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The Logic of Scientific Discovery

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1

A SURVEY OF SOME FUNDAMENTAL PROBLEMS

A scientist, whether theorist or experimenter, puts forward statements, or systems of statements, and tests them step by step. In the field of the empirical sciences, more particularly, he constructs hypotheses, or systems of theories, and tests them against experience by observation and experiment.

I suggest that it is the task of the logic of scientific discovery, or the logic of knowledge, to give a logical analysis of this procedure; that is, to analyse the method of the empirical sciences.

But what are these 'methods of the empirical sciences'? And what do we call 'empirical science'?

1 THE PROBLEM OF INDUCTION

According to a widely accepted view—to be opposed in this book — the empirical sciences can be characterized by the fact that they use '*inductive methods*', as they are called. According to this view, the logic of scientific discovery would be identical with inductive logic, *i.e.* with the logical analysis of these inductive methods.

It is usual to call an inference 'inductive' if it passes from *singular*

statements (sometimes also called 'particular' statements), such as accounts of the results of observations or experiments, to *universal statements*, such as hypotheses or theories.

Now it is far from obvious, from a logical point of view, that we are justified in inferring universal statements from singular ones, no matter how numerous; for any conclusion drawn in this way may always turn out to be false: no matter how many instances of white swans we may have observed, this does not justify the conclusion that *all* swans are white.

The question whether inductive inferences are justified, or under what conditions, is known as *the problem of induction*.

The problem of induction may also be formulated as the question of the validity or the truth of universal statements which are based on experience, such as the hypotheses and theoretical systems of the empirical sciences. For many people believe that the truth of these universal statements is '*known by experience*'; yet it is clear that an account of an experience—of an observation or the result of an experiment—can in the first place be only a singular statement and not a universal one. Accordingly, people who say of a universal statement that we know its truth from experience usually mean that the truth of this universal statement can somehow be reduced to the truth of singular ones, and that these singular ones are known by experience to be true; which amounts to saying that the universal statement is based on inductive inference. Thus to ask whether there are natural laws known to be true appears to be only another way of asking whether inductive inferences are logically justified.

Yet if we want to find a way of justifying inductive inferences, we must first of all try to establish a *principle of induction*. A principle of induction would be a statement with the help of which we could put inductive inferences into a logically acceptable form. In the eyes of the upholders of inductive logic, a principle of induction is of supreme importance for scientific method: '. . . this principle', says Reichenbach, 'determines the truth of scientific theories. To eliminate it from science would mean nothing less than to deprive science of the power to decide the truth or falsity of its theories. Without it, clearly, science would no longer have the right to distinguish its

theories from the fanciful and arbitrary creations of the poet's mind.'[1]

Now this principle of induction cannot be a purely logical truth like a tautology or an analytic statement. Indeed, if there were such a thing as a purely logical principle of induction, there would be no problem of induction; for in this case, all inductive inferences would have to be regarded as purely logical or tautological transformations, just like inferences in deductive logic. Thus the principle of induction must be a synthetic statement; that is, a statement whose negation is not self-contradictory but logically possible. So the question arises why such a principle should be accepted at all, and how we can justify its acceptance on rational grounds.

Some who believe in inductive logic are anxious to point out, with Reichenbach, that 'the principle of induction is unreservedly accepted by the whole of science and that no man can seriously doubt this principle in everyday life either'.[2] Yet even supposing this were the case—for after all, 'the whole of science' might err—I should still contend that a principle of induction is superfluous, and that it must lead to logical inconsistencies.

That inconsistencies may easily arise in connection with the principle of induction should have been clear from the work of Hume;*[1] also, that they can be avoided, if at all, only with difficulty. For the principle of induction must be a universal statement in its turn. Thus if we try to regard its truth as known from experience, then the very same problems which occasioned its introduction will arise all over again. To justify it, we should have to employ inductive inferences; and to justify these we should have to assume an inductive principle of a higher order; and so on. Thus the attempt to base the principle of induction on experience breaks down, since it must lead to an infinite regress.

Kant tried to force his way out of this difficulty by taking the

[1] H. Reichenbach, *Erkenntnis* 1, 1930, p. 186 (cf. also pp. 64 f.). Cf. the penultimate paragraph of Russell's chapter xii, on Hume, in his *History of Western Philosophy*, 1946, p. 699.

[2] Reichenbach *ibid.*, p. 67.

*[1] The decisive passages from Hume are quoted in appendix *vii, text to footnotes 4, 5, and 6; see also note 2 to section 81, below.

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principle of induction (which he formulated as the 'principle of universal causation') to be '*a priori* valid'. But I do not think that his ingenious attempt to provide an *a priori* justification for synthetic statements was successful.

My own view is that the various difficulties of inductive logic here sketched are insurmountable. So also, I fear, are those inherent in the doctrine, so widely current today, that inductive inference, although not 'strictly valid', *can attain some degree of 'reliability' or of 'probability'*. According to this doctrine, inductive inferences are 'probable inferences'.[3] 'We have described', says Reichenbach, 'the principle of induction as the means whereby science decides upon truth. To be more exact, we should say that it serves to decide upon probability. For it is not given to science to reach either truth or falsity . . . but scientific statements can only attain continuous degrees of probability whose unattainable upper and lower limits are truth and falsity'.[4]

At this stage I can disregard the fact that the believers in inductive logic entertain an idea of probability that I shall later reject as highly unsuitable for their own purposes (see section 80, below). I can do so because the difficulties mentioned are not even touched by an appeal to probability. For if a certain degree of probability is to be assigned to statements based on inductive inference, then this will have to be justified by invoking a new principle of induction, appropriately modified. And this new principle in its turn will have to be justified, and so on. Nothing is gained, moreover, if the principle of induction, in its turn, is taken not as 'true' but only as 'probable'. In short, like every other form of inductive logic, the logic of probable inference, or 'probability logic', leads either to an infinite regress, or to the doctrine of apriorism.*[2]

The theory to be developed in the following pages stands directly opposed to all attempts to operate with the ideas of inductive logic. It

[3] Cf. J. M. Keynes, *A Treatise on Probability*, 1921; O. Külpe, *Vorlesungen über Logic* (ed. by Selz, 1923); Reichenbach (who uses the term 'probability implications'), *Axiomatik der Wahrscheinlichkeitsrechnung*, *Mathem. Zeitschr.* **34**, 1932; and elsewhere.

[4] Reichenbach, *Erkenntnis* **1**, 1930, p. 186.

*[2] See also chapter 10, below, especially note 2 to section 81, and chapter *ii of the Postscript for a fuller statement of this criticism.

might be described as the theory of *the deductive method of testing*, or as the view that a hypothesis can only be empirically *tested*—and only *after* it has been advanced.

Before I can elaborate this view (which might be called 'deductivism', in contrast to 'inductivism'[5]) I must first make clear the distinction between the *psychology of knowledge* which deals with empirical facts, and the *logic of knowledge* which is concerned only with logical relations. For the belief in inductive logic is largely due to a confusion of psychological problems with epistemological ones. It may be worth noticing, by the way, that this confusion spells trouble not only for the logic of knowledge but for its psychology as well.

2 ELIMINATION OF PSYCHOLOGISM

I said above that the work of the scientist consists in putting forward and testing theories.

The initial stage, the act of conceiving or inventing a theory, seems to me neither to call for logical analysis nor to be susceptible of it. The question how it happens that a new idea occurs to a man—whether it is a musical theme, a dramatic conflict, or a scientific theory—may be of great interest to empirical psychology; but it is irrelevant to the logical analysis of scientific knowledge. This latter is concerned not with *questions of fact* (Kant's *quid facti?*), but only with questions of *justification* or *validity* (Kant's *quid juris?*). Its questions are of the following kind. Can a statement be justified? And if so, how? Is it testable? Is it logically dependent on certain other statements? Or does it perhaps contradict them? In order that a statement may be logically examined in this way, it must already have been presented to

[5] Liebig (in *Induktion und Deduktion*, 1865) was probably the first to reject the inductive method from the standpoint of natural science; his attack is directed against Bacon. Duhem (in *La théorie physique, son objet et sa structure*, 1906; English translation by P. P. Wiener: *The Aim and Structure of Physical Theory*, Princeton, 1954) holds pronounced deductivist views. (*But there are also inductivist views to be found in Duhem's book, for example in the third chapter, Part One, where we are told that only experiment, induction, and generalization have produced Descartes's law of refraction; cf. the English translation, p. 34.) So does V. Kraft, *Die Grundformen der Wissenschaftlichen Methoden*, 1925; see also Carnap, *Erkenntnis* **2**, 1932, p. 440.

