Chapter 5
History and Philosophy of Science: Thirty-Five Years Later

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5.1 Introduction

I must begin by thanking the editors of this volume, Tad Schmaltz and Sy Mauskopf, for inviting me to reflect on my views of the changing relationship between the history of science and the philosophy of science since publication of my review article, "History and Philosophy of Science: Intimate Relationship or Marriage of Convenience" (Giere 1973).[1] Of course, not only has the relationship between these two fields changed; my views of the relationship have also changed.

5.2 The Problem of Normativity

I was hired ABD in 1966 in the Department of History and Philosophy of Science at Indiana University. Founded in 1960, it was the first department of its kind in the United States. So, from the very beginning of my career, I was daily confronted with the fact that I was not in a philosophy department but an HPS department. I could not avoid reflecting on relationships between the two disciplines. By the time I arrived, the founder of the department, and a staunch advocate of a close relationship between the two disciplines, Russ Hanson, had departed for Yale. There was no such advocate left among the remaining faculty members. The "and" in HPS was just a conjunction. This separation was physically marked by the fact that all the historians' offices were on one side of the hall and the philosophers' offices on the other. The only attempt at combining the two disciplines was in the curriculum. All first year graduate students were required to take a two-semester survey in both

[1] As a sidelight, I should mention that my choice of the marriage metaphor might have been influenced by the fact that I was at that time in the midst of a divorce.

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the history of science and the philosophy of science.[2] But there was no connection between the two surveys. The history survey was a chronological presentation of the whole history of science from the fourth century BCE to the end of the nineteenth century CE. In these early years, the philosophy of science survey consisted of one semester on scientific explanation and one semester on confirmation. All students ended up as either historians of science or philosophers of science, but all ended up knowing more about the other discipline than the faculty.

It was in this context that I approached a review of a volume of *Minnesota Studies in the Philosophy of Science* entitled: *Historical and Philosophical Perspectives of Science* (Stuewer 1970).[3] The volume had papers by both historians and philosophers of science, but no one seriously faced what seemed to me the crucial issue. How can a normative philosophy of science be reconciled with a descriptive history of science? If, as Kuhn had suggested, history of science can serve as evidence for philosophical claims about science, we have a circle. Before one can use the history of science as evidence one needs a philosophical account of what constitutes evidence. So the epistemology of science must precede any use of the history of science as evidence for philosophical claims.[4] And this problem follows after the obvious prior question of how factual claims could ever be evidence for normative claims. I returned to my philosophical work on the foundations of probability and statistics.

5.3 Naturalism

It was a decade before my views on this subject underwent any radical revision. Among the influences on my thinking in the early to mid 1980s were interactions with the social psychologist and philosopher, Don Campbell, and encounters with the then new sociology of science as well as the emerging cognitive sciences. In any case, the result was that I came to the conclusion that the philosophy of science

[2]HPS at Indiana is solely a graduate department. Although some undergraduate courses are taught, there has never been an undergraduate major. That was part of the agreement with the Philosophy Department when HPS was founded.

[3]This review was personally commissioned by the then editor of BJPS, Imre Lakatos, over drinks in London. He had some very definite instructions on what the review should say. I don't remember what they were, but, in any case, I did not follow them.

[4]It is worth recalling that Hanson's view of HPS prominently featured the idea that philosophers should help historians assess the validity of arguments offered by historical figures for various hypotheses, given the evidence available at the time (Hanson 1962). Standards of validity, however, are arrived at independently of any historical facts. While acknowledging that judgments of the weight of evidence are not necessarily deductive, he did not much concerned himself with debates over the best inductive methods then going on among such major figures as Carnap, Reichenbach, and Popper, who (initially) advocated a pure deductivism. I owe these notes on Hanson's views to Matthew Lund (2010).

should be transformed into something like the theory of science. That is, philosophers should be in the business of constructing a theoretical account of how science works.[5] Philosophical claims about science would then have the status of empirical theories. In short, the philosophy of science should be naturalized.[6] This means, among other things, giving up pretensions to finding autonomous standards for the practice of science.

