Our subject is experiment. We want to understand the nature and status of experimental practices and their intellectual products. These are the questions to which we seek answers: What is an experiment? How is an experiment performed? 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? How is a successful experiment identified, and how is success distinguished from experimental failure? Behind this series of particular questions lie more general ones: Why does one do experiments in order to arrive at scientific truth? Is experiment a privileged means of arriving at consensually agreed knowledge of nature, or are other means possible? What recommends the experimental way in science over alternatives to it?

We want our answers to be historical in character. To that end, we will deal with the historical circumstances in which experiment as a systematic means of generating natural knowledge arose, in which experimental practices became institutionalized, and in which experimentally produced matters of fact were made into the foundations of what counted as proper scientific knowledge. We start, therefore, with that great paradigm of experimental procedure: Robert Boyle's researches in pneumatics and his employment of the air-pump in that enterprise.

Boyle's air-pump experiments have a canonical character in science texts, in science pedagogy, and in the academic discipline of
the history of science. Of all subjects in the history of science it might be thought that this would be the one about which least new could be said. It is an oft-told tale and, in the main, a well-told tale. Indeed, there are many aspects of Boyle's experimental work and the setting in which it occurred that have been sufficiently documented and about which we shall have little novel to say: our debt to previous historical writing is too extensive to acknowledge adequately. It is entirely appropriate that an excellent account of Boyle's pneumatic experiments of the 1660s constitutes the first of the celebrated series of Harvard Case Histories in Experimental Science. This thirty-five-year-old study admirably establishes our point of departure: it shows that Boyle's air-pump experiments were designed to provide (and have since provided) a heuristic model of how authentic scientific knowledge should be secured.

Interestingly, the Harvard history has itself acquired a canonical status: through its justified place in the teaching of history of science it has provided a concrete exemplar of how to do research in the discipline, what sorts of historical questions are pertinent to ask, what kinds of historical materials are relevant to the inquiry, what sorts are not germane, and what the general form of historical narrative and explanation ought to be. Yet it is now time to move on from the methods, assumptions, and the historical programme embedded in the Harvard case history and other studies like it. We want to look again at the air-pump experiments, to put additional questions to these materials and to rephrase traditional questions. We did not initiate our project with a view to criticizing existing accounts of Boyle's experimental work. In fact, at the outset we were doubtful that we could add much to the work of distinguished Boyle scholars of the past. Yet, as our analysis proceeded, we became increasingly convinced that the questions we wished to have answered had not been systematically posed by previous writers. Why not?

A solution might reside in the distinction between "member's accounts" and "stranger's accounts." Being a member of the culture one seeks to understand has enormous advantages. Indeed, it is difficult to see how one could understand a culture to which one was a complete stranger. Nevertheless, unreflective membership also carries with it serious disadvantages to the search for understanding, and the chief of these might be called "the self-evident

1 Conant, "Boyle's Experiments in Pneumatics"; idem, On Understanding Science, pp. 29-64.
One reason why historians have not systematically and searchingly pressed the questions we want to ask about experimental practices is that they have, to a great extent, been producing accounts coloured by the member's self-evident method. In this method the presuppositions of our own culture's routine practices are not regarded as problematic and in need of explanation. Ordinarily, our culture's beliefs and practices are referred to the unambiguous facts of nature or to universal and impersonal criteria of how people just do things (or do them when behaving "rationally"). A lay member of our culture, if asked why he calls an ostrich a bird, will probably tell his inquisitor that ostriches just are birds, or he will point to unproblematic criteria of the Linnaean system of classification by which ostriches are so categorized. By contrast, this lay member will think of a range of explanations to bring to bear upon a culture that excludes ostriches from the class of birds. In the case of experimental culture, the self-evident method is particularly noticeable in historians' accounts; and it is easy to see why this should be the case, for historians are in wide agreement in identifying Boyle as a founder of the experimental world in which scientists now live and operate. Thus, historians start with the assumption that they (and modern scientists) share a culture with Robert Boyle, and treat their subject accordingly: the historian and the seventeenth-century experimentalist are both members. The historical career of experimental culture can be enlisted in support of this assumption. Boyle's programme triumphed over alternatives and objections, and in his own country it did so very rapidly, largely aided and abetted by the vigorously partisan publicity of the Royal Society of London. The success of the experimental programme is commonly treated as its own explanation. Even so, the usual way in which the self-evident method presents itself in historical practice is more subtle—not as a set of explicit

