CHAPTER 2

HUMAN FRAILTY

Scientists are too contentious and too morally and intellectually fragile to follow any method consistently.

As the moon's disk crept across the face of the sun on May 29, 1919, a new science of gravity hung in the balance. Just a few years earlier, Albert Einstein had formulated his theory of general relativity, a conceptually radical replacement for the gravitational theory that made Isaac Newton famous at the beginning of modern science, more than two hundred years before. Whereas Newton held that massive bodies exert upon each other a "force of gravity," Einstein said that they rather bend the space and time around them, giving it a characteristic curvature. When objects do their best to trace straight lines through this twisted medium, they move in a way that suggests the existence of gravitational force—but there is in fact no such thing. Profoundly different though these two pictures may be, they make nearly identical predictions about the movements of particles, planets, and everything in between. Nearly identical, but not quite. The difference between Newton and Einstein, the fact as to whose ideas were correct, could perhaps be faintly discerned on the margins of a total eclipse of the sun.

Two months earlier, the steamer *Anselm* had left Liverpool with three telescopes and two teams of scientists on board. One group was headed to Brazil; the other to the island of Príncipe, off the coast of West Africa. At their assigned destinations, they would each photograph the sky at the moment that the light of the sun was fully obscured by the moon. The pattern of stars surrounding the eclipse would reveal the extent to which light passing close to the sun is dragged off course by our home star's intense gravitational field. In the same way that a partially submerged oar appears to bend at the point where it enters the water, due to the bending of light rays at the air/water boundary, so the stars would appear to be displaced from their usual positions to a degree corresponding to the bending force of the sun's gravity. Einstein's new theory predicted that incoming light rays would be deflected by twice the amount that Newton's old theory implied.

It was a crucial experiment in the Popperian mold. Measure the apparent shift in the stars' positions, and in the cold light of that number at most one theory could survive—either Einstein's or Newton's—or, if both predictions turned out to be wrong, neither.

Six months after the eclipse, the expedition leader Arthur Eddington announced the results: Newton was dethroned and Einstein was declared the new emperor of gravitation. The Great War was finally over, and Einstein's esoteric German physics had been confirmed by Eddington's exacting British experiment, a scientific triumph that was heard around the world—by a young Karl Popper among others—and that heralded an era of international cooperation, progress, and peace.

But the peace didn't last, nor did the story; nothing about it is quite right. Eddington awoke on the morning of the eclipse to cloudy skies over Príncipe; he was able to obtain only blurry, indistinct photographs of the surrounding stars. The stellar snapshots from Brazil were much superior, but they posed a different problem. The Brazilian team had brought with them two telescopes, and the measurements made with those telescopes said two different things. One instrument, the "4-inch"

Figure 2.1. Another cloudy day on Príncipe.

telescope, showed a shift in the positions of the stars roughly in accordance with Einstein's prediction. But the other, the "astrographic" telescope (especially designed for photographing stars), showed a shift that was almost exactly Newtonian.

How did Eddington and his collaborators reach the conclusion, then, that it was Einstein's predictions that came true?

They had three sets of data at their fingertips. First, there were 2 photographs from Príncipe that dimly depicted stars through the clouds, and which according to some rather complex calculations by Eddington showed a shift of Einsteinian magnitude. Second, there were 7 photographs from the Brazilian 4-inch telescope that also showed an Einsteinian shift (Figure 2.2 among them). Third, there were 18 photographs from the Brazilian astrographic telescope that showed the shift predicted by Newton's theory. Eddington's strategy was to argue that something had gone systematically wrong with this last set of photo-

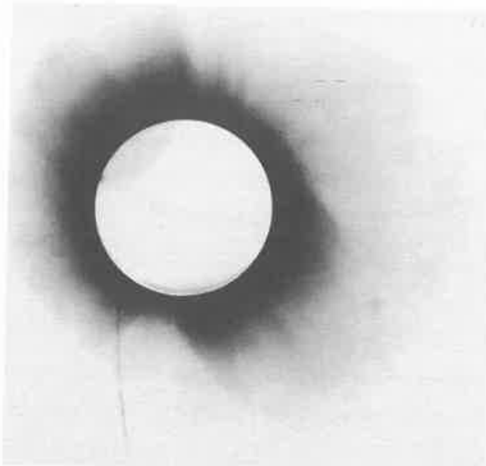

Figure 2.2. A photographic plate from Eddington's 1919 eclipse expedition. It is a negative: the eclipsed sun is the big white circle, its bright corona the dark flare surrounding the circle, and the surrounding stars are tiny black dots. Some of the crucial stars' positions are indicated by faint horizontal lines drawn on the plate.

graphs. They were, in fact, considerably blurrier than those produced by the 4-inch telescope, possibly (so he and his collaborators conjectured) because of distortions caused by the sun's uneven heating of the mirror that reflected the light from the eclipse into the telescope.

