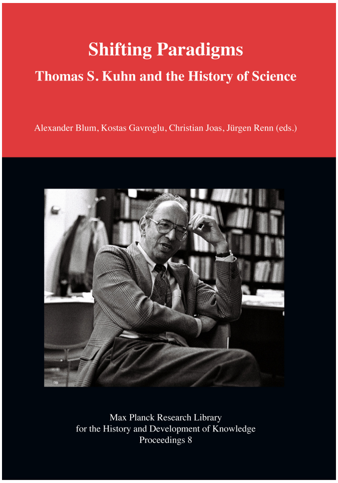
Max Planck Research Library for the History and Development of Knowledge
Proceedings 8

Ursula Klein:

Kuhn in the Cold War



In: Alexander Blum, Kostas Gavroglu, Christian Joas and Jürgen Renn (eds.): *Shifting Paradigms : Thomas S. Kuhn and the History of Science*
Online version at http://edition-open-access.de/proceedings/8/

ISBN 978-3-945561-11-9
First published 2016 by Edition Open Access, Max Planck Institute for the History of Science under

Printed and distributed by:
Neopubli GmbH, Berlin
http://www.epubli.de/shop/buch/50013

The Deutsche Nationalbibliothek lists this publication in the Deutsche Nationalbibliografie; detailed bibliographic data are available in the Internet at http://dnb.d-nb.de

Chapter 7
Kuhn in the Cold War
Ursula Klein

Fifty years after the publication of *The Structure of Scientific Revolutions*, historians and philosophers have been celebrating Thomas Kuhn and simultaneously criticizing him with respect to almost every part and parcel of his work.[1] Historians of science, in particular, question his emphasis on theory, his concept of overarching paradigms guiding the way science is done, as well as his concept of a universal structure of scientific revolutions. Mario Biagioli gave voice to recent historiographical trends, stating that "*Structure* was history-making and, half a century later, has itself become history" (2012, 479). In this essay, I am not concerned with Kuhn's *Structure* but with his equally influential *Mathematical Versus Experimental Traditions in the Development of Physical Science* (1976), which I will put into the context of Cold-War historiography of science.

In almost of all his historical and philosophical studies, Kuhn highlighted the role played by theory in the sciences. His interest in theory, in particular physical theory, is nicely illustrated by an episode Ian Hacking reported in his *Representing and Intervening* (1983). Hacking recalled that his colleague C. W. F. Everitt once wrote two papers for the *Dictionary of Scientific Biography*. One of them was on Fritz London, who was a theoretical physicist, and the other on his brother, the experimental physicist Heinz London. "The biography of Fritz was welcomed by the *Dictionary*," Hacking observed, "but that of Heinz was sent back for abridgement. The editor (in this case Kuhn) displayed the standard preference for hearing about theory rather than experiment" (Hacking 1983, 152).

Hacking's famous argument that experiments can have a life of their own implied a clear question mark concerning the scope of Kuhn's approach, with its emphasis on theory and paradigms (1983). Historical studies of experimentation in the 1980s, such as Latour and Woolgar's *Laboratory Life*, Shapin and Schaffer's *Leviathan and the Air Pump* and Galison's *How Experiments End* further undermined the significance of Kuhn's approach.[2] The main issues now discussed were scientific facts, intervening laboratory practices, instruments, tacit knowl-

[1] See, for example, the collection of essays in *Historical Studies in the Natural Sciences* 24 (5), 2012.
[2] Latour and Woolgar (1979); Shapin and Schaffer (Shapin and Schaffer 1985); Galison (1987).

edge, experimental representation and social hierarchy in the laboratory. Kuhn's view of science had even less of an impact on the new studies of material culture and materiality in the sciences.[3] Most of the questions highlighted in the latter studies, particularly those concerning the ways in which material objects condition scientific inquiry, are only of marginal importance in Kuhn's work. Thus, it is perhaps not too far-fetched to argue that Kuhn's approach and his approaches emphasizing practice and material objects are incommensurable.

There is one famous essay, however, which seems to contradict the now common view that Kuhn highlighted scientific theory at the expense of experimentation and material culture. His *Mathematical versus Experimental Traditions in the Development of Physical Science* (1976) not only addresses issues concerning the experimental sciences, but also the role of instruments in experimentation.[4] It seems to manifest a genuine interest in the experimental, or what he called "Baconian sciences," as well as in their technological context. The goal of this essay is to shed light on Kuhn's interest in writing the latter essay. Putting this essay into the context of Cold-War ideology, I will argue that Kuhn tried to find a middle ground between materialist explanations of early modern science and anti-Marxist arguments against the latter approach.

