# Thomas S. Kuhn

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BOOKS: *The Copernican Revolution: Planetary Astronomy in the Development of Western Thought* (Cambridge, Mass.: Harvard University Press, 1957);

*The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962; enlarged, 1970); **[Comment/query to Contributor 1: Deleted the 1996 edition, which seems to be just a reprint of the second with an added index.]**

*The Essential Tension: Selected Studies in Scientific Tradition and Change* (Chicago & London: University of Chicago Press, 1977);

*Black-Body Theory and the Quantum Discontinuity, 1894–1912* (Oxford: Clarendon Press / New York: Oxford University Press, 1978; enlarged edition, Chicago & London: University of Chicago Press, 1987);

*The Trouble with the Historical Philosophy of Science* (Cambridge, Mass.: Department of the History of Science, Harvard University, 1992);

*The Road since* Structure: *Philosophical Essays, 1970–1993, with an Autobiographical Interview*, edited by James Conant and John Haugeland (Chicago & London: University of Chicago Press, 2000).

OTHER: "The Function of Dogma in Scientific Research," in *Scientific Change: Historical Studies in the Intellectual, Social and Technical Conditions for Scientific Discovery and Technical Invention, from Antiquity to the Present*, edited by A. C. Crombie (London: Heinemann, 1963; New York: Basic Books, 1963), pp. 347–369;

"Dubbing and Redubbing: The Vulnerability of Rigid Designation," in *Scientific Theories*, edited by C. Wade Savage, Minnesota Studies in the Philosophy of Science, volume 14 (Minneapolis: University of Minnesota Press, 1990), pp. 298–318. **[Comment/query to Contributor 2: I have left in this section only those essays that do not appear in *The Essential Tension* or *The Road since* Structure.]**

SELECTED PERIODICAL PUBLICATION–UNCOLLECTED: "The Halt and the Blind: Philosophy and History of Science," *British Journal for the Philosophy of Science*, 31 (1980): 181–192.

Thomas S. Kuhn was the most influential philosopher of science of the second half of the twentieth century. His most important work, *The Structure of Scientific Revolutions* (1962), wasd one of the most widely read academic books of the century and had an influence far beyond the field of philosophy of science. Kuhn's thesis that the physical sciences develop in a cyclical pattern, with alternating periods of "normal" and "revolutionary" science upset the consensus that science progresses in a linear fashion, adding to existing knowledge and refining theories to bring them ever closer to the truth. Kuhn's key idea that research during periods of normal science is puzzle-solving governed by a "paradigm"–an exemplary piece of research that is overthrown and replaced by a scientific revolution–was applied by first to the natural sciences, then to the social sciences, and ultimately to activities outside science altogether. While Kuhn did not endorse all of these applications and extensions of his ideas, they helped to popularize his work and gave his use of the term *paradigm* wide currency.

Prior to *The Structure of Scientific Revolutions* the central question in the philosophy of science was: when does observational evidence confirm or disconfirm a hypothesis? The question was to be answered by logical and conceptual analysis, the methods typical of logical positivism. The assumption was that, given a satisfactory answer to this question, a study of the history of science would demonstrate that later theories are better confirmed than earlier ones and that rejected theories are less well confirmed than those that are accepted. The corollary to this assumption was that science yields a picture of the world that is ever increasing in breadth and in proximity to the truth. Kuhn's work provided both a critique of the premises of this way of doing philosophy of science and a model for a new way of pursuing it. Kuhn questioned the assumption that the relationship between observation and theory is one-way, with the choice of theory depending on the observational evidence but not vice versa, arguing that what one observes may depend on the theories one already holds. He claimed, further, that the meanings of key scientific terms depend on the theories of which they are a part. Thus, disparate theories, such as the physics of Aristotle and of Sir Isaac Newton, cannot be directly compared; they are "incommensurable." *The Structure of Scientific Revolutions* turned attention from the static relationship between evidence and theory to the dynamic question of how a discipline develops over time. Kuhn's emphasis on detailed examples from the history of science gave philosophy of science an historical turn that predominated for the next quarter of a century.

Thomas Samuel Kuhn was born on 18 July 1922 in Cincinnati to Samuel L. Kuhn, an industrial engineer, and Annette Stroock Kuhn. As the family moved from place to place, he attended schools in New York, Pennsylvania, and Connecticut. **[Comment/query to Contributor 3: Biographical information has been added throughout the entry. The chief sources were the interview in *The Road since Structure,* the *New York Times* obituary, and the entry on Kuhn in *Contemporary Authors Online*.]** Graduating summa cum laude from Harvard University with a degree in physics in 1943, he spent the remainder of the World War II years in research related to radar, first with the Radio Research Laboratory at Harvard and later in Europe with the U.S. Office of Scientific Research and Development. Returning to Harvard, he earned a master's degree in physics in 1946.

While studying for his doctorate, Kuhn was asked by Harvard president James Conant, a chemist, to assist in Conant's course in science for undergraduates in the humanities, which was part of the General Education in Science curriculum. The course was Kuhn's first exposure to science texts from earlier historical periods; Aristotle's *Physics*, in particular, struck him as so alien to modern ideas on the subject that it could not possibly be the work of one of the greatest thinkers of all time. Intense study of the text, however, yielded the insight that–understood on its own merits, rather than through the lens of subsequent history of science, and undistorted by changes in the meaning of the scientific terminology–Aristotle's work made a great deal of sense. This insight was the germ of his theory of scientific revolutions.

