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The re-emergence of hyphenated history-and-philosophy-of-science and the testing of theories of scientific change

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ABSTRACT

A basic premise of hyphenated history-and-philosophy-of-science is that theories of scientific change have to be based on empirical evidence derived from carefully constructed historical case studies. This paper analyses one such systematic attempt to test philosophical claims, describing its historical context, rationale, execution, and limited impact.

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The philosophy of science is a field of ancient vintage, dating at least from the time of Aristotle's *Posterior Analytics*. Earlier thinkers (especially Plato and Pythagoras) had interesting things to say about science but there was little systematic work on what we would call the philosophical foundations of science. History of science, as an active field of scholarship, was to emerge only much later. By the 19th century, we see earnest signs of attempts to integrate the two into a serious and coherent intellectual project. William Whewell, in his two-volume *Philosophy of the Inductive Sciences* (Whewell, 1840) and his three-volume *History of the Inductive Sciences* (Whewell, 1837), was perhaps the first thinker to devote most of his career to elaborating a detailed philosophy of science and testing it against the historical record. A few decades later, Ernst Mach and Pierre Duhem picked up the project, again attempting like Whewell, to use the historical record of the sciences as a form of testing the claims philosophers were making about science.

The emergence of logical positivism largely decimated that approach for two generations.[1] Convinced that newly discovered formal techniques were sufficient to provide all the grounding necessary for a new philosophy of science, those in–or those influenced by the ideology of–the Vienna Circle generally disdained the idea that the history of real science might have a significant bearing on the adjudication of philosophical claims. After three decades of dominance, logical empiricism began to crumble under the weight of the failure of its own formalisms. By the late 1950s and early 1960s, a number of clever young Turks (including N.R. Hanson, Stephen Toulmin, Thomas Kuhn, Paul Feyerabend, Mary Hesse, Imre Lakatos, Ernan McMullin, and Jürgen Mittelstrass) were propounding once more the idea that any coherent philosophy of science had to be grounded in a sophisticated understanding of the twists and turns of the evolution of science.

By the mid-1960s, the debate had become intense. Kuhn's *Structure of Scientific Revolutions* (Kuhn, 1962) was being widely read, and in many circles widely hailed, as having shown that changes of belief in the sciences were rarely governed by fixed methodological rules and fidelity to the data. On the contrary, said Kuhn, every paradigm has its own methodology (between which he believed there to be no grounds for rational choice), scientists routinely ignore empirical anomalies, and paradigm change was a matter of a 'gestalt shift' (usually among younger scientists) not a Bayesian computation of empirical support. Feyerabend's resonant voice added to the chorus, with his insistence, following Kuhn, on the incommensurability of rival theories (in the sense of non-translatability), the intrinsic theory-ladenness of observations, and the radically changing rules of scientific rationality.

It was at that point in the late sixties and early seventies that our brief narrative begins. Prior to that, philosophy of science had generally been based in philosophy departments (with an occasional incursion in the natural sciences). Now a series of

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[1] There were notable exceptions during this period, most especially Koyré, Cassirer and Burtt.

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administrative shifts gave concrete expression to the view that history of science and philosophy of science were unconvincing when pursued in isolation from one another. While University College London had a unit doing history and philosophy of science (albeit not particularly visibly at first) since 1921, and Gerd Buchdahl had pioneered a similar group at Melbourne in 1946, history and philosophy of science remained largely institutionally distinct until the late 1950s.

Then things took off. Leeds took up the subject, apparently at Toulmin's instigation, in 1956. Cambridge came on board about the same time. Indiana created its HPS department in 1960, staffed initially by the medievalist Ed Grant and the philosopher Russ Hanson. Chicago followed suit with its Program in the Conceptual Foundations of the Sciences, as did Pittsburgh with its large department in HPS founded by Larry Laudan in the early 1970s.

What is especially of interest is not so much the rapid institutional growth of integrated HPS but the reaction of those of us entering the profession in the late 1960s and early 1970s, including including Dudley Shapere, Philip Kitcher, Clark Glymour, Ian Hacking, Jarrett Leplin and Larry Laudan. Like our immediate predecessors, we were convinced that the positivists had ignored the history of science at enormous risk to the plausibility of their formal analyses. At the same time, however, many of us were deeply distressed to see how clumsily Kuhn and Feyerabend had been in drawing philosophical morals from the historical record. Kuhn's *Structure* was filled with throwaway references to historical events, *none* of which he actually explored in any detail in the pages of that book. We were just as appalled at the blatantly relativist turn of the epistemology of science elaborated by Kuhn and Feyerabend and taken up by their growing number of followers in the social sciences, desperate to find reasons for discounting the claims of the natural sciences to objectivity and rationality.

