Scientific Philosophy as a Topic for History of Science

By Alan Richardson*

ABSTRACT

In lieu of a programmatic argument about the general relations of history of science and philosophy of science, this essay offers a particular topic in the history of philosophy of science that should be of interest to both historians and philosophers of science. It argues that questions typical of contemporary history of science could illuminate the recent history of philosophy of science and analytic philosophy. It also suggests that the history of scientific philosophy is a particularly fruitful arena for historians of science interested in issues of marginal science.

ROUGHLY A QUARTER CENTURY AGO, in a review of a volume of papers in history and philosophy of science, Ronald N. Giere asked whether the two disciplines, history of science and philosophy of science, had an intimate relationship or a marriage of convenience. Several subsequent commentators have evoked Giere’s curious metaphor. The metaphor—in addition to other peculiarities—depends upon a clear sense that the two fields are importantly distinct but in need of finding some mutually satisfactory relationship. Indeed, Giere began by sorting the essays in the volume under review into those that were a mixture of history and philosophy of science, those that were history of science

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alone, and those that were philosophy of science alone. Of course, there are other standard ways of making out the difference between philosophy of science and history of science. A favorite account is that philosophy of science is normative and history of science is descriptive.

I have no useful intervention to make into any such discussion. I suspect that there is nothing much useful to say about the relationship between history of science and philosophy of science. The normative/descriptive distinction is probably the most thoughtful attempt at a general characterization of differences between history and philosophy of science, but it has serious shortcomings. The normative/descriptive distinction is not a timeless distinction to be invoked unproblematically wherever we wish to demarcate history from philosophy of science. No simple Humean fact/value distinction can be read into the practice of many nineteenth-century historians or philosophers, for example. History as history (and not as a heap of facts) is for Hegelians, to further the example, a description of facts subject to epistemic norms; the norms of rationality inform the Hegelian historian about what her subject matter even is. Similarly, Machian historico-criticism does not respect the normative/descriptive distinction—without, for that reason, being uninteresting or unimportant as historical or philosophical method. Indeed, it would be an engaging and important topic in history of philosophy of science to see when, where, and why the normative/descriptive distinction achieved the status of the default way among philosophers to explain the difference between the philosophy of science and the history of science.

Rather than attempt a general account, then, I wish instead to suggest a specific topic of scholarly interest in which the tools and methods of history of science can with profit be used to aid the self-understanding of philosophy of science and in which philosophy of science can come to be of interest to historians of science. This topic is the scientific ambitions of logical empiricist (and other early twentieth-century) philosophy of science. I will suggest briefly, first, that only by asking questions characteristic of contemporary cultural history of science will philosophers of science (and analytic philosophers more generally) understand their own recent past and the contours of their current discipline, and, second, that understanding the scientific ambitions of logical empiricist philosophy of science can give historians a way of thinking about philosophy of science as something other than a dialectical opponent in the enterprise of understanding science.

There is a story about the philosophical ambitions of logical empiricism that has become a taken-for-granted fact in most science and technology studies disciplines, including philosophy of science, but that has recently come under considerable pressure owing to the work of scholars of logical empiricism. The story, most prominently offered by W. V. Quine in various critical contexts, goes something as follows: Logical empiricism in the first instance was a project whose philosophical ambitions were set for it by

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2 See, e.g., Nickles, “Philosophy of Science and History of Science.”

3 Lest it seem that the Hegelian example is out of place in a discussion of history of science, we ought not forget that generally Hegelian ideas of history have informed the practice of some influential historians of science (e.g., E. A. Burtt) as well as historically minded philosophers of science (from Ernst Cassirer and A. O. Lovejoy to Imre Lakatos and Gerd Buchdahl).
classical, foundationalist empiricism. In particular, the logical empiricists took the offerings of science to be insecure unless and until the philosophical project of showing how science relates to experience was discharged. This was the project that the logical empiricists took up; most notably, it was Rudolf Carnap’s project in his 1928 Der logische Aufbau der Welt. Where Carnap and the other logical empiricists differed from traditional empiricism was in their use of the tools of modern mathematical logic, not of associationist psychology, in giving the account of how the claims of science related to the offerings of sensation. The project was a foundationalist one—the point was to show how the claims of science did not go materially beyond the conceptual resources available in experience, or, to put the point in a more Quinean fashion, it was to show how the language of science could be translated into the language of sensation, using only the means offered by modern logic. Without bogging ourselves down in the details of Quine’s own more careful accounts, we can say that it has passed into lore that the logical empiricists thought there was something uncertain about the claims of science that needed to be shored up by a philosophical project that traced the conceptual resources and evidential basis of science to the offerings of sensation.4

There are many problems with this account of logical empiricism, most of which go well beyond the point I wish to make in this essay.5 The point I wish to make here is a simple one: This story of logical empiricism misplaces the project’s primary philosophical concern, which was not to shore up or improve science but to bring the rigor, clarity, and methods of science into philosophy itself. Far from looking to philosophy to rescue a science at risk, the project was to shore up a philosophy in crisis by using the resources on offer in the exact sciences. Logical empiricism was, in the first instance, a project meant to secure the scientific status of philosophy, to find a place for philosophy within a scientific culture.