Sponsored by Conant, Kuhn was elected a junior fellow of the Society of Fellows in 1948. On 27 November 1948 he married Kathryn Louise Muhs; they had three children: Sarah, Elizabeth, and Nathaniel. Kuhn received his Ph.D. in physics in 1949 with the dissertation "The Cohesive Energy of Monovalent Metals as a Function of Their Atomic Quantum Defects." In 1951 he moved from junior fellow to instructor and took over the science course from Conant. In 1952 he became an assistant professor of general education and the history of science. He published a series of papers in the history of science, focusing on eighteenth-century theories of matter and the early history of thermodynamics. He was a Guggenheim fellow in 1954–1955.

In 1956 Kuhn was denied tenure at Harvard because the committee thought that his first book, *The Copernican Revolution: Planetary Astronomy in the Development* *of Western Thought* (1957), which it reviewed before publication, was too popularly written and not sufficiently scholarly. Kuhn promptly accepted a position as assistant professor of history and philosophy at the University of California at Berkeley. Among his colleagues at Berkeley, Stanley Cavell was particularly influential on his thinking. Cavell directed Kuhn toward the works of Ludwig Wittgenstein, whose notion that words have a meaning in virtue of their role in what he called "language games" shaped Kuhn's later contention that paradigms have a semantic function in fixing the meaning of scientific terms.

In 1957 Kuhn published *The Copernican Revolution.* The work reflects its origins in the General Education in Science program at Harvard: in the foreword Conant explains that the recounting the history of science is an effective method of getting nonscientists to understand how science works, and the text is easily accessible to nonexperts. Kuhn hoped that his description of an important historical instance of a revolution in science would show that scientific beliefs can be overthrown–that even long-held theories are not immune from criticism and do not last forever. His intention in the work is not skeptical, however: he demonstrates that a core of beliefs is retained even when many important one are rejected; rather, it was to promote a realistic attitude toward modern science. As shown by the subtitle of the book, *Planetary Astronomy in the Development of Western Thought,* Kuhn is also concerned to point out that the Copernican revolution was not merely significant in astronomy but had wider ramifications in science. The revolution also had a profound effect on philosophy, leading to a reassessment of humanity's place in the universe, of which it was no longer the center, and its conception of its relationship with God.

For the Copernican revolution to show that scientific beliefs are subject to radical revision, Kuhn has to establish that the geocentric view that preceded the revolution was a genuine scientific one and not a religious doctrine, a traditional myth, or a prescientific speculation. Every society, however ancient or scientifically primitive, has beliefs about the nature of the universe. The ancient Greeks were different from almost all other societies in giving a central place to the study of the planets and stars in forming their cosmology. In so doing they posed questions for which straightforward answers were unavailable, such as: what is the radius of the Earth? How far away is the Moon? Why is the motion of the planets not uniform? Such questions require a theoretical rather than a merely observational approach. This early development culminated in the *Almagest*, written by Claudius Ptolemy in the middle of the second century A.D. The work established a mathematically sophisticated tradition of inquiry that was transmitted to medieval Europe via Islamic scholars.

Having shown that Ptolemaic astronomy was more akin to modern science than has generally been thought, Kuhn goes on to claim that Copernican astronomy is less close to modern astronomy than the history of science has typically portrayed it as being. Although Nicolaus Copernicus's innovation in his *De revolutionibus orbium coelestium libri vi* (Six Books Concerning the Revolutions of the Heavenly Orbs, 1543) of putting the Sun at the center of the solar system had the effect of overthrowing Ptolemy's astronomy and, ultimately, Aristotle's physics, as well, Kuhn argues that Copernicus himself saw his heliocentric hypothesis as merely a modification of those traditions meant to respond to mathematical problems that arose in them—in particular, the failure of Ptolemy's system to give an accurate account of the apparent movements of the Sun and the Moon. Copernicus retained Ptolemy's system, based on the principles of Aristotle's physics, of rotating solid spheres, epicycles, and eccentrics. Copernicus's system failed, however, to provide astronomers with the increased mathematical accuracy for which he had hoped. Instead, according to Kuhn, the experts were attracted to the new system by its aesthetic properties: they appreciated Copernicus's ability to do away with ad hoc elements in Ptolemy's system, such as the equant **[Comment/query to Contributor 4: Please define *equant.*]** and the treatment of the orbits of the Sun, Mercury, and Venus. Furthermore, even though Copernicus retained Ptolemy's explanatory device of epicycles so that he could maintain the Ptolemaic-Aristotelian requirement of circular motion, his explanation of the retrograde motion of the planets was far more aesthetically pleasing than Ptolemy's.

While Copernicus intended his heliocentrism as a reformation of the tradition, those who adopted it saw that its consequences were far more profound. With the removal of Earth from the center of the universe, Aristotle's distinction between the different kinds of laws that apply on Earth, where objects tend to move in straight lines, and to the planets, whose motions are naturally circular, could not be maintained, since Earth itself was now held to be a planet. Hence, in the seventeenth century Galileo was able to challenge Aristotle's terrestrial physics, and Johannes Kepler was able to describe planetary motion as elliptical rather than circular. The culmination of these theories was Newton's unification of the laws of physics in a single universal system. Copernicus's beliefs, Kuhn says, have much more in common with those of Aristotle and Ptolemy than with those of Newton. He concludes that while one may legitimately see Copernicus as initiating modern astronomy, he can equally be seen as the last great figure of the preceding tradition.

