A Reminiscence of Thomas Kuhn

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In the fall of 1967 I entered Princeton as a Freshman intending to major in physics but interested as well in history. The catalog listed a course on the history of science, taught by a Professor Thomas Kuhn with the assistance of Michael Mahoney that seemed nicely to fit both interests. The course proved to be peculiarly intense for something about what was, after all, obsolete science as, each week, hundreds of pages of *arcana* from the distant past had to be absorbed. Professor Kuhn would pace back and forth in lecture, smoking intensely and talking rapidly to an elaborate outline drawn on the board at the beginning of each class. In tutorial, Mahoney (who passed away in 2009) developed Kuhn's points, forcing students to grapple with the meaning and significance of the many complicated texts that were assigned. That summer I worked as Mahoney's research assistant and, subsequently, as Kuhn's also.

Though the *Structure of Scientific Revolutions* was assigned in that class, Kuhn never put much explicit emphasis on it; he lectured almost entirely about the historical materials we were reading. Nevertheless it was clear that he had a guiding vision about science. Everything he spoke about, from Ptolemaic eccentrics to stationary orbits in the Bohr atom, seemed to exemplify a way of thinking about science that was certainly unusual for the time. It seemed that he was continually trying to excavate a structure beneath the dead science's apparent surface, something that could provide a key to understanding how it worked. He would often emphasize precisely what seemed to be the oddest, or the most irrelevant, passage or point in the reading. Kuhn never, or rarely, spoke explicitly about paradigms, normal science or incommensurability, but every story he told had things very much like those three elements at its core. Yet they took

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their shape and meaning not through explicit definition but rather through the examples that he developed, and through the ways he answered questions.

During my time as Kuhn's research assistant we would meet every week or two to talk about old physics. He would always emphasize the need to uncover what kinds of characteristic problems were at issue in the past, and about how these problems connected to mathematical and theoretical structures, though not much at the time about experiments proper. In the spring of 1972, Kuhn taught a graduate seminar on the history of thermodynamics. The readings—all of them primary sources—had been carefully prepared and put on reserve. Each week one of the students was responsible for taking the class through the texts. Kuhn did not want a simple summary of relevant issues. He expected you to have figured out precisely what made the text tick. He already had pretty strong notions about the materials, and if you came up with something different from what he had in mind then you had to argue for it line by line, sometimes equation by equation (since most of the texts dealt with in that course were strongly mathematical).

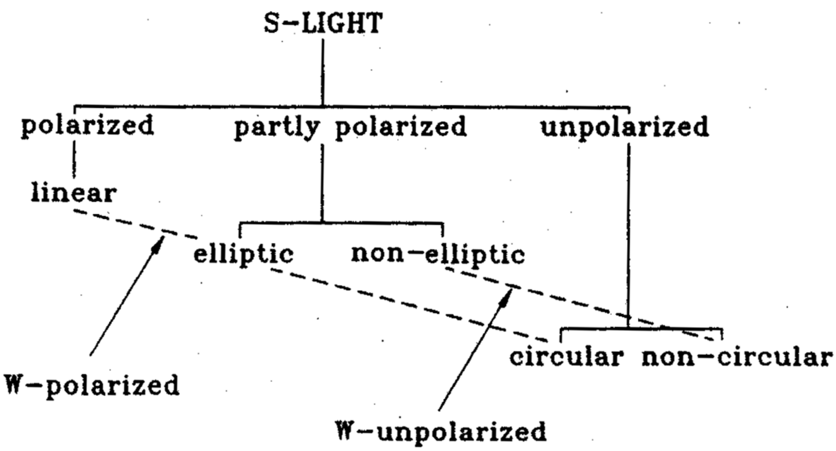
Kuhn's most detailed effort to work through a body of past physics—his *Black-body Theory and the Quantum Discontinuity*—appeared in 1978. He had been hard at work on it since 1971. To those who knew him well over the years, the book itself very nicely exemplifies Kuhn's special approach to the history of science as well as his particular views about scientific development. Like most things that he wrote, *Black-body Theory* generated controversy, some directed at its apparent failure to apply what he had himself laid out in *Structure,* some directed at his specific, technical claims. It seemed to many of us who knew him that Kuhn was not bothered much or even at all by the former critique, but he was very much concerned with technical criticisms. His need, even compulsion, to find *the*—not *a*—core of meaning that unites a disparate series of texts, to extract that largely-implicit structure and to display how it governed and connected to a set of canonical problems, powerfully directed his historical research. Technical criticism accordingly bothered him a great deal, precisely because it went to the core of what Kuhn took to be his central historical task, which was to uncover the hidden integrity of past science.

As the years went by Kuhn increasingly found historical research to be difficult. There seem to have been two reasons for his growing reluctance to read or to do history. He had trouble absorbing secondary work, in major part because he brought to histories the same intense commitment to the text's meaning that he brought to source materials. Vagueness bothered him no end, as did failure to produce the sort of analysis that he found most useful and interesting. But Kuhn was also not himself inclined to grapple with archival materials; he focused almost all of his own historical work on printed works. Yet, and he knew this to be so, the very structures that he so strongly wanted to uncover could often only be excavated from unprinted materials.

