



# The Logic of Scientific Discovery

**'One of the most important philosophical works of our century.'**

*Richard Wollheim, The Observer*

**'Wonderfully exhilarating.'**

*Naomi Bliven, New Yorker*



Karl  
**Popper**

The Logic of Scientific  
Discovery



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TO MY WIFE  
who is responsible for the revival of this book



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## TRANSLATORS' NOTE

*The Logic of Scientific Discovery* is a translation of *Logik der Forschung*, published in Vienna in the autumn of 1934 (with the imprint '1935'). The translation was prepared by the author, with the assistance of Dr. Julius Freed and Ian Freed.

The original text of 1934 has been left unchanged for the purpose of the translation. As usual, the translation is a little longer than the original. Words and phrases for which no equivalent exists had to be paraphrased. Sentences had to be broken up and rearranged—the more so as the text to be translated was highly condensed: it had been drastically cut several times to comply with the publisher's requirements. Yet the author decided against augmenting the text, and also against restoring cut passages [except for a few words indicated by square brackets or footnotes].

In order to bring the book up to date, the author has added new appendices and new footnotes. Some of these merely expand the text, or correct it; but others explain where the author has changed his mind, or how he would now reframe his arguments.

All new additions—new appendices and new footnotes—are marked by starred number; and where old footnotes have been expanded, the expansion is also marked by a star (unless it consists only of a reference to the English edition of a book originally quoted from a German edition).

In these new starred additions, references will be found to a sequel to this volume, entitled *Postscript to the Logic of Scientific Discovery* (in three volumes). Though they complement each other, they are independent.

It should also be mentioned that the numbering of the chapters of the present volume has been changed. In the original, they were numbered i to ii (part i), and i to viii (part ii). They are now numbered through from 1 to 10.

Hypotheses are nets: only he who casts will catch.  
NOVALIS

## PREFACE TO THE FIRST EDITION, 1934

The hint that man has, after all, solved his most stubborn problems . . . is small solace to the philosophic connoisseur; for what he cannot help fearing is that philosophy will never get so far as to pose a genuine problem.

M. SCHLICK (1930)

I for my part hold the very opposite opinion, and I assert that whenever a dispute has raged for any length of time, especially in philosophy, there was, at the bottom of it, never a problem about mere words, but always a genuine problem about things.

I. KANT (1786)

A scientist engaged in a piece of research, say in physics, can attack his problem straight away. He can go at once to the heart of the matter: to the heart, that is, of an organized structure. For a structure of scientific doctrines is already in existence; and with it, a generally accepted problem-situation. This is why he may leave it to others to fit his contribution into the framework of scientific knowledge.

The philosopher finds himself in a different position. He does not face an organized structure, but rather something resembling a heap of ruins (though perhaps with treasure buried underneath). He cannot



appeal to the fact that there is a generally accepted problem-situation; for that there is no such thing is perhaps the one fact which is generally accepted. Indeed it has by now become a recurrent question in philosophical circles whether philosophy will ever get so far as to pose a genuine problem.

Nevertheless there are still some who do believe that philosophy can pose genuine problems about things, and who therefore still hope to get these problems discussed, and to have done with those depressing monologues which now pass for philosophical discussions. And if by chance they find themselves unable to accept any of the existing creeds, all they can do is to begin afresh from the beginning.

VIENNA, Autumn 1934.

There is nothing more necessary to the man of science than its history,  
and the logic of discovery . . . : the way error is detected, the use of  
hypothesis, of imagination, the mode of testing.

LORD ACTON

## PREFACE TO THE FIRST ENGLISH EDITION, 1959

In my old preface of 1934 I tried to explain—too briefly, I am afraid—my attitude towards the then prevailing situation in philosophy, and especially towards linguistic philosophy and the school of language analysts of those days. In this new preface I intend to explain my attitude towards the present situation, and towards the two main schools of language analysts of today. Now as then, language analysts are important to me; not only as opponents, but also as allies, in so far as they seem to be almost the only philosophers left who keep alive some of the traditions of rational philosophy.

