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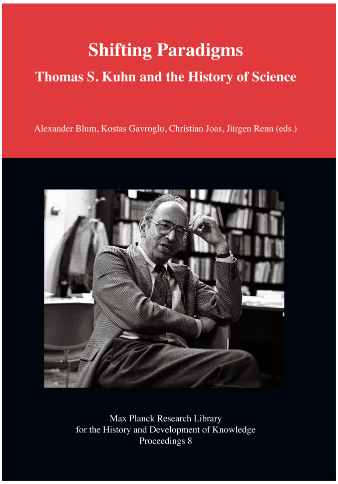

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Chapter 21
Kuhnian and Post-Kuhnian Views on How Science Evolves

Mary Jo Nye

If I had happened to glance at Thomas Kuhn's new book *Structure of Scientific Revolutions* while browsing in a bookstore in 1962 as a college freshman, I likely would have seen nothing surprising in the title. I probably would have thought that the book had to do with scientific methodology and the way in which proper scientific method ensures the scientist's rejection of wrong ideas and the discovery of revolutionary new phenomena. Of course, I would have been wrong.

In fact Kuhn's title registered two bold assertions on the basis of case histories in the physical sciences. First, scientific history is a history of distinct ruptures, as in political history, and new ways of seeing the physical world are incommensurable with the systems they have destroyed. Secondly, there is a repetitive and predictable structure in scientific change which, in Kuhn's pithy terminology, is one of "normal science" under a dominant "paradigm," followed by accumulation of "anomaly," then "crisis" and "revolution." The outcome of the process is the result not just of empirical logic, but also of the psychology of conversion and the sociology of community. The Copernican Revolution, the Chemical Revolution and the Quantum Revolution are among his exemplars (Kuhn 1962).

A few years before the publication of *Structure*, Kuhn outlined some of his main themes at a conference devoted to the identification of scientific talent (Kuhn 1959, 1977). The Soviet launching of Sputnik had just triggered a panicked infusion of federal money into science education in the United States, and the 1959 conference at the University of Utah was one of many efforts to define and promote scientific creativity and achievement. Kuhn presented a paper titled "The Essential Tension: Tradition and Innovation in Scientific Research." He likely startled participants who were expressing the usual view that the creative scientist eliminates all prejudice from the mind and cultivates "divergent thinking" from accepted opinion. This point of view coincided with the already popular critical empiricist philosophy of science of Karl Popper (1959). It also corresponded with the well-known adage of Claude Bernard, a century earlier, that the scientist must leave his imagination in the coatroom when entering the laboratory and put it on again after recording experimental results (Bernard 1957).

Kuhn's different view denied the heroic stereotype of objective scientific method in search of new and revolutionary discoveries. In contrast, said Kuhn, "almost none of the research undertaken by even the greatest scientist is designed to be revolutionary" but, on the contrary, "normal research" is a "highly convergent activity based firmly upon a settled consensus acquired from scientific education and reinforced by subsequent life in the profession." Revolutionary shifts occur, but they are rare, in part because of scientific pedagogy in mature science that teaches conformity to the textbook, with exemplary problem solutions that show the student what problems matter and how to solve them. Science rests, Kuhn said, on a "dogmatic initiation in a pre-established tradition of apprenticeship." Science produces innovations because of the ways in which the scientist's puzzle-solving activities reliably expand the matrix of scientific beliefs and occasionally call those beliefs into question following an accumulation of anomalies that can no longer be ignored. The successful scientist lives in a community of essential tension between the double roles of "traditionalist" and "iconoclast." Simply by offering this interpretation, Kuhn positioned himself as an iconoclast (Kuhn 1977, 227, 229, 230).

Of special significance at the time was Kuhn's argument that the nature of scientific knowledge lies in what he variously called dogma, belief or tradition—all of which sounds disturbingly like ideology, faith and religion. Kuhn's insistence on scientific "belief" was not entirely new, but the one million copies sold of his book in his lifetime brought the notion to a new audience. Kuhn drew brief attention in *Structure* to the earlier description by the bacteriologist Ludwik Fleck of the thought-models and thought-collectives that restrict what problems are deemed significant and what kinds of answers can be sought (Fleck 1935). Kuhn also cited the physical chemist Michael Polanyi for his statements of the role in science of established beliefs and the importance of apprenticeship through which the scientist absorbs the tacit knowledge essential to future scientific practice (Polanyi 1946, 1958).

