THE KNOWLEDGE MACHINE

How Irrationality Created Modern Science

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CONTENTS

THE
KNOWLEDGE
MACHINE

INTRODUCTION

The Knowledge Machine

Why is science so powerful?
Why did it take so long to arrive?

Were you to be transported to some randomly chosen place and time in human history, you would likely find yourself gathering pinhead-sized grains, hunting dangerous game with a sharpened stick, and living in a damp, unfurnished hollow.

If you were very lucky, however, you might wake up in the sandals of a wealthy citizen of the Greek world around the time of Alexander the Great. From this position of privilege you could enjoy just about every cultural invention that makes life worth living today. You could delight in the poetry of Homer and Sappho, visit the theater to relish *Oedipus Rex* and other masterpieces of ancient drama, hire a musician to serenade your friends after dinner. You could live in cities regulated by law and a system of courts, shaped by the architects and sculptors who built some of the seven wonders of the world, and governed in accordance with political models that have lasted to this day: monarchy, oligarchy, sweet democracy. If you had the aptitude and the inclination, you could undertake advanced studies in geometry or philosophy.

Yet—you would soon notice a few things missing even from this cultural Elysium. X-rays and MRIs, travel at speeds faster than the swiftest horse, voices and video coverage of world events streaming through the thyme-scented Mediterranean air are nowhere to be found. Most egregiously absent of all is the thing that makes our own advanced medicine, transport, and telecommunication possible: the knowledge-producing machine that we call modern science.

Civilization stretches back millennia. But the machine has been around only a few hundred years. What took so long?

Among the ancients there was no lack of desire to understand the workings of the world. Around 580 BCE, the Greek philosopher Thales gazed out from the port city of Miletus into the Aegean blue, into the summer haze where sea merges imperceptibly with sky, and proposed that everything is ultimately made of water. His student Anaximenes disagreed: the fundamental stuff, he said, is air. Heraclitus, who lived in Sicily a few decades later, suggested fire. Back in Miletus, Anaximander—another student of Thales about whom we know equally little—had meanwhile hypothesized that all things are composed of invisible stuff of unlimited potential that he called *apeiron*, or "the boundless."

Though these thinkers, their contemporaries, and their successors—Chinese scholars, Islamic doctors, medieval European monks—argued ingeniously for their points of view, no one of their ideas could gain a foothold over the others. Searching for the deep structure of nature, they contributed immeasurably to humanity's stock of brilliant and original hypotheses, but to its stock of knowledge, scarcely at all.

The reason is straightforward. Although premodern inquirers into nature sometimes hit on the right idea, they had little ability to distinguish it from its rivals. By the time of the collapse of the Western Roman Empire in the fifth century CE, almost every possible hypothesis about the relation between the earth, the planets, and the sun had been proposed: that the planets and the sun revolved around a fixed earth, that the earth and the planets revolved around a fixed sun (as the Greek philosopher Aristarchus suggested in the third century BCE), and that some or all of the planets revolved around the sun, which in turn revolved around the earth (an idea passed on by Roman writers to the philosophers of the Middle Ages and invented independently in India in the fifteenth century). It was only a thousand years after the fall of Rome, however, that it became generally agreed—and soon after, known for sure—which one of these theories was correct.

That great leap forward was made in the exhilarating period between the years 1600 and 1700, during which empirical inquiry evolved from the freewheeling, speculative frenzy of old into something with powers of discovery on a wholly new level—the knowledge machine. Driving this machine was a regimented process that subjected theories to a pitiless interrogation by observable evidence, raising up some and tearing down others, occasionally changing course or traveling in reverse but making in the long term unmistakable progress. Where Thales once surveyed the horizon and saw water, our radio telescopes look into deep space and see dark matter.

It is to mark this sudden change in the tempo and form of discovery that historians call what happened the "Scientific Revolution," and philosophers and sociologists distinguish what came after the Revolution as a new way of thinking about the world. In so doing, they set "modern science" apart from what preceded it—that is, ancient and medieval science, or what is sometimes called, to emphasize the striking discontinuity, "philosophy of nature" or "natural philosophy." The natural philosophy that came before the Scientific Revolution was not less creative than modern science, and as practiced by thinkers such as Aristotle was no less methodical and no less concerned with the evidence of the senses. Yet something, it seems, was missing.

For that extraordinary something, why the excruciating wait? Why, after philosophy and democracy and mathematics tumbled through the doors of the ancient thinkers' consciousness in quick succession, did science dawdle on the threshold? Why wasn't it the ancient Babylonians putting zero-gravity observatories into orbit around the earth; the Han Chinese building particle accelerators in the flat fields along the Yellow River; the Maya growing genetically modified corn in the Yucatán; the ancient Greeks engineering flu vaccines and transplanting hearts?

Events such as revolutions and elections, declarations and emancipations occur at particular times and in particular places. But a nonevent such as science's non-arrival happens, so to speak, almost everywhere. Modern science did not arrive in democratic Athens. It was not invented by Aristotle. It failed to develop in China a thousand years ago, in spite of that nation's cohesion, scholarly tradition, and technological prowess. Neither Islamic nor European medicine succeeded in becoming truly scientific. The Maya, the Aztecs, the Incas; Goryeo Korea and the Khmer Empire of Cambodia; the India of the Maurya and Mughal Empires: we marvel at their temples and pyramids, their rich traditions of theater and dance. But these cultures, all wealthy, powerful, and sophisticated, are equally non-inventors of science.

The long absence of science is therefore not to be explained by some particular train of events or some specific mix of custom and circumstance. It spans democracies and theocracies, east and west, pantheists and People of the Book. It seems, to all the world, that there is something about the nature of science itself that the human race finds hard to take on board.

That, I believe, is the answer: science is an alien thought form. To understand its late arrival on the human scene, we need to appreciate the inherent strangeness of the scientific method.

The first step is to take a close look at the method, at the rules that drive modern science and explain its fact-finding power. An easy task, you might think. The principles in question regulate science wherever it is found. Every university, every research facility, every industrial laboratory follows them. Go there; make a few queries. Scientists themselves will tell you what science is and how it functions.

Yet it has turned out to be anything but straightforward to obtain a satisfactory answer. Some scientists say that the essence of science is controlled or repeatable experiment, forgetting that experiments are of relatively little importance in cosmology or evolutionary biology. Some say advanced mathematical techniques are crucial, forgetting that the discoverers of genetics, for example, had no use for sophisticated math. Some say what matters is observation. That is a big-hearted answer—it does not exclude whole branches of science like the other responses—but it is too generous. The ancient Greek natural philosophers sought to explain what they saw around them, yet for all their yearning to make sense of the observable world, they had not yet laid their hands on the secret of modern science.

Carry out a poll of the scientific profession, then, and you will soon discover that although scientists know very well how to implement their methods, they don’t know what it is about those methods that really matters, and why.

What about other scholars who study the nature of science—the historians of science, the sociologists of science, the philosophers of science? You’ll find no more agreement among them than you do among working scientists. Indeed, the question of the scientific method is one of the most difficult, most contentious, most puzzling problems in modern thought.

The consequence is an argument that has sometimes smoldered, sometimes blazed for well over a hundred years—the Great Method Debate. Subtle, powerful thinkers have attempted to describe the scientific method and come to quite opposing conclusions.

More perplexing still, many who have inspected science closely have concluded that there is no such thing as the scientific method. To the question of what is new about modern science, of what changed in the Scientific Revolution, they answer "Nothing much"—or as the sociologist Steven Shapin has declared, "There was no such thing as the Scientific Revolution." Three centuries after Newton explained why the planets orbit the sun, the nature of science is, as the philosopher of science Paul Feyerabend has written, "still shrouded in darkness."

