CHAPTER 3

THE ESSENTIAL SUBJECTIVITY OF SCIENCE

The logic of scientific reasoning is by its very nature subjective.

IT TOOK THE JURY little more than an hour to find Todd Willingham guilty of setting the fire that killed his three daughters. The evidence was overwhelming. An arson specialist testified that many features of the fire could have been caused only by the laying and lighting of a trail of an accelerant, such as gasoline or charcoal lighter fluid, through the house ending at the front porch. A suspicious patch on the porch did indeed test positive for lighter fluid. Witnesses—neighbors and a fire department chaplain—testified that Willingham seemed strangely unperturbed as he watched the fire burn. And a prisoner jailed along with Willingham after his arrest testified that the accused had confessed to the crime, saying that he took "some kind of lighter fluid, squirting [it] around the walls and the floor, and set a fire." In August 1992, eight months after the conflagration, a Texas judge sentenced Willingham to death.

A decade later, as Willingham languished on death row, things were beginning to look rather less certain. A sympathetic prison visitor, Elizabeth Gilbert, had found certain discrepancies in the onlookers' testimony: their interpretation of Willingham's behavior at the time of the fire had changed significantly for the worse after they learned he was being charged with murder, a cognitive effect well known to researchers. Around the same time, the prisoner who claimed to be a recipient of Willingham's spontaneous confession filed a "Motion to Recant Testimony." And perhaps most significant of all, a greatly increased understanding of the dynamics of house fires revealed that what were considered telltale signs of arson at the time of the trial could easily be caused by entirely accidental blazes. The lighter fluid on the porch? Most likely left by a charcoal grill destroyed in the inferno.

No one will ever know whether Todd Willingham murdered his daughters. He was executed in 2004. In 1991, those who contributed to the case against him—the fire marshals, the neighbors, the chaplain, the prosecutor—took themselves to be approaching the case responsibly, sincerely, and without prejudgment. But many now believe that the state of Texas killed an innocent man.

The criminal justice system strives to uncover the truth. Even when it operates as it should, however, its interpretation of the evidence may depend on whether a witness is reliable or a theory—such as the arson investigators' assumptions about the effect of accelerants—is correct. At the moment when it matters most, there may be no objective basis for answering such questions. Information is limited, yet a determination must be made. The deliberators have no choice but to fall back on what seems most plausible to them. Much later, it may become clear that a witness was untrustworthy or that a theory was flawed. Lacking a crystal ball, the jury must do its best with what it has at the time.

It is the same in science. Sometimes it is a measurement instrument—a witness, if you will—on which the issue hinges. Sometimes, it is a theory. Scientists seeking to make sense of the evidence cannot be neutral. They must take a stand on whether the instrument is relaying the truth, on whether the theoretical assumptions hold. Having nothing further to guide them, they must go with whatever seems right. They must resort to educated guesswork, and that makes scientific reasoning irreducibly, unavoidably, essentially subjective.

CAST YOUR MIND BACK to 1919, the year of Eddington's eclipse. The rationale for the expedition to observe the eclipse was straightforward. If Einstein was right, then the light of stars close to the sun would be bent by twice as much as if Newton was right. Measure the degree of bending, then, and you will see which of the two theories is correct.

In Brazil, two members of Eddington's team focused their astrographic telescope on the eclipse and took 18 photographs. The results of those photographs are summarized in Figure 2.3, where (as you will remember) the observations are condensed into the single number in the bottom right-hand corner, showing the overall degree of bending: an almost perfectly Newtonian 0.86 arc seconds. Eddington protected his Einsteinian agenda, however, by dismissing the significance of the astrographic telescope's photographs.

In so doing, it might seem, he violated the methodological commandment that Popper made famous, the precept that a theory making false predictions must be spurned. But that is not quite correct. As we will see, Eddington broke no objective rules in his belittling of the Brazilian data. Not even the most unscrupulous scientist could have done so, because the sociologists Bruno Latour and Steve Woolgar are right: no such rules exist. Further, this is not merely a matter of sociological fact but of philosophical principle. The most concerted attempts to frame such rules, such as Popper's principle of falsification, for systematic reasons fall through, ultimately putting no objective constraint on scientists' interpretation of evidence.

