

## Chapter 5

### Selections from *The Logic of Scientific Discovery*

Karl Popper

#### A. SCIENTIFIC METHOD (1934)

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'<sup>1</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.

##### I. 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 jurist*). 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 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.

From *The Logic of Scientific Discovery*, copyright 1959 by Karl Popper. 13th impression 1987 Unwin Hyman (London). Editorial arrangement by David Miller, Popper Selections (Princeton University Press, 1985), pp. 133-161, copyright 1985 by David Miller for editorial arrangement. Reprinted by permission. Ed. note: All internal references in this chapter of the anthology are to Popper Selections.

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 here 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>2</sup>

## II. 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 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 a 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>3</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'. And a more detailed analysis of the methods of deductive testing shows that 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.

### III. Why Methodological Decisions Are Indispensable

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 [see selection 8 section I above]: I propose to adopt such rules as will ensure the testability of scientific statements; which is to say, their falsifiability.

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

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characteristic of empirical statements in their susceptibility to revision—in the fact that they can be criticized, and superseded by better ones; and who regard it as their task to analyze the characteristic ability of science to advance, and the characteristic manner in which a choice is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

I am quite ready to admit that there is a need for a purely logical analysis of theories, for an analysis which takes no account of how they change and develop. But this kind of analysis does not elucidate those aspects of the empirical sciences which I, for one, so highly prize. A system such as classical mechanics may be 'scientific' to any degree you like; but those who uphold it dogmatically—believing, perhaps, that it is their business to defend such a successful system against criticism as long as it is not

conclusively disproved—are adopting the very reverse of that critical attitude which in my view is the proper one for the scientist. In point of fact, no conclusive disproof of a theory can ever be produced; for it is always-possible to say that the experimental results are not reliable, or that the discrepancies which are asserted to exist between the experimental results and the theory are only apparent and that they will disappear with the advance of our understanding. (In the struggle against Einstein, both these arguments were often used in support of Newtonian mechanics, and similar arguments abound in the field of the social sciences.) If you insist on strict proof (or strict disproof) in the empirical sciences, you will never benefit from experience, and never learn from it how wrong you are.

If therefore we characterize empirical science merely by the formal or logical structure of its statements, we shall not be able to exclude from it that prevalent form of metaphysics which results from elevating an obsolete scientific theory into an incontrovertible truth.

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

#### IV The Naturalistic Approach to the Theory of Method

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

The positivist dislikes the idea that there should be meaningful problems outside the field of 'positive' empirical science—problems to be dealt with by a genuine philosophical theory. He dislikes the idea that there should be a genuine theory of knowledge, an epistemology or a methodology.<sup>4</sup> He wishes to see in the alleged philosophical problems mere 'pseudoproblems' or 'puzzles'. Now this wish of his—which, by the way, he does not express as a wish or a proposal but rather as a statement of fact—can always be gratified. For nothing is easier than to unmask a problem as 'meaningless' or 'pseudo'. All you have to do is to fix upon a conveniently narrow meaning for 'meaning', and you will soon be bound to say of any inconvenient question that you are unable to detect any meaning in it. Moreover, if you admit as meaningful none except problems in natural science, any debate about the concept of 'meaning' will also turn out to be meaningless. The dogma of meaning, once enthroned, is elevated forever above the battle. It can no longer be attacked. It has become (in Wittgenstein's own words) 'unassailable and definitive'.<sup>5</sup>

The controversial question whether philosophy exists, or has any right to exist, is almost as old as philosophy itself. Time and again an entirely new philosophical movement arises which finally unmask the old philosophical problems as pseudo-problems, and which confronts the wicked nonsense of philosophy with the good sense of meaningful, positive, empirical, science. And time and again do the despised defenders of 'traditional philosophy' try to explain to the leaders of the latest positivistic assault that the main problem of philosophy is the critical analysis of the appeal to the authority of 'experience'<sup>6</sup>—precisely that 'experience' which every latest discoverer of positivism is, as ever, artlessly taking for granted. To such objections, however, the positivist only replies with a shrug: they mean nothing to him, since they do not belong to empirical science, which alone is meaningful. 'Experience' for him is a programme, not a problem (unless it is studied by empirical psychology).

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

This view, according to which methodology is an empirical science in its turn—a study of the actual behaviour of scientists, or of the actual procedure of 'science'—may be described as *naturalistic*. A naturalistic methodology (sometimes called an 'inductive theory of science'<sup>7</sup>) has its value, no doubt. A student of the logic of science may well take an interest in it, and learn from it. But what I call 'methodology' should not be taken for an empirical science. I do not believe that it is possible to decide, by using the methods of an empirical science, such controversial questions as whether science actually uses a principle of induction or not. And my doubts increase when I remember that what is to be called a 'science' and who is to be called a 'scientist' must always remain a matter of convention or decision.

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

Thus I reject the naturalistic view. It is uncritical. Its upholders fail to notice that whenever they believe themselves to have discovered a fact, they have only proposed a convention.<sup>8</sup> Hence the convention is liable to turn into a dogma. This criticism of the naturalistic view applies not only to its criterion of meaning, but also to its idea of science, and consequently to its idea of empirical method.