Into that darkness *The Knowledge Machine* will plunge, searching for illumination among the tangle of competing visions of and skepticism about the scientific method. It will wrangle with philosophers such as Karl Popper, who believed that the method hinges on a certain kind of logic applied by thinkers with the right sort of temperament, and Thomas Kuhn, who thought that it is rather a special kind of social organization that is responsible for science's power. It will confront sociologists such as Steven Shapin who hold that no method exists. And it will put forward its own proposal about the nature of the method.

There are many reasons to join the Great Method Debate. Science is so vital to the quality of our modern life that even if the scientific method turned out to be something rather boring and unremarkable—a superior kind of experimental technique, say—it would be imperative to find it and to frame it in a book.

I would have no interest, however, in writing that book. What fascinates me is that science's rules of engagement are so unexpected, so unintuitive, so odd. It is this peculiarity, I believe, that accounts for science's late arrival. Even putting aside the fascinating question of science's delay, the weirdness of the scientific method is an intellectual spectacle in itself. It is to share and delight in that spectacle, as much as anything else, that I put these pages before you.

Once I have taken my turn as the P. T. Barnum of the laboratory, unveiling the monstrosity that lies at the heart of modern science, you will begin to understand why it has been so difficult to chase down. Those who have searched for a scientific method—the *methodists*—have been looking for a logical and behavioral directive that expunges human whim from scientific thought, replacing it with a standardized rule or procedure for judging theories in the light of evidence that explains science's stupendous knowledge-producing capacity. The rule that governs science and explains its success is far weaker, however, than the methodists have supposed: it tells you what counts as evidence but offers no system for interpreting that evidence. Indeed, it says nothing about the significance of evidence at all.

Further, the rule does not reside where the methodists have expected to find it, in scientists' heads. It does not tell scientists what to think privately; it merely regulates how they argue publicly. It is not a method of reasoning but a kind of speech code, a set of ground rules for debate, compelling scientists to conduct all disputes with reference to empirical evidence alone.

That explains why the methodists failed to find their method. They were looking for the wrong kind of rule. And they were looking in the wrong place.

How can a rule so scant in content and so limited in scope account for science's powers of discovery? It may dictate what gets called evidence, but it makes no attempt to forge agreement among scientists as to what the evidence says. It simply lays down the rule that all arguments must be carried out with reference to empirical evidence and then steps back, relinquishing control. Scientists are free to think almost anything they like about the connection between evidence and theory. But if they are to participate in the scientific enterprise, they must uncover or generate new evidence to argue with. And so they do, with unfettered enthusiasm.

The resulting productivity is what makes all the difference: science is a machine for motivating disputatious humans to carry out tedious measurements and perform costly and time-consuming experiments that they would otherwise not care to undertake. It is these empirical minutiae, so painful to collect, that single out the truth among the plausible falsehoods. Eventually, enough such evidence accumulates that just about every scientist, whatever their quirks, biases, and prejudices, agrees on its significance: one theory stands well above the rest as the best explainer and predictor of it all.

The apparently unassuming code of public behavior, the evidence-only constraint on scientific argument that constitutes all the method science needs to set humanity marching inexorably toward truth, deserves a grand name; I call it the *iron rule of explanation*. Much of *The Knowledge Machine* is given over to understanding where the iron rule came from, what it amounts to, and by what means it leads science toward enlightenment. That will be my attempt to settle the Great Method Debate. If I am correct, then in this book you will discover how science really works.

You will also find the answer to my opening question, learning why it took so long for the human race to discover the power of the iron rule. I won't explain science's late arrival by composing a history of the origins of the Scientific Revolution. My interest is in all the apparently propitious places and times that science *failed* to appear. That nonappearance is to be explained by something timeless: that the iron rule, the key to science's success, is unreasonably closed-minded. It works superbly well, but from the outside, it looks to be, quite simply, an irrational way to inquire into the underlying structure of things. The ancient Greeks had poetry, music, drama, philosophy, democracy, mathematics—each an expression and an elevation of human nature. Science, by contrast, requires of its practitioners the strategic suppression of human nature, indeed, the suppression of the highest element of human nature, the rational mind. What Greek philosopher could have supposed that this was the route to unbounded knowledge of the world? The mystery is not that science arrived so late, but that as a technique for discovery it was ever hit upon at all.

By the end of *The Knowledge Machine*, then, I will have answered two big questions, one philosophical and the other historical:

1. How does science work, and why is it so effective?
2. Why did science arrive so late?

To the first, philosophical question, I say: what matters is the iron rule. To the second, historical question, I say: it is the irrationality of the rule that barred it from human consciousness for so long.

The investigation of the intellectual, moral, and social structure of science that answers both the philosophical and historical questions will constitute by far the greater part of *The Knowledge Machine*, but near the end I will yield to the temptation to do something more like conventional history, explaining why science finally turned up when and where it did, in the European seventeenth century. I then comment on the impact of the iron rule's irrationality on the shape of science today and ask how we can best maintain, even improve, the knowledge machine so as to continue to profit from its penetration and power—and not least, so as to save ourselves from some of the havoc it has helped us to wreak on our planet.

The Knowledge Machine has much to say in favor of science. It sets out to defend scientific inquiry against those who doubt its legendary ability to find the truth—against fundamentalists, postmodernists, romantics, spiritualists, philosophical skeptics. The legend, I show, is firmly founded in fact.

Yet these same arguments and explanations show how peculiar and often inhuman in its workings the knowledge machine can be. It gets the job done not in spite of but in virtue of its proprietary blend of inarticulacy, closed-mindedness, and systematic irrationality. No wonder humanity was so reluctant for so long to pull the lever that brought it buzzing and spluttering to life.

My story begins at the height of the Great Method Debate, as the twentieth century's two foremost philosophers of science—Karl Popper and Thomas Kuhn—lay out opposing visions of the mechanism behind science's knowledge-making capacity. Neither effort will succeed. But sifting through the philosophical wreckage, I will find the foundation on which to construct a better theory of science.

I

THE GREAT METHOD DEBATE

CHAPTER 1

Unearthing the Scientific Method

Karl Popper's and Thomas Kuhn's profoundly different theories of the way science works—and the idea they share that points the way to the truth

In 1942, Karl Popper was excavating the bones of a gigantic extinct bird, the monumental moa, near Christchurch, New Zealand. A position teaching philosophy just north of Antarctica was his refuge from Hitler's armies, which had marched into his home city of Vienna four years before.

When he was not prospecting for oversized avian remains, he was hard at work writing a condemnation of totalitarianism, in both its Nazi and communist forms, to be published at the end of the war. The twentieth century's political and human chaos showed, Popper thought, that progress of any sort would be possible only through the vigorous exercise of the highest forms of critical thought, and for this Austrian refugee and antipodean adoptee, highest of all was scientific inquiry—perhaps the only human activity, he wrote, "in which errors are systematically criticized and fairly often, in time, corrected." Much of his life's work was therefore given over to an investigation of the rational basis of science, presented to the world in his philosophical masterpiece *The Logic of Scientific Discovery*.

The ideas in that book would change the way generations of physicists, biologists, economists, and philosophers would think about the scientific method. After Popper moved from New Zealand to take up an academic post in Britain in 1946, he was elected a Fellow of the Royal Society, knighted by Queen Elizabeth II, and declared by the Nobel Prize–winning biologist Sir Peter Medawar to be "incomparably the greatest philosopher of science that has ever been."

Popper, born in 1902, turned 12 the day that Austria-Hungary declared war on Serbia, precipitating the Great War. He came of age in the social and economic devastation that followed. “The war years and their aftermath,” he later wrote, “were in every respect decisive for my intellectual development. They made me critical of accepted opinions, especially political opinions.” He continued:

> The famine, the hunger riots in Vienna, and the runaway inflation . . . destroyed the world in which I had grown up. . . . I was over sixteen when the war ended, and the revolution incited me to stage my own private revolution. I decided to leave school, late in 1918, to study on my own. . . . There was little to eat; and as for clothing, most of us could afford only discarded army uniforms. . . . We studied; and we discussed politics.

