LEVIATHAN AND THE AIR-PUMP
HOBSES, BOYLE, AND THE EXPERIMENTAL LIFE

INCLUDING A TRANSLATION OF THOMAS HOBSES,
DIALOGUS PHYSICUS DE NATURA AERIS,
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I

Understanding Experiment

Adso: "But how does it happen," I said with admiration, "that you were able to solve the mystery of the library looking at it from the outside, and you were unable to solve it when you were inside?"

William of Baskerville: "Thus God knows the world, because He conceived it in His mind, as if from the outside, before it was created, and we do not know its rule, because we live inside it, having found it already made."

Umberto Eco, The Name of the Rose

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"

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1 Conant, "Boyle's Experiments in Pneumatics"; idem, On Understanding Science, pp. 29-64.

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method." 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.

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See, for example, Douglas, "Self-Evidence."

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 fewer 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 Schutz 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 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 artificial 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,

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5 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.

a Collins, "The Seven Sexes"; idem, "Son of Seven Sexes."
along with Descartes and Gassend.¹⁰ 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.¹¹ 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.¹² 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.¹³ 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 apparent than in classical history of science.¹⁴ 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 Biography entry, Hobbes's opponents showed his "manifold absurdities"; Croom Robertson's more extended account in the eleventh edition of the Encyclopaedia Britannica echoes that judgment; and no historian dissents.¹⁵

The situation is similar in historians' accounts of Hobbes's controversies with Boyle. There is not very much written about these disputes, and even that little has contained some fundamental errors. 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),¹⁶ 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).¹⁷ 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 determine, 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 aeris of 1661.¹⁸ True, Hobbes's Dialogus

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¹³ The Whiggish tendency in the treatment of the disputes between Boyle and Hobbes, and Linus is briefly noted in Brush, Statistical Physics, p. 16.
¹⁵ For the horror vacui claim, see Greene, "More and Boyle on the Spirit of Nature," p. 455; 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 continuous 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 Aire.' 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 precoccupation with seeing his foundation of physical science ignored" (p. 324). Lupoli highlights Boyle's proximity 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 Gods 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's 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.  

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. 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.  

McKie deals with the disputes simply by saying that "Boyle disposed very competently of Hobbes's arguments and very gracefully of his contentious and splenetic outburst."  

John Laird concludes that "the essential justice of Boyle's criticisms [of Hobbes] shows . . . that it would be unprofitable to examine much of Hobbesian special physics in detail . . ." 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)  

and R. F. Jones concurs. 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.
torians go further in wiping the historical record clean of significant opposition to the experimental programme: Marie Boas Hall, though without mentioning Hobbes by name, says that "No one but a dedicated Aristotelian" (which Hobbes most certainly was not) "could fail to find Boyle's arguments powerful and convincing," and Barbara Shapiro, in her admirable account of English empiricism and experimentalism, concludes that "Except for a tiny group of critics who poked fun at the virtuosi" (whose names she does not mention), "there was no serious opposition to the new philosophy." Pervasively, historians have drawn upon the notion of "misunderstanding" (and the reasons for it) as the basis of their causal accounting and dismissal of Hobbes's position. The Harvard Case Histories relate that Hobbes's arguments against Boyle "were based in part on a misunderstanding of Boyle's views."

M. A. Stewart refers to Boyle's pneumatics as leading "Hobbes into ill-advised controversy on matters he did not understand." Leslie Stephen and Croom Robertson both attempt to explain Hobbes's misunderstanding by referring to factors that distorted his judgment or made him unfit to appreciate the validity of Boyle's programme: he was ill-qualified in mathematics and physics; he was too old and rigid at the time of his controversies with Boyle; he was temperamentally obstinate and dogmatic; he had ideological axes to grind. (To the best of our knowledge no historian has ever suggested that Boyle may have "misunderstood" Hobbes.)

Since our way of proceeding will dispense with the category of "misunderstanding" and the asymmetries associated with it, some words on method are indicated here. Almost needless to say, our purpose is not evaluative: it is descriptive and explanatory. Nevertheless, questions relating to evaluation do figure centrally in this book, and they do so in several ways. We have said that we shall be setting out by pretending to adopt a "stranger's perspective" with respect to the experimental programme; we shall do this be-


* B. Shapiro, Probability and Certainty, p. 73; cf. p. 68.

