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

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

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."

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.

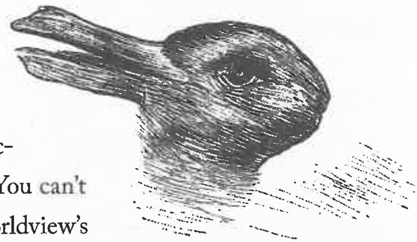

Figure 1.2. Duck or rabbit?

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

Figure 1.3. Organization men.

eager to fit into the system and carry out whatever plan was handed down from above.

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 mindset, 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

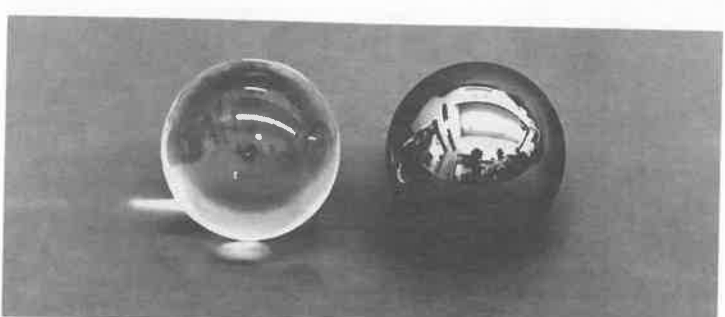

Figure 1.5. The rotors for the gyroscopes in the Gravity Probe B experiment—"the roundest objects ever manufactured." They are 1.5 inches across.

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.

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.