Part I

Introduction to the Logic of Science

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.

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*.[*3]

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

[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.

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

[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.

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'.

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.

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 *1 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.

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).

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 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.

[11] Cf. also: Planck, *Positivismus und reale 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.

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.

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*.

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.

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. Hauptstück, 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.

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 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?

be read: 'p follows from t'. Assume p to be false, which we may write '$\bar{p}$', to be read 'not-p'. Given the relation of deducibility, $t \rightarrow p$, and the assumption $\bar{p}$, we can then infer $\bar{t}$ (read 'not-t'); that is, we regard t as falsified. If we denote the conjunction (simultaneous assertion) of two statements by putting a point between the symbols standing for them, we may also write the falsifying inference thus: $((t \rightarrow p).\bar{p}) \rightarrow \bar{t}$, or in words: 'If p is derivable from t, and if p is false, then t also is false'.

By means of this mode of inference we falsify *the whole system* (the theory as well as the initial conditions) which was required for the deduction of the statement p, i.e. of the falsified statement. Thus it cannot be asserted of any one statement of the system that it is, or is not, specifically upset by the falsification. Only if p is *independent* of some part of the system can we say that this part is not involved in the falsification.[3] With this is connected the following possibility: we may, in some cases, perhaps in consideration of the *levels of universality*, attribute the falsification to some definite hypothesis—for instance to a newly introduced hypothesis. This may happen if a well-corroborated theory, and one which continues to be further corroborated, has been deductively explained by a new hypothesis of a higher level. The attempt will have to be made to test this new hypothesis by means of some of its consequences which have not yet been tested. If any of these are falsified, then we may well attribute the falsification to the new hypothesis alone. We shall then seek, in its stead, other high-level generalizations, but we shall not feel obliged to regard the old system, of lesser generality, as having been falsified. (Cf. also the remarks on 'quasi-induction' in section 85.)

[3] Thus we cannot at first know which among the various statements of the remaining sub-system t' (of which p is not independent) we are to blame for the falsity of p; which of these statements we have to alter, and which we should retain. (I am not here discussing interchangeable statements.) It is often only the scientific instinct of the investigator (influenced, of course, by the results of testing and re-testing) that makes him guess which statements of t' he should regard as innocuous, and which he should regard as being in need of modification. Yet it is worth remembering that it is often the modification of what we are inclined to regard as obviously innocuous (because of its complete agreement with our normal habits of thought) which may produce a decisive advance. A notable example of this is Einstein's modification of the concept of simultaneity.

4

FALSIFIABILITY

The question whether there is such a thing as a falsifiable singular statement (or a 'basic statement') will be examined later. Here I shall assume a positive answer to this question; and I shall examine how far my criterion of demarcation is applicable to theoretical systems—if it is applicable at all. A critical discussion of a position usually called 'conventionalism' will raise first some problems of method, to be met by taking certain *methodological decisions*. Next I shall try to characterize the logical properties of those systems of theories which are falsifiable—falsifiable, that is, if our methodological proposals are adopted.

19 SOME CONVENTIONALIST OBJECTIONS

Objections are bound to be raised against my proposal to adopt falsifiability as our criterion for deciding whether or not a theoretical system belongs to empirical science. They will be raised, for example, by those who are influenced by the school of thought known as 'conventionalism'.[1] Some of these objections have already been touched upon in

[1] The chief representatives of the school are Poincaré and Duhem (cf. *La théorie physique, son objet et sa structure*, 1906; English translation by P. P. Wiener: *The Aim and Structure of Physical Theory*, Princeton, 1954). A recent adherent is H. Dingler (among his numerous works may be mentioned: *Das Experiment*, and *Der Zusammenbruch der Wissenschaft und das Primat der*

sections 6, 11, and 17; they will now be considered a little more closely.

The source of the conventionalist philosophy would seem to be wonder at the austerely beautiful *simplicity of the world* as revealed in the laws of physics. Conventionalists seem to feel that this simplicity would be incomprehensible, and indeed miraculous, if we were bound to believe, with the realists, that the laws of nature reveal to us an inner, a structural, simplicity of our world beneath its outer appearance of lavish variety. Kant's idealism sought to explain this simplicity by saying that it is our own intellect which imposes its laws upon nature. Similarly, but even more boldly, the conventionalist treats this simplicity as our own creation. For him, however, it is not the effect of the laws of our intellect imposing themselves upon nature, thus making nature simple; for he does not believe that nature is simple. Only the '*laws of nature*' are simple; and these, the conventionalist holds, are our own free creations; our inventions; our arbitrary decisions and conventions. For the conventionalist, theoretical natural science is not a picture of nature but merely a logical construction. It is not the properties of the world which determine this construction; on the contrary it is this construction which determines the properties of an artificial world: a world of concepts implicitly defined by the natural laws which we have chosen. It is only this world of which science speaks.

According to this conventionalist point of view, laws of nature are not falsifiable by observation; for they are needed to determine what an observation and, more especially, what a scientific measurement is. It is these laws, laid down, by us, which form the indispensable basis for the regulation of our clocks and the correction of our so-called 'rigid' measuring-rods. A clock is called 'accurate' and a measuring rod 'rigid' only if the movements measured with the help of these instruments satisfy the axioms of mechanics which we have decided to adopt.[1]

The philosophy of conventionalism deserves great credit for the way it has helped to clarify the relations between theory and experiment. It recognized the importance, so little noticed by inductivists, of the part played by our actions and operations, planned in accordance with conventions and deductive reasoning, in conducting and interpreting our scientific experiments. I regard conventionalism as a system which is self-contained and defensible. Attempts to detect inconsistencies in it are not likely to succeed. Yet in spite of all this I find it quite unacceptable. Underlying it is an idea of science, of its aims and purposes, which is entirely different from mine. Whilst I do not demand any final certainty from science (and consequently do not get it), the conventionalist seeks in science 'a system of knowledge based upon ultimate grounds', to use a phrase of Dingler's. This goal is attainable; for it is possible to interpret any given scientific system as a system of implicit definitions. And periods when science develops slowly will give little occasion for conflict—unless purely academic—to arise between scientists inclined towards conventionalism and others who may favour a view like the one I advocate. It will be quite otherwise in a time of crisis. Whenever the 'classical' system of the day is threatened by the results of new experiments which might be interpreted as falsifications according to my point of view, the system will appear unshaken to the conventionalist. He will explain away the inconsistencies which may have arisen; perhaps by blaming our inadequate mastery of the system.

Philosophie, 1926). *The German Hugo Dingler should not be confused with the Englishman Herbert Dingle. The chief representative of conventionalism in the English-speaking world is Eddington. It may be mentioned here that Duhem denies (Engl. transl. p. 188) the possibility of crucial experiments, because he thinks of them as verifications, while I assert the possibility of crucial *falsifying* experiments. Cf. also my paper 'Three Views Concerning Human Knowledge', in *Contemporary British Philosophy*, iii, 1956, and in my *Conjectures and Refutations*, 1959.

[1] This view can also be regarded as an attempt to solve the problem of induction; for the problem would vanish if natural laws were definitions, and therefore tautologies. Thus according to the views of Cornelius (cf. *Zur Kritik der wissenschaftlichen Grundbegriffe*, *Erkenntnis* **2**, 1931, Number 4) the statement, 'The melting point of lead is about 335°C.' is part of the definition of the concept 'lead' (suggested by inductive experience) and cannot therefore be refuted. A substance otherwise resembling lead but with a different melting point would simply not be lead. But according to my view the statement of the melting point of lead is, *qua* scientific statement, synthetic. It asserts, among other things, that an element with a given atomic structure (atomic number 82) always has this melting point, whatever name we may give to this element.

(Added to the book in proof.) Ajdukiewicz appears to agree with Cornelius (cf. *Erkenntnis* **4**, 1934, pp. 100 f., as well as the work there announced, *Das Weltbild und die Begriffsapparatur*); he calls his standpoint 'radical conventionalism'.

Or he will eliminate them by suggesting *ad hoc* the adoption of certain auxiliary hypotheses, or perhaps of certain corrections to our measuring instruments.

In such times of crisis this conflict over the aims of science will become acute. We, and those who share our attitude, will hope to make new discoveries; and we shall hope to be helped in this by a newly erected scientific system. Thus we shall take the greatest interest in the falsifying experiment. We shall hail it as a success, for it has opened up new vistas into a world of new experiences. And we shall hail it even if these new experiences should furnish us with new arguments against our own most recent theories. But the newly rising structure, the boldness of which we admire, is seen by the conventionalist as a monument to the 'total collapse of science', as Dingler puts it. In the eyes of the conventionalist one principle only can help us to select a system as the chosen one from among all other possible systems: it is the principle of selecting the simplest system—the simplest system of implicit definitions; which of course means in practice the 'classical' system of the day. (For the problem of simplicity see sections 41–45, and especially 46.)

Thus my conflict with the conventionalists is not one that can be ultimately settled merely by a detached theoretical discussion. And yet it is possible I think to extract from the conventionalist mode of thought certain interesting arguments against my criterion of demarcation; for instance the following. I admit, a conventionalist might say, that the theoretical systems of the natural sciences are not verifiable, but I assert that they are not falsifiable either. For there is always the possibility of '. . . attaining, for any chosen axiomatic system, what is called its "correspondence with reality" ';[3] and this can be done in a number of ways (some of which have been suggested above). Thus we may introduce *ad hoc* hypotheses. Or we may modify the so-called 'ostensive definitions' (or the 'explicit definitions' which may replace them as shown in section 17). Or we may adopt a sceptical attitude as to the reliability of the experimenter whose observations, which threaten our system, we may exclude from science on the ground that they are insufficiently supported, unscientific, or not objective, or even on the ground that the experimenter was a liar. (This is the sort of attitude which the physicist may sometimes quite rightly adopt towards alleged occult phenomena.) In the last resort we can always cast doubt on the acumen of the theoretician (for example if he does not believe, as does Dingler, that the theory of electricity will one day be derived from Newton's theory of gravitation).

Thus, according to the conventionalist view, it is not possible to divide systems of theories into falsifiable and non-falsifiable ones; or rather, such a distinction will be ambiguous. As a consequence, our criterion of falsifiability must turn out to be useless as a criterion of demarcation.

[3] Carnap, *Über die Aufgabe der Physik*, *Kantstudien*, **28**, 1923, p. 100.

20 METHODOLOGICAL RULES

These objections of an imaginary conventionalist seem to me incontestable, just like the conventionalist philosophy itself. I admit that my criterion of falsifiability does not lead to an unambiguous classification. Indeed, it is impossible to decide, by analysing its logical form, whether a system of statements is a conventional system of irrefutable implicit definitions, or whether it is a system which is empirical in my sense; that is, a refutable system. Yet this only shows that my criterion of demarcation cannot be applied immediately to a *system of statements*—a fact I have already pointed out in sections 9 and 11. The question whether a given *system* should as such be regarded as a conventionalist or an empirical one is therefore misconceived. *Only with reference to the methods applied* to a theoretical system is it at all possible to ask whether we are dealing with a conventionalist or an empirical theory. The only way to avoid conventionalism is by taking a *decision*: the decision not to apply its methods. We decide that if our system is threatened we will never save it by any kind of *conventionalist stratagem*. Thus we shall guard against exploiting the ever open possibility just mentioned of '. . . attaining for any chosen . . . system what is called its "correspondence with reality" '.

A clear appreciation of what may be gained (and lost) by conventionalist methods was expressed, a hundred years before Poincaré, by Black who wrote: 'A nice adaptation of conditions will make almost any hypothesis agree with the phenomena. This will please the imagination but does not advance our knowledge.'[1]

In order to formulate methodological rules which prevent the adoption of conventionalist stratagems, we should have to acquaint ourselves with the various forms these stratagems may take, so as to meet each with the appropriate anti-conventionalist counter-move. Moreover we should agree that, whenever we find that a system has been rescued by a conventionalist stratagem, we shall test it afresh, and reject it, as circumstances may require.

The four main conventionalist stratagems have already been listed at the end of the previous section. The list makes no claim to completeness: it must be left to the investigator, especially in the fields of sociology and psychology (the physicist may hardly need the warning) to guard constantly against the temptation to employ new conventionalist stratagems—a temptation to which psycho-analysts, for example, often succumb.

As regards *auxiliary hypotheses* we propose to lay down the rule that only those are acceptable whose introduction does not diminish the degree of falsifiability or testability of the system in question, but, on the contrary, increases it. (How degrees of falsifiability are to be estimated will be explained in sections 31 to 40.) If the degree of falsifiability is increased, then introducing the hypothesis has actually strengthened the theory: the system now rules out more than it did previously: it prohibits more. We can also put it like this. The introduction of an auxiliary hypothesis should always be regarded as an attempt to construct a new system; and this new system should then always be judged on the issue of whether it would, if adopted, constitute a real advance in our knowledge of the world. An example of an auxiliary hypothesis which is eminently acceptable in this sense is Pauli's exclusion principle (cf. section 38). An example of an unsatisfactory auxiliary hypothesis would be the contraction hypothesis of Fitzgerald and Lorentz which had no falsifiable consequences but merely*[1] served to restore the agreement between theory and experiment—mainly the findings of Michelson and Morley. An advance was here achieved only by the theory of relativity which predicted new consequences, new physical effects, and thereby opened up new possibilities for testing, and for falsifying, the theory. Our methodological rule may be qualified by the remark that we need not reject, as conventionalistic, every auxiliary hypothesis that fails to satisfy these standards. In particular, there are *singular* statements which do not really belong to the theoretical system at all. They are sometimes called 'auxiliary hypotheses', and although they are introduced to assist the theory, they are quite harmless. (An example would be the assumption that a certain observation or measurement which cannot be repeated may have been due to error. Cf. note 6 to section 8, and sections 27 and 68.)

