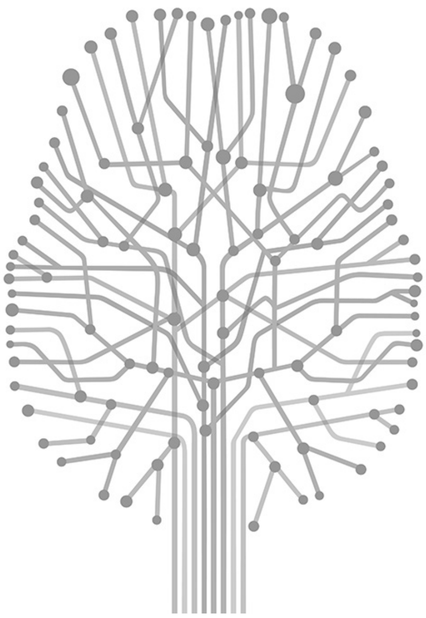
THE MEANING OF SCIENCE

An Introduction to the Philosophy of Science

Tim Lewens



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Chapter One

How Science Works

Science and Pseudoscience

There are sciences. Physics is one, chemistry another. There are also disciplines that involve the generation of knowledge and insight, but that few of us would immediately think of as sciences. History and literary studies are examples. All this is fairly uncontroversial. But there are cases where we are unsure about what counts as science, and these cases are sometimes politically and culturally explosive.

Consider the trio of economics, intelligent-design theory, and homeopathy. The only thing that unites these three endeavors is that their scientific status is regularly questioned in ways that provoke stormy debate. Is economics a science? On the one hand, like many sciences, it oozes both mathematics and authority. On the other hand it is poor at making predictions, and many of its practitioners are surprisingly blasé when it comes to finding out about how real people think and behave.[1] They would rather build models that tell us what would happen, under simplified circumstances, if people were perfectly rational.

So perhaps economics is less like science, and more akin to *The Lord of the Rings* with equations: it is a mathematically sophisticated exploration of an invented world not much like our own.

The theory of intelligent design has been promoted by organizations like the prominent US think tank The Discovery Institute, and developed by theorists including the biochemist Michael Behe and the mathematician/philosopher William Dembski. It aims to compete with the theory of evolution as an account of how species became well adapted to their surroundings. It suggests that some organic traits are too complex to have been produced by natural selection, and that they must instead have been produced by some form of intelligent oversight: perhaps God, perhaps some other intelligent agent. The theory is positioned as a science by its adherents, but many sensible commentators think that this is merely an attempt to insert a contentious interpretation of religion into schools, and that—understood as a piece of science—the theory is hopeless.[2]

Mainstream doctors sometimes value homeopathic remedies, in spite of the fact that their track record of validation by large-scale clinical studies is poor. One camp says that these are quack treatments with no scientific credentials, whose apparent effectiveness derives from nothing more than the placebo effect.[3] Another camp tells us that the dominant method by which scientific investigation establishes the credentials of medical interventions gives us generic wisdom regarding what works in typical circumstances for average patients, but that this approach ignores the need for doctors to prescribe what is right for a unique individual in idiosyncratic circumstances.[4]

These questions about the markers of proper science are important. They affect the power held by people whose advice can determine our financial and social well-being; they affect what our children are taught at school; they affect what forms of research our tax contributions can be used to fund and how our doctors advise that we maintain our health. These questions are also old: while today we might be concerned by the scientific status of enterprises like economics, intelligent design, and homeopathy, previous thinkers have been troubled by the scientific status of Marxism, psychoanalysis, and even evolutionary biology. What we need, it seems, is a clear account of what makes something a science and what makes something pseudoscience. What we need, it seems, is Karl Popper.

Sir Karl Popper (1902–1994)

It is still the case that if you ask a scientist to reflect on the general nature of science, you will probably be referred to the pronouncements of Karl Popper. Popper was born in Vienna in 1902, a time when Viennese cultural life was blessed with an extraordinary richness. He began attending the University of Vienna in 1918, where he exposed himself to the conspicuous intellectual movements of the moment. He became involved with left-wing politics, he adopted Marxism for a time, he listened to a lecture on relativity theory by Einstein, and he briefly served as a volunteer social worker in one of the clinics founded by psychotherapist Alfred Adler. In 1928 Popper was awarded a PhD in philosophy, and by 1934 he had published his first book, *Logik der Forschung* (later translated into English as *The Logic of Scientific Discovery*).[5] The broad conception of scientific progress laid out in that book would remain more or less intact in Popper's thinking until his death.

Popper—whose parents were of Jewish origin—was forced to leave Vienna in the 1930s. He moved to New Zealand, to a position at the University of Canterbury in Christchurch, where he spent nearly ten years before moving back to Europe. In 1946 he was offered a post at the London School of Economics (LSE), which he held until his retirement. The philosopher of science Donald Gillies, who first met Popper at the LSE in 1966, recently painted a lively picture of some of Popper's idiosyncrasies:

> Waiting in the lecture hall for Popper to appear was not without some amusement, because a ritual was always performed before the great man entered the door. Two of Popper's research assistants would come into the room before him, open all the windows, and urge the audience on no account to smoke, while writing: NO SMOKING on the blackboard. Popper had indeed a very strong aversion to smoking. He claimed that he had a very severe allergy to tobacco smoke, so that inhaling even a very small quantity would make him seriously ill. When his research assistants had reported back that the zone was smoke-free, Popper would enter the room.[6]

Gillies goes on to explain that when Popper went to a specialist in allergies, the expert was unable to find any evidence of an allergy to tobacco smoke: "Popper's comment on the result was: 'This goes to show how backward medical science still is.'"[7]

Perhaps the high point of Popper's reputation came in the late 1960s and early 1970s. He was knighted in 1965, and around this time a string of distinguished scientists described his work in tones of dazzled admiration. Sir Peter Medawar, a Nobel Prize winner for medicine, said simply: "I think Popper is incomparably the greatest philosopher of science that has ever been." Sir Hermann Bondi, mathematician and cosmologist, took the view that "there is no more to science than its method, and there is no more to its method than Popper has said."[8]

Some more of Donald Gillies's recollections make it clear that Popper could provoke exasperation, as well as admiration. On

Tuesday afternoons, the London School of Economics hosted the "Popper Seminar," where visiting speakers were invited to present their philosophical views. In a standard academic seminar of this kind, the speaker might talk unmolested for thirty or forty minutes, before the chair invites questions from the audience. At the Popper Seminar, things were different:

> Usually the speaker was allowed to talk for only about 5 to 10 minutes before he was interrupted by Popper. Popper would leap to his feet, saying that he wanted to make a comment, and then talk for 10 to 15 minutes. A typical intervention by Popper would have the following form. He would begin by summarising what the speaker had said so far. Then he would produce an argument against what the speaker had said, and he would usually conclude with a remark like: "Would you agree then that this is a fatal objection to your position?" As can be imagined such an attack would often have a very disconcerting effect on the visiting speaker.

