Boston Studies in the Philosophy of Science 263

Seymour Mauskopf
Tad Schmaltz *Editors*

Integrating History and Philosophy of Science

Problems and Prospects

Springer

INTEGRATING HISTORY AND PHILOSOPHY OF SCIENCE

BOSTON STUDIES IN THE PHILOSOPHY OF SCIENCE

VOLUME 263

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Problems and Prospects

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ISSN 0068-0346
ISBN 978-94-007-1744-2 e-ISBN 978-94-007-1745-9
DOI 10.1007/978-94-007-1745-9
Springer Dordrecht Heidelberg London New York

Library of Congress Control Number: 2011935944

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Chapter 1
Introduction

Seymour Mauskopf and Tad Schmaltz

Just over 50 years ago, the National Science Foundation began to support "history and philosophy of science."[1] In 1960, programs in history and philosophy of science were begun at Princeton University and Indiana University. The phrase—and its acronym, "HPS"—soon became current. The History of Science Society and the Philosophy of Science Association began to hold joint annual meetings. All of these developments certainly suggested the coming to interdisciplinary fruition of a natural affinity between the two fields.

And yet, inspection of the interactions between the relatively new discipline of the history of science and its more established philosophical partner belies, for the most part, both the natural affinity and the interdisciplinary fruition. Reflecting on his days as a history of science graduate student in the Princeton program in the late 1960s, Kenneth Caneva flatly asserts, in his contribution to this volume: "I neither knew nor cared where the philosophers were." From the philosophical side, Ronald Giere, who had been a faculty member of the Indiana program, famously characterized the yoking of history and philosophy of science as "a marriage of convenience."

Nevertheless, there remains a sense, perhaps nostalgic but perhaps also programmatic, that there are natural affinities and promises of fruitful interactions between these disciplines. The purpose of this volume is to explore the current relationships between them. The fact that both Caneva and Giere are contributors to our discussion is, itself, indicative of the interest among historians and philosophers of science to pursue exploration of these relationships.

Fifty years ago, the history of science was coming into discernable existence as a discipline both in this country and in the UK. There was, perhaps, a greater urgency among historians of science for disciplinary identity than for philosophers of science. After all, from the time of the *Prior* and *Posterior Analytics* of Aristotle, issues

[1] See Rossiter (1984) for the interesting and complex history of the genesis of NSF funding programs.

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S. Mauskopf, T. Schmaltz (eds.), *Integrating History and Philosophy of Science*,
Boston Studies in the Philosophy of Science 263, DOI 10.1007/978-94-007-1745-9_1,

in philosophy of science (formerly conceived as "natural philosophy") have always central to philosophical analysis. Before World War II, philosophers associated with the Vienna Circle had already established a clearly delineated philosophical perspective in "logical positivism." This emphasized the importance of the task of constructing a framework drawn from mathematical logic that would serve to advance current empirical research in the hard physical sciences, especially physics. Their framework entailed the analysis of completed systems of scientific knowledge, not the genesis of such systems. Consequently, there was no reference to—or particular interest in—history of science in this perspective.

If philosophy of science was comparatively well housed in philosophy, the same could not be said for history of science vis-à-vis history. History of science had virtually no purchase among historians. It had largely been pursued by scientists and by philosophers (e.g. Ernst Cassirer), not historians. Even Alexandre Koyré, the author of the *Études galiléennes* (Koyré 1939), which became the foundational work in the modern historiography of science, was professionally a philosopher. While not sundering ties to the historical profession, historians of science felt some necessity to establish a distinct professional and institutional identity. For example, the HPS Program at Princeton arose largely from Charles Gillispie's desire to provide advanced training to students with a real commitment to history of science, difficult to attract just through the history department at Princeton. On the other hand, Gillispie's initiative had little if anything to do with a felt need on his part to associate with philosophers of science. Nor, in Gillispie's account of the origins of this program, did the distinguished Vienna Circle philosopher of science, Carl G. Hempel, play any role in its formation (Gillispie 1999).[2]

Conversely, at Indiana University, the impetus from an HPS program did come from a philosopher of science, but a rather dissident one, N. R. Hanson. Although an American, Hanson came to Indiana after getting his D. Phil at Oxford and teaching philosophy of science at Cambridge, where he and the historian of science A. R. Hall constituted the teaching core of that HPS program in the early 1950s (Hall 1984). Unlike the dominant logical positivists, Hanson *was* concerned with origins and genesis of systems of scientific knowledge and, hence, with the history of science. But Hanson was unusual in this respect among philosophers of science (Hanson 1958).[3]

Yet, if only in hindsight, some general parallels in perspectives between historians and philosophers of science ca. 1960 can be perceived that might have made the union of them not unreasonable. One was an "intellectualist" approach (labeled "internalist" among historians of science). This was undoubtedly due to the impact of Koyré's studies of the Scientific Revolution on historians of science. Perhaps reflecting his philosophical training, Koyré viewed this watershed episode

[2]Gillispie's account is contained in a supplement of Isis with the title: "Catching up with the Vision: Essays on the Occasion of the 75th Anniversary of the Founding of the History of Science Society."