For a few months, Popper threw in his lot with the communists, only to back away after a violent demonstration led to several protesters’ deaths—a consequence, he thought, not only of police brutality but also of the demonstrators’ tactical aggression.

He remained a socialist, however, and around 1919 resolved to take up manual work. At this time he was squatting in an abandoned wing of a former military hospital, feeding himself by tutoring American university students. The experiment with blue-collar labor turned out badly: he was, he tells us, too feeble to wield a pickaxe and too distracted by philosophical ideas to produce the straight edges and square corners required of a cabinetmaker. Abandoning these pursuits, he became a social worker, caring for neglected children. Not much later he left behind socialism itself, reasoning that while freedom and equality are both much to be desired, to have both was impossible—and in the end, “freedom is more important than equality.”

Figure 1.1. Police attempt to contain communist demonstrators in Vienna, June 1919.

In the same year, Popper heard Einstein lecture on his new theory of relativity: "I remember only that I was dazed. This thing was quite beyond my understanding." But he was struck by Einstein's willingness to subject his theory to empirical tests that might disprove it:

> Thus I arrived, by the end of 1919, at the conclusion that the scientific attitude was the critical attitude, which did not look for verifications but for crucial tests; tests which could *refute* the theory tested, though they could never establish it.

In that single italicized word germinated Popper's greatest and most influential idea.

The idea had its roots in a conundrum posed by the Edinburgh philosopher David Hume in 1739, in the first decades of the Scottish Enlightenment. Imagine Adam, mused Hume, waking in Eden for the first time—naked, alone, wholly unspoiled by knowledge of any sort. Wandering through the primeval woods, he makes some elementary discoveries: fire burns, fruit nourishes, water drowns. Or more exactly, he makes some particular observations: he burns his fingers in some particular fire; he finds some particular pieces of fruit from some particular trees nourishing; he sees some particular animal drown in some particular river. Then he generalizes, using all his newborn wit: best you avoid getting too close to any fire; best to satisfy your hunger by eating fruit from that kind of tree; and so on. This sort of generalization from experience is called inductive reasoning, or, for short, induction.

What, Hume asked, justifies these generalizations? Why is it reasonable to think that merely because this fire burned you yesterday, it will burn you again today? It's not that Hume was recommending that you plunge your hand into the flames any time soon. He just wanted you to explain your reluctance.

There is an obvious answer to Hume's innocent inquiry: things tend to behave the same way at all times—at least most things, most of the time. Fire will tend to affect flesh similarly, yesterday, tomorrow, and next week. So, in the absence of any other information, your best bet for predicting fire's future effect is to generalize from the effects you've already seen. The practice of induction is justified, in other words, by appealing to a universal tendency to regularity or uniformity in the behavior of things. Hume considered this answer, and replied: yes, but what justifies your belief in uniformity? Why think that fire's effects are fixed? Why think that future behavior is in general like past behavior?

There's an obvious answer to that question, too. We think that behavior will be the same in the future as it was in the past because in our experience, it always has been the same. We justify our belief in uniformity, then, by saying that nature has always been uniform in the past, so we expect it to continue to be uniform in the future.

But that, as Hume observed, is itself a kind of inductive thinking, generalizing as it does from past to future. We are using induction to justify induction. Such circular reasoning cannot stand. The snake in the garden swallows its own tail.

There is no other route, Hume thought, by which inductive thinking might be vindicated. He was a philosophical skeptic: he believed that all those inferences that are so vital for our continued existence—what to eat, where to find it, what to pass over—are at bottom without justification. But like many skeptics, he was also a conservative: he advised us to press on with induction in our everyday lives without asking awkward philosophical questions. The English philosopher Bertrand Russell, writing about Hume two hundred years later, could not accept this philosophical quietism: if induction cannot be validated, he wrote, "there is no intellectual difference between sanity and insanity." We will end up like the ancient Greek skeptic who, having fallen into a ditch, declined to climb out because, for all he knew, his future life in the mud would be as good as, perhaps much better than, life above ground. Or as Russell put it, our position won't differ from that of "the lunatic who believes that he is a poached egg."

For all that, there is still no widely accepted justification for induction. Popper saw no alternative but to accept Hume's argument; unlike Hume, however, he concluded that we must abandon inductive thinking altogether. Science, if it is to be a rational enterprise, must not regard the fact that, say, fire has been hot enough to burn human skin in the past as a reason to think that it will be hot enough to burn skin in the future. Or to put it another way, the fact that fire has burned us in the past may not in any way be counted as "evidence for" the hypothesis that fire will be hot enough to burn us in the years to come. Indeed, science ought not to make any use whatsoever of the notion of "evidence for." So there can be no evidence for the hypothesis that the earth orbits the sun (since that implies that the earth will in the future continue to orbit the sun); no evidence for Newton's theory of gravitation; no evidence for the theory of evolution; no evidence in fact for anything that we've ever called a "theory."

This might sound like just the sort of insanity that Russell feared. But Popper was no poached egg. Science, he thought, had a powerful replacement for the inductive thinking undermined by Hume. There may be no such thing as evidence *for* a theory, but what there can be—and here Popper recalled his youthful bedazzlement by Einstein in 1919—is evidence *against* a theory. "If the redshift of spectral lines due to the gravitational potential should not exist," Einstein wrote of a certain phenomenon predicted by his ideas, "then [my] general theory of relativity will be untenable." As Einstein saw, we can know for sure that any theory that makes false predictions is false. To put it another way, a true theory will always make true predictions; false predictions can issue only from falsehood. No assumptions about the uniformity of nature are needed to grasp that.

If your theory says that a comet will reappear in 76 years and it doesn't turn up, there is something wrong with the theory. If it says that things can't travel faster than the speed of light and it turns out that certain particles gaily skip along at far greater speeds, there is something wrong with the theory. And indeed, if your theory says you are a poached egg and you find yourself strolling the London streets on two sturdy legs far from the nearest breakfast establishment, then that theory, too, is wrong. Russell needn't have worried. Unlike inductive thinking, this is all just straightforward, incontrovertible logic.

Such is the logic, according to Popper, that drives the scientific method. Science gathers evidence not to validate theories but to refute them—to rule them out of the running. The job of scientists is to go through the list of all possible theories and to eliminate as many as possible, or, as Popper said, to "falsify" them.

Suppose that you have accumulated much evidence and discarded many theories. Of the theories that remain on the list, it is impossible, according to Popper, to say that one is more likely to be true than any of the others: "Scientific theories, if they are not falsified, forever remain . . . conjectures." No matter how many true predictions a theory has made, you have no more reason to believe it than to believe any of its unfalsified rivals.

Let me repeat that. Popper is sometimes said (by the *New Oxford American Dictionary*, for example) to have claimed that no theory can be proved definitively to be true. But he held a far more radical view than this: he thought that of the theories that have not yet been positively disproved, we have absolutely no reason to believe one rather than another. It is not that even our best theory cannot be definitively proved; it is rather that there is no such thing as a "best theory," only a "surviving theory," and all surviving theories are equal. Thus, in Popper's view, there is no point in trying to gather evidence that supports one surviving theory over the others.

Scientists should consequently devote themselves to reducing the size of the pool of surviving theories by refuting as many ideas as possible. Scientific inquiry is essentially a process of disproof, and scientists are the disprovers, the debunkers, the destroyers. Popper's logic of inquiry requires of its scientific personnel a murderous resolve. Seeing a theory, their first thought must be to understand it and then to liquidate it. Only if scientists throw themselves single-mindedly into the slaughter of every speculation will science progress.