Kuhn's "Mathematical versus Experimental Traditions"

In his essay "Mathematical Versus Experimental Traditions," first published in 1976, Kuhn demarcated the 'classical physical sciences' from the 'Baconian sciences.' The Baconian sciences were, according to him, a novel type of sciences emerging in the period of the Scientific Revolution, and they were experimental sciences. By contrast, the classical physical science had a long tradition, but they were thoroughly reconstructed in the sixteenth and seventeenth centuries. Kuhn pointed out that the new Baconian sciences did not pursue theoretical goals, although theory (mainly corpuscular philosophy) often lurked in the background. Instead, their "typical products were the vast natural and experimental histories" (1977, 43). He further stated that Baconian experiments forcefully intervened into nature and that intervention required instruments. Hence, he argued that in "less than a century physical science became instrumental" (1977, 44). More interestingly, he observed that artisanal workshops were sites for the construction of scientific instruments as well as "subjects for learned concern," and he further

[3] See, for example, Lefèvre (1978); Latour (1987); Pickering (1995); Rheinberger (1997).

[4] Kuhn (1977, 31–65). Kuhn's *The Structure of Scientific Revolutions* (1962) includes no more than remarks on experiments and the experimental sciences; his essay *The Function of Measurement in Modern Physical Science* (1961) is more concerned with the quantification of physics than experimentation per se; *A Function for Though Experiments* (1964) is located at the borderline of experimentation and theory; see Kuhn (1977, 178–224, 240–265).

mentioned that some Renaissance and early modern “artist-engineers” participated in polite learning (1977, 57, 55).

Why did Kuhn study these kinds of issues that clearly deviated from the type of problems discussed in *Structure* and most of his other publications? It is by no means evident that natural and experimental histories were structured by paradigms, and that the concepts of “anomaly,” “crisis,” and revolutionary replacements of paradigms are able to grasp the work undertaken in experimental contexts. I argue that Kuhn’s *Mathematical versus Experimental Traditions* was indeed a detour with respect to the bulk of his work, and that this detour was provoked by an ideological campaign of historians of science during the Cold War. In what follows, I briefly outline this campaign and then show how Kuhn positioned himself within it.

In 1959, Marshall Clagett published a collection of papers entitled *Critical Problems in the History of Science*, which was based on a conference that had taken place three years before at the University of Wisconsin. Kuhn was present at this conference. In a contribution to it, entitled *The Scholar and the Craftsman in the Scientific Revolution*, the historian of science Rupert Hall took issue with some recent arguments concerning the early modern sciences and their technological and economic context (Hall 1959). Without naming any of his opponents and in an almost perfect objective rhetoric, he vehemently rejected the argument that technological change and the accompanying social revaluation of craftsmanship and technical knowledge was one of the causes of the Scientific Revolution. While he conceded that artisanal and engineering practices had stimulated early modern scholars and provided opportunities for new scholarly observation, he mainly argued that the transformations in the Scientific Revolution were achievements just of scholars, not of any other persons living and working outside the academic world. According to Hall, the Scientific Revolution was an internal scholarly process, the result of an “internal strife” between “academic innovators” and “academic conservatives.” Hall further emphasized that these “quarrels of learned men had as little to do with capitalism as with the protestant ethic” (1959, 7). The crucial point, according to him, was that the academic innovators had modified their “attitude” towards the arts and crafts, whereas the academic conservatives kept their traditional themes (1959, 16). Thus the academic innovators began to perceive things—most importantly “the success of craft empiricism”—that the conservatives continued to ignore. Hall argued that this change was entirely subjective; it had nothing to do with changes of production, trade and commerce: “It [the success of craft empiricism] was always there to be seen,” and therefore “the change was in the eye of the beholder” in the early modern period (1959). Hall’s critic cumulated in the clear demarcation of scholars from craftsmen and the rejection of what later historians called the “scholar-and-craftsman thesis." "This seems to me to be the defect of the view," he stated, "that sees the new scientist of the seventeenth century as a sort of hybrid between the older natural philosopher and the craftsman" (1959, 17).

Cold-War Historiography of Science

Who exactly were Hall's opponents? In the 1930s and early 1940s, Boris Hessen (1931), Franz Borkenau (1934), Henryk Grossmann (1935), Robert K. Merton (1938) and Edgar Zilsel (1941/42) had published studies on early modern interconnections between science, technology and the economy. Among these authors, Merton, an accepted member of the scientific community in the US, was perhaps the most unwelcome person.[5] In 1938, he had published an essay entitled *Science, Technology and Society in Seventeenth-Century England* (see below). Hall's formulation of the scholar-and-craftsman thesis, as well as his remarks about the protestant ethic and capitalism, point exactly in the direction of Merton. In a later essay, published in 1963, he formulated his critic of Merton more openly (Hall 1963). The "brilliant young scholar" Merton, he informed his readers, did not just argue the obvious, namely that "no one writing the history of science would ever divorce it completely from society's beliefs and structure." Rather, he dared to offer "principles of historical explanation," which "are complementary to, if they do not replace, those offered by the historian of science" (Hall 1963, 1). In other words, the brilliant young man was a threat to all good historians of science. What Hall did presumably not foresee, let alone wish, was the fact that his attack increased Merton's publicity and contributed to a re-publication of his 1938 essay in book form (Merton 1970).

