WHEN I received the list of participants in this course and realized that I had been asked to speak to philosophical colleagues I thought, after some hesitation and consultation, that you would probably prefer me to speak about those problems which interest me most, and about those developments with which I am most intimately acquainted. I therefore decided to do what I have never done before: to give you a report on my own work in the philosophy of science, since the autumn of 1919 when I first began to grapple with the problem, “When should a theory be ranked as scientific?” or “Is there a criterion for the scientific character or status of a theory?”

The problem which troubled me at the time was neither, “When is a theory true?” nor, “When is a theory acceptable?” My problem was different. I wished to distinguish between science and pseudo-science; knowing very well that science often errs, and that pseudo-science may happen to stumble on the truth.

I knew, of course, the most widely accepted answer to my problem: that science is distinguished from pseudo-science—or from “metaphysics”—by its empirical method, which is essentially inductive, proceeding from observation or experiment. But this did not satisfy me. On the contrary, I often formulated my problem as one of distinguishing between a genuinely empirical method and a non-empirical or even a pseudo-empirical method—that is to say, a method which, although it appeals to observation and experiment, nevertheless...

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A lecture given at Peterhouse, Cambridge, in Summer 1933, as part of a course on developments and trends in contemporary British philosophy, organized by the British Council; originally published under the title “Philosophy of Science: a Personal Report” in British Philosophy in Mid-Century, ed. C. A. Mace, 1937.
Eclipse observations which in 1919 brought the first important students to claims to scientific status. My problem perhaps first took the simple form, Einstein’s theory of gravitation. It was a great experience for us, and one among students at that time. I myself happened to come into personal contact who introduced me to the study of this theory. We all—the small circle or theories which interested me—Einstein’s theory of relativity was no doubt ‘What is wrong with sciences, had in fact more in common with primitive

analysis, and individual psychology; Freud’s psycho-analysis, and Alfred Adler’s so-called ‘individual psychology’.

There was a lot of popular nonsense talked about these theories, and especially about relativity (as still happens even today), but I was fortunate in those who introduced me to the study of this theory. We all—the small circle of students to which I belonged—were thrilled with the result of Eddington’s eclipse observations which in 1919 brought the first important confirmation of Einstein’s theory of gravitation. It was a great experience for us, and one which had a lasting influence on my intellectual development.

The three other theories I have mentioned were also widely discussed among students at that time. I myself happened to come into personal contact with Alfred Adler, and even to co-operate with him in his social work among the children and young people in the working-class districts of Vienna where he had established social guidance clinics.

It was during the summer of 1919 that I began to feel more and more dissatisfied with these three theories—the Marxist theory of history, psycho-analysis, and individual psychology; and I began to feel dubious about their claims to scientific status. My problem perhaps first took the simple form, ‘What is wrong with Marxism, psycho-analysis, and individual psychology? Why are they so different from physical theories, from Newton’s theory, and especially from the theory of relativity?’

To make this contrast clear I should explain that few of us at the time would have said that we believed in the truth of Einstein’s theory of gravitation. This shows that it was not my doubting the truth of those other three theories which bothered me, but something else. Yet neither was it that I merely felt mathematical physics to be more exact than the sociological or psychological type of theory. Thus what worried me was neither the problem of truth, at that stage at least, nor the problem of exactness or measurability. It was rather that I felt that these other three theories, though posing as sciences, had in fact more in common with primitive myths than with science; that they resembled astrology rather than astronomy.

I found that those of my friends who were admirers of Marx, Freud, and Adler, were impressed by a number of points common to these theories, and especially by their apparent explanatory power. These theories appeared to be able to explain practically everything that happened within the fields to which they referred. The study of any of them seemed to have the effect of an intellectual conversion or revelation, opening your eyes to a new truth hidden from those not yet initiated. Once your eyes were thus opened you saw confirming instances everywhere: the world was full of verifications of the theory. Whatever happened always confirmed it. Thus its truth appeared manifest; and unbelievers were clearly people who did not want to see the manifest truth; who refused to see it, either because it was against their class interest, or because of their repressions which were still ‘un-analysed’ and crying aloud for treatment.

The most characteristic element in this situation seemed to me the incessant stream of confirmations, of observations which ‘verified’ the theories in question; and this point was constantly emphasized by their adherents. A Marxist could not open a newspaper without finding on every page confirming evidence for his interpretation of history; not only in the news, but also in its presentation—which revealed the class bias of the paper—and especially of course in what the paper did not say. The Freudian analysts emphasized that their theories were constantly verified by their ‘clinical observations’. As for Adler, I was much impressed by a personal experience. Once, in 1919, I reported to him a case which to me did not seem particularly Adlerian, but which he found no difficulty in analysing in terms of his theory of inferiority feelings, although he had not even seen the child. Slightly shocked, I asked him how he could be so sure. ‘Because of my thousandfold experience,’ he replied, whereupon I could not help saying: ‘And with this new case, I suppose, your experience has become thousand-and-one-fold.’

What I had in mind was that his previous observations may not have been much sounder than this new one; that each in its turn had been interpreted in the light of ‘previous experience’, and at the same time counted as additional confirmation. What, I asked myself, did it confirm? No more than that a case could be interpreted in the light of the theory. But this meant very little, I reflected, since every conceivable case could be interpreted in the light of Adler’s theory, or equally of Freud’s. I may illustrate this by two very different examples of human behaviour: that of a man who pushes a child into the water with the intention of drowning it; and that of a man who sacrifices his life in an attempt to save the child. Each of these two cases can be explained with equal ease in Freudian and in Adlerian terms. According to Freud the first man suffered from repression (say, of some component of his Oedipus complex), while the second man had achieved sublimation. According to Adler the first man suffered from feelings of inferiority (producing perhaps the need to prove to himself that he dared to commit some crime), and so did the second man (whose need was to prove to himself that he dared to rescue the child). I could not think of any human behaviour which could not be interpreted in terms of either theory. It was precisely this fact—that they always fitted, that they were always confirmed—which in the eyes of their admirers constituted the strongest argument in favour of these theories. It began to dawn on me that this apparent strength was in fact their weakness.

With Einstein’s theory the situation was strikingly different. Take one
CONJECTURES

typical instance—Einstein's prediction, just then confirmed by the findings of
Eddington's expedition. Einstein's gravitational theory had led to the result
that light must be attracted by heavy bodies (such as the sun), precisely as
material bodies were attracted. As a consequence it could be calculated that
light from a distant fixed star whose apparent position was close to the sun
would reach the earth from such a direction that the star would seem to be
slightly shifted away from the sun; or, in other words, that stars close to the
sun would look as if they had moved a little away from the sun, and from one
another. This is a thing which cannot normally be observed since such star
are rendered invisible in daytime by the sun's overwhelming brightness; but
during an eclipse it is possible to take photographs of them. If the same con­
stellation is photographed at night one can measure the distances on the two
photographs, and check the predicted effect.

Now the impressive thing about this case is the risk involved in a pre­
diction of this kind. If observation shows that the predicted effect is definitely
absent, then the theory is simply refuted. The theory is incompatible with
certain possible results of observation—in fact with results which everybody
before Einstein would have expected. This is quite different from the situation
I have previously described, when it turned out that the theories in question
were compatible with the most divergent human behaviour, so that it was
practically impossible to describe any human behaviour that might not be
claimed to be a verification of these theories.

These considerations led me in the winter of 1919–20 to conclusions
which I may now reformulate as follows.

1. It is easy to obtain confirmations, or verifications, for nearly every
theory—if we look for confirmations.

2. Confirmations should count only if they are the result of risky pre­
dictions; that is to say, if, unenlightened by the theory in question, we should
have expected an event which was incompatible with the theory—an event
which would have refuted the theory.

3. Every 'good' scientific theory is a prohibition: it forbids certain things
to happen. The more a theory forbids, the better it is.

4. A theory which is not refutable by any conceivable event is non­
scientific. Irrefutability is not a virtue of a theory (as people often think) but
a vice.

5. Every genuine test of a theory is an attempt to falsify it, or to refute it.
Testability is falsifiability; but there are degrees of testability: some theories
are more testable, more exposed to refutation, than others; they take, as it were,
greater risks.

6. Confirming evidence should not count except when it is the result of a
genuine test of the theory; and this means that it can be presented as a serious
but unsuccessful attempt to falsify the theory. (I now speak in such cases of
'corroborating evidence'.)

