

Explaining the Success of Science

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1. Introduction

Ever since Hilary Putnam claimed that a realist philosophy is "the only philosophy that doesn't make the success of science a miracle," explanations for the success of science have proliferated in the philosophical literature (Putnam 1975, p. 73). Realists argue that the success of science, as exhibited by our ability to accurately predict and explain a wide range of phenomena, indicates that our theories have identified some of the underlying causal structures of the world (e.g., Boyd 1985, Ellis 1985, McMullin 1984, Salmon 1984). Nonrealists counter that success cannot provide a warrant for belief in the truth of scientific theories because there have been successful theories in the past that are now believed to be false (Laudan 1981b). Instead, they offer a naturalized account whereby our theories are designed to be successful predictors and only those which fulfill this goal survive (Laudan 1987, van Fraassen, 1980).

In an attempt to circumspect the apparent deadlock in this debate over a global warrant for realism, a number of philosophers have embarked upon a more local, internal examination of science (e.g., Fine 1984). This new emphasis can be seen in the recent work on the methodological practices of experimentation by Cartwright (1983), Franklin (1984, 1986), Galison (1987), Hacking (1983, 1985), and Leplin (1984, 1986). Within this genre, a type of realism is formulated that focuses not upon the literal truth or falsity of our theories but upon the existence of the entities postulated by those theories. The successful manipulation of certain theoretical entities in the laboratory by which we are consistently able to produce predicted effects indicates that those things being manipulated actually exist regardless of whether or not our theories have accurately described them. Nonrealists remain unconvinced. They argue that there is no mystery about successful laboratory manipulations that requires a realist interpretation. Our experimental apparatus is designed and calibrated in such a way that it produces consistent results (van Fraassen 1985, Laudan 1987). The reasonableness of experimental practices and the empirical success achieved therefrom is not questioned. Rather, the debate revolves around the issue of whether this success can be used as evidence for belief in the existential claims of theories.

Philosophers are not the only ones who have turned their attention to experimentation. During the past decade, the methods of science have become a subject

for sociological scrutiny (Collins 1975, Latour and Woolgar 1979, Pickering 1984). Within the sociology of science there are at least two types of project. One, that focuses upon the identification of the social factors that influenced the particular formulations of scientific theories, has had a significant impact on the philosophy of science (Kuhn 1970, Laudan 1977). Another, that extends the range of inquiry to an analysis of the method of experimentation, could perhaps more appropriately be called the sociology of the philosophy of science. In this latter project the problems are more acute, however, and it is not clear that the contributions made to date in this area are philosophically relevant to the question of the success of science. The recent work on experimentation by Steven Shapin and Simon Schaffer (1985), *Leviathan and the Air-Pump*, provides a good example of how this type of sociological analysis fails to offer any new insights into the philosophical debate.

2. Leviathan and the Air-Pump

Shapin and Schaffer begin their work by posing a series of questions that appear to relate directly to the fundamental philosophical issues. For example, they ask: "What are the means by which experiments can be said to produce matters of fact, and what is the relationship between experimental facts and explanatory constructs?" "Why does one do experiments in order to arrive at scientific truth?" (S and S, p. 1). They chastise philosophers for not having paid attention to these questions and they propose to break new ground by answering them via an historical analysis of the rise of the experimental ideal in 17th-century England (S and S, p. 3). Accordingly, they "rephrase" the questions so that they take on a historical character, although they still seem to share the epistemic dimension of the first set: "We have set ourselves the historical task of inquiring into *why* experimental practices were accounted proper and how such practices were considered to yield reliable knowledge." (S and S, p. 13) They severely criticize and ultimately reject the more traditional accounts of experimental science offered by historians of science because they see them as "coloured by the member's self-evident method," where "the success of the experimental programme is commonly treated as its own explanation." (S and S, p. 5) From these opening polemics against philosophers and historians, one might assume that Shapin and Schaffer will be addressing the epistemic questions surrounding the justification of experimental practices, but this assumption would be wrong.