In section 17 I mentioned *explicit definitions* whereby the concepts of an axiom system are given a meaning in terms of a system of lower level universality. Changes in these definitions are permissible if useful; but they must be regarded as modifications of the system, which thereafter has to be re-examined as if it were new. As regards undefined universal names, two possibilities must be distinguished: (1) There are some undefined concepts which only appear in statements of the highest level of universality, and whose use is established by the fact that we know in what logical relation other concepts stand to them. They can be eliminated in the course of deduction (an example is 'energy').[2] (2) There are other undefined concepts which occur in statements of lower levels of universality also, and whose meaning is established by usage (e.g. 'movement', 'mass-point', 'position'). In connection with these, we shall forbid surreptitious alterations of usage, and otherwise proceed in conformity with our methodological decisions, as before.

As to the two remaining points (which concern the competence of the experimenter or theoretician) we shall adopt similar rules. Intersubjectively testable experiments are either to be accepted, or to be rejected in the light of counter-experiments. The bare appeal to logical derivations to be discovered in the future can be disregarded.

[1] J. Black, *Lectures on the Elements of Chemistry*, Vol. I, Edinburgh, 1803, p. 193.

*[1] This is a mistake, as pointed out by A. Grünbaum, *B.J.P.S.* **10**, 1959, pp. 48 ff. Yet as this hypothesis is less testable than special relativity, it may illustrate *degrees of adhocness*.

[2] Compare, for instance, Hahn, *Logik, Mathematik, und Naturerkennen*, in *Einheitswissenschaft* **2**, 1933, pp. 22 ff. In this connection, I only wish to say that in my view 'constituable' (i.e. empirically definable) terms do not exist at all. I am using in their place undefinable universal names which are established only by linguistic usage. See also end of section 25.

21 LOGICAL INVESTIGATION OF FALSIFIABILITY

Only in the case of systems which would be falsifiable if treated in accordance with our rules of empirical method is there any need to guard against conventionalist stratagems. Let us assume that we have successfully banned these stratagems by our rules: we may now ask for a *logical* characterization of such falsifiable systems. We shall attempt to characterize the falsifiability of a theory by the logical relations holding between the theory and the class of basic statements.

The character of the singular statements which I call 'basic statements' will be discussed more fully in the next chapter, and also the question whether they, in their turn, are falsifiable. Here we shall assume that falsifiable basic statements exist. It should be borne in mind that when I speak of 'basic statements', I am not referring to a system of *accepted* statements. The system of basic statements, as I use the term, is to include, rather, *all self-consistent singular statements* of a certain logical form—all conceivable singular statements of fact, as it were. Thus the system of all basic statements will contain many statements which are mutually incompatible.

As a first attempt one might perhaps try calling a theory 'empirical' whenever singular statements can be deduced from it. This attempt fails, however, because in order to deduce singular statements from a theory, we always need other singular statements—the initial conditions that tell us what to substitute for the variables in the theory. As a second attempt, one might try calling a theory 'empirical' if singular statements are derivable with the help of other singular statements serving as initial conditions. But this will not do either; for even a non-empirical theory, for example a tautological one, would allow us to derive some singular statements from other singular statements. (According to the rules of logic we can for example say: From the conjunction of 'Twice two is four' and 'Here is a black raven' there follows, among other things, 'Here is a raven'.) It would not even be enough to demand that from the theory together with some initial conditions we should be able to deduce *more* than we could deduce from those initial conditions alone. This demand would indeed exclude tautological theories, but it would not exclude synthetic metaphysical statements. (For example from 'Every occurrence has a cause' and 'A catastrophe is occurring here', we can deduce 'This catastrophe has a cause'.)

In this way we are led to the demand that the theory should allow us to deduce, roughly speaking, more *empirical* singular statements than we can deduce from the initial conditions alone.*1 This means that we must base our definition upon a particular class of singular statements; and this is the purpose for which we need the basic statements. Seeing that it would not be very easy to say in detail how a complicated theoretical system helps in the deduction of singular or basic statements, I propose the following definition. A theory is to be called 'empirical' or 'falsifiable' if it divides the class of all possible basic statements unambiguously into the following two non-empty subclasses. First, the class of all those basic statements with which it is

*1 Foundations equivalent to the one given here have been put forward as criteria of the *meaningfulness of sentences* (rather than as criteria of *demarcation* applicable to theoretical *systems*) again and again after the publication of my book, even by critics who pooh-poohed my criterion of falsifiability. But it is easily seen that, if used as a criterion of *demarcation*, our present formulation is equivalent to falsifiability. For if the basic statement b_2 does not follow from b_1, but follows from b_1 in conjunction with the theory t (this is the present formulation) then this amounts to saying that the conjunction of b_1 with the negation of b_2 contradicts the theory t. But the conjunction of b_1 with the negation of b_2 is a basic statement (cf. section 28). Thus our criterion demands the existence of a falsifying basic statement, i.e. it demands falsifiability in precisely my sense. (See also note *1 to section 82).

As a criterion of *meaning* (or of 'weak verifiability') it breaks down, however, for various reasons. First, because the negations of some meaningful statements would become meaningless, according to this criterion. Secondly, because the conjunction of a meaningful statement and a 'meaningless pseudo-sentence' would become meaningful—which is equally absurd.

If we now try to apply these two criticisms to our criterion of *demarcation*, they both prove harmless. As to the first, see section 15 above, especially note *2 (and section *22 of my *Postscript*). As to the second, empirical theories (such as Newton's) may contain 'metaphysical' elements. But these cannot be eliminated by a hard and fast rule; though if we succeed in so presenting the theory that it becomes a conjunction of a testable and a non-testable part, we know, of course, that we can now eliminate one of its metaphysical components.

The preceding paragraph of this note may be taken as illustrating another *rule of method* (cf. the end of note *5 to section 80): that after having produced some criticism of a rival theory, we should always make a serious attempt to apply this or a similar criticism to our own theory.

inconsistent (or which it rules out, or prohibits): we call this the class of the *potential falsifiers* of the theory; and secondly, the class of those basic statements which it does not contradict (or which it 'permits'). We can put this more briefly by saying: a theory is falsifiable if the class of its potential falsifiers is not empty.

It may be added that a theory makes assertions only about its potential falsifiers. (It asserts their falsity.) About the 'permitted' basic statements it says nothing. In particular, it does not say that they are true.*[2]

22 FALSIFIABILITY AND FALSIFICATION

We must clearly distinguish between falsifiability and falsification. We have introduced falsifiability solely as a criterion for the empirical character of a system of statements. As to falsification, special rules must be introduced which will determine under what conditions a system is to be regarded as falsified.

We say that a theory is falsified only if we have accepted basic statements which contradict it (cf. section 11, rule 2). This condition is necessary, but not sufficient; for we have seen that non-reproducible single occurrences are of no significance to science. Thus a few stray basic statements contradicting a theory will hardly induce us to reject it as falsified. We shall take it as falsified only if we discover a *reproducible effect* which refutes the theory. In other words, we only accept the falsification if a low-level empirical hypothesis which describes such an effect is proposed and corroborated. This kind of hypothesis may be called a *falsifying hypothesis*.[1] The requirement that the falsifying

hypothesis must be empirical, and so falsifiable, only means that it must stand in a certain logical relationship to possible basic statements; thus this requirement only concerns the logical form of the hypothesis. The rider that the hypothesis should be corroborated refers to tests which it ought to have passed—tests which confront it with accepted basic statements.*[1]

Thus the basic statements play two different rôles. On the one hand, we have used the system of all *logically possible* basic statements in order to obtain with its help the logical characterization for which we were looking—that of the form of empirical statements. On the other hand, the *accepted* basic statements are the basis for the corroboration of hypotheses. If accepted basic statements contradict a theory, then we take them as providing sufficient grounds for its falsification only if they corroborate a falsifying hypothesis at the same time.

*[2] In fact, many of the 'permitted' basic statements will, in the presence of the theory, contradict each other. (Cf. section 38.) For example, the universal law 'All planets move in circles' (i.e. 'Any set of positions of any one planet is co-circular') is trivially 'instantiated' by any set of no more than three positions of one planet; but two such 'instances' together will in most cases contradict the law.

[1] The falsifying hypothesis can be of a very low level of universality (obtained, as it were, by generalising the individual co-ordinates of a result of observation; as an instance I might cite Mach's so-called 'fact' referred to in section 18). Even though it is to be inter-subjectively testable, it need not in fact be a strictly universal statement. Thus to falsify the statement 'All ravens are black' the inter-subjectively testable statement that there is a family of white ravens in the zoo at New York would suffice. *All this shows the urgency of replacing a falsified hypothesis by a better one. In most cases we have, before falsifying a hypothesis, another one up our sleeves; for the falsifying experiment is usually a *crucial experiment* designed to decide between the two. That is to say, it is suggested by the fact that the two hypotheses differ in some respect; and it makes use of this difference to refute (at least) one of them.

*[1] This reference to accepted basic statements may seem to contain the seeds of an infinite regress. For our problem here is this. Since a hypothesis is falsified by *accepting* a basic statement, we need *methodological rules for the acceptance of basic statements*. Now if these rules in their turn refer to accepted basic statements, we may get involved in an infinite regress. To this I reply that the rules we need are merely rules for accepting basic statements that falsify a well-tested and so far successful hypothesis; and the accepted basic statements to which the rule has recourse need not be of this character. Moreover, the rule formulated in the text is far from exhaustive; it only mentions an important aspect of the acceptance of basic statements that falsify an otherwise successful hypothesis, and it will be expanded in chapter 5 (especially in section 29).

Professor J. H. Woodger, in a personal communication, has raised the question: how often has an effect to be actually reproduced in order to be a '*reproducible effect*' (or a '*discovery*')? The answer is: in some cases *not even once*. If I assert that there is a family of white ravens in the New York zoo, then I assert something which can be tested *in principle*. If somebody wishes to test it and is informed, upon arrival, that the family has died, or that it has never been heard of, it is left to him to accept or reject my falsifying basic statement. As a rule, he will have means for forming an opinion by examining witnesses, documents, etc.; that is to say, by appealing to other intersubjectively testable and reproducible facts. (Cf. sections 27 to 30.)

23 OCCURRENCES AND EVENTS

The requirement of falsifiability which was a little vague to start with has now been split into two parts. The first, the methodological postulate (cf. section 20), can hardly be made quite precise. The second, the logical criterion, is quite definite as soon as it is clear which statements are to be called 'basic' (cf. section 28). This logical criterion has so far been presented, in a somewhat formal manner, as a logical relation between statements—the theory and the basic statements. Perhaps it will make matters clearer and more intuitive if I now express my criterion in a more 'realistic' language. Although it is equivalent to the formal mode of speech, it may be a little nearer to ordinary usage.

In this 'realistic' mode of speech we can say that a singular statement (a basic statement) describes an *occurrence*. Instead of speaking of basic statements which are ruled out or prohibited by a theory, we can then say that the theory rules out certain possible occurrences, and that it will be falsified if these possible occurrences do in fact occur.

The use of this vague expression 'occurrence' is perhaps open to criticism. It has sometimes been said[1] that expressions such as 'occurrence' or 'event' should be banished altogether from epistemological discussion, and that we should not speak of 'occurrences' or 'non-occurrences', or of the 'happening' of 'events', but instead of the truth or falsity of statements. I prefer, however, to retain the expression 'occurrence'. It is easy enough to define its use so that it is unobjectionable. For we may use it in such a way that whenever we speak of an occurrence, we could speak instead of some of the singular statements which correspond to it.

When defining 'occurrence', we may remember the fact that it would be quite natural to say that two singular statements which are *logically equivalent* (i.e. mutually deducible) describe the same occurrence.

[1] Especially by some writers on probability; cf. Keynes, *A Treatise on Probability*, 1921, p. 5. Keynes refers to Ancillon as the first to propose the 'formal mode of expression'; also to Boole, Czuber, and Stumpf. *Although I still regard my ('syntactical') definitions of '*occurrence*' and '*event*', given below, as adequate *for my purpose*, I do no longer believe that they are intuitively adequate; that is, I do not believe that they adequately represent our usage, or our intentions. It was Alfred Tarski who pointed out to me (in Paris, in 1935) that a 'semantic' definition would be required instead of a 'syntactical' one.

This suggests the following definition. Let p_k be a singular statement. (The subscript 'k' refers to the individual names or coordinates which occur in p_k.) Then we call the class of all statements which are equivalent to p_k the occurrence P_k. Thus we shall say that it is an occurrence, for example, *that it is now thundering here*. And we may regard this occurrence as the class of the statements 'It is now thundering here'; 'It is thundering in the 13th District of Vienna on the 10th of June 1933 at 5.15 p.m.', and of all other statements equivalent to these. The realistic formulation 'The statement p_k represents the occurrence P_k' can then be regarded as meaning the same as the somewhat trivial statement 'The statement p_k is an element of the class P_k of all statements which are equivalent to it'. Similarly, we regard the statement 'The occurrence P_k has occurred' (or 'is occurring') as meaning the same as 'p_k and all statements equivalent to it are true'.