#### V Methodological Rules as Conventions

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

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

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

(2) Once a hypothesis has been proposed and tested, and has proved its mettle, it may not be allowed to drop out without 'good reason'. A 'good reason' may be, for instance: replacement of the hypothesis by another which is better testable; or the falsification of one of the consequences of the hypothesis.<sup>9</sup>

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

[In selection 8] I tried to define empirical science with the help of the criterion of falsifiability; but as I was obliged to admit the justice of certain objections, I provided a methodological supplement to my definition. Just as chess might be defined by the rules proper to it, so empirical science may be defined by means of its methodological rules. In establishing these rules we may proceed systematically. First a supreme rule is laid down which serves as a kind of norm for deciding upon the remaining rules, and which is thus a rule of a higher type. It is the rule which says that the other rules of scientific procedure must be designed in such a way that they do not protect any statement in science against falsification.

Methodological rules are thus closely connected both with other methodological rules and with our criterion of demarcation. But the connection is not a strictly deductive or logical one.<sup>10</sup> It results, rather, from the fact that the rules are constructed with the aim of ensuring the applicability of our criterion of demarcation; thus their formulation and acceptance proceed according to a practical rule of a higher type. An example of this has been given above (rule I): theories which we decide not to submit to any further test would no longer be falsifiable. It is this systematic connection between the rules which makes it appropriate to speak of a theory of method. Admittedly the pronouncements of this theory are, as our examples show, for the most part conventions of a fairly obvious kind. Profound truths are not to be expected of methodology.<sup>11</sup> Nevertheless it may help us in many cases to clarify the logical situation, and even to solve some far-reaching problems which have hitherto proved intractable. One of these, for example, is the problem of deciding whether a probability statement should be accepted or rejected.<sup>12</sup>

It has often been doubted whether the various problems of the theory of knowledge stand in any systematic relation to one another, and also whether they can be treated systematically. I hope to show that these doubts are unjustified. The point is of some importance. My only reason for proposing my criterion of demarcation is that it is fruitful: that a great many points can be clarified and explained with its help. "Definitions are dogmas; only the conclusions drawn from them can afford us any new insight!" says Menger.<sup>13</sup> This is certainly true of the definition of the concept 'science'. It is only from the consequences of my definition of empirical science, and from the methodological decisions which depend upon this definition, that the scientist will be able to see how far it conforms to his intuitive idea of the goal of his endeavors.

The philosopher too will accept my definition as useful only if he can accept its consequences. We must satisfy him that these consequences enable us to detect inconsistencies and inadequacies in older theories of knowledge, and to trace these back to the fundamental assumptions and conventions from which they spring. But we must also satisfy him that our own proposals are not threatened by the same kind of difficulties. This method of detecting and resolving contradictions is applied also within science itself, but it is of particular importance in the theory of knowledge. It is by this method, if by any, that methodological conventions might be justified, and might prove their value.<sup>14</sup>

Whether philosophers will regard these methodological investigations as belonging to philosophy is, I fear, very doubtful, but this does not really matter much. Yet it may be worth mentioning in this connection that not a few doctrines which are metaphysical,

Selections from *The Logic of Scientific Discovery* 105

and thus certainly philosophical, could be interpreted as typical hypostatizations of methodological rules. An example of this is what is called 'the principle of causality'.<sup>15</sup> Another example is the problem of objectivity. For the requirement of scientific objectivity can also be interpreted as a methodological rule: the rule that only such statements may be introduced into science as are intersubjectively testable. It might indeed be said that the majority of the problems of theoretical philosophy, and the most interesting ones, can be reinterpreted in this way as problems of method.

Notes

1. J. Liebig, *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. P. Duhem, *The Aim and Structure of Physical Theory*, 1906 (English translation, 1954), held pronounced deductivist views. But there are also inductivist views to be found in Duhem's book, for example in the third chapter of Part I, where we are told that only experiment, induction, and generalization have produced Descartes's law of refraction (p. 34). See also V. Kraft, *Die Grundformen der Wissenschaftlichen Methoden*, 1925; and R. Carnap, *The Unity of Science*, 1934.

2. Address on Max Planck's sixtieth birthday. The passage quoted begins with the words, 'The supreme task of the physicist is to search for those highly universal laws ...'. See A. Einstein, *The World as I See It*, 1935 (translation by A. Harris), p. 125. The German word 'Einfühlung' is difficult to translate. Harris translates as 'sympathetic understanding of experience'. Similar ideas are found earlier in J. Liebig, *op. cit.*; see also E. Mach, *Prinzipien der Wärmerlehre*, 1896, pp. 443ff.

3. For this term see chapter X of *The Logic of Scientific Discovery*, and *Realism and the Aim of Science*, Part I, chapter IV.