---

3 A classic site for relativist and realist discussions of classification and the natural world is Bulmer, "Why is the Cassowary not a Bird?" Bulmer's account is crucially asymmetrical: only cultures that do not classify the cassowary as a bird arouse his curiosity. For symmetrical treatments of this question, see Bloor, "Durkheim and Mauss Revisited"; idem, Knowledge and Social Imagery, chap. 1; Barnes and Bloor, "Relativism, Rationalism and the Sociology of Knowledge," esp. pp. 37-38.

4 For a powerful nineteenth-century expression of this view, see Herschel, Preliminary Discourse on the Study of Natural Philosophy, pp. 115-116. Among many twentieth-century examples, see L. T. More, Life of Boyle, p. 239: "[Boyle's] conclusions were universally accepted, disregarding the objections of Linus and Hobbes, and he was immediately proclaimed as the highest authority in science."
claims about the rise, acceptance, and institutionalization of experiment, but as a disposition not to see the point of putting certain questions about the nature of experiment and its status in our overall intellectual map.

The member's account, and its associated self-evident method, have great instinctive appeal; the social forces that protect and sustain them are powerful. The member who poses awkward questions about "what everybody knows" in the shared culture runs a real risk of being dealt with as a troublemaker or an idiot. Indeed, there are few more reliable ways of being expelled from a culture than continuing seriously to query its taken-for-granted intellectual framework. Playing the stranger is therefore a difficult business; yet this is precisely what we need to do with respect to the culture of experiment. We need to play the stranger, not to be the stranger. A genuine stranger is simply ignorant. We wish to adopt a calculated and an informed suspension of our taken-for-granted perceptions of experimental practice and its products. By playing the stranger we hope to move away from self-evidence. We want to approach "our" culture of experiment as Alfred Schütz suggests a stranger approaches an alien society, "not [as] a shelter but [as] a field of adventure, not a matter of course but a questionable topic of investigation, not an instrument for disentangling problematic situations but a problematic situation itself and one hard to master." 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. The awareness of alternatives and the pertinence of the explanatory project go together.

Of course, we are not anthropologists but historians. How can the historian play the stranger to experimental culture, a culture we are said to share with a setting in the past and of which one of our subjects is said to be the founder? One means we can use is

---


6 See the "experiments" of Harold Garfinkel on questioning taken-for-granted rules of social interaction: Studies in Ethnomethodology, esp. chap. 2.

7 The relative advantages of the member's and stranger's perspective have been debated by sociologists undertaking participant observation of modern science. Latour and Woolgar, Laboratory Life, chap. 1, are wary of the methodological dangers of identifying with the scientists they study, whereas Collins, "Understanding Science," esp. pp. 373-374, argues that only by becoming a competent member of the community under study can one reliably test one's understanding.
the identification and examination of episodes of *controversy* in the past. Historical instances of controversy over natural phenomena or intellectual practices have two advantages, from our point of view. One is that they often involve disagreements over the reality of entities or propriety of practices whose existence or value are subsequently taken to be unproblematic or settled. In H. M. Collins' metaphor, institutionalized beliefs about the natural world are like the ship in the bottle, whereas instances of scientific controversy offer us the opportunity to see that the ship was once a pile of sticks and string, and that it was once outside the bottle. Another advantage afforded by studying controversy is that historical actors frequently play a role analogous to that of our pretend-stranger: in the course of controversy they 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. Since this is the case, participants in controversy offer the historian resources for playing stranger. It would, of course, be a great mistake for the historian simply to appropriate and validate the analysis of one side to scientific controversy, and this is not what we propose to do. We have found it valuable to note the constructive and deconstructive strategies employed by both sides to the controversy. While we use participants' accounts, we shall not confuse them with our own interpretative work: the historian speaks for himself.