In 1958 Kuhn was promoted to associate professor. The following year he delivered a paper, "The Essential Tension: Tradition and Innovation in Scientific Research," at a Salt Lake City conference on the identification of scientific talent; it became the title piece of his collection *The Essential Tension: Selected Studies in Scientific Tradition and Change* (1977). From the 1920s to the 1950s the dominant philosophy of science had been logical positivism, according to which all scientific knowledge must ultimately be based on observation–that is, on sense experience. Positivism, however, faced the problem of fully explicating the theoretical language of science in terms of the language of sense-experience, and it lacked an inductive logic–a set of rules for reasoning from particular observations to general theories–that could account for the inferences that scientists were actually making. In the 1950s an alternative to logical positivism was becoming popular among practicing scientists: the "falsificationism" of Karl Popper, according to which scientific theories are not constructed by inductive inference from observations; instead, a scientist makes a tentative, unjustified theoretical conjecture and puts it to an experimental test. If it fails the test, it is rejected, and a new conjecture is sought; if it passes the test, it is subjected to further testing. A theory is never verified; but as long as experiments fail to disconfirm it, it is provisionally accepted. Many scientists admired Popper's emphasis on rigorous testing; they thought that the picture of scientists subjecting their theories to tests that might well falsify them was a more accurate–or, at least, more flattering–depiction of their activities than the positivist conception of science as concerned with confirming evidence. Such scientists stressed the need for educating young scientists to engage in "divergent thinking"–to question common assumptions and to look at familiar subjects in new ways. Insofar as they regarded educational practices as promoting "convergent thinking," they deplored the situation.

Although Kuhn became known as the philosopher of scientific revolutions, in many ways he is most significant for the emphasis he placed on the role of tradition in scientific thinking. His work on the history of astronomy and cosmology had convinced him that Popper's description of scientific practice was incorrect: evidence often conflicts with a currently accepted theory, but the theory is not regarded as refuted. If Popper were correct, the revolutionary overthrow of theories would be the norm rather than the exception. In "The Essential Tension" he argues that for "ordinary science"–the science in which most scientists engage–convergent thinking is essential. He notes that scientific training relies heavily on textbooks in which the background knowledge required in the field is presented in a standard format. The student is tested with questions that can be solved by established techniques and that have known correct answers. This rigid training in convergent thinking has been immensely productive of important scientific innovations: agreement on accepted theories and methods of inquiry sets the stage on which discovery is possible. The tradition provides the scientist with research problems: it provides a "map" of what the world is like–of what it does and does not contain; but in certain areas the map may be drawn only in outline, and the scientist's task is to help fill in the detail. The tradition, through the scientist's training, supplies the techniques required for this development. Sometimes a phenomenon will be discovered that, at first sight, conflicts with the beliefs and theories embedded in the tradition; the scientist's task then is to try to reconcile the phenomenon with the tradition. This task may be so difficult that it can only be undertaken by those who have a thorough commitment to the tradition in which they are working. Kuhn gives the example of the orbit of Uranus, which was unusual and interesting only against the background of expectations generated by Newtonian dynamics. Belief in that Newton's system and its ability to explain all the details of planetary motion inspired researchers such as Urbain Leverrier and John Couch Adams to put considerable effort into showing that the divergence from expectation, far from being an error in Newtonian dynamics, was, in fact, caused by the existence of an unseen planet behaving perfectly in accordance with that dynamics. Without the tradition, Neptune would not have been discovered–at least, not in the way that it was.

At the same time, Kuhn acknowledges, science is not only the addition of detail to an existing map. Scientific innovations do require the rejection of accepted theories–the common picture of science as the stockpiling of knowledge is a mistake. The scientist who makes such an innovation needs to think both convergently and divergently. Convergent thinking is required for the scientist to be in a position to make the discovery in the first place–the innovator must understand and work within a tradition to see that an anomalous finding cannot be accounted for by the tradition. At the same time, solving these problems requires the ability to generate approaches that conflict with the tradition and require the rejection of entrenched beliefs, techniques, and theories: divergent thinking. This conflict is the "essential tension" that Kuhn identifies at the heart of scientific research and progress. Kuhn rejects the assumption, made both by logical positivism and by Popper, that all theories are subject to empirical testing, whether for confirmation or for falsification.

The ideas advanced in "The Essential Tension" are the kernel of the view that Kuhn sets forth in *The Structure of Scientific Revolutions.* That the book was published in the series "International Encyclopedia of Unified Science," edited by the positivists Otto Neurath and Rudolf Carnap, indicates that it was not a root-and-branch attack on positivism. Kuhn presents a picture of an alternation in science between stable periods, in which the leading theories in a particular field are accepted by virtually everyone working in that field, and briefer revolutionary episodes in which those theories come under scrutiny and are replaced by others. Kuhn calls the stable periods "normal science," the revolutionary episodes "extraordinary science" or "revolutionary science." Normal science is governed by a "paradigm." Since Kuhn's use of the term in *The Structure of Scientific Revolutions*, *paradigm* has become a cliché that is almost empty of meaning. The concept does not have an unequivocal application even in Kuhn's hands; Margaret Masterman has identified twenty-one senses of the word in *The Structure of Scientific Revolutions*. Even so, the term has two central and related uses: a broader one that refers to all of the shared commitments of a group of scientists and a narrower one that refers to a seminal scientific achievement that acts as a model for research.

Kuhn characterizes normal science as "puzzle-solving." In normal science the leading theories–those encapsulated in the paradigm in the broader sense–are not tested: they are not subject to confirmation or falsification. Paradigms in the narrower sense are exemplary "puzzle-solutions," and they guide normal science in several related ways. First, they provide examples of what counts as a worthwhile research problem. The puzzles that subsequent researchers set themselves must be similar to the exemplary puzzle if they are to be regarded as worthwhile projects. Newton's *Principia Mathematica* (1687), for example, is the paradigm that governed celestial mechanics and much of the rest of physics for 250 years. It laid out the sorts of problems that would be worth attacking: some were specific, such as describing the details of the motions of the planets and their moons; others were more general, such as finding other laws that, like Newton's law of gravitation, account for the forces objects exert on each other. Second, the paradigm provides the tools with which the solution may be sought. *Principia Mathematica* developed the techniques of the infinitesimal calculus that were employed by all of Newton's successors in their research. Third, the paradigm offers a standard by which the quality of the solution can be measured: similarity to the paradigm is a sign of scientific worth. For example, the experimental success of Coulomb's law was not the only consideration that recommended it to

George A. de Coulomb's contemporaries; just as significant was the fact that it is an exact analogue of Newton's law of gravitation.