* Conant, "Boyle's Experiments in Pneumatics," p. 49.


cause 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. As part of the same exercise we shall be adopting something close to a "member's account" of Hobbes's anti-experimentalism. That is to say, we want to put ourselves in a position where objections to the experimental programme seem plausible, sensible, and rational. Following Gellner, we shall be offering a "charitable interpretation" of Hobbes's point of view. Our purpose is not to take Hobbes's side, nor even to resuscitate his scientific reputation (though this, in our opinion, has been seriously undervalued). Our goal is to break down the aura of self-evidence surrounding the experimental way of producing knowledge, and "charitable interpretation" of the opposition to experimentalism is a valuable means of accomplishing this. Of course, our ambition is not to rewrite the clear judgment of history: Hobbes's views found little support in the English natural philosophical community. Yet we want to show that there was nothing self-evident or inevitable about the series of historical judgments in that context which yielded a natural philosophical consensus in favour of the experimental programme. Given other circumstances bearing upon that philosophical community, Hobbes's views might well have found a different reception. They were not widely credited or believed—but they were believable; they were not counted to be correct—but there was nothing inherent in them that prevented a different evaluation. (True, there were points at which Hobbes's criticisms were less than well-informed, just as there were aspects of Boyle's position that might be regarded as ill-informed and even sloppy. If the historian wanted to evaluate the actors by the standards of present-day scientific procedure, he would find both Hobbes and Boyle vulnerable.) On the other hand, our 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. We shall try to identify those features of the historical setting that bore upon intellectuals' decisions that these conventions were appropriate, that such agreement was necessary, and that the labour involved in experimental knowledge-production was worthwhile and to be preferred over alternatives.

Far from avoiding questions of "truth," "objectivity," and "proper method," we will be confronting such matters centrally. But we

shall be treating them in a manner slightly different from that which characterizes some history and much philosophy of science. "Truth," "adequacy," and "objectivity" will be dealt with as accomplishments, as historical products, as actors' judgments and categories. They will be topics for our inquiry, not resources unreflectively to be used in that inquiry. How and why were certain practices and beliefs accounted proper and true? In assessing matters of scientific method we shall be following a similar path. For us, methodology will not be treated solely as a set of formal statements about how to produce knowledge, and not at all as a determinant of intellectual practice. We shall be intermittently concerned with explicit verbal statements about how philosophers should conduct themselves, but such method-statements will invariably be analyzed in relation to the precise setting in which they were produced, in terms of the purposes of those making them, and in reference to the actual nature of contemporary scientific practice. More important to our project is an examination of method understood as real practical activity. For example, we shall devote much attention to such questions as: How is an experimental matter of fact actually produced? What are the practical criteria for judging experimental success or failure? How, and to what extent, are experiments actually replicated, and what is it that enables replication to take place? How is the experimental boundary between fact and theory actually managed? Are there crucial experiments and, if so, on what grounds are they accounted crucial? Further, we shall be endeavouring to broaden our usual appreciations of what scientific method consists of and how method in natural philosophy relates to practical intellectual procedures in other areas of culture and in the wider society. One way we shall try to do this is by situating scientific method, and controversies about it, in a social context.

By adducing "social context" it is routinely understood that one is pointing to the wider society, and, to a very large extent, we shall be concerned to show the connections between the conduct of the natural philosophical community and Restoration society in general. However, we also mean something else when we use the term "social context." We intend to display scientific method as crystallizing forms of social organization and as a means of regulating social interaction within the scientific community. To this end, we will make liberal, but informal, use of Wittgenstein's notions of a "language-game" and a "form of life." We mean to approach scientific method as integrated into patterns of activity. Just as for Wittgenstein the term 'language-game' is meant to bring into prominence the fact that the speaking of language is part of an activity or of a form of life, so we shall treat controversies over scientific method as disputes over different patterns of doing things and of organizing men to practical ends. We shall suggest that solutions to the problem of knowledge are embedded within practical solutions to the problem of social order, and that different practical solutions to the problem of social order encapsulate contrasting practical solutions to the problem of knowledge. That is what the Hobbes-Boyle controversies were about.

