Thomas S. Kuhn

1922–1996

BIOGRAPHICAL *Memoirs*

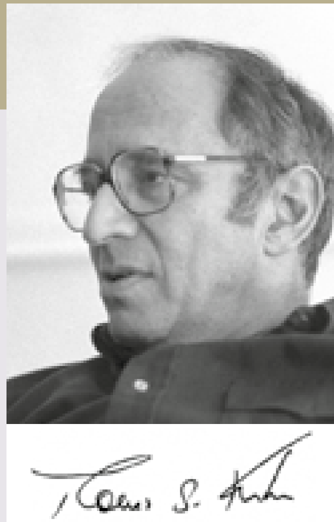
A Biographical Memoir by
N. M. Swerdlow

NATIONAL ACADEMY OF SCIENCES

THOMAS S. **KUHN**

July 18, 1922–July 17, 1996

Elected to the NAS, 1979



By N. M Swerdlow

Thomas S. Kuhn was the most important, and the most famous, historian and philosopher of science within living memory. *The Structure of Scientific Revolutions* has been read, or purchased, by more people than any book on either subject ever written—the closest competitors in philosophy must be the *Posterior Analytics* and the *Discourse on Method*—and the influence of the book has extended beyond its subject to something like common knowledge applicable to just about anything. This surprised Kuhn as his concern was answering very specific questions about the history and practice of science, and the language he used to describe it, which has become ubiquitous, was rather technical and recondite. And what is more surprising about achieving such fame is that his own interests were principally in physics, theoretical physics, and its history on a level that only those with advanced, graduate level, physics could hope to follow and which he never attempted to popularize.

Kuhn's father, Samuel Kuhn, from Cincinnati, was trained as a hydraulic engineer at Harvard and MIT, and his mother, Minette Stroock, who he later described as the intellectual in the family, was from New York. The family was Jewish, non-observant, and fairly prosperous. He was born in Cincinnati and brought to Manhattan at the age of six months where his younger brother Roger Kuhn was born. He attended a progressive school through fifth grade, and then the family moved to Croton-on-Hudson in Westchester County. There he entered sixth grade at the Hessian Hills School, a small progressive school with a decidedly left faculty, where the students were given a great deal of freedom to work on their own; it was here that he learned to explore his own interests and that he had an aptitude for mathematics. After a year at Solebury School, near New Hope, Pennsylvania, which he found less interesting, he entered the Taft School in Watertown, Connecticut in tenth grade, which he found even less interesting, for the science teaching was, as he later described it, "lousy" and he was discouraged from any attempt at thinking about kinetic theory on his own. But he did well, especially in his mathematics and science classes, and was accepted by Harvard in 1940. This was his school of choice, which he believed quite competitive, so his acceptance pleased him greatly, and only years later did he learn that he had little to worry about as his class consisted of 1016 students admitted out of 1024 eligible applicants. In his first term, he was surprised that he was not doing well in a physics course, averaging C on exams, but he soon figured out how to solve problems, which he latercalled "puzzles," and by his second year was majoring in physics and receiving A's from then on. He also wrote for the *Harvard Crimson*, and in his last year was head of the editorial board.

After the United States entered the war, he concentrated on electronics, and through an accelerated curriculum, including summers, graduated in 1943. Immediately following, he was engaged in war work at the Radio Research Laboratory at Harvard under John Van Vleck on radar countermeasure research, to find and defeatradar sites, and after a year was sent to work in England, France, and to inspect radar installations in Germany. Shortly after VE Day, he was back at Harvard, and, after the war ended, in the fall he began graduate work in physics. As an undergraduate, he had taken a course in the history of philosophy, and he took more courses in his first year in graduate school. Although he did not find the courses particularly good, he became more certain of his own interest in philosophical subjects. He did well enough in physics, although in his own view not spectacularly so, and, following his advanced courses, wrote a dissertation on solid state physics, on the cohesive energy of monovalent metals, under Van Vleck, which he completed in late 1948, receiving his PhD in 1949. He published three articles on physics, but already before he completed his dissertation, he was involved in a totally different area of research that was to determine his work for the rest of his life.

By "understanding science" Conant meant approaching science with the point of view of someone who has actually done it, which a scientist has and a layman does not, not so much from lack of knowledge as from lack of experience of the "Tactics and Strategy of Science."

