Introduction

THOMAS NICKLES

Whether one is pro-Kuhn, anti-Kuhn, or neutral, no one can deny that the work of Thomas Kuhn has been a lightning rod for debates about science, culture, and policy across many academic fields – and even in the political arena and the business world. This is especially true of Kuhn's best-known work, *The Structure of Scientific Revolutions*, originally published in 1962 and expanded in 1970. By now the book has sold over a million copies in two dozen languages – numbers almost unheard of for an academic book about abstract philosophical topics. The wide reception of his work, which greatly surprised Kuhn himself, has elevated the terms "paradigm," "paradigm change," and "paradigm shift" to household phrases and the stuff of advertising slogans, corporate boardrooms, and Washington bureaucratese. Although diverse individuals and groups have read and used (or misused!) it very differently, each according to their own abilities and needs, Kuhn's work has the merit, in these fragmented times, of serving as a common reference point and of generating cross-disciplinary discussion.

When Kuhn began writing, philosophy of science, especially in England and the United States, was dominated by the logical positivists (Rudolf Carnap, Hans Reichenbach, Carl Hempel, and others) and by Karl Popper and his followers. In *The Structure of Scientific Revolutions* (*Structure* hereafter), Kuhn gave us a very different picture of science.[1] Kuhn contended that there are two types of mature physical science, "normal science" and "extraordinary" or "revolutionary science." In a given scientific field, long periods of conservative, tradition-bound normal science are punctuated by an occasional crisis and, still less frequently, by a revolution. Normal science is highly regimented work under a paradigm. It aims to extend and articulate the paradigm, not to test it, for the paradigm *defines* the research tradition, the scientific life, of a particular discipline and its practitioners. Normal research consists in attempting to solve research puzzles by modeling them and their solutions on exemplary problem solutions previously achieved. Good science is delimited not by rules such as Popper's criterion

of falsifiability, or positivist meaning postulates, or even by more content-laden rules specific to the discipline, but by how practitioners perceive and apply these "exemplars" (as Kuhn termed them). In fact, there is no scientific method in the sense of a set of rules that guide inquiry. Surprisingly, Kuhnian normal science does not aim at essential novelty and, in that respect, is convergent rather than divergent. Yet its very focus on esoteric detail makes it almost inevitable that normal research will eventually disclose difficulties for the reigning paradigm. If these difficulties persist and turn critical, a crisis results.

During a crisis period the usual conservative strictures relax somewhat, and truly innovative ideas and practices may emerge as serious alternatives. The repeated failure of established normal scientists to handle the crisis situation, together with the emergence of a promising new approach, may trigger a revolution. What typically happens during the final phase of a scientific revolution is that a group of mostly younger practitioners advocating a new paradigm succeeds in shoving aside the old paradigm and its supporters and subsequently rewriting the history of the field to make their new paradigm appear to be the final stage in the progressive development of the field.

Clearly, paradigm change is not a rational process as understood by the traditional canons of rationality. For in revolutionary science, normal modes of decision making are no longer available. There is no longer universal agreement about a common archive of exemplars and their significance. Moreover, logic and empirical data alone are never sufficient to resolve paradigm debates, said Kuhn. Indeed, there is often disagreement about the problems, standards, and goals of research and a failure of the vocabularies of the two paradigms to match. Therefore two competing paradigms are "incommensurable," meaning, roughly, that they cannot be measured against the same standard. Yet in Kuhn's own view, paradigm decisions need not be irrational. However, in the more radical passages of *Structure*, he spoke of paradigm changes as akin to perceptual Gestalt switches, religious conversions, and political revolutions, comparisons that he later dropped. In *Structure* (and to the end of his life), he struggled to make sense of the claim that scientists working under competing paradigms "live in different worlds." Hence his conclusion that there is no point in saying that a paradigm change takes that scientific field closer to the truth about a fully determinate real world, waiting out there to be discovered.

While normal scientific results are largely cumulative, on Kuhn's account, science, overall, does not accumulate either empirical facts or

theories in a long, progressive ascent toward truth; for revolutions can undermine bodies of fact and their observational vocabularies as well as entrenched theories and research practices. A revolution carries a science off in a different direction. Near the end of *Structure*, Kuhn likened this process to biological speciation. In science no more than in biological evolution does it make sense to speak of overall progress toward a preordained goal, although we can still trace historical lineages and note significant historical differences such as the increasing specialization and accuracy of latter-day science relative to its past. Thus Kuhn attempted to mesh the two great metaphors: science is evolutionary as well as revolutionary.