In claiming that paradigms as exemplars play a role in the assessment of proposed puzzle-solutions, Kuhn breaks with his empiricist predecessors, who assumed that the rules of theory evaluation are independent of any particular theory. Carnap's inductive logic, Carl G. Hempel's hypothetico-deductive model of confirmation, and Popper's falsificationism were a priori philosophical accounts of theory evaluation; hence, they all held that the assessment of a theory should be unaffected by whether or not the theory resembles some other theory.

According to Kuhn, a state of normal science cannot last indefinitely. Scientists will discover phenomena that, despite their best efforts, cannot be explained using the resources of the paradigm. Kuhn calls such intractable puzzles "anomalies." For example, Newton's treatment of the orbit of the Moon failed to fit with astronomical observations, and his successors found that the orbit of Uranus was not what was predicted by the best application of Newton's laws. Both of these anomalies were eventually resolved within the Newtonian paradigm–the first by improved mathematical techniques, the second by the discovery of Neptune. Kuhn notes that a few isolated anomalies may be accepted as part of normal science, rather than, as Popper contended, as immediate refutations of the theories embedded in the paradigm. During normal science a scientist's inability of to solve a particular problem may be attributed to the limited capacities of that individual. An accumulation of unresolved anomalies, however, leads the science into its next phase: that of "crisis." A science is in crisis when its practitioners are no longer convinced that the current paradigm has sufficient resources to provide solutions to the increasing number of serious anomalies. During the stage of crisis the failure to resolve anomalies will be regarded as a failure of the paradigm, and a new paradigm will be required that does not suffer from these anomalies.

Kuhn calls the replacement of one paradigm by another a "scientific revolution." Although the use of the term *revolution* in connection with science was well established by this time, Kuhn's adoption of it reflects two important aspects of his view. First, it points to the cyclical nature of change in science: the adoption of a new paradigm as the result of a scientific revolution inaugurates a new period of normal science, which will lead to another crisis, another revolution, a new paradigm, and so on. Second, Kuhn draws an analogy between scientific and political revolutions. The political parallel to a paradigm is an established mechanism, such as a constitution or traditional lore and accepted practice, for resolving social and political problems. If a political conflict arises that cannot be resolved in the normal fashion, the constitution or traditional practices may have to be replaced by new ones.

Theories based on different paradigms are, Kuhn says, "incommensurable": they share no common measure. The paradigm of one period of normal science may lead scientists to judge one puzzle solution more favorably than another solution, while the paradigm from another period of normal science may lead scientists to make the opposite judgment. Kuhn is especially interested in the relationship between paradigm and language and the incommensurability of meanings that can be generated. He is particularly struck by the difficulty historians of science experience in trying to understand earlier theories. In his own case he had found that at first sight Aristotelian physics seemed absurd and full of obvious errors. A deeper understanding of Aristotle's thought and context, however, gave Kuhn a viewpoint from which the older science no longer appeared straightforwardly wrong. In particular, Aristotelian words and phrases that seem to have modern equivalents actually mean something quite different from their counterparts. But that understanding does not permit a perfect translation between the vocabularies of Aristotelianism and modern physics: according to Kuhn, no translation is both precise and accurate, and no common language can express both theories. The appropriate viewpoint cannot, therefore, be achieved simply by a reinterpretation of ancient texts. Understanding Aristotelian physics means learning to see the world in a different way. This change in perception requires the same kind of psychological leap that occurs when scientists make the transition from one paradigm to another.

Kuhn likens this leap to the sort of change that occurs when one undergoes a "gestalt shift." According to the Gestalt psychologists Max Wertheimer, Wolfgang Köhler, and Kurt Koffka, perceptual phenomena cannot be understood atomistically but only in reference to a whole, or gestalt. The Gestalt theory is commonly illustrated by pictures that can be seen as one image at one moment and as a different image at the next, as in the case of a simple drawing that can be interpreted as a rabbit or as a duck. Kuhn says that gestalt-switching should be regarded not merely as analogous to scientific revolution but also as evidence that common features underlie both the psychology of perception and the psychology of theory acceptance and theory change. A scientist's ability to "see" a proposed puzzle solution as similar to or different from an exemplary one results not from the application of logic to the problem but from the employment of a recognitional capacity similar that which is used in seeing lines on a page as a picture of a person or thing. Scientists acquire such capacities, which Kuhn calls “learned similarity relations,” by repeated exposure as students to exemplary puzzles and their exemplary solutions. The largely uniform education one finds within a scientific discipline allows scientists from different countries and cultures to respond in a similar way to scientific developments.

Kuhn also derives from the Gestalt psychology of perception the concept of “theory-dependence (or theory-ladenness) of observation.” The positivists regarded observation as a matter of recording one’s perceptual experiences, which they understood to be uninfluenced by either previous experience or theoretical commitment. Kuhn notes that the philosopher Norwood Russell Hanson challenged this assumption, using gestalt figures to argue that perceptual experience is influenced by one’s presuppositions and beliefs. Kuhn endorses Hanson’s contention and amplifies it by drawing on research carried out at Harvard by the psychologists Jerome Bruner and Leo Postman while Kuhn was there. Bruner and Postman showed that subjects had great difficulty in recognizing playing cards that were anomalous in some way, such as a three of clubs with red instead of black pips, and argued that the nature of perceptual experience depends on the expectations and needs of the subject. Kuhn takes their work to indicate the way immersion in a paradigm influences observation: adherents of different paradigms may not have the same experience when they observe the same scene or experimental apparatus. Neutral observation, then, cannot provide a common measure of theories from different paradigms. Revolutionary change is a transformation not just in theoretical belief but even in what a scientist can observe: “in a sense that I am unable to explicate further,” Kuhn says, “the proponents of competing paradigms practice their trades in different worlds.”

