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Essay Review

An aging literary revolution: Stuck with the paradigm

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Kuhn's Structure of Scientific Revolutions at Fifty: Reflections on a Science Classic, Edited Robert J. Richards, Lorraine Daston (Eds.). University of Chicago Press, Chicago (2016). 202 pp., US$25.00 (paper), US$75.00 (cloth); ISBN 9780226317205

When one hears about a new book on Kuhn and his *The Structure of Scientific Revolutions*, the first question is: What now? What new can it add to the quite substantial literature that already exists?

It is no doubt true that Kuhn's book has had a seismic impact, not only on the history and philosophy of science, but much more widely on general culture too. The book has sold many more than a million copies, and the concept of paradigm, for example, has found its way far beyond narrowly confined academic circles. As mentioned in the introduction of this anniversary edition, the fact that the *Times Literary Supplement* chose *The Structure of Scientific Revolutions* to be among the twenty most important books published in the latter part of the twentieth century only underlines the significance of the book (2[1]). David Kaiser highlights an interesting indicator of *Structure's* influence: Between 1976 and 1983 the book was the most cited in the *Arts & Humanities Citation Index*. *Structure* beat worthy competitors in this contest, such as Freud, Wittgenstein, Chomsky, Derrida and Foucault (76). Andrew Abbot adds another telling detail: *The Structure of Scientific Revolutions* was cited 15,635 times between its publication in 1962 and 2012 (168).

It sounds fitting to celebrate the fiftieth birthday of so important a book (although at the time of its publication it was the book's fifty-fourth birthday). But then it is also true that at least two other commemorative volumes have already been published: *Kuhn's Structure of Scientific Revolutions Revisited*[2] and *Kuhn's Structure of Scientific Revolutions – 50 Years On*.[3] This book emerged out of the conference organised in Chicago to commemorate the anniversary and sponsored by the publisher of *Structure*, The University of Chicago Press, among other institutions. It is notable that the most of the writers are historians (George A. Reisch, M. Norton Wise, Peter Gallison, David Kaiser, Lorraine Daston, Angela N. Greager), albeit in different forms of history, rather than in philosophy. One is a sociologist (Andrew Abbott), and (only) one a philosopher (albeit also he specialises in the *history* of philosophy) (Daniel Garber). One could be said to be sitting on the fence of philosophy and history (Ian Hacking). It would have been useful to have brief introductions to all these scholars at the beginning of this book.

Kuhn's Structure of Scientific Revolutions at Fifty is a compact book by several illustrious and first-rate scholars. At its best it provides many windows and outlooks, sometimes personal, on a time and world now gone, quite as Kuhn would have requested of historical scholarship. It describes practices that are not really possible any more, like submitting a manuscript of fifty thousand words with no footnotes and bibliography written by a relatively unknown author to a reputable publisher, or the seminar instructor smoking with long drags while thinking in front of seminar students (see Wise's paper). Of course, these single incidents have a deeper intellectual significance, for example, with regard to the notion of paradigm, as is discussed below. Many previously unseen pictures of Kuhn provide a warm and lively touch to the book.

1. Mind control à la Kuhn

In the first contribution, George A. Reisch contextualizes a key suggestion by Kuhn: that it is the mind and conceptual structures in the mind that organize and make the world comprehensible to scientists. Reisch asks whether Kuhn practices what he preaches in the beginning of *Structure* when he elevates the study of history onto a pedestal in attempts to understand science. Reisch answers this question in the negative. Kuhn's idea that scientists' mind-sets determine their understanding of science can be traced to his personal revolutionary and well-documented "Aristotle experience" in 1947, when the sensibility of Aristotle's physics suddenly dawned on Kuhn. This experience, not the research of history, prompted Kuhn to formulate the key tenets of his philosophy of science, writes Reisch.

Reisch's claim is that *Structure* and its conception of science should be perceived through the prism of Cold War culture. The scientific mind moulded by paradigms and conceptual schemes are equal to the politically captive mind feared by anti-communists such as Czesław Miłosz (author of *The Captive Mind*), James Conant and Sidney Hook. Kuhn's initial term "ideologies", later replaced by "paradigms", appears to be an application of this same idea in the context of the history and philosophy of science. If Reisch is correct here, ironies abound. While the captive mind for the anti-

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[1] All page numbers refer to *Kuhn's Structure of Scientific Revolutions at Fifty: Reflections on a Science Classic* unless indicated otherwise.

