The Structure of Scientific Revolutions at Fifty

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Fifty years ago, Thomas Kuhn, then a professor at the University of California, Berkeley, released a thin volume entitled *The Structure of Scientific Revolutions.* Kuhn challenged the traditional view of science as an accumulation of objective facts toward an ever more truthful understanding of nature. Instead, he argued, what scientists discover depends to a large extent on the sorts of questions they ask, which in turn depend in part on scientists' philosophical commitments. Sometimes, the dominant scientific way of looking at the world becomes obviously riddled with problems; this can provoke radical and irreversible scientific revolutions that Kuhn dubbed "paradigm shifts"—introducing a term that has been much used and abused. Paradigm shifts interrupt the linear progression of knowledge by changing how scientists view the world, the questions they ask of it, and the tools they use to understand it. Since scientists' worldview after a paradigm shift is so radically different from the one that came before, the two cannot be compared according to a mutual conception of reality. Kuhn concluded that the path of science through these revolutions is not necessarily toward truth but merely away from previous error.

The Structure of Scientific Revolutions
(Fiftieth Anniversary Edition)
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Kuhn's thesis has been hotly debated among historians and philosophers of science since it first appeared. The book and its disparate interpretations have given rise to ongoing disagreements over the nature of science, the possibility of progress, and the availability of truth. For some, Kuhn was a relativist, a prophet of postmodernism who considered truth a social construct built on the outlook of a community at a specific point in history. For others, Kuhn was an authoritarian whose work legitimized science as an elitist power structure. Still others considered him neither a relativist nor an authoritarian, but simply misunderstood. Kuhn's work was ultimately an examination of the borders between the scientific and the metaphysical, and between the scientific community and society at large. As he discovered, these boundaries are not always clear. It behooves us to bear this in mind as we take the occasion of the fiftieth anniversary to revisit his book and the controversies surrounding it.

Thomas Samuel Kuhn was born in Cincinnati in 1922. He attended Harvard—where his father, a hydraulic engineer, had also studied—and earned a bachelor's degree in physics in 1943. After graduating, he became a junior researcher on radar, first at Harvard and then in Europe at the U.S. Office of Scientific Research and Development (OSRD). It was in these jobs that he became close with James B. Conant, who served as both president of Harvard and the head of OSRD. After the war, Kuhn returned to academic life at Harvard, receiving a Ph.D. in physics in 1949, and continuing on to teach the history of science. But the Harvard faculty denied him tenure in 1956, after which he left for Berkeley, where he was eventually made a full professor of the history of science in 1961. He never returned to physics professionally. By 1964, he had made his way to Princeton, and ended his career at M.I.T. as a professor of philosophy, where he retired in 1991. But it was at Berkeley, in 1962, that Kuhn published the work that was to mark his career, and the course of inquiry in the philosophy of science, from that point on: *The Structure of Scientific Revolutions.*

The earliest seeds that would grow into Kuhn's famous book were planted when he was a doctoral student in 1947. Conant tasked Kuhn with giving a series of lectures on seventeenth-century theories of mechanics. It was during the preparation of these lectures that Kuhn first began to develop his ideas. He sought to grasp exactly why Newton had discovered the laws of motion, and why it had taken mankind so long to do that, considering that Aristotle's theories about motion had been so manifestly wrong. Moreover, Kuhn was confused about why Aristotle had been so wrong, when he had gotten much of biology and social science so right.