To see why, take a closer look at Eddington's rationale for ignoring the data from the astrographic telescope. He argued—controversially, but not arbitrarily—that something had gone wrong with the telescope, resulting in its systematically underreporting the gravitational bending. Although he had no direct evidence for this claim, there are a number of ways such "undermeasurement" might have occurred.

You cannot simply look through a telescope and see light bending. Rather, what you must do is photograph the apparent positions of the stars next to the sun's disk at the moment of the eclipse and compare them to the same group of stars when they are nowhere near the sun. The degree of bending is revealed by the difference in positions on the photographic plates. This difference is microscopic: the measured gravitational bending of 0.86 arc seconds is equal to 0.0002 degrees, which corresponds to a shift in position of only 1/60 of a millimeter—0.0007 inches—on the Brazilian astrographic's plates. Anything that has the slightest impact on the measurements will result in a significant error in the calculation of the bending.

There were many such potential spoilers, because the setup of the apparatus in Brazil was rather complex. In Figure 3.1 you see the astrographic telescope at home in the Greenwich Observatory in England. It is attached to a heavy, precisely engineered mount that allows it to be trained on any point in the sky. Eddington left that mount behind. In Brazil the telescopes were laid flat, pointing at the horizon (Figure 3.2). For each telescope, an external mirror reflected light from the target in the sky down the prone telescope's barrel.

Eddington and his team seized

Figure 3.1. The 13-inch astrographic telescope at the Royal Greenwich Observatory, the key optical elements of which were transported to Brazil and reassembled to constitute the "Brazilian astrographic."

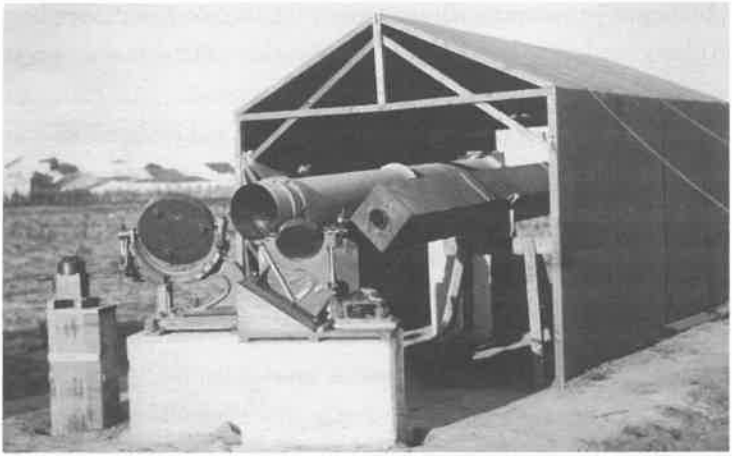

Figure 3.2. The setup of the Eddington expedition's telescopes in Brazil. The astrographic is on the left, the 4-inch on the right. The external mirrors are mounted on the block in the foreground.

on certain disadvantages of that arrangement to explain how the astrographic telescope's measurements might have gone wrong. The heat of the tropical sun beating down on the telescope's mirror before the onset of the eclipse, they conjectured, might have caused irregular expansion that distorted the photographic images. The mirror in any case had an astigmatism, though the scientists had found a way to avoid the worst consequences of this imperfection. Finally, the mechanism that kept the mirror pointing toward the sun, compensating for the earth's rotation, was operating irregularly. It would not be so difficult for these problems to introduce errors of 0.0007 inches or so in the positions of the stars, errors that would deliver Newtonian numbers from Einsteinian skies.

The "empirical fact" reported at the bottom right of Figure 2.3—the gravitational bending angle—is not, then, an observed quantity but a calculated quantity, a number whose value depends on a long chain of assumptions, some of which might easily be false. The same is true of the bending angle measured using the 4-inch telescope that lay alongside the astrographic instrument, taking in the same patterns of light but announcing a contrary verdict. Indeed, in retrospect, we know that something was systematically off kilter in the 4-inch telescope as well, since it indicated a bending angle rather larger than Einstein's theory allowed. Eddington had to make a choice. Discount the astrographic data? Overlook the 4-inch discrepancy? Declare the experiment to be inconclusive? He did not have enough information to single out an obviously correct answer. So he followed his instincts.

