GENERAL INTRODUCTION

Every inquiry makes certain assumptions about what it's trying to find and how best to look for it. Science is no exception. The philosophy of science critically examines the assumptions underlying scientific inquiry in an attempt to understand its nature and evaluate its results. It is commonly assumed, for example, that scientific inquiry gives us objective knowledge of the world by providing explanations based on laws or theories that have been confirmed through observation or experiment. To evaluate this claim, the philosophy of science asks such questions as: What is a scientific theory? What distinguishes scientific theories from nonscientific ones? How are scientific theories confirmed? What is a scientific law? What is a scientific explanation? How are different sciences related to one another? Are all sciences reducible to physics? What is the relationship between theory and observation? Is what we observe affected by the theory we accept? If so, can scientific inquiry be objective? Are scientific theories invented or discovered? If they are invented, what guarantee do we have that they correspond to reality? What is the relation between science and religion? Can everything be explained in natural terms, or must we appeal to the supernatural? By answering these questions, we not only gain a better understanding of science, but we also gain a better understanding of the scope and limits of human knowledge.

Philosophy is not the only discipline that studies science. History, psychology, and sociology have also made a study of science. But their concern is with the action of scientists, not with the underlying assumptions of science. They attempt to determine what caused certain scientists or groups of scientists to behave as they did, not whether the assumptions underlying their behavior are justified. A historian may want to know what effect the Inquisition had on the development of science. A psychologist may want to know why James Watson (one of the discoverers of DNA) treated the research of Rosaline Franklin the way he did. A sociologist may want to know why the Chinese never developed a science comparable to that of the West. But none of these disciplines attempts to analyze the conceptual underpinnings of science in the way that philosophy does.

What distinguishes good science from bad science is not a question that can be answered by science. We can't use a particular model of science to determine whether that model is adequate, because in using it we would have already assumed

its adequacy. Any judgment concerning the acceptability of a particular mode of inquiry must appeal to general principles of knowledge. Only philosophy provides the conceptual resources needed to make such a judgment.

Because of its interest in the nature of knowledge and reality, philosophy has long been concerned with the nature of science, supposedly the most reliable source of knowledge about reality. Philosophical inquiries into the scientific enterprise have not only elucidated it; they have also changed it. For over 2000 years science was conducted using an Aristotelian model: to understand something was to know its purpose. Philosophical critiques of this notion led to the scientific revolution of the sixteenth century. Scientists now seek to understand things by identifying their causes.

In the twentieth century, our understanding of the nature of science has undergone almost as radical a transformation as it did in the sixteenth. New developments in logic led a number of thinkers to believe that the notions of confirmation, explanation, and reduction could be expressed in purely formal terms. Known as logical positivists, they developed the most detailed and precise philosophy of science to date. The logical positivists were empiricists: they believed that the only source of knowledge about the world is sense experience. They believed that sense experience is the same for everybody and that it can be expressed in a language free of any theoretical bias. This, they thought, is what makes science objective. Both of these positions have been called into question, and so has the objectivity of science. Many "postmodernists" do not believe that science provides us with objective knowledge. For them, science is merely one way of making sense of the world, and not necessarily the best. To help the reader understand how someone could hold this view, this anthology presents the classic critiques of logical positivism that led up to it.

The text is divided into nine parts. The first deals with the problem of demarcation: what distinguishes science from nonscience? The second deals with the nature of scientific inference: what is the relationship between theories and their data? The third deals with the nature of scientific theories: what is a scientific explanation? The fourth deals with the question of the unity of science: are all sciences reducible to physics? The fifth deals with the nature of observation: is all observation theory laden? The sixth deals with the question of scientific objectivity: is scientific inquiry free of any bias? The seventh deals with the status of theoretical entities: do theoretical entities such as electrons exist? The eighth deals with the relationship between science and religion: is science a religion? Finally, the ninth part deals with some contemporary issues in the philosophy of physics, the philosophy of biology, and the philosophy of psychology.

PART 1

SCIENCE AND NONSCIENCE
Defining the Boundary

Science is considered by many to be the royal road to the truth. If you want to acquire knowledge about the world, they say, you should either consult a scientist or engage in scientific inquiry. But many different disciplines claim to be scientific, and not all of them are. So to keep from falling into error, we need some way of telling legitimate scientific claims from illegitimate ones. What makes a claim scientific? What distinguishes real science from pseudoscience? This is the problem of demarcation. Nowhere is this question more pressing than in the creation-versus-evolution controversy.