Certain of Eddington's contemporaries, however, found Eddington's argument to be rather fishy, as have many later historians of science. Eddington could explain the blurriness of the astrographic photos, but he gave no reason to think that they would systematically err so as to give Newtonian rather than Einsteinian values for light's degree of gravitational bending. Further, the crisp photos from the 4-inch telescope gave a value for gravitational bending that was considerably greater than that predicted by Einstein, to a degree that they could be considered to support Einstein's theory only if that telescope, too, was assumed to be systematically biased. Eddington appeared to be engaged in some rather special pleading, then: he assumed systematic errors in one direction for one of the Brazilian telescopes and in the other direction for the other telescope, so as to reach the conclusion that the results they delivered were quite consistent with Einstein's theory of relativity. As W. W. Campbell, an American astronomer and director of the Lick Observatory in San Jose, California, wrote about Eddington's analysis in 1923: "the logic of the situation does not seem entirely clear."

If Eddington's reasoning was as murky as his Príncipe photographs, his aim was pellucid. He wanted very much for Einstein's theory to be true, both because of its profound mathematical beauty and because of his ardent internationalist desire to dissolve the rancor that had some Britons calling for a postwar boycott of German science. (Eddington, as a Quaker, was a committed pacifist; protesting against the proposed boycott, he wrote that "the pursuit of truth . . . is a bond transcending human differences.") These high-minded goals he pursued using the considerable political power at his disposal. He had recruited the Astronomer Royal, Sir Frank Dyson—"the most influential figure in British astronomy"—to his cause early on; it was Dyson who, though he had no personal interest in relativity theory, proposed the eclipse expedition and then took the honorary position as principal author of the expedition's report, all at Eddington's behest.

When the expedition presented its results, Eddington won an endorsement from the president of the Royal Society and qualified support from the president of the Royal Astronomical Society. Other physicists were more dubious but also less influential and less institutionally powerful. Their reservations were written out of the story: in the wake of the eclipse, Eddington became the preeminent exponent of relativity theory in English, and his discussion of the eclipse experiments was regarded as the standard reference on the topic. While he details and celebrates the pro-Einsteinian measurements provided by the Brazilian 4-inch and Príncipe telescopes, the Brazilian astrographic results that favored Newton instead are perfunctorily dismissed. Those photographs were on the wrong side of history; consequently, they were entirely blotted out.

I have begun this chapter with the story of Eddington and the eclipse in part because there is nothing remarkable about it: it is a rather typical (if unusually well documented) tale of complicated, confused, or ambiguous data, a certain selectivity in the interpretation or reporting of that data, and a concerted effort after the report is made to bend the course of consensus making in a direction favorable to the reporter's intellectual, moral, or practical aspirations. This is the human mind operating according to its standard specifications, following a trajectory familiar to every student of history—a pattern of partiality and politicking found in Thucydides's description of the war between ancient Athens and Sparta, in Gibbon's *Decline and Fall of the Roman Empire*, in the intrigues of Renaissance Italy's city-states, and in backrooms and presidential palaces across the globe today.

But scientific reasoning is supposed to be an antidote to these primeval inclinations—and that is what is supposed to explain its extraordinary success. According to Karl Popper, the scientific knowledge machine is driven by an intense critical spirit and by the implacable principle of falsification. Neither is at all evident in Eddington's treatment of the eclipse. Eddington cosseted his own favored theory, shielding it from evidence that looked prima facie falsifying, while damning its rival using reasoning more redolent of the one-sided pleading of a criminal prosecutor than of the evenhanded and straightforward logic of falsification.

According to Thomas Kuhn, what distinguishes scientific from ordinary inquiry is scientists' agreement to conduct their research in the framework of the prevailing paradigm, which both sets their goals and instructs them in the interpretation of the evidence. But there is little sign of such a rigid scaffolding in the case of the eclipse. Eddington used his scientific work to realize an aim that lay outside anything that might be dictated by a Kuhnian paradigm, namely, a rapprochement between the British and the German scientific establishments. Further, he pursued this and his other aims by interpreting the data in a way that seems driven more by the desire to succeed than by some officially sanctioned, widely accepted procedure for bringing evidence to bear on theory, of the sort that a paradigm is supposed to prescribe. His subsequent political machinations and selective history writing equally seem more inspired by personal, albeit idealistic, ambition than by obeisance to a shared code of scientific conduct.

Science is so exceptionally powerful, Kuhn argued, because the supremacy of the paradigm guarantees to scientists (so they believe) that their research has a certain fixed significance, underwritten by the goals, experimental methods, and rules for evaluating evidence that constitute the paradigm's core. Eddington's logical and political manipulations, however, disclose exactly the kind of flexibility of rule and pliancy of institutional framework that would set the significance of scientific results perpetually adrift. The Kuhnian paradigm is supposed to preclude such inconstancy. It did not.

The 1919 eclipse is only a single example of the selective use of evidence. But the centuries since the Scientific Revolution are strewn with cases in which science's biggest names can be seen discarding or distorting difficult data so as to create the impression that experiment was in perfect harmony with their theoretical or other aims.