Gillies adds: "It is easy to see that while, from Popper's point of view, his seminar could be seen as a perfect example of 'free criticism,' it could have seemed to the speaker very much like a session of the committee on un-Popperian activities."[9]

"What Is Wrong with Marxism, Psychoanalysis, and Individual Psychology?"

Popper's basic outlook on science derived from two underlying sources of discomfort. He had grown up in a place and a time of intoxicating intellectual excitement. He recalled that "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."[10] Various grand intellectual systems of exceptional ambition—Einstein's relativity theory, Karl Marx's theory of history, diverse psychoanalytic understandings of the mind—were in common currency. And yet, Popper felt that there was a deep difference between relativity theory, which he venerated, and (for example) psychoanalytic theory, of which he was deeply suspicious.

He set himself the task of clarifying his intuition: "What is wrong," he asked himself, "with Marxism, psychoanalysis and individual psychology? Why are they so different from physical theories, from Newton's theory, and especially from the theory of relativity?"[11] Popper's view was that while Einstein had proposed a theory that was heroically vulnerable to destruction if experiment should show it false—and yet it had nonetheless enjoyed spectacular experimental successes—the psychoanalytic theory of mind was couched in such noncommittal terms that it was immune to experimental refutation. "I felt," he said, "that these other theories, though posing as sciences, had in fact more in common with primitive myths than with science; that they resembled astrology rather than astronomy."[12]

The problem with the predictions of newspaper astrology columns is not that they don't come true: the problem is that they are formulated in such a way that they cannot but come true, and because of that they say nothing of value. My own *Daily Mail* horoscope for the week I write these words tells me: "You have faced more downs than ups in recent weeks, but now things are about to change. With both the Sun and Venus, planet of harmony, entering your birth sign this week, you can stop worrying about the past and start planning for the future. This is also the time to bring to the boil something that has been on the back burner for too long."[13] How often would we think it sensible to advise someone to "stop planning for the future and start worrying about the past"? If something has indeed been on the back burner for "too long," doesn't that make it trivially true that now is the time to address it? And how on earth are we supposed to quantify the relative number of "ups" and "downs" we have had over the course of weeks? It is hard to see how we can argue with any of these platitudes.

Similarly, Sigmund Freud recalled how a female patient, whom he described as "the cleverest of all my dreamers," told him of a dream that seemed to refute his own theory of wish fulfillment. That theory says that in dreams our wishes come true:

> One day I had been explaining to her that dreams are fulfilment of wishes. Next day she brought me a dream in which she was traveling down with her mother-in-law to the place in the country where they were to spend their holidays together. Now I knew that she had violently rebelled against the idea of spending the summer near her mother-in-law and that a few days earlier she had successfully avoided the propinquity she dreaded by engaging rooms in a far distant resort. And now her dream had undone the solution she wished for: was not this the sharpest possible contradiction of my theory that in dreams wishes are fulfilled?[14]

This woman dreamed, not of something she wanted to do, but of something she abhorred: a holiday with her mother-in-law. In spite of apparent refutation, Freud argued that his theory was intact: "The dream showed I was wrong. *Thus it was her wish that I might be wrong, and her dream showed that wish fulfilled.*"[15] A dream that seems to jar against Freud's theory is explained away with the argument that the woman wanted Freud to be wrong, and the dream allowed this desire to be fulfilled. It is hard not to share Popper's discomfort in the face of examples such as these. Freud's ability to cook up interpretations of the evidence that bring it into line with his theory hardly seems a strength of his psychoanalytic approach; instead, the elastic ability of his theory to stretch around whatever evidence may confront it seems more like a weakness.

The Problem of Induction

One set of Popper's concerns derived from this urgent sense that we should be able to give a "criterion of demarcation'" that will tell us how to sort science from pseudoscience. The second set of concerns came instead from Popper's deep skepticism of what philosophers call *inductive inference.* The eighteenth-century Scottish philosopher David Hume is usually credited with being the first to pose what we now call "the problem of induction." To understand this problem, we first need to understand the nature of *deductive*—as opposed to inductive—inference.

Suppose you know that all badgers are mammals, and you know that Brock is a badger. Given these premises, you can safely conclude that Brock is a mammal. This inference is *deductively valid,* meaning that it is strictly impossible for the premises of the inference to be true, and the conclusion false. There is no way that we could imagine circumstances under which all badgers are mammals, Brock is a badger, and yet Brock is not a mammal. Good deductive inferences deal in certainty: their premises ensure their conclusions. Because of this, deductive inferences are often trivial or unproductive: there is a sense in which, armed with the knowledge that Brock is a badger, and that all badgers are mammals, you are simply spelling out a self-evident consequence of those pieces of information when you go on to conclude that Brock is a mammal.

Inductive inferences are different. Suppose you have invented a new drug—let's call it *Veritor*—and you want to find out if it is safe. You test it on ten thousand people, and over a period of many months you do not detect adverse side effects in any of them. The people you choose to test are not all the same: you make sure you have tried the drug out on men, women, people of different ages, and people from different countries. Now suppose you ask the question: "Given that everyone tested so far has experienced no side effects, should we expect Colin, who has never taken the drug before, to experience adverse side effects?" I doubt that anyone would say we can be absolutely sure that Colin will be fine, but most people would say that it is reasonable to expect, on the basis of our extensive testing of the drug, that Colin will probably experience no adverse reaction.