One summer day, it occurred to Kuhn rather suddenly that Aristotle had been operating from within a completely different framework of physics than the modern understanding. For Aristotle, the growing of a child into an adult was a similar process to that of a rock falling to the ground: each is moving toward its natural end, the place and state where it belongs. Contrary to Newtonian physics, Kuhn later explained in the preface to his 1977 collection *The Essential Tension*, "position itself was…a quality in Aristotle's physics, and a body that changed its position therefore remained the same body only in the problematic sense that the child is the individual it becomes. In a universe where qualities were primary, motion was necessarily a change-of-state rather than a state." This idea germinated in Kuhn's mind as he continued his doctoral work, and later formed part of the basis for *The Structure of Scientific Revolutions.*

The argument of *Structure* is not especially complicated. Kuhn held that the historical process of science is divided into three stages: a "normal" stage, followed by "crisis" and then "revolutionary" stages. The normal stage is characterized by a strong agreement among scientists on what is and is not scientific practice. In this stage, scientists largely agree on what are the questions that need answers. Indeed, only problems that are recognized as potentially having solutions are considered scientific. So it is in the normal stage that we see science progress not toward better questions but better answers. The beginning of this period is usually marked by a solution that serves as an example, a paradigm, for further research. (This is just one of many ways in which Kuhn uses the word "paradigm" in *Structure.*)

A crisis occurs when an existing theory involves so many unsolved puzzles, or "anomalies," that its explanatory ability becomes questionable. Scientists begin to consider entirely new ways of examining the data, and there is a lack of consensus on which questions are important scientifically. Problems that had previously been left to other, non-scientific fields may now come into view as potentially scientific.

Eventually, a new exemplary solution emerges. This new solution will be "incommensurable"—another key term in Kuhn's thesis—with the former paradigm, meaning not only that the two paradigms are mutually conflicting, but that they are asking different questions, and to some extent speaking different scientific languages. Such a revolution inaugurates a new period of normal science. Thus normal science can be understood as a period of "puzzle-solving" or "mopping-up" after the discovery or elucidation of a paradigm-shifting theory. The theory is applied in different contexts, using different variables, to fully flesh out its implications. But since every paradigm has its flaws, progress in normal science is always toward the point of another crisis.

Kuhn relies heavily on a "particularly famous case of paradigm change": the sixteenth- and seventeenth-century debate over whether the sun goes around the earth or the earth around the sun. (This had been the subject of Kuhn's previous book, *The Copernican Revolution* [1957].) Before Copernicus, Ptolemy conceived of a universe with the earth at its center. The celestial spheres wrapped around the earth like the layers of an onion, although how exactly they rested on each other so smoothly—the theory was that their natural motion in the ether was rotation—remained unknown. Ptolemy and his followers saw that the stars, the planets, the moon, and the sun all appeared to revolve in one direction around the earth in a regular order, and the exceptions—like the occasions when some planets seemed to move backwards in the sky—could be explained away. For over a thousand years, this was the dominant European conception of the universe. The model worked well for most of the questions that were asked of it; it could be used to predict future celestial movements, and as a practical matter, there was little reason to doubt it. In this "normal" stage of science, the mopping-up process was one of refining the data for more accurate predictions in the future.

But there will always be facts and circumstances any given theory cannot explain. "By the early sixteenth century," Kuhn writes in *Structure*, "an increasing number of Europe's best astronomers were recognizing that the astronomical paradigm was failing in application to its own traditional problems"—not to mention outside pressures related to calendar reform and growing medieval criticism of Aristotle. As the unexplainables began to mount, the Ptolemaic paradigm moved into a state of crisis. The Copernican Revolution was the result—a new theoretical framework that could incorporate the contradictory data into a coherent structure by putting the sun at the center of the cosmos. In Kuhn's view, Copernicus and Galileo were on the tail end of the mopping-up era of Ptolemaic astronomy; Copernicus was not intentionally overthrowing the existing model, but the way he interpreted the data was simply inconsistent with an earth-centered universe. In spite of subsequent efforts by others, such as Tycho Brahe, to synthesize the two theories, they were incompatible.

If a paradigm is "destined to win its fight, the number and strength of the persuasive arguments in its favor will increase." After a new theory is established, it attracts new supporters, often including younger scientists and perhaps the originating theorist's students. Meanwhile, Kuhn writes, "those unwilling or unable to accommodate their work" to the new theory "have often simply stayed in the departments of philosophy from which so many of the special sciences have been spawned." Older scientists have trouble adjusting to the new paradigm, in part because it puts their own work in doubt. Eventually, they are ignored. Kuhn quotes Max Planck, who famously wrote that "a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it."