Scientific creationism as propounded by the Institute for Creation Research holds (1) that the universe, energy, and life were created from nothing relatively recently (around 6,000 to 10,000 years ago), (2) that living things could not have developed from a single organism through mutation and natural selection, (3) that there is very little variation among members of the same species, (4) that humans did not develop from the apes, and (5) that the earth's geology can be explained by the occurrence of various catastrophes, including a worldwide flood. This account of the creation of the universe and its inhabitants is derived primarily from the Bible's Book of Genesis.

Promoting religion in the public schools, however, violates the establishment clause of the First Amendment, which reads, "Congress shall make no law respecting an establishment of religion." Consequently, the courts have consistently found laws requiring the teaching of creationism to be unconstitutional. Our concern, however, is not with the constitutionality of the teaching of creationism but with its status as a scientific theory. We want to know whether creationism really is as good a theory as evolution.

A. J. Ayer presents the logical positivist's view of the distinction between scientific and nonscientific claims. Scientific claims are empirically verifiable: their truth can be established through observation. Other claims, such as "The absolute enters into, but is itself incapable of, evolution and progress," are not empirically verifiable, and hence are nonscientific. Because there is no way to verify such claims, they do not say anything that can be considered true or false. And because these claims have

no truth value, they are cognitively meaningless—they do not convey any information. They may excite certain emotions in us, but they don't tell us anything about the world. The view that only verifiable claims are meaningful is a fundamental principle of logical positivism and is known as "the verifiability theory of meaning."

Karl Popper does not consider verifiability to be the mark of the scientific because verifying theories is too easy. Some theories, such as those of Freud, Adler, and Marx, have been verified many times over and yet do not tell us anything about the world. To be informative, a theory should rule something out; it should "be incompatible with certain results of observation." For example, the statement "Either it's raining or it isn't" tells us nothing about the weather because it's consistent with all possible observations. The same goes for the theories of Freud, Adler, and Marx. There is no conceivable situation that would provide grounds for rejecting these theories. They can account for every eventuality in their respective domains. Because these theories are not falsifiable, Popper claims that they are not scientific.

Thomas Kuhn argues that what's unique about science is its process, not its products. The way that practitioners conduct their inquiry is what distinguishes science from pseudoscience. Popper judges the status of a field by its fruits; if it produces claims that are falsifiable, it is scientific. Kuhn objects that this criterion is too lax because, by its lights, astrology would be a science. Not only has astrology produced many claims that are falsifiable, but many of those claims have turned out to be false. Astrology is not a science because its practitioners do not engage in the sort of puzzle solving that is characteristic of normal science. According to Kuhn, scientific inquiry is guided by a paradigm or model that indicates what problems are worth investigating and how one should go about investigating them. Astrology is a pseudoscience because it is not governed by a paradigm; there are no agreed-upon criteria that astrologers use to determine what an astrological problem is or how one should go about solving it.

Imre Lakatos agrees with Kuhn that scientific inquiry must be guided by a paradigm. But he disagrees with Kuhn's assertion that the choice of paradigms is not governed by rules or reason. For if that were true, we would have to conclude that "scientific revolution is irrational; a matter of mob psychology." Lakatos proposes what he calls a "sophisticated falsificationist theory," which maintains that one theory has been falsified if and only if another theory has been found that explains everything explained by the former theory and also predicts new facts that would have been difficult or impossible to predict from the old theory.

The boundary between science and nonscience became a legal issue after some state legislatures passed laws mandating the teaching of creationism in the classroom. In his ruling regarding the Arkansas statute, Judge Overton identified the following essential characteristics of science: "(1) It is guided by natural law; (2) it has to be explanatory by reference to natural law; (3) it is testable against the empirical world; (4) its conclusions are tentative, i.e., are not necessarily the final word; and (5) it is falsifiable" (*McLean v. Arkansas Board of Education,* 50 L.W. 2412 (1982)). Overton rejected the claim that creationism is science on the grounds that it fails to meet these criteria.