Inferences of this sort are potentially far more valuable than deductive inferences, for they promise to generate important new knowledge. By looking at large, but limited, samples of people, we presume that we can make fairly reliable predictions about how other people are likely to react. Our practices of drug testing—and almost all other forms of knowledge-generation—seem to presuppose that it is reasonable to generalize in this way, via extrapolation from a limited number of observed instances. What makes this presupposition reasonable? The challenge inherited from Hume is to provide a justification for inductive inferences of this sort.

An inductive inference can be defined as any pattern of argument that we regard as reasonable, but which does not claim deductive validity. Our inference about Colin is not deductively valid, and it does not pretend to be. It does not deal in certainty, for clearly it is possible for ten thousand people to have experienced no side effects and for poor Colin to be the first to react badly. Such circumstances can easily be imagined without contradiction—perhaps Colin has an exceptionally rare genetic mutation—and it is partly because of this that we cannot be sure that Colin will be free from adverse reactions. Even so, we do take the view that our evidence, derived from testing thousands of people, makes it reasonable to conclude that Colin is unlikely to suffer adverse reactions. What makes this inductive inference reasonable?

We might try to justify our inference by appealing to further pieces of scientific research. For example, we might point out that for Colin to react in a way that is different from every one of the ten thousand individuals we tested previously, Colin would need a very unusual sort of body. We might go on to claim that it is reasonable, although not a certainty, to think that Colin's body is typical, because human conception and development run along well-understood lines. The processes by which human bodies are typically made have been studied in painstaking detail by physiologists and developmental biologists, and this research gives us knowledge about how Colin's body probably works, what constitutes his genetic makeup, and so forth.

This appeal to background scientific knowledge does not solve Hume's problem. It simply reveals the depth of our reliance on inductive inference. Scientists have studied a limited number of embryonic unfoldings—in humans, other mammals, and various additional species. We assume that the processes that went into the construction of Colin were most likely similar to the processes that have been observed in the laboratory. Our inference about Colin's constitution is based on extrapolation, and Hume's challenge was to explain why this form of extrapolation should be thought reasonable.

The problem of induction can be put forward as a pithy dilemma: we want to know what, if anything, makes it sensible to extrapolate from a limited sample to a broader generalization. We cannot try to answer this by claiming deductive validity for our inference, for there is evidently no contradiction in the claim that our new case is freakish, and utterly unlike what we have encountered before. But if instead we try to answer our question by pointing to scientific knowledge, or even to the past successes of previous inductive inferences, it seems we are just offering yet more instances of the very extrapolations we are trying to justify. Either way, our initial challenge—what makes extrapolation reasonable—remains unanswered.[16]

It is time to bring our discussion of induction back to Popper. Faced with a tricky crossword puzzle, we know there must be a solution even if we aren't quite sure what that solution is. Most philosophers—but not Popper—think of the problem of induction as a puzzle in this same sense: they have had a devilishly difficult time figuring out what the answer to Hume's challenge is, but they are confident there must be a good answer. After all, no one gets by in day-to-day life without induction. We are all convinced that it is better to attempt to leave a room by opening a door than by walking through the wall. We are so convinced because we extrapolate from past experience of bumps, bruises, and the frustration caused by walking into solid surfaces. When our financial advisors remind us that past successes of investments may not indicate their likely future performance, we accept their warnings because we know how often healthy funds have crashed in the past. Even here, we project past patterns into the future, and we think these extrapolations are sensible.

Popper is an outlier in the debate over induction. He understood Hume to have shown that induction is a bad inferential strategy. A rational person, says Popper, is one who refuses to use inductive inference; that is, she refuses to extrapolate from past to future, from a finite number of observations to a more general theory, or from a limited number of data-points to a broader pattern. Popper's conviction was that "theories can never be inferred from observation statements, or rationally justified by them. I found Hume's refutation of inductive inference clear and conclusive."[17] Popper therefore set out to show how science could proceed using nothing but deductive reasoning.

Falsificationism

Popper's philosophy of science is founded on an undeniable logical asymmetry. As we have seen, no matter how many individuals you have tested and found to respond positively to *Veritor,* deduction will never tell you that all people respond positively to *Veritor.* On the other hand, if you find just one person who responds badly to *Veritor,* you can conclude—with deductive certainty—that the statement "All people respond positively to *Veritor*" is false. If, as Popper recommends, we need to do science without appeal to inductive reasoning, then while we can never conclude reasonably that scientific generalizations are true, we can conclude that some are false, or so it seems. That is why Popper's view is known as *falsificationism.*

One might think that scientists use a variety of data—from the fossil record, from DNA sequences, from the behavioral and anatomical features of plants and animals—to build a case for a more general claim like "All plants and animals are descendants of a common ancestor." That conception of science, says Popper, is mistaken. Only science founded on induction could aim at the slow accumulation of evidence in favor of particular hypotheses, and Popper regards induction as irrational. Instead, science must proceed by a process of "conjecture and refutation": the scientist begins by formulating a general claim about the nature of the world and then seeks to refute it by gathering data—regarding fossils, DNA, behavior, and anatomy—which, if they go the wrong way, have the potential to show decisively that our general claim about ancestry is false.

This helps us to understand Popper's use of falsificationism to supply a "criterion of demarcation," which pinpoints the difference between science and what Popper sometimes called "pseudoscience," sometimes "metaphysics." Bona fide science, he says, must be falsifiable. What makes something a genuine piece of science is its potential vulnerability to refutation. Popper was particularly impressed, for example, by the way in which Einstein's relativity theory had laid itself open to the tribunal of experiment. As we will see in more detail a little later, Einstein's theory made explicit predictions for the bending effect that the Sun would have on light arriving at the Earth. It thereby exposed itself to falsification if light turned out not to behave in this way. A properly scientific theory, says Popper, sticks its neck out regarding the sorts of events that it does not permit, hence regarding the sorts of potential pieces of evidence that would lead to the theory being abandoned.

