with Hume's logical refutation of induction but disagrees with his psychological explanation of induction (in terms of custom or habit). In the concluding sections of the essay (reprinted here), Popper returns to the problem of demarcation and relates it to the problem of induction.

The selection by John Ziman consists of extracts from his book on science. In the first part he discusses and rejects various definitions of science which have been held. He attempts to formulate a more accurate and tenable characterization based on what he takes to be the goal or objective of science, namely, consensus of rational opinion “over the widest possible field.” In the second part, he provides his answer to the question “What distinguishes science from nonscience?” The reader should attempt to decide whether his “criterion of demarcation” is an improvement over Popper’s and, if so, why. Since this selection is unusually clear and readable, no further comments are required.

Feyerabend’s essay is, no doubt, one of the most controversial ones in this volume. Feyerabend claims that he wishes to defend society and its inhabitants from all ideologies, including science. He likens them (again, including science) to fairytales “which have lots of interesting things to say but which also contain wicked lies.” He goes on to consider an argument designed to defend the exceptional status which science has in society today. According to this argument: “(1) science has finally found the correct method for achieving results and (2) there are many results to prove the excellence of the method.” In the next sections he argues against both (1) and (2). He concludes his essay with a provocative discussion of education and myth. We urge the reader to reflect seriously upon Feyerabend’s somewhat unorthodox views and to ask whether Feyerabend has adequately defended them.

Paul R. Thagard’s essay constitutes both a further discussion of some of the above-mentioned topics (such as the criterion of demarcation) and an example of the application of them. Most scientists and philosophers agree that astrology is a pseudoscience. Thagard attempts to show why it is. After presenting a brief description of astrology, he attempts to show that the major objections which have been provided do not show that it is a pseudoscience. Thagard then proposes his principle of demarcation and, upon the basis of it, to claims that and why astrology is unsound.

There is a kind of dialogue which runs through the essays in this part. We urge the reader to critically evaluate the various positions presented and attempt to come to his or her own conclusion with regard to the questions “What is science?,” “By what criteria can we distinguish science from nonscience or pseudoscience?” and so on. The Study Questions should provide assistance in gauging the reader’s understanding of the selections and in grappling with these and related questions.

E.D.K.

1

Science: Conjectures and Refutations

Sir Karl Popper

Mr. Turnbull had predicted evil consequences, . . .
and was now doing the best in his power to bring about the verification of his own prophecies.

ANTHONY TROLLOPE

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 pseudoscience; knowing very well that science often errs, and that pseudoscience may happen to stumble on the truth.

I knew, of course, the most widely accepted answer to my problem: that science is distinguished from pseudoscience—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 does not come up to scientific standards. The latter method may be exemplified by astrology with its stupendous mass of empirical evidence based on observation—on horoscopes and on biographies.

But as it was not the example of astrology which led me to my problem I should perhaps briefly describe the atmosphere in which my problem arose and the examples by which it was stimulated. After the collapse of the Austrian Empire there had been a revolution in Austria: the air was full of revolutionary slogans and ideas, and new and often wild theories. Among the theories which interested me Einstein's theory of relativity was no doubt by far the most important. Three others were Marx's theory of history, 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 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 stars 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 constellation 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 prediction 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 predictions; 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 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''.)

7. Some genuinely testable theories, when found to be false, are still upheld 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 "conventionalist 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 falsification 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 reinterpreted 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 in their practice. And as 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 Olympus. These theories describe some facts,
but 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 racialist 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 of 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.

III

Today I know, of course, that this criterion of demarcation—the criterion of testability, or 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 rather 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, 5 and 4.52) that every genuine proposition must be a truth-function of, and 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 4.11, 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 meaning 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. In this letter 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. 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. 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.

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

IV

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, my 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 criticism, 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 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 sees 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 neither 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 refutations.

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.

V

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."10

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 "hypotheticism"); 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.11

The principles 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 a logical induction. Only the falsity of the theory can be inferred from empirical evidence, and this inference is a purely deductive one.

Hume showed that it is not possible to infer a theory from observation statements; but this does not affect the possibility of refuting a theory by observation statements. The full appreciation of this possibility makes the relation between theories and observations perfectly clear.

This solves the problem of the alleged clash between the principles (a), (b), and (c), and with it Hume's problem of induction.