I. SCIENCE: CONJECTURES AND REFUTATIONS

(7) Some genuinely testable theories, when found to be false, are still up­
held by their admirers—for example by introducing ad hoc some auxiliary
assumption, or by re-interpreting the theory ad hoc in such a way that it
escapes refutation. Such a procedure is always possible, but it rescues the
theory from refutation only at the price of destroying, or at least lowering,
its scientific status. (I later described such a rescuing operation as a 'con­
tentional twist' or 'a conventionalist stratagem'.)

One can sum up all this by saying that the criterion of the scientific status
of a theory is its falsifiability, or refutability, or testability.

II

I may perhaps exemplify this with the help of the various theories so far
mentioned. Einstein's theory of gravitation clearly satisfied the criterion of
falsifiability. Even if our measuring instruments at the time did not allow us
to pronounce on the results of the tests with complete assurance, there was
clearly a possibility of refuting the theory.

Astrology did not pass the test. Astrologers were greatly impressed, and
misled, by what they believed to be confirming evidence—so much so that
they were quite unimpressed by any unfavourable evidence. Moreover, by
making their interpretations and prophecies sufficiently vague they were able
to explain away anything that might have been a refutation of the theory had
the theory and the prophecies been more precise. In order to escape falsifica­
tion they destroyed the testability of their theory. It is a typical soothsayer's
trick to predict things so vaguely that the predictions can hardly fail: that
they become irrefutable.

The Marxist theory of history, in spite of the serious efforts of some of its
founders and followers, ultimately adopted this soothsaying practice. In
some of its earlier formulations (for example in Marx's analysis of the
character of the 'coming social revolution') their predictions were testable,
and in fact falsified. Yet instead of accepting the refutations the followers of
Marx re-interpreted both the theory and the evidence in order to make them
agree. In this way they rescued the theory from refutation; but they did so at
the price of adopting a device which made it irrefutable. They thus gave a
'conventionalist twist' to the theory; and by this stratagem they destroyed its
much advertised claim to scientific status.

The two psycho-analytic theories were in a different class. They were
simply non-testable, irrefutable. There was no conceivable human behaviour
which could contradict them. This does not mean that Freud and Adler were
not seeing certain things correctly: I personally do not doubt that much of
what they say is of considerable importance, and may well play its part one
day in a psychological science which is testable. But it does mean that those
'clinical observations' which analysts naively believe confirm their theory
cannot do this any more than the daily confirmations which astrologers find

2 See, for example, my Open Society and Its Enemies, ch. 15, section ii, and notes
13–14.
in their practice. And for Freud’s epic of the Ego, the Super-ego, and the Id, no substantially stronger claim to scientific status can be made for it than for Homer’s collected stories from Olympia. These theories describe some things in the manner of myths. They contain most interesting psychological suggestions, but not in a testable form.

At the same time I realized that such myths may be developed, and become testable; that historically speaking all—or very nearly all—scientific theories originate from myths, and that a myth may contain important anticipations of scientific theories. Examples are Empedocles’ theory of evolution by trial and error, or Parmenides’ myth of the unchanging block universe in which nothing ever happens and which, if we add another dimension, becomes Einstein’s block universe (in which, too, nothing ever happens, since everything is, four-dimensionally speaking, determined and laid down from the beginning). I thus felt that if a theory is found to be non-scientific, or ‘metaphysical’ (as we might say), it is not thereby found to be unimportant, or insignificant, or ‘meaningless’, or ‘nonsensical’. But it cannot claim to be backed by empirical evidence in the scientific sense—although it may easily be, in some genetic sense, the ‘result of observation’.

(There were a great many other theories of this pre-scientific or pseudo-scientific character, some of them, unfortunately, as influential as the Marxist interpretation of history; for example, the racist interpretation of history—another of those impressive and all-explanatory theories which act upon weak minds like revelations.)

Thus the problem which I tried to solve by proposing the criterion of falsifiability was neither a problem of meaningfulness or significance, nor a problem of truth or acceptability. It was the problem of drawing a line (as well as this can be done) between the statements, or systems of statements, of the empirical sciences, and all other statements—whether they are of a religious or of a metaphysical character, or simply pseudo-scientific. Years later—it must have been in 1928 or 1929—I called this first problem mine the ‘problem of demarcation’. The criterion of falsifiability is a solution to this problem of demarcation, for it says that statements or systems of statements, in order to be ranked as scientific, must be capable of conflicting with possible, or conceivable, observations.

Today I know, of course, that this criterion of demarcation—the criterion of testability, of falsifiability, or refutability—is far from obvious; for even now its significance is seldom realized. At that time, in 1920, it seemed to me almost trivial, although it solved for me an intellectual problem which had worried me deeply, and one which also had obvious practical consequences (for example, political ones). But I did not yet realize its full implications, or its philosophical significance. When I explained it to a fellow student of the Mathematics Department (now a distinguished mathematician in Great Britain), he suggested that I should publish it. At the time I thought this absurd; for I was convinced that my problem, since it was so important for me, must have agitated many scientists and philosophers who would surely have reached my rates, obvious solution. That this was not the case I learnt from Wittgenstein’s work, and from its reception; and so I published my results thirteen years later in the form of a criticism of Wittgenstein’s criterion of meaningfulness.

Wittgenstein, as you all know, tried to show in the Tractatus (see for example his propositions 6.53; 6.54; and 5) that all so-called philosophical or metaphysical propositions were actually non-propositions or pseudo-propositions: that they were senseless or meaningless. All genuine (or meaningful) propositions were truth functions of the elementary or atomic propositions which described ‘atomic facts’, i.e.—facts which can in principle be ascertained by observation. In other words, meaningful propositions were fully reducible to elementary or atomic propositions which were simple statements describing possible states of affairs, and which could in principle be established or rejected by observation. If we call a statement an ‘observation statement’ not only if it states an actual observation but also if it states anything that may be observed, we shall have to say (according to the Tractatus, § and 4.52) that every genuine proposition must be a truth-function of, and
CONJECTURES

therefore deducible from observation statements. All other apparent propositions will be meaningless pseudo-propositions; in fact they will be nothing but nonsensical gibberish.

This idea was used by Wittgenstein for a characterization of science, as opposed to philosophy. We read (for example in 4.15, where natural science is taken to stand in opposition to philosophy): 'The totality of true propositions is the total natural science (or the totality of the natural sciences). This means that the propositions which belong to science are those deducible from true observation statements; they are those propositions which can be verified by true observation statements. Could we know all true observation statements, we should also know all that may be asserted by natural science.

This amounts to a crude verifiability criterion of demarcation. To make it slightly less crude, it could be amended thus: 'The statements which may possibly fall within the province of science are those which may possibly be verified by observation statements; and these statements, again, coincide with the class of all genuine or meaningful statements.' For this approach, then, verifiability, meaningfulness, and scientific character all coincide.

I personally was never interested in the so-called problem of meaning; on the contrary, it appeared to me a verbal problem, a typical pseudo-problem. I was interested only in the problem of demarcation, i.e. in finding a criterion of the scientific character of theories. It was just this interest which made me see at once that Wittgenstein's verifiability criterion of meaning was intended to play the part of a criterion of demarcation as well; and which made me see that, as such, it was totally inadequate, even if all misgivings about the dubious concept of meaningfulness were set aside. For Wittgenstein's criterion of demarcation—to use my own terminology in this context—is verifiability, or deducibility from observation statements. But this criterion is too narrow (and too wide): it excludes from science practically everything that is, in fact, characteristic of it (while failing in effect to exclude astrology). No scientific theory can ever be deduced from observation statements, or be described as a truth-function of observation statements.