As early as 1936, James Bryant Conant, then President of Harvard, had proposed a reform of liberal education that would concentrate on historical studies, to study, as he called it, the past development of our modern era. In 1943, he appointed a committee to write a report on the reform, published in 1945, called *General Education in a Free Society*, to this day an influential contribution to education in both secondary schools and universities. Because of his work on the *Crimson*, Kuhn had been the one student asked to write comments on the report for the committee, and when the report was published, wrote a summary for the *Harvard Alumni Bulletin*, which he later listed as the first publication in his bibliography. Among the recommendations of the report for Harvard specifically, reflecting Conant's own interests, was that general education courses in the sciences be given a significant, even central, historical component. In the fall of 1946, Conant described this method of instruction in three Terry Lectures at Yale, published the following year as *On Understanding Science*. By "understanding science" Conant meant approaching science with the point of view of someone who has actually done it, which a scientist has and a layman does not, not so much from lack of knowledge as from lack of experience of the "Tactics and Strategy of Science." The method he described was to teach the tactics and strategy of science to nonscientists through the use of case histories of important discoveries in the history of science, and in the fall of 1947 Conant himself began to do just that in a course called The Growth of the Experimental Sciences.

"I had not become an Aristotelian physicist as a result, but I had to some extent learned to think like one." This was the kind of understanding of science, to think like the earlier scientist, that Kuhn came to expect of himself and of those who knew enough to do the history of science in the right way.

Earlier that year, he had invited Kuhn to participate in the course as a teaching fellow and asked him to prepare a case history of Galileo's study of falling bodies and the pendulum. Kuhn, on his own, began by investigating the background to Galileo's mechanics, which led him back to the Aristotelian mechanics Galileo had refuted and replaced. He found that Aristotelian mechanics was not just wrong mechanics, not even what we would call mechanics, but different mechanics, and that it made a good deal of sense in its own way when the different meaning of words like "motion," "change," and "quality" were understood. He referred to this experience of discovering the sense of older, now incorrect, science in later publications, and it is an important insight for anyone who learns it—and it must be learned through experience—into the way to understand earlier science, to understand the history of science. "I had not become an Aristotelian physicist as a result, but I had to some extent learned to think like one." This was the kind of understanding of science, to think like the earlier scientist, that Kuhn came to expect of himself and of those who knew enough to do the history of science in the right way. Of course, he was hardly the first person to understand earlier science correctly or to see that it was so different from our own understanding of its subject that it is not the same thing, but he drew from it his own conclusion, that one cannot speak of an evolution from the earlier to the later science, that what occurred was a discontinuity, that it was not possible to explain or rephrase or translate the earlier science according to the understanding of the later science. One can, for example describe separately Aristotelian and Newtonian mechanics, concerning the motion of bodies, and also describe their differences, but one cannot explain, understand, one in terms of, or by means of, the other. This, strictly speaking, is a scientific revolution.

In September of 1948 Kuhn was appointed a Junior Fellow in the Harvard Society of Fellows. At the time he was completing his dissertation in physics, which was typed by his fiancé Kathryn (Kay) Muhs, and shortly after submitting the dissertation, Tom and Kay were married. (They had three children, Sarah (1952) and Elizabeth (Liza, 1954) while in Cambridge, Nathaniel (Nat, 1958) when in Berkeley.) He then turned from physics to reading in the history and philosophy of science, which he had by then decided would be his field of research and teaching, and about which he so far knew little. He had in 1947 given the lectures on early mechanics, from Aristotle to Galileo, but then did not teach until in 1950 he again lectured in the natural science course, now called Research Patterns in Physical Science. Over a period of years, a number of case histories for the natural science course were published separately and eventually collected in two volumes called *Harvard Case Studies in Experimental Science* (1957). But these were strictly experimental sciences, and included nothing of Kuhn's courses in theoretical sciences, mechanics and, later, astronomy. When his Junior Fellowship ended in 1951, he was appointed Instructor in the General Education program and in 1952 Assistant Professor of General Education and the History of Science. He had written a series of lectures, delivered at the Lowell Institute in 1951, called *The Quest for Physical Theory: Problems in the Methodology of Scientific Research*, his first attempt at presenting his ideas about the historical development of science, but decided that they were not yet ready for publication. But one of the courses in the natural science program, the Copernican Revolution, did lead to a publication. In 1954 he received a Guggenheim Fellowship to complete a book on the subject and to begin work on a project called "The Structure of Scientific Revolutions," related to his Lowell Lectures, that he had consented to write for the long-standing, since 1938, publication of the Vienna Circle, now in the United States, the *International Encyclopedia of Unified Science*. Each was to take longer than anticipated. By late 1955, the Copernican Revolution was still an uncompleted manuscript of over 500 pages, the work on Scientific Revolutions was not begun, and it was becoming clear that Kuhn did not have a future at Harvard. Fortunately, the prospect of an appointment at the University of California, Berkeley, appeared in early 1956, and in the spring he was offered an Assistant Professorship of the History of Science in the Departments of History and Philosophy. He accepted, and shortly before his move to Berkeley, Harvard University Press accepted the book on the Copernican Revolution, which appeared in 1957.