[3]Hanson brought A. R. Hall and his wife, M. B. Hall to Indiana in 1961. He and they left Indiana in 1963 but the program had been well launched and survived these losses.

in strongly intellectualist terms, delineating it as "intellectual mutation," in which even experimentation played little or no role, much less social and cultural contexts. This intellectualist approach was coupled with a strong guiding belief in scientific progress, perhaps expressed with the most sophistication in Charles Gillispie's *The Edge of Objectivity* (Gillispie 1960). Intellectualist and progressivist perspectives of science were certainly something historians and philosophers of science had in common and, in the case of the formation of the Indiana program, the intellectualist commonality, at least, was a positive factor.[4] Moreover, in this volume, Jan Golinski gives the intellectualist and progressivist perspectives contemporaneous political contexts in anti-Nazism and anti-Communism.

And then came Thomas Kuhn's *Structure of Scientific Revolutions* (Kuhn 1962). This work attracted the attention of philosophers of science in a way that no previous work in the history of science had been able to do. Part of Kuhn's attention from philosophers of science was due to the fact that contemporaries in philosophy had been developing somewhat similar approaches and perspectives to his own. Hanson was one; Stephen Toulmin was another.[5] But it was *Structure of Scientific Revolutions* that brought philosophers of science to consider seriously the history of science. There was now the thought in the work of philosophers such as Imre Lakatos—which one cannot find in the work either of the logical positivists or of their early philosophical critics—that historical studies could be used as a resource for honing philosophical analyses of scientific change.

In his final extended interview, Kuhn claimed that he thought *Structure* "when I got to it finally, as being a book for philosophers."[6] Although this retrospective assessment may have more to do with Kuhn's own later self-refashioning as a philosopher, certainly an important strand of Kuhn's perspective in *Structure* was the intellectualism of Koyré: Koyré's name was in fact the first to be mentioned in the book. The intellectualist strand in so ambitious a work as this no doubt recommended it to at least some philosophers of science.

And, yet, there were two other strands that came to undermine the commonality (however implicit) in perspective that had existed between historians and philosophers of science. The more important for this discussion was Kuhn's turn against the progressivist perspective.[7] In the *Introduction*, he strongly set forth his anti-progressivist perspective—perhap even more forcefully than he intended:

> Aristotelian dynamics, phlogistic chemistry, or caloric thermodynamics . . .were, as a whole, neither less scientific nor more the product of human idiosyncrasy than those current today.[8]

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[4] See Grau (1999, S318).

[5] See Toulmin (1961).

[6] Zammito (2004, 53). In the same interview, Kuhn made an even more astonishing repudiation of history of science: "philosophers and scientists are much closer to one another, because they all come in being concerned about what's right and wrong—not about what happened" (181).

[7] Kuhn's consideration social and professional contexts was the second important component of his historical view of scientific change. Under the influence of the hitherto neglected Polish scientist, Ludwig Fleck, Kuhn gave important—if historically very general—place to scientific *communities*.

[8] Kuhn (1996, 2).

As Kuhn made clear in the body of the text, scientific change had to be seen as consisting not in smooth advance, but rather in epochs of paradigm-guided "normal science" punctuated by episodes of revolutionary paradigm change. Theory and practice in a science in its post-revolutionary state were "incommensurable" with the pre-revolutionary aspects of the same science. Given this new account of science, it was impossible to delineate a steady, cumulative "advance" over time for a science that went through a revolution. At the end of *Structure*, Kuhn pushed the argument further, arguing that it was impossible to define scientific "progress" in terms of closer and closer approximation to natural "truth."

With the notable exception of Paul Feyerabend, philosophers who initially embraced Kuhn's view that the history of science could alter the philosophical image of science nonetheless rebelled against the anti-progressivist implications of *Structure*. Indeed, Kuhn himself subsequently equivocated over such implications.