I cannot argue sufficiently for this claim in the short compass of this essay, but let me make a preliminary case, making special reference to Carnap’s Aufbau. In both its motivational remarks and its actual procedures, Carnap’s book exhibits a concern with a metaphilosophical project, a project of bringing conceptual standards already extant in the sciences into philosophy. In the preface to the work, Carnap is at pains to stress that what is most new and most importantly on offer is a new way of doing philosophy:

The basic orientation and the line of thought of this book are not the property and achievement of the author alone but belong to a certain scientific atmosphere which is neither created nor maintained by any single individual. . . . The new type of philosophy has arisen in close contact with the work of the special sciences, especially mathematics and physics. Consequently they [i.e., the practitioners of the new scientific philosophy] have taken the strict and responsible orientation of the scientific investigator as their guide for philosophical work, while the attitude of the traditional philosopher is more like that of a poet.6


The reader steeped in cultural history of twentieth-century science will note that Carnap does not merely assert the scientific status (or ambition) of the new philosophy here. He also expresses and endorses a sensibility about what it is for a discipline to be scientific; the first sentence of the quoted passage gives voice to the Mertonian norms of universality (in the rejection of the individualism of the philosopher-poet and the adoption of a nonindividualist “scientific atmosphere”) and communism (in the endorsement of the results of science as common property).\(^7\)

Within the project of the book, Carnap constantly connects the goals of the epistemology that he proposes, as well as the means to those goals, to science itself. For example, the whole project of constructing the concepts of science is not endorsed because the constructions connect it to the certainties of sensation; Carnap does not discuss sensation using the language of certainty, and he ties the goals of construction to the scientific process of axiomatization. Indeed, Carnap’s account of sensation is informed by the science of sensation (Gestalt psychology), not the immediate offerings of sensation. Moreover, here and in the contemporaneous work *Scheinprobleme in der Philosophie* (1928), Carnap is at pains not merely to define the concepts of science but, most particularly, to give adequate definitions to the concepts of epistemology itself; after all, it is not in science but in philosophy where the pseudoproblems, wearisome controversies, and unending disputes that Carnap inveighs against lie. In the definitional process, we again look to science, and the use of logic is vouchsafed not by its peculiar philosophical certainty but by its being the framework within which any determinate claim in any subject matter must be given: “It will turn out that we can formulate the purpose of epistemological analysis without having to use these expressions of traditional philosophy [the ‘given,’ ‘reducible,’ ‘fundamental’]. We only have to go back to the concept of implication (as it is expressed in if-then-sentences). This is a fundamental concept of logic which cannot be criticized or even avoided by anyone: it is indispensable in any philosophy, nay, in any branch of science.”\(^8\)

I will not provide additional evidence that logical empiricism was self-consciously a project in making philosophy scientific or, to use the phrase in use by the actors, a project in scientific philosophy (*wissenschaftliche Philosophie*). Instead, I wish simply and briefly to note that concern with the scientific status of philosophy was rife in the German-speaking world from the mid-nineteenth century until the mid-twentieth century. It was a concern of Machian positivists, of scientific neo-Kantians, of the founders of phenomenology, and of others besides. Nor was it, of course, an exclusively German concern. Machian positivism followed on from Auguste Comte’s earlier French positivism, which also sought to find a place for a properly reconstructed philosophy within science. This concern was at issue for subsequent French positivists, neo-Kantians, conventionalists, and phenomenologists. Within Germany, the concern was given voice in a journal, founded in 1877, entitled *Vierteljahrsschrift für wissenschaftliche Philosophie* [Quarterly

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\(^7\) Merton first codified the scientific ethos in 1938. That essay, reprinted as “Science and the Social Order,” and others relevant to his sociology of science are reprinted in Robert K. Merton, *Sociology of Science* (Chicago: Univ. Chicago Press, 1973); “Science and the Social Order” is on pp. 254–266. My point is not to endorse Mertonian sociology of science but to indicate how well the exhortations in favor of a scientific philosophy made by the logical empiricists in the 1920s and 1930s exemplify Mertonian norms. I discuss this more fully in Alan Richardson, “Tolerance, Internationalism, and Scientific Community in Philosophy: Political Themes in Philosophy of Science, Past and Present,” in *Philosophy of Science and Politics*, ed. Michael Heidelberger and Friedrich Stadler (Vienna: Springer, 2003), pp. 65–89.