All this I pointed out on various occasions to Wittgensteinians and members of the Vienna Circle. In 1931-2 I summarized my ideas in a largish book (read by several members of the Circle but never published; although part of it was incorporated in my Logic of Scientific Discovery); and in 1933 I published a letter to the Editor of Erkenntnis in which I tried to compress into two pages my ideas on the problems of demarcation and induction.5 In this letter I personally was never interested in the so-called problem of meaning; on the contrary, it appeared to me a verbal problem, a typical pseudo-problem. I was interested only in the problem of demarcation, i.e. in finding a criterion of the scientific character of theories. It was just this interest which made me see at once that Wittgenstein's verifiability criterion of meaning was intended to play the part of a criterion of demarcation as well; and which made me see that, as such, it was totally inadequate, even if all misgivings about the dubious concept of meaningfulness were set aside. For Wittgenstein's criterion of demarcation—to use my own terminology in this context—is verifiability, or deducibility from observation statements. But this criterion is too narrow (and too wide): it excludes from science practically everything that is, in fact, characteristic of it (while failing in effect to exclude astrology). No scientific theory can ever be deduced from observation statements, or be described as a truth-function of observation statements.

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1 SCIENCE: CONJECTURES AND REFUTATIONS

and elsewhere I described the problem of meaning as a pseudo-problem, in contrast to the problem of demarcation. But my contribution was classified by members of the Circle as a proposal to replace the verifiability criterion of meaning by a falsifiability criterion of meaning—which effectively made nonsense of my views.6 My protests that I was trying to solve, not their pseudo-problem of meaning, but the problem of demarcation, were of no avail.

My attacks upon verification had some effect, however. They soon led to complete confusion in the camp of the verificationist philosophers of sense and nonsense. The original proposal of verifiability as the criterion of meaning was at least clear, simple, and forceful. The modifications and shifts which were now introduced were the very opposite.7 This, I should say, is now seen even by the participants. But since I am usually quoted as one of them I wish to repeat that although I created this confusion I never participated in it. Neither falsifiability nor testability were proposed by me as criteria of meaning; and although I may plead guilty to having introduced both terms into the discussion, it was not I who introduced them into the theory of meaning.

Criticism of my alleged views was widespread and highly successful. I have yet to meet a criticism of my views.8 Meanwhile, testability is being widely accepted as a criterion of demarcation.

part of his system. And after describing in detail my theory of tests, Carnap sums up his views as follows (p. 228): 'After weighing the various arguments here discussed, which bear no relation to me to the second language form with procedure B—that in the form here described—is the most adequate among the forms of scientific language at present advocated in the theory of knowledge.' This paper of Carnap's contained the first published report of my theory of critical testing. (See also my critical remarks in L.Sc.D., note 1 to section 29, on p. 104, where the date '1933' should read '1932'; and ch. 11, below, text to note 39.)

Wittgenstein's example of a nonsensical pseudo-proposition is: 'Socrates is identical'. Obviously, 'Socrates is not identical' must also be nonsensical. Thus the negation of any nonsense will be nonsense, and the negation of a meaningful statement will be meaningless, but the negation of a testable (or falsifiable) statement need not be testable, as was pointed out, first, in my L.Sc.D., e.g., pp. 38 f. and later by my critics. The answer is that by taking testability as a criterion of meaning rather than of demarcation it can easily be imagined.

The most recent example of the way in which the history of this problem is misunderstood is A. R. White's 'Note on Meaning and Verification', Mind, 63, 1954, pp. 66 ff. I. L. Evans' article, Mind, 62, 1953, pp. 1 ff., which Mr. White criticizes, is excellent in my opinion, and unusually perceptive. Understandably enough, neither of the authors can quite reconstruct the story. (Some hints may be found in my Open Society, Vol. 1, 1952, pp. 38 f.)

In L.Sc.D. I discussed, and replied to, some likely objections which afterwards were indeed raised, without reference to my replies. One of them is the contention that the falsification of a natural law is just as impossible as its verification. The answer is that this objection mixes two entirely different levels of analysis (like the objection that mathematical demonstrations are impossible since checking, no matter how often repeated, can never make it quite certain that we have not overlooked a mistake). On the one hand, there is a logical asymmetry: one singular statement—say, the theorem that mathematical demonstrations are impossible since checking, no matter how often repeated, can never make it quite certain that we have not overlooked a mistake. On the other hand, we may hesitate to accept any statement, even the simplest observation statement; and we may point out that every statement involves interpretation in the light of theories, and that it is therefore uncertain. This does not affect the fundamental asymmetry, but it is important: most dissenters of the heart before Harvey observed the wrong things—those, which they expected to see. There can never be anything like a completely safe observation,
I have discussed the problem of demarcation in some detail because I believe that its solution is the key to most of the fundamental problems of the philosophy of science. I am going to give you later a list of some of these other problems, but only one of them—the problem of induction—can be discussed here at any length.

I had become interested in the problem of induction in 1923. Although this problem is very closely connected with the problem of demarcation, I did not fully appreciate the connection for about five years.

I approached the problem of induction through Hume. Hume, I felt, was perfectly right in pointing out that induction cannot be perfectly right in pointing out that induction cannot be

He was, I think, somewhat wrong (we may not have heard the clock tick, but we may hear the observation statement: if we have had no experience, resemble those, of which we have had experience). Consequently, even after the observation of the frequent or constant conjunction of objects, we have no reason to draw any inference concerning any object beyond those of which we have had experience. For 'should it be said that we have experience?'—experience teaching us that objects constantly conjoined with certain other objects continue to be so conjoined—then, Hume says, 'I would renew my question, why from this experience we form any conclusion beyond those past instances, of which we have had experience?'. In other words, an attempt to justify the practice of induction by an appeal to experience must lead to an infinite regress. As a result, we can say that theories can never be inferred from observation statements, or rationally justified by them.

I found Hume's refutation of inductive inference clear and conclusive. But I felt completely dissatisfied with his psychological explanation of induction in terms of custom or habit.

It has often been noticed that this explanation of Hume's is philosophically not very satisfactory. It is, however, without doubt intended as a psychological rather than a philosophical theory; for it tries to give a causal explanation of a psychological fact—the fact that we believe in laws, in statements asserting regularities or constantly conjoined kinds of events—by asserting that this fact is due to (i.e., constantly conjoined with) custom or habit. But even this reformulation of Hume's theory is still unsatisfactory; for what I have just called 'a psychological fact' may itself be described as a custom or habit—free from the dangers of misinterpretation. (This is one of the reasons why the theory of induction does not work.)

The 'empirical basis' consists largely of a mixture of theories of a lower degree of universality (of 'reproducible effects'). But the fact remains that, relative to whatever basis the investigator may accept (at his peril), he can test his theory only by trying to refute it.

I SCIENCE: CONJECTURES AND REPUTATIONS

the custom or habit of believing in laws or regularities; and it is neither very surprising nor very enlightening to hear that such a custom or habit must be explained as due to, or conjoined with, a custom or habit (even though a different one). Only when we remember that the words 'custom' and 'habit' are used by Hume, as they are in ordinary language, not merely to describe regular behaviour, but rather to theorize about its origin (ascribed to frequent repetition), can we reformulate his psychological theory in a more satisfactory way. We can then say that, like other habits, our habit of believing in laws is the result of frequent repetition—of the repeated observation that things of a certain kind are constantly conjoined with things of another kind.

This genetic-philosophical theory is, as indicated, incorporated in ordinary language, and it is therefore hardly as revolutionary as Hume thought. It is not very satisfactory. It is, however, without much else, that the psychological theory was mistaken; and that it was in fact refutable on purely logical grounds.

Hume's psychology, which is the popular psychology, was mistaken, I felt, about at least three different things: (a) the typical result of repetition; (b) the genesis of habits; and especially (c) the character of those experiences or modes of behaviour which may be described as 'believing in a law' or 'expecting a law-like succession of events'.

(a) The typical result of repetition—say, of repeating a difficult passage on the piano—is that movements which at first needed attention are in the end executed without attention. We might say that the process becomes radical, and cease to be conscious: it becomes 'physiological'. Such a process, far from creating a conscious expectation of law-like succession, or a belief in a law, may on the contrary begin with a conscious belief and destroy it by making it superfluous. In learning to ride a bicycle we may start with the belief that we can avoid falling if we steer in the direction in which we threaten to fall, and this belief may be useful for guiding our movements. After sufficient practice we may forget the rule; in any case, we do not need it any longer. On the other hand, even if it is true that repetition may create unconscious expectations, these become conscious only if something goes wrong (we may not have heard the clock tick, but we may hear that it has stopped).

(b) Habits or customs do not, as a rule, originate in repetition. Even the habit of walking, or of speaking, or of feeding at certain hours, begins before repetition can play any part whatever. We may say, if we like, that they deserve to be called 'habits' or 'customs' only after repetition has played its typical part; but we must not say that the practices in question originated as the result of many repetitions.