All the key terms in this précis of *Structure* are problematic, and all are discussed in the following essays, some in great detail. Kuhn himself added a "Postscript" to the 1970 edition of *Structure* in which he provided some clarification of the highly ambiguous notion of paradigm, explained his "different worlds" position more fully, and defended himself against some prominent criticisms. A paradigm in the primary sense, Kuhn told us, is an "exemplar," that is, an exemplary historical problem solution, an achievement that serves as a model for further work. But he admitted to using "paradigm" in a larger, more global, and more social sense that he now labeled "disciplinary matrix" (a term that he employed rarely thereafter). A disciplinary matrix consists of four kinds of shared commitments that together implicitly characterize a particular research discipline and community: (1) symbolic generalizations such as Newton's laws, (2) metaphysical models of what the world is supposedly really like (e.g., gases as consisting of zillions of billiard-ball-like elastic molecules in random motion), (3) values and standards, and (4) exemplars.

Early critics such as Israel Scheffler (1967) dubbed Kuhn a radical irrationalist, subjectivist, relativist, and irrealist for denying that science gives us the objective truth about reality, even at the perceptual-phenomenal level. More recent critics, such as Alan Sokal and Jean Bricmont (1998, chap. 4), view Kuhn as a principal source of postmodern relativism and of culture-theoretical treatments of science generally – and hence as an instigator of the so-called Science Wars.[2] Other critics view Kuhn as intellectually conservative in important ways. On their account, Kuhn (for good or ill) differed rather little from the logical positivists on crucial issues, especially assumptions about language and meaning. Dudley Shapere's reviews of *Structure* are an early case in point.[3] For Steve Fuller (2000), Kuhn's work is also *politically* conservative and elitist, so much so that, owing to its great influence, it has destroyed any attempt to develop a more democratic science policy for the foreseeable future.

WAS KUHN POSTMODERN?

Since the relevance of Kuhn to postmodern culture studies is a topic that interests many readers, I offer a few suggestive remarks in that connection.[4] One ironic answer to the question "Why consider Kuhn postmodern?" is that he is so difficult to categorize. You simply cannot pin down Kuhn in your butterfly collection of intellectual positions. More seriously, postmodern is post *what*, exactly? What is commonly meant by the "modern era"? A simple answer is that there are at least two quite different referents.

The modern period in philosophy runs, very roughly, from 1600 to 1800, from Bacon and Descartes at one end to Kant at the other. It includes the Enlightenment as well as post-Kantian thinkers such as John Stuart Mill. The twentieth-century logical positivists and Karl Popper and their followers have continued to embrace Enlightenment conceptions and ideals. It was during the seventeenth and eighteenth centuries that epistemology or theory of knowledge replaced metaphysics as "first philosophy" on the ground that, before we can say what the world is really like, we must critically examine the nature and limits of knowledge itself. According to the modern philosophers and many of their successors, knowledge consists in individuals having correct mental representations (e.g., ideas, conceptions, theories) of the world, representations subject to rules or laws such as the association of ideas. Many moderns believed that there is a scientific method the discovery of which explains the seventeenth-century Scientific Revolution and subsequent progress as well as practically guaranteeing future scientific progress – and hence the social progress attendant upon the scientific banishment of ignorance and superstitious folk traditions.

A quite distinct development was modernism in literature, music, painting, and architecture, a multifaceted international avant-garde movement that occurred a century after the Kantian era, roughly from the time of Nietzsche to World War II. Modernism in this sense is also too rich to be described briefly, but it is characterized by free experimentation with alternative (nontraditional) forms – indeed, deliberate breaks with tradition and the discipline it imposes – yet also by the sometimes shameless and heterodox appropriation of traditional materials in ways that transgress artistic, gender, and cultural boundaries and by the desire to construct a future not dictated by the past. Some prominent examples of modernism are stream-of-conscious novels, Bauhaus architecture, atonal music, and cubist painting. Since by their very nature modernism and postmodernism (in this second sense) do not admit of precise definition, and since they vary from one artistic community to another, one is on safer (but never

safe) ground in considering modernism one genre at a time, an endeavor obviously inappropriate here.

Some tendencies in Kuhn's work are postmodern in both senses of "postmodern," others in neither sense. Since Kuhn is far more concerned with the philosophical tradition than with the arts, I shall confine myself to that dimension of the postmodernist debate.