Scientists are creators as well as destroyers: it is important that they explore the theoretical possibilities as thoroughly as they can, that they devise as many theories as they are able. But in a certain sense they create only to destroy: every new theoretical invention will be welcomed into the world by a barrage of experiments devised solely to ensure that its existence as a live option is as short as possible. There can be no favorites. Scientists must take the same attitude to the theories that they themselves concoct as to those of others, doing everything within their power to show that their own contributions to science are without any basis in fact. They are monsters who eat their own brainchildren.

It is carnage, this mass extermination of hypotheses. Yet Popper, the survivor of two world wars, thought it essential to human progress:

> Let our conjectures . . . die in our stead! We may still learn to kill our theories instead of killing each other.

To be an imaginative explorer of new theoretical possibilities and a ruthless critic, determined to uncover falsehood wherever it is found—that is the Popperian ideal. Scientists are both empirical warriors and intuitive artists, combining originality and openness to new ideas with an intellectual honesty that regards nothing as above suspicion.

Tough and tender, hard-eyed yet broad-minded, passionate, courageous, imaginative—who would not sit for such a self-portrait? Working scientists fell head over heels for Popper's ideas. "There is no more to science than its method, and there is no more to its method than Popper has said," proclaimed the cosmologist Hermann Bondi, declaring Popper the uncontested winner of the Great Method Debate. The eminent neuroscientist John Eccles wrote, "I learned from Popper what for me is the essence of scientific investigation—how to be speculative and imaginative in the creation of hypotheses, and then to challenge them with the utmost rigor."

Popperian formulations abound not only in philosophical panegyric but also in practice, most notably in postwar Britain, where Popper made his home. In attempting to undercut the work of the neuroendocrinologist Geoffrey Harris in 1954, the anatomist Solly Zuckerman declared that a scientific hypothesis "falls to the ground the moment it is proved contrary to any of one of the facts for which it is designed to account"; he then flaunted a single ferret brain that he supposed would annihilate Harris's career.

Popper's contribution to the mythos of science is familiar to many scientists and science lovers. I often wonder whether they grasp, however, how peculiar a view of the logic of science lies at its core—a view on which no amount of evidence can give you more reason to believe a theory than you had when it was first formulated and completely untested; a view on which induction is a lie; a view on which you have no grounds whatsoever to think that the future will resemble the past, that the universe will go on humming the same tune rather than spontaneously changing its song.

Almost every other philosopher of science finds room for induction. Some believe that Hume's problem must have a solution—that is, a philosophical argument showing that it is reasonable to suppose that nature is uniform in certain respects, though we may still be waiting for the thinker clever enough to unravel the Humean knot. Some believe, like Hume himself, that it has no solution but that we must go on thinking inductively regardless, both in our science and in our everyday lives. All believe that induction is essential to human existence. What made Popper different?

Perhaps there is a clue in a story told about Hans Reichenbach, a professor of philosophy in Berlin in the early 1930s. Like Popper, Reichenbach escaped to the English-speaking world as totalitarianism engulfed his Germanic homeland. Reichenbach had not thought much about Hume's worry that the future may fail to resemble the past until 1933. In that year, the Nazis burned the Reichstag, took control of the University of Berlin, and expelled many of its Jewish professors and staff, Reichenbach included. "Then," Reichenbach is said to have observed, "I understood at last the problem of induction."

REICHENBACH, POPPER, AND many like-minded refugees fleeing the mayhem and malevolence of Central Europe between the wars promoted an ideal of the scientist as a paragon of intellectual honesty, standing up for truth in the face of stifling opposition from the prevailing politics, culture, and ideology.

To this vision, Thomas Kuhn presented the utter antithesis, a dark and deflating conception of the internal machinery of science liable to repel working scientists and on first appraisal quite unsuited to explain science's heroic feats of discovery.

Before becoming a philosopher, Kuhn was a historian of science. Before becoming a historian, he was a physicist. The road was straight and smooth: Kuhn, born in 1922, attended an elite private school in Connecticut and then studied at Harvard for both his undergraduate and his doctoral degrees in physics. His academic career opened with a prize position at the Harvard Society of Fellows, after which he taught at Harvard, Berkeley, Princeton, and MIT. There was no pick swinging or cabinetmaking; he never taught abused youth—except, if the filmmaker Errol Morris is to be believed, his own graduate students. (Morris recalls that Kuhn, a chain-smoker of prodigious capacity, once attempted to refute an objection Morris posed by flinging a loaded ashtray at his head.)

In spite of his early advantages and successes, Kuhn was, he tells us, "a neurotic, insecure young man." He entered psychoanalysis while in graduate school in the 1940s. While he found its therapeutic value to be doubtful, he credited it with enhancing his own interpretive powers to the point that he "could read texts, get inside the heads of the people who wrote them, better than anybody else in the world."

This new ability soon manifested itself in a way that suggested to Kuhn the ideas that would make him famous. Puzzling over Aristotle's theory of physics, which "seemed to me full of egregious errors," Kuhn looked out the window and had an epiphany:

> Suddenly the fragments in my head sorted themselves out in a new way, and fell into place together. My jaw dropped, for all at once Aristotle seemed a very good physicist indeed, but of a sort I'd never dreamed possible. Now I could understand why he had said what he'd said.

Kuhn did not, of course, come to believe Aristotle's physical theory, but he did come to see it as a system that, by its own lights, constituted a coherent and powerful explanatory framework. To appreciate its cogency, however, he had to set aside his habitual ways of thinking about the world, conditioned by twentieth-century physics, and to adopt temporarily a wholly new worldview. From this experience he learned that some revisions of scientific theory are so profound that they require a complete overturning of the cognitive order—a revolution.

Kuhn's famous book *The Structure of Scientific Revolutions* was published in 1962, 15 years after his epiphany and just 3 years after Popper's own great work on the scientific method first appeared in English. Nothing before or since has had a comparable impact on the philosophy of science; nothing has so altered the course of the Great Method Debate. A book on revolutions that took the '60s by storm? You might suppose that Kuhn's picture of science was a model of intellectual ferment, radical thinking, inspired resistance to the choke hold of tradition. Not so. Science is capable of world-altering progress only because, according to Kuhn, scientists are quite incapable of questioning intellectual authority.

Any branch of science—microeconomics, nuclear physics, genetics—has at all times, says Kuhn, a single dominant ideological mind-set, something he calls a *paradigm*. The paradigm is built around a high-level theory about the way the world works, such as Newton's theory of gravitation or Mendel's laws of genetics, but it contains much more as well: it identifies, in the light of the theory, what problems are important, which methods are valid ways to go about solving the important problems, and what criteria determine that a solution to a problem is legitimate.

A paradigm functions, then, as a more or less complete set of rules and proper behaviors for doing science within a discipline. Scientists obey these rules religiously. To invoke blind devotion is not a metaphor: scientists don't follow the paradigm because they believe it is well supported by the evidence, or because it is the "official" way to do things, or because it is especially well funded, or because it seems like it might be worth a shot; rather, they follow it because they cannot imagine doing science any other way. Were they presented with an alternative paradigm, Kuhn argues, they would find it incomprehensible.

To explain this mental block, Kuhn appealed to experiments in perception conducted by the psychologist Jerome Bruner and others, in which subjects are briefly shown (for example) "trick" playing cards, such as a six of spades printed with red rather than the standard black ink. The subjects report experiencing what their prior beliefs would lead them to expect, rather than what is actually on the card; they might see a black six of spades when what's sitting in front of their eyes is manifestly red, or they might misread the card as a six of hearts. Even the direct evidence of the senses, Bruner concluded, is swayed by our beliefs about what's out there. That's possible, according to Bruner, because our raw experience of things is ambiguous, like the drawing in Figure 1.2. Is it a duck or a rabbit? Apparently a duck . . . but rotate the image a quarter turn clockwise, and it is a rabbit that stares unblinking out of the page. It is our preexisting assumptions, our theories, our prevailing worldview that disambiguate what's supplied by the senses, thereby presenting us with a determinate mental picture of the world.