[2] Kindi & Arabatzis, 2012.

[3] Devlin & Bokulin, 2015.

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communists was anathema, for Kuhn it was a necessary positive requirement for scientific development in the form of paradigm-based problem-solving and eventually also for scientific revolutions. The benign nature of "the captive mind" reaches still further. Reisch observes aptly that "in the case of Kuhn and his Aristotle experience, sudden ideological conversion was constructive for postwar history and philosophy of science" (22).

M. Norton Wise's "A Smoker's Paradigm" vividly paints the image of Kuhn thinking so intensely that even his cigarette is reduced to ash in his deep focus while everything stops. This image illustrates the intensity of conceptual problems and paradigms of science in Kuhn. "A paradigm … was a very narrow technical thing, the possession of a small group of people who had access to its *precise* and *esoteric* content, accessible to *professional* practitioners of a subspecialty alone" (32; original emphases). According to Wise, the small number of people who shared the paradigm is important in contrast to the assumption that Kuhn was concerned with major conceptual replacements and revolutions. In this reading, even subspecialties of a field have their own paradigms. And a community that shared a paradigm and effected its transition may be reduced to five scientists (in the case of Black-Body Theory and the Quantum Discontinuity, 1894–1912 (Kuhn, 1978)): Boltzmann, Planck, Lorentz, Ehrenfest and Einstein! In this way, Wise calls attention to what I would call a "professional paradox." If Kuhn surmised that the core of science is insulated from sociological forces, how is it that the later generations thought that he pointedly wished to explain science and its transitions sociologically? While many saw Kuhn as a sociological radical, it looks like he was in fact an old-fashioned intellectual historian.

Peter Galison describes Kuhn's work and experience as a civilian and war researcher in physics. His work with the early American pioneer of quantum physics Van Fleck was "a lap behind the lap behind the quantum upheaval of 1926" in Europe at the time (49). A more interesting and relatively distinct part of Galison's essay details Kuhn's intellectual developments through his reading lists, margin notes and note books. A central figure and influence was Piaget. Kuhn came up with an idea that physical science, social science and Piagetian child psychology contain moments of "formal contradiction" that must be resolved before entering a new developmental stage. Galison's central observation of this early work of Kuhn's is that "a psychological ordering of the world dominates, subordinating both logical and physical orderings of the world around us" (55). In this way Galison – similarly to Reisch, Wise, Kaiser and also Daston – focuses on the priority of the mind in theory building. Progress comes through a "conceptual reorientation" above all and only secondarily through new observational data.

In his essay David Kaiser investigates how psychological theories and thinkers influenced Kuhn and shaped *Structure*. Kaiser claims that the influence was very significant, even the extent to which "virtually every important claim in the book … is motivated by analogy to experimental psychology, rather than defended by close historical analysis of primary sources" (80–81). Kaiser studies the influence of the books and people that Kuhn read and met from the late 1940s until the early 1960s. He also charts Kuhn's correspondence after *Structure* had appeared. The surprising discovery is that almost twenty percent of the correspondence came from psychologists and that Kuhn also sought their audience. It appears that many of them thought like the psychologist Edwin G. Boring that *Structure* is "a book about psychology" (83).

Why was Kuhn then so interested in developmental psychology? Kaiser hints that it provided him material with which to challenge logical positivism and specifically the positivist theory of neutral observation language. As many authors in this volume also remark, Kuhn emphasized, in Kaiser's words, the "conceptual orderings of the world and the roles that psychological 'predispositions' play in scientists' observations" (79). It is then striking that the later Kuhn disavowed psychological analogies so explicitly (and moved "beyond a description of individual knowers" (88) already in the postscript of *Structure*).

Both Ian Hacking and Lorraine Daston assess Kuhn's concept(s) of paradigm. While Daston analyses paradigms as the shared constellation of group commitment, Hacking considers paradigms as shared examples or exemplars, linked to the history of reasoning. It is the latter aspect that Hacking focuses on in practice. Surprisingly, his paper is mainly a study of Aristotle and other history of argumentation by examples, which can be understood as a variation of inductive reasoning. In actuality, by studying the history of argumentation, Hacking's paper itself forms an argument by example, exemplifying with recourse to similar cases in history why it was also so difficult for Kuhn to provide a satisfactory analysis of a paradigm by example. The reason is that the whole family of related notions, such as analogy, models, similarity, likeness and resemblance, are "relational; what is enlightening to one audience is often devoid of use or content to another" (109). Instead of faulting Kuhn for obscurity, Hacking praises Kuhn "for giving new fire" to this kind of reasoning (109).