Over time, there again comes to be almost unanimous agreement on the validity of the predominant theory—it achieves paradigmatic status. Scientists tacitly assume agreement on the meanings of technical terms, and develop a shared and specialized technical vocabulary to facilitate data accumulation and organization. They establish journals dedicated to their scientific field, begin to cross-reference one another, and scrutinize each other's work according to whether or not it conforms to the theory. Their students, likewise, learn to approach problems in the same way they do, much as an apprentice learns from a master. Normal science has resumed and the cycle begins anew.

It was important for Kuhn that his conception of the history and *process* of science was not the same as that of scientific *progress.* He maintained that the process of science was similar to biological evolution—not necessarily evolution toward anything, only away from previous error. In this way, Kuhn was rather skeptical about the idea of progress at all. This was the most controversial aspect of his thesis, the one that most concerned the contemporary critics of *Structure*, on the basis of which they accused—or celebrated—Kuhn as a champion of relativism. As University of Toronto philosophy professor Ian Hacking notes in an introductory essay prepended to the new fiftieth-anniversary edition of *Structure*, Kuhn's notion that science moves away from previous error

> seems to call in question the overarching notion of science as aiming at the truth about the universe. The thought that there is one and only one complete true account of everything is deep in the Western tradition.... In popular versions of Jewish, Christian, and Muslim cosmology, there is one true and complete account of everything, namely what God knows. (He knows about the death of the least sparrow.)
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> This image gets transposed to fundamental physics, many of whose practitioners, who might proudly proclaim themselves to be atheists, take for granted that there just is, waiting to be discovered, one full and complete account of nature. If you think that makes sense, then it offers itself as an ideal *towards* which the sciences are progressing. Hence Kuhn's progress *away from* will seem totally misguided.

For Kuhn, a paradigm shift is fundamentally not a scientific but a philosophical change, because the incommensurability of paradigms means that there is no external stance from which one can be shown to be superior to another. Kuhn explains, "The men who called Copernicus mad because he proclaimed that the earth moved…were not either just wrong or quite wrong. Part of what they meant by 'earth' was fixed position. Their earth, at least, could not be moved." To say that the heliocentric model is true and that the geocentric model is false is to ignore the fact that the two models mean quite different things by the term "earth."

But science has long been understood as a progressive accumulation of knowledge, not a mere shift from one worldview to another, like the gestalt shift between perceiving a duck or a rabbit in the famous diagram that Kuhn liked to use for illustration. And so *Structure* was received by many as a denial of the existence of absolute truth. If competing paradigms are both comprehensible, yet are incommensurable, can they not both be true? And if they are both true, who is to be the final arbiter of truth?

Many took Kuhn's thesis to be a reduction of science to power struggles between competing views. Kuhn himself rejected this interpretation—although his attempts to do so sometimes ended up lending support in form to what they rejected in words: The physicist Freeman Dyson recounts in his 2006 book *The Scientist as Rebel* that he once attended a conference at which Kuhn's disciples were repeating these exaggerated interpretations of his thesis, and "Kuhn interrupted them by shouting from the back of the hall with overwhelming volume, 'One thing you people need to understand: I am not a Kuhnian.'"

Structure had taken on a life of its own. As Kuhn stated in a 1991 interview with science journalist John Horgan, "For Christ's sake, if I had my choice of having written the book or not having written it, I would choose to have written it. But there have certainly been aspects involving considerable upset about the response to it." As Hacking notes, a number of critics argued that the first edition was terribly vague. One reviewer in 1966 criticized Kuhn for using the word "paradigm" in twenty-one different senses in the book. Hacking also notes the strikingly ambivalent language that Kuhn often employs, using phrases like "we may want to say" and "[this] may make us wish to say" instead of offering assertions outright, leaving him open to criticism that he was unclear or hedging his argument.