Gregor Mendel, the founder of genetics, almost certainly massaged the statistics he presented in the 1860s in support of his thesis that genes lie at the root of biological inheritance. Ernst Haeckel embellished his careful drawings of animal embryos around the same time to support his thesis that "ontogeny recapitulates phylogeny"—that a human embryo, for example, passes through stages in which it takes on forms more or less identical to those of fish embryos, then amphibian embryos, then bird embryos. Robert Millikan, in pulling together the data from which he inferred the electric charge of a single electron—work that earned him the 1923 Nobel Prize in Physics—omitted many measurements that did not "look right," while claiming to have included everything. Even Isaac Newton manipulated certain empirical quantities to better fit his theories, tactics that in one case amounted, wrote his biographer Richard Westfall, to "nothing short of deliberate fraud."

There is one respect, I must note, in which Eddington and other modern scientists are almost exceptionlessly careful and methodical. In Eddington's original presentation of the eclipse experiments, you will find certain rules of reporting scrupulously followed. Let your eyes surf over the two tables from Eddington's report reproduced in Figure 2.3. That's scientific method made palpable. There's nothing feigned or dishonest about it. In the upper table is a careful accounting of each of the 18 photographic plates taken with the Brazilian astrographic telescope: the time and length of the exposure and the type of plate are noted. In the lower table are results calculated from the apparent positions of the stars in these plates (omitting plates that showed an insufficient number of stars). The numbers that matter most are in the right-hand column: these give the value of gravitational light bending suggested by each plate. At the bottom right-hand corner is the average of these values, which in a single number summarizes what all the photographs taken using the Brazilian astrographic telescope have to say about gravity's effect on light. That "astrographic bending number" is 0.86, almost exactly equal to the Newtonian prediction of 0.87 and less than half the Einsteinian prediction of 1.74.

If the systematicity and objectivity of science can be seen in the painstaking measurement and calculation and the transparent presentation of the astrographic bending number, the subjectivity and unruliness of science can be seen in what happened next: the number, with its Newtonian implications, was brushed off in a few sentences by Eddington, declared unimportant by his allies in the British scientific establishment, EXPOSURES with the 13-inch Astrographic Telescope stopped to 8 inches.

Ref. No.	G.M.T. at Commencement of Exposure.	Exposure.	Plate.	Ref. No.	G.M.T. at Commencement of Exposure.	Exposure.	Plate.
	d. h. m. s.	s.			d. h. m. s.	s.	
1	28 23 58 23	5	O.	11	29 0 1 7	5	S.R.
2	37	10	E.	12	22	10	E.
3	57	5	E.	13	36	5	E.
4	59 11	10	S.	14	51	10	S.R.
5	30	5	S.R.	15	2 10	5	S.R.
6	45	10	S.R.	16	25	10	S.R.
7	29 0 0 4	5	S.R.	17	44	5	E.
8	19	10	E.	18	58	10	E.
9	39	5	E.	19	3 18	5	O.
10	53	10	S.R.				

No. of Eclipse Plate.	Ref. No. of Comparison Plate.	No. of Stars.	Values of d, e, α in Revolutions at 50′ Distance.			α at Sun's Limb in Arc.
			d.	e.	α.	
			r	r	r	″
1	18_4	7	+0·051	+0·089	+0·033	+1·28
2	18_4	11	−0·009	+0·059	+0·025	+0·97
3	18_4	8	−0·074	+0·101	+0·028	+1·09
4	18_4	11	−0·168	+0·091	+0·033	+1·28
5	11_3	10	+0·094	+0·076	+0·025	+0·97
6	11_3	11	+0·186	+0·082	+0·021	+0·82
7	14_3	12	+0·006	+0·119	0·000	0·00
7	18_3	7	−0·054	+0·166	0·000	0·00
8	14_3	10	+0·093	+0·064	+0·021	+0·82
9	17_4	7	−0·096	+0·129	+0·008	+0·31
10	17_4	10	+0·090	+0·045	+0·026	+1·01
11	11_1	10	+0·073	+0·061	+0·032	+1·24
12	11_1	11	−0·009	+0·102	+0·049	+1·91
12	17_2	7	−0·102	+0·114	+0·019	+0·74
15	15_3	6	+0·111	+0·036	+0·018	+0·70
16	15_3	7	−0·002	+0·037	+0·018	+0·70
17	17_2	8	−0·022	+0·109	+0·012	+0·47
18	17_2	7	+0·045	0·000	+0·030	+1·17
Mean				+0·082	+0·022	+0·86

Figure 2.3. The orderly presentation of scientific data: tables summarizing results from the Brazilian astrographic telescope in Eddington's eclipse expedition of 1919.

and ultimately dropped from the textbooks altogether, leaving the more Einsteinian numbers supplied by the other Brazilian telescope and the Príncipe telescope to decide the issue conclusively against Newton and in favor of Einstein's theory of relativity.