It is not until the end of their book that one finds that Shapin and Schaffer have not merely rephrased the questions but have radically altered them. Their question now concerns how a "contest among alternative forms of life and their characteristic forms of intellectual product" is resolved (S and S, p. 344). This question still seems to bear a slight resemblance to their original set of questions, but their answer to it indicates otherwise. On their analysis, victory "depends upon the political success of the various candidates in insinuating themselves into the activity of other institutions and interest groups. He who has the most, and the most powerful, allies wins." (S and S, p. 342). Experiment has no privilege. The fact that it became the method of modern science is merely a historically contingent accident:

...the general form of an answer to the question of Boyle's 'success' begins to emerge, and it takes a satisfyingly historical form. The experimental form of life achieved local success to the extent that the Restoration settlement was secured. (S and S, p. 341)

It seems, from this "answer," that the question has become one of accounting for the acceptance of experimental science by the wider polity of Restoration England. It seems trivially true that to answer this question an assessment of the social factors that made experimental practices attractive to this society would be required. In order for an experimental community to establish itself, for example, it would seem that it would

have to be compatible with the ideals of the society within which it functioned. It also seems uncontroversial that the recommended method would share certain aspects with the practical procedures accepted by that society. But is this a satisfactory explanation for the philosopher or philosophically-minded historian who wants an explanation of why experimental science succeeded in terms of the actors' reasons for accepting it as the best alternative available for achieving their goals? (See the discussion of this issue by Laudan 1984, Lugg 1984, and Westfall 1986).

If Shapin and Schaffer are not answering the traditional questions, their analysis is *prima facie* irrelevant. But, perhaps they agree with Bloor (1983, 1984) that epistemic factors are ultimately reducible to social factors so that any explanation a historian offers in terms of reasons will automatically receive the pejorative label of "self-evident" (which seems to indicate that it is, therefore, no explanation at all). While *Leviathan and the Air-Pump* leaves the reader with the strong impression that this is their view, there are no arguments within the work that support it. Indeed, in an earlier account of his project, Shapin (1980) appears to deny this reductivist view of explanation. He states that the sociologists cannot dismiss intellectualist history because the "demonstrated connections between one set of ideas and another are the necessary starting points for historians who would put an additional set of contextualist questions to the materials." (Shapin, 1980, p. 111, see also, Collins 1982 for a similar view of this project.) If *Leviathan and the Air-Pump* is intended as a reductivist explanation of the success of experimental science, then, despite the rather shakey philosophical presuppositions underlying this view, it could be argued that it does make a contribution to the philosophical debate. If, on the other hand, it is not reductivist but rather addresses a different set of questions, then, while interesting, it does not fall within the scope of the philosophical debate over the success of science. The latter alternative appears to be the better characterization of this work because of the methodological approach that Shapin and Schaffer advocate for historical inquiry.

3. Members and Strangers

In order to counteract what they perceive to be the bias of a "members' account," Shapin and Schaffer propose to look at the 17th century from Thomas Hobbes' point of view. Since Hobbes was an "anti-experimentalist," they believe that by focusing upon his criticisms of experimental science they can achieve the perspective of a "stranger" (S and S, p. 5; see Barnes 1974, for a fuller account of the "stranger" approach). To "play the stranger," is to suspect "our taken-for-granted perceptions of experimental practice and its products." (S and S, p. 6)

If we pretend to be a stranger to experimental culture, we can seek to appropriate one great advantage the stranger has over the member in explaining the beliefs and practices of a specific culture: the stranger is in a position to know that there are alternatives to those beliefs and practices. (S and S, p. 6)

Of course, Hobbes was not a stranger to the culture of 17th century England. But, because he was a participant in a debate, his role is "analogous to that of our pretend-stranger" since in the course of controversy historical actors "attempt to deconstruct the taken-for-granted quality of their antagonists' preferred beliefs and practices, and they do this by trying to display the artifactual and conventional status of those beliefs and practices." (S and S, p. 7)

Although at this point they maintain that they do not propose to "appropriate and validate the analysis of one side to scientific controversy" (S and S, p. 7), a few pages later they write that their "treatment of Boyle's experimentalism will stress the fundamental roles of convention, of practical agreement, and of labour in the creation and positive evaluation of experimental knowledge." (S and S, p. 13) While they are

playing the stranger with respect to Boyle and his fellow-experimentalists, they admit that they are "adopting something close to a 'member's account' of Hobbes's anti-experimentalism" (S and S, p. 13). This is surely the case.