Popper's recipe has considerable intuitive appeal. Freud's theory of the mind is written off as a piece of pseudoscience, because rather than stating in clear ways the sorts of behaviors that would lead to the theory being dropped, Freud offers slippery formulations of his commitments and slippery interpretations of his data. Likewise, the problem with astrology seems to be that its claims are stated in such intolerably vague ways that we cannot judge what it would take for the theory to be shown wrong. Things seem different with astronomy: Newton's theory tells us precisely when to expect the arrival of a comet, and one might think that if things don't turn out that way, so much the worse for Newton's ideas.

The noted physicist Richard Feynman (yet another Nobel laureate) expressed a strikingly similar conception of science—surely influenced by Popper—in a lecture he gave in 1964:[18]

> In general, we look for a new law by the following process. First, we guess it. . . . No, don't laugh, that's really true. Then we compute the consequences of the guess, to see what, if this is right . . . it would imply, and then we compare those computation results to nature, or . . . to experiment or experience. We compare it directly with observations to see if it works.

Feynman continued with a short summary of the falsificationist approach to scientific method:

> If it disagrees with experiment, it's wrong. In that simple statement is the key to science. It doesn't make any difference how beautiful your guess is, it doesn't make a difference how smart you are, who made the guess, or what his name is. If it disagrees with experiment, it's wrong. That's all there is to it.

Gran Sasso

In September 2011 a team of researchers announced that subatomic particles called neutrinos, sent from the CERN facility in Geneva, had been recorded traveling faster than light when their speed was measured at the Gran Sasso facility in Italy.[19] Einstein's special theory of relativity proposes an upper speed limit governing the universe: nothing travels faster than light in a vacuum. Experiment was inconsistent with Einstein's theory. Feynman's summary of the scientific method predicts that in spite of special relativity's beauty, Einstein's name, and his formidable intelligence, the results from Gran Sasso would lead to this esteemed theory being discarded.

This is not what happened. While newspapers lingered for a while on these results, most scientists felt fairly securely that the experimental results were probably flawed. They felt they were flawed partly because of their confidence in the theory those results appeared to contradict. The truth is that scientists do not throw out their theories whenever an experiment appears to contradict them. This attitude is perfectly sensible, because we are often unsure whether experiments have been conducted properly and what their true significance might be. It is perfectly rational to bet on an experiment being flawed, as opposed to putting our money on a well-tested theory being false. This observation causes no trouble at all for the practice of science, but it causes plenty of trouble for Popper's goal of showing how science might proceed without induction.

In the first place, the Gran Sasso experiment shows the limits of the logical asymmetry on which Popper's falsificationism rests. Yes, if our theory tells us nothing can travel faster than light, and if we find something that does travel faster than light, then we know with certainty that the theory is wrong. But just as our judgment of the speed of a car depends on the accuracy of the devices we use to measure it, so we can never simply "observe" how fast a neutrino is traveling, in some self-certifying manner. We must always ask whether the apparatus was working properly, whether we have interpreted our readings correctly, whether our calculations have been appropriate and accurate.

The data, in spite of their name, are not "given" to us in some incontestable manner. Instead, they are the products of hundreds of technical assumptions, any one of which might be challenged. So, if our theory tells us that nothing travels faster than light, and if our experiment indicates that something does travel faster than light, the only thing we are entitled to conclude as a matter of deductive certainty is that somewhere or another at least one mistake has been made. Deduction cannot tell us where that mistake is, and so deduction cannot tell us, by itself, whether our theory is wrong, whether one of our myriad experimental assumptions is wrong, or whether the whole affair is shot through with errors.

Remember Feynman's claim that if a theory "disagrees with experiment, then it's wrong." At Gran Sasso the experiment disagreed with theory, and everyone instead set out to discern what was wrong with the experiment. It is interesting to note elite physicists' reactions a few days after the Gran Sasso result was announced, voiced before any direct evidence emerged for errors in the experimental setup. At this stage, the community had been presented with a result, from an exceptionally well-regarded research group, that appeared to contradict a cherished theory. Martin Rees (the Astronomer Royal, and a recent president of the Royal Society) remarked calmly that "extraordinary claims require extraordinary evidence." The Nobel laureate Steven Weinberg said, "it bothers me that there is plenty of evidence that all sorts of other particles never travel faster than light, while observations of neutrinos are exceptionally difficult."[20] These scientists (and one might cite others) suggested that if forced to bet on whether the established theory or the shocking experimental result was in error, they would put their money on experimental error. These super-luminaries were skeptical of super-luminal velocity.

Rees and Weinberg's sensible skepticism of the Gran Sasso results relies on an inductive inference: it is not available to the strict Popperian, for whom no extrapolation from a solid track record is reasonable. For Rees and Weinberg, the fact that evidence had built up in the past suggesting that other particles do not travel faster than light, and the fact that Einstein's theory itself had held up so well in the face of experimental tests, constituted reasonable grounds for doubting the Gran Sasso result. More generally, when theory and evidence conflict, scientists use inductive inference to help them decide where a mistake has most likely been made. But for the Popperian, such a decision process is irrational.

"Corroboration"

Popper tells us that scientific theories must put themselves up for test. They must stick their necks out and run the gauntlet of experiment. If observation is at odds with theory, then the theory is refuted. A theory may, of course, survive one of these tests, and some theories have survived many rounds of testing. Popper calls these theories highly "corroborated."