4. In the two years before the first publication of *The Logic of Scientific Discovery* in 1934 it was the standing criticism raised by members of the Vienna Circle against my ideas that a theory of method which was neither an empirical science nor pure logic was impossible: what was outside these two fields was sheer nonsense. (The same view was still maintained by Wittgenstein in 1946; see note 8 on p. 69 of *Conjectures and Refutations*, and *Unended Quest*, section 26.) Later, the standing criticism - became anchored in the legend that I proposed to replace the verifiability criterion by a falsifiability criterion of meaning. See *Realism and the Aim of Science*, Part I, sections 19-22, and 'Replies to My Critics', sections 1-4.

5. [See note 17 to selection 6 above.]

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

7. H. Dingier, *Physik und Hypothese*, 1921; similarly, V. Kraft, *op. cit.*

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

9. Regarding the translation 'to prove one's mettle' for 'sich bewahren' see the first footnote to chapter X of *The Logic of Scientific Discovery*. The concept 'better testable' is analysed in *op. cit.*, chapter VI.

10. See pp. 58ff. of K. Menger, *Moral, Wille, und Weltgestaltung*, 1934.

11. I am still inclined to uphold something like this, even though such theorems as 'degree of corroboration' probability or my 'theorem on truth content' (see pp. 343-53 of P. K. Feyerabend & G. Maxwell, editors. *Mind, Matter, and Method*, 1966) are perhaps unexpected and not quite on the surface.

12. See *The Logic of Scientific Discovery*, chapter VIII, especially section 68 [and also selection 15 below].

## B. FALSIFICATIONISM VERSUS CONVENTIONALISM (1934)

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.

### I 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'.<sup>1</sup> Some of these objections have already been touched upon [in section v of the previous selection] they will now be considered a little more closely.

The source of the conventionalist philosophy would seem to be wonder at the austere 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.<sup>2</sup>

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. 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 Dingier 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.<sup>3</sup>

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";<sup>4</sup> 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). Or we may adopt a skeptical 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 Dingier, 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.

## II 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 analyzing 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 selection 8, section n, and selection 9, section V]. 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"'.<sup>5</sup>

A clear appreciation of what may be gained (and lost) by conventionalist methods was expressed, a hundred years before Poincare, 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.'<sup>5</sup>

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 countermove. 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 psychoanalysts, 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.<sup>6</sup> 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. An example of an unsatisfactory auxiliary hypothesis would be the contraction hypothesis of Fitzgerald and Lorentz which had no falsifiable consequences but merely<sup>7</sup> 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. [See selection 11, section n.]

Changes in explicit definitions, whereby the concepts of an axiom system are given a meaning in terms of a system of lower level universality, 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')<sup>8</sup>. (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.

### III 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.

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.<sup>9</sup> 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 nonempty subclasses. First, the class of all those basic statements with which it is 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.<sup>10</sup>

#### IV 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. This condition is necessary, but not sufficient; for 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. 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.<sup>11</sup>

Thus the basic statements play two different roles. 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.

#### Notes

1. The chief representatives of the school are Poincaré and Duhem (*The Aim and Structure of Physical Theory*, 1906; English translation, 1954). A recent adherent is H. Dingier (among his numerous works may be mentioned: *Das Experiment*, and *Der Zusammenbruch der Wissenschaft und das Primat der Philosophie*, 1926). The German Hugo Dingier 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 (p. 188) the possibility of crucial experiments, because he thinks of them as verifications, while I assert the possibility of crucial falsifying experiments. See *Conjectures and Refutations*, chapter 3, especially section v.

2. 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 H. Cornelius, 'Zur Kritik der Wissenschaftlichen Grundbegriffe', *Erkenntnis* 2, 1931, pp. 191-218, 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.

K. Ajdukiewicz appears to agree with Cornelius (see 'Sprache und Sinn', *Erkenntnis* 4, 1934, pp. 100-38, as well as the work there announced, 'Das Weltbild und die Begriffsapparatur', *ibid.*, pp. 259—87); he calls his standpoint 'radical conventionalism'.

3. For the problem of simplicity see *The Logic of Scientific Discovery*, chapter VII, especially section 46.

4. R. Carnap, 'Über die Aufgabe der Physik und die Anwendung des Grundsatzes der Einfachheit', *Kant-Studien* 28, 1923, pp. 90-107, especially p. 100.

5. See p. 193 of J. Black, *Lectures on the Elements of Chemistry*, volume I, 1803.

6. How degrees of falsifiability are to be estimated is explained in *The Logic of Scientific Discovery*, chapter VI.

7. This is a mistake, as pointed out by A. Grünbaum, 'The Falsifiability of the Lorentz-Fitzgerald Contraction Hypothesis', *British Journal for the Philosophy of Science* 10, 1959, pp. 48—50. Yet as this hypothesis is less testable than special relativity, it may illustrate degrees of adhocness.

8. See, for instance, pp. 22ff. of H. Hahn, *Logik, Mathematik, und Naturerkennen* (*Einheitswissenschaft* 2), 1933. In this connection, I only wish to say that in my view 'constituables' (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 p. 97 above, and the end of section I of selection II.]

9. Formulations 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.