Toward the end of *The Structure of Scientific Revolutions* Kuhn makes it clear that the cyclical nature of science does not mean that scientific progress does not occur. It does occur, and not only during a period of normal science or relative to a given paradigm but also from revolution to revolution: thus, the long-term history of a science is a progressive one. He defines progress as increasing puzzle-solving power: the major factor in the choice of a new paradigm is its ability to provide a framework for solving puzzles. The new paradigm should retain much of the puzzle-solving power of its predecessor but should also be able to resolve some of the major anomalies that caused the crisis for the predecessor. Astronomy and physics can solve more puzzles today than they could in their earlier stages. But Kuhn denies that this increase in puzzle-solving power is a matter of approaching more and more closely to "the truth." For the traditional empiricist picture of science as progress toward the external goal of truth Kuhn substitutes an evolutionary model in which science aims at the internal goal of solving its puzzles. In so doing science progresses in the same sense as a species develops through natural selection: in neither case is there an ultimate perfection toward which each developmental step is a closer approximation.

Kuhn's emphasis on paradigms as arbiters of quality during periods of normal science led critics to regard him as an irrationalist who considers all scientific values, including truth, as relative to a paradigm. The accusation of irrationalism was, however, directed even more strongly at his account of revolutionary science. His analogy of scientific with political revolutions seemed to imply that the outcome of a scientific revolution is determined by highly contingent, nonscientific factors–the equivalent of the revolutionary mob, propaganda, coercion, and the like. This impression was reinforced by Kuhn's remark that "external" causes may play a major part in fixing the content of the new paradigm. Such causes include developments outside the science in question, as well as "idiosyncrasies of autobiography and personality"; "Even the nationality or the prior reputation of the innovator and his teachers can sometimes play a significant role," Kuhn writes. He had to put considerable effort into explaining the ways in which such criticism misrepresents his views. At the same time, this aspect of Kuhn's work was emphasized by many who claimed to be his followers, including sociologists of science who saw in his remarks the seed of a new approach to the explanation of scientific change: change was taken to be explicable not, as the positivists claimed, by reference to a priori rules of theory choice but by appeal to explicitly sociological factors. Others who took Kuhn's account as vindicating their views included feminist and postmodernist theorists who wanted to portray science as the expression of a traditional, male-dominated hierarchy rather than as the product of pure reason. Kuhn was thus faced with an interpretation of his work that he denied but that was shared by his detractors and supporters alike.

Another feature of *The Structure of Scientific Revolutions* that attracted charges of irrationalism was Kuhn's emphasis on incommensurability. The claim that scientists operating in different paradigms observe differently led some critics to accuse Kuhn of idealism, while some of his supporters, reading into the claim a strong kind of social constructivism, took it as further criticism of the alleged objectivity of science. Kuhn repudiated both interpretations, pointing out that his claim is clearly linked to the psychological thesis that experience is theory-dependent.

In 1964 Kuhn moved to Princeton University as professor of philosophy and history of science. The following year a conference on his work was held at the International Colloquium in the Philosophy of Science at Bedford College in London. Papers were presented by Kuhn, Popper, Masterman, John Watkins, Stephen Toulmin, and L. Pearce Williams. The papers were published, along with later contributions by Paul Feyerabend and Imre Lakatos, as *Criticism and the Growth of Knowledge* (1970). Kuhn's paper, "Logic of Discovery or Psychology of Research?" (republished in *The Essential Tension*), compares his and Popper's approaches to scientific change. He acknowledges their shared emphasis on scientific revolutions and the rejection of old theories, in contrast to the traditional view of science as a steady accretion of knowledge. As opposed to the positivists, he and Popper stress the dependence of observation on theory; they are also realists in that they take theories as attempting to explain phenomena by reference to real entities, whereas the positivists regard theories as instruments of prediction that, when properly understood, do not assert the existence of anything other than phenomena. (Although Kuhn does not mention it, he and Popper also employ evolutionary analogies in explicating their conceptions of scientific progress.)

Nonetheless, Kuhn is more concerned to explore the precise nature of their differences. For Popper, science progresses only when a theory is tested, falsified, and replaced by a new theory; for Kuhn, most scientific advances occur during periods of normal science, when an established tradition, crystallized around a paradigm of good research, provides the puzzles and the standards for an acceptable solution. Revolutions, by definition, do not occur during normal science. Popper regards falsifiability as a defining feature of a science: a true science has theories that make predictions that can be tested in such a way that some possible results would show the theory to be false. Astronomical theories, for example, make detailed quantitative predictions about the observable positions of the planets; the "predictions" made by astrologers, on the other hand, are nonquantifiable and imprecise, so that any outcome can be taken to be in accord with them. Astrological claims are, thus, not falsifiable and so are not scientific. Kuhn cannot accept such a criterion, since paradigm theories and puzzle-solutions are not tested during normal science and are not regarded as falsifiable. According to Kuhn, astronomy has a puzzle-solving tradition of research that astrology lacks; even when it was intellectually respectable, astrology was not a science but a craft. Like medicine and mechanical engineering, it was linked to a science but was not itself a science.