Eddington's situation was not at all unusual. In the interpretation of data, scientists often have great room for maneuver and all too seldom have unambiguous guidance as to which maneuvers are objectively right and wrong.

The room for maneuver exists because, as the eclipse experiment shows, theories in themselves do not make predictions about what will be observed. To say anything at all about the experimental outcome—about, say, the position of spots on a photographic plate—theories must be supported and helped along by other posits, other presumptions about the proper functioning of the experimental apparatus, the suitability of the background conditions, and more.

In other words, a theory, like a medieval knight, never fights alone, but rather rides into empirical combat with a retinue of assumptions. It is this formation as a whole—what you might call the *theoretical cohort*—that makes predictions about and gives explanations of the outcomes of experiments, measurements, and other observations. The theory gets all the attention. But it cannot engage the enemy without its coterie of men-at-arms.

Consequently, when something goes wrong, a theory can be saved from refutation by blaming the assumptions—as Eddington did when he used his considerable logical, social, and political skills to have the Brazilian astrographic measurement, the patently un-Einsteinian 0.86

arc seconds, dismissed on the grounds that something had gone wrong with the apparatus. Faced with a faulty prediction, a scientist must decide when to sacrifice an assumption to save a theory and when to accept that the theory itself has failed.

Karl Popper took this problem very seriously. He had no choice, as it seemed to undermine his central idea, that science progresses by eliminating theories that make false predictions. If a theory can be excused whenever something looks wrong by blaming an assumption—by postulating some error in the measurement apparatus, for example—then how can theories ever definitively be let go?

Popper allowed that blaming an assumption to save a theory is sometimes the right thing to do, but only under certain conditions. He required that the new assumptions made in the course of such a defense should themselves be falsifiable and that their proponents ought, in the critical Popperian spirit, to make every effort to test them. This recommendation scientists are often happy to follow. In late 2011, neutrinos created at the CERN research facility in Switzerland were clocked traveling faster than the speed of light—an athletic feat forbidden by Einstein's theory of relativity. Rather than discard relativity, the great majority of physicists supposed instead that something had gone wrong with the measurement apparatus. The matter did not rest there, however; having saved relativity from falsification, they followed Popper's advice and set to work testing the supposition on which the rescue depended: that "something had gone wrong." An exhaustive overhaul of the experimental machinery vindicated their conservatism. It turned out that a cable was loose.

Such care and attention, however, is not always feasible. Back in England, writing up his results months after the eclipse, there was no way that Eddington could double-check the effect of, for example, the mirror's expansion in the sun's heat that day. The same is true for many suspected experimental malfunctions: the supposed aberration is often temporary, and there is no way in retrospect to determine whether it happened or, if it did, to what degree. The facts of the matter are lost to history.

Popper suggested a different way to deal with these cases: do the experiment a second time, more carefully. Again, scientists are readily observed following this advice—but again, it is not always feasible. Solar eclipses are rare enough; what made Eddington's 1919 eclipse rarer still was the sun's position, at the time of totality, in the center of a field of relatively bright stars. As Eddington pointed out when touting the experiment, this happy alignment "would not occur again for many years." He might have wanted to go back for another round of stellar photography, but he could not—so he found other ways to press his case against the Brazilian astrographic and in favor of Einstein.

Eddington's course of action was unconstrained not because he disdained the rules of scientific thought but because the complexities and difficulties of empirical investigation—of making precise measurements of small or barely accessible quantities—meant that he had no rule capable of telling him how to interpret his photographic plates. Even the tenacious attempts of that great methodist Karl Popper to lay down principles for deciding, in the face of a faulty prediction, whether to blame the theory or merely a measurement instrument were of no help. Sometimes a scientist striving to interpret the significance of empirical data, like a jury member faced with questionable testimony, simply has to make a judgment call—personal, instinctive, subjective.