In *Science, Technology and Society*, Merton argued that the Protestant ethos created a favorable milieu for the early modern sciences. This part of his book became later known as *the* Merton thesis. The bulk of this book, however, was concerned with a different issue, namely technology and capitalist economy as a context of the early modern sciences. In the main part of *Science, Technology and Society*, which was first criticized and later almost completely ignored, Merton presented a number of compelling case studies that led him to conclude that technical objects and socio-economic problems had an impact on early modern scientists' choice of problems.[6] They often provoked "shifts of interests" in scientific inquiry, he argued, or created "derived scientific interests." Referring to

[5] Merton was a student of George Sarton, who had invited him in 1938 to publish his "Science, Technology and Society in Seventeenth-Century England" in the journal *Osiris*. He became one of the most influential American sociologists.

[6] It should be noted that the term "scientist" is not fully appropriate with respect to the early modern period. But here and elsewhere in this paper I use the terms that historians used in the 1960s and 1970s.

the construction of early modern fighting ships, for example, Merton pointed out that "all the major problems [in this field] had become the object of scientific study" (1970, 178). This was a clear causal argument about social and technical stimulations of scientists' interests, which was directly opposed to Hall's view. Moreover, in his discussion of case studies Merton also pointed out that "the inventor and the scientist were often one" (1970, 146). Hall transformed the latter observation into a theoretical argument named "scholar-and-craftsman thesis."

As Merton's arguments partially overlapped with those of Zilsel and other authors identified as Marxists, they fell under the spell of a predominantly anti-Marxist ideology among Cold-War historians of science. Clearly, the main goal of Hall's argument was the identification of an intellectual target for an anti-materialist and anti-Marxist crusade, covered by a polite and apparently open-minded style. There were only a few members of the historical community who had some doubts that Merton was clearly on the wrong path, and Kuhn was among them. Instead of ignoring Merton's arguments, he included them in his teaching and began to publish on related issues.[7] In 1968, he wrote that "attempts to set science in a cultural context," such as Merton's, "might enhance understanding both of its development and of its effects."[8] He conceded that Merton's view owed "something to Marxist historiography," but he was also uncomfortable with the fact that his approach was "attacked with vehemence", as was exemplified by Hall's paper published in Clagett's volume (Kuhn 1977, 115). A better way to deal with it, he proposed, was "the revision of the Merton thesis" (Kuhn 1977, 117, 118). The result of this revision was his *Mathematical versus Experimental Traditions.*

It is thus not surprising that there are many thematic intersections between Kuhn's essay and Merton's book. But Kuhn did more than just taking up historiographical issues previously highlighted by Merton, Zilsel and others. Discussing also the theoretical dimension of the theme, he tried to find a theoretical middle ground between Hall's and Merton's view. In 1968 he wrote: "If Merton were right, the new image of the Scientific Revolution would apparently be wrong" (Kuhn 1977, 116). The "new image" was R. Hall's and Alexandre Koyré's that postulated that the "radical sixteenth- and seventeenth-century revisions of astronomy, mathematics, mechanics, and even optics owed very little to new instruments, experiments, or observations" (1977). Needless to add that this implied the denial that technology played any significant role in the Scientific Revolution. In this distinct historical situation, Kuhn proposed a new argument that had also been largely ignored by Merton, Zilsel, Hessen, Borkenau and Grossman, who had all focused on the mechanical and mathematical sciences as well. Kuhn reminded

[7] I thank John Heilbron for this information.

[8] The paper entitled *The History of Science* is reprinted in Kuhn (1977, 105–126, see p. 113.).

the community of historians of science that early modern "science" should not be equated with astronomy, mathematics, mechanics and optics. Instead he argued that the "new image" must also take into account the seventeenth-century studies of electricity, magnetism, chemistry and thermal phenomena along with the ideology of Baconianism. A "revised Merton thesis," he stated, must also promote our understanding of these experimental sciences (1977, 118).

In 1976, Kuhn had fully developed his argument. On the one hand, he conceded in his "Mathematical versus Experimental Traditions" that economy and technology actually had a significant impact on the early modern sciences. Yet, on the other hand, he also emphasized that the impact of economy and technology was restricted to a distinct part of the early modern sciences, namely the experimental or Baconian sciences. As a consequence, his *Mathematical versus Experimental Traditions* took the edge off Merton's approach. Whereas Merton had not distinguished between different traditions of science when he discussed interconnections between the early modern sciences, technology and economy, Kuhn divided the field into two clearly different traditions, stating that well into "the nineteenth century the two clusters, classical and Baconian, remained distinct" (1977, 48).