The Copernican Revolution: Planetary Astronomy in the Development of Western Thought proved to be an important book, far more so than any of the other case histories, which, as interesting as they are, remained purely instructional, setting out what soon became the most widely read and authoritative treatment of the reasons for and the adoption of the heliocentric theory. But that is only part of the story, for the book is on a much larger subject, namely, the transformation over a period of two thousand years from Aristotelian to Newtonian mechanics, astronomy, and cosmology along with the transformations in world-view, to use a dated but here appropriate term, that accompanied and followed from it. The subject was not what Kuhn knew best, which was modern physics, but his intelligence and judgment more than made up for that. He brought to the book a penetrating intellect, discovering and explaining the steps in this long process, and while the history is not exhaustive, within its limits of technicality and length, of just under three hundred pages, it treats the entire subject with great insight and clarity, and with good reason is still widely read. (The one other work to this day that treats this subject, more comprehensively and on a more advanced level, although less specifically on the heliocentric theory, is E. J. Dijksteruis's *The Mechanization of the World Picture*, which appeared in Dutch in 1950 and English in 1960.) In the years of writing, Kuhn became immersed in an astonishing range of historical studies, to name only the most important: Aristotelian mechanics and cosmology; Ptolemaic mathematical astronomy; the medieval scholastic tradition in mechanics and cosmology that grew out of Aristotle; the medieval astronomical tradition that grew out of Ptolemy; the contribution of humanism to the recovery of ancient science and philosophy; and all this before Copernicus. In his examination of the history beginning with Copernicus, he set out an explanation, still cited and discussed, of Copernicus's reasons for proposing and supporting his new theory, within the limitations of what Copernicus himself could do, and following this, of the essential contributions of the observational discoveries of Tycho, although an opponent to Copernican theory, of Kepler, for describing for the first time the correct mathematical theory of the planets and a non-Newtonian mechanics of their motion, and of Galileo, for providing the most influential evidence for the theory in his discoveries with the telescope and his (flawed but apparently convincing) arguments in his *Dialogue on the Two*

Kuhn examines the problems and the solution through reprinting the text of the dedication and first ten chapters of *De revolutionibus*, the most readable parts of Copernicus's book, where these matters are considered, with an analysis of Copernicus's own understanding of the problems and why his solution appeared, at least to him, to be correct.

Great Systems of the World. Kuhn's last steps detour into corpuscularism, Cartesian and otherwise, in which he had independent interests, seemingly far from astronomy, but, as he knew, essential to Newton's unification of mechanics and astronomy, now as celestial mechanics, in his proof that everything in the world, from a particle to a planet, is subject to the same laws of mechanics and the same law of gravity.