The purpose of these rules of translation is not to assert that whoever uses, in the realistic mode of speech, the word 'occurrence' is thinking of a class of statements; their purpose is merely to give an interpretation of the realistic mode of speech which makes intelligible what is meant by saying, for example, that an occurrence P_k contradicts a theory t. This statement will now simply mean that every statement equivalent to p_k contradicts the theory t, and is thus a potential falsifier of it.

Another term, 'event', will now be introduced, to denote what may be *typical or universal* about an occurrence, or what, in an occurrence, can be described with the help of universal names. (Thus we do not understand by an event a complex, or perhaps a protracted, occurrence, whatever ordinary usage may suggest.) We define: Let P_k, P_l, . . . be elements of a class of occurrences which differ *only* in respect of the individuals (the spatio-temporal positions or regions) involved; then we call this class 'the event (P)'. In accordance with this definition, we shall say, for example, of the statement 'A glass of water has just been upset here' that the class of statements which are equivalent to it is an element of the event, 'upsetting of a glass of water'.

Speaking of the singular statement p_k, which represents an occurrence P_k, one may say, in the realistic mode of speech, that this statement asserts the occurrence of the event (P) at the spatio-temporal position k. And we take this to mean the same as 'the class P_k, of the singular statements equivalent to p_k, is an element of the event (P)'.

We will now apply this terminology[2] to our problem. We can say of a theory, provided it is falsifiable, that it rules out, or prohibits, not merely one occurrence, but always *at least one event*. Thus the class of the prohibited basic statements, i.e. of the potential falsifiers of the theory, will always contain, if it is not empty, an unlimited number of basic statements; for a theory does not refer to individuals as such. We may call the singular basic statements which belong to *one* event 'homotypic', so as to point to the analogy between *equivalent* statements describing *one* occurrence, and *homotypic* statements describing one (typical) event. We can then say that every non-empty class of potential falsifiers of a theory contains at least one non-empty class of homotypic basic statements.

Let us now imagine that the class of all possible basic statements is represented by a circular area. The area of the circle can be regarded as representing something like the totality of *all possible worlds of experience*, or of all possible empirical worlds. Let us imagine, further, that each event is represented by one of the radii (or more precisely, by a very narrow area—or a very narrow sector—along one of the radii) and that any two occurrences involving the same co-ordinates (or individuals) are located at the same distance from the centre, and thus on the same concentric circle. Then we can illustrate the postulate of falsifiability by the requirement that for every empirical theory there must be at least *one* radius (or very narrow sector) in our diagram which the theory forbids.

This illustration may prove helpful in the discussion of our various problems,*[1] such as that of the metaphysical character of purely existential statements (briefly referred to in section 15). Clearly, to each of these statements there will belong one event (one radius) such that the

[2] It is to be noted that although singular statements *represent* occurrences, universal statements do not represent events: they *exclude* them. Similarly to the concept of 'occurrence', a 'uniformity' or 'regularity' can be defined by saying that universal statements *represent* uniformities. But here we do not need any such concept, seeing that we are only interested in what universal statements *exclude*. For this reason such questions as whether uniformities (universal 'states of affairs' etc.) exist, do not concern us. *But such questions are discussed in section 79, and now also in appendix *x, and in section *15 of the *Postscript*.

*[1] The illustration will be used, more especially, in sections 31 ff., below.

various basic statements belonging to this event will each verify the purely existential statement. Nevertheless, the class of its potential falsifiers is empty; so from the existential statement *nothing follows* about the possible worlds of experience. (It excludes or forbids none of the radii.) The fact that, conversely, from every basic statement a purely existential statement follows, cannot be used as an argument in support of the latter's empirical character. For every tautology also follows from every basic statement, since it follows from any statement whatsoever.

At this point I may perhaps say a word about self-contradictory statements.

Whilst tautologies, purely existential statements and other nonfalsifiable statements assert, as it were, *too little* about the class of possible basic statements, self-contradictory statements assert *too much*. From a self-contradictory statement, any statement whatsoever can be validly deduced.*[2] Consequently, the class of its potential falsifiers is identical

*[2] This fact was even ten years after publication of this book not yet generally understood. The situation can be summed up as follows: a factually false statement 'materially implies' every statement (but it does not logically entail every statement). A logically false statement logically implies—or entails—every statement. It is therefore of course essential to distinguish clearly between a merely *factually false* (synthetic) statement and a *logically false* or *inconsistent* or *self-contradictory* statement; that is to say, one from which a statement of the form $p \cdot \bar{p}$ can be deduced.

That an inconsistent statement entails every statement can be shown as follows:

From Russell's 'primitive propositions' we get at once

(1) $$p \rightarrow (p \vee q)$$

and further, by substituting here first '$\bar{p}$' for 'p', and then '$p \rightarrow q$' for '$\bar{p} \vee q$' we get

(2) $$\bar{p} \rightarrow (p \rightarrow q),$$

which yields, by 'importation',

(3) $$\bar{p} \cdot p \rightarrow q$$

But (3) allows us to deduce, using the *modus ponens*, *any* statement q from any statement of the form '$\bar{p} \cdot p$', or '$p \cdot \bar{p}$'. (See also my note in *Mind* **52**, 1943, pp. 47 ff.) The fact that everything is deducible from an inconsistent set of premises is rightly treated as well known by P. P. Wiener (*The Philosophy of Bertrand Russell*, edited by P. A. Schilpp, 1944, p. 264); but surprisingly enough, Russell challenged this fact in his reply to Wiener (*op. cit.*, pp. 695 f.), speaking however of '*false* propositions' where Wiener spoke of '*inconsistent* premises'. Cf. my *Conjectures and Refutations*, 1963, 1965, pp. 317 ff.

with that of all possible basic statements: it is falsified by any statement whatsoever. (One could perhaps say that this fact illustrates an advantage of our method, *i.e.* of our way of considering possible falsifiers rather than possible verifiers. For if one could verify a statement by the verification of its logical consequences, or merely make it probable in this way, then one would expect that, by the acceptance of any basic statement whatsoever, any self-contradictory statements would become confirmed, or verified, or at least probable.)

24 FALSIFIABILITY AND CONSISTENCY

The requirement of consistency plays a special rôle among the various requirements which a theoretical system, or an axiomatic system, must satisfy. It can be regarded as the first of the requirements to be satisfied by *every* theoretical system, be it empirical or non-empirical.

In order to show the fundamental importance of this requirement it is not enough to mention the obvious fact that a self-contradictory system must be rejected because it is 'false'. We frequently work with statements which, although actually false, nevertheless yield results which are adequate for certain purposes.*[1] (An example is Nernst's approximation for the equilibrium equation of gases.) But the importance of the requirement of consistency will be appreciated if one realizes that a self-contradictory system is uninformative. It is so because any conclusion we please can be derived from it. Thus no statement is singled out, either as incompatible or as derivable, since all are derivable. A consistent system, on the other hand, divides the set of all possible statements into two: those which it contradicts and those with which it is compatible. (Among the latter are the conclusions which can be derived from it.) This is why consistency is the most general requirement for a system, whether empirical or non-empirical, if it is to be of any use at all.

Besides being consistent, an empirical system should satisfy a further condition: it must be *falsifiable*. The two conditions are to a large extent analogous.[1] Statements which do not satisfy the condition of consistency fail to differentiate between any two statements within the totality of all possible statements. Statements which do not satisfy the condition of falsifiability fail to differentiate between any two statements within the totality of all possible empirical basic statements.

*[1] Cf. my *Postscript*, section *3 (my reply to the 'second proposal'); and section *12, point (2).

[1] Cf. my note in *Erkenntnis* 3, 1933, p. 426. *This is now printed in appendix *i, below.

10

CORROBORATION, OR HOW A THEORY STANDS UP TO TESTS

Theories are not verifiable, but they can be 'corroborated'.

The attempt has often been made to describe theories as being neither *true* nor *false*, but instead more or less *probable*. Inductive logic, more especially, has been developed as a logic which may ascribe not only the two values 'true' and 'false' to statements, but also degrees of probability; a type of logic which will here be called '*probability logic*'. According to those who believe in probability logic, induction should determine the degree of probability of a statement. And a principle of induction should either *make it sure* that the induced statement is 'probably valid' or else it should *make it probable*, in its turn—for the principle of induction might itself be only 'probably valid'. Yet in my view, the whole problem of the probability of hypotheses is misconceived. Instead of discussing the 'probability' of a hypothesis we should try to assess what tests, what trials, it has withstood; that is, we should try to assess how far it has been able to prove its fitness to survive by standing up to tests. In brief, we should try to assess how far it has been 'corroborated'.*[1]

*[1] I introduced the terms '*corroboration*' ('*Bewährung*') and especially '*degree of corroboration*' ('*Grad der Bewährung*', '*Bewährungsgrad*') in my book because I wanted a *neutral* term to describe the degree to which a hypothesis has stood up to severe tests, and thus 'proved its mettle'. By 'neutral' I mean a term not prejudging the issue whether, by standing up to tests, the hypothesis becomes 'more probable', in the sense of the probability calculus. In other words, I introduced the term 'degree of corroboration' mainly in order to be able to discuss the problem whether or not 'degree of corroboration' could be indentified with 'probability' (either in a frequency sense or in the sense of Keynes, for example).

Carnap translated my term 'degree of corroboration' ('*Grad der Bewährung*'), which I had first introduced into the discussions of the Vienna Circle, as 'degree of confirmation'. (See his 'Testability and Meaning', in *Philosophy of Science* **3**, 1936; especially p. 427); and so the term 'degree of confirmation' soon became widely accepted. I did not like this term, because of some of its associations ('make firm'; 'establish firmly'; 'put beyond doubt'; 'prove'; 'verify': 'to confirm' corresponds more closely to '*erhärten*' or '*bestätigen*' than to '*bewähren*'). I therefore proposed in a letter to Carnap (written, I think, about 1939) to use the term 'corroboration'. (This term had been suggested to me by Professor H. N. Parton.) But as Carnap declined my proposal, I fell in with his usage, thinking that words do not matter. This is why I myself used the term 'confirmation' for a time in a number of my publications.

Yet it turned out that I was mistaken: the associations of the word 'confirmation' did matter, unfortunately, and made themselves felt: 'degree of confirmation' was soon used—by Carnap himself—as a synonym (or 'explicans') of 'probability'. I have therefore now abandoned it in favour of 'degree of corroboration'. See also appendix *ix, and section *29 of my *Postscript*.

79 CONCERNING THE SO-CALLED VERIFICATION OF HYPOTHESES

The fact that theories are not verifiable has often been overlooked. People often say of a theory that it is verified when some of the predictions derived from it have been verified. They may perhaps admit that the verification is not completely impeccable from a logical point of view, or that a statement can never be finally established by establishing some of its consequences. But they are apt to look upon such objections as due to somewhat unnecessary scruples. It is quite true, they say, and even trivial, that we cannot know for certain whether the sun will rise tomorrow; but this uncertainty may be neglected: the fact that theories may not only be improved but that they can also be *falsified by new experiments* presents to the scientist a serious possibility which may at any moment become actual; but never yet has a theory had to be regarded as falsified owing to the sudden breakdown of a well-confirmed law. It never happens that old experiments one day yield new results. What happens is only that new experiments decide against an old theory. The old theory, even when it is superseded, often retains its validity as a kind of limiting case of the new theory; it still applies, at least with a high degree of approximation, in those cases in which it was successful before. In short, regularities which are directly testable by experiment do not change. Admittedly it is conceivable, or logically possible, that they might change; but this possibility is disregarded by empirical science and does not affect its methods. On the contrary, scientific method presupposes *the immutability of natural processes*, or the 'principle of the uniformity of nature'.

There is something to be said for the above argument, but it does not affect my thesis. It expresses the metaphysical faith in the existence of regularities in our world (a faith which I share, and without which practical action is hardly conceivable).*[1] Yet the question before us—the question which makes the non-verifiability of theories significant in the present context—is on an altogether different plane. Consistently with my attitude towards other metaphysical questions, I abstain from arguing for or against faith in the existence of regularities in our world. But I shall try to show that *the non-verifiability of theories is methodologically important*. It is on this plane that I oppose the argument just advanced.

I shall therefore take up as relevant only one of the points of this argument—the reference to the so-called 'principle of the uniformity of nature'. This principle, it seems to me, expresses in a very superficial way an important methodological rule, and one which might be derived, with advantage, precisely from a consideration of the non-verifiability of theories.*[2]

Let us suppose that the sun will not rise tomorrow (and that we shall nevertheless continue to live, and also to pursue our scientific interests). Should such a thing occur, science would have to try to *explain* it, i.e. to derive it from laws. Existing theories would presumably require to be drastically revised. But the revised theories would not merely have to account for the new state of affairs: *our older experiences would also have to be derivable from them*. From the methodological point of view one sees that

*[1] Cf. appendix *x, and also section *15 of my *Postscript*.