Perhaps the most frequently repeated example of this sort of corroboration is Arthur Eddington's experimental test of Einstein's general theory of relativity. As noted earlier, Einstein's theory predicted that light from distant stars would be bent by the gravitational field of the Sun. This bending effect could be observed only during an eclipse, because otherwise the Sun's own brightness would obscure the stars in question. Eddington traveled in 1919 to the island of Principe, off the West African coast, while his colleagues traveled to Sobral in Brazil, in order to be present during a total eclipse of the Sun. Would Einstein's theory be falsified by Eddington's measurements? No: "The results of the expeditions to Sobral and Principe," wrote Eddington and his colleagues, "can leave little doubt that a deflection of light takes place in the neighbourhood of the Sun and that it is of the amount demanded by Einstein's generalised theory of relativity, as attributable to the Sun's gravitational field."[21]

Eddington's results are typically thought of these days as providing strong evidence in favor of Einstein's theory. But when Popper says that a theory is highly "corroborated," he does not mean that the theory is likely to be correct. "Corroboration" is merely a statement of a theory's past success, and since, for Popper, past success provides no guide whatsoever for future prospects—to think it did would involve a form of inductive inference—this also means we have no reason to think a highly corroborated theory is likely to pass the next test thrown at it.[22]

There is a sense in which, for Popper, our credence in a scientific hypothesis should be unaffected by whether the theory in question has just been plucked from thin air, or whether instead it has a long and distinguished track record of remarkable success in the face of searching experiment. Since corroboration can bear no weight for Popper, this also makes it hard to see how, on Popper's view, scientists like Rees or Weinberg could ever be justified in thinking that because Einstein's ideas have held up so well in the face of severe tests, our suspicions should probably lie with the manner in which the equipment at Gran Sasso was set up.

Theory and Observation

What is the status of the pieces of data, or reports of observations, that the falsificationist thinks scientists can use to reject general theories? Popper insists, with good reason, that observation is "theory-laden." Roughly speaking, this means that apparently neutral statements about observational data are invariably shot through with assumptions about scientific theory. For example, a claim like "We observed a neutrino travel in excess of the speed of light" can be made only when a vast amount of knowledge is presupposed about how neutrinos behave, how they can be detected, and how our instruments work. By itself, this dependence of observation on theory is unproblematic: indeed, if scientific observation were not enabled by theory, then the ability of scientists to probe the inner workings of the universe could not make progress. But the "theory-ladenness of observation," as philosophers like to call it, leads to special problems for Popper.

Popper's rejection of induction means he denies that limited numbers of observations can ever provide support for general theoretical claims. But he also recognizes that statements about what has been observed—what scientists would usually call their data—rely on general theoretical claims as well. In fact, Popper takes the view that *all* "observation statements" are laden with theory—not just exotic claims about how fast a neutrino has traveled but apparently more banal claims about whether a piece of litmus paper turned blue, whether a Geiger counter registered a click, and so forth. Since the data presuppose theory, Popper concludes that observation statements are no less conjectural—hence no less provisional—than the theories they are supposed to falsify.

Popper's deductive method is far less powerful than we might initially think. On the face of things, Popper offers us the consoling thought that even if we can never conclude reasonably that a theory is likely to be true, we can at least conclude that some theories are false. But showing that a theory is false requires that we have justified confidence in the observations that we use to refute the theory in question. If observations themselves are mere conjectures that draw on general theories, and if those general theories cannot be supported by induction, then this confidence can never be had. What scientists can do, in Popper's scheme, is to show that one set of statements—general ones, about how things work—are in logical tension with another set of statements—specific ones, about particular events. Science cannot give us any confidence about which, if any, of these statements are likely to be correct. Science cannot do this, so long as it shuns inductive inference.

Piles in a Swamp

When observation and theory clash, how does Popper think scientists are supposed to decide whether to discard theory (on the grounds that the observations in tension with it are to be trusted) or observation (on the grounds that it has been generated through dubious experiment)? Popper's stance on the status of observation statements is striking:

> Science does not rest upon solid bedrock. The bold structure of its theories rises, as it were, above a swamp. It is like a building erected on piles. The piles are driven down from above into the swamp, but not down to any natural or given "base": and if we stop driving the piles deeper, it is not because we have reached firm ground. We simply stop when we are satisfied that the piles are firm enough to carry the structure, at least for the time being.[23]

The thought that science "does not rest upon solid bedrock" might be comforting to those humble scientists who rightly stress the fallibility of their work. Only a fool would claim cast-iron certainty for a piece of experimental data. But Popper's piles give him discomfort. Sink piles into a swamp, and they have something to grip on. It is possible to build there. But what weight can observation carry, once induction has been rejected?

Popper thinks that we can use a certain class of observation statements—namely, the ones we "decide to accept"—as the basis for the falsification of theories. These are the statements the scientific community views as uncontroversial. Popper calls them "basic statements." But one hopes that science is built on more than mere group agreement. It is important that scientists' judgments about acceptable observation statements are shared because those judgments are also reasonable, or reliable. On the matter of the reliability of observation, Popper has nothing to say:

> The basic statements at which we stop, which we decide to accept as satisfactory, and as sufficiently tested, have admittedly the character of *dogmas,* but only in so far as we may desist from justifying them by further arguments (or by further tests). But this kind of dogmatism is innocuous since, should the need arise, these statements can easily be tested further. I admit that this too makes the chain of deduction in principle infinite. But this kind of "*infinite regress*" is also innocuous since in our theory there is no question of trying to prove any statements by means of it.[24]

Popper tells us that, in practice, scientists can decide whether it is theory or observation that is at fault, because the community simply accepts, by common convention, that a certain class of observation statements will be viewed as unproblematic. If a theory disagrees with these statements, then so much the worse for that theory. But group endorsement might arise from all sorts of pathological sources. What answer can Popper give to the skeptic who says that the data-points science aims to systematize are merely the product of collective fantasy or collective conspiracy?

The strict deductivist cannot justify the decision to regard these data as "satisfactory, as sufficiently tested," by appeal to their track record, because the thought that these claims have held up so well that they are likely to be true is a piece of inductive inference. The deductivist can, of course, point to the possibility of evaluating these statements, by subjecting them to further test. Hence they are not pure dogma. But these tests, too, involve seeing how our supposed observations tally with other forms of equally conjectural data.

And so we ask our question again: What makes any of these conjectures anything more than collective confabulation? Popper thinks the regress innocuous because proof is not the aim of science. This gives the impression that we can settle for something short of proof: reasonable grounds, or a decent justification for our observation claims. But on Popper's view we have no reason for thinking that observation statements are reliable, or trustworthy. Once we deny ourselves induction, we lose any chance that our theories might grip onto reality. Popper's scientific edifice is not a building erected on piles in a swamp; it is a castle in the air.