Scientists, like anyone else, see and understand things at any one time from within a particular worldview. That may sound innocent enough, but it shuts down scientists' capacity to comprehend genuine novelty. To grasp a new worldview, you would need to appreciate it from the perspective of some worldview or other. You can't appreciate it from the new worldview's perspective (that is, its own perspective), because you haven't yet grasped that framework. But if the old worldview is incompatible with the new, then you can't see the new view from the perspective of the old view either. The new view is simply out of sight.

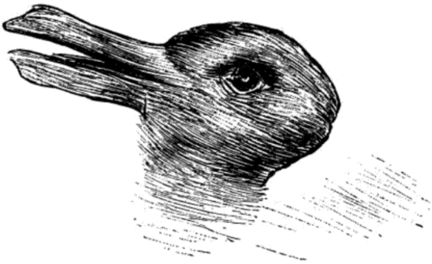


Figure 1.2. Duck or rabbit?

The contrast with Popper is stark. For Popper, what matters above all else to the successful operation of the knowledge machine is scientists' acute faculty for critical thought. They can survey the theoretical possibilities, and they see clearly how each theory might, in the face of the evidence, collapse. For Kuhn, such a survey, the essential precondition for criticism, is psychologically impossible.

In supposing that scientists could not simultaneously contemplate rival grand theories, Kuhn was putting enormous conceptual weight on a few empirical findings and philosophical arguments, no doubt inspired by his own experience with Aristotle's physics. He was moving with the zeitgeist, however, and his readers, or enough of them, went along with it. When Kuhn's book appeared in 1962, it was still the age of the military-industrial complex, the man in the gray flannel suit, and William Whyte's "organization man"—a complacent and compliant figure eager to fit into the system and carry out whatever plan was handed down from above.

Figure 1.3. Organization men.

The prevailing paradigm's staffers cannot conceive of any other way to do science. And yet, Kuhn observes, a paradigm is not forever. Existing ideas crumble during events that historians call scientific revolutions, intellectual cataclysms during which a new paradigm replaces the old. (A lowercase scientific revolution should not be confused with the uppercase Scientific Revolution, of which there has been only one. In a lowercase scientific revolution, one way of doing science is replaced by another. In the uppercase Scientific Revolution, something that was not science—natural philosophy, I have called it—was replaced by a far more effective form of empirical inquiry, modern science itself.)

Before Kuhn wrote about scientific paradigms, he wrote a history of the sixteenth-and early seventeenth-century Copernican revolution, arguably the first scientific revolution of all. The old regime, overthrown by the revolution, was the ancient Greek system of astronomy, perfected in the work of the Greco-Egyptian mathematician Ptolemy, according to whom the sun, the moon, the stars, and all the planets orbit the earth. The revolutionary new idea was Copernicus's system, published in 1543, in which the moon orbits the earth and everything else, including the earth, orbits the sun. As developed by Johannes Kepler in the early 1600s, it predicted the paths of the celestial bodies more accurately and more elegantly than Ptolemy's theory.

A shift to the Copernican system, in spite of its predictive superiority, was at the very least troubling. It meant taking on board the rather deflating realization that the earth is not, after all, the center of the universe—though a certain grim satisfaction could perhaps be had from the accompanying realization that there is no distinction between the corrupt earth and the supposedly perfect, symmetrical, unblemished heavens, that every celestial body is equally rough-hewn, dog-eared, moth-eaten, coarse.

A less soulful and more visceral drawback of Copernicanism was its implication that the earth is moving very fast—rotating every 24 hours and racing around the sun in 365 days (at a speed, we now know, of about 66,600 miles per hour). How could we not have noticed? The answer lay in a second and parallel revolution in physics that accompanied the revolution in astronomy. The radical new physical idea was that a person or thing moving at an approximately constant velocity, like the seas and trees and people on the surface of the earth, will not experience the speed at all; however fast they're moving, they will feel as though they're standing still.

It was not easy for human minds to let go of the centrality of the earth, the perfection of the heavens, and the palpability of speed. This intellectual stasis Kuhn put down to the paradigm's stifling embrace. Copernicus triumphed all the same. And from then on, paradigms continued to topple. Newton's theory of gravity replaced Aristotle's story, according to which rocks fall to earth because they are seeking their proper place at the center of the universe, along with various notions of the medieval philosophers. In the nineteenth century, Darwin's theory of evolution by natural selection replaced the theory of special creation, according to which every species was created separately by God. And shortly after the beginning of the twentieth century, Newton's physics was replaced in turn by Einstein's theory of relativity and by quantum physics.

How does this happen? How do paradigms end? A scientist working within a paradigm is not seeking to undermine it. On the contrary, according to Kuhn, they have no inkling that it can be undermined, or at least they don't regard its being overthrown as a serious possibility: "Normal science . . . is predicated on the assumption that the scientific community knows what the world is like. . . . [It] does not aim at novelties of fact or theory and, when successful, finds none." But scientists' very commitment to the paradigm can push it to the point of destruction: they abide by its prescriptions, they faithfully execute its plan, yet they run into insoluble problems because the paradigm is inadequate in some way. From on high, the paradigm guarantees that a certain method will result in answers; following the method, however, leads increasingly to questions, problems, inconsistencies, perplexities. Planets stray from their assigned paths; fossils are unearthed suggesting that human ancestors bore a startling resemblance to apes; light itself can't decide whether to act as a particle or as a wave. The result is what Kuhn calls crisis, a progressive decline of researchers' faith in the paradigm's power.

Without faith, a Kuhnian scientist is lost. The only recipe they have for doing science is the one prescribed by the paradigm that looks to have deserted them. Their enthusiasm for the old system of belief is gone, but if they are to be a scientist at all, they must follow its rituals nevertheless.

There things might hang for decades or longer. Eventually, however, some visionary "deeply immersed in crisis" is able to shrug off the pull of the old ideas; a new way of doing things comes to them "all at once, sometimes in the middle of the night." The prevailing paradigm has competition at last. Given its inadequacies, scientists ought to grasp hungrily at any promising alternative. So they would, perhaps, if they knew what they were grasping for. On Kuhn's understanding of the scientific mind-set, however, it is impossible for an adherent of one paradigm to appreciate or even to understand the significance of another. (Kuhn writes that the creators of new paradigms escape the pull of the old because they are "either very young or very new to the field"; their minds have yet to set.)

Here is the predicament, then, of scientists who grew up with the old paradigm—such as adherents of Ptolemy when the Copernican revolution crested in the seventeenth century, or of Newton as Einstein precipitated the twentieth-century revolution in gravitational theory. They know that something has gone badly wrong. Their paradigm has ceased to bestow scientific blessings. Weariness and confusion have taken hold. At the same time, they know there is a new paradigm. They don't themselves understand it, but they see that its followers have all the enthusiasm and joy in discovery that has trickled away from their own intellectual lives. What to do?

Some adherents of the old paradigm will die disillusioned. Some will fight theoretical novelty to the end. But some, the apostates, will undertake to abandon the old theory and to make a move to the new. They will set out to live among its followers or, if that is impossible, to immerse themselves in the new paradigm's canonical writings. Eventually, if conditions in the minds of these apostates are right, the new doctrines will come to supplant the old. The scientist will have undergone what Kuhn calls a "conversion experience."

