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Thomas S. Kuhn (1922–1996)

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Thomas S. Kuhn's second monograph, *The Structure of Scientific Revolutions* (1962) is the most widely read and most influential book on the philosophy of science of the twentieth century. It spawned the ubiquitous use of the term "paradigm" in popular culture, including cartoons and business management courses, and a million copies have been sold in almost twenty languages.

The central thesis of the book is that the nature of scientific development had been seriously misunderstood by philosophers and scientists, and that, in the words of the opening sentence: "History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed." The image he sought to transform was one in which science is cumulative, varying in the speed of its progress, but always moving forward, an image in which scientific controversies are a small and unimportant part of the process, friction in the wheels of progress.

The contrasting image he championed portrays mature sciences as alternating between two kinds of change. The first are periods of cumulative progress in which scientists apply generally accepted theories to the unresolved questions in a domain according to a shared understanding of what constitutes a reasonable scientific question and of what criteria are used to judge answers. This "normal science" is a very sophisticated form of puzzle-solving and can require great ingenuity, but occurs within a stable framework of tradition. In contrast, the alternating periods of "revolutionary science" consist of confrontation between two diverse understandings of what constitutes a reasonable question and what criteria should be used to adjudicate disputes. In *The Structure of Scientific Revolutions* Kuhn used the term "paradigm" to both define and explain the difference between the two kinds of science: normal science consists of the elaboration of an accepted paradigm, while revolutionary science consists of the overthrow, or attempt to overthrow, an accepted paradigm. In addition, the notion of paradigm played an essential role in distinguishing prescientific preludes to a science, for example optics before Newton, because the critical step in making the transition to a science consisted of convergence by a scientific community on a paradigm.

Reactions to the book by philosophers and natural scientists were numerous, vociferous, and almost all negative. Some critics said that Kuhn made most of science – normal science – seem pedestrian and almost unnecessary, in spite of his clear

insistence that it was only the persistent pursuit of puzzles by first-rate minds that would eventually generate the anomalies which would lead to revolutionary science. But the majority of critics focused instead on the account of revolutionary science; according to many, the processes of revolutionary change as described by Kuhn constituted irrational mob rule and were antithetical to the view of science as the epitome of reason. Much of the attention centered on the claim that opposing paradigms are incommensurable, that the meanings and often the referents of the terms of the theories differ so that no direct simple comparison between them is possible. The reaction among social scientists was more mixed; some embraced the central themes and became obsessed with whether their field had yet completed its preparadigmatic preparation for sciencehood.

The analytic apparatus of the book, especially the central notion of a paradigm, came in for particularly severe scrutiny. One reviewer discerned twenty-two distinguishable senses of "paradigm" in the text. Kuhn was astounded at what he saw as widespread misunderstanding and misrepresentation of his ideas, but recognized the need to clarify the central notion of paradigm and related apparatus. The first fruition of this rethinking appeared in a number of papers during the late 1960s and in a "Postscript," which was published in the second edition of *Structure* in 1970. Before elaborating on the modification in the "Postscript," it will be useful to sketch some of the path by which he reached the views behind *Structure*.

Kuhn's status as a philosopher is difficult to assess because his training, career, and indeed the nature of his influence are very unusual. Thomas S. Kuhn was born in Cincinnati, Ohio in 1922 and received his Bachelor's (1943), Master's (1946) and doctoral degrees (1949) from Harvard University in physics. He only began to read seriously in the history of science when asked by James B. Conant, then President of Harvard, to assist in preparing a historically oriented undergraduate science course for non-science students.