Larry Laudan finds Judge Overton's decision suspect because creationism does meet some of these criteria, and those it doesn't meet are not essential to science. Laudan claims that creationism is both testable and falsifiable because it makes a

number of claims whose falsity can be established through observation, such as the earth is 6,000 years old, there was a worldwide flood, and dinosaurs and men walked the earth at the same time. What's wrong with creationism is not that it isn't falsifiable, but that its claims have been tested and have turned out to be false. What's more, contrary to what Ruse, the author of the next reading, would have us believe, many scientific theories, such as Darwinian evolution, were not initially guided by natural law or explanatory by reference to natural law. In Laudan's view, Judge Overton made the right decision but for the wrong reasons.

Michael Ruse testified at the Arkansas trial and offered the criteria that Laudan attacks. He acknowledges that Darwin's theory of evolution may not have been guided by natural law or explained by reference to natural law when it was introduced in the nineteenth century. But he claims that our concept of science has evolved since then. What we admit as science should be consistent with our current conception of it, which in the case of Darwinian evolution does include those criteria. As for testability, falsifiability, and revisability, Ruse claims that creationism meets none of these criteria because there are no possible circumstances under which the creationists would give up any of the central doctrines. They believe that the Bible is the word of God, and if the evidence seems to conflict with the holy word, it must be mistaken.

1

A. J. AYER

The Elimination of Metaphysics

The traditional disputes of philosophers are, for the most part, as unwarranted as they are unfruitful. The surest way to end them is to establish beyond question what should be the purpose and method of a philosophical inquiry. And this is by no means so difficult a task as the history of philosophy would lead one to suppose. For if there are any questions which science leaves it to philosophy to answer, a straightforward process of elimination must lead to their discovery.

We may begin by criticizing the metaphysical thesis that philosophy affords us knowledge of a reality transcending the world of science and common sense. Later on, when we come to define metaphysics and account for its existence, we shall find that it is possible to be a metaphysician without believing in a transcendent reality; for we shall see that many metaphysical utterances are due to the commission of logical errors, rather than to a conscious desire on the part of their authors to go beyond the limits of experience. But it is convenient for us to take the case of those who believe that it is possible to have knowledge of a transcendent reality as a starting-point for our discussion. The arguments which we use to refute them will subsequently be found to apply to the whole of metaphysics.

One way of attacking a metaphysician who claimed to have knowledge of a reality which transcended the phenomenal world would be to inquire from what premises his propositions were deduced. Must he not begin, as other men do, with the evidence of his senses? And if so, what valid process of reasoning can possibly lead him to the conception of a transcendent reality? Surely from empirical premises nothing whatsoever concerning the properties, or even the existence, of anything super-empirical can legitimately be inferred. But this objection would be met by a denial on the part of the metaphysician that his assertions were ultimately based on the evidence of his senses. He would say that he was endowed with a faculty of intellectual intuition which enabled him to know facts that could not be known through sense-experience. And even if it could be shown that he was relying on empirical premises, and that his venture into a nonempirical world was therefore logically unjustified, it would not follow that the assertions which he made concerning this nonempirical world could not be true. For the fact that a conclusion does not follow from its putative premise is not sufficient to show that it is false. Consequently one cannot overthrow a system of transcendent metaphysics merely by criticizing the way in which it comes into being. What is required is rather a criticism of the nature of the actual statements which comprise it. And this is the line of argument which we shall, in fact, pursue. For we shall maintain that no statement which refers to a "reality" transcending the limits of all possible sense-experience can possibly have any literal significance; from which it must follow that the labors of those who have striven to describe such a reality have all been devoted to the production of nonsense.

It may be suggested that this is a proposition which has already been proved by Kant. But although Kant also condemned transcendent metaphysics, he did so on different grounds. For he said that the human understanding was so constituted that it lost itself in contradictions when it ventured

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Language, Truth, and Logic (New York: Dover, 1952), pp. 33–45. Reprinted by permission of the publisher.

out beyond the limits of possible experience and attempted to deal with things in themselves. And thus he made the impossibility of a transcendent metaphysic not, as we do, a matter of logic, but a matter of fact. He asserted, not that our minds could not conceivably have had the power of penetrating beyond the phenomenal world, but merely that they were in fact devoid of it. And this leads the critic to ask how, if it is possible to know only what lies within the bounds of sense-experience, the author can be justified in asserting that real things do exist beyond, and how he can tell what are the boundaries beyond which the human understanding may not venture, unless he succeeds in passing them himself. As Wittgenstein says, "in order to draw a limit to thinking, we should have to think both sides of this limit,"[1] a truth to which Bradley gives a special twist in maintaining that the man who is ready to prove that metaphysics is impossible is a brother metaphysician with a rival theory of his own.[2]