*[2] I mean the rule that any new system of hypotheses should yield, or explain, the old, corroborated, regularities. See also section *3 (third paragraph) of my *Postscript*.

the principle of the uniformity of nature is here replaced by the postulate of *the invariance of natural laws*, with respect to both space and time. I think, therefore, that it would be a mistake to assert that natural regularities do not change. (This would be a kind of statement that can neither be argued against nor argued for.) What we should say is, rather, that it is part of our *definition* of natural laws if we postulate that they are to be invariant with respect to space and time; and also if we postulate that they are to have no exceptions. Thus from a methodological point of view, the possibility of falsifying a corroborated law is by no means without significance. It helps us to find out what we demand and expect from natural laws. And the 'principle of the uniformity of nature' can again be regarded as a metaphysical interpretation of a methodological rule—like its near relative, the 'law of causality'.

One attempt to replace metaphysical statements of this kind by principles of method leads to the 'principle of induction', supposed to govern the method of induction, and hence that of the verification of theories. But this attempt fails, for the principle of induction is itself metaphysical in character. As I have pointed out in section 1, the assumption that the principle of induction is empirical leads to an infinite regress. It could therefore only be introduced as a primitive proposition (or a postulate, or an axiom). This would perhaps not matter so much, were it not that the principle of induction would have in any case to be treated as a *non-falsifiable statement*. For if this principle—which is supposed to validate the inference of theories—were itself falsifiable, then it would be falsified with the first falsified theory, because this theory would then be a conclusion, derived with the help of the principle of induction; and this principle, as a premise, will of course be falsified by the *modus tollens* whenever a theory is falsified which was derived from it.*[3] But this means that a falsifiable principle of induction would be falsified anew with every advance made by science. It would be necessary, therefore, to introduce a principle of induction assumed not to be falsifiable. But this would amount to the

*[3] The premises of the derivation of the theory would (according to the inductivist view here discussed) consist of the principle of induction *and* of observation statements. But the latter are here tacitly assumed to be unshaken and reproducible, so that they cannot be made responsible for the failure of the theory.

misconceived notion of a synthetic statement which is *a priori* valid, i.e. an irrefutable statement about reality.

Thus if we try to turn our metaphysical faith in the uniformity of nature and in the verifiability of theories into a theory of knowledge based on inductive logic, we are left only with the choice between an infinite regress and *apriorism*.

80 THE PROBABILITY OF A HYPOTHESIS AND THE PROBABILITY OF EVENTS: CRITICISM OF PROBABILITY LOGIC

Even if it is admitted that theories are never finally verified, may we not succeed in making them secure to a greater or lesser extent—more probable, or less so? After all, it might be possible that the question of *the probability of a hypothesis* could be reduced, say, to that of *the probability of events*, and thus be made susceptible to mathematical and logical handling.*[1]

Like inductive logic in general, the theory of the probability of hypotheses seems to have arisen through a confusion of psychological with logical questions. Admittedly, our subjective feelings of conviction are of different intensities, and the degree of confidence with which we await the fulfilment of a prediction and the further corroboration of a hypothesis is likely to depend, among other things, upon the way in which this hypothesis has stood up to tests so far—upon its past corroboration. But that these psychological questions do not belong to epistemology or methodology is pretty well acknowledged even by the believers in probability logic. They argue, however, that it is possible, on the basis of inductivist decisions, to ascribe degrees of probability to *the hypotheses themselves*; and further, that it is possible to reduce this concept to that of the probability of events.

The probability of a hypothesis is mostly regarded as merely a special case of the general problem of the *probability of a statement*; and this in turn is regarded as nothing but the problem of the *probability of an event*, expressed in a particular terminology. Thus we read in Reichenbach, for example: 'Whether we ascribe probability to statements or to events is only a matter of terminology. So far we have regarded it as a case of the probability of events that the probability of 1/6 has been assigned to the turning up of a certain face of a die. But we might just as well say that it is the *statement* "the face showing the I will turn up" which has been assigned the probability of 1/6.'[1]

This identification of the probability of events with the probability of statements may be better understood if we recall what was said in section 23. There the concept 'event' was defined as a class of singular statements. It must therefore also be permissible to speak of the *probability of statements* in place of the probability of events. So we can regard this as being merely a change of terminology: the reference-sequences are interpreted as sequences of statements. If we think of an 'alternative', or rather of its elements, as represented by statements, then we can describe the turning up of heads by the statement '*k* is heads', and its failure to turn up by the negation of this statement. In this way we obtain a sequence of statements of the form $p_j, p_k, \bar{p}_l, p_m, \bar{p}_n, \ldots$, in which a statement p_i is sometimes characterized as 'true', and sometimes (by placing a bar over its name) as 'false'. Probability within an alternative can thus be interpreted as *the relative 'truth-frequency'*[2] *of statements within a sequence of statements* (rather than as the relative frequency of a property).

If we like, we can call the concept of probability, so transformed, the 'probability of statements' or the 'probability of propositions'. And we can show a very close connection between this concept and the concept of 'truth'. For if the sequence of statements becomes shorter and shorter and in the end contains only one element, i.e. only *one single* statement, then the probability, or truth-frequency, of the sequence can assume only one of the two values 1 and 0, according to whether the single statement is true or false. The truth or falsity of a statement can thus be looked upon as a limiting case of probability; and conversely, probability can be regarded as a generalization of the concept

*[1] The present section (80) contains mainly a criticism of Reichenbach's attempt to interpret *the probability of hypotheses* in terms of *a frequency theory of the probability of events*. A criticism of Keynes's approach is contained in section 83. *Note that Reichenbach is anxious to reduce *the probability of a statement or hypothesis* (what Carnap many years later called '*probability*[1]') to a frequency ('*probability*[2]').

[1] Reichenbach, *Erkenntnis* 1, 1930, pp. 171 f.

[2] According to Keynes, *A Treatise on Probability*, 1921, p. 101 ff., the expression 'truth-frequency' is due to Whitehead; cf. the next note.

of truth, in so far as it includes the latter as a limiting case. Finally, it is possible to define operations with truth-frequencies in such a way that the usual truth-operations of classical logic become limiting cases of these operations. And the calculus of these operations can be called '*probability logic*'.[3]

But can we really identify *the probability of hypotheses* with the probability of statements, defined in this manner, and thus indirectly with the probability of events? I believe that this identification is the result of a confusion. The idea is that the probability of a hypothesis, since it is obviously a kind of probability of a statement, must come under the head of 'probability of statements' *in the sense just defined*. But this conclusion turns out to be unwarranted; and the terminology is thus highly unsuitable. Perhaps after all it would be better never to use the expression 'probability of statements' if we have the probability of events in mind.*[2]

However this may be, I assert that the issues arising from the concept of a *probability of hypotheses* are not even touched by considerations based on probability logic. I assert that if one says of a hypothesis that it is not true but 'probable', then this statement can under *no* circumstances be translated into a statement about the probability of events.

For if one attempts to reduce the idea of a probability of hypotheses to that of a truth-frequency which uses the concept of a sequence of statements, then one is at once confronted with the question: *with reference to what sequence* of statements can a probability value be assigned

[3] I am giving here an outline of the construction of the probability logic developed by Reichenbach (*Wahrscheinlichkeitslogik*, *Sitzungsberichte der Preussischen Akademie der Wissenschaften*, Physik-mathem. Klasse **29**, 1932, p. 476 ff.) who follows E. L. Post (*American Journal of Mathematics* **43**, 1921, p. 184), and, at the same time, the frequency theory of von Mises. Whitehead's form of the frequency theory, discussed by Keynes, *op. cit.* p. 101 ff. is similar.

*[2] I still think (a) that the so-called 'probability of hypotheses' cannot be interpreted by a truth-frequency; (b) that it is better to call a probability defined by a relative frequency—whether a truth-frequency or the frequency of an event—the 'probability of an event'; (c) that the so-called 'probability of a hypothesis' (in the sense of its acceptability) is *not* a special case of the 'probability of statements'. And I should now regard the 'probability of statements' as one interpretation (the logical interpretation) among several possible interpretations of the formal calculus of probability, rather than as a truth-frequency. (Cf. appendices *ii, *iv, and *ix, and my *Postscript*.)

to a hypothesis? Reichenbach identifies an 'assertion of natural science'—by which he means a scientific hypothesis—itself with a reference-sequence of statements. He says, '. . . the assertions of natural science, which are never singular statements, are in fact sequences of statements to which, strictly speaking, we must assign not the degree of probability I but a smaller probability value. It is therefore only probability logic which provides the logical form capable of strictly representing the concept of knowledge proper to natural science.'[4] Let us now try to follow up the suggestion that the hypotheses themselves are sequences of statements. One way of interpreting it would be to take, as the elements of such a sequence, the various singular statements which can contradict, or agree with, the hypothesis. The probability of this hypothesis would then be determined by the truth-frequency of those among these statements which agree with it. But this would give the hypothesis a probability of $\frac{1}{2}$ if, on the average, it is refuted by every second singular statement of this sequence! In order to escape from this devastating conclusion, we might try two more expedients.*[3] One would be to ascribe to the hypothesis a certain probability—perhaps not a very precise one—on the basis of an estimate of the ratio of all the tests passed by it to all the tests which have not yet been attempted. But this way too leads nowhere. For this estimate can, as it happens, be computed with precision, and the result is always that the probability is zero. And finally, we could try to base our estimate upon the ratio of those tests which led to a favourable result to those which led to an indifferent result—*i.e.* one which did not produce a clear decision. (In this way one might indeed obtain something resembling a measure of the subjective feeling of confidence with which the experimenter views his results.) But this last expedient will not do either, even if we disregard the fact that with this kind of estimate we have strayed a long way from the concept of a truth-frequency, and that of a probability of events. (These concepts are based upon the ratio of the true statements to those which are false,

[4] Reichenbach, *Wahrscheinlichkeitslogik* (*op. cit.* p. 488), p. 15 of the reprint.

*[3] It is here assumed that we have by now made up our minds that whenever there is a clear-cut falsification, we will attribute to the hypothesis the probability zero, so that the discussion is now confined to those cases in which no clear-cut falsification has been obtained.

and we must not, of course, equate an indifferent statement with one that is objectively false.) The reason why this last attempt fails too is that the suggested definition would make the probability of a hypothesis hopelessly subjective: the probability of a hypothesis would depend upon the training and skill of the experimenter rather than upon objectively reproducible and testable results.

But I think it is altogether impossible to accept the suggestion that a hypothesis can be taken to be a sequence of statements. It would be possible if universal statements had the form: 'For every value hf *k* it is true that at the place *k* so-and-so occurs.' If universal statements had this form, then we could regard basic statements (those that contradict, or agree with, the universal statement) as elements of a sequence of statements—the sequence to be taken for the universal statement. But as we have seen (cf. sections 15 and 28), universal statements do not have this form. Basic statements are never derivable from universal statements alone.*4 The latter cannot therefore be regarded as sequences of basic statements. If, however, we try to take into consideration the sequence of those negations of basic statements which *are* derivable from universal statements, then the estimate for *every* self-consistent hypothesis will lead to the same probability, namely 1. For we should then have to consider the ratio of the *non-falsified* negated basic statements which can be derived (or other derivable statements) to the *falsified* ones. This means that instead of considering a truth frequency we should have to consider the complementary value of a falsity frequency. This value however would be equal to 1. For the class of derivable statements, and even the class of the derivable negations of basic statements, are both infinite; on the other hand, there cannot be more than at most a finite number of accepted falsifying basic statements. Thus even if we disregard the fact that universal statements are never sequences of statements, and even if we try to interpret them as something of the kind and to correlate with them sequences of completely decidable singular statements, even then we do not reach an acceptable result.

We have yet to examine another, quite different, possibility of explaining the probability of a hypothesis in terms of sequences of statements. It may be remembered that we have called a given singular occurrence 'probable' (in the sense of a 'formally singular probability statement') if it is an *element of a sequence* of occurrences with a certain probability. Similarly one might try to call a hypothesis 'probable' if it is an *element of a sequence of hypotheses* with a definite truth-frequency. But this attempt again fails—quite apart from the difficulty of determining the reference sequence (it can be chosen in many ways; cf. section 71). For we cannot speak of a truth-frequency within a sequence of hypotheses, simply because we can never know of a hypothesis whether it is true. If we *could* know this, then we should hardly need the concept of the probability of a hypothesis at all. Now we might try, as above, to take the complement of the falsity-frequency within a sequence of hypotheses as our starting point. But if, say, we define the probability of a hypothesis with the help of the ratio of the non-falsified to the falsified hypotheses of the sequence, then, as before, the probability of *every* hypothesis within *every infinite* reference sequence will be equal to 1. And even if a *finite* reference sequence were chosen we should be in no better position. For let us assume that we can ascribe to the elements of some (*finite*) sequence of hypotheses a degree of probability between 0 and 1 in accordance with this procedure—say, the value 3/4. (This can be done if we obtain the information that this or that hypothesis belonging to the sequence has been falsified.) In so far as these *falsified* hypotheses are elements of the sequence, we thus would have to ascribe to them, *just because of this information*, not the value o, but 3/4. And in general, the probability of a hypothesis would decrease by $1/n$ in consequence of the information that it is false, where n is the number of hypothesis in the reference sequence. All this glaringly contradicts the programme of expressing, in terms of a '*probability of hypotheses*', the degree of reliability which we have to ascribe to a hypothesis in view of supporting or undermining evidence.