But what about the problem of circularity that had so bothered me earlier? That charge assumes that there is an a priori way of determining what a good scientific argument should be. If one gives up that assumption, then one can investigate the reliability of various methods for judging scientific claims using other methods which, in turn, depend on some prior empirical claims. To take an especially clear and simple example, we can explain theoretically why, when testing the effectiveness of some treatment, randomized designs are better than prospective designs. The latter require that the experimental and control groups be matched for a set of known variables. A randomized trial in effect controls, probabilistically, for all variables, known and unknown. It presumes, however, that one already has very good evidence that the method for randomizing is sound.

Here one is more or less forced to embrace aspects of Pragmatism, particularly the idea that there is no foundational method. Rather, inquiry always beings with the beliefs one has. In that context, anything can be questioned, but not everything at once. And there has to be a reason to question claims hitherto accepted, such as conflict with new data. Science is to be valued not only for the theories it has given us, but also for the methods it uses to modify old theories and establish new ones.

It is also possible to recover a form of normativity. Our theories of how science works are normative in the same way other scientific theories are normative, namely in a conditional rather than categorical form. If one wants to get a rocket to the moon, then one should rely on classical mechanics. We know it works well for that purpose.

It must be stressed that a naturalized philosophy of science is not descriptive in a simple-minded way, just describing the gross behavior of scientists in everyday categories. Rather, it is theoretical in the way most sciences are theoretical. It seeks to uncover underlying processes in the practice of science. Thus, for example, during the last several decades, many philosophers of science have investigated the

[5]This applies only to what is called the "general" philosophy of science. The philosophy of the special sciences should be treated separately. This work is sometimes done within an autonomous philosophical framework, such as logic. It is also sometimes done in the scientists' own frameworks, in which case it is automatically naturalized.

[6]I announced this program in my 1985 paper, "Philosophy of Science Naturalized" (Giere 1985). The title was obviously modeled on Quine's "Epistemology Naturalized" (1969), although I was little influenced by Quine. In fact, I came up with the title first and then hurried to write a paper to go with it since others were sure to come up with the same idea. Indeed, shortly before the paper came out, Don Campbell telephoned me to suggest we write a paper together with just that title. I sheepishly informed him that my paper with that title was forthcoming.

nature and role of models in science. The concept of a model provides a theoretically richer way of understanding scientific practice than just blanket notions of "theory" and "observation." Most recently, various notions of modeling have proven fruitful for understanding the growing role of computer simulations in science (Winsberg 2010).

Finally, of course, a naturalized philosophy of science is fully compatible with the history of science which, by its nature, is a naturalistic study of past science and scientists. Nevertheless, in spite of a historical turn among some philosophers of science, few, if any, thought that the philosophy of science could be wholly assimilated into the history of science.

5.4 Cognitive Science and the Sociology of Science

If one is going to naturalize the philosophy of science, to what is it to be naturalized? At the time there were two obvious candidates: the cognitive sciences and the sociology of science. I leaned toward the cognitive sciences, and the subtitle of *Explaining Science* (Giere 1988) was "A Cognitive Approach". But I never regarded these options as exclusive. On the contrary, I assumed that a comprehensive theory of science would have to include elements of both. Much of the book was organized around the two categories of representation and judgment, both major topics in the cognitive sciences. On the representation side, I emphasized the use of models. On the judgment side, I urged understanding scientific inference not as a kind of logic but as decision making, with individuals and groups of scientists deciding which hypotheses provisionally to accept or reject.

My reason for emphasizing the cognitive over the social was the latter's rejection of any form of scientific realism. Realistic talk among scientists was not to be taken at all literally. And there are no standards for scientific reasoning. In rejecting these views I was in agreement with most philosophers of science. But I was also more sympathetic to the new sociology of science than most philosophers because it emphasized the role of scientists, that is, agents, in the production of scientific knowledge. I had already concluded that a naturalized philosophy of science, like most history of science, should focus on the activities of scientists (Giere 1989). I thus explicitly rejected he positivist attempt to objectify scientific representation in terms of semantic relations between symbols and the world, and scientific judgment in terms of logical relations among statements.

