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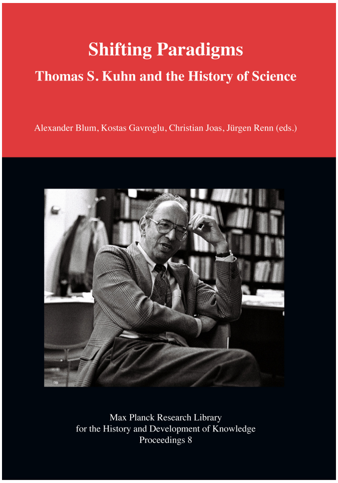

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Chapter 14
On Reading Kuhn's *Black-Body Theory and the Quantum Discontinuity, 1894–1912*

Richard Staley

I am one of that small group of people for whom Thomas Kuhn's *Black-Body Theory* was much more important than *The Copernican Revolution* or *The Structure of Scientific Revolutions*. Although both his earlier and more widely-known books were set in my undergraduate courses in History and Philosophy of Science at the University of Melbourne in the 1980s, *Black-Body Theory* was clearly the most vital and engaging book amongst those I read while writing my doctoral dissertation at the University of Cambridge on the education and early career of the German physicist Max Born. It is very likely still the most dog eared and coffee stained book on my shelves, and I have often lent it out—since I have urged it on any serious students of the history of physics as the most fundamental and exciting study in the intellectual history of our field. Yet the book has been approached so differently over time that I can hope that a short and relatively informal note on just some of the ways it has been read will contribute to the broader aim of building a better understanding of the history of the history of science.[1]

With or Without *Structure*?

Black-Body Theory is one of those relatively small number of books that attracted such diverse responses that the controversy it aroused on its publication in 1979 helped bind the reviews it received to the history and understanding of the book itself (it shares this fate with Paul Forman's article on Weimar culture and Andrew Pickering's study of quarks, but not Gerald Holton's *Thematic Origins of Science* or Peter Galison's *Image and Logic*, all significant contributions).[2] As a result, quite a few of those who read Kuhn's book, particularly in the period through to 2000, would also have read several early responses—perhaps especially the reviews of Martin J. Klein, Abner Shimony and Trevor Pinch in *Isis*, and of Galison in the *British Journal for the Philosophy of Science* (Klein, Shimony, and

[1] See Staley (2013) for a study of the history and historiography of physics in the twentieth century.
[2] Forman (1971); Galison (1997); Hendry (1980); Holton (1973); Kuhn (1978); Pickering (1984).

Pinch 1979; Galison 1981). Even if they did not read any reviews, the article Kuhn published in 1984 and included as an afterword in the reprint of the volume in 1987 would have indicated some of the problems Kuhn faced convincing his contemporaries.[3]

What such reviews showed very clearly was that many were puzzled why a book that made such a major revisionist argument about the origins of quantum theory had so little to say about whether and how the conceptual apparatus that Kuhn had offered in *Structure* applied to his consideration of the work of Max Planck, Albert Einstein and others. The basic question of whether this was a revolution was answered in the affirmative, even as Kuhn's account threw into question its timing—in ways that he subtly illustrated in the first paragraph. This noted that Part One would describe the conception and gestation of the new quantum theory in Planck's work before 1906, while Part Two would offer an account of its birth and gestation in the work of others, and Part Three would consider Planck's response to their "apparently revolutionary reformulation" (Kuhn 1987, 3). That is surely a case of offering a back door entrance to what has normally played the starring role! Yet even while Kuhn so obviously urged a new chronology on the basis of his argument that Planck did not hold the concept of energy quantization usually attributed to him, Kuhn left unsaid what stood as the anomalies, crises and paradigms at issue, and whether these events illustrated incommensurability and gestalt switches. Kuhn's counter that the apparent "misfit" amongst his publications was in fact the best and most representative of his historical works would only have further puzzled many of those who raised questions about the issues that had been so central to Kuhn's philosophical approach to the history of science (Kuhn 1987, 349). Read in the light of *Structure*, then, the new book was a disappointment. Coupled with the fact that Kuhn had not been able to persuade Klein, the preeminent authority on Planck and the history of statistical mechanics, the controversy and discussion around its publication might even have given the impression that this book was destined for a short shelf life. Unlike the discussion surrounding Forman's article—which occasioned exchanges and debates on both the methodological questions around internal and external approaches to the history of science and on Forman's characterization of the period—many of those who responded to Kuhn's argument primarily urged Kuhn to clarify the implications of the book for his methodological stance towards scientific change. Rather than black-body radiation they were more interested in Kuhn's body of work.

[3]Kuhn (1987); the afterword had originally been published in Kuhn (1984).