Hobbes' criticisms are presented in a manner reminiscent of those of contemporary sociological critiques of science. They characterize Hobbes as having viewed the Royal Society as an "experimental confederacy," that "refused entry to Hobbes and Hobbesian philosophy. What were proclaimed public truths were in reality the private judgments of a select few." (S and S, p. 320) Further:

...there was nothing special about experimenters or their practices. They were just as politically motivated as any confederacy....The experimenters were just another conspiratorial group whose interests were in obtaining power over citizens, and whose devious confederacy sought an illegitimate autonomy from the state. (S and S, p. 320)

Shapin and Schaffer decidedly take sides. Their final sentence reads: "Hobbes was right." (S and S, p. 344) According to them, he was right because he recognized that knowledge was man-made in opposition to Boyle and other members of the Royal Society who were involved in a game "in which knowledge is, so to speak, ultimately vouched for not by human agency (individual or collective) but by reality itself." (S and S, p. 150) Boyle is characterized as a naive "empiricist" who regarded "the man-made component of knowledge as a distortion of the mind's mirroring of reality" (S and S, p. 150; this description is dependent upon Rorty's (1979) analysis). And, his experimental works are presented as little more than the routine application of enumerative induction totally lacking in any consistent philosophical justification of such a method (S and S, p. 49).

It is perfectly appropriate to take sides in a dispute, but one would hope that both sides would be fairly represented. It would not seem to be a good practice to allow one side of the debate (Hobbes) to define the position of the other side (Boyle). Why should Hobbes' interpretation of experimental science be the privileged one? Shapin and Schaffer maintain that Hobbes did not misunderstand Boyle's position. But, if their characterization of his interpretation is correct, Hobbes' certainly misrepresented it. Simply put, Boyle was neither an empiricist nor an inductivist.¹ Their appropriation of Hobbes' view has led them to present a seriously flawed account of what it is that they were to explain (experimental science). One wonders about the worth of the explanans when the explanandum has been incorrectly defined.

Shapin and Schaffer have produced a very interesting socio-political history of some of the issues that concerned members of the intellectual community of Restoration England (and their account of the number and type of air-pumps in circulation during this period is a valuable historical resource). Because Boyle was publicly apolitical, Hobbes' point of view is crucial for such a project. But, one does not need to look to Hobbes to discover that there were serious methodological alternatives present in this age. The very fact that Boyle felt compelled to argue in defense of experimentation indicates that alternatives were not merely possible but actual (see, e.g., Boyle, Vol. I, pp. 298-311, and Vol. II, pp. 1-191). It is irrelevant to the philosophical debate over the status of experimental evidence to deconstruct Boyle's defense and speculate about his supposed political motivations based upon the motivations of other members of his class. There is no such thing as an historical explanation *per se* the appeal to which would make Shapin and Schaffer's explanation more satisfying than an intellectualist account of the history of experiment. Historical explanation is judged satisfactory according to how well it has provided an answer to the historian's question. In the case of Boyle's experimental science, if the historian desires an account of the reasons behind his advocacy of the new method, then what is required is a reconstruction of Boyle's reasons based upon both his words and his practice. Granted, this would mean that the historian would produce an account wherein to

some extent Boyle and the historian would be presented as "members" of the same project. But, this type of historical prejudgment is necessary for the task and it differs little, if at all, from Shapin and Schaffer's approach wherein they and Hobbes appear to share the same critical attitude towards the social component of science.

To base sweeping philosophical conclusions about the political motivations of experimental science upon Hobbes' politically biased interpretation of the enterprise is dubious at best. But, even if one wanted to argue for the appropriateness of such an approach, the actual philosophical conclusions reached by Shapin and Schaffer are inconsequential because of their failure to appreciate and accurately represent the sophistication and complexity within contemporary philosophy of science.

4. Philosophical Conclusions

Shapin and Schaffer maintain:

In common speech, as in the philosophy of science, the solidity and permanence of matters of fact reside in the absence of human agency in their coming to be ...matters of fact are regarded to be the very 'mirror of nature.' (S and S, p. 23)

Accordingly, they seem to believe that their conclusion provides a devastating critique of the philosophy of science:

As we come to recognize the conventional and artifactual status of our forms of knowing, we put ourselves in a position to realize that it is ourselves and not reality that is responsible for what we know. (S and S, p. 344)

Who are these naive philosophers of science who would deny that we are responsible for what we know? There may be some, but certainly the philosophers mentioned above who are involved in an examination of experimental science would not be among them. Yet, it is based upon this view of the philosophy of science that Shapin and Schaffer believe that:

Any attack upon the validity and objectivity of experimental knowledge production [can] proceed by way of a display of its conventional basis: showing the work of production involved and exhibiting the lack of obligation to assent. (S and S, p. 79)

There are three components to this "attack": (1) the presence of conventions, (2) the presence of activity, and (3) the lack of logical compulsion. But these factors are certainly not new discoveries of the sociologists. The first, the necessity of the use of conventional wisdom in the design and interpretation of experiments has long been recognized by philosophers and there have been a number of positive arguments put forward in defense of this use of background knowledge (e.g. Achinstein 1983, Glymour 1980 and 1984, and Shapere, 1982).

The second component of their attack, the fact that experimental scientists (indeed, almost by definition) actively construct the phenomena which they study, has received less attention, but there are still positive arguments in defense of this practice. Boyle himself noted that a great amount of labor was required for the validation of experimental facts, when he cautioned that one must:

try those experiments very carefully, and more than once, upon which you mean to build considerable superstructures whether theoretical or practical; and to think it unsafe to rely too much upon single experiments. (Vol. I, pp. 348-49)

Most recently, Kockelmans has used the activity of scientists in support of the epistemic status of our scientific theories:

When we actively engage in our scientific practices...,when we do scientific work and engage in an intentional interaction with the phenomena we have selected for investigation, then our entire engagement is true to the degree that it reveals the relevant phenomena in the way they manifest themselves to be independent of the particular claim we now make about them....(1987, p. 23)

Finally, there is the problem of the underdetermination of theory by experimental results. Shapin and Schaffer devote a considerable amount of space to this factor, presenting Hobbes' alternative interpretation of Boyle's air-pump experiments as a "concrete exemplar" of the Duhem thesis (S and S, p. 112). However, in so doing, they neglect Duhem's own resolution of this problem. After an initial stage of indecision between competing hypotheses:

The day arrives when good sense comes out so clearly in favor of one of the two sides that the other side gives up the struggle even though pure logic would not forbid its continuation. (Duhem 1982, p. 218)

Boyle's resolution, remarkably similar to Duhem's, is also ignored by Shapin and Schaffer:

...it must be acknowledged, that rational assent may be founded upon proofs, that reach not to rigid demonstrations, it being sufficient that they are strong enough to deserve a wise man's acquiescence in them. (Boyle, Vol. IV, p. 450)

As Galison notes at the end of his recent examination of experiments in 20th century physics, the goal of the philosophical task is "to capture the building up of a persuasive argument about the world around us, even in the absence of the logician's certainty" (1987, p. 277). The lack of logical compulsion does not entail the dire consequences for experimental science that Shapin and Schaffer suppose. To quote Galison again:

Experimental physics cannot be rewritten as a logical fantasy in which all theorizing is forbidden until "facts" clinch the argument. Nor can experimentation be parodied as if it were no more grounded in reason than negotiations over the price of a street fair antique. (1987, p. 277)

In summary, *Leviathan and the Air-Pump* fails to contribute any new philosophical insights.² The historical prejudgments that Shapin and Schaffer employ make the resultant analysis offered by them of dubious evidential quality for the philosophical issues surrounding the success of experimental science. Further, even if this were not the case, their naive belief that the epistemic status of experimental results can be not only challenged but actually defeated by the presence of conventions and activity, and the lack of logical compulsion is unwarranted. They, more so than contemporary philosophers of science, seem to be wedded to an old-fashioned logicist outlook that would allow the inference to be made from the defeasibility of our attempts to understand the world to the actual defeat of these attempts.

Notes

¹Space does not permit a detailed argument for this claim. Aside from the fact that Boyle stated that "The experimental philosopher is not an empiric" (Vol. V, p. 524), and

advocated the non-empirical goal of the "discovery of the true genuine causes" (Vol. II, p. 84), his actual experimental practices do not reveal the work of an inductivist. For detailed discussions of the non-empirical elements in Boyle's philosophy see: Alexander 1985, Hall 1965, Laudan 1981a, Rogers 1972, Sargent 1986, and Woolhouse 1971.

²Perhaps Shapin and Schaffer would argue that they had no intention of joining the philosophical debate, but this would be a curious response for them to make given their lengthy polemics against the philosophy of science.

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