Popper and Popularity

When we think of the gilded Knights of the British Empire and Fellows of the Royal Society who have queued up to endorse Popper's image of science, it will perhaps be a surprise to learn that, on Popper's view, we have no reason whatsoever to think that our best scientific theories are true, close to the truth, or even likely to be close to the truth. These worries about Popper's system are not new: several generations of undergraduate students have trotted out similar lines of attack. Why, then, does Popper continue to be held in such high esteem by so many scientists?

Part of the reason, of course, is reciprocity: Popper himself had unwavering respect for the work of the sciences, and scientists feel they should return the favor. I also suspect that when scientists read Popper, they come away with a watered-down, more palatable form of Popperianism, one that overlooks Popper's strict skepticism of inductive inference. Popper says science does not deal in certainty. He is right about this. Scientists are keen to stress that their theories are never held dogmatically, that they are always open to challenge, that even long-held theories might fall prey to uncomfortable facts, and that scientific data, just as much as scientific theories, are hard to attain, and potentially revisable. But note how far this sensible form of fallibilism—"we might have got it wrong"—is from Popper's anti-inductivism—"there is no reason to think we have got it right." It is the difference between acknowledging that Usain Bolt might stumble and lose and arguing that there is no reason to think Bolt will go faster than anyone else who happens to be running.

Popper also stresses that, in designing experiments, scientists are not simply looking to collect facts that their theories can account for. Again, he is right about this. Scientists praise Popper for understanding that they are trying to ask probing questions of nature. An experiment should be designed so that if its results go one way the theory it tests will be in trouble, whereas if they go the other way the theory receives an evidential boost. Scientists take Popper's insistence on falsifiability to be a means of stressing the importance of demanding tests. But I suspect few scientists would agree with Popper that even when many of these tests have been passed, we have no reason for placing any confidence in the theory; and I suspect even fewer would accept his view that the standing of both theory and evidence is ultimately a matter of collective convention. Popper's philosophy of science is not the mild view that science is a fallible enterprise, which seeks demanding tests for its theories.

Demarcation Revisited

It is possible to isolate an eviscerated and attractive Popperianism that does away with Popper's own strict rejection of induction, stressing instead the important themes of testability and fallibility. What are the prospects for using this sort of mild falsificationism for the purposes of demarcation? Is a genuinely scientific theory one that is testable?

For a theory to be testable, it needs to make predictions. No theory—not even an intuitively "scientific" one that we think should fall on the good side of the demarcation line—makes predictions all by itself. Newton's laws of motion, taken on their own, do not tell us where we will observe objects. Darwin's principle of natural selection does not tell us all by itself what sorts of organisms will exist. Instead, these theories make predictions only when they are supplemented with a whole catalog of additional assumptions.

If one adds to Newton's laws a rich set of claims about where objects are located, how massive they are, and so forth, then we can use those laws to make predictions about these objects' later locations. If one adds to Darwin's principle of natural selection an even richer set of claims about genetic mutation rates, developmental processes, typical interactions between species members, and so forth, then that principle, too, can tell us something about how a species will change over time. So we cannot fault intelligent-design theory, or astrology, on the grounds that they make no concrete predictions, for no theory makes predictions when considered in isolation.

Moreover, just like Newton's laws or Darwin's principle of natural selection, these theories can be supplemented with additional assumptions so that they do make specific predictions: in other words, astrology and intelligent-design theory can become falsifiable. There is nothing to stop an astrologer foretelling in rather specific terms that Cancerians like me will have a nasty accident next Tuesday; there is nothing to stop an intelligent-design theorist from predicting that, since God is wise and beneficent, human anatomy in general will turn out to be well designed. But what will the astrologer say if everything seems to go fine for me next Tuesday? What will the intelligent-design theorist say if an anatomist points out the apparently perverse layout of the male urinary system, which requires the urethra to pass inside the prostate gland, causing misery for men when the prostate becomes enlarged and the urethra becomes constricted? If we want to use a Popperian criterion to determine the scientific status of theories, we need to focus on how the theorists responsible for them handle failed predictions. Unfortunately, there doesn't seem to be any clear recipe that will tell us what sort of response is "scientific," and what sort of response is "unscientific."

We do not want to say that a theory is scientific only if the theorists who put it forward are prepared to reject it the moment its predictions appear to be contradicted by experiment. It is perfectly reasonable for a theorist to dig in and say that, while the experiment might seem to be bad news for the theory, she believes fault to lie with the experimental setup itself. That is exactly how the scientific elite responded to the apparent demonstration of faster-than-light neutrinos at Gran Sasso. But if particle physicists are allowed to evade refutation by suggesting that the blame for a failed prediction does not lie with their theories, but lies instead with other factors external to those theories, then what is to stop the astrologist, or the intelligent-design theorist, from pointing the finger at something other than the view that our lives are influenced by the stars, or something other than the view that organic traits are the products of conscious design, when I fail to have an accident on a Tuesday, or when my prostate swells to constrict my urethra? Cannot they, too, offload the blame for failed prediction on an error of calculation, or a hidden assumption, or a misunderstanding of the theory itself? What, precisely, is the difference between an intelligent-design theorist telling us that we cannot fathom God's peculiar intentions for my urinary anatomy and a physicist insisting that the apparatus at Gran Sasso must have been malfunctioning in some as-yet-undetermined way? Don't all of these theorists use similar tactics to preserve their theories from refutation?

The obvious response to all of this is to say that the difference between the scientific and the nonscientific attitudes is a matter of how shameless one is when it comes to persistently delaying the rejection of a theory, in favor of rejigging one's ancillary assumptions. A view of this broad variety—greatly elaborated and backed up by historical examples—was defended by Popper's admirer and LSE colleague Imre Lakatos.