If the new paradigm is sufficiently fruitful, and its followers dedicated enough in their scientific missionary work, almost every remaining adherent of the old paradigm will, feeling their life's former foundation sinking under their feet, throw themselves into the new way of doing things, the new theory. A scientific revolution will have occurred.

Kuhn scandalized the world of science with this picture of revolutionary scientific change. Previous historians and philosophers had seen scientific change as a largely rational process: the ideas of Copernicus, of Kepler, of Galileo, of Newton, however radical, were accepted because they were so clearly superior to the old ideas, both in their predictive successes and in their explanatory beauty.

If Kuhn is right, then this older, more dignified conception of scientific progress must be wrong, for in Kuhn's view, it is impossible to compare paradigms: "When paradigms enter, as they must, into a debate about paradigm choice, their role is necessarily circular." Perhaps if you had two brains as you have two hands, you could weigh one paradigm against another. But you have only a single brain, and a single brain is capable of grasping only a single paradigm. You cannot simultaneously appreciate the merits of the Aristotelian and the Newtonian worldviews any more than you can simultaneously be a fervent Muslim and a devoted Roman Catholic. At the height of his rhetoric, Kuhn wrote that the Aristotelian and the Newtonian live in different worlds; you can live in one world or the other, but you cannot be in two different places at the same time. A rational comparison of competing paradigms is therefore humanly impossible.

In the place of logical evaluation, Kuhn posits a leap of faith: a giddy jump through ideologically empty space from the traditional view of things to the revolutionary way of thinking, undertaken in the hope that life will somehow be better under a new scientific sign.

You might imagine what Popper, quitting the Old World with open-eyed defiance, would say about this blind lunge into theoretical darkness, what he would think about Kuhn's contention that "as in political revolutions, so in paradigm choice—there is no standard higher than the assent of the relevant community." Popper's student Imre Lakatos, also a refugee from

European totalitarianism, accused Kuhn of making science a matter of “mob psychology.”

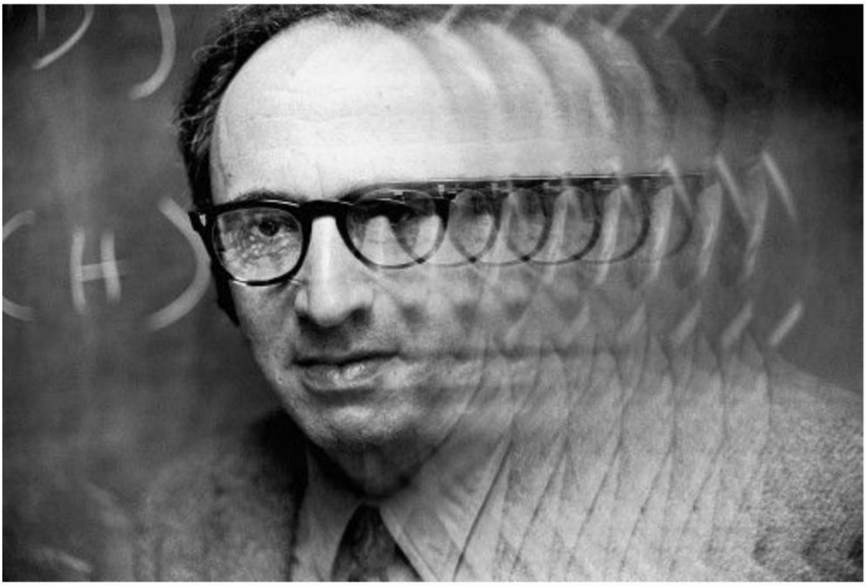


Figure 1.4. To grasp many paradigms takes many minds. Portrait of Thomas Kuhn by Bill Pierce for *Life* magazine.

Kuhn’s critics were sickened by the thought that the major transitions in scientific thinking were episodes of conversion rather than careful deliberation. But equally, they were puzzled by Kuhn’s faith that an arational process could have led us to the state of scientific sophistication we enjoy today. If it is impossible to compare objectively the merits of Ptolemaic and Copernican astronomy, how did we get the structure of the solar system right? How did we figure out that the earth does indeed go around the sun, rather than vice versa?

Some of Kuhn’s radical followers insinuated that we believe our paradigm is a great improvement over earlier ideas for the same reason that we believe our religion is true or our child is beautiful—not because the empirical evidence says so, but because it is *ours*. Kuhn himself, at least in his later writings, repudiated this view and argued for real progress in science. The Copernican paradigm genuinely is better, objectively, than the Ptolemaic paradigm, he held, because it has superior “puzzle-solving ability." One kind of puzzle is the problem of predicting the future; the theories of Copernicus and Ptolemy both aspire, for example, to forecast the paths of the planets across the night sky. A part of what Kuhn is saying, then, is that later paradigms tend to have more predictive power than earlier paradigms. This growth in predictive power, and not something more parochial, is what accounts for our sense that scientific knowledge is seeing ever more deeply into the nature of things, along with our ability to perform ever more impressive feats with that knowledge—to talk across continents, to fly around the globe, to walk on the moon.

The later Kuhn believed, then, that when scientists make the jump from an old to a new paradigm, they tend to jump from a less to a more predictive paradigm, though they are incapable, as they launch themselves, of appreciating the underlying reasons for the new paradigm's superior future-predicting potential. This restores a pinch of rationality to scientific proceedings: Kuhn's revolutionaries are making covert cost-benefit calculations even as they surge through the streets, subtly targeting their leaps of faith in the general direction of predictive and other kinds of puzzle-solving power.

THE KUHNIAN SCIENTIST IS, when not in revolt, a pedestrian character, dull and deferential. But science itself, Kuhn believed, is supreme among belief systems in its ability to create new knowledge. It is far from the only thought system capable of generating novel and original ideas—philosophy, for example, is its equal in this respect. What is unparalleled is its ability to test those ideas thoroughly, to drive them to their logical or illogical conclusions. Central to science's extraordinary rigor is precisely the limitedness of the individual scientist, their inability to see outside the prevailing paradigm. This intellectual blindness is, then, the core of Kuhn's answer to my big philosophical question about science, the question of what arrived in the Scientific Revolution that made scientific inquiry so much more fruitful than the natural philosophy that had come before.

That science's success is explained by a kind of intellectual confinement—that is the single most astonishing thesis in Kuhn's celebrated book. It is easy to see how the characteristic intellectual demeanor of the Popperian scientist—unbounded imaginer, unrelenting refuter—might sustain the extraordinary productivity of the knowledge machine. But Kuhn's scientists? How could their inability to contemplate or even comprehend new ideas possibly drive discovery?

Science is boring. Science is frustrating. Or at least, that is true 99 percent of the time. Readers of popular science see the 1 percent: the intriguing phenomena, the provocative theories, the dramatic experimental refutations or verifications. Behind these achievements, however—as every working scientist knows—are long hours, days, months of tedious laboratory labor. The single greatest obstacle to successful science is the difficulty of persuading brilliant minds to give up the intellectual pleasures of continual speculation and debate, theorizing and arguing, and to turn instead to a life consisting almost entirely of the production of experimental data.

Many important scientific studies have required of their practitioners a degree of single-mindedness that is quite inhuman. Through the 1960s, the rival endocrinologists Roger Guillemin and Andrew Schally fought to be the first to find the structure of the hormone TRH, a substance used by the hypothalamus, a small but crucial structure at the base of the brain, to set off a chain of signals controlling processes ranging from daily metabolism to early brain development. The full significance of TRH is not yet understood, but some sense of its importance and power can be discerned from the US Army's commissioning, in 2012, of a study to examine its possible use in a nasal spray to quell suicidal urges.