My personal favourite in this collection is Lorraine Daston's paper. Masterfully, she charts the recent history of science studies remarking the turn towards local studies and explanations: "the focus in these disciplines has shifted from the streamlined to the dense and detailed" and crystallizes this turn wonderfully as a "shift in intellectual sensibility from Bauhaus to Baroque" (117). If "structure" was a word "to conjure with in 1962," in the decades after, it feels "dusty" and "dated" (116). Instead of looking for the shared structure, historians of science have become full-blooded historicists. Daston notes something striking yet entirely plausible: because historicism (and localism, I would add) has fragmented "the once-monolithic 'science' … the history of science may soon dissolve its own subject matter" (118).

However, even if this was so, there is something that points history of science into another direction. That is, the history of science is a form of history writing and it shares the mode of writing histories with other historiographies, whatever exactly that mode turns out to be. Also, Daston concludes that the reason why the cherished philosophical concepts of the earlier generation (incommensurability, irrationality, relativism) fell off the agenda is that "historians of science became historians" (119). In other words, even if there wasn't one monolithic science, there still might be (fundamentally) one historiography with shared problems and challenges.

Kuhn's idea that the mind shapes the world and our vision of it also holds a central place in Daston's essay. The whole essay is written on two simultaneous levels: one drafts a psychological experiment with inverted glasses and follows how the test subject learns to live with the inverted world within a week. This is contrasted to an account of a "changed vision" in science studies since the appearance of *Structure*. Yet Daston's actual target of criticism is that no one has managed "hammering out a systematic, analytical language for talking about knowledge without rules" (126). Notions like "intuition", "tacit knowledge" and "conversion" carry murkiness around them, raising the suspicion that they are not knowledge at all. The last "bastion of the otherwise outmoded 'structures' of the 1960s" can be found in a belief in rationality that is rule-governed like an algorithm (and separated from more individualistic and deliberative reason and reasoning). Instead of worrying why non-rule-bound knowledge can nevertheless be systematic, we ought to consider the meaning of "rule" and be concerned with its narrowing to its algorithmic variant, writes Daston. Perhaps reasoning from exemplars can be shown as rule-bound in a weaker

sense and then "yield a structure capacious enough to bring history, philosophy and the sociology of science … back together" (130).

Daniel Garber's essay deals relatively little directly with Kuhn. Nevertheless, it analyses the interesting Kuhn-relevant question whether *the* scientific revolution of the sixteenth and seventeenth centuries really was a revolution, and the answer is given in the title: "Why the Scientific Revolution Wasn't a Scientific Revolution, and Why It Matters." While not denying that this was a period "remarkable for its fecundity" (134), Garber asks whether it is appropriate to compare it to a (political) revolution in which an authority is replaced fairly quickly? Was it the replacement of the traditional Aristotelian science by the new science (as even some contemporaries, such as Descartes, saw the matter)? The reason why it was not is that this period comprises "a bundle of competing paradigms" (142) rather than a replacement of one dominant paradigm by another. Garber suggests that perhaps Kuhn would not have been disturbed by this conclusion because his examples were of a more limited kind, such as Copernican, Newtonian, Darwinian etc. revolutions.

Angela N. H. Creager's paper is another discussion about the concept of paradigm. This is so at least nominally because the focus of the paper is in application, and the direct contribution to Kuhn scholarship is therefore fairly thin. Her paper considers the applicability of "paradigm" as exemplars or extendable model solutions in the context of biomedicine. Creager's essay details certain recent innovations brought by the material turn in science studies, such as Rheinberger's studies of "experimental systems". Her message is that "model systems [in biomedical sciences] … function quite clearly as exemplars in Kuhn's sense, if not explicitly in his terms" (153).