(c) Belief in a law is not the same thing as behaviour which betrays an expectation of a law-like succession of events; but these two are sufficiently closely connected to be treated together. They may, perhaps, in exceptional cases, result from a mere repetition of sense impressions (as in the case of the
CONJECTURES

stepping clock). I was prepared to concede this, but I contended that normally, and in most cases of any interest, they cannot be so explained. As Hume admits, even a single striking observation may be sufficient to create a belief or an expectation—a fact which he tries to explain as due to an inductive habit, formed as the result of a vast number of long repetitive sequences which had been experienced at an earlier period of life. But this, I contended, was merely his attempt to explain away unfavourable facts which threatened to come back to the source of the smell and to an expectation—a fact which he tries to explain as due to an inductive fact.

We are not only romancing, but forgetting that in the clever puppies’ short lives there must be room not only for repetition but also for a great deal of novelty, and consequently of non-repetition.

But it is not only that certain empirical facts do not support Hume; there are decisive arguments of a purely logical nature against his psychological theory.

The central idea of Hume’s theory is that of repetition, based upon similarity (or ‘resemblance’). This idea is used in a very uncrirical way. We are led to think of the water-drop that hollows the stone: of sequences of unquestionably like events slowly forcing themselves upon us, as does the tick of the clock. But we ought to realize that in a psychological theory such as Hume’s, only repetition-for-us, based upon similarity-for-us, can be allowed to have any effect upon us. We must respond to situations as if they were equivalent; take them as similar; interpret them as repetitions. The clever puppies, we may assume, showed by their response, their way of acting or of reacting, that they recognized or interpreted the second situation as a repetition of the first; that they expected its main element, the objectionable smell, to be present. The situation was a repetition-for-them because they responded to it by anticipating its similarity to the previous one.

This apparently psychological criticism has a purely logical basis which may be summed up in the following simple argument. (It happens to be the one from which I originally started my criticism.) The kind of repetition envisaged by Hume can never be perfect; the cases he has in mind cannot be cases of perfect sameness; they can only be cases of similarity. Thus they are repetitions only from a certain point of view. (What has the effect upon me of a repetition may not have this effect upon a spider.) But this means that, for logical reasons, there must always be a point of view—such as a system of

11 Treatise, section iii; section xv, rule 4.
CONJECTURES

cast out the logical theory of induction by repetition he struck a bargain with
common sense, meekly allowing the re-entry of induction by repetition, in
the guise of a psychological theory. I proposed to turn the tables upon this
theory of Hume's. Instead of explaining our propensity to expect regularities
as the result of repetition, I proposed to explain repetition-for-us as the
result of our propensity to expect regularities and to search for them.

Thus I was led by purely logical considerations to replace the psychological
theory of induction by the following view. Without waiting, passively, for
repetitions to impress or impose regularities upon us, we actively try to im-
pose regularities upon the world. We try to discover similarities in it, and to
interpret it in terms of laws invented by us. Without waiting for premises we
jump to conclusions. These may have to be discarded later, should observa-
tions show that they are wrong.

This was a theory of trial and error—of conjectures and refutations. It
made it possible to understand why our attempts to force interpretations upon
the world were logically prior to the observation of similarities. Since there
were logical reasons behind this procedure, I thought that it would apply in
the field of science also; that scientific theories were not the digest of observa-
tions, but that they were inventions—conjectures boldly put forward for trial,
to be eliminated if they clashed with observations; with observations which
were rarely accidental but as a rule undertaken with the definite intention of
testing a theory by obtaining, if possible, a decisive refutation.

The belief that science proceeds from observation to theory is still so widely
and so firmly held that my denial of it is often met with incredulity. I have
ev
ever been suspected of being insincere—of denying what nobody in his senses
can doubt.

But in fact the belief that we can start with pure observations alone, without
anything in the nature of a theory, is absurd; as may be illustrated by the
story of the man who dedicated his life to natural science, wrote down every-
thing he could observe, and bequeathed his priceless collection of observations
to the Royal Society to be used as inductive evidence. This story should
show us that though beetles may profitably be collected, observations may not.

Twenty-five years ago I tried to bring home the same point to a group of
physics students in Vienna by beginning a lecture with the following in-
structions: 'Take pencil and paper; carefully observe, and write down what
you have observed!' They asked, of course, what I wanted them to observe.
Clearly the instruction 'Observe!' is absurd. It is not even idiomatic, even if
the object of the transitive verb can be taken as understood.) Observation is
always selective. It needs a chosen object, a definite task, an interest, a point
of view, a problem. And its description presupposes a descriptive language,
with property words; it presupposes similarity and classification, which in its
turn presupposes interests, points of view, and problems. 'A hungry animal';

writes Katz, 14 'divides the environment into edible and inedible things. An
animal in flight sees roads to escape and hiding places. . . generally speak-
ing, objects change . . . according to the needs of the animal.' We may add
that objects can be classified, and can become similar or dissimilar, only in
this way—by being related to needs and interests. This rule applies not only
to animals but also to scientists. For the animal a point of view is provided
by its needs, the task of the moment, and its expectations; for the scientist by
his theoretical interests, the special problem under investigation, his con-
jectures and anticipations, and the theories which he accepts as a kind of back-
ground: his frame of reference, his 'horizon of expectations'.

The problem: 'Which comes first, the hypothesis (H) or the observation
(O)?' is soluble; as is the problem, 'Which comes first, the hen (H) or the
egg (O)'. The reply to the latter is, 'An earlier kind of egg'; to the former, 'An
earlier kind of hypothesis'. It is quite true that any particular hypothesis
we choose will have been preceded by observations—the observations, for
example, which it is designed to explain. But these observations, in their
turn, presupposed the adoption of a frame of reference: a frame of expecta-
tions: a frame of theories. If they were significant, if they created a need for explana-
tion and thus gave rise to the invention of a hypothesis, it was because they
could not be explained within the old theoretical framework, the old horizon
of expectations. There is no danger here of an infinite regress. Going back to
more and more primitive theories and myths we shall in the end find un
conscious, inborn expectations.

The theory of inborn ideas is absurd, I think; but every organism has
inborn reactions or responses; and among them, responses adapted to im-
pending events. These responses we may describe as 'expectations' without
implying that these 'expectations' are conscious. The new born baby 'expects',
this, in his sense, to be fed and, one could even argue, to be protected and
loved). In view of the close relation between expectation and knowledge we
may even speak in quite a reasonable sense of 'inborn knowledge'. This
'knowledge' is not, however, valid a priori; an inborn expectation, no matter
how strong and specific, may be mistaken. The newborn child may be
abandoned, and starved.

Thus we are born with expectations; with 'knowledge' which, although not
valid a priori, is psychologically or genetically a priori, i.e., prior to all observa-
tional experience. One of the most important of these expectations is the
expectation of finding a regularity. It is connected with an inborn propensity
to look out for regularities, or with a need to find regularities, as we may see
from the pleasure of the child who satisfies this need.

This 'instinctive' expectation of finding regularities, which is psycho-
logically a priori, corresponds very closely to the 'law of causality' which Kant
believed to be part of our mental outfit and to be a priori valid. One might
thus be inclined to say that Kant failed to distinguish between psychologi-
cally a priori ways of thinking or responding a priori valid beliefs. But I do

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1 See section 30 of L.S.E.D.

14 Katz, loc. cit.
not think that his mistake was quite as crude as that. For the expectation of finding regularities is not only psychologically a priori, but also logically a priori: it is logically prior to all observational experience, for it is prior to any recognition of similarities, as we have seen; and all observation involves the recognition of similarities (or dissimilarities). But in spite of being logically a priori in this sense the expectation is not valid a priori. For it may fail: we can easily construct an environment (it would be a lethal one) to find regularities. (All natural laws could remain valid: environments of this kind have been used in the animal experiments mentioned in the next section.)

Thus Kant's reply to Hume came near to being right; for the distinction between an a priori valid expectation and one which is both genetically and logically prior to observation, but not a priori valid, is really somewhat subtle. But Kant proved too much. In trying to show how knowledge is possible, he proposed a theory which had the unavoidable consequence that our quest for knowledge must necessarily succeed, which is clearly mistaken. When Kant said, 'Our intellect does not draw its laws from nature but imposes its laws upon nature', he was right. But in thinking that these laws are necessarily true, or that we necessarily succeed in imposing them upon nature, he was wrong.15 Nature very often resists quite successfully, forcing us to discard our laws as refuted; but if we live we may try again.