Kuhn was also criticized for building a wall between basic science (that is, science conducted for its own sake) and applied science (that is, science aimed at achieving specific, often socially important, goals). Against Bacon's dictum that the proper aim of science is "the relief of man's estate," Kuhn argued that scientists in the "normal" stage must ignore "socially important problems" and should instead just focus on solving puzzles within the paradigm. In other words, problems that must be solved to improve human life but cannot be solved by the methods of a given paradigm are a distraction from the work necessary during the "normal" phase of science. This suggests that scientists must cloister themselves, at least to an extent, in order to make progress within the confines of their paradigm. Moreover, as Steve Fuller, professor of sociology at the University of Warwick, notes in *Thomas Kuhn: A Philosophical History for Our Times* (2000), Kuhn felt that a paradigm should be "sheltered from relentless criticism in its early stages." So not only can a paradigm "insulate the community" of scientists from the demands of society, in Kuhn's words, but scientists must in turn insulate the paradigm from harsh criticism.

Kuhn was left having to do some "mopping up" of his own, which he attempted in the years after *Structure* was published. For example, in a 1973 lecture (collected in *The Essential Tension*), Kuhn sought to counter the charge that he was a relativist. He argued that some theories and paradigms are better than others, based on five rational criteria: accuracy, consistency, scope, simplicity, and fruitfulness. Much later, in the 1991 interview with Horgan, Kuhn insisted

> that he did not mean to be condescending by using terms such as "mopping up" or "puzzle-solving" to describe what most scientists do. "It was meant to be descriptive." He ruminated a bit. "Maybe I should have said more about the glories that result from puzzle solving, but I thought I was doing that."

Continuity in a paradigm is not necessarily a bad thing, Kuhn explained in his later years; indeed, it enables scientists to organize the greater and greater amounts of knowledge that grow through the cumulative process of scientific inquiry.

Criticisms aside, whether Kuhn even deserves full credit for the ideas put forth in his seminal work has rightly been questioned. As early as the mid-1940s, the Hungarian-British scientist-philosopher Michael Polanyi had published very similar ideas about the significance of scientists' personal commitments to a framework of beliefs and the role of learning by example in scientific training. As Kuhn later admitted, he became familiar with those works during his studies under Conant, and through a talk that Polanyi delivered and Kuhn attended in 1958. Polanyi's most extensive work on the subject, *Personal Knowledge*, was published the same year. In the early 1960s, Kuhn explicitly described his own thought as closely aligned with that of Polanyi, but he did not mention his name in *Structure*, except for a brief footnote in the first edition and an additional mention in the

1970 second edition. When Polanyi struggled to receive recognition for his thoughts independently of Kuhn's, Kuhn admitted in private correspondence that he might owe "a major debt" to the older scholar. But shortly before Kuhn's death (and long after Polanyi's), he revised those concessions and claimed that Polanyi had not in fact had a great influence on him, and that he had delayed reading *Personal Knowledge* until after finishing *Structure* out of a fear that he "would have to go back to first principles and start over again, and I wasn't going to do that."

Despite the fact that Polanyi's work preceded Kuhn's and was more philosophically rigorous, it was Kuhn whose book became a bestseller and whose terminology entered contemporary parlance. Steve Fuller notes "many Kuhn-like ideas were 'in the air' both before and during the time *Structure* was written," often from better-known philosophers. Perhaps Kuhn simply hit not only on the right ideas, but more importantly on the right distillation of them, and the right terminology, at the right time.

The reader of Kuhn's work is struck by his extensive focus on the physical sciences, and the dearth of attention to biology and the social sciences. To some extent, this is hardly surprising, given Kuhn's background as a theoretical physicist. But it is also true that the public prominence of the physical sciences in the first half of the twentieth century and the early periods of the Cold War provided a unique window into the community of scientists and the patterns by which scientific theory develops.

