

*British Journal for the Philosophy of Science* [2015], doi:10.1093/bjps/axv054), but mainly, even acknowledging the importance that nonselectionist dynamics play in culture, it seems reasonable that to study cultural evolution, one should commit at least to the idea that in some domains, or at some level of analysis, selection plays an important role in explaining the spread of cultural traits (A. Acerbi and A. Mesoudi, “If We Are All Cultural Darwinians What’s the Fuss About? Clarifying Recent Disagreements in the Field of Cultural Evolution,” *Biology and Philosophy* 30 [2015]: 481–503). If one does not commit to this idea, it seems legitimate to wonder what the reasons are to use the “evolution” label.

The picture of cultural evolution that Lewens outlines may have the advantage of protecting the approach from criticisms of reductionism and making it more palatable to other social and human scientists but may also partly limit the radicalness of its contribution. Its wide-ranging coverage may have the advantage of including in the cultural evolution paradigm a wealth of excellent naturalistic research but may also have the consequence of losing the detail on interesting differences between perspectives, such as the above-mentioned Sperber versus Boyd and Richerson schools, which are not particularly considered in Lewens’s book. Whether they will recognize themselves in this picture or not, cultural evolutionists should be flattered that a philosopher took great care to examine their work: the field can only benefit from thought-provoking books such as Lewens’s.

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Robert J. Richards and Lorraine Daston, eds., *Kuhn’s Structure of Scientific Revolutions at Fifty: Reflections on a Science Classic*. Chicago: University of Chicago Press (2016), 202 pp., \$25.00 (paper); \$75.00 (cloth).

This volume of essays is one of several put together as part of the celebrations of the fiftieth anniversary of the publication of Kuhn’s *Structure of Scientific Revolutions* (see also V. Kindi and T. Arabatzis, eds., *Kuhn’s Structure of Scientific Revolutions Revisited* [London: Routledge, 2012]; W. J. Devlin and A. Bokulich, eds., *Kuhn’s Structure of Scientific Revolutions: 50 Years On*, Boston Studies in the Philosophy and History of Science 311 [Dordrecht: Springer, 2015]; Alexander Blum, Kostas Gavroglou, Christian Joas, and Jürgen Renn, eds., *Shifting Paradigms: Thomas S. Kuhn and the History of Science* [Berlin: Max-Planck-Gesellschaft zur Förderung der Wissenschaften, 2016]). The volume under review is based on a conference held in 2012 sponsored by the Fishbein Center for the History of Science and Medicine at the University of Chicago and the Max Planck Institute for the History of Science

in Berlin. It includes nine papers, five by historians, three by philosophers, and one by a sociologist. The level at which each paper engages *Structure* or Kuhn's other works varies considerably.

Lorraine Daston provides a valuable assessment of Kuhn's influence on the history of science. She argues that the account of the professionalization of scientific fields described in *Structure* was exemplified by the history of science after the publication of *Structure*. The professionalization of the history of science created strains between historians of science, on the one hand, and philosophers of science and sociologists of science, on the other hand. The methods, practices, and goals of historians of science are at odds with the aims of philosophers and sociologists studying science. Concern with detailed analyses of very specific episodes in the history of science and with understanding the scientific cultures of the past in their own terms proves to be antithetical to the aims of philosophers and sociologists, who seek to develop a general theory of science or scientific change. Interestingly, Daston suggests that *Structure*, the book, has no part to play in the professionalized discipline of history of science because it does not meet the standards of contemporary historical scholarship.

Both Peter Galison's and Angela Creager's papers reinforce Daston's assessment of the situation by example. Creager provides a comparison of Kuhn's paradigms with the notion of a model organism employed in biomedical research. Although her remarks on model organisms are thoughtful, her engagement with Kuhn's work is quite superficial and, at times, misleading. For example, Creager suggests that Kuhn regarded anomalies as "catalysts of crisis . . . rather than motors of scientific change" (157). Kuhn, though, did not see anomalies as catalysts of crisis only (so-called significant anomalies). As far as Kuhn was concerned, anomalies are pervasive and are also sources of the research problems of the normal scientist.