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us. Someone must have formulated it, and submitted it to logical examination.

Accordingly I shall distinguish sharply between the process of conceiving a new idea, and the methods and results of examining it logically. As to the task of the logic of knowledge—in contradistinction to the psychology of knowledge—I shall proceed on the assumption that it consists solely in investigating the methods employed in those systematic tests to which every new idea must be subjected if it is to be seriously entertained.

Some might object that it would be more to the purpose to regard it as the business of epistemology to produce what has been called a '*rational reconstruction*' of the steps that have led the scientist to a discovery—to the finding of some new truth. But the question is: what, precisely, do we want to reconstruct? If it is the processes involved in the stimulation and release of an inspiration which are to be reconstructed, then I should refuse to take it as the task of the logic of knowledge. Such processes are the concern of empirical psychology but hardly of logic. It is another matter if we want to reconstruct rationally the *subsequent tests* whereby the inspiration may be discovered to be a discovery, or become known to be knowledge. In so far as the scientist critically judges, alters, or rejects his own inspiration we may, if we like, regard the methodological analysis undertaken here as a kind of 'rational reconstruction' of the corresponding thought-processes. But this reconstruction would not describe these processes as they actually happen: it can give only a logical skeleton of the procedure of testing. Still, this is perhaps all that is meant by those who speak of a 'rational reconstruction' of the ways in which we gain knowledge.

It so happens that my arguments in this book are quite independent of this problem. However, my view of the matter, for what it is worth, is that there is no such thing as a logical method of having new ideas, or a logical reconstruction of this process. My view may be expressed by saying that every discovery contains 'an irrational element', or 'a creative intuition', in Bergson's sense. In a similar way Einstein speaks of the 'search for those highly universal laws . . . from which a picture of the world can be obtained by pure deduction. There is no logical path', he says, 'leading to these . . . laws. They can only be reached by

intuition, based upon something like an intellectual love ('*Einfühlung*') of the objects of experience.'[6]

3 DEDUCTIVE TESTING OF THEORIES

According to the view that will be put forward here, the method of critically testing theories, and selecting them according to the results of tests, always proceeds on the following lines. From a new idea, put up tentatively, and not yet justified in any way—an anticipation, a hypothesis, a theoretical system, or what you will—conclusions are drawn by means of logical deduction. These conclusions are then compared with one another and with other relevant statements, so as to find what logical relations (such as equivalence, derivability, compatiblity, or incompatibility) exist between them.

We may if we like distinguish four different lines along which the testing of a theory could be carried out. First there is the logical comparison of the conclusions among themselves, by which the internal consistency of the system is tested. Secondly, there is the investigation of the logical form of the theory, with the object of determining whether it has the character of an empirical or scientific theory, or whether it is, for example, tautological. Thirdly, there is the comparison with other theories, chiefly with the aim of determining whether the theory would constitute a scientific advance should it survive our various tests. And finally, there is the testing of the theory by way of empirical applications of the conclusions which can be derived from it.

The purpose of this last kind of test is to find out how far the new consequences of the theory—whatever may be new in what it asserts—stand up to the demands of practice, whether raised by purely scientific experiments, or by practical technological applications. Here too the procedure of testing turns out to be deductive. With the help of

[6] Address on Max Planck's 60th birthday (1918). The passage quoted begins with the words, 'The supreme task of the physicist is to search for those highly universal laws . . .,' etc. (quoted from A. Einstein, *Mein Weltbild*, 1934, p. 168; English translation by A. Harris: *The World as I see It*, 1935, p. 125). Similar ideas are found earlier in Liebig, *op. cit.*; *cf.* also Mach, *Principien der Wärmelehre*, 1896, pp. 443 ff. *The German word '*Einfühlung*' is difficult to translate. Harris translates: 'sympathetic understanding of experience'.

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other statements, previously accepted, certain singular statements—which we may call 'predictions'—are deduced from the theory; especially predictions that are easily testable or applicable. From among these statements, those are selected which are not derivable from the current theory, and more especially those which the current theory contradicts. Next we seek a decision as regards these (and other) derived statements by comparing them with the results of practical applications and experiments. If this decision is positive, that is, if the singular conclusions turn out to be acceptable, or *verified*, then the theory has, for the time being, passed its test: we have found no reason to discard it. But if the decision is negative, or in other words, if the conclusions have been *falsified*, then their falsification also falsifies the theory from which they were logically deduced.

It should be noticed that a positive decision can only temporarily support the theory, for subsequent negative decisions may always overthrow it. So long as theory withstands detailed and severe tests and is not superseded by another theory in the course of scientific progress, we may say that it has 'proved its mettle' or that it is '*corroborated*'*[1] by past experience.

Nothing resembling inductive logic appears in the procedure here outlined. I never assume that we can argue from the truth of singular statements to the truth of theories. I never assume that by force of 'verified' conclusions, theories can be established as 'true', or even as merely 'probable'.

In this book I intend to give a more detailed analysis of the methods of deductive testing. And I shall attempt to show that, within the framework of this analysis, all the problems can be dealt with that are usually called '*epistemological*'. Those problems, more especially, to which inductive logic gives rise, can be eliminated without creating new ones in their place.

4 THE PROBLEM OF DEMARCATION

Of the many objections which are likely to be raised against the view here advanced, the most serious is perhaps the following. In rejecting

*[1] For this term, see note *1 before section 79, and section *29 of my *Postscript*.

the method of induction, it may be said, I deprive empirical science of what appears to be its most important characteristic; and this means that I remove the barriers which separate science from metaphysical speculation. My reply to this objection is that my main reason for rejecting inductive logic is precisely that it *does not provide a suitable distinguishing mark* of the empirical, non-metaphysical, character of a theoretical system; or in other words, that it *does not provide a suitable 'criterion of demarcation'*.

The problem of finding a criterion which would enable us to distinguish between the empirical sciences on the one hand, and mathematics and logic as well as 'metaphysical' systems on the other, I call the *problem of demarcation*.[1]

This problem was known to Hume who attempted to solve it.[2] With Kant it became the central problem of the theory of knowledge. If, following Kant, we call the problem of induction 'Hume's problem', we might call the problem of demarcation 'Kant's problem'.

Of these two problems—the source of nearly all the other problems of the theory of knowledge—the problem of demarcation is, I think, the more fundamental. Indeed, the main reason why epistemologists with empiricist leanings tend to pin their faith to the 'method of induction' seems to be their belief that this method alone can provide a suitable criterion of demarcation. This applies especially to those empiricists who follow the flag of 'positivism'.

The older positivists wished to admit, as scientific or legitimate, only those *concepts* (or notions or ideas) which were, as they put it, 'derived from experience'; those concepts, that is, which they believed to be logically reducible to elements of sense-experience, such as sensations (or sense-data), impressions, perceptions, visual or auditory memories, and so forth. Modern positivists are apt to see more clearly that science is not a system of concepts but rather a

[1] With this (and also with sections 1 to 6 and 13 to 24) compare my note in *Erkenntnis* **3**, 1933, p. 426; *It is now here reprinted, in translation, in appendix *i.

[2] Cf. the last sentence of his *Enquiry Concerning Human Understanding*. *With the next paragraph (and my allusion to epistemologists) compare for example the quotation from Reichenbach in the text to note 1, section 1.

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system of *statements*.*[1] Accordingly, they wish to admit, as scientific or legitimate, only those statements which are reducible to elementary (or 'atomic') statements of experience—to 'judgments of perception' or 'atomic propositions' or 'protocol-sentences' or what not.*[2] It is clear that the implied criterion of demarcation is identical with the demand for an inductive logic.

Since I reject inductive logic I must also reject all these attempts to solve the problem of demarcation. With this rejection, the problem of demarcation gains in importance for the present inquiry. Finding an acceptable criterion of demarcation must be a crucial task for any epistemology which does not accept inductive logic.