*4 As explained in section 28 above, the singular statements which *can* be deduced from a theory—the 'instantial statements'—are not of the character of basic statements or of observation statements. If we nevertheless decide to take the sequence of these statements and base our probability upon the truth frequency within this sequence, then the probability will be always equal to 1, however often the theory may be falsified; for as has been shown in section 28, note *1, almost any theory is 'verified' by almost all instances (i.e. by almost all places *k*). The discussion following here in the text contains a very similar argument—also based upon 'instantial statements' (i.e. negated basic statements)—designed to show that the probability of a hypothesis, if based upon these negated basic statements, would always be equal to one.

This seems to me to exhaust the possibilities of basing the concept of the probability of a hypothesis on that of the frequency of true statements (or the frequency of false ones), and thereby on the frequency theory of the probability of events.*5

I think we have to regard the attempt to identify the probability of a

*5 One might summarize my foregoing attempts to make sense of Reichenbach's somewhat cryptic assertion that the probability of a hypothesis is to be measured by a truth frequency, as follows. (For a similar summary, with criticism, see the penultimate paragraph of appendix *i.)

Roughly, we can try two possible ways of defining the probability of a theory. One is to count the number of experimentally testable statements belonging to the theory, and to determine the relative frequency of those which turn out to be true; this relative frequency can then be taken as a measure of the probability of a theory. We may call this *a probability of the first kind*. Secondly, we can consider the theory as an element of a class of ideological entities—say, of theories proposed by other scientists—and we can then determine the relative frequencies within this class. We may call this *a probability of the second kind*.

In my text I tried, further, to show that each of these two possibilities of making sense of Reichenbach's idea of truth frequency leads to results which must be quite unacceptable to adherents of the probability theory of induction.

Reichenbach replied to my criticism, not so much by defending his views as by attacking mine. In his paper on my book (*Erkenntnis* **5**, 1935, pp. 267–284), he said that 'the results of this book are completely untenable', and explained this by a failure of my 'method'—by my failure 'to think out all the consequences' of my conceptual system.

Section iv of his paper (pp. 274 f.) is devoted to our problem—the probability of hypotheses. It begins: 'In this connection, some remarks may be added about the probability of theories—remarks which should render more complete my so far all too brief communications of the subject, and which may remove a certain obscurity which still surrounds the issue.' After this follows a passage which forms the second paragraph of the present note, headed by the word 'Roughly' (the only word which I have added to Reichenbach's text).

Reichenbach remained silent about the fact that his attempt to remove 'the obscurity which still surrounds the issue' is but a summary—a rough one, admittedly—of some pages of the very book which he is attacking. Yet in spite of this silence I feel that I may take it as a great compliment from so experienced a writer on probability (who at the time of writing his reply to my book had two books and about a dozen papers on the subject to his credit) that he did accept the results of my endeavours to 'think out the consequences' of his 'all too brief communications on the subject'. This success of my endeavours was due, I believe, to a rule of 'method': that we should always try to clarify and to strengthen our opponent's position as much as possible before criticizing him, if we wish our criticism to be worth while.

hypothesis with the probability of events as a complete failure. This conclusion is quite independent of whether we accept the claim (it is Reichenbach's) that *all hypotheses of physics* are 'in reality', or 'on closer examination' nothing but probability statements (about some average frequencies within sequences of observations which always show deviations from some mean value), or whether we are inclined to make a distinction between two different *types* of natural laws—between the 'deterministic' or 'precision' laws on the one hand, and the 'probability laws' or 'hypotheses of frequency' on the other. For both of these types are hypothetical assumptions which in their turn can never become 'probable': they can only be corroborated, in the sense that they can 'prove their mettle' under fire—the fire of our tests.

How are we to explain the fact that the believers in probability logic have reached an opposite view? Wherein lies the error made by Jeans when he writes—at first in a sense with which I can fully agree—that '. . . we can know nothing . . . *for certain*', but then goes on to say: 'At best we can only deal in *probabilities*. [And] the predictions of the new quantum theory agree so well [with the observations] that the odds in favour of the scheme having some correspondence with reality are *enormous*. Indeed, we may say the scheme is *almost certain* to be quantitatively true . . .'?[5]

Undoubtedly the commonest error consists in believing that hypothetical estimates of frequencies, that is to say, hypotheses regarding probabilities, can in their turn be only probable; or in other words, in ascribing to *hypotheses of probability* some degree of an alleged *probability of hypotheses*. We may be able to produce a persuasive argument in favour of this erroneous conclusion if we remember that hypotheses regarding probabilities are, as far as their logical form is concerned (and without reference to our methodological requirement of falsifiability), neither verifiable nor falsifiable. (Cf. sections 65 to 68.) They are not verifiable because they are universal statements, and they are not strictly falsifiable because they can never be logically contradicted by any basic statements. They are thus (as Reichenbach puts it) *completely*

[5] Jeans, *The New Background of Science*, 1934, p. 58. (Only the words 'for certain' are italicized by Jeans.)

undecidable.[6] Now they can, as I have tried to show, *be better, or less well*, '*confirmed*', which is to say that they may agree more, or less, with accepted basic statements. This is the point where, it may appear, probability logic comes in. The symmetry between verifiability and falsifiability accepted by classical inductivist logic suggests the belief that it must be possible to correlate with these 'undecidable' probability statements some scale of degrees of validity, something like 'continuous degrees of probability whose unattainable upper and lower limits are truth and falsity',[7] to quote Reichenbach again. According to my view, however, probability statements, just because they are completely undecidable, are *metaphysical* unless we decide to make them falsifiable by accepting a methodological rule. Thus the simple result of their non-falsifiability is not that they can be better, or less well corroborated, but *that they cannot be empirically corroborated at all*. For otherwise—seeing that they rule out nothing, and are therefore compatible with every basic statement—they could be said to be 'corroborated' by *every arbitrarily chosen basic statement* (of any degree of composition) provided it describes the occurrence of some relevant instance.

I believe that physics uses probability statements only in the way which I have discussed at length in connection with the theory of probability; and more particularly that it uses probability assumptions, just like other hypotheses, as falsifiable statements. But I should decline to join in any dispute about how physicists 'in fact' proceed, since this must remain largely a matter of interpretation.

We have here quite a nice illustration of the contrast between my view and what I called, in section 10, the 'naturalistic' view. What can be shown is, first, the internal logical consistency of my view, and secondly, that it is free from those difficulties which beset other views. Admittedly it is impossible to prove that my view is correct, and a controversy with upholders of another logic of science may well be futile. All that can be shown is that my approach to this particular

[6] Reichenbach, *Erkenntnis* **1**, 1930, p. 169 (cf. also Reichenbach's reply to my note in *Erkenntnis* **3**, 1933, pp. 426 f.). Similar ideas about the degrees of probability or certainty of inductive knowledge occur very frequently (cf. for instance Russell, *Our Knowledge of the External World*, 1914, pp. 225 f., and *The Analysis of Matter*, 1927, pp. 141 and 398).

[7] Reichenbach, *Erkenntnis* 1, 1930, p. 186 (cf. note 4 to section 1).

problem is a consequence of the conception of science for which I have been arguing.*6

81 INDUCTIVE LOGIC AND PROBABILITY LOGIC

The probability of hypotheses cannot be reduced to the probability of events. This is the conclusion which emerges from the examination carried out in the previous section. But might not a different approach lead to a satisfactory definition of the idea of a *probability of hypotheses*?

I do not believe that it is possible to construct a concept of the probability of hypotheses which may be interpreted as expressing a 'degree of validity' of the hypothesis, in analogy to the concepts 'true' and 'false' (and which, in addition, is sufficiently closely related to the concept 'objective probability', *i.e.* to relative frequency, to justify the use of the word 'probability').[1] Nevertheless, I will now, for the sake of argument, adopt the *supposition* that such a concept has in fact been successfully constructed, in order to raise the question: how would this affect the problem of induction?

Let us suppose that a certain hypothesis—say Schrödinger's theory—is recognized as 'probable' in some definite sense; either as 'probable to this or that numerical degree', or merely as 'probable', without specification of a degree. The statement that describes Schrödinger's theory as 'probable' we may call its *appraisal*.

*6 The last two paragraphs were provoked by the 'naturalistic' approach sometimes adopted by Reichenbach, Neurath, and others; cf. section 10, above.

[1] (Added while the book was in proof.) It is conceivable that for estimating degrees of corroboration, one might find a formal system showing some limited formal analogies with the calculus of probability (*e.g.* with Bayes's theorem), without however having anything in common with the frequency theory. I am indebted to Dr. J. Hosiasson for suggesting this possibility to me. I am satisfied, however, that it is quite impossible to tackle *the problem of induction* by such methods with any hope of success. *See also note 3 to section *57 of my *Postscript*.

* Since 1938, I have upheld the view that 'to justify the use of the word probability', as my text puts it, we should have to show that the axioms of the formal calculus are satisfied. (Cf. appendices *ii to *v, and especially section *28 of my *Postscript*.) This would of course include the satisfaction of Bayes's theorem. As to the formal analogies between Bayes's theorem on *probability* and certain theorems on *degree of corroboration*, see appendix *ix, point 9 (vii) of the first note, and points (12) and (13) of section *32 of my *Postscript*.

An appraisal must, of course, be a synthetic statement—an assertion about 'reality'—in the same way as would be the statement 'Schrödinger's theory is true' or 'Schrödinger's theory is false'. All such statements obviously say something about the adequacy of the theory, and are thus certainly not tautological.*[1] They say that a theory is adequate or inadequate, or that it is adequate in some degree. Further, an appraisal of Schrödinger's theory must be a *non-verifiable* synthetic statement, just like the theory itself. For the 'probability' of a theory—that is, the probability that the theory will remain acceptable—cannot, it appears, be deduced from basic statements *with finality*. Therefore we are forced to ask: How can the appraisal be justified? How can it be tested? (Thus the problem of induction arises again; see section 1.)

As to the appraisal itself, this may either be asserted to be 'true', or it may, in its turn, be said to be 'probable'. If it is regarded as 'true' then it must be a *true synthetic statement* which has not been empirically verified—a synthetic statement which is *a priori* true. If it is regarded as

*[1] The probability statement '$p(S,e) = r$', in words, 'Schrödinger's theory, given the evidence e, has the probability r'—a statement of relative or conditional logical probability—may certainly be tautological (provided the values of e and r are chosen so as to fit each other: if e consists only of observational reports, r will have to equal zero in a sufficiently large universe). But the 'appraisal', in our sense, would have a different form (see section 84, below, especially the text to note *2)—for example, the following: $P_k(S) = r$, where k is today's date; or in words: 'Schrödinger's theory has *today* (in view of the actual total evidence now available) a probability of r.' In order to obtain this assessment, $p_k(S) = r$, from (i) the tautological statement of relative probability $p(S,e) = r$, and (ii) the statement 'e is the total evidence available today', we must apply a *principle of inference* (called the 'rule of absolution' in my *Postscript*, sections *43 and *51). This principle of inference looks very much like the *modus ponens*, and it may therefore seem that it should be taken as analytic. But if we take it to be analytic, then this amounts to the decision to consider p_k as *defined* by (i) and (ii), or at any rate as meaning *no more* than do (i) and (ii) together; but in this case, p_k cannot be interpreted as being of any practical significance: it *certainly* cannot be interpreted as a practical measure of acceptability. This is best seen if we consider that in a sufficiently large universe, $p_k(t,e) \approx 0$ for *every* universal theory t, provided e consists only of singular statements. (Cf. appendices, *vii and *viii.) But in practice, we certainly do accept some theories and reject others.

If, on the other hand, we interpret p_k as *degree of adequacy or acceptability*, then the principle of inference mentioned—the 'rule of absolution' (which, on this interpretation, becomes a typical example of a 'principle of induction')—is simply *false*, and therefore clearly non-analytic.

'probable', then we need a *new* appraisal: an appraisal of the appraisal, as it were, and therefore an appraisal on a higher level. But this means that we are caught up in an infinite regress. The appeal to the probability of the hypothesis is unable to improve the precarious logical situation of inductive logic.

Most of those who believe in probability logic uphold the view that the appraisal is arrived at by means of a 'principle of induction' which ascribes probabilities to the induced hypotheses. But if they ascribe a probability to this principle of induction in its turn, then the infinite regress continues. If on the other hand they ascribe 'truth' to it then they are left with the choice between infinite regress and *a priorism*. 'Once and for all', says Heymans, 'the theory of probability is incapable of explaining inductive arguments; for precisely the same problem which lurks in the one also lurks in the other (in the empirical application of probability theory). In both cases the conclusion goes beyond what is given in the premises.'[2] Thus nothing is gained by replacing the word 'true' by the word 'probable', and the word 'false' by the word 'improbable'. Only if *the asymmetry between verification and falsification* is taken into account—that asymmetry which results from the logical relation between theories and basic statements—is it possible to avoid the pitfalls of the problem of induction.

Believers in probability logic may try to meet my criticism by asserting that it springs from a mentality which is 'tied to the frame-work of classical logic', and which is therefore incapable of following the methods of reasoning employed by probability logic. I freely admit that I am incapable of following these methods of reasoning.