Whatever force these objections may have against the Kantian doctrine, they have none whatsoever against the thesis that I am about to set forth. It cannot here be said that the author is himself overstepping the barrier he maintains to be impassable. For the fruitlessness of attempting to transcend the limits of possible sense-experience will be deduced, not from a psychological hypothesis concerning the actual constitution of the human mind, but from the rule which determines the literal significance of language. Our charge against the metaphysician is not that he attempts to employ the understanding in a field where it cannot profitably venture, but that he produces sentences which fail to conform to the conditions under which alone a sentence can be literally significant. Nor are we ourselves obliged to talk nonsense in order to show that all sentences of a certain type are necessarily devoid of literal significance. We need only formulate the criterion which enables us to test whether a sentence expresses a genuine proposition about a matter of fact, and then point out that the sentences under consideration fail to satisfy it. And this we shall now proceed to do. We shall first of all formulate the criterion in somewhat vague terms, and then give the explanations which are necessary to render it precise.

The criterion which we use to test the genuineness of apparent statements of fact is the criterion of verifiability. We say that a sentence is factually significant to any given person, if, and only if, he knows how to verify the proposition which it purports to express—that is, if he knows what observations would lead him, under certain conditions, to accept the proposition as being true, or reject it as being false. If, on the other hand, the putative proposition is of such a character that the assumption of its truth, or falsehood, is consistent with any assumption whatsoever concerning the nature of his future experience, then, as far as he is concerned, it is, if not a tautology, a mere pseudo-proposition. The sentence expressing it may be emotionally significant to him; but it is not literally significant. And with regard to questions the procedure is the same. We inquire in every case what observations would lead us to answer the question, one way or the other; and, if none can be discovered, we must conclude that the sentence under consideration does not, as far as we are concerned, express a genuine question, however strongly its grammatical appearance may suggest that it does.

As the adoption of this procedure is an essential factor in the argument of this book, it needs to be examined in detail.

In the first place, it is necessary to draw a distinction between practical verifiability, and verifiability in principle. Plainly we all understand, in many cases believe, propositions which we have not in fact taken steps to verify. Many of these are propositions which we could verify if we took enough trouble. But there remain a number of significant propositions, concerning matters of fact, which we could not verify even if we chose; simply because we lack the practical means of placing ourselves in the situation where the relevant observations could be made. A simple and familiar example of such a proposition is the proposition that there are mountains on the farther side of the moon.[3] No rocket has yet been invented which would enable me to go and look at the farther side of the moon, so that I am unable to decide the matter by actual observation. But I do know what observations would decide it for me, if, as is theoretically conceivable, I were once in a position to make them. And therefore I say that the proposition is verifiable in principle, if not in practice, and is accordingly significant. On the other hand, such a

metaphysical pseudo-proposition as "the Absolute enters into, but is itself incapable of, evolution and progress,"[4] is not even in principle verifiable. For one cannot conceive of an observation which would enable one to determine whether the Absolute did, or did not, enter into evolution and progress. Of course it is possible that the author of such a remark is using English words in a way in which they are not commonly used by English-speaking people, and that he does, in fact, intend to assert something which could be empirically verified. But until he makes us understand how the proposition that he wishes to express would be verified, he fails to communicate anything to us. And if he admits, as I think the author of the remark in question would have admitted, that his words were not intended to express either a tautology or a proposition which was capable, at least in principle, of being verified, then it follows that he has made an utterance which has no literal significance even for himself.

A further distinction which we must make is the distinction between the "strong" and the "weak" sense of the term "verifiable." A proposition is said to be verifiable, in the strong sense of the term, if, and only if, its truth could be conclusively established in experience. But it is verifiable, in the weak sense, if it is possible for experience to render it probable. In which sense are we using the term when we say that a putative proposition is genuine only if it is verifiable?