And there is yet more. Kuhn had not left behind his intention to write about how changes, small and large, which are quite different, occur in the sciences, why and how science has a history. How does it come about that a "conceptual scheme," descriptions and theories, believed for the best scientific reasons, empirical and theoretical, as well as extra-scientific reasons, philosophical, psychological, even theological, is superseded by different, even radically different, descriptions and theories? This remained the center of his interest. And what larger change could there be than that from Aristotelian to Newtonian mechanics and cosmology, with Copernicus's heliocentric theory and the motion of the earth as the essential or necessitating reason for that change? The answer turned on discrepancies, but not in a simple way. Empirical discrepancies alone are not decisive because the observations or measurements showing them can be defective and adjustments in parameters can fix them without changing underlying theory. Nor are theoretical discrepancies, as conflicts between different models to account for the same observations, decisive because models, like parameters, can be adjusted, and there still may be no way of deciding whether one or the other is correct. "What is it that transforms an apparently temporary discrepancy into an inescapable conflict?" The answer here is found in Copernicus's own description of the state of astronomy, with theoretical objections to its incoherence, no unified and consistent order and scale of distances of the planets, and physical impossibilities, violations of the principle of uniform circular motion. Although the word is not used, this is what Kuhn later called a "crisis," requiring, not adjustment of the flawed theory, but the search for a replacement, for a new foundation that provides a solution to all the problems. And what is it that convinced Copernicus that his heliocentric theory provided the solution, that it was true? Since there were then no empirical criteria for distinguishing geocentric and heliocentric theory, the answer was in criteria that Kuhn calls "aesthetic," that relations of characteristic planetary phenomena and motions that are arbitrary, rules without an underlying cause, in geocentric theory, have a specific cause in the order and periods of the planets in heliocentric theory. Kuhn examines the problems and the solution through reprinting the text of the dedication and first ten chapters of *De revolutionibus*, the most readable parts of Copernicus's book, where these matters are considered, with an analysis of Copernicus's own understanding of the problems and why his solution appeared, at least to him, correct. But the Copernican Revolution was not complete, for Copernicus's own arguments, which he could have expressed more clearly, while convincing to him were convincing to few others, and if he were correct, the consequence was a great number of new problems, for an innovation in one science may create any number of difficulties for other sciences, in this case for bodies on and near the rotating earth, for mechanics, and beyond the sciences for philosophy, even for theology (not really solved to this day). The last two chapters are devoted to these issues as well as to later discoveries in astronomy, to the contributions of, among others, Tycho, Kepler, Galileo, Descartes, and Newton referred to earlier. So here was the story of a very great scientific revolution. Was it in form the story of others?

That was to be the subject of "The Structure of Scientific Revolutions" for the *International Encyclopedia of Unified Sciences*, which Kuhn believed could be completed rapidly after the book on the Copernican Revolution. Of course it took longer. During his first years at Berkeley, he was immersed in teaching new courses in the history and philosophy of science, including a survey of the history of science from antiquity to the early twentieth century—it was then considered that after, say, 1913-1916 was not yet history—a very time-consuming task as anyone who has attempted it knows. In 1958 he was promoted to Associate Professor with tenure in the Departments of History and Philosophy, and then began a year at the Center for Advanced Study in the Behavioral Sciences at Stanford, intending to complete the monograph on Scientific Revolutions. After a year, he had only completed drafts of the first chapter, but he had also given the entire subject a great deal of thought and formulated what he called a "paradigm" to described the received theory and practice of "normal science," and that, he later said, was the essential step after which the remainder of the work came rapidly. In the next year or so he completed a draft of the entire work, which he sent for comments, and, with only slight revision, submitted the final version to The University of Chicago Press, from which he received the promise that the work would appear separately in addition to its inclusion in the *Encyclopedia. The Structure of Scientific Revolutions* appeared in 1962, and in a second edition in 1970, Kuhn added a Postscript–1969 commenting on questions that had been raised since the original publication. The literature on it has continued ever since. The book has been translated into more than twenty languages and has sold over a million copies. Kuhn referred to it as an "essay"—"this work remains an essay rather than the full-scale book my subject will ultimately demand"—and even though quite a long one, that is an appropriate term.

The description of the practice of science and a scientific revolution in *Structure* is very well known and can be stated briefly although the important words in the description are terms of art with specific meanings subject to much discussion: Normal science, characterized as puzzle-solving, is practiced according to a paradigm, the examples of science and practice, theories and procedures, of a community of scientists, which may be large or small depending upon the subject of research. Normal science, in addition to successful solutions, turns up anomalies, not obviously contained within or explained by prevailing theory, or compatible with all empirical evidence. Most of the time, further research shows that the anomalies are due to errors, as of observation or steps of derivation, sometimes just miscalculation, or are in fact explained by prevailing theory, perhaps with some adjustment or enlargement that does not threaten, and may even strengthen, the theory. In this sense, normal science constantly refines and adds to science; it is not just worthwhile, but essential to the practice of science, it is the practice of science. But in some cases no correction of observations or derivations, no adjustment or enlargement of theory, succeeds in removing or explaining the anomalies. This is what is called a crisis, and the resolution of a crisis, if and when it comes, is a change, in theory but also often of the paradigm of theories and procedures in the practice of a science, so fundamental as to be called a scientific revolution. These may be large, affecting the foundations of an entire science, or limited, affecting one part of a particular science, at least initially, but the term is usually reserved for the large transformations, as Newtonian mechanics or special and general relativity or quantum theory and later quantum mechanics. And here we see the result of such a scientific revolution, that it affects, not just the science that produced it, but related sciences, Newtonian mechanics beyond mechanics to fundamental transformations of astronomy, and later the physical sciences in general, special and general relativity to transformations of mechanics, electromagnetism, and cosmology, quantum theory and quantum mechanics to electromagnetism and atomic theory; and beyond these, even to transformations in world-view, to use that dated but here appropriate term again. Further, the transformations may be so fundamental that the understanding of the science itself, of the very meaning of words, differ, that the earlier and later understanding are incommensurable, which makes the decision between them difficult at the time and continues to pose an obstacle to the historical understanding of just what it is that happened. And the adjustment of other sciences may take a great deal of time and work, in discovering the applications of the required transformation, which may itself be the practice of normal science; and the effect on world-view may take still longer or never happen at all.