Journal for Scientific Philosophy] and originally edited by the positivist Richard Avenarius.9

It was a concern in other regions of the world also: it was certainly an issue that was frequently debated in the early meetings of the American Philosophical Association in the very beginning of the twentieth century.10 Some of the anxieties about the scientific status of philosophy that animated logical empiricists in the 1920s and 1930s could be seen, for example, in the 1910 position paper of the American “New Realists,” which begins:

Philosophy is famous for its disagreements, which have contributed not little towards bringing it into disrepute as being unscientific, subjective, or temperamental. These disagreements are due in part, no doubt, to the subject matter of philosophy, but chiefly to the lack of precision and uniformity in the use of words and to the lack of deliberate co-operation in research. . . . A conspicuous result of this lack of co-operation, common terminology, and a working agreement as to fundamental presuppositions is that genuine philosophical problems have been obscured, and real philosophical research has been seriously hindered.11

Any movement meant to secure the scientific status of philosophy ought to be of interest to historians of science, for whom the question of scientific status is importantly a topic; both they themselves (since they ultimately have to decide what their subject matter is) and, often, their historical actors must deal with this issue. Philosophy was far from unique in its practitioners’ arguments in the late nineteenth century about whether it was or how it might become scientific. Such debates were commonplace in fields such as psychology, sociology, geography, pedagogy, and many others. Indeed, both changes in the natural and life sciences and the increasing prestige of the sciences engendered disputes in the late nineteenth century about the significance of the scientific status of theories such as Darwin’s evolution by natural selection. Here, for scientists like T. H. Huxley and others, as for the positivists, the conception of science was in dynamic tension with what were understood to be superseded theological and philosophical concerns.

There are at least three ways in which the case of philosophy is especially interesting. First, in many of the nineteenth-century debates the idea of acquiring scientific status was opposed to remaining within or under the sway of philosophy—to become scientific (or positive) was to become, in some important sense, nonphilosophical. This obviously cannot be a successful strategy for scientific philosophy, which was meant to remain philosophy even as it became scientific. Thus, scientific philosophy was subject to certain discursive ambivalences and persuasive troubles that other fields could avoid. Second, as we have already seen in the remarks from the logical empiricists and New Realists given above, the informal suggestions offered about how to achieve scientific status often contained an implicit or explicit model of the properly constructed scientific or research

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11 Edwin B. Holt, Walter T. Marvin, W. P. Montague, Ralph Barton Perry, Walter B. Pitkin, and Edward Gleason Spaulding, “The Program and Platform of Six New Realists,” Journal of Philosophy, Psychology, and Scientific Methods, 1910, 7:393—401, on p. 393. It is of interest that this journal specified its area of special concern as “scientific philosophy”; see the unpaginated penultimate page of the Journal of Philosophy, Psychology, and Scientific Methods, 1904, 1—where, in text that is repeated at the end of every issue for several years, the journal says of itself that “there is no similar journal in the field of scientific philosophy.”
community. Indeed, very often the need for a research community was stressed against the background of an image of the philosopher as individual genius, creating from his own mind an entire system of the world; that is, scientific philosophy argued for a new vision of the philosopher, one modeled on the special scientist or coworker in a research endeavor. Third, the movement for scientific philosophy was highly relevant to the actual formation of the community of analytic philosophers that still dominates Anglo-American philosophy. In many ways the vision of the philosophical research community endorsed by the scientific philosophers has been achieved within the community of analytic philosophers. What is less clear is that establishing such a research community has solved the questions regarding the epistemic authority of philosophy that the scientific philosophers hoped it would. Arguably, however, many of the peculiarities of the discipline of academic analytic philosophy can be profitably considered from the standpoint of their only partially successful struggle for epistemic authority. The case of philosophy, moreover, mirrors the case of history and many other disciplines in that the philosophers, especially in the United States, engaged in these disputes within an effort to professionalize the discipline, taking the successes of the sciences as models.12