Paradigms Lost and Found

As editor of *Isis*, Arnold Thackray found a pithy way of summing up this kind of impression by asking in the title of their review symposium whether this was "Paradigm Lost?" Yet when I read the book in the mid to late 1980s, it seemed immediately obvious that many of Kuhn's philosophical assumptions about the nature of revolutionary change were implicit in his approach (and especially his interest in relating technical details to changes in world views or paradigms writ large). I was hardly tempted to ask for a more detailed explication of the relations between *Structure* and *Black-Body Theory* either. One reason for this is surely the fact that by then both philosophically and sociologically oriented studies of the history of science were much less immediately engaged with Kuhn's *Structure* and distinctions between revolutionary change and normal science. Instead they debated realism and relativism around the work of Latour (especially) and Pickering's *Constructing Quarks* (in modern physics): it seemed everyone wanted to find their own way of saying that despite the underdetermination of theory by data, nature did constrain theoretical choice. But even more important than changing philosophical and sociological interests, I read Kuhn's *Black-Body Theory* against the background of other historical studies of turn of the century physics, and in that company it simply stood out.

As I began my dissertation on the early work of Max Born I naturally worked through earlier scholarship on the history of relativity and quantum theory. I can vividly remember being diverted by the engaging clarity of Klein's studies, reading some of them in the archive at the Staatsbibliothek Preussischer Kulturbesitz in Berlin when I should have been concentrating on Born's letters, for example. But it was Kuhn's *Black-Body Theory* that really stunned me. This was in part for the ambition of his claims and for the power of historical disclosure that he demonstrated, making apparent tensions that had previously simply been invisible to scientists and historians who had glossed over features of Planck's work that Kuhn showed demanded close attention. But it was also because he explained so much about the development and reception of quantum theory by a close investigation of the relations between interpretation and theoretical resources, as these had been developed through Planck's independent research, or by others such as Einstein, Ehrenfest and Jeans in the course of their education and research papers. For me, this was an explanatory intellectual sociology, and history of science at its most revelatory.

So *Black-Body Theory* was undoubtedly the most important work I read as a student, and I took it as an exemplar—a paradigm. The most important lesson I thought it conveyed was that individual variations in the approaches taken by different physicists could be rigorously related to their different educational backgrounds and fields of mathematical and physical expertise (a quite general lesson, applicable to many kinds of resources). Further, Kuhn showed that when physicists like Einstein, Ehrenfest and Lorentz brought their diverse expertise to bear on Planck's research, they not only interpreted and reinterpreted Planck's work—sometimes radically—they also abstracted it from its original context, thereby bringing it within the range of expertise of greater numbers of physicists (Staley 1992, chap. 6). Thus I saw Kuhn's account of Planck and responses to his work to have raised a similar question about authorship in quantum theory to those that were so evidently at issue in discussions of the relative contributions of Lorentz, Poincaré, Einstein, Minkowski and Hilbert in relativity (debating authorship was a major issue in the historiography of modern physics of the period). I thought Kuhn's thesis of the incommensurability of scientific paradigms had led historians to overemphasize the difficulties scientists exhibit in understanding different points of view in theoretical or experimental work. Yet whereas historians of relativity tended to relate acceptance of Einstein's contributions to proper or improper understanding, Kuhn related the character of understanding more thoroughly to training and research backgrounds and approached variation without the normative strictures customary in such approaches to relativity. In these respects his work was exemplary, methodologically valuable for its display of critical, investigative historical research. Had I looked back to *Structure*, I would have seen Kuhn's *Black-Body Theory* as a major contribution to the "historiographic revolution" he discerned in the study of science, but one that was most important as a practical paradigm.[4]

Later I saw particular limitations in Kuhn's approach. He had focused largely on theory and intellectual factors conveyed through technical education (formal or informal). But although he said so little about why black-body research was so important industrially, and we needed the work of David Cahan and Dieter Hoffman (taking Kangro's study further) to begin recognizing this, Kuhn did point very clearly to the importance of experimental work on the specific heat of solids, and Nernst's advocacy, for the propagation of Einstein's work. Thus, Kuhn indicated the significance of experiment and more materially focused histories, even if he had not written one himself (and one can see the same impetus in the diverse factors he saw to be important in his account of the "simultaneous discovery" of thermodynamics).[5] Similarly, the work of John Heilbron (drawing on Stanley Goldberg's studies of Planck) also integrated the closely focused technical history that Kuhn had offered with a more wide-ranging understanding of the cultural importance of absolutes in Planck's thinking (Goldberg 1976; Heilbron 1986). In this respect, too, Kuhn's work had offered an important point of origin

[4]For a brief description of this historiographical revolution, see Kuhn (1996, 3).