Eddington's and Pasteur's self-regarding maneuvers, Latour's ethnographical investigations—these were bad enough for methodism, showing that scientists both famous and obscure fail to follow an objective guide when assessing the impact of their evidence. Now the situation appears positively dire: in many cases, there are no such guides. Not even if science were flawless, populated by paragons of temperance, rationality, and selflessness, could it assess evidential weight objectively.

Or at least that is true when the significance of the evidence depends on the contested credibility of a measuring instrument. It is also true, just as in the courtroom, when what the evidence says depends on the plausibility of a controversial theoretical assumption—as we'll now see.

GEOLOGISTS BEGAN TO fathom in the early 1800s that the earth is extraordinarily ancient. Charles Darwin's theory of evolution by natural selection, proposed in 1859, needs as many millennia as it can get its hands on in order to provide time enough for all the diversity and complexity of life to emerge from a common ancestor—for flecks of floating protoplasm to sprout branches and leaves, heads and legs, and to take over the planet's surface. Darwin therefore seized on the new geology to argue that the earth must be at the very least hundreds of millions of years old.

This staggering idea ran full tilt into a formidable obstacle. The name of the obstacle was William Thomson, one of the most famous and influential physicists of the time. Thomson was a prodigy: born in Belfast in 1824, he published three scientific papers while still in school and at the age of 22 was appointed a professor at the University of Glasgow, where he remained his entire life. He made important discoveries in the new sciences of energy and heat and pioneered the notion of the heat death of the universe—the inevitable dispersion of energy that would result in the world's becoming a quiet, dark, homogeneous, and lifeless place in which everything was at the same temperature and nothing more could happen. Turning to engineering and commerce, he joined the effort to lay an undersea telegraph cable between Britain and the United States; after years of accidents and false starts, the connection was made in 1866 and Thomson was knighted for his contributions. He then headed the opposition to Irish home rule in the Liberal Party, a service for which he was ennobled in 1892, becoming Lord Kelvin—the name by which he is usually identified today.

Kelvin was throughout his life conventionally religious. He was a latitudinarian, meaning that he considered distinctions between denominations—in particular, between Anglicanism and Presbyterianism—to be unimportant; indeed, as a source of revelation, he preferred nature to the pulpit. Look around and you will see at work "a creating and directing power," he wrote, and so "if you think strongly enough you will be forced by science to the belief in God, which is the foundation of all religion." But the science of Darwinism, as he understood it, threatened to broadcast exactly the contrary message.

Kelvin had long hoped to calculate the age of the earth using the

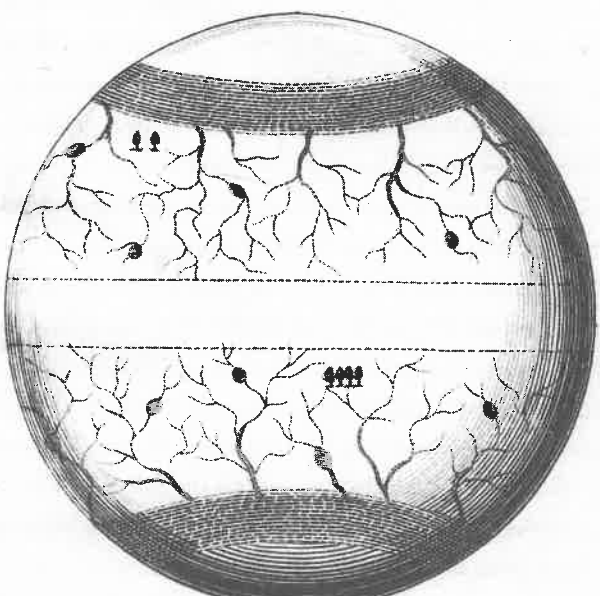

Figure 3.3. The earth at the dawn of creation, according to Thomas Burnet's *Sacred Theory of the Earth*, published in 1681. Rivers flow from the poles to the equator. A speculative location for the Garden of Eden is marked by a line of four trees in the southern hemisphere.

physics of heat. His idea was straightforward. The colder a cup of coffee, the longer it must have been sitting out on the counter since it was poured. Likewise, the colder the earth's crust, the longer the planet must have been cooling since its formation. The earth's age could be estimated, then, if both its original temperature and the current temperature of its outer layer were known. Kelvin presumed the original temperature to be that of molten rock, but for the current temperature he had to wait some years, until the Scottish physicist and glaciologist J. D. Forbes made a series of measurements of the temperature of the subsurface rock around Edinburgh.

