Science: Conjectures and Refutations

KARL POPPER

Karl Popper (1902–1994) was one of the great philosophers of science of the twentieth century.

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

I

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.

From Karl Popper, *Conjectures and Refutations* (London: Routledge, 1963), pp. 33–38. Reprinted by permission of the estate of Karl Popper.

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, psychoanalysis, 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? Nor 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 nonscientific. 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 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 re-interpreted both the theory and the evidence in order to make them agree. In this way they rescued the theory from refutation; but they did so at the price of adopting a device which made it irrefutable. They thus gave a "conventionalist twist" to the theory; and by this stratagem they destroyed its much advertised claim to scientific status.

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

Believing Where We Cannot Prove

PHILIP KITCHER

Philip Kitcher is Professor of Philosophy at Columbia University. He has published widely in philosophy of mathematics and philosophy of science.

OPENING MOVES

Simple distinctions come all too easily. Frequently we open the way for later puzzlement by restricting the options we take to be available. So, for example, in contrasting science and religion, we often operate with a simple pair of categories. On one side there is science, proof, and certainty; on the other, religion, conjecture, and faith.

The opening lines of Tennyson's *In Memoriam* offer an eloquent statement of the contrast:

> *Strong Son of God, immortal love,*
> *Whom we, that have not seen Thy face,*
> *By faith, and faith alone, embrace,*
> *Believing where we cannot prove.*

A principal theme of Tennyson's great poem is his struggle to maintain faith in the face of what seems to be powerful scientific evidence. Tennyson had read a popular work by Robert Chambers, *Vestiges of the Natural History of Creation,* and he was greatly troubled by the account of the course of life on earth that the book contains. *In Memoriam* reveals a man trying to believe where he cannot prove, a man haunted by the thought that the proofs may be against him.

Like Tennyson, contemporary Creationists accept the traditional contrast between science and religion. But where Tennyson agonized, they attack. While they are less eloquent, they are supremely confident of their own solution. They open their onslaught on evolutionary theory by denying that it is a science. In *The Troubled Waters of Evolution,* Henry Morris characterizes evolutionary theory as maintaining that large amounts of time are required for evolution to produce "new kinds." As a result, we should not expect to see such "new kinds" emerging. Morris comments, "Creationists in turn insist that this belief is not scientific evidence but only a statement of faith. The evolutionist seems to be saying, Of course, we cannot really *prove* evolution, since this requires ages of time, and so, therefore, you should accept it as a proved fact of science! Creationists regard this as an odd type of logic, which would be entirely unacceptable in any other field of science" (Morris 1974b, 16). David Watson makes a similar point in comparing Darwin with Galileo: "So here is the difference between Darwin and Galileo: Galileo set a demonstrable *fact* against a few words of Bible poetry which the Church at that time had understood in an obviously naive way; Darwin set an unprovable *theory* against eleven chapters of straightforward history which cannot be reinterpreted in any satisfactory way" (Watson 1976, 46).

The idea that evolution is conjecture, faith, or "philosophy" pervades Creationist writings. It is absolutely crucial to their case for equal time for "scientific" Creationism. This ploy has succeeded in winning important adherents to the Creationist cause. As he prepared to defend

From Philip Kitcher, *Abusing Science: The Case Against Creationism,* pp. 30–38, 42–46, 48–49 (Cambridge: MIT Press, 1982). Reprinted by permission of MIT Press.

Arkansas law 590, Attorney General Steven Clark echoed the Creationist judgment. "Evolution," he said, "is just a theory." Similar words have been heard in Congress. William Dannemeyer, a congressman from California, introduced a bill to limit funding to the Smithsonian with the following words: "If the theory of evolution is just that—a theory—and if that theory can be regarded as a religion . . . then it occurs to this Member that other Members might prefer it not to be given exclusive or top billing in our Nation's most famous museum but equal billing or perhaps no billing at all."

In their attempt to show that evolution is not science, Creationists receive help from the least likely sources. Great scientists sometimes claim that certain facts about the past evolution of organisms are "demonstrated" or "indubitable." But Creationists also can (and do) quote scientists who characterize evolution as "dogma" and contend that there is no conclusive proof of evolutionary theory. Evolution is not part of science because, as evolutionary biologists themselves concede, science demands proof, and, as other biologists point out, proof of evolution is not forthcoming.

The rest of the Creationist argument flows easily. We educate our children in evolutionary theory as if it were a proven fact. We subscribe officially, in our school system, to one faith—an atheistic, materialistic faith—ignoring rival beliefs. Antireligious educators deform the minds of children, warping them to accept as gospel a doctrine that has no more scientific support than the true Gospel. The very least that should be done is to allow for both alternatives to be presented.

We should reject the Creationists' gambit. Eminent scientists notwithstanding, science is not a body of demonstrated truths. Virtually all of science is an exercise in believing where we cannot prove. Yet, scientific conclusions are not embraced by faith alone. Tennyson's dichotomy was too simple.

INCONCLUSIVE EVIDENCE

Sometimes we seem to have conclusive reasons for accepting a statement as true. It is hard to doubt that $2 + 2 = 4$. If, unlike Lord Kelvin's ideal mathematician, we do not find it obvious that

$$\int_{-\infty}^{+\infty} e^{-x^2}\,dx = \sqrt{\pi},$$

at least the elementary parts of mathematics appear to command our agreement. The direct evidence of our senses seems equally compelling. If I see the pen with which I am writing, holding it firmly in my unclouded view, how can I doubt that it exists? The talented mathematician who has proved a theorem and the keen-eyed witness of an episode furnish our ideals of certainty in knowledge. What they tell us can be engraved in stone, for there is no cause for worry that it will need to be modified.

Yet, in another mood, one that seems "deeper" or more "philosophical," skeptical doubts begin to creep in. Is there really anything of which we are so certain that later evidence could not give us reason to change our minds? Even when we think about mathematical proof, can we not imagine that new discoveries may cast doubt on the cogency of our reasoning? (The history of mathematics reveals that sometimes what seems for all the world like a proof may have a false conclusion.) Is it not possible that the most careful observer may have missed something? Or that the witness brought preconceptions to the observation that subtly biased what was reported? Are we not *always* fallible?

I am mildly sympathetic to the skeptic's worries. Complete certainty is best seen as an ideal toward which we strive and that is rarely, if ever, attained. Conclusive evidence always eludes us. Yet even if we ignore skeptical complaints and imagine that we are sometimes lucky enough to have conclusive reasons for accepting a claim as true, we should not include scientific reasoning among our paradigms of proof. Fallibility is the hallmark of science.

This point should not be so surprising. The trouble is that we frequently forget it in discussing contemporary science. When we turn to the history of science, however, our fallibility stares us in the face. The history of the natural sciences is strewn with the corpses of intricately organized theories, each of which had, in its