To sum up this logical criticism of Hume's psychology of induction we may consider the idea of building an induction machine. Placed in a simplified 'world' (for example, one of sequences of coloured counters) such a machine may through repetition 'learn', or even 'formulate', laws of succession which hold in its 'world'. If such a machine can be constructed (and I have no doubt that it can) then it might be argued, my theory must be wrong; for if a machine is capable of performing inductions on the basis of repetition, there can be no logical reasons preventing us from doing the same.

The argument sounds convincing, but it is mistaken. In constructing an induction machine we, the architects of the machine, must decide a priori what constitutes its 'world'; what things are to be taken as similar or equal; and what kind of 'laws' we wish the machine to be able to 'discover' in its 'world'. In other words we must build the machine a framework determining what is relevant or interesting in its world: the machine will have its 'inborn' selection principles. The problems of similarity will have been solved for it by its makers who thus have interpreted the 'world' for the machine.

15 Kant believed that Newton's dynamics was a priori valid. (See his Metaphysical Foundations of Natural Science, published between the first and second editions of the Critique of Pure Reason.) But if, as he thought, we can explain the validity of Newton's theory by the fact that our intellect imposes its laws upon nature, it follows, I think, that our intellect must succeed in this; which makes it hard to understand why a priori knowledge such as Newton's should be so hard to come by. A somewhat fuller statement of this criticism can be found in ch. 2, especially section 5, and chs. 7 and 8 of the present volume.
and changing world: we know from experiments on animals that varying degrees of neurotic behaviour may be produced at will by correspondingly varying difficulties.

I found many other links between the psychology of knowledge and psychological fields which are often considered remote from it—for example the psychology of art and music; in fact, my ideas about induction originated in a conjecture about the evolution of Western polyphony. But you will be spared this story.

VII

My logical criticism of Hume’s psychological theory, and the considerations connected with it (most of which I elaborated in 1926–7, in a thesis entitled ‘On Habit and Belief in Laws’ 16) may seem a little removed from the field of the philosophy of science. But the distinction between dogmatic and critical thinking, or the dogmatic and the critical attitude, brings us right back to our central problem. For the dogmatic attitude is clearly related to the tendency to verify our laws and schemata by seeking to apply them and to confirm them, even to the point of neglecting refutations, whereas the critical attitude is one of readiness to change them— to test them; to refute them; to falsify them, if possible. This suggests that we may identify the critical attitude with the scientific attitude, and the dogmatic attitude with the one which we have described as pseudo-scientific.

It further suggests that genetically speaking the pseudo-scientific attitude is more primitive than, and prior to, the scientific attitude: that it is a pre-scientific attitude. And this primitivity or priority also has its logical aspect. For the critical attitude is not so much opposed to the dogmatic attitude as super-imposed upon it: criticism must be directed against existing and influential beliefs in need of critical revision—in other words, dogmatic beliefs. A critical attitude needs for its raw material, as it were, theories or beliefs which are held more or less dogmatically.

Thus science must begin with myths, and with the criticism of myths; neither with the collection of observations, nor with the invention of experiments, but with the critical discussion of myths, and of magical techniques and practices. The scientific tradition is distinguished from the pre-scientific tradition in having two layers. Like the latter, it passes on its theories; but it also passes on a critical attitude towards them. The theories are passed on, not as dogmas, but rather with the challenge to discuss them and improve upon them. This tradition is Hellenic: it may be traced back to Thales, founder of the first school (I do not mean ‘of the first philosophical school’, but simply ‘of the first school’) which was not mainly concerned with the preservation of a dogma. 17

The critical attitude, the tradition of free discussion of theories with the

16 A thesis submitted under the title ‘Gewohnheit und Gesetserlebnis’ to the Institute of Education of the City of Vienna in 1927. (Unpublished.)
17 Further comments on these developments may be found in chs. 4 and 5, below.
may have served well enough in the days of Aristotle and Pytheas of Massalia — the great traveller who for centuries was called a liar because of his tales of Thule, the land of the frozen sea and the midnight sun.

The method of trial and error is not, of course, simply identical with the scientific or critical approach — with the method of conjecture and refutation. The method of trial and error is applied not only by Einstein but, in a more dogmatic fashion, by the amoeba also. The difference lies not so much in the scientific or critical approach — with the method of conjecture and refutation. The scientist consciously and cautiously tries to uncover in order to refute his theories with searching arguments, including appeals to the most severe experimental tests which his theories and his ingenuity permit him to design.

The critical attitude may be described as the conscious attempt to make our theories, our conjectures, suffer in our stead in the struggle for the survival of the fittest. It gives us a chance to survive the elimination of an inadequate hypothesis — when a more dogmatic attitude would eliminate it by eliminating us. (There is a touching story of an Indian community which disappeared because of its belief in the holiness of life, including that of tigers.) We thus obtain the fittest theory within our reach by the elimination of those which are less fit. (By 'fitness' I do not mean merely 'usefulness' but truth; see chapters 3 and 10, below.) I do not think that this procedure is irrational or in need of any further rational justification.

VIII

Let us now turn from our logical criticism of the psychology of experience to our real problem — the problem of the logic of science. Although some of the things I have said may help us here, in so far as they may have eliminated certain psychological prejudices in favour of induction, the treatment of the logical problem of induction is completely independent of this criticism, and of all psychological considerations. Provided you do not dogmatically believe in the alleged psychological fact that we make inductions, you may now forget my whole story with the exception of two logical points: my logical remarks on testability or falsifiability as the criterion of demarcation; and Hume's logical criticism of induction.

From what I have said it is obvious that there was a close link between the two problems which interested me at that time: demarcation, and induction or scientific method. It was easy to see that the method of science is critical, i.e. attempted falsifications. Yet it took me a few years to notice that the two problems — of demarcation and of induction — were in a sense one.

Why, I asked, do so many scientists believe in induction? I found they did so because they believed natural science to be characterized by the inductive method — by a method starting from, and relying upon, long sequences of observations and experiments. They believed that the difference between genuine science and metaphysical or pseudo-scientific speculation depended solely upon whether or not the inductive method was employed. They

### I. SCIENCE: CONJECTURES AND REFUTATIONS

believed (to put it in my own terminology) that only the inductive method could provide a satisfactory criterion of demarcation.

I recently came across an interesting formulation of this belief in a remarkable philosophical book by a great physicist — Max Born's _Natural Philosophy of Cause and Chance_. He writes: 'Induction allows us to generalize a number of observations into a general rule: that night follows day and day follows night... But while everyday life has no definite criterion for the validity of an induction, science has worked out a code, or rule of craft, for its application.' Born nowhere reveals the contents of this inductive code (which, as his wording shows, contains a 'definite criterion for the validity of an induction'); but he stresses that 'there is no logical argument' for its acceptance: 'it is a question of faith'; and he is therefore 'willing to call induction a metaphysical principle'. But why does he believe that such a code of valid inductive rules must exist? This becomes clear when he speaks of the 'vast communities of people ignorant of, or rejecting, the rule of science, among them the members of anti-vaccination societies and believers in astrology. It is useless to argue with them; I cannot compel them to accept the same criteria of valid induction in which I believe: the code of scientific rules.' This makes it quite clear that 'valid induction was here meant to serve as a criterion of demarcation between science and pseudo-science.'

But it is obvious that this rule or craft of 'valid induction' is not even metaphysical; it simply does not exist. No rule can ever guarantee that a generalization inferred from true observations, however often repeated, is true. (Born himself does not believe in the truth of Newtonian physics, in spite of its success, although he believes that it is based on induction.) And the success of science is not based upon rules of induction, but depends upon luck, ingenuity, and the purely deductive rules of critical argument.

I may summarize some of my conclusions as follows:

1. Induction, i.e. inference based on many observations, is a myth. It is not a psychological fact, nor a fact of ordinary life, nor one of scientific procedure.

2. The actual procedure of science is to operate with conjectures: to jump to conclusions — often after one single observation (as noticed for example by Hume and Born).