With these numbers in hand, Kelvin published his calculations in 1863 using a well established theory of cooling to show—by the time he gave his final estimate in 1897—that the earth could be no more than 20 to 40 million years old and "probably much nearer 20 than 40." Its crust was too warm for the planet to have been cooling any longer. Further, other late nineteenth-century estimates of the earth's age based on the probable age of the sun, some made by Kelvin himself, also came in as low as 20 million years. It seemed that physics would give evolution no time to create life's variety. Darwin stood refuted.

To the rescue came Darwin's most tenacious defender, his "bulldog," the anatomist Thomas Huxley. In 1860, Huxley had famously seen off Darwin's critic Bishop Samuel "Soapy Sam" Wilberforce, proclaiming in the face of Wilberforce's trite mockery that he would rather be descended from apes than from a man who scoffed at serious debate. Huxley then fought a running battle with the paleontologist Richard Owen, who argued that the resemblances between the brains of apes and humans were merely superficial and therefore no evidence for their descent from a common ancestor. Now the bulldog's tactical skills were called upon once again.

Huxley did not, in truth, know a great deal about the physics of heat. But he knew how to win an argument, and so armed, he went to work on the plausibility of Kelvin's assumptions. Kelvin's mathematics was impeccable, Huxley acknowledged, but accurate calculation was not enough:

> What you get out depends on what you put in; and as the grandest mill in the world will not extract wheat-flour from peascods [pea pods], so pages of formulae will not get a definite result out of loose data.

Further, Huxley opined, Kelvin was a mere "passer-by" who did not understand the deep foundations of the geology and biology on which Darwin's theory was built. But geology is a branch of physics, Kelvin replied, and so as a physicist he was, far from being a passer-by, an expert whose opinion about its foundations ought to be taken very seriously indeed.

Kelvin's response was disappointingly polite, thought his friend and colleague Peter Guthrie Tait, who charged into the debate implying that natural historians such as Darwin and Huxley were "beetle-hunters" and "crab-catchers" incapable of recognizing the power of mathematical thought. He concluded with an estimate of the earth's age that undercut even Kelvin's: "Natural Philosophy [that is, physics] already points to a period of ten or fifteen millions of years as all that can be allowed for the purposes of the geologist and paleontologist; and . . . it is not unlikely that with better experimental data, this period may be still farther reduced."

The earth is actually over 4.5 billion years old, and it has harbored life for at least 3.5 billion of those years. How did Kelvin get it so badly wrong? Like Todd Willingham's jurors, who were presented with an inadequate theory of the way in which house fires develop and burn, he was relying on assumptions that were mistaken in several respects. First, though he had no way of knowing it, the heat of the rock making up the continents is considerably increased by the decay of radioactive elements. Second, heat is transported from the earth's core not by conduction through solid rock, as Kelvin had supposed, but by convection, in which rock in the earth's mantle flows from the core to the crust carrying heat with it, warming the crust far more efficiently than conduction could. As a consequence, an old earth can have, as ours does, a surprisingly warm crust.

In effect, Kelvin sipped the top layer of a cup of coffee and, finding it to be piping hot, concluded that it had been freshly poured. In fact it had been there for ages, but sitting on a warmer and continually stirred. Huxley was right. Kelvin had poured chaff into his finely calibrated mathematical mill and produced indigestible grit.

Were Kelvin, Tait, and other advocates of a "youngish earth" being unscientific? Not at all; there were good reasons and great uncertainty on both sides. The physicists saw their assumptions about the geological structure of the earth as natural and reasonable compared with the largely speculative theory of evolution; if something had to go, it was biological guesswork, not careful physical extrapolation. The biologists saw their grand explanation of life's intricacy, though hardly proven, as a breathtaking achievement based on extensive observation of nature across the globe, a breakthrough that could scarcely be discarded on the grounds of pure conjecture about the unseen goings-on thousands of miles beneath their feet. Such contrary attitudes are business as usual in science, an unexceptional manifestation of the subjectivity that swirls through all scientific reasoning, planning, and debate.

