an oil derrick by the assertion that its purpose is to give us certain experiences: not oil, but rather the sight and smell of oil; not money, but rather the feeling of having money.

28 BASIC STATEMENTS

It has already been briefly indicated what rôle the basic statements play within the epistemological theory I advocate. We need them in order to decide whether a theory is to be called falsifiable, i.e. empirical. (Cf. section 21.) And we also need them for the corroboration of falsifying hypotheses, and thus for the falsification of theories. (Cf. section 22.)

Basic statements must therefore satisfy the following conditions. (a) From a universal statement without initial conditions, no basic statement can be deduced.*¹ On the other hand, (b) a universal statement

⁴ The expression is Böhm-Bawerk’s (‘Produktionsumweg’).

⁵ Frank, *Das Kausalgesetz und seine Grenzen*, 1932, p. 1. *Concerning instrumentalism, see note *1 before section 12, and my Postscript, especially sections *12 to *15.

*¹ When writing this, I believed that it was plain enough that from Newton’s theory alone, without initial conditions, nothing of the nature of an observation statement can be deducible (and therefore certainly no basic statements). Unfortunately, it turned out that this fact, and its consequences for the problem of observation statements or ‘basic statements’, was not appreciated by some of the critics of my book. I may therefore add here a few remarks.

First, nothing observable follows from any pure all-statement—‘All swans are white’, say. This is easily seen if we contemplate the fact that ‘All swans are white’ and ‘All swans are black’ do not, of course, contradict each other, but together merely imply that there are no swans—clearly not an observation statement, and not even one that can be ‘verified’. (A unilaterally falsifiable statement like ‘All swans are white’, by the way, has

and a basic statement can contradict each other. Condition (b) can only be satisfied if it is possible to derive the negation of a basic statement from the theory which it contradicts. From this and condition (a) it follows that a basic statement must have a logical form such that its negation cannot be a basic statement in its turn.

We have already encountered statements whose logical form is different from that of their negations. These were universal statements and existential statements: they are negations of one another, and they differ in their logical form. Singular statements can be constructed in an analogous way. The statement: 'There is a raven in the space-time region k' may be said to be different in its logical form—and not only in its linguistic form—from the statement 'There is no raven in the space-time region k'. A statement of the form 'There is a so-and-so in the region k' or 'Such-and-such an event is occurring in the region k' (cf. section 23) may be called a 'singular existential statement' or a 'singular there-is statement'. And the statement which results from negating it, i.e. 'There is no so-and-so in the region k' or 'No event of such-and-such a kind is occurring in the region k', may

the same logical form as 'There are no swans', for it is equivalent to 'There are no non-white swans').

Now if this is admitted, it will be seen at once that the singular statements which can be deduced from purely universal statements cannot be basic statements. I have in mind statements of the form: 'If there is a swan at the place k, then there is a white swan at the place k.' (Or, 'At k, there is either no swan or a white swan.') We see now at once why these 'instantial statements' (as they may be called) are not basic statements. The reason is that these instantial statements cannot play the role of test statements (or of potential falsifiers) which is precisely the role which basic statements are supposed to play. If we were to accept instantial statements as test statements, we should obtain for any theory (and thus both for 'All swans are white' and for 'All swans are black') an overwhelming number of verifications—indeed, an infinite number, once we accept as a fact that the overwhelming part of the world is empty of swans.

Since 'instantial statements' are derivable from universal ones, their negations must be potential falsifiers, and may therefore be basic statements (if the conditions stated below in the text are satisfied). Instantial statements, *vice versa*, will then be of the form of negated basic statements (see also note *4 to section 80). It is interesting to note that basic statements (which are too strong to be derivable from universal laws alone) will have a greater informative content than their instantial negations; which means that the content of basic statements exceeds their logical probability (since it must exceed 1/2).

These were some of the considerations underlying my theory of the logical form of basic statements. (See my *Conjectures and Refutations*, 1963, pp. 386 f.)

be called a ‘singular non-existence statement’, or a ‘singular there-is-not statement’.