3. Repeated observations and experiments function in science as tests of our conjectures or hypotheses, i.e. as attempted falsifications.

4. The mistaken belief in induction is fortified by the need for a criterion of demarcation which, it is traditionally but wrongly believed, only the inductive method can provide.

5. The conception of such an inductive method, like the criterion of verifiability, implies a faulty demarcation.

6. None of this is altered in the least if we say that induction makes theories only probable rather than certain. (See especially chapter 10, below.)

18 Max Born, _Natural Philosophy of Cause and Chance_, Oxford, 1940, p. 7.
If, as I have suggested, the problem of induction is only an instance or facet of the problem of demarcation, then the solution to the problem of demarcation must provide us with a solution to the problem of induction. This is indeed the case, I believe, although it is perhaps not immediately obvious.

For a brief formulation of the problem of induction we can turn again to Born, who writes: '... no observation or experiment, however extended, can give more than a finite number of repetitions'; therefore, 'the statement of a law—B depends on A—always transcends experience. Yet this kind of statement is made everywhere and all the time, and sometimes from scanty material.'

In other words, the logical problem of induction arises from (a) Hume's discovery (so well expressed by Born) that it is impossible to justify a law by observation or experiment, since it 'transcends experience'; (b) the fact that science proposes and uses laws 'everywhere and all the time'. (Like Hume, Born is struck by the 'scanty material', i.e. the few observed instances upon which the law may be based.) To this we have to add (c) the principle of empiricism which asserts that in science, only observation and experiment may decide upon the acceptance or rejection of scientific statements, including laws and theories.

These three principles, (a), (b), and (c), appear at first sight to clash; and this apparent clash constitutes the logical problem of induction.

Faced with this clash, Born gives up (c), the principle of empiricism (as Kant and many others, including Bertrand Russell, have done before him), in favour of what he calls a 'metaphysical principle': a metaphysical principle which he does not even attempt to formulate; which he vaguely describes as a 'code or rule of craft'; and of which I have never seen any formulation which even looked promising and was not clearly untenable.

But in fact the principles (a) to (c) do not clash. We can see this the moment we realize that the acceptance by science of a law or of a theory is tentative only; which is to say that all laws and theories are conjectures, or tentative hypotheses (a position which I have sometimes called 'hypothetico'); and that we may reject a law or theory on the basis of new evidence, without necessarily discarding the old evidence which originally led us to accept it.

The principle of empiricism (c) can be fully preserved, since the fate of a theory, its acceptance or rejection, is decided by observation and experiment—by the result of tests. So long as a theory stands up to the severest tests we can design, it is accepted; if it does not, it is rejected. But it is never inferred, in any sense, from the empirical evidence. There is neither a psychological nor

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19 Natural Philosophy of Cause and Chance, p. 6.
20 I do not doubt that Born and many others would agree that theories are accepted only tentatively. But the widespread belief in induction shows that the far-reaching implications of this view are rarely seen.

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Thus the problem of induction is solved. But nothing seems less wanted than a simple solution to an age-old philosophical problem. Wittgenstein and his school hold that genuine philosophical problems do not exist; from which it clearly follows that they cannot be solved. Others among my contemporaries do believe that there are philosophical problems, and respect them; but they seem to respect them too much; they seem to believe that they are insoluble, if not taboo; and they are shocked and horrified by the claim that there is a simple, neat, and lucid, solution to any of them. If there is a solution it must be deep, they feel, or at least complicated.

However this may be, I am still waiting for a simple, neat and lucid criticism of the solution which I published first in 1933 in my letter to the Editor of Erkenntnis,22 and later in The Logic of Scientific Discovery.

Of course, one can invent new problems of induction, different from the one I have formulated and solved. (Its formulation was half its solution.) But I have yet to see any reformulation of the problem whose solution cannot be easily obtained from my old solution. I am now going to discuss some of these re-formulations.

One question which may be asked is this: how do we really jump from an observation statement to a theory?

Although this question appears to be psychological rather than philosophical, one can say something positive about it without invoking psychology. One can say first that the jump is not from an observation statement, but from a problem-situation, and that the theory must allow us to explain the observations which created the problem (that is, to deduce them from the theory strengthened by other accepted theories and by other observation statements, the so-called initial conditions). This leaves, of course, an immense number of possible theories, good and bad; and it thus appears that our question has not been answered.

But this makes it fairly clear that when we asked our question we had more in mind than, 'How do we jump from an observation statement to a theory?' The question we had in mind was, it now appears, 'How do we jump from an observation statement to a good theory?' But to this the answer is: by jumping first to any theory and then testing it, to find whether it is good or not; i.e.

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21 Wittgenstein still held this belief in 1946; see note 8 to ch. 2, below.
22 See Note 5 above.
by repeatedly applying the critical method, eliminating many bad theories, and inventing many new ones. Not everybody is able to do this; but there is no other way.

Other questions have sometimes been asked. The original problem of induction, it was said, is the problem of justifying induction, i.e. of justifying inductive inference. If you answer this problem by saying that what is called an 'inductive inference' is always invalid and therefore clearly not justifiable, the following new problem must arise: how do you justify your method of trial and error? Reply: the method of trial and error is a method of eliminating false theories by observation statements; and the justification for this is the purely logical relationship of deducibility which allows us to assert the falsity of universal statements if we accept the truth of singular ones.

Another question sometimes asked is this: why is it reasonable to prefer non-falsified statements to falsified ones? To this question some involved answers have been produced, for example pragmatic answers. But from a pragmatic point of view the question does not arise, since false theories are often shown to be false, although they may be excellent approximations and easy to handle; and they are used with confidence by people who know them to be false.

The only correct answer is the straightforward one: because we search for truth (even though we can never be sure we have found it), and because the falsified theories are known or believed to be false, while the non-falsified theories may still be true. Besides, we do not prefer every non-falsified theory—only one which, in the light of criticism, appears to be better than its competitors; which solves our problems, which is well tested, and of which we think, or rather conjecture or hope (considering other provisionally accepted theories), that it will stand up to further tests.

It has also been said that the problem of induction is, 'Why is it reasonable to believe that the future will be like the past?', and that a satisfactory answer to this question should make it plain that such a belief is, in fact, reasonable. My reply is that it is reasonable to believe that the future will be very different from the past in many vitally important respects. Admittedly it is perfectly reasonable to act on the assumption that it will, in many respects, be like the past, and that well-tested laws will continue to hold (since we can have no better assumption to act upon); but it is also reasonable to believe that such a course of action will lead us at times into severe trouble, since some of the laws upon which we now heavily rely may easily prove unreliable. (Remember the midwife! One might even say that to judge from past experience, and from our general scientific knowledge, the future will not be like the past, in perhaps most of the ways which those have in mind who say that it will. Water will sometimes not quench thirst, and air will choke those who breathe it. An apparent way out is to say that the future will be like the past in the sense that the laws of nature will not change, but this is begging the question. We speak of a 'law of nature' only if we think that we have before us a regularity which does not change; and if we find that it changes then we shall not continue to call it a 'law of nature'. Of course our search for natural laws indicates that we hope to find them, and that we believe that there are natural laws; but our belief in any particular natural law cannot have a safer basis than our unsuccessful critical attempts to refute it.

I think that those who put the problem of induction in terms of the reasonableness of our beliefs are perfectly right if they are dissatisfied with a Humean or post-Humean, sceptical despair of reason. We must indeed reject the view that a belief in science is as irrational as a belief in primitive magical practices—that both are a matter of accepting a 'total ideology', a convention or a tradition based on faith. But we must be cautious if we formulate our problem, with Hume, as one of the reasonableness of our beliefs. We should split this problem into three—our old problem of demarcation, or of how to distinguish between science and primitive magic; the problem of the rationality of the scientific or critical procedure, and of the role of observation within it; and lastly the problem of the rationality of our acceptance of theories for scientific and for practical purposes. To all three problems solutions have been offered here.

One should also be careful not to confuse the problem of the reasonableness of the scientific procedure and the (tentative) acceptance of the results of this procedure—i.e. the scientific theories—with the problem of the rationality or otherwise of the belief that this procedure will succeed. In practice, in practical scientific research, this belief is no doubt unavoidable and reasonable, there being no better alternative. But the belief is certainly unjustifiable in a theoretical sense, as I have argued (in section v). Moreover, if we could show, on general logical grounds, that the scientific quest is likely to succeed, one could not understand why anything like success has been so rare in the long history of human endeavours to know more about our world.