Positivists usually interpret the problem of demarcation in a *naturalistic* way; they interpret it as if it were a problem of natural science. Instead of taking it as their task to propose a suitable convention, they believe they have to discover a difference, existing in the nature of things, as it were, between empirical science on the one hand and metaphysics on the other. They are constantly trying to prove that metaphysics by its very nature is nothing but nonsensical twaddle—'sophistry and illusion', as Hume says, which we should 'commit to the flames'.*[3]

If by the words 'nonsensical' or 'meaningless' we wish to express no more, by definition, than 'not belonging to empirical science', then the characterization of metaphysics as meaningless nonsense would be

*[1] When I wrote this paragraph I overrated the 'modern positivists', as I now see. I should have remembered that in this respect the promising beginning of Wittgenstein's *Tractatus*—'The world is the totality of facts, not of things'—was cancelled by its end which denounced the man who 'had given no meaning to certain signs in his propositions'. See also my *Open Society and its Enemies*, chapter 11, section ii, and chapter *i of my *Postscript*, especially sections *ii (note 5), *24 (the last five paragraphs), and *25.

*[2] Nothing depends on names, of course. When I invented the new name 'basic statement' (or 'basic proposition'; see below, sections 7 and 28) I did so only because I needed a term not burdened with the connotation of a perception statement. But unfortunately it was soon adopted by others, and used to convey precisely the kind of meaning which I wished to avoid. Cf. also my *Postscript*, *29.

*[3] Thus Hume, like Sextus, condemned his own *Enquiry* on its last page; just as later Wittgenstein condemned his own *Tractatus* on its last page. (See note 2 to section 10.)

trivial; for metaphysics has usually been defined as non-empirical. But of course, the positivists believe they can say much more about metaphysics than that some of its statements are non-empirical. The words 'meaningless' or 'nonsensical' convey, and are meant to convey, a derogatory evaluation; and there is no doubt that what the positivists really want to achieve is not so much a successful demarcation as the final overthrow[3] and the annihilation of metaphysics. However this may be, we find that each time the positivists tried to say more clearly what 'meaningful' meant, the attempt led to the same result—to a definition of 'meaningful sentence' (in contradistinction to 'meaningless pseudo-sentence') which simply reiterated the criterion of demarcation of their *inductive logic*.

This 'shows itself' very clearly in the case of Wittgenstein, according to whom every meaningful proposition must be *logically reducible*[4] to elementary (or atomic) propositions, which he characterizes as descriptions or 'pictures of reality'[5] (a characterization, by the way, which is to cover all meaningful propositions). We may see from this that Wittgenstein's criterion of meaningfulness coincides with the inductivists' criterion of demarcation, provided we replace their words 'scientific' or 'legitimate' by 'meaningful'. And it is precisely over the problem of induction that this attempt to solve the problem of demarcation comes to grief: positivists, in their anxiety to annihilate metaphysics, annihilate natural science along with it. For scientific laws, too, cannot be logically reduced to elementary statements of experience. If consistently applied, Wittgenstein's criterion of meaningfulness rejects as meaningless those natural laws the search for which, as Einstein says,[6] is 'the supreme task of the physicist': they can never be accepted as genuine or legitimate statements. Wittgenstein's attempt to unmask the problem of induction as an empty pseudo-problem was formulated

[3] Carnap, *Erkenntnis* 2, 1932, pp. 219 ff. Earlier Mill had used the word 'meaningless' in a similar way, *no doubt under the influence of Comte; cf. Comte's *Early Essays on Social Philosophy*, ed. by H. D. Hutton, 1911, p. 223. See also my *Open Society*, note 51 to chapter 11.

[4] Wittgenstein, *Tractatus Logico-Philosophicus* (1918 and 1922), Proposition 5. *As this was written in 1934, I am dealing here of course *only* with the *Tractatus*.

[5] Wittgenstein, *op. cit.*, Propositions 4.01; 4.03; 2.221.

[6] Cf. note 1 to section 2.

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by Schlick*[4] in the following words: 'The problem of induction consists in asking for a logical justification of *universal statements* about reality . . . We recognize, with Hume, that there is no such logical justification: there can be none, simply because *they are not genuine* statements.'[7]

This shows how the inductivist criterion of demarcation fails to draw a dividing line between scientific and metaphysical systems, and why it must accord them equal status; for the verdict of the positivist dogma of meaning is that both are systems of meaningless pseudo-statements. Thus instead of eradicating metaphysics from the empirical sciences, positivism leads to the invasion of metaphysics into the scientific realm.[8]

In contrast to these anti-metaphysical stratagems—anti-metaphysical in intention, that is—my business, as I see it, is not to bring about the overthrow of metaphysics. It is, rather, to formulate a suitable characterization of empirical science, or to define the concepts 'empirical science' and 'metaphysics' in such a way that we shall be able to say of a

*[4] The idea of treating scientific laws as pseudo-propositions—thus solving the problem of induction—was attributed by Schlick to Wittgenstein. (Cf. my *Open Society*, notes 46 and 51 f. to chapter 11.) But it is really much older. It is part of the instrumentalist tradition which can be traced back to Berkeley, and further. (See for example my paper 'Three Views Concerning Human Knowledge', in *Contemporary British Philosophy*, 1956; and 'A Note on Berkeley as a Precursor of Mach', in *The British Journal for the Philosophy of Science* **4**, 1953, pp. 26 ff., now in my *Conjectures and Refutations*, 1959. Further references in note *1 before section 12 (p. 37). The problem is also treated in my *Postscript*, sections *11 to *14, and *19 to *26.)

[7] Schlick, *Naturwissenschaften* **19**, 1931, p. 156. (The italics are mine). Regarding natural laws Schlick writes (p. 151), 'It has often been remarked that, strictly, we can never speak of an absolute verification of a law, since we always, so to speak, tacitly make the reservation that it may be modified in the light of further experience. If I may add, by way of parenthesis', Schlick continues, 'a few words on the logical situation, the above-mentioned fact means that a natural law, in principle, does not have the logical character of a statement, but is, rather, a prescription for the formation of statements.' *('Formation' no doubt was meant to include transformation or derivation.) Schlick attributed this theory to a personal communication of Wittgenstein's. See also section *12 of my *Postscript*.

[8] Cf. Section 78 (for example note 1). *See also my *Open Society*, notes 46, 51, and 52 to chapter 11, and my paper. 'The Demarcation between Science and Metaphysics', contributed in January 1955 to the Carnap volume of the *Library of Living Philosophers*, edited by P. A. Schilpp and now in my *Conjectures and Refutations*, 1963 and 1965.

given system of statements whether or not its closer study is the concern of empirical science.

My criterion of demarcation will accordingly have to be regarded as a *proposal for an agreement or convention*. As to the suitability of any such convention opinions may differ; and a reasonable discussion of these questions is only possible between parties having some purpose in common. The choice of that purpose must, of course, be ultimately a matter of decision, going beyond rational argument.*[5]

Thus anyone who envisages a system of absolutely certain, irrevocably true statements[9] as the end and purpose of science will certainly reject the proposals I shall make here. And so will those who see 'the essence of science . . . in its dignity', which they think resides in its 'wholeness' and its 'real truth and essentiality'.[10] They will hardly be ready to grant this dignity to modern theoretical physics in which I and others see the most complete realization to date of what I call 'empirical science'.

The aims of science which I have in mind are different. I do not try to justify them, however, by representing them as the true or the essential aims of science. This would only distort the issue, and it would mean a relapse into positivist dogmatism. There is only *one* way, as far as I can see, of arguing rationally in support of my proposals. This is to analyse their logical consequences: to point out their fertility—their power to elucidate the problems of the theory of knowledge.

Thus I freely admit that in arriving at my proposals I have been guided, in the last analysis, by value judgments and predilections. But I hope that my proposals may be acceptable to those who value not only logical rigour but also freedom from dogmatism; who seek practical applicability, but are even more attracted by the adventure of science, and by discoveries which again and again confront us with new and unexpected questions, challenging us to try out new and hitherto undreamed-of answers.

The fact that value judgments influence my proposals does not mean

*[5] I believe that a reasonable discussion is always possible between parties interested in truth, and ready to pay attention to each other. (Cf. my *Open Society*, chapter 24.)

[9] This is Dingler's view; cf. note 1 to section 19.

[10] This is the view of O. Spann (*Kategorienlehre*, 1924).