Kuhn's first complaint against Popper, then, is that he fails to recognize the existence of normal science. But Popper's account of revolutions is also inaccurate, according to Kuhn. Popper holds the overthrow of an existing theory to be the logical result of testing the theory and finding that it fails the test, while Kuhn argues that revolutions are produced by crises that result from the accumulation of anomalies; hence, revolutions occur, in Kuhn's view, without the underlying theory being put to the test. Furthermore, crises and the response to them are psychological, rather than logical, in character: no logical criterion determines the number of anomalies sufficient to constitute a crisis, nor does logic require that a crisis be resolved by a revolutionary change in theory. Kuhn notes that Popper himself recognizes that conclusive disproof of a theory is not available, since various ad hoc moves, such as denying the reliability of experimental results, may be made. But this admission represents a threat to Popper's basic position, whereas the lack of a conclusive ground for the rejection of a paradigm is central to Kuhn's view. As a crisis develops, "conservatives" can retain their faith in the existing paradigm, and "radicals" can reject it, with neither side committing a logical error. The very fact that uncertainty exists concerning the paradigm indicates a lack of consensus on what had previously been the shared basis for the interpretation of experiments and observations. Thus, although Popper is right that theories are overthrown in revolutions, he is wrong in thinking that such overthrows are dictated by falsification. Falsification, says Kuhn, takes place only during normal science, when agreement on a paradigm permits the unambiguous reading of data collected in the process of puzzle solving; in such cases the underlying theory of the paradigm is not falsified, for that theory is the basis of the test. Rather, the attempted puzzle solution is disproved. Popper, says Kuhn, makes the mistake of transferring the process of falsification from normal science to revolutionary science and, thereby, from puzzle-solutions to theories.

A second edition of *The Structure of Scientific Revolutions* was published in 1970; it includes "Postscript 1969," in which Kuhn clarifies some of the ideas presented in the first edition that had been misunderstood and also develops and modifies some of his earlier views. Among the ideas clarified is the notion of paradigms and their function. Kuhn now explicitly distinguishes between the broader conception of paradigm, which he calls a "disciplinary matrix" and defines as "the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community," and the narrower conception, which he calls the "exemplar" and defines as "the concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science." A sociological concept, the disciplinary matrix refers to the objects of consensus that bind practitioners of a scientific discipline together. The key components of a disciplinary matrix are symbolic generalizations (formal theories, key equations, laws, definitions, and so forth); metaphysical beliefs; scientific values such as accuracy, consistency, breadth of scope, simplicity, and fruitfulness; heuristic models (analogies that help direct thinking); and exemplary puzzle-solutions. The last element of the disciplinary matrix is its most important feature and constitutes the narrower notion of paradigm, which, Kuhn says, was the most novel and least understood aspect of *The Structure of Scientific Revolutions.* He emphasizes the role of exemplars in forging learned similarity relations–the ability to see one puzzle solution as similar to another. This ability is acquired through training with examples, Kuhn says, not by learning rules. Exemplars are also used to establish the meanings of scientific terms: the symbols employed in symbolic generalizations such as laws and equations derive their meanings from exemplary scientific puzzle-solutions.

"Postscript 1969" also addresses the accusations of Kuhn's critics that he had advanced a relativistic account of science that left no room for the traditionally accepted value of absolute truth. Kuhn notes that his evolutionary picture of scientific progress holds that later theories are better than their predecessors in making more-accurate predictions and solving more problems and is, therefore, not relativistic. But he repeats his denial of any need for the notion of truth in explaining the development of science, going even further to argue that the idea of science presenting ever better representations of what nature is really like, or of theories growing closer to the truth, is actually incoherent: scientists have no way of knowing what is "really there" independently of their theories, and this situation deprives the idea of truth–understood as a match between a theory and an independent reality–of any sense.

In the decade and a half following the original publication of *The Structure of Scientific Revolutions* Kuhn most of Kuhn's work was devoted to developing themes from the book or responding to criticism of it. Then, in 1978, he published *Black-Body Theory and the Quantum Discontinuity, 1894–1912,* his second book dealing with a particular episode in the history of science. Unlike *The Copernican Revolution,* the work is highly technical and not readily accessible to readers without a solid background in physics. Also unlike *The Copernican Revolution,* which had referred to sun-worship and Neoplatonism in explaining how the heliocentric viewpoint came to be accepted, the new book ignores factors outside of science. The absence of such factors surprised those who regarded Kuhn as promoting sociological explanations in the history of science. Readers were also surprised by the lack of references to paradigms, disciplinary matrices, incommensurability, normal science, crises, and so on; while many historians and philosophers of science had adopted Kuhn's vocabulary, Kuhn himself seemed to have abandoned it.

Kuhn's aim in *Black-Body Theory and the Quantum Discontinuity, 1894–1912* is to show that the traditional understanding of the origin of the quantum revolution in physics is mistaken. In classical physics, particles change from one energy level to another in a continuous fashion, whereas in quantum physics they "jump" from level to level without passing through intermediate levels: change in energy is discontinuous. The discovery of quantum discontinuity was generally believed to have been made by Max Planck in 1900 or 1901, when he described black-body radiation–the distribution of energy within a cavity–as divided into multiples of the unit *h*O, **[Note to typesetter/copyeditor: The preceding should be an italicized "h" followed by the Greek letter nu.]** in which O is the frequency of radiation and *h* is a constant ($6.6260755 \times 10^{-34}$ joule-second, now known as Planck's constant). Kuhn's contention is that at this time Planck was using a statistical technique, previously employed by Ludwig Boltzmann, of dividing the range of possible continuous energies into "cells" that could be treated together for mathematical purposes. Indeed, Kuhn says, Planck was puzzled that he could he get the result he wanted only by fixing the cell size at *h*O; the technique should have worked for any way of dividing the cells, as long as they were small enough but not too small. According to Kuhn, Planck did not arrive at the genuine quantum concept until 1908, two years after Albert Einstein and Paul Ehrenfest.