Yet another way of putting the problem of induction is in terms of probability. Let $P(t,e)$ be the theory and $e$ the evidence: we can ask for $P(t,e)$, that is to say, the probability of $t$, given $e$. The problem of induction, it is often believed, can then be put thus: construct a calculus of probability which allows us to work out for any theory $t$ what its probability is, relative to any given empirical evidence $e$; and show that $P(t,e)$ increases with the accumulation of supporting evidence, and reaches high values—at any rate values greater than 1.

In The Logic of Scientific Discovery I explained why I think that this approach to the problem is fundamentally mistaken. To make this clear, I introduced there the distinction between probability and degree of corroboration or confirmation. (The term 'confirmation' has lately been so much used and misused that I have decided to surrender it to the verificationists and to use for my own purposes 'corroboration' only. The term 'probability' is best

23 L.Sc.D. (see note 5 above), ch. x, especially sections 80 to 83, also section 24 ff. See also my note 'A Set of Independent Axioms for Probability', Mind, N.S. 47, 1938, p. 275. (This note has since been reprinted, with corrections, in the new appendix 6 of L.Sc.D. See also the next note but one to the present chapter.)
I explained in my book why we are interested in theories with a high degree of corroboration. And I explained why it is a mistake to conclude from this that we are interested in highly probable theories. I pointed out that the probability of a statement (or set of statements) is always the greater the less the statement says: it is inverse to the content or the deductive power of the statement, and thus to its explanatory power. Accordingly every interesting and powerful statement must have a low probability; and vice versa: a statement with a high probability will be scientifically uninteresting, because it says little and has no explanatory power. Although we seek theories with a high degree of corroboration, as scientists we do not seek highly probable theories but explanations: that is to say, powerful and improbable theories.

The opposite viewpoint—that science aims at high probability—is a characteristic development of verificationism: if you find that you cannot verify a theory, or 'Ersatz', you may turn to probability as a kind of 'Erst' for certainty, in the hope that induction may yield at least that much.

I have discussed the two problems of demarcation and induction at some length. Yet since I set out to give you in this lecture a kind of report on the work I have done in this field I shall have to add, in the form of an appendix, a few words about some other problems on which I have been working between 1934 and 1953. I was led to most of these problems by trying to think out the consequences of the solutions to the two problems of demarcation and induction. But time does not allow me to continue my narrative, and to tell you how my new problems arose out of my old ones: Since I cannot even think of trying to solve problems with the help of rational argument, and thus for my unwillingness to participate wholeheartedly in the developments, trends, and drifts, of contemporary philosophy.

APPENDIX: SOME PROBLEMS IN THE PHILOSOPHY OF SCIENCE

My first three items in this list of additional problems are connected with the calculus of probabilities.

1. The frequency theory of probability. In The Logic of Scientific Discovery I was interested in developing a consistent theory of probability as it is used in science; which means, a statistical or frequency theory of probability. But I also operated there with another concept which I called 'logical probability'. I therefore felt the need for a generalization—for a formal theory of probability which allows different interpretations: (a) as a theory of the logical probability of a statement relative to any given evidence; including a theory of absolute logical probability, i.e., of the measure of the probability of a statement relative to zero evidence; (b) as a theory of the probability of an event relative to any given ensemble (or 'collective') of events. In solving this problem I obtained a simple theory which allows a number of further interpretations: it may be interpreted as a calculus of contents, or of deductive systems, or as a class calculus (Boolean algebra) or as propositional calculus; and also as a calculus of propensities.

24 A definition, in terms of probabilities (see the next note), of C(e), i.e., of the degree of corroboration (of a theory t relative to the evidence e) satisfying the demands indicated in my Logic, sections 82 to 83, is the following:

\[ C(t, e) = E(t, e) (1 + P(t|P(e))) \]

where \( E(t, e) = \frac{P(e)}{P(e) + P(\neg e)} \) (a non-additive measure of the explanatory power of \( t \) with respect to \( e \)). Note that \( C(t, e) \) is not a probability: it may have values between 0 and \( 1 \) (refutation of \( e \) by \( t \)) and \( C(t, e) < 1 \). Statements \( t \) which are lawlike and thus non-verifiable cannot even reach \( C(t, e) = C(t) \) upon empirical evidence. \( C(t) \) is the degree of corroboration of \( t \), and is equal to the degree of testability of \( t \), or to the content of \( t \). Because of the demands implied in point (b) at the end of section 1 above, I do not think, however, that it is possible to give a complete formalization of the idea of corroboration (or, as I previously used to say, of confirmation).

(A) if \( P(y) \neq 0 \), then \( P(x|y) = \frac{P(x,y)}{P(y)} \) (Definition of relative Probability)

25 See my note in Mind, loc. cit. The axiom system given there for elementary (i.e., non-continuous) probability can be simplified as follows (\( \neg \) denotes the complement of \( X \), \( \cap \) the intersection or conjunction of \( X \) and \( Y \)):

(A) \( P(x,y) = P(y|x) \) (Commutation)

(B) \( P(x) = \frac{P(x,y)}{P(y)} \) (Association)

(C) \( P(x) = \frac{P(x,y)}{P(y)} \) (Monotony)

(D) \( P(x,y) = P(x)P(y) \) (Addition)

(E) \( P(x|y) = \frac{P(x,y)}{P(y)} \) (Definition of relative Probability)
(2) This problem of a propensity interpretation of probability arose out of my interest in Quantum Theory. It is usually believed that Quantum Theory has to be interpreted statistically, and no doubt statistics is essential for its empirical tests. But this is a point where, I believe, the dangers of the testability theory of meaning become clear. Although the tests of the theory are statistical, and although the theory (say, Schrödinger's equation) may imply statistical consequences, it need not have a statistical meaning: and one can give examples of objective propensities (which are something like generalized forces) and of fields of propensities, which can be measured by statistical methods without being themselves statistical. (See also the last paragraph of chapter 3, below, with note 35.)

(3) The use of statistics in such cases is, in the main, to provide empirical tests of theories which need not be purely statistical; and this raises the question of the refutability of statistical statements—a problem treated, but not to my full satisfaction, in the 1934 edition of my _The Logic of Scientific Discovery_. I later found, however, that all the elements for constructing a satisfactory solution lay ready for use in that book; certain examples I had given allow a mathematical characterization of a class of infinite chance-like sequences which are, in a certain sense, the shortest sequences of their kind. A statistical statement may now be said to be testable by comparison with these 'shortest sequences'; it is refuted if the statistical properties of the tested ensembles differ from the statistical properties of the initial sections of these 'shortest sequences'.

(4) There are a number of further problems connected with the interpretation of the formalism of a quantum theory. In a chapter of _The Logic of Scientific Discovery_ I criticized the 'official' interpretation, and I still think that my criticism is valid in all points but one: one example which I used (in section 77) is mistaken. But since I wrote that section, Einstein, Podolski, and Rosen have published a thought-experiment which can be substituted for my example, although their tendency (which is deterministic) is quite different from mine. Einstein's belief in determinism (which I had occasion to discuss with him) I, believe, unfounded, and also unfortunate: it robs his criticism of much of its force, and it must be emphasized that much of his criticism is quite independent of his determinism.

(5) As to the problem of determinism itself, I have tried to show that even classical physics, which is deterministic in a certain _prima facie_ sense, is misinterpreted if used to support a deterministic view of the physical world in Laplace's sense.

(6) In this connection, I may also mention the problem of simplicity—of the simplicity of a theory, which I have been able to connect with the content of a theory. It can be shown that what is usually called the simplicity of a theory is associated with its logical improbability, and not with its probability, as has often been supposed. This, indeed, allows us to deduce, from the theory of science outlined above, why it is always advantageous to try the simplest theories first. They are those which offer us the best chance to submit them to severe tests: the simpler theory has always a higher testability than the more complicated one. (Yet I do not think that this settles all problems about simplicity. See also chapter 10, section xviii, below.)