It seems to me that if we adopt conclusive verifiability as our criterion of significance, as some positivists have proposed,[5] our argument will prove too much. Consider, for example, the case of general propositions of law—such propositions, namely, as "arsenic is poisonous"; "all men are mortal"; "a body tends to expand when it is heated." It is of the very nature of these propositions that their truth cannot be established with certainty by any finite series of observations. But if it is recognized that such general propositions of law are designed to cover an infinite number of cases, then it must be admitted that they cannot, even in principle, be verified conclusively. And then, if we adopt conclusive verifiability as our criterion of significance, we are logically obliged to treat these general propositions of law in the same fashion as we treat the statements of the metaphysician.

In face of this difficulty, some positivists[6] have adopted the heroic course of saying that these general propositions are indeed pieces of nonsense, albeit an essentially important type of nonsense. But here the introduction of the term "important" is simply an attempt to hedge. It serves only to mark the authors' recognition that their view is somewhat too paradoxical, without in any way removing the paradox. Besides, the difficulty is not confined to the case of general propositions of law, though it is there revealed most plainly. It is hardly less obvious in the case of propositions about the remote past. For it must surely be admitted that, however strong the evidence in favor of historical statements may be, their truth can never become more than highly probable. And to maintain that they also constituted an important, or unimportant, type of nonsense would be unplausible, to say the very least. Indeed, it will be our contention that no proposition, other than a tautology, can possibly be anything more than a probable hypothesis. And if this is correct, the principle that a sentence can be factually significant only if it expresses what is conclusively verifiable is self-stultifying as a criterion of significance. For it leads to the conclusion that it is impossible to make a significant statement of fact at all.

NOTES

1. *Tractatus Logico-Philosophicus,* Preface.
2. Bradley, *Appearance and Reality,* 2nd ed., p. 1.
3. This example has been used by Professor Schlick to illustrate the same point.
4. A remark taken at random from *Appearance and Reality,* by F. H. Bradley.
5. E.g., M. Schlick, "Positivismus und Realismus," *Erkenntnis,* Vol. I, 1930. F. Waismann, "Logische Analyse des Warscheinlichkeitsbegriffs," *Erkenntnis,* Vol. I, 1930.
6. E.g., M. Schlick, "Die Kausalität in der gegenwärtigen Physik," *Naturwissenschaft,* Vol. 19, 1931.

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KARL R. POPPER

Science: Conjectures and Refutations

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 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 cooperate 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,

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Conjectures and Refutations: The Growth of Scientific Knowledge (London: Routledge and Kegan Paul, 1963), pp. 33–39. Reprinted with permission.

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-analyzed' 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 analyzing 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 behavior: 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 behavior 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 favor 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.[1] 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 behavior, so that it was practically impossible to describe any human behavior 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 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'.)
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 reinterpreting 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 unfavorable 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.[2] 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 behavior 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 naïvely believe confirm their theory cannot do this any more than the daily confirmations which astrologers find in their practice.[3] 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'.[4] 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.

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 refute 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. (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. 315: '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.')

4. The case of astrology, nowadays a typical pseudo-science, may illustrate this point. It was attacked, by Aristotelians and other rationalists, down to Newton's day, for the wrong reason—for its now accepted assertion that the planets had an 'influence' upon terrestrial ('sublunar') events. In fact Newton's theory of gravity, and especially the lunar theory of the tides, was historically speaking an offspring of astrological lore. Newton, it seems, was most reluctant to adopt a theory which came from the same stable as for example the theory that 'influenza' epidemics are due to an astral 'influence'. And Galileo, no doubt for the same reason, actually rejected the lunar theory of the tides; and his misgivings about Kepler may easily be explained by his misgivings about astrology.

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THOMAS S. KUHN

Logic of Discovery or Psychology of Research?

My object in these pages is to juxtapose the view of scientific development outlined in my book, *The Structure of Scientific Revolutions,* with the better-known views of our chairman, Sir Karl Popper.[1] Ordinarily I should decline such an undertaking, for I am not so sanguine as Sir Karl about the utility of confrontations. Besides, I have admired his work for too long to turn critic easily at this date. Nevertheless, I am persuaded that for this occasion the attempt must be made. Even before my book was

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Criticism and the Growth of Knowledge, Imre Lakatos and Alan Musgrave, eds. (New York: Cambridge University Press, 1970), pp. 1–23. Reprinted with permission of the publisher.