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that I am making the mistake of which I have accused the positivists—that of trying to kill metaphysics by calling it names. I do not even go so far as to assert that metaphysics has no value for empirical science. For it cannot be denied that along with metaphysical ideas which have obstructed the advance of science there have been others—such as speculative atomism—which have aided it. And looking at the matter from the psychological angle, I am inclined to think that scientific discovery is impossible without faith in ideas which are of a purely speculative kind, and sometimes even quite hazy; a faith which is completely unwarranted from the point of view of science, and which, to that extent, is 'metaphysical'.[11]

Yet having issued all these warnings, I still take it to be the first task of the logic of knowledge to put forward a *concept of empirical science*, in order to make linguistic usage, now somewhat uncertain, as definite as possible, and in order to draw a clear line of demarcation between science and metaphysical ideas—even though these ideas may have furthered the advance of science throughout its history.

5 EXPERIENCE AS A METHOD

The task of formulating an acceptable definition of the idea of an 'empirical science' is not without its difficulties. Some of these arise from *the fact that there must be many theoretical systems* with a logical structure very similar to the one which at any particular time is the accepted system of empirical science. This situation is sometimes described by saying that there is a great number—presumably an infinite number—of 'logically possible worlds'. Yet the system called 'empirical science' is intended to represent only *one* world: the 'real world' or the 'world of our experience'.*[1]

In order to make this idea a little more precise, we may distinguish three requirements which our empirical theoretical system will have to satisfy. First, it must be *synthetic*, so that it may represent a

[11] Cf. also: Planck. *Positivismus und reule Aussenwelt* (1931) and Einstein, *Die Religiosität der Forschung*, in *Mein Weltbild*, 1934, p. 43; English translation by A. Harris: *The World as I See It*, 1935, pp. 23 ff. *See also section 85, and my *Postscript*.

*[1] Cf. appendix *x.

non-contradictory, a *possible* world. Secondly, it must satisfy the criterion of demarcation (cf. sections 6 and 21), *i.e.* it must not be metaphysical, but must represent a world of possible *experience*. Thirdly, it must be a system distinguished in some way from other such systems as the one which represents *our* world of experience.

But how is the system that represents our world of experience to be distinguished? The answer is: by the fact that it has been submitted to tests, and has stood up to tests. This means that it is to be distinguished by applying to it that deductive method which it is my aim to analyse, and to describe.

'Experience', on this view, appears as a distinctive *method* whereby one theoretical system may be distinguished from others; so that empirical science seems to be characterized not only by its logical form but, in addition, by its distinctive *method*. (This, of course, is also the view of the inductivists, who try to characterize empirical science by its use of the inductive method.)

The theory of knowledge, whose task is the analysis of the method or procedure peculiar to empirical science, may accordingly be described as a theory of the empirical method—*a theory of what is usually called 'experience'*.

6 FALSIFIABILITY AS A CRITERION OF DEMARCATION

The criterion of demarcation inherent in inductive logic—that is, the positivistic dogma of meaning—is equivalent to the requirement that all the statements of empirical science (or all 'meaningful' statements) must be capable of being finally decided, with respect to their truth *and* falsity; we shall say that they must be *'conclusively decidable'*. This means that their form must be such that *to verify them and to falsify them* must both be logically possible. Thus Schlick says: '. . . a genuine statement must be capable of *conclusive verification*';[1] and Waismann says still more clearly: 'If there is no possible way to *determine whether a statement is true* then that statement has no meaning whatsoever. For the meaning of a statement is the method of its verification.'[2]

[1] Schlick, *Naturwissenschaften* **19**, 1931, p. 150.

[2] Waismann, *Erkenntnis* **1**, 1903, p. 229.

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Now in my view there is no such thing as induction.*[1] Thus inference to theories, from singular statements which are 'verified by experience' (whatever that may mean), is logically inadmissible. Theories are, therefore, *never* empirically verifiable. If we wish to avoid the positivist's mistake of eliminating, by our criterion of demarcation, the theoretical systems of natural science,*[2] then we must choose a criterion which allows us to admit to the domain of empirical science even statements which cannot be verified.

But I shall certainly admit a system as empirical or scientific only if it is capable of being *tested* by experience. These considerations suggest that not the *verifiability* but the *falsifiability* of a system is to be taken as a criterion of demarcation.*[3] In other words: I shall not require of a scientific system that it shall be capable of being singled out, once and for all, in a positive sense; but I shall require that its logical form shall be such that it can be singled out, by means of empirical tests, in a negative sense: *it must be possible for an empirical scientific system to be refuted by experience.*[3]

*[1] I am not, of course, here considering so-called 'mathematical induction'. What I am denying is that there is such a thing as induction in the so-called 'inductive sciences': that there are either 'inductive procedures' or 'inductive inferences'.

*[2] In his *Logical Syntax* (1937, pp. 321 f.) Carnap admitted that this was a mistake (with a reference to my criticism); and he did so even more fully in 'Testability and Meaning', recognizing the fact that universal laws are not only 'convenient' for science but even 'essential' (*Philosophy of Science* **4**, 1937, p. 27). But in his inductivist *Logical Foundations of Probability* (1950), he returns to a position very like the one here criticized: finding that universal laws have zero probability (p. 511), he is compelled to say (p. 575) that though they need not be expelled from science, science can very well do without them.

*[3] Note that I suggest falsifiability as a criterion of demarcation, but *not of meaning*. Note, moreover, that I have already (section 4) sharply criticized the use of the idea of meaning as a criterion of demarcation, and that I attack the dogma of meaning again, even more sharply, in section 9. It is therefore a sheer myth (though any number of refutations of my theory have been based upon this myth) that I ever proposed falsifiability as a criterion of meaning. Falsifiability separates two kinds of perfectly meaningful statements: the falsifiable and the non-falsifiable. It draws a line inside meaningful language, not around it. See also appendix *i, and chapter *i of my *Postscript*, especially sections *17 and *19, and my *Conjectures and Refutations*, chs. 1 and 11.

[3] Related ideas are to be found, for example, in Frank, *Die Kausalität und ihre Grenzen*, 1931, ch. I, §10 (pp. 15f.); Dubislav, *Die Definition* (3rd edition 1931), pp. 100 f. (Cf. also note 1 to section 4, above.)

(Thus the statement, 'It will rain or not rain here tomorrow' will not be regarded as empirical, simply because it cannot be refuted; whereas the statement, 'It will rain here tomorrow' will be regarded as empirical.)

Various objections might be raised against the criterion of demarcation here proposed. In the first place, it may well seem somewhat wrong-headed to suggest that science, which is supposed to give us positive information, should be characterized as satisfying a negative requirement such as refutability. However, I shall show, in sections 31 to 46, that this objection has little weight, since the amount of positive information about the world which is conveyed by a scientific statement is the greater the more likely it is to clash, because of its logical character, with possible singular statements. (Not for nothing do we call the laws of nature 'laws': the more they prohibit the more they say.)

Again, the attempt might be made to turn against me my own criticism of the inductivist criterion of demarcation; for it might seem that objections can be raised against falsifiability as a criterion of demarcation similar to those which I myself raised against verifiability.

This attack would not disturb me. My proposal is based upon an *asymmetry* between verifiability and falsifiability; an asymmetry which results from the logical form of universal statements.*[4] For these are never derivable from singular statements, but can be contradicted by singular statements. Consequently it is possible by means of purely deductive inferences (with the help of the *modus tollens* of classical logic) to argue from the truth of singular statements to the falsity of universal statements. Such an argument to the falsity of universal statements is the only strictly deductive kind of inference that proceeds, as it were, in the 'inductive direction'; that is, from singular to universal statements.

A third objection may seem more serious. It might be said that even if the asymmetry is admitted, it is still impossible, for various reasons, that any theoretical system should ever be conclusively falsified. For it is always possible to find some way of evading falsification, for example

*[4] This asymmetry is now more fully discussed in section *22 of my *Postscript*.

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by introducing *ad hoc* an auxiliary hypothesis, or by changing *ad hoc* a definition. It is even possible without logical inconsistency to adopt the position of simply refusing to acknowledge any falsifying experience whatsoever. Admittedly, scientists do not usually proceed in this way, but logically such procedure is possible; and this fact, it might be claimed, makes the logical value of my proposed criterion of demarcation dubious, to say the least.