In the methodist's dream of science, the bodies of data from the three telescopes, the three measurements of gravity's power to bend light, would be assessed by a procedure that evaluated the evidential weight of each as carefully and as coldly as Eddington had in the first place calculated the numbers. The method would act, in effect, like a high-minded tribunal, objective and authoritative, sorting truth from falsehood without playing favorites or allowing personal or moral or self-aggrandizing considerations to enter into its deliberations.

If the Eddington story is any guide, this is pure mythology. There was no tribunal, no method, to sort the good photographic plates from the bad. The matter was settled the old-fashioned way, by a mix of partisan argument, political maneuvers, and propaganda.

SCIENTIFIC TRIBUNALS MAY be uncommon, but they have been assembled on an ad hoc basis from time to time, and one in particular offers some signal lessons about science.

Louis Pasteur is perhaps the most renowned of all French scientists—and surely the most revered by the French themselves. In his lifetime, from 1822 to 1895, he pioneered vaccination against anthrax and rabies, helped to discover the nature of fermentation, developed a sterilization technique ("pasteurization") to prevent milk and wine from spoiling, laid the foundations for the germ theory of disease, and uncovered the first evidence for the remarkable fact that the chemistry of life is overwhelmingly composed of "right-handed" molecules.

A few years ago, while visiting the École normale supérieure in Paris, I was given the privilege of using Pasteur's old office for a few weeks. (Pasteur served as the scientific director of the ENS from 1858 to 1867; at one point he banned smoking at the school, whereupon almost every student resigned.) Sitting at the antique desk, hoping for the greatness that lingered in that room to diffuse through my nerves and into my fingertips as I typed, I would occasionally be interrupted by visitors knocking at the door, eager to breathe in the august atmosphere of nineteenth-century experimentation and discovery. For the French, Pasteur is scientific thinking made flesh.

One of Pasteur's great victories was the refutation of the doctrine of spontaneous generation. Boil hay in water and decant the resulting fluid into an airtight container. Nothing happens. But let in a little air, and mold begins to grow. Where does it come from? Some nineteenth-century scientists held that the inanimate matter in the hay infusion reacted with the air to form, spontaneously, life where there was none before. Pasteur held, to the contrary, that with the air from the outside came dust containing invisible "germs" or "spores" of mold, which took root in the infusion. To nurture life, the solution had to be seeded with life.

It was clear enough in principle how to decide between these two opposing views. Introduce air that is free of "dust" or "spores" to the mixture. If life develops, spontaneous generation is real.

In practice, the problem is knowing that you have successfully found or created sterile air, given that the stuff we breathe looks much the same with or without spores. Many ingenious solutions were proposed. Air was heated or passed through acid to kill the spores. Experiments were conducted in long-neglected archives, where all dust was supposed to have long ago settled. Air was stored in a container coated with grease to trap the dust or passed through a long and sinuous tube that was supposed to perform the same function (Figure 2.4).

Figure 2.4. Swan neck flask.

The most scenic route to dust-free air was up a mountain trail. In 1860, Pasteur took 20 carefully prepared infusions to the Mer de Glace, a glacier on the Mont Blanc massif in the French Alps, where he exposed them to a chill, pure alpine wind more than 6,000 feet above sea level. Back in Paris, only one developed a moldy growth. Air alone, it seemed, could not bring organisms into existence. But Pasteur had competition. His great rival Felix Pouchet retaliated by performing the same experiment high in the Pyrenees, and all 8 of his infusions, on return to sea level, sprang to life.

Pasteur and Pouchet had sparred the previous year over their contrary views about spontaneous generation. A committee of the French Académie des sciences was convened to issue a prize for the best experimental investigation of the question—a competition whose outcome was understood by both parties to constitute a definitive verdict on the possibility that slime and mold might be created as a matter of course from inorganic ingredients. When the committee assembled, Pouchet discovered that it was packed with allies of Pasteur. He withdrew rather than face such a suspect tribunal. Now, after the success of his Pyrenees experiments, he and Pasteur negotiated a rematch. Once more Pouchet turned up only to find that the judging committee was composed entirely of opponents of his theory. He suggested a change to the rules, which Pasteur persuaded his friends to resist. Again Pouchet withdrew. That was the end of spontaneous generation.

Some writers have accused Pasteur of ensuring that both tribunals were stacked; they point to his reputation (which has been somewhat tarnished by the recent release of his laboratory notebooks) as a combative and unfair disputant in scientific argument. I understand the episode rather as a kind of real-life parable, illustrating the fact that in the scientific process, the weighing of the evidence—the tribunal's task—is seldom objective, seldom particularly methodical, always open to personal and political influence, and ever issuing decisions that are guided as much by expedience as by logic.

My story so far has relied largely on case studies—on anecdote, if you will—but the moral is brought home by several rather unsettling examinations of industry-sponsored research.

Commercial interests sometimes fund independent scientific investigations in the hope of turning up facts conducive to their profits. It emerges that of two groups of scientists working on a question, one financed by industry and one not, the industry-supported group is considerably more likely to produce commercially favorable findings, even when that group consists of university scientists not affiliated with the industry in any other way.