Kuhn's move to the Department of Philosophy at MIT in 1979 exemplifies his own sense that the issues with which he was most directly concerned were philosophical in nature, though he remained deeply committed to careful historical understanding, as he conceived it. In 1986 he wrote me a letter that contained the following remark: "I think of my primary talent as a hard-earned ability to read a text, find a way to make it make sense by discovering the conceptual structure that lies behind it. It's the experience of finding hidden structures that underlies *The Structure of Scientific Revolutions* and that I'm now back trying to analyse again." Those of us who studied under him, and many who knew him over the years, will recognize here his distinctive voice and point of view. Voice and view demanded and conveyed an uncompromising, rigorous attempt to push beneath the surface of technical work, to find out how it worked.

In 2001 I became director of the Dibner Institute for the History of Science and Technology at MIT, where each week a Fellow would give a talk. Tom attended many of these, and once a month or so we would have lunch together. During these last years of his life he was trying hard to develop a lexical understanding of what it is about scientific work that produces difficulties of mutual comprehension between proponents of different systems that ostensibly cover the same phenomenal range. The problem, that is, of incommensurability. Many of our talks ranged over examples of that sort of thing, taken not however from such wide-ranging schemes as Ptolemaic versus Copernican astronomy, but from much more limited structures, such as the arguments between proponents of an optics based on waves and those who thought in terms of rays. Or between British developers of electromagnetic fields and their German counterparts. Tom's new understanding orbited about his conviction that the deepest differences between scientific schemes concern the ways in which they respectively divide their universes into kinds of entities. Incommensurability, he thought, was not a vague difference in views, but a specific violation by the one scheme of another's affiliation among kinds—a violation of the principle that a given kind can be an immediate subset of at most one other. That, it seemed to him, was a general property of scientific systems which captures differences among them.

We spoke about this many times, which led me at his urging to write a paper applying the idea to the history of wave optics.[1] We talked about how to do this as the paper took shape, and the diagram that it included resulted directly from our discussions. The dark lines represent the kinds of polarized and unpolarized light that were deployed by those who thought of light in terms of rays in the early 1800s, satisfying the one-immediate-ancestor criterion. The dotted lines show instead how practitioners of wave optics grouped kinds of light together in ways that violated the groupings of ray practitioners. These differences had instrumental consequences that appear quite directly in the literature of the period. In our discussions Tom was interested for the most part in the categorical groupings, less so in their connections to measurement processes, though he did tell me that he intended to think through the latter in more detail in relation to kinds. He never really did find the time to do so.

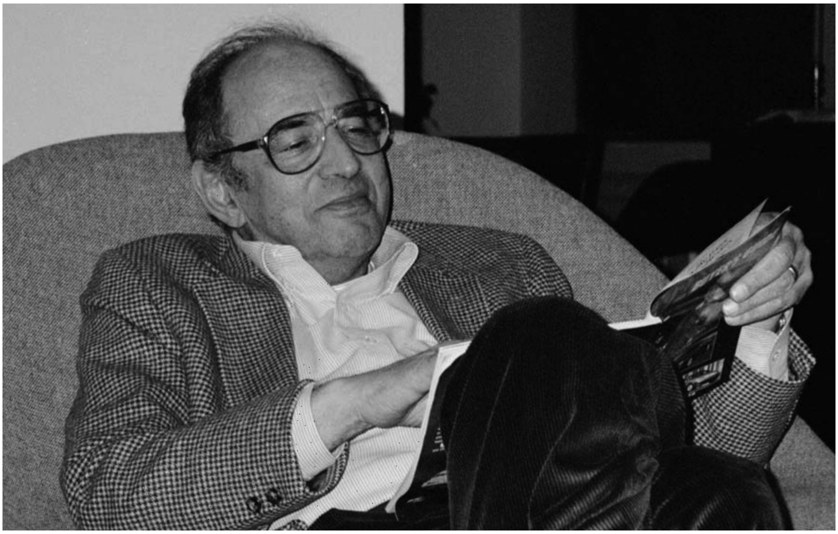


A tree of kinds for polarized light, developed by Buchwald during discussions with Tom Kuhn in the early 1990s.

I continue to think that Tom was substantially correct about the importance of incommensurability in scientific practice, and that the concept is best conceived in terms of a tree structure for kinds. Certainly his way of understanding cannot easily encompass the sort of thing that takes place when, say, someone trained as a physicist moves into biology, giving rise perhaps to new regimes with concomitant developments in social, cultural

1. "Kinds and the Wave Theory of Light." *Studies in the History and Philosophy of Science,* 23:39–74.

and institutional structures. Though Tom would occasionally talk about such things, he really had very little to say about them in later years since they do not map simply onto issues of incommensurability in the way that he had come to think about the latter. That notion occupied him to the end of his life and, he often told me, constituted his most important contribution to understanding the character of scientific work.



Taken by Buchwald at Tom and Jehane Kuhn's home on Memorial Drive in Cambridge in the spring of 1991.