I must admit the justice of this criticism; but I need not therefore withdraw my proposal to adopt falsifiability as a criterion of demarcation. For I am going to propose (in sections 20 f.) that the *empirical method* shall be characterized as a method that excludes precisely those ways of evading falsification which, as my imaginary critic rightly insists, are logically possible. According to my proposal, what characterizes the empirical method is its manner of exposing to falsification, in every conceivable way, the system to be tested. Its aim is not to save the lives of untenable systems but, on the contrary, to select the one which is by comparison the fittest, by exposing them all to the fiercest struggle for survival.

The proposed criterion of demarcation also leads us to a solution of Hume's problem of induction—of the problem of the validity of natural laws. The root of this problem is the apparent contradiction between what may be called 'the fundamental thesis of empiricism'—the thesis that experience alone can decide upon the truth or falsity of scientific statements—and Hume's realization of the inadmissibility of inductive arguments. This contradiction arises only if it is assumed that all empirical scientific statements must be 'conclusively decidable', i.e. that their verification and their falsification must both in principle be possible. If we renounce this requirement and admit as empirical also statements which are decidable in one sense only—unilaterally decidable and, more especially, falsifiable—and which may be tested by systematic attempts to falsify them, the contradiction disappears: the method of falsification presupposes no inductive inference, but only the tautological transformations of deductive logic whose validity is not in dispute.[4]

[4] For this see also my paper mentioned in note 1 to section 4, *now here reprinted in appendix *i; and my *Postscript*, esp. section *2.

7 THE PROBLEM OF THE 'EMPIRICAL BASIS'

If falsifiability is to be at all applicable as a criterion of demarcation, then singular statements must be available which can serve as premisses in falsifying inferences. Our criterion therefore appears only to shift the problem—to lead us back from the question of the empirical character of theories to the question of the empirical character of singular statements.

Yet even so, something has been gained. For in the practice of scientific research, demarcation is sometimes of immediate urgency in connection with theoretical systems, whereas in connection with singular statements, doubt as to their empirical character rarely arises. It is true that errors of observation occur and that they give rise to false singular statements, but the scientist scarcely ever has occasion to describe a singular statement as non-empirical or metaphysical.

Problems of the empirical basis—that is, problems concerning the empirical character of singular statements, and how they are tested—thus play a part within the logic of science that differs somewhat from that played by most of the other problems which will concern us. For most of these stand in close relation to the *practice* of research, whilst the problem of the empirical basis belongs almost exclusively to the *theory* of knowledge. I shall have to deal with them, however, since they have given rise to many obscurities. This is especially true of the relation between *perceptual experiences* and *basic statements*. (What I call a 'basic statement' or a 'basic proposition' is a statement which can serve as a premise in an empirical falsification; in brief, a statement of a singular fact.)

Perceptual experiences have often been regarded as providing a kind of justification for basic statements. It was held that these statements are 'based upon' these experiences; that their truth becomes 'manifest by inspection' through these experiences; or that it is made 'evident' by these experiences, etc. All these expressions exhibit the perfectly sound tendency to emphasize the close connection between basic statements and our perceptual experiences. Yet it was also rightly felt that *statements can be logically justified only by statements*. Thus the connection between the perceptions and the statements remained obscure, and was described by correspondingly obscure expressions which elucidated nothing, but slurred over the difficulties or, at best, adumbrated them through metaphors.

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Here too a solution can be found, I believe, if we clearly separate the psychological from the logical and methodological aspects of the problem. We must distinguish between, on the one hand, *our subjective experiences or our feelings of conviction*, which can never justify any statement (though they can be made the subject of psychological investigation) and, on the other hand, the *objective logical relations* subsisting among the various systems of scientific statements, and within each of them.

The problems of the empirical basis will be discussed in some detail in sections 25 to 30. For the present I had better turn to the problem of scientific objectivity, since the terms 'objective' and 'subjective' which I have just used are in need of elucidation.

8 SCIENTIFIC OBJECTIVITY AND SUBJECTIVE CONVICTION

The words 'objective' and 'subjective' are philosophical terms heavily burdened with a heritage of contradictory usages and of inconclusive and interminable discussions.

My use of the terms 'objective' and 'subjective' is not unlike Kant's. He uses the word 'objective' to indicate that scientific knowledge should be *justifiable*, independently of anybody's whim: a justification is 'objective' if in principle it can be tested and understood by anybody. 'If something is valid', he writes, 'for anybody in possession of his reason, then its grounds are objective and sufficient.'[1]

Now I hold that scientific theories are never fully justifiable or verifiable, but that they are nevertheless testable. I shall therefore say that the objectivity of scientific statements lies in the fact that they can be *inter-subjectively tested*.*[1]

[1] *Kritik der reinen Vernunft*, Methodenlehre, 2. Haupstück, 3. Abschnitt (2nd edition, p. 848; English translation by N. Kemp Smith, 1933: *Critique of Pure Reason*, The Transcendental Doctrine of Method, chapter ii, section 3, p. 645).

*[1] I have since generalized this formulation; for inter-subjective *testing* is merely a very important aspect of the more general idea of inter-subjective *criticism*, or in other words, of the idea of mutual rational control by critical discussion. This more general idea, discussed at some length in my *Open Society and Its Enemies*, chapters 23 and 24, and in my *Poverty of Historicism*, section 32, is also discussed in my *Postscript*, especially in chapters *i, *ii, and *vi.

The word 'subjective' is applied by Kant to our feelings of conviction (of varying degrees).[2] To examine how these come about is the business of psychology. They may arise, for example, 'in accordance with the laws of association'.[3] Objective reasons too may serve as 'subjective *causes* of judging',[4] in so far as we may reflect upon these reasons, and become convinced of their cogency.

Kant was perhaps the first to realize that the objectivity of scientific statements is closely connected with the construction of theories—with the use of hypotheses and universal statements. Only when certain events recur in accordance with rules or regularities, as is the case with repeatable experiments, can our observations be tested—in principle—by anyone. We do not take even our own observations quite seriously, or accept them as scientific observations, until we have repeated and tested them. Only by such repetitions can we convince ourselves that we are not dealing with a mere isolated 'coincidence', but with events which, on account of their regularity and reproducibility, are in principle inter-subjectively testable.[5]

Every experimental physicist knows those surprising and inexplicable apparent 'effects' which in his laboratory can perhaps even be reproduced for some time, but which finally disappear without trace. Of course, no physicist would say in such a case that he had made a scientific discovery (though he might try to rearrange his experiments so as to make the effect reproducible). Indeed the scientifically significant *physical effect* may be defined as that which can be regularly

[2] *Ibid.*

[3] Cf. *Kritik der reinen Vernunft*, Transcendentale Elementarlehre §19 (2nd edition, p. 142; English translation by N. Kemp Smith, 1933: *Critique of Pure Reason*, Transcendental Doctrine of Elements, §19, p. 159).

[4] Cf. *Kritik der reinen Vernuft*, Methodenlehre, 2. Haupstück, 3. Abschnitt (2nd edition, p. 849; English translation, chapter ii, section 3, p. 646).

[5] Kant realized that from the required objectivity of scientific statements it follows that they must be at any time inter-subjectively testable, and that they must therefore have the form of universal laws or theories. He formulated this discovery somewhat obscurely by his 'principle of temporal succession according to the law of causality' (which principle he believed that he could prove *a priori* by employing the reasoning here indicated). I do not postulate any such principle (cf. section 12); but I agree that scientific statements, since they must be inter-subjectively testable, must always have the character of universal hypotheses. *See also note *1 to section 22.

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reproduced by anyone who carries out the appropriate experiment in the way prescribed. No serious physicist would offer for publication, as a scientific discovery, any such 'occult effect', as I propose to call it—one for whose reproduction he could give no instructions. The 'discovery' would be only too soon rejected as chimerical, simply because attempts to test it would lead to negative results.[6] (It follows that any controversy over the question whether events which are in principle unrepeatable and unique ever do occur cannot be decided by science: it would be a metaphysical controversy.)