We may now lay down the following rule concerning basic statements: basic statements have the form of singular existential statements. This rule means that basic statements will satisfy condition (a), since a singular existential statement can never be deduced from a strictly universal statement, i.e. from a strict non-existence statement. They will also satisfy condition (b), as can be seen from the fact that from every singular existential statement a purely existential statement can be derived simply by omitting any reference to any individual space-time region; and as we have seen, a purely existential statement may indeed contradict a theory.

It should be noticed that the conjunction of two basic statements, p and r , which do not contradict each other, is in turn a basic statement. Sometimes we may even obtain a basic statement by joining one basic statement to another statement which is not basic. For example, we may form the conjunction of the basic statement, r ‘There is a pointer at the place k ’ with the singular non-existence statement \bar{p} , ‘There is no pointer in motion at the place k ’. For clearly, the conjunction $r \cdot \bar{p}$ (‘ r -and-non- p ’) of the two statements is equivalent to the singular existential statement ‘There is a pointer at rest at the place k ’. This has the consequence that, if we are given a theory t and the initial conditions r , from which we deduce the prediction p , then the statement $r \cdot \bar{p}$ will be a falsifier of the theory, and so a basic statement. (On the other hand, the conditional statement ‘ $r \rightarrow p$ ’ i.e. ‘If r then p ’, is no more basic than the negation \bar{p} , since it is equivalent to the negation of a basic statement, viz. to the negation of $r \cdot \bar{p}$.)

These are the formal requirements for basic statements; they are satisfied by all singular existential statements. In addition to these, a basic statement must also satisfy a material requirement—a requirement concerning the event which, as the basic statement tells us, is occurring at the place k . This event must be an ‘observable’ event; that is to say, basic statements must be testable, inter-subjectively, by ‘observation’. Since they are singular statements, this requirement can of course only refer to observers who are suitably placed in space and time (a point which I shall not elaborate).

No doubt it will now seem as though in demanding observability, I

have, after all, allowed psychologism to slip back quietly into my theory. But this is not so. Admittedly, it is possible to interpret the concept of an observable event in a psychologistic sense. But I am using it in such a sense that it might just as well be replaced by 'an event involving position and movement of macroscopic physical bodies'. Or we might lay it down, more precisely, that every basic statement must either be itself a statement about relative positions of physical bodies, or that it must be equivalent to some basic statement of this 'mechanistic' or 'materialistic' kind. (That this stipulation is practicable is connected with the fact that a theory which is inter-subjectively testable will also be inter-sensually¹ testable. This is to say that tests involving the perception of one of our senses can, in principle, be replaced by tests involving other senses.) Thus the charge that, in appealing to observability, I have stealthily readmitted psychologism would have no more force than the charge that I have admitted mechanism or materialism. This shows that my theory is really quite neutral and that neither of these labels should be pinned to it. I say all this only so as to save the term 'observable', as I use it, from the stigma of psychologism. (Observations and perceptions may be psychological, but observability is not.) I have no intention of defining the term 'observable' or 'observable event', though I am quite ready to elucidate it by means of either psychologistic or mechanistic examples. I think that it should be introduced as an undefined term which becomes sufficiently precise in use: as a primitive concept whose use the epistemologist has to learn, much as he has to learn the use of the term 'symbol', or as the physicist has to learn the use of the term 'mass-point'.

Basic statements are therefore—in the material mode of speech—statements asserting that an observable event is occurring in a certain individual region of space and time. The various terms used in this definition, with the exception of the primitive term 'observable', have been explained more precisely in section 23; 'observable' is undefined, but can also be explained fairly precisely, as we have seen here.

¹ Carnap, *Erkenntnis* 2, 1932, p. 445.

29 THE RELATIVITY OF BASIC STATEMENTS. RESOLUTION OF FRIES'S TRILEMMA

Every test of a theory, whether resulting in its corroboration or falsification, must stop at some basic statement or other which we decide to accept. If we do not come to any decision, and do not accept some basic statement or other, then the test will have led nowhere. But considered from a logical point of view, the situation is never such that it compels us to stop at this particular basic statement rather than at that, or else give up the test altogether. For any basic statement can again in its turn be subjected to tests, using as a touchstone any of the basic statements which can be deduced from it with the help of some theory, either the one under test, or another. This procedure has no natural end.¹ Thus if the test is to lead us anywhere, nothing remains but to stop at some point or other and say that we are satisfied, for the time being.