What Kuhn noticed was that competing paradigms in physics never coexist for very long, and that progress in normal science occurs precisely when scientists work within only one paradigm. But the social sciences are a special kind of science, because they cannot set aside fundamental philosophical concerns as easily as the physical sciences. Moreover, the social sciences are defined by multiple paradigms that are sometimes mutually contradictory. Kuhn pointed out that some social sciences may never be able to enter the paradigmatic stage of normal science for that reason. Unlike physical scientists, social scientists generally cannot in the face of a disagreement revert to an agreed-upon exemplary solution to a problem; their controversies are precisely about what the exemplar ought to be. The social sciences are grounded on competing views of what the world is and should be: certain basic concepts, such as "the state," "institutions," or "identity," cannot be defined by consensus. Competing paradigms—such as those of Marxist, Keynesian, and Hayekian economists—will continue to coexist. So there necessarily will be limits to what the social sciences can achieve, since the lack of unanimity inevitably means that arguments turn on questions of theory, rather than on the application of theory. In addition, since it is more difficult in the social sciences to carry out true experiments and test counterfactuals, the social sciences are inhibited from closely following the model of the physical sciences. And the passage of time is a relevant factor. As social scientist Wolfgang Streeck explains, "What has historically happened cannot be undone—which also means that there can never be an exact return to a past condition, as the memory of what happened in between will always be present. A military dictatorship that has returned after having overthrown a democracy is not the same as a military dictatorship following, say, a foreign occupation."

Despite these criticisms, many social scientists embraced—or perhaps appropriated—Kuhn's thesis. It enabled them to elevate the status of their work. The social sciences could never hope to meet the high standards of empirical experimentation and verifiability that the influential school of thought called positivism demanded of the sciences. But Kuhn proposed a different standard, by which science is actually defined by a shared commitment among scientists to a paradigm wherein they refine and apply their theories. Although Kuhn himself denied the social sciences the status of paradigmatic science because of their lack of consensus on a dominant paradigm, social scientists argued that his thesis could still apply to each of those competing paradigms individually. This allowed social scientists to claim that their work was scientific in much the way Kuhn described physics to be.

Disagreements over what counts as science, and how society can hold scientists in any field accountable to a standard of truth, became most heated in the aftermath of a debate between Kuhn and the philosopher Karl Popper. The now-famous debate between Kuhn and the older and far more seasoned Popper took place in London on July 13, 1965. Although no particularly significant exchange between the two took place either before or after this encounter, their disagreement is commonly featured in textbooks and college courses as a major event in the development of the philosophy of science in the twentieth century. The popular view of the conflict, advanced primarily by supporters of Kuhn—the supposed winner of the debate—is that Kuhn was a revolutionary in his field who championed free inquiry, in opposition to the strict empirical and logical standards of the positivists. Popper, on the other hand, is often taken to be a quasi-positivist defender of the authority of science. But, as Steve Fuller argues in his 2003 book *Kuhn vs. Popper: The Struggle for the Soul of Science*, this popular conception is not only a caricature but an inversion of the truth about these two thinkers.

Popper held science to a higher standard than did Kuhn. Popper's famous proposition was that a seemingly scientific claim, in order to be actually scientific, must be *falsifiable*, meaning that it is possible to devise an experiment under which the claim could be disproved. A classic example of a falsifiable science is Einsteinian physics, which made specific, well-defined predictions that could be tested through observation—as opposed to, say, Freudian psychology, which did not make well-defined predictions and proved adept at reformulating its explanations to fit observations, changing the details so as to salvage the theory.

By defining science in terms of rational criteria of empirical observation, Popper seemed to place scientific tools equally in the hands of philosophers of science, skeptics, and common persons who needed some means to question scientists who tried to back their claims by appealing to their own scientific authority. For Popper, novel scientific theories should be greeted with skepticism from the outset. But for Kuhn, one of the key characteristics of the healthy functioning of the community of scientists is its practice of singling out a successful theory from its competitors—without concern for its social implications, and in isolation from public scrutiny.