Language analysts believe that there are no genuine philosophical problems, or that the problems of philosophy, if any, are problems of linguistic usage, or of the meaning of words. I, however, believe that there is at least one philosophical problem in which all thinking men are interested. It is the problem of cosmology: *the problem of understanding the world—including ourselves, and our knowledge, as part of the world*. All science is cosmology, I believe, and for me the interest of philosophy, no less than of science, lies solely in the contributions which it has made to it. For me, at any rate, both philosophy and science would lose all their attraction if they were to give up that pursuit. Admittedly, understanding the functions of our language is an important part of it; but explaining away our problems as merely linguistic ‘puzzles’ is not.

Language analysts regard themselves as practitioners of a method peculiar to philosophy. I think they are wrong, for I believe in the following thesis.

Philosophers are as free as others to use any method in searching for truth. *There is no method peculiar to philosophy.*

A second thesis which I should like to propound here is this.

The central problem of epistemology has always been and still is the problem of the growth of knowledge. *And the growth of knowledge can be studied best by studying the growth of scientific knowledge.*

I do not think that the study of the growth of knowledge can be replaced by the study of linguistic usages, or of language systems.

And yet, I am quite ready to admit that there is a method which might be described as 'the one method of philosophy'. But it is not characteristic of philosophy alone; it is, rather, the one method of all rational discussion, and therefore of the natural sciences as well as of philosophy. The method I have in mind is that of stating one's problem clearly and of examining its various proposed solutions critically.

I have italicized the words 'rational discussion' and 'critically' in order to stress that I equate the rational attitude and the critical attitude. The point is that, whenever we propose a solution to a problem, we ought to try as hard as we can to overthrow our solution, rather than defend it. Few of us, unfortunately, practise this precept; but other people, fortunately, will supply the criticism for us if we fail to supply it ourselves. Yet criticism will be fruitful only if we state our problem as clearly as we can and put our solution in a sufficiently definite form—a form in which it can be critically discussed.

I do not deny that something which may be called 'logical analysis' can play a role in this process of clarifying and scrutinizing our problems and our proposed solutions; and I do not assert that the methods of 'logical analysis' or 'language analysis' are necessarily useless. My thesis is, rather, that these methods are far from being the only ones which a philosopher can use with advantage, and that they are in no way characteristic of philosophy. They are no more characteristic of philosophy than of any other scientific or rational inquiry.

It may perhaps be asked what other 'methods' a philosopher might use. My answer is that though there are any number of different

'methods', I am really not interested in enumerating them. I do not care what methods a philosopher (or anybody else) may use so long as he has an interesting problem, and so long as he is sincerely trying to solve it.

Among the many methods which he may use—always depending, of course, on the problem in hand—one method seems to me worth mentioning. It is a variant of the (at present unfashionable) historical method. It consists, simply, in trying to find out what other people have thought and said about the problem in hand: why they had to face it: how they formulated it: how they tried to solve it. This seems to me important because it is part of the general method of rational discussion. If we ignore what other people are thinking, or have thought in the past, then rational discussion must come to an end, though each of us may go on happily talking to himself. Some philosophers have made a virtue of talking to themselves; perhaps because they felt that there was nobody else worth talking to. I fear that the practice of philosophizing on this somewhat exalted plane may be a symptom of the decline of rational discussion. No doubt God talks mainly to Himself because He has no one worth talking to. But a philosopher should know that he is no more godlike than any other man.

There are several interesting historical reasons for the widespread belief that what is called 'linguistic analysis' is the true method of philosophy.

One such reason is the correct belief that *logical paradoxes*, like that of the liar ('I am now lying') or those found by Russell, Richard, and others, need the method of linguistic analysis for their solution, with its famous distinction between meaningful (or 'well-formed') and meaningless linguistic expressions. This correct belief is then combined with the mistaken belief that the traditional problems of philosophy arise from the attempt to solve *philosophical paradoxes* whose structure is analogous to that of *logical paradoxes*, so that the distinction between meaningful and meaningless talk must be of central importance for philosophy also. That this belief is mistaken can be shown very easily. It can be shown, in fact, by logical analysis. For this reveals that a certain characteristic kind of reflexivity or self-reference which is present in all *logical paradoxes* is absent from all the so-called *philosophical paradoxes*—even from Kant's antinomies.

perception or knowledge or belief by the analysis of the phrases 'I see' or 'I perceive', or 'I know', 'I believe', 'I hold that it is probable'; or perhaps by that of the word 'perhaps'.