Karl Popper sought, by laying down rules of scientific method, to resolve disputes such as this, to decide whether observations of the earth's surface temperature falsified Darwin's theory or whether somewhere in Kelvin's thinking "something had gone wrong." But Popper's precepts were no more useful in the age-of-the-earth controversy than they were in the Eddington affair. When making theoretical assumptions, he said, be bold. Choose hypotheses that, by making strong claims, expose themselves forthrightly to falsification. Kelvin and Darwin certainly did that. But they had no way, in their lifetimes, of testing their claims.

Popper might perhaps have counseled both physicists and biologists to keep an open mind, to refrain from taking sides, until more was known and a definitive falsification of either biological theories or physical hypotheses was achieved. Such a prescription is hardly realistic: it is precisely the sort of stricture that scientists, being also humans, will consistently fail to follow. And in any case it is, as we saw in the case of Wegener and continental drift, bad advice. Science is driven onward by arguments between people who have made up their minds and want to convert or at least to confute their rivals. Opinion that runs hot-blooded ahead of established fact is the life force of scientific inquiry.

For these reasons, Popper is now thought by most philosophers of science to fall short of providing a rule for bringing evidence to bear on theories that is both fully objective and adequate to science's needs. What kind of rule might do better? There is philosophical consensus on this matter, too—and the answer is *none*. An objective rule for weighing scientific evidence is logically impossible.

The impossibility arises from the same fact that makes Popperian falsification often such a contentious matter: a scientific theory issues predictions only when it is combined with various assumptions to compose a theoretical cohort. The members of the cohort—what philosophers call "auxiliary assumptions"—are a diverse range of suppositions. Some are high-level theories themselves, such as Kelvin's assumption about the solid structure of the earth's interior. Some are assumptions about the functioning and calibration of measuring instruments, such as Eddington's assumptions, positive and negative, about his various telescopes. The auxiliary assumptions are like links in a chain leading from the theory to the evidence. The chain is only as strong as its weakest link; thus, to assess the strength of the chain—to assess the strength of a

Figure 3.4. To evaluate the overall strength of a scientific argument, you must evaluate the strength of each of the argument's pieces.

piece of evidence for or against a hypothesis—you must have an opinion about the strength of each of the links.

It is, in other words, impossible to judge the impact of a piece of evidence on a theory without having a view about the auxiliary assumptions. If you think that the Brazilian astrographic telescope was working perfectly, you will count its measurements of the bending angle of light as powerful evidence against Einstein's theory of relativity. If you find it quite plausible that something went systematically wrong in those measurements—if you suspect that this particular link in the evidential chain is faulty—then you won't regard the evidence as at all strong; you will, like Eddington, be inclined to overrule it on the basis of evidence obtained from other instruments in which you place greater trust.

Likewise, if you think that Kelvin's assumption about the solidity of the earth's insides is on firm ground, you will (provided you have some faith in his other assumptions) interpret nineteenth-century measurements of the temperature of the earth's crust as strong evidence for a youngish earth, and consequently as doing great damage to Darwin's theory of evolution. If, by contrast, you think of the solidity assumption as an exceptionally risky conjecture, supposing as it does that the rigid structure found in the top few miles of the earth's surface must continue down unchanged for another 4,000 miles to the planet's core, then you will consider the temperature measurements to be only piffling evidence against Darwin's ideas.

A rule that strives to lay down the law about the significance of scientific evidence, then, must also lay down the law about the likelihood of all relevant auxiliary assumptions, in the same way that a procedure for determining chain strength must estimate the strength of every link. The rule's judgments can be objectively valid only if its estimates of the auxiliary's likelihoods are objectively valid. An objective rule for weighing any and every piece of evidence is therefore possible only if there is an objective fact of the matter about the likelihood of each relevant auxiliary assumption, given the available evidence.

As we have seen, however, opinions about auxiliary assumptions can differ wildly—not because scientists ignore the rules of right reasoning, but because there are simply not enough known facts to nail down likelihoods for every auxiliary assumption in a theoretical cohort.