Guillemin and Schally finished in a dead heat, sharing the 1977 Nobel Prize in Physiology or Medicine for their discovery of TRH's molecular makeup. It had been not so much a race as an epic slog. Literally tons of brain tissue, obtained from sheep or pigs, had to be mashed up and processed to obtain just 1 milligram of TRH for analysis. Several rivals dropped out of the competition, unable to countenance the "immense amount of hard, dull, costly, and repetitive work" required. As Schally later explained:

> Nobody before had to process millions of hypothalami. . . . The key factor is not the money, it's the will . . . the brutal force of putting in 60 hours a week for a year to get one million fragments.

Still, the investigation of TRH was over in a flash compared with the Gravity Probe B experiment at Stanford University, which undertook to launch a satellite into orbit around the earth that would measure the "geodetic" and "frame-dragging" effects implied by Einstein's general theory of relativity. The project was initiated in 1964 and made its final report to NASA—after overcoming extraordinary setbacks and technical problems and creating, as components of its gyroscopes, the most perfectly spherical objects ever fashioned by human hands—in 2008 (Figure 1.5). The director of the project, Francis Everitt, stuck with it for all four-plus decades, 74 years old when he signed that report.

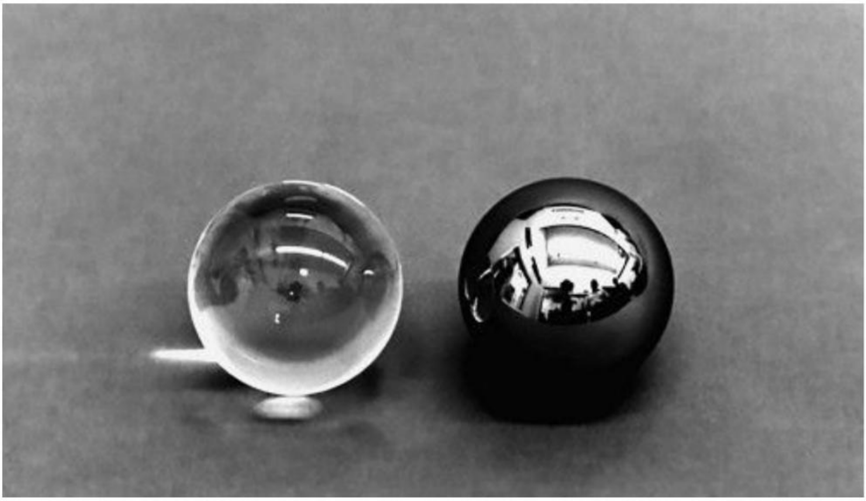


Figure 1.5. The rotors for the gyroscopes in the Gravity Probe B experiment—"the roundest objects ever manufactured." They are 1.5 inches across.

In another 40-year epic, the evolutionary biologists Peter and Rosemary Grant have since 1973 spent their summers on the tiny Galápagos island of Daphne Major, observing, trapping, numbering, and measuring finches in order to demonstrate "evolution in action" as body and beak size adapt over generations to drought, flood, and other environmental changes (Figure 1.6). In 1981, they began to track, in particular, a finch that was larger and had a different song than any known variety. Thirty-one years later, having followed that finch's offspring bird by bird for six generations, they had enough data to conclude that they had observed the origin and establishment of a new species.

A longitudinal study in economics or medicine can likewise involve decades of data collection: the Dunedin Multidisciplinary Health and Development Study has been monitoring a thousand New Zealanders since the early 1970s and will continue into the 2020s.

Figure 1.6. The Galápagos island of Daphne Major is neither large nor hospitable. It is less than half a mile across.

Such Herculean efforts would perhaps be worth countenancing if the data generated by the experiments were guaranteed to result in major theoretical revelations. But as Kuhn noted, the relevance of experimental inquiry frequently hinges on the validity of the paradigm: if there is something conceptually or factually wrong with the method, the results may be of negligible importance.

To detect the frame-dragging effect, the Gravity Probe B apparatus needed to find a rotational change in its gyroscopes on the order of one hundred-thousandth of a degree per year, that is, a change that would take 36 million years to turn the gyroscope rotor in a full circle. Such a microscopic movement could have scientific significance only given a raft of specific physical assumptions. Were any of those assumptions false, the probe's painstakingly precise, excruciatingly expensive measurements would be worthless.

By the time the Dunedin study in New Zealand concludes, plenty more will have been learned about human health from other sources. The project labors, then, in the shadow of the possibility that information about some previously unknown crucial variable is being inadvertently neglected or that some variable thought to be crucial is unimportant—as was the case in the first longitudinal study ever conducted, Lewis Terman's decades-long "Genetic Studies of Genius," which assumed a tight correlation between IQ and genius that decades later turned out not to exist. And the Grants' meticulous finch counting might not have uncovered any particularly interesting patterns of population change, let alone the appearance of a new species; their hard labor and privation would in that case have been for the sake of nothing much at all.

The same is true for scientific investigation on a more modest scale. In a typical physics experiment, it may take years simply to get the apparatus to function properly; in cognitive psychology or the life sciences, it may take years to run pilot studies and to rehearse experimental designs seeking something that will deliver a significant outcome.

The geochemist and biologist Hope Jahren tells of a summer she spent in Colorado monitoring the flowering of a group of hackberry trees. Her aim, part of her PhD research at Berkeley, was to determine the effect of temperature and water chemistry on the composition of the hackberries' fruit. The trees never bloomed; there was no fruit. Jahren's summer was wasted. She asked a phlegmatic local why there were no flowers. The answer? "It just happens sometimes." So she got in her car and drove back to California.

Even when the machinery is running smoothly and the statistics flow plentifully, the results characteristically concern some abstruse matter—the structure of a plant's seed case; the time taken to react to a contrived visual stimulus; the pattern of bright and dark created by intersecting beams of light—whose value rests entirely on the significance it accrues within a larger theoretical framework. What if that framework is mistaken? Years of work, years of life, wasted on the minute inspection of inconsequential trivia.

Science has, as a consequence, a problem of motivation. It is not the problem of motivating students to become scientists; that they might do for many reasons, not least the thrill of discovery. Nor is it the problem of motivating scientists to turn up to the lab each day—they get paid for that—or to observe, measure, experiment when they get there, since that is a standard part of the job description. It is the problem of motivating the extraordinary intensity and long-term commitment with which empirical testing must be carried out in order to do the most valuable science.

How to persuade scientists to pursue a single experiment relentlessly, to the last measurable digit, when that digit might be quite meaningless? "You have to believe that whatever you're working on right now is *the* solution to give you the energy and passion you need to work," says the MIT physicist Seth Lloyd. Or as Andrew Schally wrote about his search for the structure of TRH and other molecules:

> Only a person such as myself with strong faith in the presence of these materials would have the patience to go through the many fastidious steps of the isolation procedure.

That is the Kuhnian answer to the motivation problem: mold scientists' minds so that they fail to see that their research might be based on an error, on a false presupposition. If the validity of the paradigm is accepted without question, then the value of long and arduous empirical toil is also beyond question. The purpose of narrowing scientists' horizons is to encourage them to work harder, to dig deeper, to go further than they would go if they could see their destination in perspective, if they had an accurate sense of their project's proportion.

Ultimately, it is only because scientists' faith in the paradigm guarantees the importance of their research that they feel secure enough to work the paradigm to death—to experiment in such detail, with such precision, as to expose the paradigm's shortcomings, to drive science to a crisis, and so to establish the preconditions for revolution. This is Kuhn's marvelous paradox: *A paradigm can change only because the scientists working within it cannot imagine it changing*. It is their certainty of its success that secures its destruction.

POPPER AND KUHN, though different in so many ways, were equally right about some exceptionally important things.

First, they were correct in thinking that what is special about science—what distinguishes scientific thought from the philosophical thought that preceded it—is not so much the capacity to generate new theories as the capacity to eliminate old theories, removing them permanently from humankind's running list of viable options. In either philosopher's story, science's success is due to the unbending search for and pitiless exploitation of even the most minute discrepancies between theory and evidence.