Now to those who favour this approach to the theory of knowledge I should reply as follows. Although I agree that scientific knowledge is merely a development of ordinary knowledge or common-sense knowledge, I contend that the most important and most exciting problems of epistemology must remain completely invisible to those who confine themselves to analysing ordinary or common-sense knowledge or its formulation in ordinary language.

I wish to refer here only to one example of the kind of problem I have in mind: the problem of the growth of our knowledge. A little reflection will show that most problems connected with the growth of our knowledge must necessarily transcend any study which is confined to common-sense knowledge as opposed to scientific knowledge. For the most important way in which common-sense knowledge grows is, precisely, by turning into scientific knowledge. Moreover, it seems clear that the growth of scientific knowledge is the most important and interesting case of the growth of knowledge.

It should be remembered, in this context, that almost all the problems of traditional epistemology are connected with the problem of the growth of knowledge. I am inclined to say even more: from Plato to Descartes, Leibniz, Kant, Duhem and Poincaré; and from Bacon, Hobbes, and Locke, to Hume, Mill, and Russell, the theory of knowledge was inspired by the hope that it would enable us not only to know more about knowledge, but also to contribute to the advance of knowledge—of scientific knowledge, that is. (The only possible exception to this rule among the great philosophers I can think of is Berkeley.) Most of the philosophers who believe that the characteristic method of philosophy is the analysis of ordinary language seem to have lost this admirable optimism which once inspired the rationalist tradition. Their attitude, it seems, has become one of resignation, if not despair. They not only leave the advancement of knowledge to the scientists: they even define philosophy in such a way that it becomes, by definition, incapable of making any contribution to our knowledge of the world. The self-mutilation which this so surprisingly persuasive definition requires does not appeal to me. There is no such thing as an

The main reason for exalting the method of linguistic analysis, however, seems to have been the following. It was felt that the so-called 'new way of ideas' of Locke, Berkeley, and Hume, that is to say the psychological or rather pseudo-psychological method of analysing our ideas and their origin in our senses, should be replaced by a more 'objective' and a less genetic method. It was felt that we should analyse words and their meanings or usages rather than 'ideas' or 'conceptions' or 'notions'; that we should analyse propositions or statements or sentences rather than 'thoughts' or 'beliefs' or 'judgments'. I readily admit that this replacement of Locke's 'new way of ideas' by a 'new way of words' was an advance, and one that was urgently needed.

It is understandable that those who once saw in the 'new way of ideas' the one true method of philosophy may thus have turned to the belief that the 'new way of words' is the one true method of philosophy. From this challenging belief I strongly dissent. But I will make only two critical comments on it. First, the 'new way of ideas' should never have been taken for the main method of philosophy, let alone for its one true method. Even Locke introduced it merely as a method of dealing with certain preliminaries (preliminaries for a science of ethics); and it was used by both Berkeley and Hume chiefly as a weapon for harrying their opponents. Their own interpretation of the world—the world of things and of men—which they were anxious to impart to us was never based upon this method. Berkeley did not base his religious views on it, nor Hume his political theories (though he based his determinism on it).

But my gravest objection to the belief that either the 'new way of ideas' or the 'new way of words' is the main method of epistemology—or perhaps even of philosophy—is this.

The problem of epistemology may be approached from two sides: (1) as the problem of ordinary or *common-sense knowledge*, or (2) as the problem of *scientific knowledge*. Those philosophers who favour the first approach think, rightly, that scientific knowledge can only be an extension of common-sense knowledge, and they also think, wrongly, that common-sense knowledge is the easier of the two to analyse. In this way these philosophers come to replace the 'new way of ideas' by an analysis of *ordinary language*—the language in which common-sense knowledge is formulated. They replace the analysis of vision or

essence of philosophy, to be distilled and condensed into a definition. A definition of the word 'philosophy' can only have the character of a convention, of an agreement; and I, at any rate, see no merit in the arbitrary proposal to define the word 'philosophy' in a way that may well prevent a student of philosophy from trying to contribute, *qua* philosopher, to the advancement of our knowledge of the world.