Jean-François Lyotard (1984, p. xxiv) defines postmodern as "incredulity toward metanarratives." Kuhn is best known for challenging the master narrative of modern science, a narrative that became a fixture of the Enlightenment.[5] This is the grand story of human progress toward the ultimate truth about the world and the resulting emancipation from ignorance and from the social problems that it engenders. This progress is to be achieved by the hard labor of our brightest citizens studying the natural world intensely. In some respects it is complementary to, or a secular parallel to, the grand Judeo-Christian religious narrative of the fall and redemption.

Kuhn famously (or notoriously) denied that the history of science tells one linear, continuous, cumulative, unified story. Rather, like other cultural institutions, science (or, rather, the historical succession of sciences) is (are) beset by discontinuities, incommensurabilities, and disunities; and its products are as much constructed or invented as discovered. In this respect, Kuhn decentered the Enlightenment account. The history of science provides no master text of reality, nor is there any reason to think that there is one privileged language of nature. In some passages, Kuhn suggested that science is not so much a self-legitimating project as a diverse but overlapping cluster of alternative forms of life. In deconstructing deep modernist myths about the nature of science, he unintentionally opened the door to attacks upon science itself.

Kuhn's work challenges traditional epistemology in several ways. Clearly, his "historical Kantian" relativism and his rejection of strong realism and traditional conceptions of truth, rationality, objectivity, and justification in science are relevant here. Kuhn dismissed all attempts to put knowledge on permanent foundations. He rejected both traditional rationalism and traditional empiricism, including the latter's sharp distinction between a neutral observational language and a theoretical language. There is no "given" in either experience or thought. Every feature of scientific experience and thought is acquired and, in principle, contestable (albeit not within normal science). He denied that explicit rules and representations exhaust what scientists know and that they even constitute the most fundamental dimensions of that knowledge. On the contrary, the most

important knowledge is embodied in expert experimental and theoretical practice and in the learned but tacit cognitive similarity metrics upon which skilled practices depend. Kuhn denied that there is any such thing as the "scientific method" or even methods (plural) construed as sets of timeless rules of inquiry. Kuhn posited communities of specialists rather than solitary individuals as the bearers of knowledge and insisted that there is no higher form of justification than the assent of the relevant community of experts. Moreover, he noted that scientists, unlike most philosophers, are forward-looking problem solvers rather than backward-looking justifiers of claims about the world: justification of present commitments can be more a matter of future promise than of past success.

Kuhn can therefore be read as reversing some main tendencies of Enlightenment thought. There is no universal reason or intelligence distinct from the content of the specific disciplines. Here Kuhn was indebted to Quine's challenge to the analytic–synthetic distinction and to Quine's naturalism, fallibilism, and holism. However, Kuhn went on to reject Quine's view (which Quine held in common with many positivists) that symbolic logic provides a canonical language for understanding scientific work. There is no privileged language or logic that provides a royal road to clarity or truth, that adequately captures the real world or even our experience of it. In the old debate between logic and rhetoric, Kuhn came down on the side of rhetoric in the sense that, for him, human cognition is governed at bottom by rhetorical relations of similarity, analogy, metaphor, and modeling rather than by logical relations and rules. Scientific thinking does not consist in applying purely logical rules so much as matching present perceptions and problems to domain-specific exemplars; and a great deal of scientific work consists in the construction and use of models. The early Kuhn stressed both direct modeling and the importance of historical patterns of development over static logical patterns, while the later Kuhn expanded his cognitive themes at the expense of the historical ones. In any case, the history of science discloses not steady progress toward a universal, canonical language of science but rather a collection of diverse local discourse communities, all of which eventually find their linguistic and conceptual resources contested as anomalies begin to accumulate. In crisis and revolutionary periods, these irruptions produce the various linguistic and practical failures, failures of translation and of mutual intelligibility, that Kuhn labels incommensurability. These failures of what, previously, to its practitioners, seemed to be the language of nature can serve to open up new possibilities for description and action, new forms of intelligibility.