It is fairly easy to see that we arrive in this way at a procedure according to which we stop only at a kind of statement that is especially easy to test. For it means that we are stopping at statements about whose acceptance or rejection the various investigators are likely to reach agreement. And if they do not agree, they will simply continue with the tests, or else start them all over again. If this too leads to no result, then we might say that the statements in question were not inter-subjectively testable, or that we were not, after all, dealing with observable events. If some day it should no longer be possible for scientific observers to reach agreement about basic statements this would amount to a failure of language as a means of universal communication. It would amount to a new 'Babel of Tongues': scientific discovery would be reduced to absurdity. In this new Babel, the soaring edifice of science would soon lie in ruins.

¹ Cf. Carnap, *Erkenntnis* 3, 1932, p. 224. I can accept this report by Carnap of my theory, save for a few not too important details. These are, first, the suggestion that basic statements (called by Carnap 'protocol statements') are the starting points from which science is built up; secondly, the remark (p. 225) that a protocol statement can be confirmed 'with such and such degree of certainty'; thirdly that 'statements about perceptions' constitute 'equally valid links in the chain' and that it is these statements about perception to which we 'appeal in critical cases'. Cf. the quotation in the text to the next note. I wish to take this opportunity of thanking Professor Carnap for his friendly words about my unpublished work, at the place mentioned.

Just as a logical proof has reached a satisfactory shape when the difficult work is over, and everything can be easily checked, so, after science has done its work of deduction or explanation, we stop at basic statements which are easily testable. Statements about personal experiences—i.e. protocol sentences—are clearly not of this kind; thus they will not be very suitable to serve as statements at which we stop. We do of course make use of records or protocols, such as certificates of tests issued by a department of scientific and industrial research. These, if the need arises, can be re-examined. Thus it may become necessary, for example, to test the reaction-times of the experts who carry out the tests (i.e. to determine their personal equations). But in general, and especially ‘... in critical cases’ we do stop at easily testable statements, and not, as Carnap recommends, at perception or protocol sentences; i.e. we do not ‘... stop just at these ... because the inter-subjective testing of statements about perceptions ... is relatively complicated and difficult’.²

What is our position now in regard to Fries’s trilemma, the choice between dogmatism, infinite regress, and psychologism? (Cf. section 25.) The basic statements at which we stop, which we decide to accept as satisfactory, and as sufficiently tested, have admittedly the character of dogmas, but only in so far as we may desist from justifying them by further arguments (or by further tests). But this kind of dogmatism is innocuous since, should the need arise, these statements can easily be tested further. I admit that this too makes the chain of deduction in principle infinite. But this kind of ‘infinite regress’ is also innocuous since in our theory there is no question of trying to prove any statements by means of it. And finally, as to psychologism: I admit, again, that the decision to accept a basic statement, and to be satisfied with it, is causally connected with our experiences—especially with our perceptual experiences. But we do not attempt to justify basic statements by these experiences. Experiences can motivate a decision, and hence an acceptance or a rejection of a statement, but a basic

² Cf. the previous note. *This paper of Carnap’s contained the first published report of my theory of testing hypotheses; and the view here quoted from it was there erroneously attributed to me.

statement cannot be justified by them—no more than by thumping the table.³

30 THEORY AND EXPERIMENT

Basic statements are accepted as the result of a decision or agreement; and to that extent they are conventions. The decisions are reached in accordance with a procedure governed by rules. Of special importance among these is a rule which tells us that we should not accept stray basic statements—i.e. logically disconnected ones—but that we should accept basic statements in the course of testing theories; of raising searching questions about these theories, to be answered by the acceptance of basic statements.

Thus the real situation is quite different from the one visualized by the naïve empiricist, or the believer in inductive logic. He thinks that we begin by collecting and arranging our experiences, and so ascend the ladder of science. Or, to use the more formal mode of speech, that if we wish to build up a science, we have first to collect protocol sentences. But if I am ordered: ‘Record what you are now experiencing’ I shall hardly know how to obey this ambiguous order. Am I to report that I am writing; that I hear a bell ringing; a newsboy shouting; a loudspeaker droning; or am I to report, perhaps, that these noises irritate me? And even if the order could be obeyed: however rich a collection of statements might be assembled in this way, it could never add up to a science. A science needs points of view, and theoretical problems.