The controversy with which we are concerned took place in England in the 1660s and early 1670s. The protagonists were Robert Boyle (1627-1691) and Thomas Hobbes (1588-1679). Boyle appears as the major practitioner of systematic experimentation and one of the most important propagandists for the value of experimental practices in natural philosophy. Hobbes takes the role of Boyle's most vigorous local opponent, seeking to undermine the particular claims and interpretations produced by Boyle's researches and, crucially, mobilizing powerful arguments why the experimental programme could not produce the sort of knowledge Boyle recommended. There are a number of reasons why the Hobbes-Boyle disputes are particularly intractable ones for the historian to analyze. One reason is the extent to which the figure of Hobbes as a *natural philosopher* has disappeared from the literature. Kargon rightly says that "Hobbes was one of the three most important mechanical philosophers of the mid-seventeenth century,

---

8 Collins, "The Seven Sexes"; idem, "Son of Seven Sexes."
along with Descartes and Gassend." There is no lack of evidence of the seriousness with which Hobbes's natural philosophical views were treated in the seventeenth century, especially, but not exclusively, by those who considered them to be seriously flawed. We know that as late as the early eighteenth century Hobbes's natural philosophical tracts formed an important component of the Scottish university curriculum. Yet by the end of the eighteenth century Hobbes had largely been written out of the history of science. The entry on Hobbes in the 1797 third edition of the Encyclopaedia Britannica scarcely mentions Hobbes's scientific views and totally ignores the tracts written against Boyle. Much the same is true of the Encyclopaedia's 1842 Dissertation on the History... of Mathematical and Physical Science: Hobbes is to be remembered as an ethical, political, psychological, and metaphysical philosopher; the unity of those concerns with the philosophy of nature, so insisted upon by Hobbes, has been split up and the science dismissed from consideration. Even Mintz's article on Hobbes in the Dictionary of Scientific Biography is biased heavily towards his moral, political, and psychological writings. Fortunately for us, since Brandt's 1928 monograph on Hobbes's mechanical philosophy, this situation has begun to improve. Our indebtedness to recent work on Hobbes's science by scholars such as R. H. Kargon, J.W.N. Watkins, Alan Shapiro, Miriam Reik, and Thomas Spragens will be evident in what follows. Nevertheless, we are still very far from appreciating Hobbes's true place in seventeenth-century natural philosophy, and, if this book stimulates further research, one of its functions will have been fulfilled.

Kargon suggests that one of the reasons for the neglect of Hobbes by historians of science lies in the fact that he disagreed with the hero Boyle and, accordingly, suffered ostracism from the Royal Society of London. There is no doubt that Hobbes's scientific controversies in England, all of which his contemporaries considered he decisively lost, have much to do with his dismissal by historians. Within the tradition of "Whig" history, losing sides have little interest, and in no type of history has this tendency been more

---

9 Kargon, Atomism in England, p. 54.
11 Anon., "Hobbes"; Mackintosh, "Dissertation Second," pp. 316-323 (on ethical philosophy); Playfair, "Dissertation Third" (on mathematical and physical science, where Hobbes is scarcely mentioned at all); Mintz, "Hobbes."
apparent than in classical history of science. This book is
concerned with Hobbes’s natural philosophical controversies, yet his
mathematical disputes with John Wallis and Seth Ward, which we
cannot treat in any detail, were lost even more spectacularly and
have disappeared from the historical record more thoroughly than
the fight with Boyle. In Leslie Stephen’s Dictionary of National Bi-
ography entry, Hobbes’s opponents showed his “manifold absurd-
ities”; Croom Robertson’s more extended account in the eleventh
edition of the Encyclopaedia Britannica echoes that judgment; and
no historian dissents.