Galison signals the need to move past Kuhn but expresses gratitude for his leading us to take the study of the practice of science more seriously. Galison provides some engaging reflections on Kuhn's work in physics, first, as Kuhn worked on radar as part of the war effort and, later, with John Van Vleck, his PhD thesis supervisor at Harvard. Galison casts Kuhn as a typical "normal scientist," engaged in merely applying accepted theory to specific and theoretically unimportant problems. Galison also argues that Kuhn's understanding of scientific practice was outdated. As a result, Kuhn's account of science is not suited to making sense of collaborative research in such facilities as CERN, for example. Kuhn's outdated understanding of scientific practice, Galison suggests, was a consequence of the fact that American physicists, even those working at Harvard, were trailing behind their European counterparts in the 1930s.

Norton Wise provides a more sympathetic reading of Kuhn and his importance. As a former student of Kuhn, Wise draws on his many lively interactions with Kuhn, both in the seminar room and in the profession. Wise

astutely notes that when Kuhn emphasizes the social dimensions of science he has in mind a rather narrow sense of social, one not informed by sociological theory. Scientists working in the same research area are united by shared exemplars. The shared exemplars provide the social glue that holds the group together. Indeed, Wise suggests that Kuhn's resistance to the Strong Programme's appropriation of his work was due to his frustration with their preoccupation with various social aspects of science that were orthogonal to his concerns.

David Kaiser provides an engaging analysis of the influence of psychology on Kuhn's thinking in *Structure*, as well as psychologists' subsequent engagement with *Structure*. Kaiser argues that if one looks carefully at the text, one will realize that many of Kuhn's most contentious claims are not supported by evidence from the history of science but rather from research in psychology. For example, Kuhn draws heavily on Bruner and Postman's study of the anomalous playing cards, as well as research in Gestalt psychology. Even Kuhn's approach to studying the history of science, Kaiser argues, emphasizes the need to get inside someone else's head, to understand his or her train of thought.

Andrew Abbott takes seriously Kuhn's claim to be describing the dynamics of the natural sciences only. As a sociologist, Abbott is interested in understanding the dynamics of social scientific research, that is, research in fields where there is no single reigning paradigm but multiple competing incompatible theoretical frameworks accepted at any given time. Abbott provides a brief summary of the account of scientific change in the social sciences that he developed in detail in *Chaos of Disciplines* (Chicago: University of Chicago Press, 2001). Abbott suggests that the social sciences are often recycling or revisiting concepts from the past. This recycling and modification of older concepts is an important source of creativity and progress in the social sciences.

Let me now turn to the three contributions by philosophers. Dan Garber discusses the relationship between the Scientific Revolution and the sorts of scientific revolutions that Kuhn made popular. In line with much contemporary research in the history of early modern science, Garber argues that there really was no such thing as the Scientific Revolution in the early modern period, despite the rhetoric of many of the key figures from that period. Indeed, he notes that even rather obscure figures that play no part in the early modern canon used the same sort of rhetoric as Descartes and Bacon, announcing the beginnings of a new science. Despite the fact that the notion of the "Scientific Revolution" is obsolete, Garber does believe that Kuhn draws attention to something important in his use of the term "scientific revolution." Kuhn's scientific revolutions are of a significantly different scale. They are the disruptive changes of theory that affect small groups of scientists working on the same research topics. Garber believes that this is a worthwhile concept for understanding the dynamics of scientific change.

Ian Hacking revisits the notion of paradigms, a notion for which Kuhn encountered extensive and sometimes rather unfair criticism. Hacking argues that Kuhn encountered an old problem when explicating the notion, a problem that Hacking traces back to Aristotle. What they were both trying to understand is how we reason from cases, from comparing one thing to another. True to his characteristic style, Hacking does not resolve the problem or suggest that it is easily resolved. Instead, we are left with a deeper appreciation for the problem itself, as well as for Aristotle's and Kuhn's attempts to shed light on the issue. When we compare one thing to another, we are reasoning nondeductively, but it does not resemble the sort of reasoning that we engage in when we engage in enumerative induction, generalizing from an experience of many similar particulars. Still, this pattern of reasoning is a genuine phenomenon and certainly one relevant to the way scientists sometimes reason.