Clearly, there is not space enough here to discuss most of these topics at all or any of them adequately. Instead I will offer only an illustrative example. It exemplifies certain themes in contemporary history of science and might lead analytic philosophers (including philosophers of science) to raise more important questions than they ordinarily do regarding their own history and current philosophical projects. Some analytic philosophers have recently raised anew anxieties about the epistemic authority of philosophy. Interestingly, the framing of the question is usually different than it was for the scientific philosophers; it is a backward-looking concern, but not really a historical one. For example, Avrum Stroll recently wrote a monograph on the history of analytic philosophy in the twentieth century. At the end of the book, he reveals the sort of anxiety about analytic philosophy that motivated the work by asking these questions: “Has the past century been a period of substantive philosophical achievement? Have any of the persons discussed [in this history of analytic philosophy in the twentieth century] been of first-rate importance? A number of contemporary authors have answered each of these questions with a resounding no.” In further specifying the question about the first-rate importance of the individual analytic philosophers, Stroll writes this: “How shall we assess the chances of ‘immortality’ for the analytic philosophers we have dealt with in this study—Frege, Russell, Moore, Carnap, Wittgenstein, Ryle, Austin, and Quine? Will any of them turn out to be the Descartes or the Kant of the twenty-first or twenty-second century? My own guess is that only Wittgenstein is a plausible candidate for that status.” Stroll’s reasons for this assessment of Wittgenstein and the others are not important here; here I wish only to query the sensibility about philosophy that leads naturally from a question about the intellectual value of analytic philosophy to a question about the greatness of individual analytic philosophers. What is interesting is that all of Stroll’s potential candidates for greatness in analytic philosophy, with the possible exception of his favored candidate, Wittgenstein, would think that the question about immortality was a bad question, since they were self-consciously distanciing themselves from a view in which philosophical achievement was due to and dependent on individual greatness. Indeed,

12 See the literature cited in note 11, above.
Stroll has missed the absolutely central sensibility of early analytic philosophy that part of its newness and its promise was exactly that its contributors would not cover “all or most” of the main fields of philosophy in an expression of individual greatness but would seek to be collaborators in a piecemeal, technical solution to problems.

This is certainly true of logical empiricist scientific philosophers. Here, for example, is Hans Reichenbach in an essay written in 1929 on the then still curious notion of “philosophical research”: “That it is possible today to speak of philosophical research can itself be regarded as a characteristic of modern philosophy. For the very word ‘research’ designates a method. In admitting that it engages in research, philosophy classifies itself, from the methodological standpoint, among the special sciences, which progress step by step by solving particular problems.” The relation of this to “great philosophers” is spelled out when Reichenbach continues, a bit later: “The exaggerated stature attributed to the great men of the past could not but cripple initiative, could not but turn the attention of present thinkers from present possibilities and produce that unproductive epigonism the sight of which must repel anyone who works in the stimulating atmosphere of the sciences and who has even once looked at contemporary philosophy from this vantage point.” The repellent nature of traditional philosophy to those who have imbibed the scientific spirit is also expressed by Carnap, again from the introduction to his 1928 Der logische Aufbau der Welt:

In philosophy we witness the spectacle (which must be depressing to a person of scientific orientation) that one after another and side by side a multiplicity of incompatible philosophical systems is erected. If we allot to the individual in philosophical work as in the special sciences only a partial task, then we can look with more confidence into the future. Each collaborator contributes only what he can endorse and justify before the whole body of his co-workers.14

Now Stroll is not quite unaware that this sort of specialization, although a mark against greatness, is a feature of the development of analytic philosophy. He approvingly cites this (seemingly rather wistful) remark by Nicholas Rescher: “For better or for worse, in the late twentieth-century we have entered a new philosophical era where what counts is not just a dominant elite but a vast host of lesser mortals.” Stroll’s remark on this is limited to one terse comment: “The reasons for this ‘democratization’ of philosophy remain obscure. But that such a phenomenon has taken place is evident.”15

Historians of science might find Stroll’s inability to muster more curiosity on this point puzzling. On the penultimate page of his book, Stroll discovers a genuine social process associated with the rise of the research sensibilities embedded within analytic philosophy in the twentieth century—and he has nothing to say about it. Yet the process is indicative not just of professionalization and not just of the expansion of higher education, but also of a self-conscious effort by the scientific philosophers to promote a vision of the division of labor in philosophy that was modeled on the research organization of the special sciences as they understood it. Indeed, this vision of the proper workings of the research

culture of philosophy was promoted in an effort to overcome the destructive influence of philosophical concern with greatness.