Andrew Abbot's paper provides lots of bibliometrical details of Kuhn's wide influence. He offers an "examination of the phenomenon of Kuhn" by studying Web of Science citation data. The number of citations to *Structure* between 1962 and 2012 is simply staggering: 15,635. That equals almost one citation a day! Citation data implies that Kuhn has had three major audiences. The first one is his "home fields" of philosophy, history and philosophy of science, and sociology of science. The second one is the social sciences, and the third one is applied fields, such as management literature. According to Abbot, this data shows that *Structure* continues to be of interest in his "home field", that it has gained a generic citation status in the social sciences, and that the applied sciences use it heavily in unspecifiable ways. Two interesting details emerge from this data. One is Abbot's observation that all but a handful of citations are for the whole book, and his conclusion from this is that "we can safely guess that the majority of those who have cited this book have not read most – or perhaps any – of it" (175)! If this is so, Kuhn's *Structure* is like a cultural icon to be displayed but not studied. The other interesting feature is the fact that Kuhn's book has apparently never had much influence in general history. This is striking considering its earth-shaking influence in the humanities in general and that it is a book about the *history* of science, after all.

Are there any general lessons to be inferred from *Kuhn's Structure of Scientific Revolutions at Fifty*? I think that at least some observations of recurrent features of the Kuhn phenomenon can be made. The first is the surprising claim about the lack of historical analysis in *Structure* (Reisch and Kaiser). Just think about it: a book about the history of science which transformed philosophy of science and gave an incentive to history and philosophy of science by the most influential historical philosopher of science is not fundamentally based on historical research. Instead, as the several authors stress, Kuhn assumed the priority of the mind, the psychological-conceptual, in forming our view of the world (Daston, Galison, Kaiser, Reisch, and Wise). This book demonstrates the links of this assumption to the mind-set of his own time and to scholars in psychology, and how this assumption formed Kuhn's own view of scientific revolutions and paradigm shifts. The third theme that emerges is the focus on the paradigm concept. Several papers in this book analyse the meaning of it in Kuhn and try to find applications either in the history of science or in contemporary science (Hacking, Creager, Daston, Garber, and Wise). The concept of paradigm might be one of the few, if not the only, concept of *Structure* that interests scholars also in a sense other than historical. Specifically, the notion of paradigm as exemplar is still seen as fruitful and applicable. The fourth general observation has already been emphasized: *Structure*'s massive influence and popularity over several decades in the humanities and beyond. In light of the numbers brought forward it becomes even more striking and concrete (Abbott). Finally, it is the elusiveness of *Structure* that once more awes. Just as the Barthian idea of the death of the author indicates, the author Kuhn has not had much control over his own product, for it is unlikely that he could have meant all the ideas attributed to him over many decades (see Kaiser, 72).

2. Denaturalising Kuhn

Kuhn's role in the rationality disputes is something that could have been studied further in this book. I don't mean only in relation to the dispute of scientific method narrowly conceived, as discussed by Popper, Lakatos, Feyreabend and Kuhn himself. There is more to be said about this too, but the real issue that calls for more reflection is the status and role of rationality itself in the history of science and beyond.

The legend of Kuhn and *Structure* has it that Kuhn is the prime naturalizer of scientific rationality. *Structure* challenged the rigid ahistorical logical positivist's and empiricist's view of science. What Kuhn and *Structure* showed, above all, is that there is no one ahistorical form of rationality, or scientific method. Standards of evaluation are variable from paradigm to paradigm and may be even incommensurable between two paradigms. To think that there is something like verification (with stable observational language), corroboration, falsification or some other shared method of science throughout the history of science determining the rationality of theory choice is just a pipe dream of the Old School Rationalist. The legend states that Kuhn appealed to the history of science to prove the point.

In a mundane sense, Kuhn and *Structure* did give us a "naturalist" lesson: all views of science should be tested empirically, i.e. with recourse to the history of science. At a minimum, there should be a two-way feedback loop from empirical research to our conceptions of science and back. There is no reason to challenge this more mundane view that the empirical and the conceptual, history and philosophy, are relevant to each other. This is widely accepted and it is all for the best of the history and philosophy of science.