What is more, he linked this distinction with a normative judgment: only his "classical physical sciences" met his criteria of science in the proper sense. By contrast, before the nineteenth century, he argued, the Baconian sciences were "underdeveloped" and practiced by "amateurs."[9] Hence unlike the classical sciences, "research in these fields added little to man's understanding of nature during the seventeenth century" (1977, 118).

Kuhn's *Mathematical versus Experimental Traditions* revised the Merton thesis mainly by restricting its significance to those sciences that were not proper "science." Both Merton's and Hall's arguments were correct if restricted to their appropriate field of application. Whereas technology and economy had a significant impact on the emerging experimental sciences, which lacked features of the developed sciences, they did not affect astronomy, mechanics and other developed "classical sciences." Revisions of the latter during the Scientific Revolution were internal processes, well described by Hall and Koyré. The good historians of science could be relieved: their view of the Scientific Revolution was perhaps incomplete, but it was basically correct. Kuhn had made a lame duck of Merton.

References

Biagioli, M. (2012). Productive Illusions: Kuhn's *Structure* as a Recruitment Tool. *Historical Studies in the Natural Sciences* 42(5):479–484.

[9] Kuhn (1977, 47, 51). Kuhn considered chemistry to be an exception in this respect (p. 51).

Borkenau, F. (1934). *Der Übergang vom feudalen zum bürgerlichen Weltbild: Studien zur Geschichte der Philosophie der Manufakturperiode*. Paris: F. Alcan.
Clagett, M. (1959). *Critical Problems in the History of Science*. Madison: The University of Wisconsin Press.
Galison, P. (1987). *How Experiments End*. Chicago: The University of Chicago Press.
Grossmann, H. (2009 [1935]). The Social Foundations of the Mechanistic Philosophy and Manufacture. In: *The Social and Economic Roots of the Scientific Revolution: Texts by Boris Hessen and Henryk Grossmann*. Ed. by G. Freudenthal and P. McLaughlin. Boston Studies in the Philosophy of Science, vol. 278. Dordrecht: Springer.
Hacking, I. (1983). *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science*. Cambridge: Cambridge University Press.
Hall, R. (1959). The Scholar and the Craftsman in the Scientific Revolution. In: *Critical Problems in the History of Science*. Ed. by M. Clagett. Madison: University of Wisconsin Press, 3–23.
— (1963). Merton Revisited or Science and Society in the Seventeenth Century. *History of Science* 2:1–16.
Hessen, B. (2009 [1931]). The Social and Economic Roots of Newton's *Principia*. In: *The Social and Economic Roots of the Scientific Revolution: Texts by Boris Hessen and Henryk Grossmann*. Ed. by G. Freudenthal and P. McLaughlin. Boston Studies in the Philosophy of Science, vol. 278. Dordrecht: Springer.
Kuhn, T. S. (1961). The Function of Measurement in Modern Physical Science. *Isis* 52:161–190. Reprinted in Kuhn 1977, 178–224.
— (1962). *The Structure of Scientific Revolutions*. Chicago: The University of Chicago Press.
— (1976). Mathematical vs. Experimental Traditions in the Development of Physical Science. *The Journal of Interdisciplinary History* 7:1–31.
— (1977). *The Essential Tension: Selected Studies in Scientific Tradition and Change*. Chicago: The University of Chicago Press.
Latour, B. (1987). *Science in Action: How to Follow Scientists and Engineers through Society*. Cambridge: Harvard University Press.
Latour, B. and S. Woolgar (1979). *Laboratory Life: The Social Construction of Scientific Facts*. London: Sage Publication.
Lefèvre, W. (1978). *Naturtheorie und Produktionsweise*. Darmstadt: Luchterhand.
Merton, R. K. (1938). Science, Technology and Society in Seventeenth-Century England. *Osiris* 4: 360–632.
— (1970). *Science, Technology and Society in Seventeenth-Century England*. New York: Howard Fertig.
Pickering, A. (1995). *The Mangle of Practice: Time, Agency, and Science*. Chicago: The University of Chicago Press.
Rheinberger, H.-J. (1997). *Toward a History of Epistemic Things: Synthesizing Proteins in the Test Tube*. Stanford: Stanford University Press.
Shapin, S. and S. Schaffer (1985). *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life*. Princeton: Princeton Universty Press.
Zilsel, Edgar (2000 [1941/42]). *The Social Origins of Modern Science*. Ed. by Diederick Raven and Wolfgang Krohn. Boston Studies in the Philosophy of Science, vol. 200. Dordrecht: Kluwer.