[2] Heymans, *Gesetze und Elemente des wissenschaftlichen Denkens* (1890, 1894), pp. 290 f.; *third edition, 1915, p. 272. Heymans's argument was anticipated by Hume in his anonymous pamphlet. *An Abstract of a Book lately published entitled A Treatise of Human Nature*, 1740. I have little doubt that Heymans did not know this pamphlet which was re-discovered and attributed to Hume by J. M. Keynes and P. Sraffa, and published by them in 1938. I knew neither of Hume's nor of Heymans's anticipation of my arguments against the probabilistic theory of induction when I presented them in 1931 in an earlier book, still unpublished, which was read by several members of the Vienna Circle. The fact that Heymans's passage had been anticipated by Hume was pointed out to me by J. O. Wisdom; cf. his *Foundations of Inference in Natural Science*, 1952, p. 218. Hume's passage is quoted below, in appendix *vii, text to footnote 6 (p. 386).

82 THE POSITIVE THEORY OF CORROBORATION: HOW A HYPOTHESIS MAY 'PROVE ITS METTLE'

Cannot the objections I have just been advancing against the probability theory of induction be turned, perhaps, against my own view? It might well seem that they can; for these objections are based on the idea of an *appraisal*. And clearly, I have to use this idea too. I speak of the '*corroboration*' of a theory; and corroboration can only be expressed as an appraisal. (In this respect there is no difference between corroboration and probability.) Moreover, I too hold that hypotheses cannot be asserted to be 'true' statements, but that they are 'provisional conjectures' (or something of the sort); and this view, too, can only be expressed by way of an appraisal of these hypotheses.

The second part of this objection can easily be answered. The appraisal of hypotheses which indeed I am compelled to make use of, and which describes them as 'provisional conjectures' (or something of the sort) has the status of a *tautology*. Thus it does not give rise to difficulties of the type to which inductive logic gives rise. For this description only paraphrases or interprets the assertion (to which it is equivalent by definition) that strictly universal statements, i.e. theories, cannot be derived from singular statements.

The position is similar as regards the first part of the objection which concerns appraisals stating that a theory is corroborated. The appraisal of the corroboration is not a hypothesis, but can be derived if we are given the theory as well as the accepted basic statements. It asserts the fact that these basic statements do not contradict the theory, and it does this with due regard to the degree of testability of the theory, and to the severity of the tests to which the theory has been subjected, up to a stated period of time.

We say that a theory is 'corroborated' so long as it stands up to these tests. The appraisal which asserts corroboration (the corroborative appraisal) establishes certain fundamental relations, *viz.* compatibility and incompatibility. We regard incompatibility as falsification of the theory. But compatibility alone must not make us attribute to the theory a positive degree of corroboration: the mere fact that a theory has not yet been falsified can obviously not be regarded as sufficient. For nothing is easier than to construct any number of theoretical systems

which are compatible with any given system of accepted basic statements. (This remark applies also to all 'metaphysical' systems.)

It might perhaps be suggested that a theory should be accorded some positive degree of corroboration if it is compatible with the system of accepted basic statements, and if, in addition, part of this system can be derived from the theory. Or, considering that basic statements are not derivable from a purely theoretical system (though their negations may be so derivable), one might suggest that the following rule should be adopted: a theory is to be accorded a positive degree of corroboration if it is compatible with the accepted basic statements and if, in addition, a non-empty sub-class of these basic statements is derivable from the theory in conjunction with the other accepted basic statements.*[1]

I have no serious objections to this last formulation, except that it seems to me insufficient for an adequate characterization of the positive degree of corroboration of a theory. For we wish to speak of theories as being better, or less well, corroborated. But the *degree of corroboration* of a theory can surely not be established simply by counting the number of the corroborating instances, i.e. the accepted basic statements which are derivable in the way indicated. For it may happen

*[1] The tentative definition of 'positively corroborated' here given (but rejected as insufficient in the next paragraph of the text because it does not explicitly refer to the results of severe tests, *i.e.* of attempted refutations) is of interest in at least two ways. First, it is closely related to my criterion of demarcation, especially to that formulation of it to which I have attached note *1 to section 21. In fact, the two agree except for the restriction to *accepted* basic statements which forms part of the present definition. Thus if we omit this restriction, the present definition turns into my criterion of demarcation.

Secondly, if instead of omitting this restriction we restrict the class of the *derived* accepted basic statements further, by demanding that they should be accepted as the results of sincere attempts to refute the theory, then our definition becomes an adequate definition of 'positively corroborated', though not, of course, of 'degree of corroboration'. The argument supporting this claim is implicit in the text here following. Moreover, the basic statements so accepted may be described as 'corroborating statements' of the theory.

It should be noted that 'instantial statements' (*i.e.* negated basic statements; see section 28) cannot be adequately described as corroborating or confirming statements of the theory which they instantiate, owing to the fact that we know that *every universal law is instantiated* almost everywhere, as indicated in note *1 to section 28. (See also note *4 to section 80, and text.)

that one theory appears to be far less well corroborated than another one, even though we have derived very many basic statements with its help, and only a few with the help of the second. As an example we might compare the hypothesis 'All crows are black' with the hypothesis (mentioned in section 37) 'the electronic charge has the value determined by Millikan'. Although in the case of a hypothesis of the former kind, we have presumably encountered many more corroborative basic statements, we shall nevertheless judge Millikan's hypothesis to be the better corroborated of the two.

This shows that it is not so much the number of corroborating instances which determines the degree of corroboration as *the severity of the various tests* to which the hypothesis in question can be, and has been, subjected. But the severity of the tests, in its turn, depends upon the *degree of testability*, and thus upon the simplicity of the hypothesis: the hypothesis which is falsifiable in a higher degree, or the simpler hypothesis, is also the one which is corroborable in a higher degree.[1] Of course, the degree of corroboration actually attained does not depend *only* on the degree of falsifiability: a statement may be falsifiable to a high degree yet it may be only slightly corroborated, or it may in fact be falsified. And it may perhaps, without being falsified, be superseded by a better testable theory from which it—or a sufficiently close approximation to it—can be deduced. (In this case too its degree of corroboration is lowered.)

The degree of corroboration of two statements may not be comparable in all cases, any more than the degree of falsifiability: we cannot define a numerically calculable degree of corroboration, but can speak only roughly in terms of positive degree of corroboration, negative degrees of corroboration, and so forth.[*2] Yet we can lay down various

rules; for instance the rule that we shall not continue to accord a positive degree of corroboration to a theory which has been falsified by an inter-subjectively testable experiment based upon a falsifying hypothesis (cf. sections 8 and 22). (We may, however, under certain circumstances accord a positive degree of corroboration to another theory, even though it follows a kindred line of thought. An example is Einstein's photon theory, with its kinship to Newton's corpuscular theory of light.) In general we regard an inter-subjectively testable falsification as final (provided it is well tested): this is the way in which the asymmetry between verification and falsification of theories makes itself felt. Each of these methodological points contributes in its own peculiar way to the historical development of science as a process of step by step approximations. A corroborative appraisal made at a later date—that is, an appraisal made after new basic statements have been added to those already accepted—can replace a positive degree of corroboration by a negative one, but not *vice versa*. And although I believe that in the history of science it is always the theory and not the experiment, always the idea and not the observation, which opens up the way to new knowledge, I also believe that it is always the experiment which saves us from following a track that leads nowhere: which helps us out of the rut, and which challenges us to find a new way.

Thus the degree of falsifiability or of simplicity of a theory enters into the appraisal of its corroboration. And this appraisal may be regarded as one of the logical relations between the theory and the accepted basic statements: as an appraisal that takes into consideration the severity of the tests to which the theory has been subjected.

[1] This is another point in which there is agreement between my view of simplicity and Weyl's; cf. note 7 to section 42. *This agreement is a consequence of the view, due to Jeffreys, Wrinch, and Weyl (cf. note 7 to section 42), that the paucity of the parameters of a function can be used as a measure of its simplicity, taken in conjunction with my view (cf. sections 38 ff.) that the paucity of the parameters can be used as a measure of testability or improbability—a view rejected by these authors. (See also notes *1 and *2 to sections 43.)

[*2] As far as practical application to existing theories goes, this seems to me still correct; but I think now that it is possible to define 'degree of corroboration' in such a way that we can *compare* degrees of corroboration (for example, those of Newton's and of Einstein's theory of gravity). Moreover, this definition makes it even possible to attribute numerical degrees of corroboration to statistical hypotheses, and perhaps even to other statements *provided* we can attribute degrees of (absolute and relative) logical probability to them *and to the evidence statements*. See also appendix *ix.

83 CORROBORABILITY, TESTABILITY, AND LOGICAL PROBABILITY*[1]

In appraising the degree of corroboration of a theory we take into account its degree of falsifiability. A theory can be the better corroborated the better testable it is. Testability, however, is converse to the concept of *logical probability*, so that we can also say that an appraisal of corroboration takes into account the logical probability of the statement in question. And this, in turn, as was shown in section 72, is related to the concept of objective probability—the probability of events. Thus by taking logical probability into account the concept of corroboration is linked, even if perhaps only indirectly and loosely, with that of the probability of events. The idea may occur to us that there is perhaps a connection here with the doctrine of the probability of hypotheses criticized above.

When trying to appraise the degree of corroboration of a theory we may reason somewhat as follows. Its degree of corroboration will increase with the number of its corroborating instances. Here we usually accord to the first corroborating instances far greater importance than to later ones: once a theory is well corroborated, further instances raise its degree of corroboration only very little. This rule however does not hold good if these new instances are very different from the earlier ones, that is if they corroborate the theory in a *new field of application*. In this case, they may increase the degree of corroboration very considerably. The degree of corroboration of a theory which has a higher degree of universality can thus be greater than that of a theory which has a lower degree of universality (and therefore a lower degree of falsifiability). In a similar way, theories of a higher degree of precision can be better corroborated than less precise ones. One of the reasons why we do not accord a positive degree of corroboration to the typical prophecies of palmists and soothsayers is that their predictions are so cautious and imprecise that the logical probability of their being correct is extremely high. And if we are told that more precise and thus

*[1] If the terminology is accepted which I first explained in my note in *Mind*, 1938, then the word 'absolute' should be inserted here throughout (as in section 34, etc.) before 'logical probability' (in contradistinction to 'relative' or 'conditional' logical probability); cf. appendices *ii, *iv, and *ix.

logically less probable predictions of this kind have been successful, then it is not, as a rule, their success that we are inclined to doubt so much as their alleged logical improbability: since we tend to believe that such prophecies are non-corroborable, we also tend to argue in such cases from their low degree of corroborability to their low degree of testability.

If we compare these views of mine with what is implicit in (inductive) probability logic, we get a truly remarkable result. According to my view, the corroborability of a theory—and also the degree of corroboration of a theory which has in fact passed severe tests, stand both, as it were,*[2] in inverse ratio to its logical probability; for they both increase with its degree of testability and simplicity. *But the view implied by probability logic is the precise opposite of this.* Its upholders let the probability of a hypothesis increase in *direct proportion* to its logical probability—although there is no doubt that they *intend* their 'probability of a hypothesis' to stand for much the same thing that I try to indicate by 'degree of corroboration'.*[3]

*[2] I said in the text '*as it were*': I did so because I did not really believe in numerical (absolute) logical probabilities. In consequence of this, I wavered, when writing the text, between the view that the degree of corroborability is *complementary* to (absolute) logical probability and the view that it is inversely proportional; or in other words, between a definition of $C(g)$, i.e. the degree of corroborability, by $C(g) = 1 - P(g)$ which would make *corroborability equal to content*, and by $C(g) = 1/P(g)$, where $P(g)$ is the absolute logical probability of g. In fact, definitions may be adopted which lead to either of these consequences, and both ways seem fairly satisfactory on intuitive grounds; this explains, perhaps, my wavering. There are strong reasons in favour of the first method, or else of a logarithmic scale applied to the second method. See appendix *ix.

*[3] The last lines of this paragraph, especially from the italicized sentence on (it was not italicized in the original) contain the crucial point of my criticism of the probability theory of induction. The point may be summarized as follows.

We want *simple* hypotheses—hypotheses of a high *content*, a high degree of *testability*. These are also the highly *corroborable* hypotheses, for the degree of corroboration of a hypothesis depends mainly upon the severity of its tests, and thus upon its testability. Now we know that testability is the same as high (absolute) logical *improbability*, or low (absolute) logical *probability*.

But if two hypotheses, h_1 and h_2, are comparable with respect to their content, and thus with respect to their (absolute) logical probability, then the following holds: let the (absolute) logical probability of h_1 be smaller than that of h_2. Then, whatever the evidence e, the (relative) logical probability of h_1 given e can never exceed that of h_2 given e. Thus *the better testable and better corroborable hypothesis can never obtain a higher probability, on the given evidence, than the less testable one.* But this entails that *degree of corroboration cannot be the same as probability.*

Among those who argue in this way is Keynes who uses the expression '*a priori* probability' for what I call 'logical probability'. (See note 1 to section 34.) He makes the following perfectly accurate remark[1] regarding a 'generalization' *g* (i.e. a hypothesis) with the 'condition' or antecedent or protasis φ and the 'conclusion' or consequent or apodosis f: 'The more comprehensive the condition φ and the less comprehensive the conclusion f, the greater *à priori**[4] probability do we attribute to the generalization *g*. With every increase in φ this probability increases, and with every increase in f it will diminish.' This, as I said, is perfectly accurate, even though Keynes does not draw a sharp distinction*[5] between what he calls the 'probability of a generalization'—corresponding to what is here called the 'probability of a hypothesis'—and its '*a priori* probability'. Thus in contrast to my *degree of corroboration*, Keynes's *probability of a hypothesis* increases with its *a priori logical probability*. That Keynes nevertheless intends by his 'probability' the same as I do by my 'corroboration' may be seen from the fact that his 'probability' rises with the number of corroborating instances, and also (most important) with the increase of diversity

This is the crucial result. My later remarks in the text merely draw the conclusion from it: if you value high probability, you must say very little—or better still, nothing at all: tautologies will always retain the highest probability.