History of science as currently practiced would invite curiosity about several topics following on from these considerations. Most generally, the development of a scientific philosophy at this time should itself be a central concern for historians of science. Contemporaneous efforts in the social sciences and in fields such as art and literature have been extensively studied by historians, the philosophical case surprisingly less so. More specific topics also invite curiosity. For example, how and to what extent is the current division of research labor in philosophy due to the collaborative and piecemeal scientific ethos in philosophy that the scientific philosophers promoted? What do we learn about the sociology of scientific knowledge by attending to the process of instituting the division of labor in analytic philosophy—a sort of philosophy that, despite having adopted such a research ethos, still notably lacks the ability persuasively to claim the sorts of results that science can claim? These are issues in the consideration of scientific and analytic philosophy as marginal science. Analytic philosophy is, indeed, much more interesting than, say, astrology or phrenology as an example of marginal science, since it has been more successful in adopting certain aspects of the scientific research organization and ethos; it retains a place within the academic world and, thus, a degree of epistemic authority; and the figure of the philosopher still has a certain cultural resonance and significance (for example, occasions for eloquence and edification, such as graduation speeches, often bristle with philosophers, but rarely or never with astrologers and phrenologists, historical or contemporary).

The specialization of philosophical research and its effects in delimiting the proper topics and methods of philosophy has not escaped the attention of a few scholars interested in the social history of knowledge. One scholar who is both a philosopher and a sociologist of knowledge, Martin Kusch, has written extensively and persuasively on role hybridization and role differentiation in philosophy in the late nineteenth and early twentieth centuries. Similarly, the historian of science Peter Galison has provided some of the key essays in the cultural history of logical empiricism, while the intellectual historian Bruce Kuklick has written at length about the professionalization of American philosophy in the same period. It was briefly discussed along Kuklick’s lines by the leading American social historian of medicine, Charles Rosenberg, in an essay first published in 1979. There Rosenberg remarks with some sharpness that moral philosophy was still central to the learned man’s intellectual world in 1865; by the 1920s academic philosophy had turned away from consideration of those eternal problems that philosophy had always addressed. The increasingly internal orientation of philosophers has made the products of their scholarship well-nigh irrelevant to the society that supports the discipline’s linguistic, logical, or mathematical investigations. In some ways, the differentiation of the moral philosopher’s role into a half dozen specialized descendants—among them philosopher, economist, psychologist, political scientist, sociologist—provides an exemplary paradigm for all those shifts that marked the changing context of knowledge in the half century between the Civil War and World War One.16

Rosenberg is, clearly, expressing an opinion as well as describing a process. Is it the case, as he here suggests, that the topics and methods analytic philosophy has adopted in order to organize itself on the model of a science have led it to be irrelevant to the society that supports its research? There is clearly something more than a hint in Rosenberg’s comment that analytic philosophers have ceded authority on everything worth talking about in pursuit of special topics and methods that they can unproblematically call their own. That is, obviously, contentious, but it is an issue that makes the history of analytic philosophy actually interesting and important. Moreover, no history of analytic philosophy written as an exposition of the doctrines of ten or twelve philosophers who somehow dropped from the sky and started philosophizing will ever begin to illuminate such questions. Only serious scholarly attention to the historical development of the field can get us beyond a history of the doctrines of great men that seems to animate philosophers and a few suggestive comments by historians.

I hope that I have made a preliminary case that history of science and philosophy of science can interact on the level of something more than unhappy lovers, disgruntled spouses, or intellectual rivals. Certain topics and methods of history of science, when applied to issues raised by scientific philosophy, can help philosophers of science sort out their relationships to logical empiricism more adequately than they have done so far. Most philosophers of science engage with logical empiricism only insofar as they are concerned to claim that they have gone beyond it. No sustained effort has been made to produce an empirically, conceptually, and explanatorily adequate understanding of the significance of logical empiricism as a project in twentieth-century philosophy. Yet the question of what contemporary philosophy of science owes to logical empiricism and how it has advanced beyond it can be adequately answered only with such a history. Only through such a history can philosophers fully understand both their sense of what is philosophical in their own projects and how they ought to engage in philosophical inquiry. Similarly, rather than viewing philosophy of science mainly as a rival perspective on science, historians of science can see at least some aspects of the recent history of philosophy of science as a fruitful topic of research. A case could easily be made for a similar philosophical engagement with recent history and sociology of science. If we adopt such critical perspectives toward understanding our own and one another’s intellectual projects, we might work better, both individually and collaboratively, without the disciplines worrying about whether they are making a marriage commitment. Such collaboration would be a worthy twenty-first-century legacy for scientific philosophy.


17 Of course, such work has been on offer from scholars such as Michael Friedman, Peter Galison, David Hollinger, Don Howard, and Thomas Uebel for some time. It has begun to gain a wider foothold among younger historians such as Francesca Bordogna, Andrew Jewett, and Joel Isaac, as well as within a much broader philosophical community, largely through the efforts of HOPOS.