In a sense, Popper and Kuhn each saw himself as a defender of free inquiry—but their notions of free inquiry were fundamentally opposed. Kuhn's thesis reserved free inquiry specifically for scientists, by considering legitimate whatever paradigm scientists happened to agree upon at a given time. But Popper, given his longstanding concern for the open society, thought that this idea marginalized the role of skepticism, only regarding it as important at the point of crisis, and that it thus undermined free inquiry as a methodological commitment to truth.

Popper particularly targeted the tendency among some influential social scientists to advance their political and social theories without revealing their philosophical underpinnings. Some of the great catastrophes of the twentieth century resulted from the widespread acceptance of theories that reduced society to a machine that could be steered by competent authorities. Popper's falsification principle was meant in part to moderate the authority of social science, which—to the extent that it attempted to predict and regulate society—could lead to a passive public and technocratic governance at best, or modern serfdom and totalitarianism at worst. Kuhn himself was hardly a great booster of the social sciences. But the application of Kuhn's ideas to social science seemed to imply that a theory, however false, should be allowed to dominate the opinion of scientists and the public until it buckles under the weight of its own flaws.

For their part, Kuhn and his followers argued that Popperian falsifiability was an impossible and historically unrealistic standard for science, and noted that any paradigm has at least a few anomalies. In fact, these anomalies are critical for determining which puzzles normal science seeks to solve. Popper's standard, on the other hand, would seem to require scientists to be forever preoccupied with metaphysical, pre-paradigmatic arguments. But in a sense, this was the point: Popper's insistence on falsification was precisely meant to sustain the need of the social sciences to focus on questions of first principle, so as to avoid the rise of any new dangerous philosophies falsely carrying the banner of science.

While the physical sciences were the most prominent in the public mind when Kuhn was writing *Structure* in the early 1960s, today biology is in ascendance. It is striking, as Hacking notes in his introductory essay, that Kuhn does not explore whether Darwin's revolution fits within his thesis. It is far from clear that Kuhn's thesis can adequately account for not only Darwin's revolution but also cell theory, Mendelian or molecular genetics, or many of the other major developments in the history of biology.

The differences between physics and biology—their varying methods and metaphors—matter immensely for the way we understand ourselves and our world. Beginning in the mid-nineteenth century, the assumptions of modern science began to play a much more prominent role in political philosophy. A scientific way of thinking permeated the writings of Auguste Comte and Karl Marx, and by the end of the century, with the work of Max Weber and Émile Durkheim, the era of social science had begun in earnest. Many of the early social scientists came to view society in terms of contemporary physics; they adopted the Enlightenment belief in science as the source of progress, and considered physics the archetypical science. They understood society as a mechanism that could be engineered and adjusted. These early social scientists began to deem philosophical questions irrelevant or even inappropriate to their work, which instead became about how the mechanism of society operated and how it could be fixed. The preeminence of physics and mechanistic thinking was passed down through generations of social scientists, with qualitative characterization considered far less valuable and less "scientific" than quantitative investigations. Major social scientific theories, from behaviorism to functionalism to constructivism and beyond, tacitly think of man and society as machines and systems.

Given the dominance of physics and mechanism in social scientific thinking, the fact that Kuhn based his thesis almost exclusively on physics gave social scientists reason to consider their philosophical commitments legitimate. They saw *Structure* as a confirmation of their entire approach.

But in the half century since Kuhn wrote his book, biology has taken the place of physics as the dominant science—and so in the social sciences, the conception of society as a machine has gone out of vogue. Social scientists have increasingly turned to biology and ecology for possible analogies on which to build their social theories; organisms are supplanting machines as the guiding metaphor for social life. In 1991, the *Journal of Evolutionary Economics* was launched with an eye toward advancing a Darwinian understanding of economics, complete with genotypes and phenotypes. The justification for this kind of model is straightforward: one of the biggest difficulties for economists is the dynamism of any given economy. As Joseph Schumpeter rightly pointed out, economies change; they evolve, rather than staying fixed like a Newtonian machine with merely moving parts. Since machines do not change, whereas societies do, it is reasonable to move the study of economics away from the metaphor of systems and toward that of organisms.