[1] Keynes, *A Treatise on Probability*, 1921, pp. 224 f. Keynes's condition φ and conclusion f correspond (cf. note 6 to section 14) to our conditioning statement function φ and our consequence statement function f; cf. also section 36. It should be noticed that Keynes called the condition or the conclusion *more comprehensive* if its *content*, or its intension, rather than its extension, is the greater. (I am alluding to the inverse relationship holding between the intension and the extension of a term.)

*[4] Keynes follows some eminent Cambridge logicians in writing '*à priori*' and '*à posteriori*'; one can only say, *à propos de rien*—unless, perhaps, apropos of '*à propos*'.

*[5] Keynes does, in fact, allow for the distinction between the *a priori* (or 'absolute logical', as I now call it) probability of the 'generalization' *g* and its probability with respect to a given piece of evidence *h*, and to this extent, my statement in the text needs correction. (He makes the distinction by assuming, correctly though perhaps only implicitly—see p. 225 of the *Treatise*—that if $\varphi = \varphi_1\varphi_2$, and $f = f_1f_2$, then the *a priori* probabilities of the various *g* are: $g(\varphi, f_1) \geqslant g(\varphi, f) \geqslant g(\varphi_1, f)$.) And he correctly *proves* that the *a posteriori* probabilities of these hypotheses *g* (relative to *any* given piece of evidence *h*) change in the same way as their *a priori* probabilities. Thus while his probabilities change like (absolute) logical probabilities, it is my cardinal point that degrees of corroborability (*and* of corroboration) change in the opposite way.

among them. But Keynes overlooks the fact that theories whose corroborating instances belong to widely different fields of application will usually have a correspondingly high degree of universality. Hence his two requirements for obtaining a high probability—the least possible universality and the greatest possible diversity of instances—will as a rule be incompatible.

Expressed in my terminology, Keynes's theory implies that corroboration (or the probability of hypotheses) *decreases* with testability. He is led to this view by his belief in inductive logic.*[6] For it is the tendency of inductive logic to make scientific hypotheses as *certain* as possible. Scientific significance is assigned to the various hypotheses only to the extent to which they can be justified by experience. A theory is regarded as scientifically valuable only because of the close *logical proximity* (cf. note 2 to section 48 and text) between the theory and empirical statements. But this means nothing else than that the *content* of the theory must go *as little as possible* beyond what is empirically established.*[7] This view is closely connected with a tendency to deny the value of prediction. 'The peculiar virtue of prediction' Keynes writes[2] '. . . is altogether imaginary. The number of instances examined and the analogy between them are the essential points, and the question as to whether a particular hypothesis happens to be propounded before or after their examination is quite irrelevant.' In reference to hypotheses which have been '*a priori* proposed'—that is, proposed before we had sufficient support for them on inductive grounds—Keynes writes: '. . . if it is a mere guess, the lucky fact of its preceding some or all of the cases which verify it adds nothing whatever to its value.' This view of prediction is certainly consistent. But it makes one wonder why we should ever have to generalize at all. What possible reason can there be for constructing all these theories and hypotheses? The standpoint of inductive logic makes these activities quite incomprehensible. If what we value most is the securest

*[6] See my *Postscript*, chapter *ii. In my theory of corroboration—in direct opposition to Keynes's, Jeffreys's, and Carnap's theories of probability—corroboration does not *decrease* with testability, but tends to *increase* with it.

*[7] This may also be expressed by the unacceptable rule: 'Always choose the hypothesis which is most *ad hoc*!'

[2] Keynes, *op. cit.*, p. 305.

knowledge available—and if predictions as such contribute nothing towards corroboration—why then may we not rest content with our basic statements?*8

Another view which gives rise to very similar questions is that of Kaila.[3] Whilst I believe that it is the simple theories, and those which make little use of auxiliary hypotheses (cf. section 46) which can be well corroborated, just because of their logical improbability, Kaila interprets the situation in precisely the opposite way, on grounds similar to Keynes's. He too sees that we usually ascribe a high probability (in our terminology, a high 'probability of hypotheses') to *simple* theories, and especially to those needing few auxiliary hypotheses. But his reasons are the opposite of mine. He does not, as I do, ascribe a high probability to such theories because they are severely testable, or logically improbable; that is to say because they have, *a priori* as it were, *many opportunities of clashing with basic statements*. On the contrary he ascribes this high probability to simple theories with few auxiliary hypotheses because he believes that a system consisting of *few* hypotheses will, *a priori*, have *fewer* opportunities of clashing with reality than a system consisting of many hypotheses. Here again one wonders why we should ever bother to construct these adventurous theories. If we shrink from conflict with reality, why invite it by making assertions? The safest course is to adopt a system *without any* hypotheses. ['*Speech is silvern, silence is golden*.']

My own rule which requires that auxiliary hypotheses shall be used as sparingly as possible (the 'principle of parsimony in the use of hypotheses') has nothing whatever in common with considerations

*8 Carnap, in his *Logical Foundations of Probability*, 1950, believes in the *practical* value of predictions; nevertheless, he draws part of the conclusion here mentioned—that we might be content with our basic statements. For he says that theories (he speaks of 'laws') are 'not indispensable' for science—not even for making predictions: we can manage throughout with singular statements. 'Nevertheless', he writes (p. 575) 'it is expedient, of course, to state universal laws in books on physics, biology, psychology, *etc*.' But the question is not one of expediency—it is one of scientific curiosity. *Some scientists want to explain the world*: their aim is to find satisfactory explanatory theories—well testable, i.e. simple theories—and to test them. (See also appendix *x and section *15 of my *Postscript*.)

[3] Kaila, *Die Principien der Wahrscheinlichkeitslogik* (*Annales Universitatis Aboensis*, Turku 1926), p. 140.

such as Kaila's. I am not interested in merely keeping down the number of our statements: I am interested in their *simplicity in the sense of high testability*. It is this interest which leads, on the one hand, to my rule that auxiliary hypotheses should be used as sparingly as possible, and on the other hand, to my demand that the number of our axioms—of our most fundamental hypotheses—should be kept down. For this latter point arises out of the demand that statements of a high level of universality should be chosen, and that a system consisting of many 'axioms' should, if possible, be deduced from (and thus explained by) one with fewer 'axioms', and with axioms of a higher level of universality.

84 REMARKS CONCERNING THE USE OF THE CONCEPTS 'TRUE' AND 'CORROBORATED'

In the logic of science here outlined it is possible to avoid using the concepts 'true' and 'false'.*1 Their place may be taken by logical

*1 Not long after this was written, I had the good fortune to meet Alfred Tarski who explained to me the fundamental ideas of his theory of truth. It is a great pity that this theory—one of the two great discoveries in the field of logic made since *Principia Mathematica*—is still often misunderstood and misrepresented. It cannot be too strongly emphasized that Tarski's idea of truth (for whose definition with respect to formalized languages Tarski gave a method) is the same idea which Aristotle had in mind and indeed most people (except pragmatists): the idea that *truth is correspondence with the facts* (or with reality). But what can we possibly mean if we say of a *statement* that it corresponds with the *facts* (or with reality)? Once we realize that this correspondence cannot be one of structural similarity, the task of elucidating this correspondence seems hopeless; and as a consequence, we may become suspicious of the concept of truth, and prefer not to use it. Tarski solved (with respect to formalized languages) this apparently hopeless problem by making use of a semantic metalanguage, reducing the idea of correspondence to that of 'satisfaction' or 'fulfilment'.

As a result of Tarski's teaching, I no longer hesitate to speak of 'truth' and 'falsity'. And like everybody else's views (unless he is a pragmatist), my views turned out, as a matter of course, to be consistent with Tarski's theory of absolute truth. Thus although my views on formal logic and its philosophy were revolutionized by Tarski's theory, my views on science and its philosophy were fundamentally unaffected, although clarified.

Some of the current criticism of Tarski's theory seems to me wide of the mark. It is said that his definition is artificial and complex; but since he defines truth with respect to formalized languages, it has to be based on the definition of a well-formed formula in such a language; and it is of precisely the same degree of 'artificiality' or 'complexity' as this definition. It is also said that only propositions or statements can be true or false, but considerations about derivability relations. Thus we need not say: 'The prediction *p* is true provided the theory *t* and the basic statement *b* are true.' We may say, instead, that the statement *p* follows from the (non-contradictory) conjunction of *t* and *b*. The falsification of a theory may be described in a similar way. We need not say that the theory is 'false', but we may say instead that it is contradicted by a certain set of accepted basic statements. Nor need we say of basic statements that they are 'true' or 'false', for we may interpret their acceptance as the result of a conventional decision, and the accepted statements as results of this decision.

This certainly does not mean that we are forbidden to use the concepts 'true' and 'false', or that their use creates any particular difficulty. The very fact that we can avoid them shows that they cannot give rise to any new fundamental problem. The use of the concepts 'true' and 'false' is quite analogous to the use of such concepts as '*tautology*', '*contradiction*', '*conjunction*', '*implication*' and others of the kind. These are non-empirical concepts, logical concepts.[1] They describe or appraise a statement irrespective of any changes in the empirical world. Whilst we assume that the properties of physical objects (of 'genidentical' objects in Lewin's sense) change with the passage of time, we decide to use these logical predicates in such a way that the logical properties of statements become timeless: if a statement is a tautology, then it is a tautology once and for all. This same timelessness we also attach to the concepts 'true' and 'false', in agreement with common usage. It is not common usage to say of a statement that it was perfectly true yesterday but has become false today. If yesterday we appraised a statement as true which today we appraise as false, then we implicitly assert today

not sentences. Perhaps 'sentence' was not a good translation of Tarski's original terminology. (I personally prefer to speak of 'statement' rather than of 'sentence'; see for example my 'Note on Tarski's Definition of Truth', *Mind* **64**, 1955, p. 388, footnote 1.) But Tarski himself made it perfectly clear that an uninterpreted formula (or a string of symbols) cannot be said to be true or false, and that these terms only apply to interpreted formulae—to '*meaningful* sentences' (as the translation has it). Improvements in terminology are always welcome; but it is sheer obscurantism to criticize a theory on terminological grounds.

[1] (Added in 1934 in proof.) Carnap would probably say 'syntactical concepts' (cf. his *Logical Syntax of Language*).

that *we were mistaken yesterday*; that the statement was false even yesterday—timelessly false—but that we erroneously 'took it for true'.

Here one can see very clearly the difference between truth and corroboration. The appraisal of a statement as corroborated or as not corroborated is also a logical appraisal and therefore also timeless; for it asserts that a certain logical relation holds between a theoretical system and some system of accepted basic statements. But we can never simply say of a statement that it is as such, or in itself, 'corroborated' (in the way in which we may say that it is 'true'). We can only say that it is *corroborated with respect to some system of basic statements*—a system accepted up to a particular point in time. 'The corroboration which a theory has received up to yesterday' is *logically not identical* with 'the corroboration which a theory has received up to today'. Thus we must attach a subscript, as it were, to every appraisal of corroboration—a subscript characterizing the system of basic statements to which the corroboration relates (for example, by the date of its acceptance).[*2]

Corroboration is therefore not a 'truth value'; that is, it cannot be placed on a par with the concepts 'true' and 'false' (which are free from temporal subscripts); for to one and the same statement there may be any number of different corroboration values, of which indeed all can be 'correct' or 'true' at the same time. For they are values which are logically derivable from the theory and the various sets of basic statements accepted at various times.

The above remarks may also help to elucidate the contrast between my views and those of the pragmatists who propose to *define* 'truth' in *terms of the success of a theory—and thus of its usefulness, or of its confirmation or of its corroboration*. If their intention is merely to assert that a logical appraisal of the success of a theory can be no more than an appraisal of its corroboration, I can agree. But I think that it would be far from '*useful*' to identify the concept of corroboration with that of truth.[*3] This is also avoided in ordinary usage. For one might well say of a theory that it has hardly been corroborated at all so far, or that it is still

[*2] Cf. note *1 to section 81.

[*3] Thus if we were to define 'true' as 'useful' (as suggested by some pragmatists), or else as 'successful' or 'confirmed' or 'corroborated', we should only have to introduce a new 'absolute' and 'timeless' concept to play the role of 'truth'.

uncorroborated. But we should not normally say of a theory that it is hardly true at all so far, or that it is still false.

85 THE PATH OF SCIENCE

One may discern something like a general direction in the evolution of physics—a direction from theories of a lower level of universality to theories of a higher level. This is usually called the 'inductive' direction; and it might be thought that the fact that physics advances in this 'inductive' direction could be used as an argument in favour of the inductive method.

Yet an advance in the inductive direction does not necessarily consist of a sequence of inductive inferences. Indeed we have shown that it may be explained in quite different terms—in terms of degree of testability and corroborability. For a theory which has been well corroborated can only be superseded by one of a higher level of universality; that is, by a theory which is better testable and which, in addition, *contains* the old, well corroborated theory—or at least a good approximation to it. It may be better, therefore, to describe that trend—the advance towards theories of an ever higher level of universality—as 'quasi-inductive'.