VI

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;12 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,13 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 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. 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 often serve well enough: most formulae used in engineering or navigation are known 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 midnight sun!) 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, skeptical 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 these 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. 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 t 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 ½.

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 corrobororation 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 "corrobororation" only. The term "probability" is best used in some of the many senses which satisfy the well-known calculus of probability, axiomatized, for example, by Keynes, Jeffreys, and myself; but nothing of course depends on the choice of words, as long as we do not assume, uncritically, that degree of corrobororation must also be a probability—that is to say, that it must satisfy the calculus of probability.)

I explained in my book why we are interested in theories with a high degree of corrobororation. 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 improvable theories. The opposite view—that science aims at high probability—is a characteristic development of verificationism: if you find that you cannot verify a theory, or make it certain by induction, you may turn to probability as a kind of "Ersatz" for certainty, in the hope that induction may yield at least that much. . . .

NOTES

1. This is a slight oversimplification, for about half of the Einstein effect may be derived from the classical theory, provided we assume a ballistic theory of light.

2. See, for example, my Open Society and Its Enemies, ch. 15, section iii, and notes 13–14.

3. "Clinical observations," like all other observations, are interpretations in the light of theories; and for this reason alone they are apt to seem to support those theories in the light of which they were interpreted. But real support can be obtained only from observations undertaken as tests (by attempted refutations); and for this purpose criteria of refutation have to be laid down beforehand; it must be agreed which observable situations, if actually observed, mean that the theory is refuted. But what kind of clinical responses would refuse to the satisfaction of the analyst not merely a particular analytic diagnosis but psycho-analysis itself? And have such criteria ever been discussed or agreed upon by analysts? Is there not, on the contrary, a whole family of analytic concepts, such as "ambivalence" (I do not suggest that there is no such thing as ambivalence), which would make it difficult, if not impossible, to agree upon such criteria? Moreover, how much headway has been made in investigating the question of the extent to which the (conscious or unconscious) expectations and theories held by the analyst influence the "clinical responses" of the patient? (To say nothing about the conscious attempts to influence the patient by proposing interpretations to him, etc.) Years ago I introduced the term "Oedipus effect" to describe the influence of a theory or expectation or prediction upon the event which it predicts or describes: it will be remembered that the causal chain leading to Oedipus' parricide was started by the oracle's prediction of this event. This is a characteristic and recurrent theme of such myths, but one which seems to have failed to attract the interest of the analysts, perhaps not accidentally.

4. The problem of confirmatory dreams suggested by the analyst is discussed by Freud, for example in Gesammelte Schriften, III., 1925, where he says on p. 314: "If anybody asserts that most of the dreams which can be utilized in an analysis . . . owe their origin to [the analyst's] suggestion, then no objection can be made from the point of view of analytic theory. Yet there is nothing in this fact," he surprisingly adds, "which would detract from the reliability of our results."

5. My Logic of Scientific Discovery (1959, 1960, 1961), here usually referred to as L.Sc.D., is the translation of Logik der Forschung (1934), with a number of additional notes and appendices, including (on pp. 312–14) the letter to the Editor of Erkenntnis mentioned here in the text which was first published in Erkenntnis, 3, 1933, pp. 426 f.

6. Concerning my never published book mentioned here in the text, see R. Carnap's "Uber Protokollätze" (On Protocol-Sentences), Erkenntnis, 3, 1932, pp. 215–28 where he gives an outline of my theory on pp. 223–6, and accepts it. He calls my theory "procedure B," and says (p. 224, top): "Starting from a point of view different from Neurath's" (who developed what Carnap calls on p. 223 "procedure A"). "Popper developed procedure B as part of his system." And after describing in detail my theory of tests, Carnap sums up his views as follows (p. 238): "After weighing the various arguments here discussed, it appears to me that the second language form with procedure B—that is 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, p. 104, where the date "1933" should read "1932"; and ch. 11, below, text to note 39.)

7. Wittgenstein's example of a nonsensical pseudo-proposition is: 'Socrates is identical. Obviously, 'Socrates is not identical' must also be nonsense. Thus the negation of any nonsense will be nonsense, and that of a meaningful statement will be meaningful. 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 confusion caused by taking testability as a criterion of meaning rather than of demarcation can easily be imagined.