Newton's laws were used to predict the orbit of Uranus. Uranus was instead found to take a course different from the predicted one. Astronomers refused to reject the Newtonian framework, suggesting instead that perhaps an unknown planet was pulling Uranus off course. Such a move would seem desperate—a blatant case of evading the tribunal of experiment—except that the new planet Neptune was subsequently discovered in just the position required to disturb Uranus's orbit. And when our particle physicists suggested that something might be awry in the Gran Sasso experiment, their bet also paid off in the end: it was subsequently confirmed that a fast-running clock and a faulty connection had combined to produce a mistaken calculation for the journey time of the neutrinos.[25]

The examples of Neptune and Gran Sasso are vindications of a refusal to relinquish a good theory in the face of problematic evidence. But note how difficult it is to turn these anecdotes into a hard-and-fast set of rules regarding scientific status. Is a scientist being suitably tenacious in the face of experimental adversity, developing a masterful theory whose confirming data is just around the corner? Or is he just being pig-headed in response to a manifest lack of evidence in favor of his views?

Looking back, it is tempting to credit Darwin, for example, with a kind of prescient knowledge of the merits of his theory. His claim that the diverse species of plants and animals are all descended by gradual steps from a small number of common ancestors has the implication that some time in the past there must have been species whose anatomy and physiology fill in the gaps between the distinct forms we see today. Darwin was not able to point to such intermediate forms. He argued that his inability to produce them did not constitute a problem for his theory, but was instead a symptom of the rarity with which fossils are preserved.[26] We can give Darwin credit *in retrospect,* because in the intervening years we have discovered many "missing links," each of which adds further support to Darwin's view of common descent. But how are we to apply this sort of criterion *prospectively,* if what we want to do is sort the scientific wheat from the pseudoscientific chaff right now?

"The Inquiring Mindset"

Popper is of little help if we want a practical, prospective criterion of demarcation. In spite of everything that we read about the importance of the "scientific method," it remains unclear what that method is. The basic mathematical tools of statistical inference form a fairly constant part of the scientist's toolkit. There are also, of course, plenty of scientific *methods:* there are techniques of observation and analysis specific to individual sciences. We can use randomized controlled trials for understanding the efficacy of medicines, we can use X-ray crystallography for understanding the structure of molecules. But when we try to pinpoint a recipe for inquiry that all successful sciences have in common, we run into trouble.

Yet another Nobel laureate, Sir Harry Kroto, suggested in *The Guardian* a few years ago that we may have to settle for a loose account: "The scientific method is based on what I prefer to call the inquiring mindset."[27] The scientist approaches nature in a spirit of curiosity; she asks honest questions of nature. She proposes a hypothesis and seeks out evidence, often through a well-designed experiment, that will adjudicate on the truth of that hypothesis. But while this account does indeed help us to explain what makes science an admirable activity, it does not isolate a method that distinguishes the sciences from other branches of inquiry. Historians, too, can propose bold hypotheses, before delving into a historical archive in the spirit of honest inquiry. The same goes for other researchers in the humanities.

Kroto added to his very capacious remark on "the inquiring mindset" that this favored attitude "includes all areas of human thoughtful activity that categorically eschew 'belief,' the enemy of rationality. This mindset is a nebulous mixture of doubt, questioning, observation, experiment and, above all, curiosity, which small children possess in spades."[28] Kroto is right, of course, to stress that the sciences, as traditionally understood, do not have a monopoly on critical inquiry. But his doubts over the value of "belief" overlook the positive role of stubborn dogma. As we have seen, good scientists do not reject a theory the moment it fails to line up with experimental data. Instead, they frequently throw the blame for failure onto an unknown fault with their equipment, an unreliable observation, or a whole mistaken tradition that has led to a misunderstanding of what the apparent "evidence" amounts to. These sorts of tactics—which may look for many years like head-in-the-sand obfuscation, and which are regarded only in the light of later evidence as the foresight of genius—are often productive.

The value of blind conviction in producing valuable scientific results is one of the central themes of Paul Feyerabend's notorious book *Against Method:*

> Newton's theory of gravitation was beset, from the very beginning, by difficulties serious enough to provide material for refutation. Even quite recently and in the non-relativistic domain it could be said that there "exist numerous discrepancies between observation and theory." Bohr's atomic model was introduced, and retained, in the face of precise and unshakeable contrary evidence. The special theory of relativity was retained despite Kaufmann's unambiguous results of 1906.[29]

Feyerabend is alluding all too briskly to a series of theories—due to Isaac Newton, Niels Bohr, and Albert Einstein—which we now take to be triumphs of scientific inquiry, and which were kept alive in infancy in spite of the problems they faced. Newton, for example, was not able to explain why the solar system should be a regular system at all. Why wasn't it thrown into chaos by the mutual gravitational attractions of planets and comets? Bohr proposed that the atom itself is similar in structure to the solar system, with electrons orbiting a central nucleus. His initial model was unable to account for data concerning the behavior of hydrogen when it emits energy—particularly the so-called Pickering-Fowler ultraviolet series—that was known before Bohr's model was put forward, and which was explained by a rival theory. Walter Kaufmann's experiment of 1906, which aimed to determine whether electrons were rigid spheres or whether they could instead be deformed (as Einstein's theory seemed to entail), was widely thought at the time to have produced a result at odds with Einstein's theory of the electron.

Feyerabend's language is inflammatory, but his underlying argument is a reasonable one. In claiming that Newton's views could have been *refuted,* he implies that they could have been proven false. In claiming that the contrary evidence against Bohr was *unshakeable,* he implies that this theory, too, was known to be false at the moment it was introduced. We do not need to go this far to see that Newton's theory, and the others he mentions, were borne into hostile evidential environments. It took time, for example, for Bohr to develop a model of the atom that could account for the problematic Pickering-Fowler series. Feyerabend is surely right in saying that if scientists didn't sometimes stick resolutely to their theories in spite of abundant problems that seem—perhaps mistakenly—to undermine them, then the scientists in question would never be able to develop both mature theory and a properly interpreted body of evidence, of the sort that future generations take to be indicative of a visionary scientific achievement. The scientific mind is often open, creative, and sensitive to evidential detail. But sometimes scientists, like horses, progress best when their blinders are on.

Further Reading

On Popper's life, see his autobiography:

Karl Popper, *Unended Quest: An Intellectual Autobiography* (London: Routledge, 1992).

Popper's own writings are highly accessible, especially the following:

Karl Popper, *Conjectures and Refutations: The Growth of Scientific Knowledge* (London: Routledge, 1963).