Our focus, in contrast, was on developing philosophical accounts of science which both did justice to the historical record *and* which captured the complex and subtle rationality of scientific reasoning. We were not persuaded by the arguments for incommensurability and non-translatability and went to some pains to refute them. Nor were we convinced that scientists working in 'different paradigms' invariably or even typically failed to accept similar methodological principles of theory evaluation. The challenge, as we saw it, was whether we could construct new narratives of the history of science that could preserve its rationality without reducing that rationality to a set of formal, unchanging rules.

The issue of changing rules of scientific rationality through time was particularly vexatious for Gerd Buchdahl and Larry Laudan, and was one of the reasons why the two of us jointly founded *Studies in History and Philosophy of Science*. We agreed with Kuhn and Feyerabend that the history of science was replete with methodological disagreements among scientists but insisted that, just as the content of science had grown and progressed through time, so had the methodology of science. It was, and remains, our belief that the history *of* the philosophy of science must be an integral part of the larger history and philosophy of science enterprise. Scientists *have* learned from their mistakes and repeatedly corrected them; grasping the ways in which they have done so is essential to understanding how science can be regarded (as Peirce claimed) as a self-correcting enterprise. That is why we insisted in the "Editors' Note" in the first number of *SHPS* that the journal would (we hoped) include many studies "on the history of the philosophy of science," (Buchdahl and Laudan, 1970, p.2) since that evolution was, in our view, simultaneously a key part of the sciences and of the philosophy of science.

Indeed, as Larry Laudan recalls conversations with Gerd around 1970, we saw the field of HPS (and thus our plans for the journal) as a combination of the following:

- Historically-oriented philosophy of science (HPOS).
- Philosophically-savvy history of science (PHOS).
- History of the philosophy of science (HOPOS).
- History of philosophy and science (HOP&S).

We did not anticipate that either technology or sociology might enter the picture; and we certainly did not foresee the emergence of feminist epistemology of science nor the [de]constructivist approaches to science.[2]

With departments set up, a journal founded, and research being published, two intellectual worries continued to nag. First, our generation was not only reacting to what we regarded as the excesses of Kuhn and Feyerabend. Many of those who rejected the Vienna Circle (and who, like the logical positivists, were blissfully ignorant about the history of science), were likewise pushing claims about history, which seemed to be wholly without foundation. There was Putnam's 'miracles argument' that theories of the past that had been successful must have been true and that science through time has been 'convergent'. There was Wilfrid Sellars' overconfident claim that, if older theories were rationally replaced by newer ones, the latter must have been able to explain why earlier theories were successful to the degree that they were. Plenty of other philosophers of science (Richard Boyd, Bill Newton-Smith, Ilkka Niiniluoto) fell into the same trap. Many of us spilt a lot of ink trying to show that such claims were historically bogus. The history of science is not strictly cumulative; it is not true that later theories have always had the conceptual resources to explain the apparent success of their now-discredited predecessors.

Second, some of us were growing increasingly uneasy about the way ill-documented case studies were casually invoked as purported evidence for one or another of the various theories of scientific change. The series of "case studies"—a term that had gained currency in history and philosophy of science via James Conant's *Case Histories in Experimental Science* (Conant, 1948)—often undertaken by the promulgator of one or another of the theories of scientific change (or one of his students) simply failed to live up to its intended probative role. Indeed all the theorists of scientific change would have been quite horrified had the scientists they referred to treated evidence so cavalierly. The cases were patently not probative, and at best they were merely illustrative.

In the early 1980s, Virginia Polytechnic Institute and State University, better known as Virginia Tech, was forming a Science Studies Center. The perceived urgency of undertaking a more rigorous approach to the empirical foundations of scientific change in light of all the developments list above neatly coincided with the desirability of a joint project to bring the members of the Center together and to give the Center some visibility.

We chatted at some length with the social scientist, Donald T. Campbell. Not only had he thought long and hard about how to test theories when experiments were not possible (his pioneering study with Julian Stanley, *Experimental and Quasi-Experimental Designs for Research* [Campbell and Stanley, 1966] had become the standard in social policy evaluation) but was himself much interested in what was going on in science studies. With the philosopher Alex Rosenberg he had organized a conference on Epistemologically Relevant Sociology of Science in 1981, a project that he later described as premature.

[2] The distinguished epistemologist Susan Haack in 1993 (Haack, 1993) succinctly summed up the reaction of many of us then in the HPS community when she noted that: "Well, since the idea that there is an epistemology properly called "feminist" rests on false presuppositions, the label is at best sloppy. What is most troubling is that the label is designed to convey the idea that *inquiry should be politicized*. And *that* is not only mistaken, but dangerously so."

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