(7) Closely related to this problem is the problem of the ad hoc character of a hypothesis, and of degrees of this _ad hoc_ character (of 'ad hocness'), if I may so call it. One can show that the methodology of science (and the history of science also) becomes understandable in its details if we assume that the aim of science is to get explanatory theories which are as little _ad hoc_ as possible: a 'good' theory is not _ad hoc_, while a 'bad' theory is. On the other hand, one can show that the probability theories of induction imply, inadvertently but necessarily, the unacceptable rule: always use the theory which is the most _ad hoc_, i.e. which transends the available evidence as little as possible. (See also my paper 'The Aim of Science', mentioned in note 28 below.)

(8) An important problem is the problem of the layers of explanatory hypotheses which we find in the more developed theoretical sciences, and of

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**Notes:**

1. See _L.S.C.D._, p. 153 (section 55); see especially the new appendix xxxi.
2. Ibid., sections 41 to 46. But see now also ch. 10, section xviii.
the relations between these layers. It is often asserted that Newton's theory can be induced or even deduced from Kepler's and Galileo's laws. But it can be shown that Newton's theory (including his theory of absolute space) strictly speaking contradicts Kepler's (even if we confine ourselves to the two-body problem\(^\text{28}\) and neglect the mutual attraction between the planets) and also Galileo's; although approximations to these two theories can, of course, be deduced from Newton's. But it is clear that neither a deductive nor an inductive inference can lead, from consistent premises, to a conclusion strictly speaking contradicts Kepler's (even if we confine ourselves to the two-body problem\(^\text{28}\) and neglect the mutual attraction between the planets). These considerations allow us to analyse the logical relations between 'layers' of theories, and also the idea of an approximation, in the two senses of (a) The theory \(x\) is an approximation to the theory \(y\); and (b) The theory \(x\) is 'a good approximation to the facts'. (See also chapter 10, below.)

(9) A host of interesting problems is raised by operationalism, the doctrine that theoretical concepts have to be defined in terms of measuring operations. Against this view, it can be shown that measurements presuppose theories. There is no measurement without a theory and no operation which can be satisfactorily described in non-theoretical terms. The attempts to do so are always circular; for example, the description of the measurement of length needs a (rudimentary) theory of heat and temperature-measurement; but these, in turn, involve measurements of length.

The analysis of operationalism shows the need for a general theory of measurement; a theory which does not, naïvely, take the practice of measuring as 'given', but explains it by analysing its function in the testing of scientific hypotheses. This can be done with the help of the doctrine of degrees of testability.

Connected with, and closely parallel to, operationalism is the doctrine of behaviourism, i.e. the doctrine that, since all test-statements describe behaviour, our theories too must be stated in terms of possible behaviour. But the inference is as invalid as the phenomenalist doctrine which asserts that since all test-statements are observational, theories too must be stated in terms of possible observations. All these doctrines are forms of the verifiability theory of meaning; that is to say, of inductivism.

Closely related to operationalism is instrumentalism, i.e. the interpretation of scientific theories as practical instruments or tools for such purposes as the prediction of impending events. That theories may be used in this way cannot be doubted; but instrumentalism asserts that they can be best understood as instruments; and that this is mistaken, I have tried to show by a comparison of the different functions of the formulae of applied and pure science. In this context the problem of the theoretical (i.e. non-practical) function of predictions can also be solved. (See chapter 3, section 5, below.)

It is interesting to analyse from the same point of view the function of language—as an instrument. One immediate finding of this analysis is that we use descriptive language in order to talk about the world. This provides new arguments in favour of realism.

Operationalism and instrumentalism must, I believe, be replaced by 'theoreticism', if I may call it so: by the recognition of the fact that we are always operating within a complex framework of theories, and that we do not aim simply at correlations, but at explanations.

(10) The problem of explanation itself: It has often been said that scientific explanation is reduction of the unknown to the known. If pure science is meant, nothing could be further from the truth. It can be said without paradox that scientific explanation is, on the contrary, the reduction of the known to the unknown. In pure science, as opposed to an applied science which takes pure science as 'given' or 'known', explanation is always the logical reduction of hypotheses to others which are of a higher level of universality; of 'known' facts and 'known' theories to assumptions of which we know very little as yet, and which have still to be tested. The analysis of degrees of explanatory power, and of the relationship between genuine and sham explanation and between explanation and prediction, are examples of problems which are of great interest in this context.

(11) This brings me to the problem of the relationship between explanation in the natural sciences and historical explanation (which, strangely enough, is logically somewhat analogous to the problem of explanation in the pure and applied sciences); and to the vast field of problems in the methodology of the social sciences, especially the problems of historical prediction; historical realism and historical determinism; and historical relativism. These problems are linked, again, with the more general problems of determinism and relativism, including the problems of linguistic relativism.\(^\text{29}\)

(12) A further problem of interest is the analysis of what is called 'scientific objectivity'. I have treated this problem in several places, especially in connection with a criticism of the so-called 'sociology of knowledge'.\(^\text{30}\)

(13) One type of solution of the problem of induction should be mentioned here again (see section iv, above), in order to warn against it. (Solutions of this kind are, as a rule, put forth without a clear formulation of the problem which they are supposed to solve.) The view I have in mind may be described...
CONJECTURES

as follows. It is first taken for granted that nobody seriously doubts that we
do, in fact, make inductions, and successful ones. (My suggestion that this is a
myth, and that the apparent cases of induction turn out to be cases of the method of trial and error, is treated with the con­
tempt which an utterly unreasonable suggestion of this kind deserves.) It is
then said that the task of a theory of induction is to describe and classify our
inductive policies or procedures, and perhaps to point out which of them are
the most successful and reliable ones and which are less successful or reliable;
and that any further question of justification is misplaced. Thus the view I
have in mind is characterized by the contention that the distinction between
the factual problem of describing how we argue inductively (quid facti?), and
the problem of the justification of our inductive arguments (quid juris?) is
a misplaced distinction. It is also said that the justification required is un­
reasonable, since we cannot expect inductive arguments to be 'valid' in the
same sense in which deductive ones may be 'valid': induction simply is not
deduction, and it is unreasonable to demand from it that it should conform to
the standards of logical—that is, deductive—validity. We must therefore
judge it by its own standards—by inductive standards——of reasonableness.

I think that this defence of induction is mistaken. It not only takes a myth
for a fact, and the alleged fact for a standard of rationality, with the result
that a myth becomes a standard of rationality; but it also propagates, in this
way, a principle which may be used to defend any dogma against any criti­
cism. Moreover, it mistakes the status of formal or 'deductive' logic. (It mis­
takes it just as much as those who saw it as the systematization of our factual,
that is, psychological, 'laws of thought'). For deduction, I contend, is not
valid because we choose or decide to adopt its rules as a standard, or decree
that they shall be accepted; rather, it is valid because it adopts, and incorpo­
rates, the rules by which truth is transmitted from (logically stronger) premises
to (logically weaker) conclusions, and by which falsity is re-transmitted from
conclusions to premises. (This re-transmission of falsity makes formal logic
the Organon of rational criticism—that is, of refutation.)

One point that may be conceded to those who hold the view I am criticizing
here is this. In arguing from premises to the conclusion (or in what may be
called the 'deductive direction'), we argue from the truth or the certainty or
the probability of the premises to the corresponding property of the conclu­
sion; while if we argue from the conclusion to the premises (and thus in
what we have called the 'inductive direction'), we argue from the falsity or the
uncertainty or the impossibility or the improbability of the conclusion to the
property of the premises; accordingly, we must indeed concede that standards such as, more especially, certainty, which apply to arguments in
the inductive direction, do not also apply to arguments in the deductive direction. Yet even this concession of mine turns in the end against
those who hold the view which I am criticizing here; for they assume,
wrongly, that we may argue in the inductive direction, though not to the
certainty, yet to the probability of our 'generalizations'. But this assumption

is mistaken, for all the intuitive ideas of probability which have ever been
suggested.31

This is a list of just a few of the problems of the philosophy of science to
which I was led in my pursuit of the two fertile and fundamental problems
whose story I have tried to tell you.

31 (Added 1961.) Since 1953, when this lecture was delivered, and since 1955, when I read
the proofs, the list given in this appendix has grown considerably, and some more recent
contributions which deal with problems not listed here will be found in this volume (see
especially ch. 10, below) and in my other books (see especially the new appendices to my
L.Sc.D., and the new Addendum to vol. ii of my Open Society which I have added to the
fourth edition, 1962). See especially also my paper 'Probability Magic, or Knowledge out