George Reisch provides an interesting analysis of the cultural context in which Kuhn was working. Kuhn's involvement in James B. Conant's General Education Natural Sciences courses at Harvard, and his early work on *Structure*, was at the peak of the Cold War. Reisch argues that the psychology of Cold War thinking, in particular, the notions of ideology and indoctrination, played a crucial role in Kuhn's thinking about science. Kuhn, though, turns these concepts on their heads, regarding the "indoctrination" of young scientists in training and the "ideology" of the reigning theory in a scientific field as playing important constructive roles in science. By the time Kuhn completed *Structure* these loaded concepts had been replaced by other concepts, but Reisch makes a persuasive case for the role they played in the development of Kuhn's understanding of science and scientific change.

As a whole, this collection of essays is wide ranging, perhaps too wide ranging. But it is an apt tribute to Kuhn's *Structure*. The range of topics and the range of attitudes toward *Structure* exemplified in the papers provide a vivid demonstration of the way the book has been received over the years. Some scholars borrow from it selectively, merely using the book and its concepts as a launchpad for their own musings. Others engage with the details and aim to treat the vision of science developed in the book with integrity. Not surprisingly, the latter sort of work is by far the most rewarding to read.

Kuhn scholarship has undergone a process of professionalization in the last decade or so, with scholars now often drawing on unpublished sources, including the vast and rich resources in the Thomas S. Kuhn Papers at the Institute Archives at the Massachusetts Institute of Technology. This trend is exemplified in some of the papers, for example, Kaiser's and Reisch's. And our understanding of Kuhn is greatly enriched by this development. Collectively the various fiftieth anniversary volumes on *Structure* mentioned above provide us with a richer picture of Kuhn's significance for the philosophy of science, the history of science, and the sociology of science. No single one of these volumes stands out as providing a definitive assessment of

Kuhn, but, read together, they do provide scholars interested in Kuhn's work with abundant resources that should keep them busy until the next significant anniversary.

Two themes that run across numerous papers rightly deserve further study: (i) the relationship between history of science and philosophy of science and (ii) the nature of the social dimensions of science that concerned Kuhn. Despite the ongoing research in "integrated" history and philosophy of science, I must admit that I share Daston's assessment of the current state of affairs. The two fields operate largely independently of each other, and their methods, practices, and goals are so far apart that it is challenging to imagine what a truly integrated history and philosophy of science would look like. The citation practices in philosophy of science suggest that the history of science, that is, the history of science as practiced by historians, plays almost no role in contemporary philosophy of science (see K. B. Wray, "Philosophy of Science: What Are the Key Journals?" *Erkenntnis* 72 [2010]: 423–30).

Contemporary sociological studies of science, including studies in the interdisciplinary field of Science Studies, tend to emphasize the porous boundaries between science and society. These studies, although often illuminating, seem to overlook the social dimensions of science that concerned Kuhn. He was well aware of the fact that "external" factors influence science. But he also wanted to develop a richer understanding of the social dynamics of those special groups of people, scientists engaged in research on esoteric topics, whose novel contributions only they are qualified to judge. This is still an important topic, even granting that scientists are often unable to shield themselves from the influences of society at large. Indeed, most of the formal work in the social epistemology of science, the work on the effectiveness of various types of communication networks and the work on the (mis)labeled division of labor in science, aim to shed light on the sorts of communities that concerned Kuhn (see, e.g., K. Zollman, "The Communication Structure of Epistemic Communities," *Philosophy of Science* 74 [2007]: 574–87; P. Kitcher, *Advancement of Science: Science without Legend, Objectivity without Illusions* [Oxford: Oxford University Press, 1993]).

For those interested in Kuhn or the social dimensions of science, this book is certainly worth reading. The value of the book could have been enhanced by inclusion of a few papers from scholars who have studied Kuhn's work with care, most obviously, people like Paul Hoyningen-Huene and Alexander Bird. But this shortcoming is somewhat compensated for by the inclusion of a series of photographs of Kuhn, taken from when he was a teen to when he was a tenured professor at the University of California, Berkeley.

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