While the Enlightenment thinkers championed the science of their day for its cosmopolitan character, Kuhn stressed the local aspects of scientific communities. Kuhn was not an expressive Romantic in the sense of the Romantic poets and artists in their reaction to the Enlightenment; but, like the Romantics, he prized the wisdom and intelligence (and intelligibility) of local, discipline-specific, historical traditions over the claims for pure reason. Contrary to the Cartesian tradition, pure reason does not issue in self-intelligible, clear, and distinct ideas with their allegedly self-evident applications in context. Rather, traditions (in a broad enough sense to include established community practices) are what constitute the basis for intelligibility. Furthermore, Kuhn portrayed scientific specialist communities as surprisingly like medieval guilds, with their masters and apprentices learning by example. In this sense he was postmodern because premodern. And despite being an internalist intellectual historian and philosopher in his own work, Kuhn's religious and political metaphors in *Structure* challenged the traditionally sharp distinctions between fact and value, and between internal and external factors in science.

While some of these tendencies were radical, especially for their day, Kuhn's conception of science was also conservative in other ways. Unlike many contributors to present-day cultural studies, Kuhn was not at all antiscience. On the contrary, he considered modern science a good thing, something of great intellectual and social value; and he resisted any efforts to change it even with the intention of improving it. (This is one reason why Fuller charges Kuhn with cultural and political conservatism.) As for the new science studies that his work encouraged, Kuhn famously rejected the Strong Programme in Sociology of Knowledge as "deconstruction gone mad" (Kuhn 2000, p. 110). As indicated earlier, many critics have noted how close some of Kuhn's views about language and meaning were to those of positivists such as Carnap, an observation that is sometimes reversed to demonstrate that the positivists themselves were not the "conservative heavies" that they are often portrayed to be.

I have already mentioned the quasi-medieval, convergent, tradition-bound, authoritarian nature of normal science. Many prominent critics have rejected Kuhn's conception of paradigms themselves as dogmatic, totalizing centers of scientific thought and practice. In addition, Kuhn's own perspective in *Structure* is not that of a committed normal scientist more or less imprisoned within his local Kantian world of experience. Rather, Kuhn pretends to stand outside the history of science as a godlike but skeptical observer and to declare that mature natural sciences must fit one simple, repeating pattern: normal science → revolution → new normal science, a

pattern that must employ arbitrary assumptions in every cycle and hence can never hope to find the warranted truth about the world (Nickles 1998). So, in these particular respects, even Kuhn offers us a totalizing narrative.

However, Kuhn's narrative is nuanced. For Kuhnian paradigms are not dogmatic creeds so much as forms of practical life. Paradigms are not rigid, deductive, logical structures that all practitioners must believe in, articulate, and justify in the same way. Each subspecialty develops its own local paradigm as well as its own practical understanding of the global paradigm that characterizes the scientific field as a whole.

KUHN'S LIFE AND CAREER

Thomas Samuel Kuhn was born in 1922 in Cincinnati, Ohio, the first child of a father who was a hydraulic engineer turned investment consultant and an educator mother who did professional editing.[6] The family soon moved to New York City and later to a country town an hour away up the Hudson River. Young Tom Kuhn attended various politically progressive private schools in the eastern United States. In 1940 he was proud to be admitted to Harvard, his father's college, as an undergraduate. Much later in life he was surprised and amused to learn that, in those days, nearly all qualified applicants were admitted to Harvard.

Kuhn's forte as a schoolboy had been mathematics and physical science, so he became a physics major. He also enjoyed literature and philosophy while having limited time to pursue them. He found Kant's philosophy a "revelation," a discovery that foreshadowed Kuhn's later intellectual development. It was surely his editorials in the *Harvard Crimson* that brought him to the attention of James B. Conant, the chemist president of Harvard and a national leader in science policy circles and in academe's response to the outbreak of World War II. Kuhn compressed his undergraduate work into three years in order to graduate and join the war effort. He worked in radar for the U.S. government in Boston and then in England, with bits on the Continent, work that he found increasingly tedious – certainly relative to the events unfolding around him. In 1945, with the war ending and having witnessed the liberation of Paris, he returned to enter Harvard Graduate School in physics.

Kuhn's academic career has been described in terms of "cumulative advantage" (Merton 1977, p. 89) and as "being there" (Fuller 2000), but it also had its rough side. Although Kuhn was a physics graduate student, he suspected that his heart was in philosophy; so he received permission to

spend part of his first year taking philosophy courses. However, finding his background in philosophy too thin to consider switching fields at that point, he decided to finish his degree in physics, writing a dissertation in solid state physics under James Van Vleck, later a Nobel laureate. But by this time Kuhn's more important mentor had become Conant himself, who recruited Kuhn to teach his newly conceived undergraduate history of science course. This was the famous Harvard "case studies" course. Conant's purpose in organizing it, apparently, was not only to increase scientific literacy among nonscientists but also to lure talented undergraduates into the fields of science and technology, especially as policy makers (Fuller 2000).