In Kuhn's view, however, Polanyi put too much emphasis on the individual scientist—the "personal" in Polanyi's terminology—and on the individual experience of conversion that can be likened to a change in Gestalt. In contrast, Kuhn said that he wanted to emphasize the collective process in the scientific community by which innovation is recognized and legitimated. Reading Fleck, wrote Kuhn, made him realize that his own ideas about scientific tradition and scientific revolution needed to be set within a sociological account of the scientific community.

At this time, the sociology of science in the United States was just emerging from its recent association with Left and Marxist alliances. Anti-Marxist views affected the reception in the US in the 1930s of Boris Hessen's account of the social and economic origins of Newton's *Principia* (Hessen 1931), J. D. Bernal's description of the social organization and social function of science (Bernal 1939) and Karl Mannheim's sociology of knowledge (Mannheim 1929, 1936), all of which nonetheless got the attention of the young sociologist Robert K. Merton, whose 1936 dissertation offered a powerful but non-Marxist interpretation of the social and economic aspects of scientific development in seventeenth-century England (Merton 1936, 1970). Merton's attitude toward Mannheim was especially important. Merton wrote a review for *Isis* of the English translation of *Ideology and Utopia*, familiarizing himself with Mannheim's arguments for the social determinants of what Mannheim called "thought-models" in the social sciences. Merton noted Mannheim's exclusion of the natural sciences from analysis and suggested, presciently, that the sociology of scientific knowledge was a future task for sociologists once they had accomplished the project of empirically studying the institutions, norms and values, priority and reward systems, and disciplinary networks of the scientific community (Merton 1937).

It was this latter kind of sociology that Kuhn had in mind for better understanding the workings of normal science and its traditions of belief and practice. In a 1968 essay on "The History of Science" for the *International Encyclopedia for the Social Sciences*, Kuhn noted past Marxist influences in the "external" study of non-intellectual aspects of scientific culture. With mention of Merton and sociologists such as Joseph Ben-David and Warren Hagstrom, Kuhn suggested that the greatest challenge now facing the history of science profession was to bring together the "internal" and "external" approaches (Kuhn 1968).

This is exactly what happened after the dust settled from early debates about *Structure*. More recently, in 2012, various conferences marked the anniversary of *Structure*. Some scholars said that Kuhn's book generated no Kuhnian research school, despite the fact that Kuhn taught and collaborated with some later quite distinguished historians of science. Some insisted that Kuhn's main personal interest lay in the history of ideas (and in the philosophy of language and incommensurability), rather than in the study of scientific institutions and scientific communities, much less sociology of knowledge. Fair enough. Nonetheless, Kuhn's call for a history of science that would combine the so-called internal and external approaches was important, and Kuhn's evolving notion of the nature of the paradigm and normal science as what he began calling a "disciplinary matrix" provided historians of science with a powerful tool. With that tool, they have studied and expanded the scope of the history of science by studying in great detail in different times and places the many ways in which scientific traditions in the natural sciences have been codified, transmitted and transformed.

Some of this work has seemed to undermine confidence in the integrity and reliability of science in ways that Kuhn—as well as Polanyi, Fleck, Mannheim and others—certainly did not intend in their emphasis on what they saw as the constructive and stabilizing constraints of scientific tradition, dogmas or thought-models in scientific practice. One of the most widely read early essays to express concern was Stephen G. Brush's 1974 article in *Science* titled "Should the History of Science Be Rated X?" Brush suggested that recent historians' accounts of the way that scientists behave might not provide a good model for science students. Among Brush's examples were recent articles on "fudge factors" in the work of scientific heroes such as Newton, Mendel and Millikan (Brush 1974). Here, as Kuhn had argued, were accounts of great heroes of science who resisted anomalies and discrepancies because of their committed theoretical beliefs.

By the mid-1970s the new field of social studies of science joined the Merton school in influencing the history of science. Whether in Paris or Edinburgh or Philadelphia, science studies paid attention to social determinants of scientific knowledge and its thought-models. The science studies principle of impartiality demanded social explanation, rather than rational explanation, of widely accepted theories in the natural sciences, including the physical sciences. Rather than claiming that a theory is true or false, its acceptance must be explained through understanding the motives and strategies of the producers of knowledge and of dominant social interests, an argument made independently by the philosopher Michel Foucault for viewing disciplinary regimes in the social and human sciences.[1] Was this line of scholarship also reason to rate the history-of-science X? The notion that science is just belief, relatively independent of something like objective empiricism or convergent reality, could legitimate the arguments by science-deniers that theories of evolutionary biology or climate change are scientific dogmas controlled by a power elite within scientific disciplines.