This brief review, although I believe close to correct as a description, is really in no way adequate, first, and the most important, because Structure contains and depends upon detail and illustration, from examples of the history of science, that cannot be captured in a summary, which is arid by comparison; then because every notable word mentioned in this review, every term of art, all so well known that I need not distinguish them by italics or quotation marks, has been subject to endless discussion and interpretation, beginning in the essay itself; and every step described has also been subject to discussion and interpretation, also to criticism, for something now close to fifty years. And the description here is limited to the steps in the practice of science, more or less following the essay, without taking into account factors that may enter from outside the sciences, which are mentioned in the essay and have received enormous enlargement since its publication—let alone specifying what is meant by a world-view. Further, *Structure* goes well beyond the outline given here to consider other issues in the history and philosophy of science, and these have also been subject to extensive later discussion. There are, however, a few points that can be added to this review. First, the essay is not about the philosophy of science, at least not as usually understood, that is, about the correct method of science, about evidence and proof and such, but about what can be called a philosophy of the history of science—and perhaps philosophy is not the right word, perhaps better is how to understand the history of science—not about the static or idealized method of science, but about the dynamic development, the history, of science, not what science should do, but what it actually does do or has done. For a principal point, in fact the first point in its Introduction, is that this can only be understood from science itself and from its history. Second, the essay has been subject to an enormous secondary literature, mostly by philosophers or historians who would be philosophers, more of it critical than explanatory, differing on points of philosophy or on examples of its application, although Kuhn never suggested that its application was universal and has written clarifications and additions in later publications, and for years now this literature has concentrated on specific points, even specific words, so that the argument itself has been, although not forgotten, buried under minutia. (I leave aside its appearance, in other fields and in popular usage, which misunderstand and misapply it completely.) But third, the essay really does stand on its own apart from all the literature on it, and is and will remain of greater interest, more worth reading, than all of this literature. One reason that the essay achieved such great fame is that it is truly interesting, even exciting; this was the expression of readers. In writing this memoir, I reread the essay, which I had not read for many years, and found that it only became better; that as I had learned more I could better appreciate what Kuhn had accomplished, and also found it so clear that I wonder about the need for all the secondary literature. I did also read some amount of the secondary literature—no one could read anything close to all of it—and found it by comparison, let us say, academic. Conclusion: do read the essay; pass over the later literature in silence.

During the period he was writing *Structure*, in 1960, Kuhn received an offer of a professorship in the history of science at Johns Hopkins with generous support and the promise of three or four additional appointments. When he brought this to the attention of the History and Philosophy Departments at Berkeley, he was asked what would be required to keep him. He mentioned promotion to professor, additional appointments, and a PhD field in history of science within philosophy, which he considered his principal department, in which he had students of the history of science. The results were curious. He was informed that he would be promoted, that there would be an additional appointment, but that the philosophy department had no interest in a field in the history of science or in having him continue as a member of the department as he was not considered a philosopher. This really happened, and so the philosophy department at Berkeley has the distinction of having thrown out the most distinguished philosopher of science since, well, make your own choice, certainly of our time. Since he had, perhaps too optimistically, declined the offer from Hopkins, he stayed, but Berkeley was of less and less interest. In 1963 he accepted an offer from the Program in History and Philosophy of Science at Princeton, represented principally by Charles Gillispie in history and Carl Hempel in philosophy, and began there in the fall of 1964. The program, affiliated with the Departments of History and Philosophy, turned out to be well intentioned but did not really work. The students, of which there were a fair number, called the program history or philosophy of science, few of the students were up to the technical level of Kuhn's courses, fewer still up to writing a dissertation under his direction, and eventually the philosophy part of the program was discontinued. In 1972 he took up a half-time appointment at The Institute for Advanced Study as a long-term member, and for the rest of the time he was in Princeton, that is where he worked on his largest, most difficult, and most important historical study. This in itself has a history.