A recent paper in the journal *Theory in Biosciences* perfectly encapsulates the desire for a more biological perspective in the social sciences, arguing for "Taking Evolution Seriously in Political Science." The paper outlines the deterministic dangers in the view of social systems as Newtonian machines, as well as the problems posed by the reductionist belief that elements of social systems can be catalogued and analyzed. By contrast, the paper argues that approaching social sciences from an evolutionary perspective is more appropriate philosophically, as well as more effective for scientific explanation. This approach allows us to examine the dynamic nature of social changes and to explain more consistently which phenomena last, which disappear, and which are modified, while still confronting persistent questions, such as *why* particular institutions change.

This shift from a mechanistic to an evolutionary model seems like a step in the right direction. The new model aims less at predicting the future and derives its strength instead from its apparent ability to explain a wide array of phenomena. It may be better equipped than its predecessor to account for the frequent changes in the stability of modern economies. Furthermore, a biological model can correctly recognize humans as purposeful and creative beings, whereas mechanistic models reduce people to objects that merely react to outside stimuli.

Nevertheless, a biological approach to the social sciences is reductionistic in its own way, and limited in what it can explain. Biological sciences, much like physical sciences, have been stripped of philosophical concerns, of questions regarding the soul or the meaning of life, which have been pushed off to the separate disciplines of philosophy and theology. Much of modern biology seeks to emulate physics by reducing the human organism to a complex machine: thinking becomes merely chemical potentials and electric bursts, interest and motivation become mere drives to perpetuate the genome, and love becomes little more than an illusion. Such accounts can become problematic if we consider them the only ways to understand human nature—and not least because our answers to these non-scientific questions are at the foundation of how we view the world, and so also of how we interpret scientific findings.

Every model that social scientists use, whether it is derived from physics, biology, or ecology, embodies certain philosophical assumptions about human nature and about the optimal functioning of a society. Viewing social relations as movements of a clock implies a set of beliefs quite unlike those of perceiving the same relations as functions of a cell. Since the work of social scientists is so closely tied to these philosophical concerns on which we tend to disagree, we usually see a number of models compete for acceptance at the same time. And because these metaphysical assumptions are usually unspoken, they set the stage for the competition between models to take the place of what was once an explicit competition between differing philosophical accounts of the world—only now while largely denying that any philosophical debate is taking place.

Perhaps the greatest limitation in the social sciences is that, however good a theory's explanatory abilities, it can say very little about whether or not a particular action *ought* to be performed in order to bring about social change. Since human relations are the object of the social sciences, questions of ethics—about whether or not a change should be induced, who should be responsible for it, and how it should occur—must always be at the forefront. It may be desirable, for instance, to reduce alcoholism; but it does not follow that all actors, such as churches, governments, businesses, public and private mental-health experts, and the pressure of social norms, are equally responsible for undertaking the task, or can equally do so without altering society in other ways. Decisions of this sort inevitably depend on our views of the proper function of institutions and on what constitutes the well-being of society.

Regardless of whether we view society as akin to a physical machine, or a biosphere, or an organism, it remains crucial that we recognize the limitations of each model. But what we learn from Kuhn is that any science that separates itself from its philosophical bases renders itself incapable of addressing such questions even within its own limited scope.