When he was writing *Structure* in the heyday of post-World War II and post-Sputnik public support for science, Kuhn did not foresee such outcomes. Nor did he likely realize just how catchy his book title and his scheme of paradigms and revolutions might become during the 1960s political turmoil of civil-rights, women's-rights, anti-Vietnam-war movements and the Paris and Czech uprisings of May '68. These political developments brought unexpected attention to Kuhn's book on revolutions, along with new commercial possibilities for Berkeley street vendors who began selling bumper stickers that read "Subvert the Dominant Paradigm." Among historians of science, however, a surprising thing happened. Kuhn's notions of the dominant paradigm, sudden rupture and discontinuity were undermined by decades of historical studies combining the so-called internal and external history that he had highlighted as the challenge for the future.

Post-Kuhn historians have built upon Kuhn's notion of tradition, but especially his definition of disciplinary matrices, to study in detail research groups

[1] Bloor (1973); Barnes and Bloor (1982); Collins (1992); Foucault (1967, 1970).

and schools, laboratories and instruments, periodicals and textbooks, techniques of pedagogy, development of scientific lexicons, scientists' responses to anomalies and scientific controversies. The results, for example in the field of the history of chemistry, have largely undermined Kuhn's claims of sudden and incommensurable change except perhaps for the notion of incommensurable methodology (Chang 2012). Historians have found the long century of Lavoisier's so-called Chemical Revolution to be a period of small and gradual changes in chemistry and a period characterized by continued use of old practices alongside new ones in chemical methods, theories and languages (Holmes 1989; Klein and Lefèvre 2007). Historians of nineteenth-century chemistry have found change to be substantial, but so gradual and so endemic that it constituted what Alan J. Rocke has called a "quiet revolution" (Rocke 1993). Similarly, in mid-twentieth century chemistry, theoretical frameworks as competitive and different as Linus Pauling's atomic valence-bond theory and Robert Mulliken's molecular-orbital framework have turned out to be complementary despite the different premises, languages and tools of the two paradigms (Brush 1999).

These kinds of results in the history of the natural sciences tend to support the gradualist and evolutionary explanation of scientific change that Kuhn briefly broached at the very conclusion of *Structure*, at odds with the book's main argument. In his Rothschild Lecture at Harvard in 1991, Kuhn may have surprised some people in his audience by saying that: the "episodes that I once described as scientific revolutions are intimately associated with . . . speciation" that produces a "variety of niches within which the practitioners of these various specialties practice their trade" (Kuhn 1992). Detailed historical studies of developments in physical chemistry, solid state physics, molecular biology and other "hybrid" fields that have emerged alongside older disciplinary matrices seem to confirm this gradualist interpretation, as do many philosophical studies.

Our histories of science now differ greatly from those familiar to Thomas Kuhn at the time that he published *Structure*. For one thing, they are less heroic. For another, they rarely take the form of simply tracing materially and culturally independent or disembodied ideas. Our histories mostly are finely grained in their timelines and locales, as we analyze the investigative pathways and social settings within which science has been practiced and as we study its cultural meanings. Our histories also reflect recent changes in science. Big Science has become even bigger. The numbers of women in the sciences have greatly increased, as have the numbers of non-European scientists working in the traditional Western bloc and outside that bloc. The assistants and technicians aiding scientists have become more numerous and more visible. The distinction between fundamental and applied scientists has become harder to make. Correspondingly, we have made these people visible in our histories of past science and have explained the social mechanisms that long made them absent or invisible, finding continuities between past and present that sometimes surprise us.

In the end, I think that Kuhn's legacy is stronger than sometimes now claimed, although not entirely as he might have wished it to be. It is ironic that the history of tradition rather than revolution became the legacy. The first excitement and the first dissent over *Structure* centered on Kuhn's statement of the dogmatic character of scientific belief (which he incorporated into the notion of paradigm) and scientific revolution as a dramatic process of historical discontinuity between two incommensurable paradigms. Revolution was a catchword in the 1960s. The next generations of historians of science mostly disconfirmed the thesis of rupture and discontinuity in favor of gradualism and continuity, as they restudied the so-called scientific revolutions and focused on the everyday practices and everyday scientists of what Kuhn called the traditions of normal science. The idea of the influence in science of tradition and belief is no longer heretical. For this we owe Kuhn and others a considerable debt for giving us conceptual tools that have expanded the history of science away from the heroic and into the ordinary practices of science, however fallible but also committed its practitioners may be.

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