During the period 1961-64 at Berkeley, Kuhn was the director of the *Sources for the History of Quantum Physics*, defined as the period 1898-1933, later extended to 1950, supported by the National Science Foundation and supervised by a joint committee of the American Physical Society and the American Philosophical Society. The assistant director was John Heilbron, the senior editor and archivist Paul Forman, and Lini Allen the administrative officer. The object was to conduct interviews with scientists still living who had worked in the field and to catalogue and microfilm manuscripts, e.g. correspondence, drafts of papers, laboratory records, and such. The initial results were described, catalogued, in *Sources for History of Quantum Physics: An Inventory and Report* (Philadelphia, American Philosophical Society, 1967), and the project continued until it amounted to over one hundred interviews running to hundreds of hours of sound recordings with transcriptions and three hundred reels of microfilm. Kuhn himself conducted many of the interviews, from which he learned that, when compared to the contemporary records, published and unpublished, Erinnerungen were frequently unreliable, something that also applies to written reminiscences. Beware of scientists bearing memoirs. Still, the entire project proved of great importance for the history of modern physics, with publications based upon the collection if not on the interviews, beginning with a paper written with John Heilbron on the Bohr atom (1969), although only a fraction of the materials collected has thus far been utilized. It also served as an immersion in quantum physics for Kuhn, and in that contributed to his final historical work, on the origins of quantum theory.

Black-Body Theory and the Quantum Discontinuity, 1894-1912 (Oxford, 1978) was intended by Kuhn as a contribution to the history of science as he believed it should be done, “the most fully realized illustration of the concept of history of science basic to my historical publications,” with complete understanding of the science and very close attention to every detail of the sources and evidence. It was also modern physics, the history of science he could do best. The received account of Planck’s derivation of his law for black-body radiation is that the energy, dependent upon frequency and temperature, of linear electrical “resonators,” as he called the things that absorb and emit energy, later called “oscillators,”with frequency ν was restricted integral multiples of the energy “element” $\varepsilon = h\nu$, later called the energy “quantum,” the constant of proportionality h later known as Plank’s constant; thus the energy was discontinuous. It was known that Planck’s derivation depended upon Boltzmann’s derivation in 1877 and later in 1896 of the *H*-theorem, of the increase of entropy in kinetic theory of gases, using a distribution of the total energy of a gas into cells of size ε, but that did not assume that the energy, the velocity of molecules, was actually discontinuous. Both Planck's and Boltzmann's investigations were of the maximum entropy, the most probable, equilibrium states of systems with energies of random distributions, the difference was that in Boltzmann's derivation the size of the cells ε, of molecules with different velocities, was arbitrary, while for Planck's the cells, of resonators with different frequencies, were proportional to multiples of $\varepsilon = h\nu$. Through an examination of Planck's series of papers thermal radiation from 1894, including those on Wien's displacement law, to the papers of 1900-1901 using discrete energy elements, Kuhn found that Planck did not require or mention actual discrete energy states for resonators any more than Boltzmann required actual discrete velocities of molecules, the assumption of which was in both cases for the derivation of the most probable, equilibrium state of the energy of a gas or of thermal radiation. Hence, there was at the time no real discontinuity in Planck's derivation, which was of a continuum theory of thermal radiation. And the same is true of Planck's derivation in his *Lectures on the Theory of Thermal Radiation* of 1906, likewise of a continuum theory. This is not to say that Planck's derivation was correct, only that it did not require discontinuity, as Planck then understood it. There was no question that Planck's formula accurately represented black-body radiation, as was confirmed by further experimental evidence; the question concerned its derivation and physical interpretation.