Agreement upon the acceptance or rejection of basic statements is reached, as a rule, on the occasion of applying a theory; the agreement, in fact, is part of an application which puts the theory to the test. Coming

³ It seems to me that the view here upheld is closer to that of the ‘critical’ (Kantian) school of philosophy (perhaps in the form represented by Fries) than to positivism. Fries in his theory of our ‘predilection for proofs’ emphasizes that the (logical) relations holding between statements are quite different from the relation between statements and sense experiences; positivism on the other hand always tries to abolish the distinction: either all science is made part of my knowing, ‘my’ sense experience (monism of sense data); or sense experiences are made part of the objective scientific network of arguments in the form of protocol statements (monism of statements).

to an agreement upon basic statements is, like other kinds of applications, to perform a purposeful action, guided by various theoretical considerations.

We are now, I think, in a position to solve such problems as, for instance, Whitehead's problem of how it is that the tactile breakfast should always be served along with the visual breakfast, and the tactile *Times* with the visible and the audibly rustling *Times*.^{*1} The inductive logician who believes that all science starts from stray elementary perceptions must be puzzled by such regular coincidences; they must seem to him entirely 'accidental'. He is prevented from explaining regularity by theories, because he is committed to the view that theories are nothing but statements of regular coincidences.

But according to the position reached here, the connections between our various experiences are explicable, and deducible, in terms of theories which we are engaged in testing. (Our theories do not lead us to expect that along with the visible moon we shall be served a tactile moon; nor do we expect to be bothered by an auditory nightmare.) One question, certainly, does remain—a question which obviously cannot be answered by any falsifiable theory and which is therefore 'metaphysical': how is it that we are so often lucky in the theories we construct—how is it that there are 'natural laws'?^{*2}

All these considerations are important for the epistemological theory of experiment. The theoretician puts certain definite questions to the experimenter, and the latter, by his experiments, tries to elicit a decisive answer to these questions, and to no others. All other questions he tries hard to exclude. (Here the relative independence of sub-systems of a theory may be important.) Thus he makes his test with respect to this one question '... as sensitive as possible, but as insensitive as possible with respect to all other associated questions.... Part of this work consists in screening off all possible sources of error.'¹ But it is a mistake to suppose that the experimenter proceeds in this way 'in

^{*1} A. N. Whitehead, *An Enquiry Concerning the Principles of Natural Knowledge* (1919), 1925, p. 194.

^{*2} This question will be discussed in section 79 and in appendix *x; see also my Postscript, especially sections *15 and *16.

¹ H. Weyl, *Philosophie der Mathematik und Naturwissenschaft*, 1927, p. 113; English Edition: *Philosophy of Mathematics and Natural Science*, Princeton, 1949, p. 116.

order to lighten the task of the theoretician',² or perhaps in order to furnish the theoretician with a basis for inductive generalizations. On the contrary, the theoretician must long before have done his work, or at least what is the most important part of his work: he must have formulated his question as sharply as possible. Thus it is he who shows the experimenter the way. But even the experimenter is not in the main engaged in making exact observations; his work, too, is largely of a theoretical kind. Theory dominates the experimental work from its initial planning up to the finishing touches in the laboratory.*³

This is well illustrated by cases in which the theoretician succeeded in predicting an observable effect which was later experimentally produced; perhaps the most beautiful instance is de Broglie's prediction of the wave-character of matter, first confirmed experimentally by Davisson and Germer.*⁴ It is illustrated perhaps even better by cases in which experiments had a conspicuous influence upon the progress of theory. What compels the theorist to search for a better theory, in these cases, is almost always the experimental falsification of a theory, so far accepted and corroborated: it is, again, the outcome of tests guided by theory. Famous examples are the Michelson-Morley experiment which led to the theory of relativity, and the falsification, by Lummer and Pringsheim, of the radiation formula of Rayleigh and Jeans, and of that of Wien, which led to the quantum theory. Accidental discoveries occur too, of course, but they are comparatively rare. Mach³ rightly speaks in such cases of a 'correction of scientific opinions by accidental