The situation is similar in historians’ accounts of Hobbes’s con-
troversies with Boyle. There is not very much written about these
disputes, and even that little has contained some fundamental er-
rors. For example, one writer has claimed that Hobbes’s objections
to Boyle’s natural philosophy stemmed from Hobbes’s belief in the
Aristotelian horror vacui (which is quite wrong), and another, more
sensitive, writer has argued that Hobbes approved of a central role
for experimentation in natural philosophy (which we shall be at
pains to show to be wrong). It is possible that part of the reason
for these errors, and for the general neglect of the Hobbes-Boyle
controversies, is documentary. So far as we have been able to de-
termine, only two historians give solid indications that they have
opened the crucial text and digested any of its contents: Hobbes’s
Dialogus physicus de natura aeriis of 1661. True, Hobbes’s Dialogus

15 The Whiggish tendency in the treatment of the disputes between Boyle, Hobbes,
and Linus is briefly noted in Brush, Statistical Physics, p. 16.
14 Stephen, “Hobbes,” esp. p. 935 (cf. idem, Hobbes, pp. 51-54); Robertson,
est. pp. 18-21, 40-41. See also Scott, “John Wallis,” p. 65. For work on Hobbes’s
geometry and the controversies with the Oxford professors, see Sacksteder,
Breidert, “Les mathématiques et la méthode mathématique chez Hobbes”; Scott,
The Mathematical Work of Wallis, ch. 10.
15 For the horror vacui claim, see Greene, “More and Boyle on the Spirit of Nature,”
p. 463; for a note pointing out this error, see Applebaum, “Boyle and Hobbes.”
16 Watkins, Hobbes’s System, p. 70n. This claim is dealt with in detail in chapter 4
below.
17 The exceptions are Gargani, Hobbes e la scienza, pp. 278-285, and Lupoli, “La
polemica tra Hobbes e Boyle.” Gargani points out that the Dialogus “belongs to a
fairly advanced stage of Hobbes’s philosophical and scientific career.” Gargani does
not see the Dialogus as developing anything original; instead, he reads it as contin-
uous with the plenist physics and the critique of naive experimentalism in earlier
writings (notably De corpore and the Short Tract on First Principles: see pp. 134-138,
271-278). But Gargani only cites the two prefatory dedications of Hobbes’s Dialogus
has never been translated from the Latin original, and this may go some way to explain its neglect. (To remedy this state of affairs, we offer an English translation, by Schaffer, as an appendix to this book.) With these two exceptions, historians have been content to align themselves with the victorious Boyle and his associates, to repeat Boyle's judgment on Hobbes's text, and to keep silent about what Hobbes actually had to say. Even Brandt, who wrote the most detailed study of Hobbes's science, declined engagement with the *Dialogus physicus* and later natural philosophical texts. Brandt, too, accepted Boyle's evaluation of Hobbes's views:

We will not examine the works subsequent to *De Corpore* [of 1655, six years before the *Dialogus physicus*] . . . . No less than three times during these years Hobbes took up his physics for further elaboration . . . , but it retains exactly the same character as the physics of *De Corpore*. This character becomes especially conspicuous in Hobbes' attack on Boyle's famous "New Experiments touching the Spring of the Air." Here again Hobbes shows how little he understands the significance of the experiment. In spite of the continual experiments on vacuity, and pays no attention to the actual text or to the attack on Boyle's air-pump programme. Lupoli gives a full and valuable exposition of Boyle's response to Hobbes in the *Examen*. He places the controversy in the context of the earlier pneumatic trials in Italy and France in the 1640s, notably the Pascal-Noël debate. Lupoli suggests that Hobbes attacked Boyle because of his "disappointment at being excluded from the new scientific association, but above all the disillusion and preoccupation with seeing his foundation of physical science ignored" (p. 324). Lupoli highlights Boyle's prolixity as a response to Hobbes's attack on the "rhetoric of ingenuity," and Boyle's tactic of point-by-point refutation of empirical claims as a means of avoiding a direct confrontation with Hobbes's whole physical programme (p. 329). But Lupoli is much more interested in Boyle's utterances on method and on experimental philosophy, and does not give any detailed account of the sources of Hobbes's own polemic. We are grateful to Agostino Lupoli for a copy of his paper (received after our manuscript was written): it is the only source we have found that cites the *Dialogus* in detail. Other major recent sources for Hobbes's natural philosophy do not treat the controversies with Boyle in any detail, nor do they examine the contents of Hobbes's *Dialogus physicus*; see, for example, Spragens, *The Politics of Motion*, esp. chap. 3; Reik, *The Golden Lands of Hobbes*, chap. 7; Goldsmith, *Hobbes's Science of Politics*, chap. 2, although each of these is valuable in other connections. In addition, there are many allusions to Hobbes's science by mainstream Hobbes scholars. They have tended to mine his philosophy of nature because of the generally high evaluation that historians of ideas have placed upon the significance of Hobbes's political and psychological theories and because of their conviction that there must be an overall pattern in his thought. Historians of science, given their low evaluation of Hobbes's natural philosophy and mathematics, have not tended to search for such a pattern.
in spite of the invention of the air-pump, Hobbes still adhered to his view of the full world. Hobbes' last years were rather tragic. He did not well understand the great development of English empirical science that took place just at that time. . . . And when the members of the Royal Society adopted the experimental method of research . . . Hobbes could no longer keep abreast of them.18