Researchers funded by Coca-Cola, PepsiCo, and other soda manufacturers have been five times more likely than others to find that there is no connection between drinking sugar-sweetened soda and obesity. Those funded by cigarette companies have been seven times more likely than others to find that secondhand smoke has no deleterious effect on health. And whereas non-industry-funded investigators of the efficacy of new drugs may find that the drugs do what they are supposed to do in about 80 percent of studies, investigators funded by the drugs' creators find a positive result almost 100 percent of the time. It looks as though something is guiding the science above and beyond cold, hard facts, something that closely shadows cold, hard cash.

How is it that science, for all its protocols and procedures and statistical handbooks, remains so malleable, so subject in its deliverances to personal, social, financial interests? Do scientists knowingly and deliberately subvert or ignore the scientific method, saluting it in public but then in private doing whatever best suits their ends? Or is the scientific method itself a unicorn, a name for something that isn't really there—leaving scientists to muddle through as best they can using the same rules of thumb that humans have relied on for millennia, subject to all the usual prejudices? Neither explanation, I think, is quite right.

But the lesson is clear: the outcome of the scientific process is powerfully influenced by the aims and interests of its practitioners, from

Eddington's desire for a European peace to the more utilitarian concerns of researchers funded by Big Tobacco or Big Soda. That is one sense in which science is, in spite of the methodists' hopes, decidedly subjective.

In the cases I have described, the "subject" in "subjective"—the researcher—imposes, wittingly or otherwise, their own goals on the course of science. Alongside this steering of science toward personal ends, there is another quite separate sense in which science is run through with subjectivity: scientists impose on science not only their goals but also their theoretical and explanatory tastes. Let me tell another story. It begins with a brand-new book.

BEFORE THERE WAS the internet, nothing said "information" so loudly and lavishly as an intricate map—except perhaps a volume of maps. In 1911, a young German scientist and explorer named Alfred Wegener looked into such a volume, a new edition of Andree's venerable *Allgemeine Handatlas* that made available for the first time the measurements of the ocean's depth conducted by the British *Challenger* expedition. What Wegener saw in the atlas astounded and provoked him.

Many previous geologists, cartographers, and map lovers had noticed the curious similarity between the coastlines of South America and Africa, which suggested that they had been cut from the same colossal slab of rock and dropped on opposite sides of the Atlantic. But nineteenth-century geographers knew that sea levels had risen and sunk considerably over the ages. When the sea level changes, the shape of the coast changes; coastlines, then, are ephemeral, and so the fact that there right now happens to be a suggestive match between the American and African seaboards tells you nothing much about the ultimate origins of those continents.

Thanks to the *Challenger* data, the new German atlas was able to show the outlines not only of landforms but also of continental shelves, those underwater extensions of continents in virtue of which offshore waters are relatively shallow—until they plunge suddenly to truly oceanic depths. The forms of the continents plus their shelves are fixed in a way that coastlines are not: they do not change as the sea rises and falls. What Wegener saw in the pages of the atlas was an almost perfect match between the eastern continental shelf of South America and the western shelf of Africa. Such a match, he thought, could be no coincidence.

Inspired, he wrote what was to become one of the most controversial books of the new century. *The Origin of Continents and Oceans*, which was

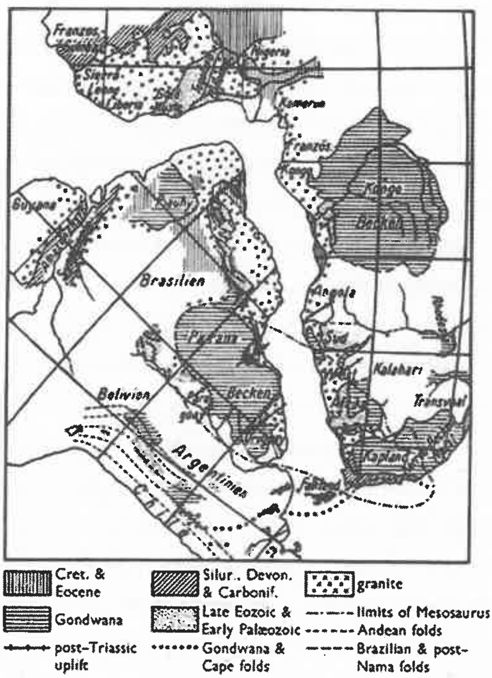

Figure 2.5. A map from a later edition of Wegener's *Origin of Continents and Oceans*, based on the work of the South African geologist Alexander du Toit, showing the geological and fossil (*Mesosaurus*) continuity in the South American and African landmasses.

published in 1915, the same year as Einstein's relativistic theory of gravity, drew on geological and paleontological evidence to argue that Africa and South America must once have been nestled together in an intercontinental embrace. Not only were their shelves a perfect fit; a number of rock formations and fossil remains in one continent left off at the margins of the Atlantic only to pick up again on the other side, in just the place you would expect if the two continents had originally been a single landmass (Figure 2.5). How, then, did they come to be separated by one of the world's great oceans? In some way, suggested Wegener, the continents must have found a way to move over the surface of the earth. Thus was born the theory of continental drift.