² Weyl, *ibid.*

*³ I now feel that I should have emphasized in this place a view which can be found elsewhere in the book (for example in the fourth and the last paragraphs of section 19). I mean the view that observations, and even more so observation statements and statements of experimental results, are always interpretations of the facts observed; that they are interpretations in the light of theories. This is one of the main reasons why it is always deceptively easy to find verifications of a theory, and why we have to adopt a highly critical attitude towards our theories if we do not wish to argue in circles: the attitude of trying to refute them.

*⁴ The story is briefly and excellently told by Max Born in *Albert Einstein, Philosopher-Scientist*, edited by P. A. Schilpp, 1949, p. 174. There are better illustrations, such as Adams's and Leverrier's discovery of Neptune, or that of Hertzian waves.

³ Mach, *Die Prinzipien der Wärmelehre* 1896, p. 438.

circumstances' (thus acknowledging the significance of theories in spite of himself).

It may now be possible for us to answer the question: How and why do we accept one theory in preference to others?

The preference is certainly not due to anything like a experiential justification of the statements composing the theory; it is not due to a logical reduction of the theory to experience. We choose the theory which best holds its own in competition with other theories; the one which, by natural selection, proves itself the fittest to survive. This will be the one which not only has hitherto stood up to the severest tests, but the one which is also testable in the most rigorous way. A theory is a tool which we test by applying it, and which we judge as to its fitness by the results of its applications.*⁵

From a logical point of view, the testing of a theory depends upon basic statements whose acceptance or rejection, in its turn, depends upon our decisions. Thus it is decisions which settle the fate of theories. To this extent my answer to the question, 'how do we select a theory?' resembles that given by the conventionalist; and like him I say that this choice is in part determined by considerations of utility. But in spite of this, there is a vast difference between my views and his. For I hold that what characterizes the empirical method is just this: that the convention or decision does not immediately determine our acceptance of universal statements but that, on the contrary, it enters into our acceptance of the singular statements—that is, the basic statements.

For the conventionalist, the acceptance of universal statements is governed by his principle of simplicity: he selects that system which is the simplest. I, by contrast, propose that the first thing to be taken into account should be the severity of tests. (There is a close connection between what I call 'simplicity' and the severity of tests; yet my idea of simplicity differs widely from that of the conventionalist; see section 46.) And I hold that what ultimately decides the fate of a theory is the result of a test, i.e. an agreement about basic statements. With the

*⁵ For a criticism of the 'instrumentalist' view see however the references in note *1 before section 12 (p. 37), and in the starred addition to note 1, section 12.

conventionalist I hold that the choice of any particular theory is an act, a practical matter. But for me the choice is decisively influenced by the application of the theory and the acceptance of the basic statements in connection with this application; whereas for the conventionalist, aesthetic motives are decisive.

Thus I differ from the conventionalist in holding that the statements decided by agreement are not universal but singular. And I differ from the positivist in holding that basic statements are not justifiable by our immediate experiences, but are, from the logical point of view, accepted by an act, by a free decision. (From the psychological point of view this may perhaps be a purposeful and well-adapted reaction.)

This important distinction, between a justification and a decision—a decision reached in accordance with a procedure governed by rules—might be clarified, perhaps, with the help of an analogy: the old procedure of trial by jury.

The verdict of the jury (*vere dictum* = spoken truly), like that of the experimenter, is an answer to a question of fact (*quid facti?*) which must be put to the jury in the sharpest, the most definite form. But what question is asked, and how it is put, will depend very largely on the legal situation, i.e. on the prevailing system of criminal law (corresponding to a system of theories). By its decision, the jury accepts, by agreement, a statement about a factual occurrence—a basic statement, as it were. The significance of this decision lies in the fact that from it, together with the universal statements of the system (of criminal law) certain consequences can be deduced. In other words, the decision forms the basis for the application of the system; the verdict plays the part of a ‘true statement of fact’. But it is clear that the statement need not be true merely because the jury has accepted it. This fact is acknowledged in the rule allowing a verdict to be quashed or revised.