The isolation of the problems in Planck's derivation, and the recognition of the necessity of a real discontinuity, form the second part of Kuhn's study. Here he considers the contributions of Ehernfest, Einstein, and Lorentz. Each came to the subject in different ways, but what all had in common was a recognition that a continuum theory of radiation must lead to the Rayleigh-Jeans law, anticipated by Lorenz in 1903 for long wave lengths, low frequencies, but generally known after Jeans's publication of 1905, which leads to excessively high energy levels at high frequencies, what Ehernfest called the "ultraviolet catastrophe,"as was recognized both theoretically and experimentally. Hence, it cannot be correct. But what can Planck's law, which did correctly describe the distribution with frequency, possibly mean? It was Einstein, in a paper of 1906, following his, later famous but then generally discounted, 1905 paper on the "heuristic viewpoint" of light particles, who showed, through his own derivation, that if the energy of resonators varies continuously, can take any value, the result must be the Rayleigh-Jeans law, and that the Planck distribution *requires* that the energy is in fact restricted to integral multiples of ε, that, as he put it, Planck's theory implicitly makes use of the "light-quantum hypothesis." And in 1909, he pointed out that Planck's theory *must be modified* to require integral multiples of $\varepsilon = h\nu$, clearly recognizing that Planck himself did not require it. The most influential recognition came from Lorentz. In 1908 he presented another derivation of the Rayleigh-Jeans law for longer wave lengths, acknowledging the problem for shorter, and suggesting, in agreement with Jeans, that the experimental results for shorter wavelengths may be subject to effects due to failure of the radiation to reach equilibrium. He hoped that further experiments would allow a decision between Jeans and Planck, whose theory he did not yet recognize as requiring discontinuity. This conclusion did not go without criticism. Lummer and Pringsheim, who had carried out the most recent experiments on cavity radiation, immediately pointed out that according to the "Jeans-Lorentz formula," a black body, and substances tested experimentally, as steel, would glow in the dark, emit visible light, at room temperature. Lorentz's response was to concede that Jeans's law could no longer be defended and that only Planck's was possible. And in correspondence with Planck, recognized that the energy of resonators must be discrete, which he presented in a lecture in Utrecht in 1909. Planck's own recognition of the necessity of discontinuity came at the same time, and it was in 1910 that he changed the name of the thing that absorbs and emits energy from "resonator" to "oscillator" and, more significantly, of the discrete unit of energy $\varepsilon = h\nu$ from "element" to "quantum," which, as is worthy of notice, means "what amount." Still, Planck continued to have difficulties with discontinuity, in the second edition of his *Lectures* of 1912 considering the possibility that the absorption of energy is continuous while emission occurs in discrete units of $h\nu$ only when some threshold is reached; and over a decade he worked through various theories of the relation of continuous and discontinuous radiation. Planck's later theories are the subject of an epilogue, the third part of Kuhn's study.

The number of readers of the *Black-Body* book has not been large since it requires a minimum of graduate level electromagnetism, thermodynamics, and statistical mechanics as well as a more than basic understanding of electron theory and quantum theory as of 1900-1912. Kuhn was writing for his peers, and there were not many. The reception was at best respectful. Some who attempted to read it, and I emphasize attempted, were frustrated not to find the familiar language of *Structure*; in fact, although that is not what Kuhn was looking for, it was a near perfect example of just such a revolution, including all that followed from it, as anyone who understands it can see. The same readers were also disappointed not to find sociology of science, not that Kuhn had ever written on it, and seemed resentful that the physics was over their heads, that Kuhn was an "internalist," which means writing about the science. Also troubling was the response of physicists, especially of those who had earlier contributed to quantum mechanics—no one who worked in the old quantum theory was still living—who would consider no alternative to the received account that Planck discovered the quantum and discontinuity in 1900, and anything that suggested otherwise was just due to confusion or lack of clarity in early formulations. Perhaps these reactions were to be expected. What did disturb Kuhn was a review by his old friend, who really was his peer, Martin Klein, who argued that Kuhn was wrong, that Planck did require a real discontinuity in 1900, even if his Lectures of 1906 present only a continuum theory of radiation inconsistent with his earlier papers. Kuhn, he wrote, was trying too hard to establish internal consistency in Planck's position, and seems unwilling to consider that Planck himself was not always completely clear about what he was doing. But one of Kuhn's points was that Planck was consistent, and was clear, if the radiation was understood as continuous, and that the inconsistencies arose only when it was understood as discontinuous in 1900 and continuous in 1906. In 1984 he published a paper, "Revisiting Planck," going over the arguments and evidence again, but less technically, including the evidence that could be cited against his interpretation, showing that properly understood, this very evidence actually supported and strengthened his position, and was entirely consistent with it. The paper also addresses the issue of why one should not invoke being "confused" to explain what appear to be inconsistencies in the writings of earlier scientists—not that scientists do not make mistakes—as it is more likely that we are imposing upon them what we know to be correct, or later consequences of their work, rather than understanding what they wrote, which was actually something different.