We may now return to a point made in the previous section: to my thesis that a subjective experience, or a feeling of conviction, can never justify a scientific statement, and that within science it can play no part except that of an object of an empirical (a psychological) inquiry. No matter how intense a feeling of conviction it may be, it can never justify a statement. Thus I may be utterly convinced of the truth of a statement; certain of the evidence of my perceptions; overwhelmed by the intensity of my experience: every doubt may seem to me absurd. But does this afford the slightest reason for science to accept my statement? Can any statement be justified by the fact that K. R. P. is utterly convinced of its truth? The answer is, 'No'; and any other answer would be incompatible with the idea of scientific objectivity. Even the fact, for me to so firmly established, that I am experiencing this feeling of conviction, cannot appear within the field of objective science except in the form of a *psychological hypothesis* which, of course, calls for inter-subjective testing: from the conjecture that I have this feeling of conviction the psychologist may deduce, with the help of psychological and other theories, certain predictions about my behaviour; and these may be confirmed or refuted in the course of experimental tests. But from the epistemological point of view, it is quite irrelevant whether my

[6] In the literature of physics there are to be found some instances of reports, by serious investigators, of the occurrence of effects which could not be reproduced, since further tests led to negative results. A well-known example from recent times is the unexplained positive result of Michelson's experiment observed by Miller (1921–1926) at Mount Wilson, after he himself (as well as Morley) had previously reproduced Michelson's negative result. But since later tests again gave negative results it is now customary to regard these latter as decisive, and to explain Miller's divergent result as 'due to unknown sources of error'. *See also section 22, especially footnote *1.

feeling of conviction was strong or weak; whether it came from a strong or even irresistible impression of indubitable certainty (or 'self-evidence'), or merely from a doubtful surmise. None of this has any bearing on the question of how scientific statements can be justified.

Considerations like these do not of course provide an answer to the problem of the empirical basis. But at least they help us to see its main difficulty. In demanding objectivity for basic statements as well as for other scientific statements, we deprive ourselves of any logical means by which we might have hoped to reduce the truth of scientific statements to our experiences. Moreover we debar ourselves from granting any favoured status to statements which describe experiences, such as those statements which describe our perceptions (and which are sometimes called 'protocol sentences'). They can occur in science only as psychological statements; and this means, as hypotheses of a kind whose standards of inter-subjective testing (considering the present state of psychology) are certainly not very high.

Whatever may be our eventual answer to the question of the empirical basis, one thing must be clear: if we adhere to our demand that scientific statements must be objective, then those statements which belong to the empirical basis of science must also be objective, i.e. inter-subjectively testable. Yet inter-subjective testability always implies that, from the statements which are to be tested, other testable statements can be deduced. Thus if the basic statements in their turn are to be inter-subjectively testable, *there can be no ultimate statements in science*: there can be no statements in science which cannot be tested, and therefore none which cannot in principle be refuted, by falsifying some of the conclusions which can be deduced from them.

We thus arrive at the following view. Systems of theories are tested by deducing from them statements of a lesser level of universality. These statements in their turn, since they are to be inter-subjectively testable, must be testable in like manner—and so *ad infinitum*.

It might be thought that this view leads to an infinite regress, and that it is therefore untenable. In section 1, when criticizing induction, I raised the objection that it may lead to an infinite regress; and it might well appear to the reader now that the very same objection can be urged against that procedure of deductive testing which I myself advocate. However, this is not so. The deductive method of testing cannot

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establish or justify the statements which are being tested; nor is it intended to do so. Thus there is no danger of an infinite regress. But it must be admitted that the situation to which I have drawn attention—testability *ad infinitum* and the absence of ultimate statements which are not in need of tests—does create a problem. For, clearly, tests cannot in fact be carried on *ad infinitum*: sooner or later we have to stop. Without discussing this problem here in detail, I only wish to point out that the fact that the tests cannot go on for ever does not clash with my demand that every scientific statement must be testable. For I do not demand that every scientific statement must *have in fact been tested* before it is accepted. I only demand that every such statement must be *capable* of being tested; or in other words, I refuse to accept the view that there are statements in science which we have, resignedly, to accept as true merely because it does not seem possible, for logical reasons, to test them.

2

ON THE PROBLEM OF A THEORY OF SCIENTIFIC METHOD

In accordance with my proposal made above, epistemology, or the logic of scientific discovery, should be identified with the theory of scientific method. The theory of method, in so far as it goes beyond the purely logical analysis of the relations between scientific statements, is concerned with *the choice of methods*—with decisions about the way in which scientific statements are to be dealt with. These decisions will of course depend in their turn upon the *aim* which we choose from among a number of possible aims. The decision here proposed for laying down suitable rules for what I call the 'empirical method' is closely connected with my criterion of demarcation: I propose to adopt such rules as will ensure the testability of scientific statements; which is to say, their falsifiability.

9 WHY METHODOLOGICAL DECISIONS ARE INDISPENSABLE

What are rules of scientific method, and why do we need them? Can there be a theory of such rules, a methodology?

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The way in which one answers these questions will largely depend upon one's attitude to science. Those who, like the positivists, see empirical science as a system of statements which satisfy certain *logical criteria*, such as meaningfulness or verifiability, will give one answer. A very different answer will be given by those who tend to see (as I do) the distinguishing characteristic of empirical statements in their susceptibility to revision—in the fact that they can be criticized, and superseded by better ones; and who regard it as their task to analyse the characteristic ability of science to advance, and the characteristic manner in which a choice is made, in crucial cases, between conflicting systems of theories.

I am quite ready to admit that there is a need for a purely logical analysis of theories, for an analysis which takes no account of how they change and develop. But this kind of analysis does not elucidate those aspects of the empirical sciences which I, for one, so highly prize. A system such as classical mechanics may be 'scientific' to any degree you like; but those who uphold it dogmatically—believing, perhaps, that it is their business to defend such a successful system against criticism as long as it is not *conclusively disproved*—are adopting the very reverse of that critical attitude which in my view is the proper one for the scientist. In point of fact, no conclusive disproof of a theory can ever be produced; for it is always possible to say that the experimental results are not reliable, or that the discrepancies which are asserted to exist between the experimental results and the theory are only apparent and that they will disappear with the advance of our understanding. (In the struggle against Einstein, both these arguments were often used in support of Newtonian mechanics, and similar arguments abound in the field of the social sciences.) If you insist on strict proof (or strict disproof*[1]) in the empirical sciences, you will never benefit from experience, and never learn from it how wrong you are.

If therefore we characterize empirical science merely by the formal

*[1] I have now here added in brackets the words 'or strict disproof' to the text (a) because they are clearly implied by what is said immediately before ('no conclusive disproof of a theory can ever be produced'), and (b) because I have been constantly misinterpreted as upholding a criterion (and moreover one of *meaning* rather than of *demarcation*) based upon a doctrine of 'complete' or 'conclusive' falsifiability.

or logical structure of its statements, we shall not be able to exclude from it that prevalent form of metaphysics which results from elevating an obsolete scientific theory into an incontrovertible truth.

Such are my reasons for proposing that empirical science should be characterized by its methods: by our manner of dealing with scientific systems: by what we do with them and what we do to them. Thus I shall try to establish the rules, or if you will the norms, by which the scientist is guided when he is engaged in research or in discovery, in the sense here understood.

10 THE NATURALISTIC APPROACH TO THE THEORY OF METHOD

The hint I gave in the previous section as to the deep-seated difference between my position and that of the positivists is in need of some amplification.

The positivist dislikes the idea that there should be meaningful problems outside the field of 'positive' empirical science—problems to be dealt with by a genuine philosophical theory. He dislikes the idea that there should be a genuine theory of knowledge, an epistemology or a methodology.*[1] He wishes to see in the alleged philosophical problems mere 'pseudo-problems' or 'puzzles'. Now this wish of his—which, by the way, he does not express as a wish or a proposal but rather as a statement of fact*[2]—can always be gratified. For nothing is easier than to unmask a problem as 'meaningless' or 'pseudo'. All you have to do is to fix upon a conveniently narrow meaning for 'meaning', and you will soon be bound to say of any inconvenient question that you are unable to detect any meaning in it. Moreover, if you admit as

*[1] In the two years before the first publication of this book, it was the standing criticism raised by members of the Vienna Circle against my ideas that a theory of method which was neither an empirical science nor pure logic was impossible: what was outside these two fields was sheer nonsense. (The same view was still maintained by Wittgenstein in 1948; cf. my paper 'The Nature of Philosophical Problems', *The British Journal for the Philosophy of Science* 3, 1952, note on p. 128.) Later, the standing criticism became anchored in the legend that I had proposed to replace the verifiability criterion by a falsifiability criterion of *meaning*. See my *Postscript*, especially sections *19 to *22.