The notion of millions of square miles of mountain, forest, river valley, desert, and steppe sailing blithely through the ocean on a voyage to parts unknown is even today difficult to take on board. Skeptics could not accept Wegener's theory without a persuasive hypothesis about the mechanism of drift, a hypothesis that was both plausible and for which there existed some direct evidence. Wegener offered only unsubstantiated speculation about the mechanism—he had no more idea than the skeptics how drift occurred—but he thought that the evidence that it had happened, one way or another, was overwhelming. The fate of the theory teetered: in some places drift was looked on with favor, in others with doubt verging on incredulity. In 1943 it was attacked, however, by the eminent evolutionary biologist George Gaylord Simpson, and it sank under the weight of his reputation. Wegener was by then unable to defend himself. He had perished in 1930 in the most adventurous of circumstances, on a desperate mission to resupply a scientific outpost in Greenland in the face of the oncoming winter. Though he had enjoyed a successful scientific career, the scorn directed at his ideas about drift left him, at his death, a disappointed man.

Why did Wegener fail to persuade the world? He was quite right to think that his accumulated evidence reflected the diverse effects of continental drift. But his critics, including Simpson, were right to think that the lack of a well-evidenced mechanism for such a titanic process was a major consideration against the theory. Who was more right? As so often in science, both the pros and the cons were persuasive.

If the continents move, there must be a physical process by which they accomplish this feat. Wegener's contemporaries knew of no such mechanism. Those who wrote against drift thought wrongly that they understood enough about the earth's geology to rule out any plausible physical story about drift. Those who wrote in its favor were prepared to take the chance that a mechanism existed, though they could only make guesses about its nature. The opponents of drift were overconfident; the proponents were extraordinarily bold, perhaps reckless.

The most reasonable response to Wegener's ideas, you might think, would be simply to sit out the dispute. That would be a mistake. Scientific judgments must sometimes be made using decidedly incomplete evidence—inaction may be disastrous, as now seems to have been true in the case of global climate change. More important still, as both Kuhn and Popper urged, in order to test a theory properly, to dig out all the most telling evidence both for and against it, you need partisans. Only those who are committed to proving the theory true—Kuhn's paradigm-bound scientists constitutionally incapable of doubt—or to proving it false—Popper's agents of generalized theoretical destruction—will have the motivation to perform years or even decades of necessary experimental work. If every scientist had reacted to Wegener by deciding to "wait and see," we might still be waiting.

So scientists must make decisions as to how the evidence bears on theory when there are compelling arguments on both sides. It comes down, often enough, to personal taste—or perhaps to professional circumstances. An establishment man like Simpson—a professor at Columbia and Harvard and a winner of several prestigious awards around the time he set out to refute the hypothesis of continental drift—may feel more at home with the apparently safe or orthodox view, while an outsider and adventurer like Wegener—who was among other things a record-breaking balloonist—may be more ready to take the risk that what's vital to his theory lies just over the scientific horizon. Sometimes the decision is taken on shallow practical grounds: scientists in the United States, where Simpson was more powerful, tended to be far more skeptical about drift than those in Britain and Europe.

In making their contrary calls of judgment, Simpson and Wegener did not ignore the available evidence, but they were heavily influenced also by their temperament, their social position in the institution of science, and no doubt countless other elements of the psyche that differ from one mind to the next. That's how human thinking works. So we get splendid variety; so we get bitter dispute.

According to the thinkers I call methodists, what makes science special is a standardized rule or procedure for conducting empirical inquiry. If what you have seen of science in this chapter so far is representative, nothing of the kind is true. It is not method that conquers human frailty; it is human frailty that conquers method. Just when objectivity matters most, scientists—great scientists, perhaps, above all—are apt to draw on their deepest rhetorical and political resources to skew the course of inquiry to favor their own ends. There is no higher authority to curb the chaos. Science has no single impartial judge to hand down decrees to which all researchers are obliged to subscribe, but rather numerous contending Pasteurian tribunals, each packed with the advocates of a particular set of interests or a particular way of seeing the world.

I began *The Knowledge Machine* by presenting the ideas of two great methodist thinkers not only because methodism is simple, appealing, and historically important, but also because I, too, am a kind of methodist: I want to appeal to a shared scientific code to account for science's supreme ability to find theories of great predictive and explanatory power. In that case, I seem to have run into something of a problem: a code is not much use if it is ignored just when it matters most. Chapter 2 is not yet over and methodism is already battered, down on its knees, begging for mercy.

To the rescue comes an idea entertained by many concerned scientists, advanced by Karl Popper, and articulated here by the writer and surgeon Atul Gawande:

> Individual scientists . . . can be famously bull-headed, overly enamored of pet theories, dismissive of new evidence, and heedless of their fallibility. . . . But as a community endeavor, [science] is beautifully self-correcting.