Here we see the germ of a standard historiographic strategy for dealing with the Hobbes-Boyle controversy, and, arguably, for handling rejected knowledge in general. We have a dismissal, the rudiments of a causal explanation of the rejected knowledge (which implicitly acts to justify the dismissal), and an asymmetrical handling of rejected and accepted knowledge. First, it is established that the rejected knowledge is not knowledge at all, but error. This the historian accomplishes by taking the side of accepted knowledge and using the victorious party's causal explanation of their adversaries' position as the historian's own. Since the victors have thus disposed of error, so the historian's dismissal is justified.19 Thus, L. T. More notes that Hobbes's "sneers" at Boyle were "a farrago of nonsense," and quotes Boyle's decisive riposte without detailing what Hobbes's position was.20 McKie deals with the disputes simply by saying that "Boyle disposed very competently of Hobbes's arguments and very gracefully of his contentious and sullen outburst."21 John Laird concludes that "the essential justice of Boyle's criticisms [of Hobbes] shows . . . that it would be unprofitable to examine much of Hobbian special physics in detail. . . ."22 Peters claims that Hobbes's criticisms "would have come better from one . . . who had himself done some experiments" (which cannot be the best way of seeking to understand a controversy over the validity and value of experiment),23 and R. F. Jones concurs.24 Other his-

19 For alternative sociological and historical approaches to rejected knowledge, see the contributions to Wallis, ed., On the Margins of Science, and Collins and Pinch, Frames of Meaning.
20 L. T. More, Life of Boyle, p. 97. Maddison's more recent Life of Boyle (pp. 106-109) has even less to say about the controversy.
21 McKie, "Introduction," pp. xii*-xiii*.
22 Laird, Hobbes, p. 117.
LEVIATHAN AND THE AIR-PUMP
HOBSES, BOYLE, AND THE EXPERIMENTAL LIFE

Steven Shapin and Simon Schaffer

"... Steven Shapin and Simon Schaffer have ventured beyond ordinary history of science or history of ideas to produce a novel 'exercise in the sociology of scientific knowledge.' . . . [a] historical study rich in new interpretations and notable for the use of sources of a kind not hitherto fully exploited by scholars."

—I. Bernard Cohen, American Historical Review

"Shapin and Schaffer work out the implications of these debates [between Hobbes and Boyle] for the history of science with great skill of interpretation and exposition. They use their findings and their analysis to give an explanation of the experimental enterprise in general, which, although it is not philosophical in nature, always takes philosophy most seriously. This is simply one of the most original, enjoyable and important books published in the history of science in recent years."

—Owen Hannaway, Technology and Culture

"If any proof of the intellectual buoyancy or intrinsic worth of the history and philosophy for science was needed, nothing better could be provided than this study by Steven Shapin and Simon Schaffer. . . . Their findings suggest the futility of wrenching science from its ideological context, and not only with respect to the seventeenth century; they also detect parallels with the crisis of confidence affecting contemporary science."

—Charles Webster, Times Literary Supplement

Steven Shapin is Professor of Sociology at the University of California, San Diego. Simon Schaffer is Lecturer in the Department of History and Philosophy of Science, Cambridge University.