Kuhn and his wife were divorced in September 1978. **[Comment/query to Contributor 5: Kuhn left a widow, Jehane R. Kuhn. I assume that she was his second wife. What was her maiden name, and when were they married? (If any other marriages intervened, please add details at the appropriate points in the biography as to when they occurred, wives' names, and how the marriages ended.)]** The following year he became professor of philosophy and history at the Massachusetts Institute of Technology. Over the next decade he began to seek a linguistic account of incommensurability. In his essay "Commensurability, Comparability, Communicability," delivered at the biennial meeting of the Philosophy of Science Association in 1982 and republished, with revisions, in *The Road since* Structure: *Philosophical Essays, 1970–1993* (2000)., Kuhn says that incommensurability can be understood as a certain kind of untranslatability between the language of a new theory or paradigm and that of the old one or, in some cases, as untranslatability between competing paradigms. He points out that contrary to the interpretation of some critics, he did not intend incommensurability to mean that holders of different paradigms cannot communicate or that their paradigms cannot be compared. Incommensurability does imply, though, that communication will be imperfect and imprecise, just as it is not possible to translate the English *carpet* exactly into French. **[Comment/query to Contributor 6: What is the difference between *carpet* and *tapis?*]** Similarly, theories cannot be compared in the point-by-point manner that philosophers such as Popper had envisaged. Kuhn notes that in his contribution to *Criticism and the Growth of Knowledge* he had associated his incommensurability thesis with Quine's "indeterminacy of translation" thesis but has now abandoned this parallel. Quine's claim was that too many translations could be found for any single translation to be determinately correct, while Kuhn's contention is that no translation is possible between the languages of incommensurable paradigms. He did retain from Quine, however, the idea that incommensurable languages divide the world into kinds of things in different ways, and he says that learning kind-terms by means of visual exemplars would make it possible to create different divisions by using different exemplars.

Kuhn goes on to develop a taxonomic account of incommensurability. A scientist uses a taxonomy in categorizing entities–a biologist, for example, will have a taxonomy that divides living beings into species, genera, families, and so on. The terms used to express the taxonomy–its lexicon–are related to one another in a "lexical network." The internal relations among members of the lexical network are constitutive of their meanings; hence, one cannot change part of the network without changing all of it. Kuhn identifies incommensurability with differences in taxonomy brought about by changes to lexical networks or differences between networks. Avoiding incommensurability would require adding to one lexicon new terms to translate the terms of the other, as one might import a French word into English when no exact English equivalent exists. In scientific taxonomies, however, Kuhn claims that such importation is not allowed: a "no-overlap" principle prohibits the kind-categories of a taxonomy from cutting across one another. Either the categories are entirely disjoint, sharing no members, or one category is a subkind of the other category, such that all the members of the first are included in the second.

In 1982 Kuhn was awarded the Sarton Medal by the History of Science Society. Two years later he became professor emeritus at MIT. That same year a second edition of *Black-Body Theory and the Quantum Discontinuity, 1894–1912* appeared. It includes an afterword, "Revisting Planck," that Kuhn had published separately earlier that year in which he seeks to dispel the misconception that *Black-Body Theory and the Quantum Discontinuity, 1894–1912* represents a change in his views from *The Copernican Revolution* and *The Structure of Scientific Revolutions.* The omission of the influence of factors from outside the realm of science in *Black-Body Theory and the Quantum Discontinuity, 1894–1912* resulted from the greater professionalization and professionalization of science in the late nineteenth and early twentieth centuries as compared to the seventeenth century. In regard to the absence of the terminology of *The Structure of Scientific Revolutions*, Kuhn explains that he did not believe it was necessary to employ that vocabulary in examining specific episodes in the history of science. He argues, however, that *Black-Body Theory and the Quantum Discontinuity, 1894–1912* reflects several of the themes of *The Structure of Scientific Revolutions.* One such theme is the rejection of the traditional conception of science as the addition of discrete discoveries to the stockpile of knowledge–a conception that motivates disputes over priority in the sciences–in favor of the view that discovery is a process spread out over time, the product of which may turn out to be incompatible with the thinking that initiated it. This idea was illustrated in *The Copernican Revolution:* Copernicus viewed his work as a means of reconciling Aristotelianism and much of the Ptolemaic system with the need for greater mathematical accuracy in describing planetary motion; yet, the eventual effect of his innovation was the repudiation of Aristotle's physics and all of the remaining elements of Ptolemaic astronomy. As the initiator of this revolution, Copernicus is typically seen as a modern thinker whose conception of the solar system was largely similar to the current one; but for Kuhn, this view is mistaken. Similarly, Planck's conception of the quantum *h*O was quite different in 1901 from what it was ten years later; yet, the prevailing tendency is to think of Planck's earlier use as being the same as the familiar later one. Both cases, Kuhn says, are instances of incommensurability. Where a modern interpretation is readily available, historians tend to see scientists of the past such as Copernicus and the early Planck as being more like themselves than they really are; where no such interpretation is available, they tend to reject the scientists' thought as irrational, as the young Kuhn did with Aristotle's physics. Kuhn notes that such errors can be made by the scientists themselves: Planck, Otto Stern, and Niels Bohr all misremembered the thinking that led them to important findings because of the incommensurability of their earlier ideas with what they went on to discover.