In contrast, historians of science were taken by the anti-progressivist rhetoric of *Structure* even as they tended to ignore Kuhn's abstract schema of paradigms, normal science and revolutions in their research. Part of their reaction may have been influenced by the more general contemporaneous cultural turn from a positive to a critical valuation of science in the 1960s and 1970s. Part was also a symptom of the maturation of history of science as an historical discipline.[9] Historians of science had become more and more focused on the understanding passė scientific enterprises *for their own sake and in their own terms* with as little concern as possible with how they related—much less contributed—to contemporary scientific understanding. Much of this had little or nothing to do with the impact of Kuhn's *Structure*.[10] To employ a distinction used by a number of our authors, history of science became irrevocably *descriptive* (and historicist), whereas philosophy of science, even of those reacting favorably to Kuhn, remained *normative* (and progressivist).

Kuhn's anti-progressivist rhetoric had a more direct and overt influence on the work of a group of sociologists, mainly British, who in the early 1970s introduced a program they called "sociology of scientific knowledge" (SSK). SSK had eclectic roots—the philosophy of Wittgenstein and sociology, anthropology and perhaps Marxist historiography, among others. But it was the impact of Kuhn's *Structure* that proved critical. The Edinburgh formulation of SSK, termed the 'Strong Programme' by David Bloor and Barry Barnes, was grounded upon a number of methodological principles. Most fundamental was the prescription that science be studied like any other aspect of human culture, without regard to its truth value. Associated with this was a second prescription, the "symmetry postulate." Particularly important

[9]This, despite feelings of marginalization within the historical discipline. It should be pointed out that the historical discipline itself changed profoundly in the 1960s and 1970s and did so in directions that undercut its relationship to history of science and traditional intellectual history. The major changes were away from elitist and Eurocentric history.

[10]Koyré's approach had been very much in this vein and it was his careful attention to intellectual context in late sixteenth century thought to the work of Galileo and Descartes that made his work "foundational" for historians of science.

methodologically in the analysis of scientific controversy, this postulate prescribed that neither side of the controversy be assumed to have intrinsically superior scientific merit or epistemic privilege.

These prescriptions could be seen—and were seen by their formulators—as conclusions developed from Kuhn's perspective on scientific change. What was particularly influential in Kuhn was the view of science as involving the imprinting of "paradigms" on the student through scientific training. In SSK this view was transmuted into scientific "practices," carried on and passed on by scientific "subcultures," with no one having methodological or epistemic superiority. Moreover, the old talk of scientific "discovery" was to be replaced by talk of natural knowledge "construction." Historians of science increasingly saw their concerns as naturally linked to the project in SSK of offering normatively neutral contextual explanations of past and present scientific constructs.

If philosophers reacted negatively to Kuhn's anti-progressivist stance, they continued to be influenced by *Structure* in others ways. For example, since the 1970s there has been an increasing emphasis among philosophers of science on attention to the details of work in specific scientific disciplines. In this way, philosophers were taking seriously Kuhn's injunctions against focusing on abstract scientific frameworks that are disconnected from actual scientific practice. Moreover, there was a turn away from the positivistic procedure of using physics as a model for a unified account of science, and a concern to be sensitive to the fact that disciplines such as chemistry and biology, as well as the social sciences, proceed in ways that differ from physics.

One might think that these developments in philosophy of science would bring this field closer to a history of science that stresses the value of narratives concerning practices in particular sciences. However, there remain significant tensions between the disciplines, as indicated by the emphasis among proponents of the Strong Programme that their explanations in terms of social factors do not merely differ from but also are intended to replace the normatively-charged evaluations in philosophy of science. This sort of emphasis has prompted the suspicion among many philosophers of science that any history of science informed by the Strong Programme has little to contribute to their research.

Even so, there have been some signs that historians and philosophers of science have become dissatisfied with this mutual estrangement of the disciplines. There have been attempts to explore a rapprochement. For instance, a recent issue of *Isis* devoted a group of essays to this topic from prominent scholars.[11] Moreover, there has been a series of workshops—announced with the new acronym '&HPS'—intended to bring historians and philosophers of science together to discuss integrative strategies for studying science. These workshops are described as guided by the conviction that "good history and philosophy of science is not just history of science into which some philosophy of science may enter, or philosophy of science into which some history of science may enter," but rather "work that is both

[11] "Focus: Changing Directions in the History and Philosophy of Science," *Isis* 99 (2007): 88–134.