Second, Popper and Kuhn were right in thinking that in order to explain science's critical power, proprietary forms of motivation are at least as important as proprietary logical tools. The tools tell you what to do with the evidence, but that is of no use unless you have the right kind of data, and plenty of it. The production of such data requires, in most cases, an intense and prolonged focus on details of little intrinsic interest. Scientific inquiry needs something, then, to induce thinkers to devote their lives to an enterprise that is in its daily routine mundane and largely negative—while discouraging them from the glamorous alternative, the philosophical strategy of inventing new ideas and new styles of thought at every turn.

Popper finds his motivation in the immense appetite for refutation shared by every good scientist. Kuhn's motivator is more subtle and a little sinister. Individual Kuhnian scientists are not critics at all; they accept the prevailing paradigm with barely a contrary thought. But in their enthusiasm to squeeze every last drop of predictive power from that paradigm, they crush the life out of it.

Science's empirical implacability is, for both Popper and Kuhn, possible only because scientists adhere scrupulously to a method. For Popper that method is universal, fixed for all time—falsification is *the* scientific method. For Kuhn, the method is prescribed by the paradigm, and so it changes whenever scientific revolutionaries impose a new recipe for doing research. The beauty of the Kuhnian story is that it doesn't much matter what the recipe is, provided that it is sensitive to puzzle-solving power, and in particular, to predictive power. Even as the method itself mutates, the fact that science is method-bound, paradigm governed, endows it with its falsifying power. Kuhn is therefore, like Popper, what I have called a "methodist": a believer in the importance of scientists' dutifully following a set formula for pursuing their theoretical inquiries.

The method matters because it exposes predictive deficiencies, but also because it gives scientists the confidence to press on with their experimental lives. Popperian scientists know that since the logic of falsification is indisputable, their colleagues will attribute the same significance to their experimental labors that they do themselves. Kuhnian scientists have the same expectation because they know that their colleagues subscribe to a single set of rules inherent in the governing paradigm. It is not enough that the rules make sense, then. They must be widely agreed to make sense. On this matter, too, I think that Popper and Kuhn are correct.

The Knowledge Machine will build its own explanation of science's success on these insights, these contributions by Popper and Kuhn to the Great Method Debate. But I must first explain why modern theorists of science almost universally reject both thinkers' ideas.

Popper's and Kuhn's theories are not merely philosophical; they make claims about the actual organization of science and about the way the organization changes over time. To assess the theories, then, it makes sense to turn to specialists in these matters, namely, sociologists and historians of science.

Does contemporary science display the paradigmatic structure described by Kuhn, in which a single ideology and methodology guides all scientists working in any given domain? Ask the sociologists. Was there a sudden and unprecedented onset of paradigm-governed groupthink in the Scientific Revolution? Ask the historians. Do scientists fight to preserve the status quo, as Kuhn's theory would tend to suggest, or to overthrow it, as Popper would have it? For contemporary scientists, ask the sociologists; for the scientists of yore, the historians.

Over the past few decades, the answers have come in. They are almost entirely negative. There is little evidence, as you will see, for a dispassionate Popperian critical spirit, but also little evidence for universal subservience to a paradigm. Indeed, in their thinking about the connection between theory and data, scientists seem scarcely to follow any rules at all.

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" 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.

Figure 2.1. Another cloudy day on Príncipe.

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 photographs. 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.

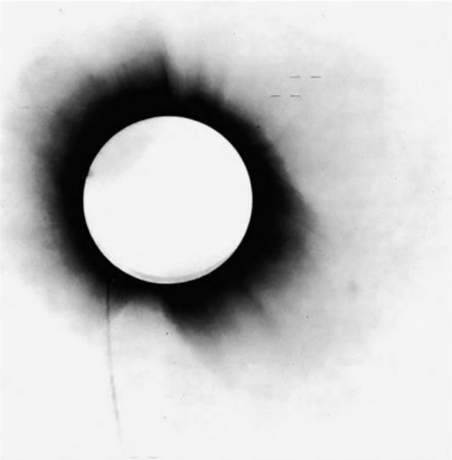


Figure 2.2. A photographic plate from Eddington's 1919 eclipse expedition. It is a negative: the eclipsed sun is the big white circle, its bright corona the dark flare surrounding the circle, and the surrounding stars are tiny black dots. Some of the crucial stars' positions are indicated by faint horizontal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

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.

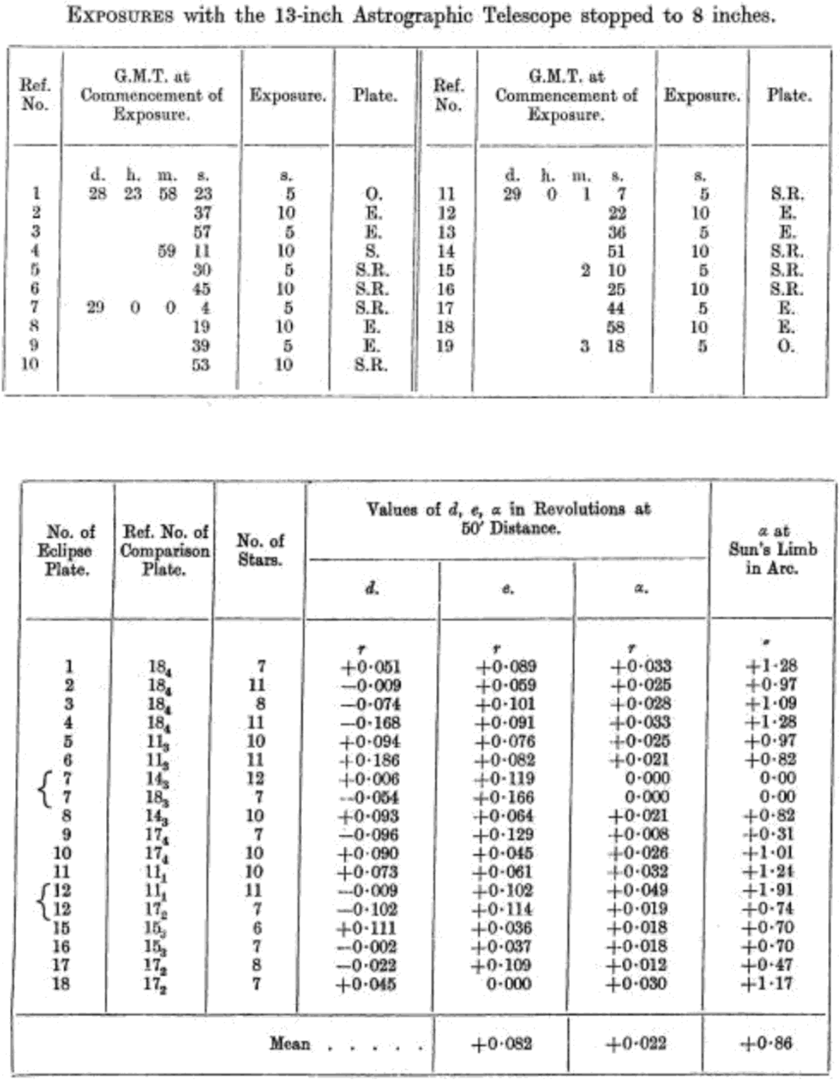
The 1919 eclipse is only a single example of the selective use of evidence. But the centuries since the Scientific Revolution are strewn with cases in which science's biggest names can be seen discarding or distorting difficult data so as to create the impression that experiment was in perfect harmony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



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.

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 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 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.

INDEX

Page numbers listed correspond to the print edition of this book. You can use your device's search function to locate particular terms in the text.

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