A pivotal moment occurred in 1947 while he was reading Aristotle, trying to ascertain how much mechanics Aristotle understood. His conclusion was that Aristotle understood little or no mechanics and indeed seemed to be a poor observer and unsystematic scientist. He was puzzled by how one of the greatest intellects in the history of western thought could have been so confused. Then, suddenly, "the fragments in my head sorted themselves out in a new way, and fell into place together. My jaw dropped, for all at once Aristotle seemed a very good physicist indeed, but of a sort I'd never dreamed possible" (Thalheimer lecture p. 32). Among the pieces that had sorted themselves out was the insight that for Aristotle, the Greek expression that is translated as "motion" means not only a change of location, but any of a wide variety of changes, of which change of location is only one. Looking at the world in this new way with this transformed vocabulary, Aristotelian mechanics made very good sense of many observations, albeit many of those observations would not be regarded as relevant to modern mechanics.

After completing his dissertation in physics, he spent three years as a member of the Harvard Society of Fellows broadening his historical and philosophical knowledge. Then ensued an appointment teaching history of science at Berkeley in which much of his time, by his own observation, was spent preparing lectures in a field in which he had no formal training. In 1957 he published *The Copernican Revolution*, a well-received

account of the conceptual and technical obstacles to making the transition from a geocentric to a heliocentric universe. The central ideas of *Structure* are discernible in this first book, but the claims are much narrower and generally less philosophical. There is considerable focus on the idea that the transitions from the Aristotelian–Ptolemaic universe to the Copernican–Galilean–Newtonian one are not transitions that can be arrived at by small incremental steps. To see the universe as centered on the sun, not on the earth, can only be accomplished as a dramatic change. But no wider claims are made in the first book about how widespread this kind of transformation has been in the history of science.

Structure represented the generalization of that idea to the larger canvas of the physical sciences generally. The preface to *Structure* indicates in some detail the extent to which he is aware that there are serious gaps and shortcomings in the philosophical development of key concepts. However, he had contracted to produce a monograph within fairly severe size limits and the editors were pressing him to complete the manuscript.

One little known ironical aspect of the publication of the book is that although the logical positivist conception of science is a primary target of Kuhn's criticisms, the monograph was first published as Volume II, no. 2, of *The Encyclopedia of Unified Science*, the publishing organ of the logical positivist movement. The editors responsible for soliciting and encouraging the manuscript, Charles Morris and Rudolph Carnap, were enthusiastic about the monograph and its importance for philosophy of science. The second edition of the book indicated its status as part of the *Encyclopedia* less saliently, and by the third edition in 1996 no mention is made of the original imprimatur.

In 1964 Kuhn moved from Berkeley to Princeton, becoming a member of the history department but also joining the graduate program in the history and philosophy of science. In seminars there, as well as in lectures and correspondence, he revised and clarified the ideas of *Structure*. In particular, the use of "paradigm" was to be replaced by either "disciplinary matrix" or "exemplar," thus recognizing a major two-fold ambiguity in the original term. A disciplinary matrix consists of symbolic generalizations, metaphysical assumptions, models, values, instruments, and exemplars. Thus a disciplinary matrix is a constellation of elements which define a world-view and characterize a scientific community. Since a disciplinary matrix contains many elements, there can be varying degrees of congruence among members of a community.

The symbolic generalizations are the most familiar element; these would be equations such as Newton's Laws or Boyle's Law. Metaphysical assumptions concern the basic elements of the universe; examples would be the assumption that a vacuum is impossible, that action-at-a-distance is impossible, that the universe is governed by deterministic laws, and that all matter consists of atoms in a void. Models are easier to illustrate than describe: the model of the atom as a miniature solar system, the model of a gas as a collection of a large number of very small particles in rapid motion, heat as a fluid, and so on. Values include simplicity, generality or scope, accuracy, reproducibility of results.

Exemplars, which are both an element of disciplinary matrices, but also a significant second sense of paradigm, are examples of notable scientific accomplishment which set a standard for future researchers. For example, the rigor of Euclid's geometry was an exemplar for many disciplines, and the number of fields that have proclaimed

Copernican revolutions is legion. More recent examples might be the predicted discovery of Uranus or the discovery of the double helical structure of DNA. The exemplars provide a glue for the elements of a disciplinary matrix by bringing together the examples in concrete accomplishments. It is an essential part of the Kuhnian picture that the examples can be extended in various ways, so that the exemplars provide guidance but not rules.