During the dissertation stage of his graduate training, Kuhn finally decided to switch fields. He boldly persuaded Conant to support his appointment as a Junior Fellow of Harvard's Society of Fellows in order to transform himself into a historian of science as a route to the philosophical issues he really wished to investigate. Kuhn, who had not liked the history course he took as an undergraduate and who would never relish careful archival research, thus became, in his own words, "a physicist turned historian for philosophical purposes" (Kuhn 2000, p. 320). A high-strung, rather nervous and impatient person, Kuhn was never completely comfortable in any professional field any more than he had felt fully at home in any discipline as a student. Indeed, Kuhn was always something of an amateur, largely self-taught in philosophy and even in history of science. The latter is not surprising, however, since in those days history of science was only beginning to emerge as a professional discipline.

After three years as a Fellow, Kuhn became an instructor and then an assistant professor at Harvard. But it was still not smooth sailing, for it eventually became clear that he would not be awarded tenure at Harvard. So Kuhn accepted an assistant professorship post on the opposite coast, at the University of California, Berkeley. The position was initially offered by the Philosophy Department but was then turned into a joint appointment with History. Kuhn's job was to teach history of science and intellectual history from a scientific point of view. Not long after heading west, he spent a year at the Institute for Advanced Study in the Behavioral Sciences in Palo Alto working on the material that would eventually become *Structure*. Then, some years later, it happened again. When he came up for promotion to full professor, having published *The Copernican Revolution* and *Structure* as well as numerous historical essays, the Philosophy Department supported his promotion only in History, not in Philosophy. This was a severe blow to a man who considered himself a philosopher first and whose abiding interest was the philosophical consequences of the history of science.

While spending a year in Copenhagen working on an archive for the history of quantum mechanics,[7] Kuhn received an offer from Princeton to join the new Program in History and Philosophy of Science, a position that seemed ideally suited to his aspirations. He moved to Princeton in 1964 and remained there until 1979, when he returned to Cambridge – but now to MIT rather than Harvard, as the Laurence S. Rockefeller Professor of Philosophy. He retired from MIT in 1991.

Thomas Kuhn died of cancer in 1996 at the age of seventy-three.

While at Berkeley, Kuhn had published two books, *The Copernican Revolution* (1957) and *The Structure of Scientific Revolutions* (1962). The former emerged from Kuhn's lectures and already challenged orthodox understandings of science in various ways. Among other things, this book was the first major expression of Kuhn's abiding interest in revolutionary cognitive shifts arising out of his own earlier epiphany in making sense of Aristotle[8] and his still earlier encounter with Kant. Indeed, all of Kuhn's work was deeply personal.

Structure was solicited by none other than Rudolf Carnap, the leading positivist philosopher and logician, for the *Encyclopedia of Unified Science*, the large encyclopedia project of the logical positivists, originally conceived by Otto Neurath and published by The University of Chicago Press. The Press agreed to publish *Structure* also as a separate volume.

A crucial event in Kuhn's gaining a major reputation was the International Colloquium in the Philosophy of Science, held at Bedford College, London, in July 1965. Kuhn was invited as a rising young historian of science whose ideas had philosophical implications. He left as a major player among the competing "big systems" in methodology of science. Among the other players were Karl Popper, Imre Lakatos, Paul Feyerabend, Stephen Toulmin, and, of course, the positivists collectively, including Kuhn's new colleague, Carl Hempel. The proceedings of this conference, with many of the contributions appearing in revised form in order to respond to Kuhn's challenge, appeared in 1970 as *Criticism and the Growth of Knowledge*, edited by Lakatos and Alan Musgrave.

The Essential Tension, a collection of Kuhn's more influential historical and methodological essays, appeared in 1977, followed a year later by *Black-Body Theory and the Quantum Discontinuity: 1894–1912*, an unorthodox history of the emergence of the early quantum theory. Kuhn's central thesis in that book was that, contrary to the received view, Max Planck was not the founder of quantum theory in 1900, for he was then still working in a well-established classical tradition. Rather, it was Einstein's and Ehrenfest's misreading of Planck's work as an attempt to solve *their* problems that