Is the History of Science Relevant to the Philosophy of Science?[1]

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Science is the subject matter of the philosophy of science. Like evolution, science refers to both a process and a product. It is both an activity of production and the set of products resulting from this activity. Philosophers, though, have mainly focused on scientific results, i.e. the products of science, whether current or past. The question I want to raise here is: should philosophers also be concerned with the process of science?

The most general answer to this question is that it depends on the specific tasks being pursued. Our discipline does not have a fixed set of tasks. For example, logical positivists aimed to discern the structure and logical form of scientific theories. For this purpose, the history of science was of interest only as a repository of cases. Since the products of science are in constant flux, logical positivists have often been subjected to the criticism that by looking at so-called text-book versions of scientific theories, they provided a distorted view of the sciences. In my opinion, this criticism is unjustified. We can appropriately question the logical positivists' narrow conception of the philosophy of science, and even their narrow conception of science. But it is unfair to criticize their lack of historical perspective because the problems in which they were interested did not require study of the temporal dimension of science. But, as a matter of fact, the goals of our discipline have broadened since the times of logical positivism. Furthermore, the abandonment of their research program was a result, I would argue, not only of the shortcomings in their ideas, but of the increasing awareness that their conception of the tasks of the philosophy of science was too restrictive.

In the past few decades, historical scholarship has produced a radical transformation in our image of science, and this, in turn, has led us to rethink the tasks that the philosophy of science should pursue. In large part insights into the history of science have led philosophers of science to new problems and tasks, including such prominent ones as: the Duhem-Quine thesis of adjusting theories by introducing auxiliary hypotheses, incommensurability, the pervasive referential instability in the history of science, the non-cumulative character of science and other problems associated with scientific revolutions, etc. All of these issues become visible only through an analysis of the development of science. These problems originated - and they could only have done so - from the study of the process of science, not from the study of any of its products. Thus, the history of science had much to say to the philosophy of science: it

PSA 1992, Volume 2, pp. 490-496

called attention to a whole realm of problems "invisible" in any study of idealized versions of scientific theories.

If history has provided many of the most important problems in the field, it would seem natural, then, that we should turn to it for the solutions. Contemporary philosophers of science study scientific methodology, the aims of science, the ontological status of the hypothetical entities postulated in scientific theories, the conceptual, social, and political implications of scientific discoveries, the plausible reconstruction of science as a rational pursuit, and, in general, the nature of the scientific enterprise. It seems unlikely that any of these issues could be clarified - let alone "solved"- without a knowledge of the process of science. For example, philosophical interpretations of scientific methodology depend on the analysis of the development of methods and their justification in different sciences and different periods. Furthermore, philosophical positions that arise from a particular reading of the history of science (like relativism, conventionalism, realism, etc.) should be based on a careful examination of the evolution of the sciences, because only a historical analysis can make visible the existence of patterns—if any— in the way that we have approached, conceptualized, and manipulated the world. To understand *scientific practice*, then, we need to look at the *practice of science*.

In the recent past, philosophers have become aware of the need to improve their knowledge of the historical development of the sciences. As a consequence, we have witnessed an increase in the use of historical examples in the philosophy of science. Much good work has been done to integrate the philosophy of science with the history of the sciences. However, I would like to point out two problematic areas in the contemporary philosophy of science that, in my opinion, require further consideration if we are to make more fruitful use of the history of science: one, the use of historical case-studies as evidence to support one's philosophical conclusions; two, the need to articulate how we should use the history of science to address our philosophical concerns.

1. Limitations of the "Historical Case-Studies Approach"

For reasons of space I can only briefly point out some of the problems with the "historical case-studies approach" in the philosophy of science. Often the episodes studied are used only for their heuristic value; then, they are helpful to illustrate or even articulate one's position. To use historical cases in this way has a very limited value, but it is not illegitimate. In other cases, however, the episodes analyzed are used as demonstrations of one's ideas or evidence for one's position. But for some scholars, attributing an evidential role to history raises problems of circularity: if we have to adopt a specific position to interpret historical data, how can we use these same data to support our philosophical position? I do not believe, however, that this problem is fatal. For we have realized that there are no "unvarnished facts" in any field of the natural and social sciences, but no one has ever argued that scientific claims should not be based on facts. Furthermore, the risk of circularity is a problem for all fields that include a historical component, e.g. historical sociology and evolutionary biology. The solution to the possible dangers is not a retreat from them, thus renouncing the use of what is, arguably, the most important evidence we could find for our positions. Rather, the solution is to accept the challenge to articulate when and how we can use specific information from the history of science. Thus, although we must be careful not to commit the philosopher's greatest sin of circularity, this should not deter us from using just about the only evidence available for our positions, namely, historical evidence.