A major emphasis in Kuhn's discussion of scientific change was the sudden and involuntary transformation of perception and belief. This clearly originated in his own experience in understanding Aristotle and he illustrated it by giving examples of Gestalt switch figures in his book, for example, a line drawing which can be seen either as an old woman from one perspective or a young woman from another. These ideas biographically stemmed back to Kuhn's experience in 1947, but others had also been struck by similar ideas, and an articulate presentation of them could be found in N. R. Hanson's *Patterns of Scientific Discovery* four years before the appearance of *Structure*, which refers approvingly to Hanson in a number of places.

Kuhn was particularly perplexed and frustrated by the accusation that he was undermining the rationality of science. He strongly believed that science is an epitome of rationality, and thus the processes involved in the development of science, including both normal and revolutionary science, must be essential ingredients in the rationality of science. His goal in overthrowing the accepted image of scientific processes was to cast aside a false understanding of rationality and to begin the process of replacing it with a more sophisticated and historically accurate apprehension.

He made one strategic decision in completing the manuscript of *Structure* and publishing his ideas in that abbreviated and highly unfinished form. He made a second decision in the late 1960s to publish the "Postscript" at the end of the second edition of *Structure*, rather than attempting a thorough revision that would systematically replace the occurrences of the ambiguous "paradigm" with the appropriate term from the vocabulary of disciplinary matrices and exemplars. This meant that even after 1970 new readers of *Structure* became aware of the extensive terminological and conceptual changes only after reading the original 170-page text and being thoroughly immersed in the sweeping and ambiguous vocabulary of "paradigms."

This decision was the result of Kuhn's recognition that reworking *Structure* was not a very good option since he was still in the midst of changing his views, and so the "Postscript" strategy was a stopgap until he could reach the stage where a new and more thorough book was prepared. During the 1960s and 1970s he gave frequent graduate seminars on *Structure* and his further thoughts, as well as giving lectures and publishing intermediate hints of his elaborations. Two major influences on his thinking were conferences in London in 1965 and Champaign, Illinois in 1969, at which *Structure* was a major critical focus. The proceedings of these were eventually published as *Criticism and the Growth of Knowledge* (edited by Lakatos and Musgrave) and *The Structure of Scientific Theories* (edited by Suppe). In 1977 he published *The Essential Tension*, a collection of his essays ranging from reprintings of pre-*Structure* papers to items that appeared for the first time in that volume. The essential tension referred to is that between the desire to assimilate all data and observations within the current paradigm and the desire to find revolutionary new solutions.

He continued to be heavily involved in history of science, the main culmination of which was the publication in 1978 of *Black-Body Theory and the Quantum Discontinuity, 1894–1912*. In 1979 he left Princeton for the MIT department of philosophy and linguistics, where he became a professor of philosophy for the first time. Subsequently his research focused more exclusively on refining his answers to the questions raised about *Structure*: about the nature of incommensurability, the relation between disciplinary matrices and scientific communities, the elements of disciplinary matrices, rationality, and theory choice. His analytic tools also shifted; the "Postscript" was phrased in terms familiar to readers of Quine's *Word and Object*, whereas his later work invoked possible worlds (see LEWIS) and rigid designators (see KRIPKE).

One recurring issue was the clarification of his ontological views. Probably the most infamous sentence of *Structure* occurs on p. 150: "In a sense I am unable to explicate further, the proponents of competing paradigms practice their trades in different worlds." Well disposed critics urged that he probably did not really mean to say that they were in different worlds, just that the world looked very different to them. But he was adamant that there was an important insight in the stronger claim. This was important to him because, for instance, he also wanted to claim that before the medieval paradigm change that introduced the concept of the pendulum, there were no pendulums but only swinging stones (p. 120). His attempts to clarify this and related locutions led him to further investigations of the interrelations of language, concepts, and perception and to propose that these were at least partially constitutive of the world.