Also, it seems to me paradoxical that philosophers who take pride in specializing in the study of ordinary language nevertheless believe that they know enough about cosmology to be sure that it is in essence so different from philosophy that philosophy cannot make any contribution to it. And indeed they are mistaken. For it is a fact that purely metaphysical ideas—and therefore philosophical ideas—have been of the greatest importance for cosmology. From Thales to Einstein, from ancient atomism to Descartes's speculation about matter, from the speculations of Gilbert and Newton and Leibniz and Boscovic about forces to those of Faraday and Einstein about fields of forces, metaphysical ideas have shown the way.

Such are, in brief, my reasons for believing that even within the province of epistemology, the first approach mentioned above—that is to say, the analysis of knowledge by way of an analysis of ordinary language—is too narrow, and that it is bound to miss the most interesting problems.

Yet I am far from agreeing with all those philosophers who favour that other approach to epistemology—the approach by way of an analysis of scientific knowledge. In order to explain more easily where I disagree and where I agree, I am going to sub-divide the philosophers who adopt this second approach into two groups—the goats and the sheep, as it were.

The first group consists of those whose aim is to study 'the language of science', and whose chosen philosophical method is the construction of artificial model languages; that is to say, the construction of what they believe to be models of 'the language of science'.

The second group does not confine itself to the study of the language of science, or any other language, and it has no such chosen philosophical method. Its members philosophize in many different ways, because they have many different problems which they want to solve; and any method is welcome to them if they think that it may help them



to see their problems more clearly, or to hit upon a solution, however tentative.

I turn first to those whose chosen method is the construction of artificial models of the language of science. Historically, they too take their departure from the 'new way of ideas'. They too replace the (pseudo-) psychological method of the old 'new way' by linguistic analysis. But perhaps owing to the spiritual consolations offered by the hope for knowledge that is 'exact' or 'precise' or 'formalized', the chosen object of their linguistic analysis is 'the language of science' rather than ordinary language. Yet unfortunately there seems to be no such thing as 'the language of science'. It therefore becomes necessary for them to construct one. However, the construction of a full-scale working model of a language of science—one in which we could operate a real science such as physics—turns out a little difficult in practice; and for this reason we find them engaged in the construction of intricate working models in miniature—of vast systems of minute gadgets.

In my opinion, this group of philosophers gets the worst of both worlds. By their method of constructing miniature model languages they miss the most exciting problems of the theory of knowledge—those connected with its advancement. For the intricacy of the outfit bears no relation to its effectiveness, and practically no scientific theory of any interest can be expressed in these vast systems of minutiae. These model languages have no bearing on either science or common sense.

Indeed, the models of 'the language of science' which these philosophers construct have nothing to do with the language of modern science. This may be seen from the following remarks which apply to the three most widely known model languages. (They are referred to in notes 13 and 15 to appendix \*vii, and in note \*2 to section 38.) The first of these model languages lacks even the means of expressing identity. As a consequence, it cannot express an equation: it does not contain even the most primitive arithmetic. The second model language works only as long as we do not add to it the means of proving the usual theorems of arithmetic—for example, Euclid's theorem that there is no greatest prime number, or even the principle that every number has a successor. In the third model language—the most

elaborate and famous of all—mathematics can again not be formulated; and, what is still more interesting, there are no measurable properties expressible in it. For these reasons, and for many others, the three model languages are too poor to be of use to any science. They are also, of course, essentially poorer than ordinary languages, including even the most primitive ones.

The limitations mentioned were imposed upon the model languages simply because otherwise the solutions offered by the authors to their problems would not have worked. This fact can be easily proved, and it has been partly proved by the authors themselves. Nevertheless, they all seem to claim two things: (a) that their methods are, in some sense or other, capable of solving problems of the theory of scientific knowledge, or in other words, that they are applicable to science (while in fact they are applicable with any precision only to discourse of an extremely primitive kind), and (b) that their methods are ‘exact’ or ‘precise’. Clearly these two claims cannot both be upheld.

Thus the method of constructing artificial model languages is incapable of tackling the problems of the growth of our knowledge; and it is even less able to do so than the method of analysing ordinary languages, simply because these model languages are poorer than ordinary languages. It is a result of their poverty that they yield only the most crude and the most misleading model of the growth of knowledge—the model of an accumulating heap of observation statements.