8. 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. J. L. Evans's article, Mind, 62, 1953, p. 11 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, notes 46, 51 and 52 to ch. 11; and a fuller analysis in ch. 11 of Conjectures and Refutations (1963).

9. 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 first level, there is a logical asymmetry: one singular statement—say about the perihelion of Mercury—can formally falsify Kepler's laws; but these cannot be formally verified by any number of singular statements. The attempt to minimize this asymmetry can only lead to confusion. On another level, 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 dissectors of the heart before Harvey observed the wrong end. There can never be anything like a completely safe observation, 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 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.


11. 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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12. Wittgenstein still held this belief in 1946.
13. See note 5 above.
14. L.Sc.D. (see note 5 above), ch. x., especially sections 80 to 83, also section 34 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 II of L.Sc.D.)
15. A definition, in terms of probabilities, of \( C(t,e) \), i.e. of the degree of corroboration (of a theory \( t \) relative to the evidence \( e \)) satisfying the demands indicated in my L.Sc.D., sections 82 to 83, is the following:

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

where \( E(t,e) = (P(e,t) - P(e))/(P(e,t) + P(e)) \) is 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 \(-1\) (refutation of \( t \) by \( e \)) and \( C(t,e) < +1 \). Statements which are lawlike and thus non-verifiable cannot even reach \( C(t,e) = C(t) \), upon empirical evidence \( e \). \( C(t) \) is the degree of corroboration of \( t \), and is equal to the degree of testability of \( t \), or to the content of \( t \). Because the demands implied in point (6) 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).

(Added 1955 to the first proofs of this paper.)

See also my note "Degree of Confirmation," British Journal for the Philosophy of Science, 5, 1954, pp. 143 ff. (See also 5, pp. 334.) I have since simplified this definition as follows (B.J.P.S., 1955, 5, p. 359):

\[
C(t,e) = (P(e,t) - P(e))/P(e,t) - P(e,t) + P(e)
\]

For a further improvement, see B.J.P.S. 6, 1955, p. 56.

2

What Is Science?

John Ziman

To answer the question "What is Science?" is almost as presumptuous as to try to state the meaning of Life itself. Science has become a major part of the stock of our minds; its products are the furniture of our surroundings. We must accept it, as the good lady of the fable is said to have agreed to accept the Universe.

Yet the question is puzzling rather than mysterious. Science is very clearly a conscious artifact of mankind, with well-documented historical origins, with a definable scope and content, and with recognizable professional practitioners and exponents. The task of defining Poetry, say, whose subject matter is by common consent ineffable, must be self-defeating. Poetry has no rules, no method, no graduate schools, no logic: the bards are self-anointed and their spirit bloweth where it listeth. Science, by contrast, is rigorous, methodical, academic, logical and practical. The very facility that it gives us, of clear understanding, of seeing things sharply in focus, makes us feel that the instrument itself is very real and hard and definite. Surely we can state, in a few words, its essential nature.

It is not difficult to state the order of being to which Science belongs. It is one of the categories of the intellectual commentary that Man makes on his World. Amongst its kith and kin we would put Religion, Art, Poetry, Law, Philosophy, Technology, etc.—the familiar divisions or “Faculties” of the Academy or the Multiversity.

At this stage I do not mean to analyse the precise relationship that exists between Science and each of these cognate modes of thought; I am merely asserting that they are on all fours with one another. It makes some sort of sense (though it may not always be a stating a truth) to substitute these words for one another, in phrases like “Science teaches us . . .” or “The Spirit of Law is . . .” or “Technology benefits mankind by . . .” or “He is a student of
of presentation should be followed. (However, it is not necessary that every reading within each part be used.)

A few other comments are in order. First, although all three of us worked closely together on all of the material included, the general introduction and the introductions to the six parts were written individually, not collectively. (The initials at the end of each indicate primary authorship.) Second, the Study Questions at the end of each of the six parts were composed by the author of the introduction to that part. Third, the bibliographies—one is given at the end of each part (and at the end of the general introduction)—are not exhaustive. They are intended to provide further sources which deal with some of the major issues discussed in the volume. Fourth, although we have spent much time in revising our format and the selection of readings, we may have overlooked some items which ought to have been included. If so, we shall be grateful to hear from instructors who use the volume and to receive their suggestions with regard to this—or any other—issue.

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PART ONE


PART TWO