It will not escape our readers' notice that this book is an exercise in the sociology of scientific knowledge. One can either debate the possibility of the sociology of knowledge, or one can get on with the job of doing the thing. We have chosen the latter option. It follows from our decision that we shall be making relatively few references to the theoretical literature in the sociology of knowledge that has been a major and continuing source of inspiration to our project. Nevertheless, we trust that our practical historical procedures will bear sufficient witness to our obligations in that quarter. Our methodological debts also extend in many other directions, and they are too deep and extensive to be adequately acknowledged. Among Hobbes scholars we are especially indebted to J.W.N. Watkins (for his insistence upon the relationships between the natural and civic philosophy), even while we dissent from him on the issue of Hobbes's attitudes to experiment; and to Quentin Skinner (for aspects of his historiography), even while diverging from him over Hobbes's relations with the Royal Society. Among historians of science we have found substantial inspiration in recent studies of the actual nature of experimental practice: we have particularly in mind the work of Robert Frank and John Heilbron. The particular orientation to the understanding of scientific experiment that we have found most valuable derives from the work

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39 Wittgenstein, Philosophical Investigations, I, 23; idem, Blue and Brown Books, pp. 17, 81; Bloor, Wittgenstein, chap. 3. Foucault's "discourse" has a number of interesting similarities with Wittgenstein's "language-game," but we prefer Wittgenstein because of his stress on the primacy of practical activity. For Foucauldian usages, see, especially, The Archaeology of Knowledge, chaps. 1-2.

38 The present state of the sociology of scientific knowledge as an empirical practice is examined in Shapin, "History of Science and Its Sociological Reconstructions."
of British and French micro-sociologists of science: H. M. Collins, T. J. Pinch, Bruno Latour, and Andrew Pickering, and from the pioneering Ludwik Fleck.

Since these debts are obvious and evident, it may be of some interest to acknowledge two pieces of empirical history whose connection with our own project may be less readily apparent, but which exemplify similar orientations to those employed here. John Keegan opens his magnificent study of the history of battle with the following confession:

I have not been in a battle; not near one, nor heard one from afar, nor seen the aftermath . . . . I have read about battles, of course, have talked about battles, have been lectured about battles. . . . But I have never been in a battle. And I grow increasingly convinced that I have very little idea of what a battle can be like.34

It is a graceful admission of an ignorance that Keegan recognized in himself as a teacher at Sandhurst and in many military historians. Without this recognition, Keegan would have been unable to write the vivid and moving history that he ultimately produced. As we began the research for this book, we felt ourselves to be in a position similar to Keegan’s. We had read much about experiment; we had both even performed a few as students; but we did not feel that we had a satisfactory idea of what an experiment was and how it yielded scientific knowledge. The parallel with Keegan’s account of battle extends even farther. Keegan identifies a dominant variety of military history, shaped by Count von Moltke, which he refers to as “General Staff History.” In General Staff History, what is of overarching significance is the role of the generals, their strategic planning, their rational decision-making, and their influence on the ultimate course of the battle. What is systematically left out of General Staff History is the contingency and the confusion of actual combat, the role of small groups of soldiers, the relationship between battle on the ground and the planning of the generals. It would not be a flight of fancy to recognize in General Staff History a family resemblance to “rational reconstructionist” tendencies in the history and philosophy of science. The “von Moltkes” of the history of science have shown similar disinclinations to engage with actual scientific practice, preferring idealizations and simplifications to messy contingencies, speech of essences to the identification of conventions, references to unproblematic facts of nature and transcendent criteria of scientific method to the historical work done by real scientific actors.35 It is too much to think that we have added to the history of experiment a fraction of what Keegan has contributed to military history, but we are happy to be engaged in the same historiographic enterprise.