On the one hand, as the Eddington affair illustrates, assumptions about experimental conditions and the transient state of the measurement apparatus are often impossible in retrospect to check, and experiments and observations are often too difficult or too expensive to repeat—in the short term, at least.

On the other hand, to form an opinion about a theoretical auxiliary assumption, such as Kelvin's assumption that the earth is entirely solid, requires further evidence, and the significance of this evidence for the auxiliary assumption will itself depend on further auxiliary assumptions. Among these assumptions may appear the original hypothesis, forming an unbreakable circle.

When Louis Pasteur, for example, ventured to show in the 1860s that life could not form spontaneously from an inorganic mix of "hay soup" and air, he needed a supply of air that was sterile, that is, free of the "spores" that he hypothesized to be the source of all mold, slime, and other growth in the soup. As you may remember, he and other experimenters tried various procedures to obtain spore-free air: heating the air, storing it in a greasy container, sampling it from alpine peaks. That such air is indeed sterile is a classic auxiliary assumption, essential for the validity of the experiment. But how to ensure that it holds true? The only way that Pasteur knew of testing his auxiliary was to mix the air with the soup to see whether life developed; if it did, the air contained spores, and if not, not. But such a test of course assumes the very theory that Pasteur was trying to prove, that life could not emerge spontaneously from sterile air and soup. The experimenters on both sides of the spontaneous generation debate at the time had no way of independently verifying their most important auxiliary assumption.

There is no way to get started, in such a situation, without assigning some likelihoods from scratch—not arbitrarily, exactly, but without the constraints imposed by a preexisting scheme for interpreting evidence. Needless to say, different scientists will choose different starting places, heavily influenced by personal tastes or aspirations. From that point on their estimates of evidential weight are liable to head in disparate directions. That is the origin of the essential subjectivity of science.

Subjectivity need not mean anarchy. There are rules for interpreting evidence, but they are rules that allocate a role to subjectively formed estimates of a hypothesis's likelihood, or, as I will call them, *plausibility rankings.*

As an example, consider a precept that has already had a vigorous workout: a piece of evidence should count for less the more likely it is that the apparatus that produced it malfunctioned. Both Eddington and his critic, the American astronomer W. W. Campbell, followed this rule in interpreting the Brazilian astrographic telescope's photographic plates, each applying their personal plausibility ranking. Eddington thought it very likely that something went wrong with the telescope during the eclipse and so put very little weight on the plates; Campbell thought it somewhat less likely, and so gave the plates more weight. Each used their personal plausibility rankings as proxies for the likelihood of a malfunction; consequently, though they followed exactly the same rule, it instructed one of them to treat the evidence differently from the other.

So it goes with all scientific reasoning: the interpretation of evidence demands likelihoods, and scientists are not only permitted but encouraged to use their subjective plausibility rankings in that role.

When these rankings agree, scientists agree on the treatment of the evidence. In 2016, a marten—a small member of the weasel family—gnawed through a power cable at the Large Hadron Collider at CERN, in Switzerland, destroying itself and seriously damaging the collider's power supply. There was no discrepancy of plausibility rankings in this case: the many scientists working at the facility contemplated the small, smoking corpse and concurred that "something had gone wrong." The collider would need major repairs and subsequent batteries of tests before its data could be trusted. But far more often, plausibility rankings and so the interpretation of data diverge; the subjectivity of the rankings flows directly into scientific reasoning itself. The heart of scientific logic is a human heart.

Kelvin's allegiance to physics over biology and his religiously motivated skepticism about evolution; Eddington's hopes for the new theory of relativity and for international reconciliation after the Great War: did these inclinations derail Kelvin's and Eddington's reasoning, throwing it off the narrow track laid down by objective scientific logic and dragging it into the swamp of human passion and ambition? No; there is no logical track, no one true way, no answer key that science can use to "self-correct" its course. There is only the swamp. Each scientist finds a way through the swamp as best they can. They follow rules, but the rules tolerate and indeed depend on their users' subjective judgments.