As I see it, the problem does not lie in using "historical evidence" per se, rather it lies in using "historical cases" as historical evidence. We should ask, first, what is the evidential support conferred by a historical "case"? Second, what is the historical value of a

"historical" case? Nickles (1986) has called attention to the first problem by arguing that among the vagaries of the history of science one can find evidence to support almost any conceptual point. For this reason, in my opinion, the evidential role that can be attributed to an isolated episode from the history of science is usually very low. Compare it to studying biological evolution. What allows us to have a theory of evolution is the study of the patterns, not the study of any isolated episode, in the history of life. I am not saying that we cannot do philosophy of science unless we focus on patterns in the evolution of the sciences, since there may not be any. I am only saying that we should be aware of the limited evidential value of analyses of isolated episodes in the sciences.

The limitations of the evidential role of case studies also raise methodological problems. For example, if we can make claims only about specific episodes, it seems difficult, if not impossible, to reach closure or consensus about more global issues in philosophy. By focusing on case studies we also run the risk of selecting only those episodes which support our views. Some philosophers focus on scientific change, other scholars on scientific controversies, others still on particular methodological issues. They often select specific episodes to support generalized views about science. Some of the *philosophical* interpretations that certain sociologists of science have defended by analyzing a few cases of scientific controversies are clear examples of this *methodologically* suspect strategy.

Let's look now at the second question: what is "historical" in an approach which focuses only on isolated episodes in science? For such an approach, history is valuable because it allows us to analyze cases from the past, not because the diachronical dimension of science matters for our understanding of it. Yet, at the heart of any developmental process, including science, is its temporal dimension. The problem with this approach, then, is that it does not take history seriously. We should not eliminate this from our analysis precisely because the temporal dimension of science first made visible many problems that we are dealing with today in the philosophy of science, central among them being the very historicity of knowledge. However, by using the history of science only as a repository of cases we are not dealing with the history of science, but only with past episodes in science. Thus, we are not learning from history; at most, we are learning from the past.

I would like to suggest that a better way to understand the process of science might be to develop a Systematics of science. In biology, Systematics identifies species and organizes them into higher taxonomic categories. For our purposes, what is interesting in Systematics is the analysis of the genealogical relationship between species. Analogously, if we want to understand scientific methodology, for example, we should establish a systematics of the methods used in different sciences to deal with different problems. A Systematics of science should focus mainly on the genealogical relationships between different belief systems, methodologies, and evaluative strategies. We could eventually map the patterns - if there are any to be found - in the ways that the different sciences have evolved.

Nevertheless, like a biological system, science can be studied both diachronically (historically) and synchronically (statically). Thus, we should recognize as legitimate the study of particular versions of theories. One studies a version by taking a snapshot of the theory at a given moment in time in order to analyze its structural components, similarly to how one would study the molecular biology of a living system. Another legitimate static approach is to elaborate models of theories. As idealized representations, models should not be judged by their capacity to represent accurately the complexity of science, but by their fruitfulness and by their capacity to illuminate specific issues in the philosophy of science. Models need to conform to existent data, and the

data in a sense validate their accuracy. Models, however, should be judged mainly by their capacity to bring out features of science relevant to our understanding of it.

In conclusion, historical studies are necessary in our philosophical pursuits, although other types of inquiries are also important. Science is a historical enterprise and theories are historical entities which change through time, but some tasks in the philosophy of science do not require a knowledge of history. Temporal and static studies can complement one another. The challenge is to find ways to integrate them fruitfully.

2. Articulating the Role of History in the Philosophy of Science: The Realism-Antirealism Debate

This section focuses on the need to reflect upon the legitimate roles that history should have in elaborating and justifying our philosophical positions. To illustrate this, I want to consider the debate about realism and antirealism in the philosophy of science. This is a notoriously complicated issue, and it would be impossible to do justice to it in the limited space available. I am using this controversy, however, only as an example of an area where we need to think about the relationship between history and philosophy. I will therefore make no attempt to offer a solution to this controversy. My aim here is more modest. I want to show that one of the problems hindering the progress of the discussion is our failure to articulate the role that specific evidence from the history of science should play in it. That the history of science is essential to this debate is not at all surprising. After all, the overthrow of long-standing research programs in the course of science has made evident the fallibility of knowledge.

In my opinion, many disagreements between realists and antirealists stem from the lack of clarity about the evidential role of historical information. We have conducted numerous studies to find out whether Galileo, Newton, Einstein, Morgan and other great scientists were realists or antirealists. However, it is not clear what weight the discovery that, for example, Einstein was indeed a realist or that Morgan believed in the existence of genes should have in the general discussion about realism. What is the relevance of what particular scientists think? What is the relevance of the fate of major research programs? How should we weigh the information extracted from the different sciences? Is the fate of a research program in microbiology as important as the fate of Newtonian mechanics? Is the stability of the Mendelian paradigm support for a realist position and the instability of psychological theories evidence for antirealism? How do we assess how much continuity and discontinuity there is in science? Should we simply count how many research programs persisted through time and how many were overthrown by an incommensurable paradigm? Should we make a list of all the great scientists and find out who were realists and who were antirealists? We need to address these questions to make progress in the discussion.