[5]Cahan (1989); Hoffmann (2001); Kangro (1976); Kuhn (1987, chap. 9), 1959).

for rigorous studies of the cultural significance of technical developments—even if with his focus on rewriting our understanding of "modern physics," Kuhn had been blind to just how novel and interesting it was to think of Planck's work as either a contribution to or departure from "classical physics." Thinking equally of industry, experiment and cultural studies, it would then be a mistake to focus too closely on Kuhn's own inclinations, and far better to see what his colleagues and students could make of his work. In this respect we should recognize that the work of one of the great theorists of the scientific community was as important for the concrete seeds it offered for quite different studies within our own community.

It did offer a stimulus to studies of quantum theory too, and as the publication of studies around the centenary of Planck's famous papers showed, over time Needell, Darrigol, Gearhart and later Badino have been able to offer a still more refined understanding of Planck's work than Kuhn had realized, often also taking pains to show why there had been such diverse responses to Kuhn's argument. Conceptually, these historians often found it important to note that Planck's caution towards the value of microphysical assumptions may also have led him to remain uncommitted about the nature of the energy elements that he invoked in following Boltzmann's work. Historiographically, their work has surely made reference to the original debate around Kuhn's study less necessary than it was previously, although appreciating its character will remain important to understanding the history of the history of science and several features of those earliest reviews have been confirmed by these later studies, as well as my own research.[6]

Along these lines I want to conclude by highlighting two points underlined by these early reviews. The first is the skepticism Pinch expressed about the central role of the dichotomy between quantum and classical theory in Kuhn's work, noting that Kuhn had assumed their incompatibility and perhaps incommensurability, without due historical analysis. The second is Galison's argument that Kuhn's search to exhibit a coherence in the work of pivotal scientists, both within their work and with prior paradigmatic problem solutions, might not be justified in the fluid situation characteristic of innovative work.[7] In my own research on the co-creation of classical and modern physics I used methods I could describe as Kuhnian to trace the earliest uses of concepts of classical in a careful chronological sweep through a wide variety of physical studies. Yet I found to my surprise that references to classical theory were absent from just those discussions of the

[6]Badino (2009); Darrigol (2001); Gearhart (2002); Needell (1980).

[7]Galison (1981, 83–84); Klein, Shimony and Pinch (1979, 438–439). Even when setting out the key elements of the historiographic revolution he saw in recent work in 1962, Kuhn had emphasized that analyzing earlier work from the viewpoint which gave it maximum internal coherence (and the closest fit to nature) was integral to the attempt to display the integrity of past science and trace "different, often less than cumulative, developmental lines for the sciences," Kuhn (1996, 3).

equipartition theorem in which both physics text books and the studies of historians like Klein and Kuhn would have led one to expect them to appear. As I worked chronologically forward from the 1890s through the 1900s and past the critical years following Einstein's work in 1905 and 1906—working systematically with research on mechanics, thermodynamics and other subjects—I began to suspect that the first occasion on which I would find our now customary use of the classical in conjunction with the equipartition theorem would occur in the papers and transcribed discussion of the Solvay Council of 1911. Had I reread Kuhn's *Black-Body Theory* at this point I would have had a further reason for betting on what turned out to be a good guess.

On Kuhn, Writing Backwards

In its highly autobiographical preface Kuhn tells us that he did not initially intend to undertake the project that led to *Black-Body Theory and the Quantum Discontinuity, 1894–1912*, in passages that also underline just how important his own experience of gestalt shifts and incommensurability was to the conduct of Kuhn's research. What Kuhn had wanted to write was a history of quantum conditions up to the inventions of Lande's vector model of the atom and Bohr's theory of the periodic table in 1922 and 1923 respectively, and the most unclear part of his plan was the appropriate date from which to begin. Looking for origins, Kuhn had then decided to reexamine Planck's work chronologically from 1895 in order to establish the first occasions on which physicists "asked about the nature of the restrictions placed by the quantum on the motion of systems more general than Planck's one-dimensional harmonic oscillator." That endeavor had ultimately given Kuhn the possibility of a radical rereading of Planck's key papers of 1900 and 1901; but I had not previously noted that Kuhn also tells us something more about his starting point. Revealingly, he found this search necessary because although he knew quantum conditions had been a major point of discussion at the first Solvay Congress in 1911, neither the proceedings of the congress nor the extant secondary literature provided clues to the origins of such questions (Kuhn 1987, vii). The comment indicates that even as Kuhn sought origins and embarked on a new account of Planck's introduction of energy elements, his reading of the Solvay Council proceedings had helped provide a concept of the classical that Kuhn unwittingly read back into that earlier period. While Kuhn warned us against a teleology that would seek the permanent contributions of an older science to our present advantage, in this instance he remained unaware just how deeply his own understanding of Planck's work had been shaped by Planck's innovative use of a concept of past, "classical" physics in 1911—one that melded previous, incompatible uses of the classical in the fields of mechanics and thermodynamics, and did so for present advantage.[8]

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[8]The point is argued in Staley (2008, chaps. 9 and 10).

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