He became an emeritus professor in 1989 but rather than diminishing his efforts, he used this as an opportunity to spend more time on his research agenda. At the time of his death in 1996 the solutions were still not in his grasp and the envisioned conclusive manuscript was still in an early stage. An extensive study of his later work is *Reconstructing Scientific Revolutions*, a 1993 translation of Paul Hoyningen-Huene's 1989 book. Hoyningen-Huene worked closely with Kuhn in producing the book and it is almost a collaboration. A thorough evaluation of Kuhn's work can be found in *World Changes*, edited by Paul Horwich, which is the revised product of a 1990 conference on Kuhn's work and includes responses to his critics.

I have waxed biographical to underline the peculiarity of evaluating Kuhn from the context of analytic philosophy. He had no formal training in philosophy, and his most influential work was completed before he was very thoroughly conversant with the intricacies of the analytic tradition. But he was already sufficiently familiar with it at the time of the writing of *Structure* to recognize that he would be accused of confusing the context of scientific discovery with the context of scientific justification, a distinction formalized by Reichenbach but which was widespread in the tradition before his articulation. Kuhn's response to the accusation was to question the distinction: to argue that only a historically inaccurate and oversimplified view of scientific development would permit such a distinction and that to maintain such a distinction was to doom epistemology to sterility.

Other philosophers of science – Hanson, Toulmin, Feyerabend, Hesse, among others – published books with at least similar themes in the late 1950s and 1960s, but none of those had the same effect or, possibly excepting Feyerabend's *Against Method*, produced so strong a reaction in readers and reviewers.

It would be easy to underestimate the influence *Structure* and Kuhn's subsequent work had directly on philosophy of science and indirectly on analytic philosophy generally. A large percentage of a generation of philosophers of science spent a considerable portion of their careers showing that Kuhn was wrong – wrong about incommensurability, wrong about paradigms, wrong about the role of scientific communities, wrong about rationality, wrong about the relevance of psychology for philosophy of science, and most significantly, wrong about the import of history of science for philosophy of science.

However, the results of the inquiries demonstrating the defects and errors of *Structure* bear a far greater resemblance to *Structure* than to its predecessors. The situation seems comparable to the role of Piaget in developmental psychology. Few, if any, of Piaget's specific claims about developmental stages or even about the abilities (and inabilities) of children at various ages have withstood further more sophisticated research. But Piaget brought the field into existence and without his impetus it is not clear that any of the further research would have been done.

One could argue that I have overstated the impact of Kuhn's work; other philosophers, including Carnap and Hempel, as well as the previously mentioned authors, were calling, albeit more quietly, for a rethinking of the image of science that had been dominating philosophy of science (see CARNAP and HEMPEL). The received view of scientific theories was under attack both from those who questioned the pivotal distinction between theoretical and observational vocabulary, but also from the structuralist approach to theories championed by Braithwaite, Suppes, van Fraassen, and Suppe. On the other hand, the most refined version of structuralist theories, that produced by Stegmuller, Sneed, and others, drew strong inspiration from Kuhn.

The importance of careful historical case studies, of consideration of the broader context of scientific developments, of the cognitive abilities and constraints on scientists, of the "external" influences such as motivation and competition, are all now taken for granted as part of philosophy of science. Debate rages about the relative importance, interpretations, and so on, but in the background there are shared assumptions that were not in place before the influence of Kuhn's work. Examples of important recent books that are not always cognizant of their Kuhnian heritage, but which can be seen to be following in a Kuhnian tradition are Longino's *Science as Social Knowledge*, Giere's *Explaining Science*, and Kitcher's *The Advancement of Science.* His work has also inspired the development of historicist, feminist, and sociological movements in the philosophy of science with whose doctrines he often disagreed.

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