4

FALSIFIABILITY

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

19 SOME CONVENTIONALIST OBJECTIONS

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

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

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

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

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

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

instruments satisfy the axioms of mechanics which we have decided to adopt.[2]

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.

[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 Cornelius (cf. *Zur Kritik der wissenschaftlichen Grundbegriffe*, *Erkenntnis* **2**, 1931, Number 4) the statement, 'The melting point of lead is about 335°C.' is part of the definition of the concept 'lead' (suggested by inductive experience) and cannot therefore be refuted. A substance otherwise resembling lead but with a different melting point would simply not be lead. But according to my view the statement of the melting point of lead is, *qua* scientific statement, synthetic. It asserts, among other things, that an element with a given atomic structure (atomic number 82) always has this melting point, whatever name we may give to this element.

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

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

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

Thus my conflict with the conventionalists is not one that can be ultimately settled merely by a detached theoretical discussion. And yet it is possible I think to extract from the conventionalist mode of thought certain interesting arguments against my criterion of demarcation; for instance the following. I admit, a conventionalist might say, that the theoretical systems of the natural sciences are not verifiable, but I assert that they are not falsifiable either. For there is always the possibility of '. . . attaining, for any chosen axiomatic system, what is called its "correspondence with reality" ';[3] and this can be done in a number of ways (some of which have been suggested above). Thus we may introduce *ad hoc* hypotheses. Or we may modify the so-called 'ostensive definitions' (or the 'explicit definitions' which may replace them as shown in section 17). Or we may adopt a sceptical attitude as to the reliability of the experimenter whose observations, which threaten our system, we may exclude from science on the ground that they are insufficiently supported, unscientific, or not objective, or even

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

on the ground that the experimenter was a liar. (This is the sort of attitude which the physicist may sometimes quite rightly adopt towards alleged occult phenomena.) In the last resort we can always cast doubt on the acumen of the theoretician (for example if he does not believe, as does Dingler, that the theory of electricity will one day be derived from Newton's theory of gravitation).

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

20 METHODOLOGICAL RULES

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

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

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

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

As regards *auxiliary hypotheses* we propose to lay down the rule that only those are acceptable whose introduction does not diminish the degree of falsifiability or testability of the system in question, but, on the contrary, increases it. (How degrees of falsifiability are to be estimated will be explained in sections 31 to 40.) If the degree of falsifiability is increased, then introducing the hypothesis has actually strengthened the theory: the system now rules out more than it did previously: it prohibits more. We can also put it like this. The introduction of an auxiliary hypothesis should always be regarded as an attempt to construct a new system; and this new system should then always be judged on the issue of whether it would, if adopted, constitute a real advance in our knowledge of the world. An example of an auxiliary hypothesis which is eminently acceptable in this sense is Pauli's exclusion principle (cf. section 38). An example of an unsatisfactory auxiliary hypothesis would be the contraction hypothesis of Fitzgerald and Lorentz which had no falsifiable consequences but merely[*1] served to restore the agreement between theory and experiment—mainly the

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

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

findings of Michelson and Morley. An advance was here achieved only by the theory of relativity which predicted new consequences, new physical effects, and thereby opened up new possibilities for testing, and for falsifying, the theory. Our methodological rule may be qualified by the remark that we need not reject, as conventionalistic, every auxiliary hypothesis that fails to satisfy these standards. In particular, there are *singular* statements which do not really belong to the theoretical system at all. They are sometimes called 'auxiliary hypotheses', and although they are introduced to assist the theory, they are quite harmless. (An example would be the assumption that a certain observation or measurement which cannot be repeated may have been due to error. Cf. note 6 to section 8, and sections 27 and 68.)

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

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

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

21 LOGICAL INVESTIGATION OF FALSIFIABILITY

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

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

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

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

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

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

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

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

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

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

22 FALSIFIABILITY AND FALSIFICATION

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

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

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

[1] The falsifying hypothesis can be of a very low level of universality (obtained, as it were, by generalising the individual co-ordinates of a result of observation; as an instance I might cite Mach's so-called 'fact' referred to in section 18). Even though it is to be inter-subjectively testable, it need not in fact be a strictly universal statement. Thus to falsify the statement 'All ravens are black' the inter-subjectively testable statement that there is a family of white ravens in the zoo at New York would suffice. *All this shows the urgency of replacing a falsified hypothesis by a better one. In most cases we have, before falsifying

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

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

a hypothesis, another one up our sleeves; for the falsifying experiment is usually a *crucial experiment* designed to decide between the two. That is to say, it is suggested by the fact that the two hypotheses differ in some respect; and it makes use of this difference to refute (at least) one of them.

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

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

23 OCCURRENCES AND EVENTS

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

which yields, by 'importation',

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

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

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

24 FALSIFIABILITY AND CONSISTENCY

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

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

Besides being consistent, an empirical system should satisfy a further condition: it must be *falsifiable*. The two conditions are to a large extent analogous.[1] Statements which do not satisfy the condition of

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

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

consistency fail to differentiate between any two statements within the totality of all possible statements. Statements which do not satisfy the condition of falsifiability fail to differentiate between any two statements within the totality of all possible empirical basic statements.