The political philosopher Eric Voegelin, in his 1952 book *The New Science of Politics*, provides a helpful treatment on this point in his assessment of the fifteenth-century English judge Sir John Fortescue. Long before the current trend toward the biological sciences, Fortescue used a biological metaphor, arguing, as Voegelin writes, "that a realm must have a ruler like a body a head," and that a political community grows into an articulate, defined body as though out of an embryo. Rulers were necessary because otherwise the community would be, in Voegelin's words, "*acephalus*, headless, the trunk of a body without a head." Yet Fortescue recognized that the analogy between an organic body and a political realm was limited: by itself, it would have provided an incomplete view of both the individual and society. He therefore introduced into his political theory the Christian notion of a *corpus mysticum*: society is held together not only by a head but also by an inner spiritual bond, a heart that nourishes the head as well as the rest of the body. As Voegelin puts it, however, this heart "does not serve as the identification of some member of a society with a corresponding organ of the body, but, on the contrary, it strives to show that the animating center of a social body is not to be found in any of its human members…but is the intangible living center of the realm as a whole."

By extending the analogy in this way, Fortescue went beyond what we now recognize as the limits of biology, and even of political science as such, in the attempt to capture a fuller sense of human nature and of a political body. Neither biology nor political science by itself would have been capable of producing any such holistic image of society. Most significantly, Fortescue understood that his borrowing from biology was merely metaphorical—and so avoided the mistake that plagues the social sciences today, of treating what is really political theory as straightforward scientific truth.

Value judgments are always at the core of the social sciences. "In the end," wrote Irving Kristol, "the only authentic criterion for judging any economic or political system, or any set of social institutions, is this: what kind of people emerge from them?" And precisely because we differ on what kind of people *should* emerge from our institutions, our scientific judgments about them are inevitably tied to our value commitments.

But this is not to say that those values, or the scientific work that rests on them, cannot be publicly debated according to recognized standards. Thomas Kuhn's thesis has often been taken to mean that choices between competing theories or paradigms are arbitrary—merely a matter of subjective taste. As noted earlier, Kuhn challenged the claim that he was a relativist in a 1973 lecture, offering a list of five standards by which we may defend the superiority of one theory over another: accuracy, consistency, scope, simplicity, and fruitfulness. What these criteria precisely mean, how they apply to a given theory, and how they rank in priority are themselves questions subject to dispute by scientists committed to opposing theories. But it is the existence of recognized standards, even if the standards are open to debate, that allows any judgment to be available for public discussion. And we may add that if social scientists recognize the same standards, then debates over their meaning, application, and priority are harder to settle than in physics because the social sciences are intertwined with philosophical questions that are themselves concerned with what our standards of rationality ought to be.

The lasting value of Kuhn's thesis in *The Structure of Scientific Revolutions* is that it reminds us that any science, however apparently purified of the taint of philosophical speculation, is nevertheless embedded in a philosophical framework—and that the great success of physics and biology is due not to their actual independence from philosophy but rather to physicists' and biologists' dismissal of it. Those who are inclined to take this dismissal as meaning that philosophy is dead altogether, or has been replaced by science, will do well to recognize the force by which Kuhn's thesis opposes this stance: History has repeatedly demonstrated that periods of progress in normal science—when philosophy seems to be moot—may be long and steady, but they lead to a time when non-scientific, philosophical questions again become paramount.

One persisting trouble with Kuhn's classic work is that its narrow focus left too many questions unanswered—including the question not just of what science *is* but of what science *should be*. Here many other philosophers of science, including Popper, offered not just descriptions of science but powerful prescriptions for it. Kuhn's work is largely silent on the value of science and the wellbeing of society, and entirely silent on the wrongheadedness of blindly accepting scientific authority and discarding the philosophical questions that must always come first, even when we pretend otherwise.

Although Kuhn, who died in 1996, was sometimes stung by the criticism he received, he understood the importance of all the poking and prodding. In his 1973 lecture, he argued that "scientists may always be asked to explain their choices, to exhibit the bases of their judgments. Such judgments are eminently discussable, and the man who refuses to discuss his own cannot expect to be taken seriously." Even the great Einstein, who failed to give a full defense for his skepticism of the fundamental randomness posited by quantum theory, became somewhat marginalized later in his career. Kuhn deserves the respect of the rigorous criticism that has come his way. It is fitting that his provocative thesis has faced blistering scrutiny—and remarkable that it has survived to instruct and vex us five decades later.

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