*[2] Some positivists have since changed this attitude; see note 6, below.

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meaningful none except problems in natural science,[1] any debate about the concept of 'meaning' will also turn out to be meaningless.[2] The dogma of meaning, once enthroned, is elevated forever above the battle. It can no longer be attacked. It has become (in Wittgenstein's own words) 'unassailable and definitive'.[3]

The controversial question whether philosophy exists, or has any right to exist, is almost as old as philosophy itself. Time and again an entirely new philosophical movement arises which finally unmasks the old philosophical problems as pseudo-problems, and which confronts the wicked nonsense of philosophy with the good sense of meaningful, positive, empirical, science. And time and again do the despised defenders of 'traditional philosophy' try to explain to the leaders of the latest positivistic assault that the main problem of philosophy is the critical analysis of the appeal to the authority of 'experience'[4]—precisely that 'experience' which every latest discoverer of positivism is, as ever, artlessly taking for granted. To such objections, however, the positivist only replies with a shrug: they mean nothing to him, since they do not belong to empirical science, which alone is meaningful. 'Experience' for him is a programme, not a problem (unless it is studied by empirical psychology).

I do not think positivists are likely to respond any differently to my own attempts to analyse 'experience' which I interpret as the method of empirical science. For only two kinds of statement exist for them: logical tautologies and empirical statements. If methodology is not logic, then, they will conclude, it must be a branch of some empirical science—the science, say, of the behaviour of scientists at work.

This view, according to which methodology is an empirical science in its turn—a study of the actual behaviour of scientists, or of the actual

[1] Wittgenstein, *Tractatus Logico-Philosophicus*, Proposition 6.53.

[2] Wittgenstein at the end of the *Tractatus* (in which he explains the concept of meaning) writes, 'My propositions are elucidatory in this way: he who understands me finally recognizes them as senseless. . . .' Cp. Sextus Adv. Log. ii, 481; Loeb edn.ii, 488.)

[3] Wittgenstein, *op. cit.*, at the end of his Preface.

[4] H. Gomperz (*Weltanschauungslehre I*, 1905, p. 35) writes: 'If we consider how infinitely problematic the concept of *experience* is . . . we may well be forced to believe that . . . enthusiastic affirmation is far less appropriate in regard to it . . . than the most careful and guarded criticism . . . .'

procedure of 'science'—may be described as '*naturalistic*'. A naturalistic methodology (sometimes called an 'inductive theory of science'[5]) has its value, no doubt. A student of the logic of science may well take an interest in it, and learn from it. But what I call 'methodology' should not be taken for an empirical science. I do not believe that it is possible to decide, by using the methods of an empirical science, such controversial questions as whether science actually uses a principle of induction or not. And my doubts increase when I remember that what is to be called a 'science' and who is to be called a 'scientist' must always remain a matter of convention or decision.

I believe that questions of this kind should be treated in a different way. For example, we may consider and compare two different systems of methodological rules; one with, and one without, a principle of induction. And we may then examine whether such a principle, once introduced, can be applied without giving rise to inconsistencies; whether it helps us; and whether we really need it. It is this type of inquiry which leads me to dispense with the principle of induction: not because such a principle is as a matter of fact never used in science, but because I think that it is not needed; that it does not help us; and that it even gives rise to inconsistencies.

Thus I reject the naturalistic view. It is uncritical. Its upholders fail to notice that whenever they believe themselves to have discovered a fact, they have only proposed a convention.[6] Hence the convention is liable to turn into a dogma. This criticism of the naturalistic view applies not only to its criterion of meaning, but also to its idea of science, and consequently to its idea of empirical method.

[5] Dingler, *Physik und Hypothesis*, Versuch einer induktiven Wissenschaftslehre, 1921; similarly V. Kraft, *Die Grundformen der wissenschaftlichen Methoden*, 1925.

[6] (Addition made in 1934 while this book was in proof.) The view, only briefly set forth here, that it is a matter for decision what is to be called 'a genuine statement' and what 'a meaningless pseudo-statement' is one that I have held for years. (Also the view that the exclusion of metaphysics is likewise a matter for decision.) However, my present criticism of positivism (and of the naturalistic view) no longer applies, as far as I can see, to Carnap's *Logische Syntax der Sprache*, 1934, in which he too adopts the standpoint that all such questions rest upon decisions (the 'principle of tolerance'). According to Carnap's preface, Wittgenstein has for years propounded a similar view in unpublished works. (*See however note *1 above.) Carnap's *Logische Syntax* was published while the present book was in proof. I regret that I was unable to discuss it in my text.

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11 METHODOLOGICAL RULES AS CONVENTIONS

Methodological rules are here regarded as *conventions*. They might be described as the rules of the game of empirical science. They differ from the rules of pure logic rather as do the rules of chess, which few would regard as part of *pure* logic: seeing that the rules of pure logic govern transformations of linguistic formulae, the result of an inquiry into the rules of chess could perhaps be entitled 'The Logic of Chess', but hardly 'Logic' pure and simple. (Similarly, the result of an inquiry into the rules of the game of science—that is, of scientific discovery—may be entitled 'The Logic of Scientific Discovery'.)

Two simple examples of methodological rules may be given. They will suffice to show that it would be hardly suitable to place an inquiry into method on the same level as a purely logical inquiry.

(1) The game of science is, in principle, without end. He who decides one day that scientific statements do not call for any further test, and that they can be regarded as finally verified, retires from the game.

(2) Once a hypothesis has been proposed and tested, and has proved its mettle,*[1] it may not be allowed to drop out without 'good reason'. A 'good reason' may be, for instance: replacement of the hypothesis by another which is better testable; or the falsification of one of the consequences of the hypothesis. (The concept 'better testable' will later be analysed more fully.)

These two examples show what methodological rules look like. Clearly they are very different from the rules usually called 'logical'. Although logic may perhaps set up criteria for deciding whether a statement is testable, it certainly is not concerned with the question whether anyone exerts himself to test it.

In section 6 I tried to define empirical science with the help of the criterion of falsifiability; but as I was obliged to admit the justice of certain objections, I promised a methodological supplement to my definition. Just as chess might be defined by the rules proper to it, so empirical science may be defined by means of its methodological rules.

*[1] Regarding the translation 'to prove one's mettle' for 'sich bewähren', see the first footnote to chapter 10 (Corroboration), below.

In establishing these rules we may proceed systematically. First a supreme rule is laid down which serves as a kind of norm for deciding upon the remaining rules, and which is thus a rule of a higher type. It is the rule which says that the other rules of scientific procedure must be designed in such a way that they do not protect any statement in science against falsification.

Methodological rules are thus closely connected both with other methodological rules and with our criterion of demarcation. But the connection is not a strictly deductive or logical one.[1] It results, rather, from the fact that the rules are constructed with the aim of ensuring the applicability of our criterion of demarcation; thus their formulation and acceptance proceeds according to a practical rule of a higher type. An example of this has been given above (*cf.* rule 1): theories which we decide not to submit to any further test would no longer be falsifiable. It is this systematic connection between the rules which makes it appropriate to speak of a *theory* of method. Admittedly the pronouncements of this theory are, as our examples show, for the most part conventions of a fairly obvious kind. Profound truths are not to be expected of methodology.*[2] Nevertheless it may help us in many cases to clarify the logical situation, and even to solve some far-reaching problems which have hitherto proved intractable. One of these, for example, is the problem of deciding whether a probability statement should be accepted or rejected. (Cf. section 68.)

It has often been doubted whether the various problems of the theory of knowledge stand in any systematic relation to one another, and also whether they can be treated systematically. I hope to show in this book that these doubts are unjustified. The point is of some importance. My only reason for proposing my criterion of demarcation is that it is fruitful: that a great many points can be clarified and explained with its help. 'Definitions are dogmas; only the conclusions drawn from them can afford us any new insight', says Menger.[2] This is

[1] Cf. K. Menger. *Moral, Wille und Weltgestaltung*, 1934, pp. 58 ff.