The verdict is reached in accordance with a procedure which is governed by rules. These rules are based on certain fundamental principles which are chiefly, if not solely, designed to result in the discovery of objective truth. They sometimes leave room not only for subjective convictions but even for subjective bias. Yet even if we

disregard these special aspects of the older procedure and imagine a procedure based solely on the aim of promoting the discovery of objective truth, it would still be the case that the verdict of the jury never justifies, or gives grounds for, the truth of what it asserts.

Neither can the subjective convictions of the jurors be held to justify the decision reached; although there is, of course, a close causal connection between them and the decision reached—a connection which might be stated by psychological laws; thus these convictions may be called the ‘motives’ of the decision. The fact that the convictions are not justifications is connected with the fact that different rules may regulate the jury’s procedure (for example, simple or qualified majority). This shows that the relationship between the convictions of the jurors and their verdict may vary greatly.

In contrast to the verdict of the jury, the judgment of the judge is ‘reasoned’; it needs, and contains, a justification. The judge tries to justify it by, or deduce it logically from, other statements: the statements of the legal system, combined with the verdict that plays the rôle of initial conditions. This is why the judgment may be challenged on logical grounds. The jury’s decision, on the other hand, can only be challenged by questioning whether it has been reached in accordance with the accepted rules of procedure; i.e. formally, but not as to its content. (A justification of the content of a decision is significantly called a ‘motivated report’, rather than a ‘logically justified report’.)

The analogy between this procedure and that by which we decide basic statements is clear. It throws light, for example, upon their relativity, and the way in which they depend upon questions raised by the theory. In the case of the trial by jury, it would be clearly impossible to apply the ‘theory’ unless there is first a verdict arrived at by decision; yet the verdict has to be found in a procedure that conforms to, and thus applies, part of the general legal code. The case is analogous to that of basic statements. Their acceptance is part of the application of a theoretical system; and it is only this application which makes any further applications of the theoretical system possible.

The empirical basis of objective science has thus nothing ‘absolute’

about it.⁴ Science does not rest upon solid bedrock. The bold structure of its theories rises, as it were, above a swamp. It is like a building erected on piles. The piles are driven down from above into the swamp, but not down to any natural or ‘given’ base; and if we stop driving the piles deeper, it is not because we have reached firm ground. We simply stop when we are satisfied that the piles are firm enough to carry the structure, at least for the time being.

Addendum, 1972

- (1) My term ‘basis’ has ironical overtones: it is a basis that is not firm.
- (2) I assume a realist and objectivist point of view: I try to replace perception as ‘basis’ by critical testing.
- (3) Our observational experiences are never beyond being tested; and they are impregnated with theories.
- (4) ‘Basic statements’ are ‘test statements’: they are, like all language, impregnated with theories. (Even a ‘phenomenal’ language permitting statements like ‘now here red’ would be impregnated with theories about time, space, and colour.)

⁴ Weyl (op. cit., p. 83, English edition p. 116) writes: ‘. . . this pair of opposites, subjective-absolute and objective-relative seems to me to contain one of the most profound epistemological truths which can be gathered from the study of nature. Whoever wants the absolute must get subjectivity—ego-centrism—into the bargain, and whoever longs for objectivity cannot avoid the problem of relativism.’ And before this we find, ‘What is immediately experienced is subjective and absolute . . . ; the objective world, on the other hand, which natural science seeks to precipitate in pure crystalline form . . . is relative’. Born expresses himself in similar terms (*Die Relativitätstheorie Einsteins und ihre physikalischen Grundlagen*, 3rd edition, 1922, Introduction). Fundamentally, this view is Kant’s theory of objectivity consistently developed (cf. section 8 and note 5 to that section). Reininger also refers to this situation. He writes in *Das Psycho-Physische Problem*, 1916, p. 29, ‘Metaphysics as science is impossible . . . because although the absolute is indeed experienced, and for that reason can be intuitively felt, it yet refuses to be expressed in words. For “Spricht die Seele, so spricht, ach! schon die Seele nicht mehr”. (If the soul speaks then alas it is no longer the soul that speaks.)’