Beyond this mundane interpretation of Kuhn, nevertheless, there are perhaps two more interesting interpretative lines of Kuhnian naturalisation. Let's call them the right-wing and left-wing naturalism in science studies. The right-wing naturalists emphasise that rationality is always contextual and its specific form can only be gleaned off from studying specific scientific practices. Although there is scientific rationality, or perhaps rationalities, there is no general theory of scientific rationality. Against the background of the history of philosophy that may sound radical enough, but the left-wing naturalists go further. To them, the lesson from Kuhn is that any idea of rationality is empirically baseless and reflects the philosopher's normative obsession. Rationality as a concept has no use value if our aim is to understand the functioning of science. Therefore it is better to abandon the notion of rationality (among other normative concepts) altogether. Science can be fully explained sociologically and by reference to community-based

practices. The right-wing naturalists are typically philosophical naturalists (e.g. Bird, 2000, 2005; Kornblith, 1995; Roth, 2006; Zammito, 2004) and the left-wing naturalists tend to be sociologists and historians of science (e.g. Barnes, 1976; Bloor, 1991; Shapin, 1992, 2010; see also; Golinski, 2005).

Are these images of Kuhn the naturalizer accurate? I will briefly consider Kuhn's own words and practice as a historian, taking into account the views expressed in this book. It is probably evident to most that the early interpretation of Kuhn as irrationalist by Lakatos (e.g. Lakatos, 1970, p. 178) and others is mistaken. Even if Kuhn did not accept that there is a uniform scientific method, he did not wish to reject rationality altogether. Kuhn is often quoted saying that scientific rationality simply is the best of rationality, albeit not perfect, that exists: "I do not for a moment believe that science is an intrinsically irrational enterprise … Scientific behavior, taken as a whole, is the best example we have of rationality" (Kuhn 1971, 143–144). The right-wing naturalism appears thus plausible.

However, some of Kuhn's words and specifically his practice make the naturalist interpretation problematic. First, Kuhn saw himself as "a pretty straight internalist" (2000, 287). The idea that there are rules internal to the content of science does not sit well together with the historicist-naturalist assumption, which says in effect that rationality does not have an essence and may in principle undergo transformations without any limits. When Kuhn's practice is studied, it is clear that a standard set of epistemic values feature prominently in *Structure* and emerge in his other writings recurrently: consistency, coherence, scope, empirical adequacy and fruitfulness. These are values and do not provide an algorithm of rationality but they offer nevertheless clear guidance on theory choice.[4] Daston's suggestion that the notion of rule should be something laxer than an algorithm appears apt in this regard, although in its current form the suggestion remains an inconclusive gesture.

The main problem for the naturalistic interpretation is that, using the set of epistemic values I just listed, Kuhn identifies an invariable and inter-paradigmatic framework of rationality. The real shock effect of this volume is that it further diminishes the role in Kuhn of historical research, and historicism. The latter is replaced by a pre-empirical mind-set, which moulds both Kuhn's own thinking and his view of how science makes the world intelligible and observational data comprehensible. If this is true, Kuhn had more or less decided how science works prior to any empirical-historical studies (through his Aristotle-experience). He then fit historical studies to his preconceptions. Indeed, I think the label "Kantian with movable categories" (Kuhn, 2000, p. 104; 207; 264) is a more appropriate characterization of Kuhn's position than naturalism. The idea is that these conceptual categories make the world intelligible – they make "our world" – but they are not fixed in the same sense as the real transcendental categories. But what would explain their relative stability? And where do they stem from, if they are not like Kantian universal transcendental categories?

These are big questions and impossible to answer with any satisfaction in anything less than a book. Nevertheless, I venture a suggestion. Kuhn (and others) might take the Wittgensteinian idea of language community seriously here and use it to understand scientific rationality too. I find particularly fruitful Gilbert Ryle's idea that there is a "logical geography of concepts" (Ryle, 1949, p. 10). In other words, there are specific ways of talking about concepts moored in a language community. They are relatively fixed because a community is accustomed to using them and their members typically learn them as infants. Kuhn himself seems to have acquired the thought of a "captive mind" in his linguistic community and then applied it to the history of science. And because there is a right way and a wrong way of talking about certain concepts in a community, determined by their "conceptual grammar," "rationality" cannot be and mean just anything in our discourse. The "logical geography of concepts" may change at extraordinary times, but not easily and not without friction. The "logical geography" thus provides us with the rules of the *conceptual*, based on a communal praxis, but on nothing like an algorithm.

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[4] For an attempt to develop a coherentist epistemology on this basis, see Kuukkanen, 2007.