I now turn to the last group of epistemologists—those who do not pledge themselves in advance to any philosophical method, and who make use, in epistemology, of the analysis of scientific problems, theories, and procedures, and, most important, of scientific discussions. This group can claim, among its ancestors, almost all the great philosophers of the West. (It can claim even the ancestry of Berkeley despite the fact that he was, in an important sense, an enemy of the very idea of rational scientific knowledge, and that he feared its advance.) Its most important representatives during the last two hundred years were Kant, Whewell, Mill, Peirce, Duhem, Poincaré, Meyerson, Russell, and—at least in some of his phases—Whitehead. Most of those who belong to this group would agree that scientific knowledge is the result of the growth of common-sense knowledge. But all of them discovered that scientific knowledge can be more easily studied than common-sense

knowledge. For it is *common-sense knowledge writ large*, as it were. Its very problems are enlargements of the problems of common-sense knowledge. For example, it replaces the Humean problem of 'reasonable belief' by the problem of the reasons for accepting or rejecting scientific theories. And since we possess many detailed reports of the discussions pertaining to the problem whether a theory such as Newton's or Maxwell's or Einstein's should be accepted or rejected, we may look at these discussions as if through a microscope that allows us to study in detail, and objectively, some of the more important problems of 'reasonable belief'.

This approach to the problems of epistemology gets rid (as do the other two mentioned) of the pseudo-psychological or 'subjective' method of the new way of ideas (a method still used by Kant). It suggests that we analyse scientific discussions, and also scientific problem situations. And so it can help us to understand the history of scientific thought.

I have tried to show that the most important of the traditional problems of epistemology—those connected with the *growth of knowledge*—transcend the two standard methods of linguistic analysis and require the analysis of scientific knowledge. But the last thing I wish to do, however, is to advocate another dogma. Even the analysis of science—the 'philosophy of science'—is threatening to become a fashion, a specialism. yet philosophers should not be specialists. For myself, I am interested in science and in philosophy only because I want to learn something about the riddle of the world in which we live, and the riddle of man's knowledge of that world. And I believe that only a revival of interest in these riddles can save the sciences and philosophy from narrow specialization and from an obscurantist faith in the expert's special skill, and in his personal knowledge and authority; a faith that so well fits our 'post-rationalist' and 'post-critical' age, proudly dedicated to the destruction of the tradition of rational philosophy, and of rational thought itself.

PENN, BUCKINGHAMSHIRE, Spring 1958.

## ACKNOWLEDGMENTS, 1960 and 1968

I wish to thank Mr. David G. Nicholls for communicating to me the admirable passage, now printed on page xvii, which he discovered among the Acton Manuscripts in the Library of Cambridge University (Add. MSS 5011:266). The reprint of the book gives me the welcome opportunity to quote this passage.

Summer 1959

In this *second English edition* four short *Addenda* have been added to the appendices. Minor mistakes have been corrected, and I have made a few linguistic improvements. Misprints have been corrected that were brought to my notice by Imre Lakatos, David Miller, and Alan Musgrave. They also suggested many new entries in the Index of Subjects. I am very grateful to them.

My greatest debt is to Paul Bernays who, shortly after this book had appeared in English, checked through my axiomatization of the probability calculus, especially the new appendix \*v. I value his approval more highly than I can express in words. It does not, of course, absolve me from bearing the sole responsibility for any mistake I may have made.

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# Part I

Introduction to the  
Logic of Science



# 1

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## 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.'<sup>1</sup>

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'.<sup>2</sup> 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;<sup>\*1</sup> 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

<sup>1</sup> 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.

<sup>2</sup> Reichenbach *ibid.*, p. 67.

<sup>\*1</sup> 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’.<sup>3</sup> ‘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’.<sup>4</sup>

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.\*<sup>2</sup>

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

<sup>3</sup> Cf. J. M. Keynes, *A Treatise on Probability*, 1921; O. Külpe, *Vorlesungen über Logik* (ed. by Selz, 1923); Reichenbach (who uses the term ‘probability implications’), *Axiomatik der Wahrscheinlichkeitsrechnung*, *Mathem. Zeitschr.* **34**, 1932; and elsewhere.