The quasi-inductive process should be envisaged as follows. Theories of some level of universality are proposed, and deductively tested; after that, theories of a higher level of universality are proposed, and in their turn tested with the help of those of the previous levels of universality, and so on. The methods of testing are invariably based on deductive inferences from the higher to the lower level;*1 on the other hand, the levels of universality are reached, in the order of time, by proceeding from lower to higher levels.

The question may be raised: 'Why not invent theories of the highest level of universality straight away? Why wait for this quasi-inductive evolution? Is it not perhaps because there is after all an inductive element contained in it?' I do not think so. Again and again suggestions are

*1 The 'deductive inferences from the higher to the lower level' are, of course, *explanations* (in the sense of section 12); thus the hypotheses on the higher level are *explanatory* with respect to those on the lower level.

put forward—conjectures, or theories—of all possible levels of universality. Those theories which are on too high a level of universality, as it were (that is, too far removed from the level reached by the testable science of the day) give rise, perhaps, to a 'metaphysical system'. In this case, even if from this system statements should be deducible (or only semi-deducible, as for example in the case of Spinoza's system), which belong to the prevailing scientific system, there will be no *new* testable statement among them; which means that no crucial experiment can be designed to test the system in question.*2 If, on the other hand, a crucial experiment can be designed for it, then the system will contain, as a first approximation, some well corroborated theory, and at the same time also something new—and something that can be tested. Thus the system will not, of course, be 'metaphysical'. In this case, the system in question may be looked upon as a new advance in the quasi-inductive evolution of science. This explains why a link with the science of the day is as a rule established only by those theories which are proposed in an attempt to meet the current problem situation; that is, the current difficulties, contradictions, and falsifications. In proposing a solution to these difficulties, these theories may point the way to a crucial experiment.

To obtain a picture or model of this quasi-inductive evolution of science, the various ideas and hypotheses might be visualized as particles suspended in a fluid. Testable science is the precipitation of these particles at the bottom of the vessel: they settle down in layers (of universality). The thickness of the deposit grows with the number of these layers, every new layer corresponding to a theory more universal than those beneath it. As the result of this process ideas previously floating in higher metaphysical regions may sometimes be reached by the growth of science, and thus make contact with it, and settle. Examples of such ideas are atomism; the idea of a single physical 'principle' or ultimate element (from which the others derive); the theory of terrestrial motion (opposed by Bacon as fictitious); the

*2 It should be noted that I mean by a crucial experiment one that is designed to refute a theory (if possible) and more especially one which is designed to bring about a decision between two competing theories by refuting (at least) one of them—without, of course, proving the other. (See also note x to section 22, and appendix *ix.)

age-old corpuscular theory of light; the fluid-theory of electricity (revived as the electron-gas hypothesis of metallic conduction). All these metaphysical concepts and ideas may have helped, even in their early forms, to bring order into man's picture of the world, and in some cases they may even have led to successful predictions. Yet an idea of this kind acquires scientific status only when it is presented in falsifiable form; that is to say, only when it has become possible to decide empirically between it and some rival theory.

My investigation has traced the various consequences of the decisions and conventions—in particular of the criterion of demarcation—adopted at the beginning of this book. Looking back, we may now try to get a last comprehensive glimpse of the picture of science and of scientific discovery which has emerged. (What I have here in mind is not a picture of science as a biological phenomenon, as an instrument of adaptation, or as a roundabout method of production: I have in mind its epistemological aspects.)

Science is not a system of certain, or well-established, statements; nor is it a system which steadily advances towards a state of finality. Our science is not knowledge (*epistēmē*): it can never claim to have attained truth, or even a substitute for it, such as probability.

Yet science has more than mere biological survival value. It is not only a useful instrument. Although it can attain neither truth nor probability, the striving for knowledge and the search for truth are still the strongest motives of scientific discovery.

We do not know: we can only guess. And our guesses are guided by the unscientific, the metaphysical (though biologically explicable) faith in laws, in regularities which we can uncover—discover. Like Bacon, we might describe our own contemporary science—'the method of reasoning which men now ordinarily apply to nature'—as consisting of 'anticipations, rash and premature' and of 'prejudices'.[1]

But these marvellously imaginative and bold conjectures or 'anticipations' of ours are carefully and soberly controlled by systematic tests. Once put forward, none of our 'anticipations' are dogmatically upheld. Our method of research is not to defend them, in order to prove how

[1] Bacon, *Novum Organum* I, 26.

right we were. On the contrary, we try to overthrow them. Using all the weapons of our logical, mathematical, and technical armoury, we try to prove that our anticipations were false—in order to put forward, in their stead, new unjustified and unjustifiable anticipations, new 'rash and premature prejudices', as Bacon derisively called them.*[1]

It is possible to interpret the ways of science more prosaically. One might say that progress can '. . . come about only in two ways: by gathering new perceptual experiences, and by better organizing those which are available already'.[2] But this description of scientific progress, although not actually wrong, seems to miss the point. It is too reminiscent of Bacon's induction: too suggestive of his industrious gathering of the 'countless grapes, ripe and in season',[3] from which he expected the wine of science to flow: of his myth of a scientific method that starts from observation and experiment and then proceeds to theories. (This legendary method, by the way, still inspires some of the newer sciences which try to practice it because of the prevalent belief that it is the method of experimental physics.)

*[1] Bacon's 'anticipation' ('*anticipatio*'; *Novum Organum* I, 26) *means almost the same as 'hypothesis' (in my usage). Bacon held that, to prepare the mind for the intuition of the true essence or nature of a thing,* it has to be meticulously cleansed of all anticipations, prejudices, and idols. For the source of all error is the impurity of our own minds: Nature itself does not lie. The main function of eliminative induction is (as with Aristotle) to assist the purification of the mind. (*See also* my *Open Society*, chapter 24; note 59 to chapter 10; note 33 to chapter 11, where Aristotle's theory of induction is briefly described). Purging the mind of prejudices is conceived as a kind of ritual, prescribed for the scientist who wishes to prepare his mind for the interpretation (the unbiassed reading) of the Book of Nature: just as the mystic purifies his soul to prepare it for the vision of God. (Cf. the Introduction to my *Conjectures and Refutations* (1963) 1965.)

[2] P. Frank, *Das Kausalgesetz und seine Grenzen*, 1932. *The view that the progress of science is due to the accumulation of perceptual experiences is still widely held (cf. my second Preface, 1958). My denial of this view is closely connected with the rejection of the doctrine that science or knowledge is *bound* to advance since our experiences are *bound* to accumulate. As against this, I believe that the advance of science depends upon the free competition of thought, and thus upon freedom, and that it must come to an end if freedom is destroyed (though it may well continue for some time in some fields, especially in technology). This view is more fully expounded in my *Poverty of Historicism* (section 32). I also argue there (in the Preface) that the growth of our knowledge is unpredictable by scientific means, and that, as a consequence, the future course of our history is also unpredictable.

[3] Bacon, *Novum Organum* I, 123.

The advance of science is not due to the fact that more and more perceptual experiences accumulate in the course of time. Nor is it due to the fact that we are making ever better use of our senses. Out of uninterpreted sense-experiences science cannot be distilled, no matter how industriously we gather and sort them. Bold ideas, unjustified anticipations, and speculative thought, are our only means for interpreting nature: our only organon, our only instrument, for grasping her. And we must hazard them to win our prize. Those among us who are unwilling to expose their ideas to the hazard of refutation do not take part in the scientific game.

Even the careful and sober testing of our ideas by experience is in its turn inspired by ideas: experiment is planned action in which every step is guided by theory. We do not stumble upon our experiences, nor do we let them flow over us like a stream. Rather, we have to be active: we have to '*make*' our experiences. It is we who always formulate the questions to be put to nature; it is we who try again and again to put these question so as to elicit a clear-cut 'yes' or 'no' (for nature does not give an answer unless pressed for it). And in the end, it is again we who give the answer; it is we ourselves who, after severe scrutiny, decide upon the answer to the question which we put to nature—after protracted and earnest attempts to elicit from her an unequivocal 'no'. 'Once and for all', says Weyl,[4] with whom I fully agree, 'I wish to record my unbounded admiration for the work of the experimenter in his struggle to wrest *interpretable facts* from an unyielding Nature who knows so well how to meet our theories with a decisive *No*—or with an inaudible *Yes*.'

The old scientific ideal of *epistēmē*—of absolutely certain, demonstrable knowledge—has proved to be an idol. The demand for scientific objectivity makes it inevitable that every scientific statement must remain *tentative for ever*. It may indeed be corroborated, but every corroboration is relative to other statements which, again, are tentative. Only in our subjective experiences of conviction, in our subjective faith, can we be 'absolutely certain'.[5]

[4] Weyl, *Gruppentheorie und Quantenmechanik*, 1931, p. 2. English translation by H. P. Robertson: *The Theory of Groups and Quantum Mechanics*, 1931, p. xx.

[5] Cf. for example note 3 to section 30. This last remark is of course a psychological remark rather than an epistemological one; cf. sections 7 and 8.

With the idol of certainty (including that of degrees of imperfect certainty or probability) there falls one of the defences of obscurantism which bar the way of scientific advance. For the worship of this idol hampers not only the boldness of our questions, but also the rigour and the integrity of our tests. The wrong view of science betrays itself in the craving to be right; for it is not his *possession* of knowledge, of irrefutable truth, that makes the man of science, but his persistent and recklessly critical *quest* for truth.

Has our attitude, then, to be one of resignation? Have we to say that science can fulfil only its biological task; that it can, at best, merely prove its mettle in practical applications which may corroborate it? Are its intellectual problems insoluble? I do not think so. Science never pursues the illusory aim of making its answers final, or even probable. Its advance is, rather, towards an infinite yet attainable aim: that of ever discovering new, deeper, and more general problems, and of subjecting our ever tentative answers to ever renewed and ever more rigorous tests.

This is the end of the text of the original book.
The Appendices i–vii which are here printed on
pp. 285–310 were also part of that original edition.

Addendum, 1972

In the preceding chapter of my book (which was the final chapter) I tried to make clear that by the *degree of corroboration* of a theory I mean a brief report that summarizes the way in which the theory has stood up to tests, and how severe these tests were.

I have never deviated from this view; see for example the beginnings of the new Appendices *vii, p. 378; *ix, p. 406; and especially the last section (*14) of *ix, pp. 441 f. Here I wish to add the following points:

(1) The logical and methodological problem of induction is not insoluble, but my book offered a negative solution: (a) *We can never rationally justify a theory*, that is to say, our belief in the truth of a theory, or in its being probably true. This negative solution is compatible with the following positive solution, contained in the *rule of preferring theories* which are better corroborated than others: (b) *We can sometimes rationally*

justify the preference for a theory in the light of its corroboration, that is, of the present state of the critical discussion of the competing theories, which are critically discussed and compared from the point of view of assessing their nearness to the truth (verisimilitude). The current state of this discussion may, in principle, be reported in the form of their degrees of corroboration. The degree of corroboration is not, however, a measure of verisimilitude (such a measure would have to be timeless) but only a report of what we have been able to ascertain up to a certain moment of time, about the comparative claims of the competing theories by judging the available reasons which have been proposed for and against their verisimilitude.

(2) A metaphysical problem raised by the idea of verisimilitude is: are there genuine regularities in nature? My reply is 'yes'. One of the arguments (non-scientific but perhaps 'transcendental'; see pp. 384–5) in favour of this reply is: if no regularities were apparent in nature then neither observations nor language could exist: neither a descriptive nor an argumentative language.

(3) The force of this reply depends on some kind of commonsense realism.

(4) The pragmatic problem of induction solves itself: the practical preference for the theory which in the light of the rational discussion appears to be nearer to the truth is risky but rational.

(5) The psychological problem (why do we *believe* that the theory so chosen will continue to be worthy of our trust?) is, I suggest, trivial: a belief or trust is always irrational, but it may be important for action.

(6) Not all possible 'problems of induction' are solved in this way. (See also my forthcoming book: *Objective Knowledge: An Evolutionary Approach*.)

APPENDIX i

Definition of the Dimension of a Theory (*cf.* sections 38 and 39)

The definition which follows here should be regarded as only provisional.*1 It is an attempt to define the dimension of a theory so as to make it agree with the dimension of the set of curves which results if the field of application of the theory is represented by a graph paper. A difficulty arises from the fact that we should not assume that either a metric or even a topology is defined for the field, to begin with; in particular, we should not assume that any neighbourhood relations are defined. And I admit that this difficulty is circumvented rather than overcome by the definition proposed. The possibility of circumventing

*1 A simplified and slightly more general definition is this. Let A and X be two sets of statements. (Intuitively, A is a set of universal laws, X a set—usually infinite—of singular test statements.) Then we say that X is a (homogeneous) field of application with respect to A (in symbols: $X = F_A$) if and only if for every statement a in A, there exists a natural number $d(a) = n$ which satisfies the following two conditions: (i) any conjunction c_n of n different statements of X is compatible with a; (ii) for any such conjunction c_n there exist two statements x and y in X such that $x.c_n$ is incompatible with a and $y.c_n$ is derivable from $a.c_n$, but neither from a nor from c_n.

$d(a)$ is called the dimension of a, or the degree of composition of a, with respect to $X = F_A$; and $1/d(a)$ or, say, $1/(d(a) + 1)$, may be taken as a measure of the simplicity of a.

The problem is further developed in appendix *viii.