That "science is self-correcting" is often heard in the wake of revelations of fraud or methodological recklessness. Those who mount this defense of scientific objectivity, such as Gawande, ruefully concede that the bad or at least careless behavior of scientists such as Eddington or Pasteur is a serious impediment to scientific progress: it may slow, divert, even temporarily reverse the growth of knowledge. But scientific inquiry is both competitive and cooperative enough that its practitioners check up on one another, hoping either to demonstrate the reliability of research they plan to build on or to debunk work that conflicts with their own results. Even if scientists lack the proper Popperian critical perspective on their own work, they have many reasons to apply that exacting and skeptical attitude to the work of others.

Further, in the opinion of most "self-correctors," the transgressions documented in this chapter are real but unrepresentative. They are the most extreme cases, selected for anecdotal rather than statistical effect. Were you to raid some science lab at random, you would find goings on far less salacious: not saintly by any means, but for the most part a creditable attempt to more or less follow the objective rules of inference laid down by the logic of the scientific method.

As a matter of fact, maybe a visit like that is not such a bad idea.

IN 1975, a young French anthropologist named Bruno Latour went to live among the natives of an unusual southern Californian subculture. His subjects were researchers working in the laboratory of the endocrinologist Roger Guillemin—the scientist who was to share the Nobel Prize in Medicine two years later for determining the structure of the brain hormone TRH.

At the time, Latour's "knowledge of science was non-existent," he later wrote—adding that he was "completely unaware of the social studies of science"—but for these very reasons he was "in the classic position of the ethnographer sent to a completely foreign environment," like Margaret Mead in the Samoan archipelago or Napoleon Chagnon among the Yanomami of the Amazon rainforest.

Latour spent two years inside Guillemin's knowledge machine at the Salk Institute in San Diego. He found a great wheel, a self-replenishing cycle of being. Into the Guillemin lab came vast quantities of chemicals, animals, and energy, which fueled a complex physical and social process that produced scientific reports by way of "inscription devices" whose function was to "transform pieces of matter into written documents" (Figure 2.6). These documents, the articles published in scientific journals, were in turn transformed into "credit," or scientific reputation, which is valuable largely not as an end in itself but because it provides the means to win funding that will buy more chemicals, animals, and energy, along with new inscription devices and the services of scientists and technicians to turn them into more papers and thus still more credit. A science lab, in the telling of Latour and his collaborator Steve

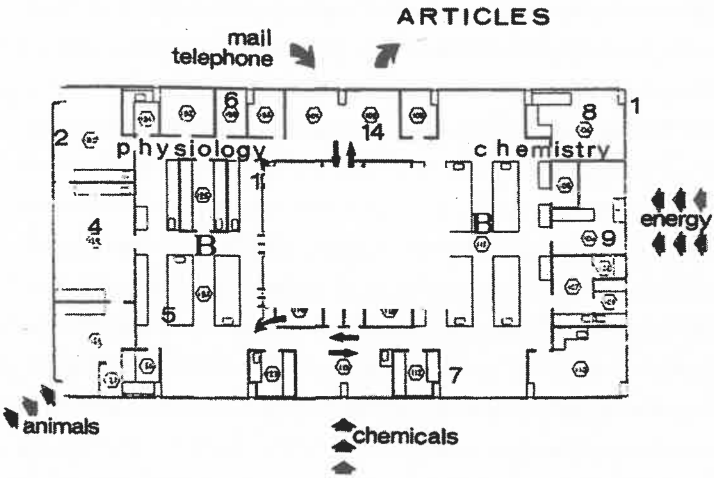

Figure 2.6. Guillemin's knowledge machine, as annotated by Latour.

Woolgar in their book *Laboratory Life*, is not so much an appliance as an organism: its primary concern is survival and reproduction.

What about the rules of thought that, according to Gawande and Popper, govern most ordinary scientific inquiry—the code of intellectual conduct against which scientists' reasoning is periodically compared, allowing science to "self-correct" and so to stay on a more or less objective path?

Latour did see much that was objective in the Guillemin lab. Exacting procedures for pulverizing brain tissue and extracting substances such as TRH were known to all the technicians and were carefully followed. The same is true of the preparation of the data that was used to test hypotheses about TRH and other molecules' structure. In one such technique, a synthetic substance with the hypothesized structure was created and compared with TRH, to see whether there was a match. The comparison involved images created using the two substances, called "chromatograms." If two (correctly prepared) substances have the same chromatograms, they have the same components; identical chromatograms, then, show that the hypothesized structure is correct. A chromatogram is prepared using a machine that can be ordered from a scientific supply company, and that machine comes with a manual. In the manual are instructions for using the machine correctly; if a scientist fails to follow the manual, they have erred objectively and conspicuously, a mistake that can be corrected, just as Gawande and Popper maintain.

But although the preparation of evidence may have answered to objective rules, Latour found that the interpretation of evidence did not. There will always be small differences between two chromatograms of the same substance, in the same way that there are small differences in two photographs of the same person. Scientists must decide, then, when those differences are small enough not to matter. If there was a rule to decide these questions, Latour's subjects did not follow it. They resorted, rather, to "local tacit negotiations, constantly changing evaluations, and unconscious or institutionalized gestures." The same was true of all other important questions of interpretation: in deciding how the evidence bore on the hypotheses that it was supposed to test, there was no appeal to shared rules, to objective criteria. Instead there were arguments, gut reactions, bargains, local cultures. Summing up, Latour and Woolgar wrote, "We were unable to identify explicit appeal to the norms of science."