<sup>4</sup> Reichenbach, *Erkenntnis* **1**, 1930, p. 186.

\*<sup>2</sup> See also chapter 10, below, especially note 2 to section 81, and chapter \*ii of the Postscript for a fuller statement of this criticism.

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

Before I can elaborate this view (which might be called ‘deductivism’, in contrast to ‘inductivism’<sup>5</sup>) 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

<sup>5</sup> 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.'<sup>6</sup>

### 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, compatibility, 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

<sup>6</sup> 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, *Prinzipien 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*’<sup>\*1</sup> 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

<sup>\*1</sup> 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.<sup>1</sup>

This problem was known to Hume who attempted to solve it.<sup>2</sup> 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

<sup>1</sup> 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.

<sup>2</sup> 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.\*<sup>1</sup> 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.\*<sup>2</sup> 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'.\*<sup>3</sup>

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

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

\*<sup>2</sup> 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.

\*<sup>3</sup> 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<sup>3</sup> 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*<sup>4</sup> to elementary (or atomic) propositions, which he characterizes as descriptions or 'pictures of reality'<sup>5</sup> (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,<sup>6</sup> 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

<sup>3</sup> 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.

<sup>4</sup> 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*.

<sup>5</sup> Wittgenstein, *op. cit.*, Propositions 4.01; 4.03; 2.221.

<sup>6</sup> Cf. note 1 to section 2.

by Schlick<sup>\*4</sup> 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.'<sup>7</sup>

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.<sup>8</sup>

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

<sup>\*4</sup> 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.)

<sup>7</sup> 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*.

<sup>8</sup> 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.\*<sup>5</sup>

Thus anyone who envisages a system of absolutely certain, irrevocably true statements<sup>9</sup> 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'.<sup>10</sup> 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

\*<sup>5</sup> 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.)

<sup>9</sup> This is Dingler's view; cf. note 1 to section 19.

<sup>10</sup> 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’.<sup>11</sup>

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’.<sup>\*1</sup>

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

<sup>11</sup> 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*.

<sup>\*1</sup> Cf. appendix \*x.

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

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

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

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

## 6 FALSIFIABILITY AS A CRITERION OF DEMARCATION

The criterion of demarcation inherent in inductive logic—that is, the positivistic dogma of meaning—is equivalent to the requirement that all the statements of empirical science (or all 'meaningful' statements) must be capable of being finally decided, with respect to their truth and falsity; we shall say that they must be 'conclusively decidable'. This means that their form must be such that to *verify them* and to *falsify them* must both be logically possible. Thus Schlick says: '... a genuine statement must be capable of *conclusive verification*';<sup>1</sup> 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.'<sup>2</sup>

<sup>1</sup> Schlick, *Naturwissenschaften* 19, 1931, p. 150.

<sup>2</sup> Waismann, *Erkenntnis* 1, 1903, p. 229.

Now in my view there is no such thing as induction.\*<sup>1</sup> 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,\*<sup>2</sup> 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.\*<sup>3</sup> 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.<sup>3</sup>

\*<sup>1</sup> 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’.

\*<sup>2</sup> 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.

\*<sup>3</sup> 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.

<sup>3</sup> 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.\*<sup>4</sup> 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

\*<sup>4</sup> 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.<sup>4</sup>

<sup>4</sup> 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.'<sup>1</sup>

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*.<sup>\*1</sup>

<sup>1</sup> *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).

<sup>\*1</sup> 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).<sup>2</sup> To examine how these come about is the business of psychology. They may arise, for example, 'in accordance with the laws of association'.<sup>3</sup> Objective reasons too may serve as 'subjective causes of judging',<sup>4</sup> 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.<sup>5</sup>

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

<sup>2</sup> Ibid.

<sup>3</sup> 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).

<sup>4</sup> Cf. *Kritik der reinen Vernunft*, Methodenlehre, 2. Hauptstück, 3. Abschnitt (2nd edition, p. 849; English translation, chapter ii, section 3, p. 646).

<sup>5</sup> 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.<sup>6</sup> (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 intersubjective 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

<sup>6</sup> 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.