After spending the academic year 1984–1985 studying Kuhn's philosophy of science at MIT, where he had extended discussions with Kuhn himself, the German scholar Paul Hoyningen-Huene published the first monograph devoted exclusively to Kuhn: *Die Wissenschaftsphilosophie Thomas S. Kuhns: Rekonstruktion und Grundlagenprobleme* (1989; translated as *Reconstructing Scientific Revolutions: Thomas S. Kuhn's Philosophy of Science,* 1993). Hoyningen-Huene interprets Kuhn as employing a Kantian distinction between the "world-in-itself," which is independent of scientific investigations, and the phenomenal world, which is partly constituted by the human mind imposing categories on the phenomena. While Immanuel Kant conceived of the categories as immutable mental structures, however, Kuhn regards them as subject to change. An individual's categories are, in part, the product of his or her immersion in a paradigm-governed tradition; hence, when the paradigm changes, the categories through which the individual views the world are likely to change, too. The phenomenal world thus changes, even if the world-in-itself does not. Kuhn endorsed this understanding of his outlook, describing himself in an interview in *The Road since* Structure as a "Kantian with moveable categories." Thus, the charge of early critics that the views put forward in *The Structure of Scientific Revolutions* are idealistic is vindicated, at least in regard to the Kantian form of idealism.

Kuhn's influence has been immense not only in the philosophy and history of science but also in other disciplines and even beyond academia; *The Structure of Scientific Revolutions* is one of the most frequently cited books of all time. Kuhn was especially influential among sociologists, in part because his ideas improved their image as scientists. According to traditional criteria, physics and biology are exemplary sciences: they have impressive histories with bodies of accumulated knowledge, widely recognized achievements, and persisting research programs that seem to be getting ever closer to the truth; sociology has none of those features. But Kuhn denies that such criteria are definitive of science (and that the approximation to truth is not even possible); to be a science a discipline requires a paradigm to govern its research, and such can be found in sociology. And the disputes among rival theories in sociology corresponds to Kuhn's description of a science in a young state, before a consensus has formed around a single paradigm.

Kuhn also provided sociologists with a new field of study: science itself. Before Kuhn, the standard explanation of changes in science was that they were demanded by rational assessments of the evidence; and since the study of rationality is within the domain of philosophy, the explanation of scientific change was considered a philosophical task. Only in the rare cases when a change went against the requirements of rationality could there be room for a sociological explanation. But Kuhn shows that the standard development of a science does have sociological determinants. Although he acknowledged such extra-scientific factors as the personality or nationality of a key figure, the sociological influences he emphasizes are almost always internal to the practice of science. For example, as a matter of group consensus the disciplinary matrix is a sociological phenomenon; but it is a consensus on a scientific exemplar, on scientific values, and so forth. Similarly, crisis is both sociological–a matter of shared doubt about the capacity of the existing paradigm to generate solutions to anomalies–and scientific, since the doubt is about scientific matters. Some sociologists sought to extend the range of explanations of scientific events to include extra-scientific factors such as power relations among scientists and between scientists and the society at large. Proponents of the "Strong Programme" in the sociology of science, founded by David Bloor and Barry Barnes, deny that rationality and irrationality play any explanatory role in the study of science. Kuhn repudiated the Strong Programme in a lecture at Harvard in 1991, though some have objected that he was attacking a caricature of the position.

Kuhn may have tried to distance himself from the Strong Programme because of his sensitivity over his reputation as an irrationalist about science. Lakatos had commented **[Comment/query to Contributor 7: In his paper in *Criticism and the Growth of Knowledge*? If not, where?]** that in Kuhn's view the motor of revolutionary change in science is "mob psychology." By the end of his career, however, Kuhn was no longer seen in this light: having started his professional life as an historian of science, he finished it as a philosopher of science. Some critics regard Kuhn as having moderated his views because of the attacks on him: under fire from professional philosophers, they claim, he gave up his radical but ultimately untenable position and retreated to one that, though more philosophically defensible, was not nearly as interesting. Others claim **[Comment/query to Contributor 8: I have changed your original statement, "it is more just to Kuhn to say that . . .": unless there is a consensus of critical opinion, we're not supposed to defend a particular position in the *DLB*.]** that while this evaluation may apply to the changes in his treatment of the concepts of paradigm and incommensurability, his philosophical work after *The Structure of Scientific Revolutions* consists largely of explications and reformulations of the ideas advanced there. Whereas in the book he expressed those insights in psychological terms, thus incurring accusations of irrationality from traditionalists who regarded psychological and rational explanations as incompatible, he later recast them in philosophical terms.

Kuhn died on 17 June 1996 of cancer of the throat and bronchial tubes. His failure, despite his wide influence, to leave behind anything like a Kuhnian "school" may be attributed to his shift from an historical, psychological, and sociological approach to a philosophical one. Had he developed his earlier insights, which in *The Structure of Scientific Revolutions* are thought-provoking but sketchy, he might have given more direction to research in the social sciences in which he was so influential. On the other hand, Kuhn's philosophical legacy now seems to have been shorter-lived than might have been expected. In the 1960s and 1970s Kuhn, Feyerabend, and Lakatos showed, in opposition to positivism, that the philosophy of science needed to take the history of science seriously, and departments of the history and philosophy of science were established at many English-speaking universities. Although such departments still thrive, this explicitly historical approach to the philosophy of science now appears to have been a temporary phenomenon. Kuhn's qualms about truth–and so, implicitly, about the notion of knowledge; his skepticism that scientific progress is anything more than improved puzzle-solving; and his view that the meanings of scientific terms are closely tied to the theories with which they are connected are symptomatic of the empiricist tradition to which he was objecting. **[Comment/query to Contributor 9: I deleted the references to Cartesianism, since it has not been mentioned previously in the entry.]** In part because of Kuhn, philosophers have repudiated much of empiricism; but in doing so they have rejected not only the views Kuhn himself criticized but have also rejected other empiricist assumptions that he implicitly endorsed. To many contemporary philosophers, the philosophy of science that is needed is one that is not historically grounded but one that is historically sensitive but still philosophically grounded. In this light one may regard Kuhn not as having provided a new paradigm in the philosophy of science but rather, like Copernicus and Planck, as inaugurating a revolution that went beyond what he himself foresaw.

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