Meanwhile, historians of science were busy assimilating the new sociology of science. They were prepared for such assimilation since the field of history as a whole had for some time been moving in the direction of social history. At that time, this was thus a much more natural match for historians of science than the philosophy of science. The change in the history of science was quite rapid. By the end of the century, a stalwart supporter of history of science as intellectual history sadly admitted to me that "they have won," the "they" being either social historians of science or sociologists of science.

5.5 History of Science, Philosophy of Science, and Sociology of Science

In many ways, Science and Technology Studies (STS) is the natural successor to History and Philosophy of Science (HPS). Its major components are the History of Science and Technology, the Philosophy of Science and Technology and the Sociology (also Anthropology) of Science and Technology. Both the History of Technology and the Sociology of Technology are well developed; the Philosophy of Technology less so. The explicit inclusion of technology is significant because the connections between technology and the rest of society are much tighter than those between science and the rest of society. From the standpoint of the culture at large, it is probably more important that we understand the workings of technology than the workings of science. Nevertheless, I will focus on science, mainly because I have had little engagement with studies of technology.

In many ways, my current views of the relationship between the history of science and the philosophy of science are similar to what they were at the beginning. In particular, I still think these are different disciplines with different goals and methods. The goal of a naturalized philosophy of science is to construct a theory of how science works. It need not assume, however, that science has worked the same way throughout its history. But, as a matter of fact, the philosophy of science is quite present oriented. Historians of science seek to tell us something about how particular historical episodes large and small, unfolded. They rarely try to construct general theories of science, even ones focused on a particular historical period. The different methods of naturalized philosophy of science and history of science are suited to their respective goals.

On the other hand, and already noted, relations between the history of science and the sociology of science are much closer. It may be difficult to tell the difference between a work in the social history of science and one in the historical sociology of science. But some sociologists of science also construct theories of science, sometimes ones intended to be all inclusive. The many works of Bruno Latour provide a prominent example. And the sociologist of science, Andy Pickering (1995), has tried to subsume all of STS under the umbrella of cultural studies. I have always viewed sociological theories of science as complementary to a generally naturalized philosophy of science, and the cognitive study of science in particular.[7]

The main difference in the relationship between the history of science and the philosophy of science since I began is that, whereas there used to be almost no interaction between the two fields, there is now considerable interaction. No longer do philosophers of science write about black ravens and the shadows of flagpoles. Their examples are taken from real science, both contemporary and historical. Sometimes philosophers of science do their own history of science.

[7] The foremost advocate of the cognitive study of science (Nersessian 2005), has recently attempted to cross the divide between cognitive and social studies of science, coming from the cognitive side.

For a naturalized philosophy of science, Kuhn's suggestion that the history of science can provided evidence useful to the philosophy of science can be realized. But the evidential relationship here is not especially crisp. There are no crucial experiments to be had. It is more a matter of developing a general interpretive framework to understand scientific activities, past or present. Not all interpretations are equally good, of course, but the criteria for judging differing frameworks are multiple.

Unfortunately, the interaction tends not to be symmetrical. Many philosophers would like to think that the concepts they are developing can be useful to historians in their approach to understanding historical episodes in science. But the presumption that what philosophers say is irrelevant to the work of historians of science seems still strong. And, indeed, a few decades ago even historically minded philosophers of science were busy constructing theories of "methodology" with the aim of showing that the development of science is "rational" or "progressive."[8] Historians were wise to reject these ideas.

My hope for the future is that, as the philosophy of science becomes more thoroughly naturalized, this side of the relationship between history of science and philosophy of science will improve. Again, the distinctions between theories and models, and the general importance of models in science, would seem to provide good candidates for deployment by historians of science. Whether they will take up such ideas remains to be seen.

For me, the big question remains: How are we humans capable of knowing such things as that we ourselves are the product of millions of years of organic evolution and billions of years of the physical evolution of the universe? That, of course, is a philosophical question, but it requires a scientific answer, to which all of us, historians, philosophers and sociologists of science, can contribute.

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[8] I am referring, of course, to works such as those of Lakatos (1970) and Laudan (1977), which were responses to the perceived "irrationality" in Kuhn's (1962) picture of science.

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