Karl Popper, *The Logic of Scientific Discovery* (London: Routledge, 1992).

Most introductions to the philosophy of science include discussions of Popper's work. A lively (and uncharitable) critique can be found in:

David C. Stove, *Popper and After: Four Modern Irrationalists* (Oxford: Pergamon, 1982).

Meanwhile, a far more sympathetic assessment of Popper's work is provided in:

David Miller, *Critical Rationalism: A Restatement and Defence* (Chicago: Open Court, 1994).

For a sophisticated form of Popperianism that aims to bring Popper's basic views into alignment with the history of science, see:

Imre Lakatos, *The Methodology of Scientific Research Programmes* (Cambridge: Cambridge University Press, 1980).

Notes

Introduction: The Wonder of Science

1. This remark is widely credited to Feynman, but there is no decisive evidence that confirms he really made it.

2. The text of Einstein's letter is taken from D. Howard, "Einstein's Philosophy of Science," in E. N. Zalta, ed., *The Stanford Encyclopedia of Philosophy* (Summer 2010 edition), http://plato.stanford.edu/archives/sum2010/entries/einstein-philscience/.

3. This important theme has been stressed in the work of my Cambridge colleague Hasok Chang.

Chapter 1: How Science Works

1. A. Rosenberg, *Economics—Mathematical Politics or Science of Diminishing Returns?* (Chicago: University of Chicago Press, 1992).

2. W. Dembski and M. Ruse, eds., *Debating Design: From Darwin to DNA* (Cambridge: Cambridge University Press, 2004); S. Sarkar, *Doubting Darwin?: Creationist Designs on Evolution* (Oxford: Blackwell, 2007).

3. See, for example, S. Singh and E. Ernst, *Trick or Treatment: Alternative Medicine on Trial* (London: Bantam Press, 2008).

4. This was the case made in favor of homeopathic remedies at a debate I attended at the Nuffield Council on Bioethics in May 2014.

5. K. Popper, *The Logic of Scientific Discovery* (London: Routledge, 1992); K. Popper, *Unended Quest: An Intellectual Autobiography* (London: Routledge, 1992).

6. D. Gillies, "Lakatos, Popper, and Feyerabend: Some Personal Reminiscences," talk at University College London, Department of Science and Technology Studies, February 28, 2011, http://www.ucl.ac.uk/silva/sts/staff/gillies/gillies_2011_lakatos_popper_feyerabend.pdf.

7. Ibid.

8. Medawar and Bondi are quoted in B. Magee, *Popper* (London: Fontana, 1973), p. 9.

9. Gillies, "Lakatos, Popper, and Feyerabend: Some Personal Reminiscences."

10. K. Popper, "Science: Conjectures and Refutations," in *Conjectures and Refutations: The Growth of Scientific Knowledge* (London: Routledge, 1963), p. 44.

11. Ibid., p. 45.

12. Ibid.

13. This example comes from a *Daily Mail* horoscope published in August 2014: http://www.dailymail.co.uk/home/you/article-1025205/This-weeks-horoscopes-Sally-Brompton.html (accessed August 12, 2014).

14. S. Freud, *The Standard Edition of the Complete Psychological Works,* Vol. 4 (London: Hogarth Press, 1900), p. 150. For further discussion of this example from a philosophical perspective, see A. Grünbaum, "The Psychoanalytic Enterprise in Philosophical Perspective," in C. W. Savage, ed., *Scientific Theories: Minnesota Studies in Philosophy of Science,* Vol. 14 (Minneapolis: University of Minnesota Press, 1990), pp. 41–58.

15. Ibid.; italics in original.

16. For a limpid introduction to the problem of induction, see the first chapter of P. Lipton, *Inference to the Best Explanation, 2nd ed.* (London: Routledge, 2004).

17. Popper, "Science: Conjectures and Refutations," p. 56.

18. I have transcribed Feynman's comments from a video of his lecture, which is available on YouTube (http://youtu.be/EYPapE-3FRw).

19. G. Brumfiel, "Particles Break Light-Speed Limit," *Nature,* September 22, 2011, http://www.nature.com/news/2011/110922/full/news.2011.554.html#update1.

20. Rees's and Weinberg's comments are cited in J. Matson, "Faster-Than-Light Neutrinos? Physics Luminaries Voice Doubts," *Scientific American,* September 26, 2011, http://www.scientificamerican.com/article/ftl-neutrinos/.

21. F. Dyson, A. Eddington, and C. Davidson, "A Determination of the Deflection of Light by the Sun's Gravitational Field, from Observations Made at the Total Eclipse of May 29, 1919," *Philosophical Transactions of the Royal Society of London A* 220 (1920): 332.

22. H. Putnam, "The 'Corroboration' of Theories," in R. Boyd, P. Gasper, and D. Trout, eds., *The Philosophy of Science* (Cambridge, MA: MIT Press, 1991).

23. Popper, *The Logic of Scientific Discovery,* p. 94.

24. Ibid., p. 87.

25. E. Reich, "Embattled Neutrino Project Leaders Step Down", *Nature,* April 2, 2012, http://www.nature.com/news/embattled-neutrino-project-leaders-step-down-1.10371.

26. C. Darwin, *On the Origin of Species* (London: John Murray, 1859).

27. H. Kroto, "The Wrecking of British Science," *The Guardian,* May 22, 2007.

28. Ibid.

29. P. Feyerabend, *Against Method: Outline of an Anarchist Theory of Knowledge* (New York: Verso, 1975), p. 40.

Chapter 2: Is *That* Science?

1. "The Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel," *Nobelprize.org,* The Official Web Site of the Nobel Prize, n.d., http://www.nobelprize.org/nobel_prizes/economic-sciences/.

2. For an introduction to this research, see D. Kahneman, *Thinking, Fast and Slow* (London: Penguin, 2012).

3. J. Henrich et al., "'Economic Man' in Cross-Cultural Perspective: Behavioral Experiments in 15 Small-Scale Societies," *Behavioral and Brain Sciences* 28 (2005): 795–855.

4. A. Sen, *Poverty and Famines: An Essay on Entitlements and Deprivation* (Oxford: Oxford University Press, 1983).