Even the darkest and most disturbing demonstrations of scientists' human frailty take on a new hue when plausibility rankings are understood as an essential part of, rather than a corrupter of, scientific reasoning. Scientists sponsored by soda or tobacco companies, I noted earlier, tend to produce results more commercially favorable to those products than scientists with independent funding. Why? The central role of plausibility rankings does allow for cold-blooded calculation: where any of a wide range of rankings could be assigned, a miscreant might intentionally select those that will maximize fame, opportunity, filthy lucre. But although humans are quite capable of such deeds, they are also eminently warm-blooded organisms, whose enthusiasms, hopes, and fears mold their thinking far below the threshold of awareness. Just as referees favor the home team, so scientists' plausibility rankings most likely unconsciously favor their benefactors' businesses. If these were failures to conform to the logically prescribed code of scientific thought, they could be uncovered and corrected by a thorough audit. Examine the rulebook at the points where preferences and prejudice flow into scientists' reasoning, however, and it turns out to say, "Here, apply your plausibility rankings." That is precisely what the scientists in question have done. There is no misdeed; they have acted just as their logic advised.

Science surely does have its malefactors. The rules are sometimes flouted, sometimes deliberately gamed, not least by the leading lights—as on occasion by Newton, Pasteur, Mendel, Haeckel, Millikan, and perhaps Eddington. But even if the advocates of science's powers of self-correction, such as Karl Popper and Atul Gawande, are right to think that such wrongdoing is sporadic or manageable or for some other reason does only limited damage to the scientific enterprise, they cannot appeal to an objective logic to explain science's success. There is no such logic; the evaluation of hypotheses in the light of evidence is thoroughly subjective, fluid all the way down to its core.

THE ESSENTIAL SUBJECTIVITY of the interpretation of evidence is, I have said, not a wholly regrettable thing: by allowing raw, unchecked opinion to animate the process of reasoning, it gives scientific inquiry a vitality and a positive momentum, spurring fruitful argument and competition—Huxley versus Kelvin, Pasteur versus Pouchet—that a more judicious, consensus-making rule would find hard to match.

Yet all the same, by giving up on an objective logic of scientific reasoning, we seem to be abandoning the Great Method Debate, and in so doing losing our grip on the question of what makes science special, of what changed for the better in the course of the Scientific Revolution. It wasn't in the seventeenth century, after all, that the human race first became opinionated or developed a taste for partisan argument.

Objectivity is vital to a methodist such as Popper or Kuhn, you will recall, because it makes possible a systematic, unflagging, uncompromising search through the scientific possibilities, discarding those that exhibit even the slightest weakness. For Popper, what is most important is objectivity's discriminating power: the Popperian rule of falsification purports to detect any discrepancy between theory and the observed facts. For Kuhn, what is most important is objectivity's motivating power. A single set of standards for doing and judging almost everything—the prevailing paradigm—gives scientists the fervent devotion to a research program needed to push it to its empirical breaking point. Neither vision of what distinguishes science from prescientific thinking is viable unless the subjectivity, the individual differences, are sucked out of scientific thought.

Not only through the veins and arteries of all-too-human scientists, however, but also through the hard, rectilinear channels of logic itself, subjectivity surges, giving scientific reasoning its life. By the end of the previous chapter, the notion of an objective scientific method was beaten. It was begging for quarter, blindsided by the self-regard, self-absorption, and slanted perspective of even the sharpest scientific minds. Now it's on its back, discredited, defunct—apparently out of contention for all time.

And yet there is a lifeline: a fine, almost invisible thread of objectivity running through scientific practice. The thread takes the form of a precept regulating scientific argument that is compatible with all I have said about the subjectivity of science so far.

The rule I have in mind allows partialities and power politics to dominate day-to-day scientific inquiry. It allows unfashionable innovators to be ignored and theories with strong social connections to be kept on the books even when their performance is mediocre. But while it tolerates human frailty and condones, indeed draws strength from, the essential subjectivity of scientific reasoning, it brings a subtle pressure to bear. That pressure operates in the long term to do exactly what the methodists have hoped their "scientific method" would do: harvest the facts and distill the scientific truth.

This is the unobtrusive yet irresistible principle that I call the iron rule of explanation. It is time to see it in action.