*[2] I am still inclined to uphold something like this, even though such theorems as '*degree of corroboration* ≠ *probability*', or my 'theorem on truth-content' (see the Feigl Festschrift: *Mind, Matter, and Method*, edited by P. K. Feyerabend and G. Maxwell, 1966, pp. 343–353) are perhaps unexpected and not quite on the surface.

[2] K. Menger, *Dimensionstheorie*, 1928, p. 76.

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certainly true of the definition of the concept 'science'. It is only from the consequences of my definition of empirical science, and from the methodological decisions which depend upon this definition, that the scientist will be able to see how far it conforms to his intuitive idea of the goal of his endeavours.*[3]

The philosopher too will accept my definition as useful only if he can accept its consequences. We must satisfy him that these consequences enable us to detect inconsistencies and inadequacies in older theories of knowledge, and to trace these back to the fundamental assumptions and conventions from which they spring. But we must also satisfy him that our own proposals are not threatened by the same kind of difficulties. This method of detecting and resolving contradictions is applied also within science itself, but it is of particular importance in the theory of knowledge. It is by this method, if by any, that methodological conventions might be justified, and might prove their value.[3]

Whether philosophers will regard these methodological investigations as belonging to philosophy is, I fear, very doubtful, but this does not really matter much. Yet it may be worth mentioning in this connection that not a few doctrines which are metaphysical, and thus certainly philosophical, could be interpreted as typical hypostatizations of methodological rules. An example of this, in the shape of what is called 'the principle of causality', will be discussed in the next section. Another example which we have already encountered is the problem of objectivity. For the requirement of scientific objectivity can also be interpreted as a methodological rule: the rule that only such statements may be introduced in science as are inter-subjectively testable (see sections 8, 20, 27, and elsewhere). It might indeed be said that the majority of the problems of theoretical philosophy, and the most interesting ones, can be re-interpreted in this way as problems of method.

*[3] See also section *15, 'The Aim of Science', of my *Postscript*.

[3] In the present work I have relegated the critical—or, if you will, the 'dialectical'—method of resolving contradictions to second place, since I have been concerned with the attempt to develop the practical methodological aspects of my views. In an as yet unpublished work I have tried to take the critical path; and I have tried to show that the problems of both the classical and the modern theory of knowledge (from Hume via Kant to Russell and Whitehead) can be traced back to the problem of demarcation, that is, to the problem of finding the criterion of the empirical character of science.

Part II

Some Structural Components of a Theory of Experience

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3

THEORIES

The empirical sciences are systems of theories. The logic of scientific knowledge can therefore be described as a theory of theories.

Scientific theories are universal statements. Like all linguistic representations they are systems of signs or symbols. Thus I do not think it helpful to express the difference between universal theories and singular statements by saying that the latter are 'concrete' whereas theories are *merely* symbolic formulae or symbolic schemata; for exactly the same may be said of even the most 'concrete' statements.*[1]

Theories are nets cast to catch what we call 'the world': to

*[1] This is a critical allusion to a view which I later described as 'instrumentalism' and which was represented in Vienna by Mach, Wittgenstein, and Schlick (cf. notes *4 and 7 to section 4, and note 5 to section 27). It is the view that a theory is *nothing but* a tool or an instrument for prediction. I have analysed and criticized it in my papers 'A Note on Berkeley as a Precursor of Mach', *Brit. Journ. Philos. Science* 6, 1953, pp. 26 ff.; 'Three Views Concerning Human Knowledge', in *Contemporary British Philosophy* iii, 1956, edited by H. D. Lewis, pp. 355 ff.; and more fully in my *Postscript*, sections *11 to *15 and *19 to *26. My point of view is, briefly, that our ordinary language is full of theories: that observation is always *observation in the light of theories*; that it is only the inductivist prejudice which leads people to think that there could be a phenomenal language, free of theories, and distinguishable from a 'theoretical language'; and lastly, that the theorist is interested in explanation as such, that is to say, in testable explanatory theories: applications and predictions interest him only for theoretical reasons—because they may be used as *tests* of theories. See also the new appendix *x.

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rationalize, to explain, and to master it. We endeavour to make the mesh ever finer and finer.

12 CAUSALITY, EXPLANATION, AND THE DEDUCTION OF PREDICTIONS

To give a *causal explanation* of an event means to deduce a statement which describes it, using as premises of the deduction one or more *universal laws*, together with certain singular statements, the *initial conditions*. For example, we can say that we have given a causal explanation of the breaking of a certain piece of thread if we have found that the thread has a tensile strength of 1 *lb.* and that a weight of 2 *lbs.* was put on it. If we analyse this causal explanation we shall find several constituent parts. On the one hand there is the hypothesis: 'Whenever a thread is loaded with a weight exceeding that which characterizes the tensile strength of the thread, then it will break'; a statement which has the character of a universal law of nature. On the other hand we have singular statements (in this case two) which apply only to the specific event in question: 'The weight characteristic for this thread is 1 *lb.*', and 'The weight put on this thread was 2 *lbs.*'*[1]

We have thus two different kinds of statement, both of which are necessary ingredients of a complete causal explanation. They are (1) *universal statements*, i.e. hypotheses of the character of natural laws, and (2) *singular statements*, which apply to the specific event in question and which I shall call 'initial conditions'. It is from universal statements in conjunction with initial conditions that we *deduce* the singular statement, 'This thread will break'. We call this statement a specific or singular *prediction*.*[2]

The initial conditions describe what is usually called the '*cause*' of the

*[1] A clearer analysis of this example—and one which distinguishes *two* laws as well as two initial conditions—would be the following: 'For every thread of a given structure S (determined by its material, thickness, etc.) there is a characteristic weight w, such that the thread will break if any weight exceeding w is suspended from it.'—'For every thread of the structure S_1, the characteristic weight w_1 equals 1 *lb.*' These are the two universal laws. The two initial conditions are, 'This is a thread of structure S_1' and, 'The weight to be put on this thread is equal to 2 *lbs.*'

*[2] The term 'prediction', as used here, comprises statements about the past ('retrodictions'), or even 'given' statements which we wish to explain ('*explicanda*'); cf. my *Poverty of Historicism*, 1945, p. 133 of the edition of 1957, and the *Postscript*, section *15.

event in question. (The fact that a load of 2 *lbs.* was put on a thread with a tensile strength of 1 *lb.* was the 'cause' of its breaking.) And the prediction describes what is usually called the '*effect*'. Both these terms I shall avoid. In physics the use of the expression '*causal explanation*' is restricted as a rule to the special case in which the universal laws have the form of laws of 'action by contact'; or more precisely, of *action at a vanishing distance*, expressed by differential equations. This restriction will not be assumed here. Furthermore, I shall not make any general assertion as to the universal applicability of this deductive method of theoretical explanation. Thus I shall not assert any '*principle of causality*' (or 'principle of universal causation').

The 'principle of causality' is the assertion that any event whatsoever *can* be causally explained—that it *can* be deductively predicted. According to the way in which one interprets the word 'can' in this assertion, it will be either tautological (analytic), or else an assertion about reality (synthetic). For if 'can' means that it is always logically possible to construct a causal explanation, then the assertion is tautological, since for any prediction whatsoever we can always find universal statements and initial conditions from which the prediction is derivable. (Whether these universal statements have been tested and corroborated in other cases is of course quite a different question.) If, however, 'can' is meant to signify that the world is governed by strict laws, that it is so constructed that every specific event is an instance of a universal regularity or law, then the assertion is admittedly synthetic. But in this case it is *not falsifiable*, as will be seen later, in section 78. I shall, therefore, neither adopt nor reject the 'principle of causality'; I shall be content simply to exclude it, as 'metaphysical', from the sphere of science.

I shall, however, propose a methodological rule which corresponds so closely to the 'principle of causality' that the latter might be regarded as its metaphysical version. It is the simple rule that we are not to abandon the search for universal laws and for a coherent theoretical system, nor ever give up our attempts to explain causally any kind of event we can describe.[1] This rule guides the scientific investigator in his

[1] The idea of regarding the principle of causality as the expression of a rule or of a decision is due to H. Gomperz, *Das Problem der Willensfreiheit*, 1907. Cf. Schlick, *Die Kausalitat in der gegenwartigen Physik*, *Naturwissenschaften* **19**, 1931, p. 154.

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