In 1978 his marriage to Kay ended, and in 1979 he left Princeton for MIT as Professor of Philosophy in the Department of Linguistics and Philosophy, in 1983 as Laurance S. Rockefeller Professor. In 1982 he married Jehane Burns, who he had met shortly after moving to Boston. (Kay, Jehane, his children Sara, Liza, Nat, and younger brother Roger survive him.) From this point on his work can be described as the philosophy of the history of science, or how to understand the history of science. He had, as he once remarked, paid his debt to the history of science in the *Black-Body* book, and now he was again taking up the subjects of *Structure* with a deeper understanding of what he had proposed in outline so many years ago and a closer attention to detail. The issue to which he gave the most attention was incommensurability, which he now considered more in terms of language, that the lexicon of different sciences, both their empirical and theoretical content, of what is in principle the same thing, may have such distinct meanings that there is no way of translating between them. The historian, for the concern here is, not the scientist doing his work, but the historian doing his work, must learn to understand both, learn the lexicon of both, and this can only be done by treating each science separately as there is no way of translating from one to another. It is rather like the example given earlier, that one can describe separately Aristotelian and Newtonian mechanics, and also describe their differences, but one cannot explain either one in terms of the other. He wrote articles on this issue, and also treated it at greater length, along with related questions, in series of lectures, *Scientific Development and Lexical Change*, delivered at Johns Hopkins in 1984, and *The Presence of Past Science*, at University College, London, in 1987. These were never completed to his satisfaction, and have not been published. What he was trying to do, as always, was get it right, and he was his own most severe critic.

Let me close with a personal reminiscence. Tom Kuhn thought about things more deeply than anyone I have known, something that came through when you asked for his advice. His knowledge of the sciences and of the history and philosophy of science was extensive, but what he knew best came more from thinking than from reading, and his thinking was like his speaking, alive, intense. “Look, …,” he would say. Really original people are better off thinking than reading, really original people don’t have to read. But whether he knew about a particular subject or not didn’t matter because if he did not, he would ask questions, sometimes just one, that got right to the heart of what was important. If you could answer his questions, you knew what you were doing; if you could not answer his questions, you did not know what you were doing and you had better think a lot harder. It was as simple as that. Science is serious, great science is profound, and Tom’s first concern was to understand it, however difficult it might be, and to make sure that you understand it. Some years ago Tom called me because he had learned from Jed Buchwald that I thought Galileo was right about something paradoxical in the *Dialogue on the Two Great Systems of the World* that Tom, who had probably not looked at the book for thirty years, just knew could not be correct. So I looked at it again, and again, and sure enough, there was a mistake that had been too subtle for my dense mind to catch. Tom knew it, Tom was right, as usual. We both agreed that it was too bad because we wanted Galileo to be right. Galileo was also an original, and preferred thinking to reading.

Kuhn was the recipient of many honors. He received his first honorary degree from Notre Dame in 1973, and eight more followed, including Columbia, Chicago, and Padua. In addition to the National Academy (to which he was elected in 1979), he was a member of several learned societies, among them the American Philosophical Society, American Academy of Arts and Sciences, the Leopoldina Academy, and the Académie

Internationale d'Histoire des Sciences. He was President of the History of Science Society, 1968–70, and of the Philosophy of Science Association, 1988–90. In May of 1990, before he became Professor Emeritus in 1991, there was a conference in his honor at MIT with papers by philosophers and historians, most of whom he had known for many years, reflecting upon aspects of his work in relation to their own very diverse interests. In the published volume, *World Changes, Thomas Kuhn and the Nature of Science*, ed. by Paul Horwich, MIT Press, 1993, he provided his usual astute comments on all of the papers.

NOTES

There is a large literature on Kuhn and his work, numerous books, including collections, and articles. The few following are of interest for biography and for his best-known publications.

A Discussion with Thomas S. Kuhn: A Physicist who Became a Historian for Philosophical Purposes. This is the closest Kuhn came to an autobiography. Listed in the bibliography, 1997, and reprinted in *The Road since Structure*, 2000, which contains a complete bibliography of Kuhn's publications.

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Published since 1877, *Biographical Memoirs* are brief biographies of deceased National Academy of Sciences members, written by those who knew them or their work. These biographies provide personal and scholarly views of America's most distinguished researchers and a biographical history of U.S. science. *Biographical Memoirs* are freely available online at www.nasonline.org/memoirs.