Of course, we have to be clear about what a theory of realism is supposed to do before deciding what historical information can count as evidence. One way to start might be to delineate the different levels of the discussion and to assess the import of the history of science at each one of them. Bas v. Fraassen (personal communication) suggests that we should distinguish between two points of choice. The first, which he calls *alpha*, is the point of *theory choice*. The issue here is whether one should accept a theory merely as empirically adequate or believe that it is true. The second point of choice, which he calls *beta*, involves the philosopher who tries to understand *what science is*. Here, according to v. Fraassen, the philosopher asks whether a scientist as a scientist must choose between believing a theory or only accepting it.

We must figure out whether the history of science has anything to say at these two points of choice. Let's start with *alpha*. The question here would be: is the history of a theory relevant to our assessment of it? In my opinion, this is precisely the question that divides many realists and antirealists. For example, I think that the differences between v. Fraassen and E. McMullin boil down to their different answers to this question. McMullin argues that features like resilience, fertility, etc. are important in assessing whether a theory deserves our trust and whether we should believe in the hypothetical entities it introduces. These features can be appraised only when we look at the development of a given theory over time. Thus, the past record of the theory is important in deciding the support that the theory deserves. v. Fraassen, however, thinks that we need only to take a snapshot of the theory and see whether it makes claims about observable or unobservable entities. In the latter case, we are not rationally compelled to believe in the existence of those entities. However, I think that v. Fraassen would agree that features like fertility, resilience, etc. are important to decide whether we should accept a theory (although they would not provide reasons to believe in it). Therefore, the past record of a theory is important for both realists and antirealists, and this could be an area of common interest in the history of science.

Now consider point of choice *beta*. The question here is: what is science? Is the choice between accepting a theory as empirically adequate or believing in it as true required in science as such? I want to distinguish two different approaches here. In the first, which I will call the *naturalized approach*, realism and antirealism are theses about what scientists aim at, believe, and actually do. To decide between realism and antirealism, we have to study how particular scientists regard their theories, whether they believe in the existence of the hypothetical entities introduced by them, which criteria they use to develop and justify their hypotheses, etc. For this approach, realism and antirealism are empirical hypotheses which make claims about the actual development of science and the beliefs and actions of its practitioners. Consequently, historical studies can and should have an evidential role. In fact, the history of science would refute or corroborate realism. Once we had the data points, the choice should be fairly straight forward.

I will call another approach to answering "What is science?" *hermeneutic*. I call it so because it involves a certain degree of interpretation. I believe that the philosophy of science has the right to interpret its object of study (science) and not only to describe it. I think (contrary to Fine 1986) that interpreting what science is does not amount to looking for the essence of science. From the moment that we accept that science is a historical enterprise, we recognize its contingent character. Nevertheless, we can certainly interpret what science has actually been and happens to be.

In the hermeneutic approach, realism and antirealism are stands that we take towards science as it actually has been and currently is. The stand taken depends on an interpretation of science as a cognitive enterprise. So, what is the relevance, if any, of the history of science here? To take a stand towards something, one certainly needs to know how it works. Thus, one's stance toward science should be *informed by* the development of research programs and the actual beliefs of scientists. But it does not necessarily have to *conform to* what the practitioners of science do and believe. A philosophical stand can neither be reduced to a description of what scientists do nor be bound by what they believe they are doing. However, it is important to analyze the past record of science to take an epistemological stand *qua* philosophers. Therefore although in this approach realism and antirealism are not empirical theses, but philosophical stands, they are based upon facts, namely, the facts of history.

Thus, hermeneutic realism and antirealism are not *apriori* but *aposteriori* positions taken only after an analysis of how science has actually developed. This is as it should be given the contingent character of any historical enterprise. Had science developed in a different way, the correct stand might be a different one. How much of this world happens to be accessible to creatures like us will depend on particular and contingent features of the world, and we will only find out through the analysis of the particular methods that science has employed and the particular results it has obtained. Furthermore, stands, as v. Fraassen has argued, can be vindicated. It is precisely history that does or does not vindicate our stands.

3. Concluding Remarks

I have indicated why I believe that the history of science matters and have argued that we need to reflect on the ways in which we can make a more fruitful use of history. In particular we need to go beyond the use of isolated case studies and we need to articulate the ways in which we can use history to address our philosophical concerns. I further argued that whatever reasons there might be for being a realist or antirealist, these are to be found in the history of science. Our epistemic values and our understanding of the world are the results of the contingent facts of history. This is why the history of science is relevant to the philosophy of science. After all, in history we may find reasons that logic alone cannot show us.

Note

[1]I am very grateful to Michael Ruse for giving me the privilege to participate in this Symposium. I would also like to thank J. Beatty, R. Burian, R. Creath, T. Nickles, E. Sober, and M. Pera for helpful discussion. Mark Solovey showed me the many ways in which time and articulation are important. I am also truly grateful to him.

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