In Guillemin's lab, then, there was much objective weighing of brains and their juices, but little objective weighing of evidence. When it came to determining what the carefully collated data said about theories of TRH's structure and the like, not even the methodological yeomen—the postdoctoral fellows, the junior scientists, the quiet, decent majority flying in science's economy class—followed rules. Heedless of official restrictions, they went on stuffing the overhead bins of scientific inference with their moral, psychological, political, and cultural baggage.

It is the way things are throughout the scientific world. The hematologist James Zimring reports the dismay of novice scientists upon encountering for the first time the reality of the lab:

> The work they were doing and that of their fellow researchers seemed "messed up." It was chaotic, did not progress logically. . . . Often rationalizations were made up after the fact to account for progress.

Science can hardly correct itself if no one is paying the least attention to standards for correctness.

Where is science's method, then? It is nowhere at all, say the followers and fellow travelers of Latour. There is no structure or system that makes science a more objective, a more valid, a more truth-directed way of knowing the world than any other. The Great Method Debate is an argument over a fiction. That is the thesis, endorsed by many contemporary sociologists and historians of science, that I call *radical subjectivism*. It is the antithesis of methodism.

According to the radical subjectivists, then, the world of scientific inquiry is, for all its specialized apparatus and ideology, essentially a microcosm of the multiplicity of human society, its tens of thousands of participants each having their own idea of what is worth doing and how it might be done, traveling more often at cross-purposes than together, sometimes not talking at all, sometimes arguing with each other, sometimes subtly undermining each other, sometimes seeing only what they want to see, sometimes seeing only what they've been told they'll see, sometimes only seeing their status relative to their rivals in an endeavor whose content, the stuff of scientific theories, may be treated more as a means to self-promotion than as an end. Science, in the radical subjectivist view, is just another venue for the Machiavellian masterpiece theater that is the human condition.

What about the force of the empirical facts? "The natural world has a small or non-existent role in the construction of scientific knowledge," writes the sociologist of science Harry Collins; similarly, the sociologist Stanley Aronowitz says, "Science legitimates itself by linking its discoveries with power, a connection which *determines* (not merely influences) what counts as reliable knowledge." The facts are, in short, pawns in a game in which the "strongest team decrees what counts as truth."

Popper's focus on falsification, Kuhn's paradigm-by-paradigm approach, any other system or method that asks scientists to put aside their mortal ways: hopeless, each and every one, according to the radical subjectivists.

Scientists are all too human. They do not follow a universally valid script. They do not follow the locally valid script of a Kuhnian paradigm. They are irrepressibly contentious, contextual, and quirky.

The radical subjectivists are, I think, right about the subjectivity of science. But they cannot be right in their further claim, that there is nothing whatsoever to distinguish science from ordinary thought or philosophical contemplation. That would explain everything about the messy human business of scientific inquiry except what matters most: the great wave of progress that followed the Scientific Revolution. Medical progress, technological progress, and progress in understanding how it all hangs together, how everything works. Immense, undeniable, life-changing progress.

Some radical subjectivists hint that the supposed contribution of scientific understanding to the advance of human happiness is mere propaganda put about by Big Science, arguing that technological advances issue more from trial and error than from deep knowledge of the underlying structure of things. But most acknowledge science's theoretical as well as its practical success. "Science remains . . . certainly the most reliable body of natural knowledge we have got," writes the sociologist Steven Shapin. Such an abrupt conclusion to the historical and sociological litany of scientific fallacy, frailty, and confabulation adds up to something not unlike this retelling of the famous fairy tale: *Cinderella was a poor and bedraggled child, beaten and abused by her stepmother and stepsisters, treated little better than a slave. And so she married the handsome prince and lived happily ever after.* A part of the story must be missing—the best part.

Among the detritus left behind after the ball—among the ulterior motives, self-serving explanations, power plays, crushed flowers, shattered beakers, broken promises—you might still hope to spot the gleam of a glass slipper, the clue that will join together the two halves of the tale. The slipper is a scientific method, an objective rule that lays down a standard of scientific conduct followed even by the Eddingtons and the Pasteurs, answering the big philosophical question about the source of science's knowledge-finding power.

If it exists, the slipper must be a subtle and exquisite thing. It must fit with everything said previously about the malleability of scientists' words and actions while providing an alternative to the radical subjectivist interpretation. It must show that scientific inquiry is not human business as usual, but rather that there is something unique and objective about the conduct of science that explains its success. It must permit but at the same time overcome our humanity.

Is that an impossible demand? I will show you that it is even more difficult to satisfy than you might have supposed. Not only human nature but also the very laws of logic contend against the possibility of objectivity in scientific thought.