Our other unexpected model is closer in its empirical focus to our own objects of study: Svetlana Alpers’ The Art of Describing. Unfortunately for us, Alpers’ book was published when our own work was substantially completed, and we have not been able to engage with it as extensively as we would have liked. Nevertheless, the parallels with our project are highly important, and we want briefly to point them out. Alpers is concerned with Dutch descriptive art in the seventeenth century. In particular, she wants to understand the assumptions behind Dutch preferences for descriptive painting and the conventions employed in making such pictures. She writes: “It was a particular assumption of the seventeenth century that finding and making, our discovery of the world and our crafting of it, are presumed to be one.”36 She shows that such assumptions spread across disparate areas of culture: universal language projects, the experimental programme in science, and painting, and that they were particularly pronounced in the Netherlands and in England. Both Dutch descriptive painting and English empiricist science involved a perceptual metaphor for knowledge: “By this I mean a culture that assumes that we know what we know through the mind’s mirroring of nature.”37 The basis for certain knowledge was to be nature witnessed. The craft of the painter,

34 Keegan, The Face of Battle, p. 15; see also Keegan’s more detailed account of a World War II series of battles, Six Armies in Normandy.
35 The deep-rooted bias against the study of experimental practice displayed by historians of science has been noted by several writers; see, for example, Eklund, The Incompletist Chymist, p. 1. Even philosophers are now beginning to admit the anti-practice and pro-theory prejudices of their discipline; see Hacking, Representing and Intervening, chap. 9, esp. pp. 149-150: “History of the natural sciences is now almost always written as a history of theory. Philosophy of science has so much become the philosophy of theory that the very existence of pre-theoretical observations or experiments has been denied.”
36 Alpers, The Art of Describing, p. 27. Similar exercises in art history that offer valuable insights to the sociologically inclined historian of science include Baxandall’s Painting and Experience, his Lime Wood Sculptors of Renaissance Germany, and Edgerton’s The Renaissance Discovery of Linear Perspective.
and the art of the experimentalist, was, therefore, to make
terpretations that reliably imitated the act of unmediated seeing.

There are two points in Alpers’ account of special interest to us.
One is the contrast she draws between Northern (and particularly
Dutch) conceptions of the picture and those characteristic of Italian
painting. In the latter the painting was conceived primarily as a
gloss on a text; in the former the textual meaning of the picture
was dispensed with in favour of direct visual apprehension of
natural reality. Although the details of the contrast cannot concern us
here, Alpers concludes that different theories of picturing expressed
different conceptions of knowledge: the text versus the
eye. The parallel between the Hobbes-Boyle controversy, and its
underlying conflict over theories of knowledge, is far from exact;
nevertheless, in the case of conflicts over the propriety of experimental
methods we see a quite similar dispute over the reliability
of the eye, and of witnessing, as the basis for generating and warran-
ting knowledge. Secondly, Alpers adopts what we might term a
“stranger’s perspective” to the nature of realist images. Their
“mirroring” of reality is treated as the product of convention and
of craft: “To appear lifelike, a picture has to be carefully made.”
The craft of realist representation is predicated upon the acceptance
of Hooke’s conventions for making realist statements in science:
the “sincere hand” and the “faithful eye.”38 With the acceptance
of this convention for knowledge, and with the execution of the
chart of representation, the artful nature of making representations
disappears, and they acquire the status of mirrors of reality.
Our project, therefore, is the same as Alpers’: to display the con-
ventions and the craft.

In the following chapter we examine the form of life that Boyle
proposed for experimental philosophy. We identify the technical,
literary, and social practices whereby experimental matters of fact
were to be generated, validated, and formed into bases for con-
sensus. We pay special attention to the operation of the air-pump
and the means by which experiments employing this device could
be made to yield what counted as unassailable knowledge. We dis-
cuss the social and linguistic practices Boyle recommended to ex-
perimentalists; we show how these were important constitutive
elements in the making of matters of fact and in protecting such facts
from items of knowledge that were thought to generate discord

38 Alpers, The Art of Describing, pp. 72-73 (quoting Robert Hooke’s Micrographia
[1665], sig a2r).
Boyle’s experimentalism and Hobbes’s demonstrative way were both offered as solutions to the problem of order. In chapter 7 we attempt to locate solutions to this problem in the wide-ranging Restoration debate over the nature and bases of assent and order in society. This debate provided the context in which different programmes for the production and protection of order were evaluated. We seek to show here the nature of the intersection between the history of natural philosophy and the history of political thought and action. One solution (Boyle’s) was to set the house of natural philosophy in order by remedying its divisions and by withdrawing it from contentious links with civic philosophy. Thus repaired, the community of natural philosophers could establish its legitimacy in Restoration culture and contribute more effectively to guaranteeing order and right religion in society. Another solution (Hobbes’s) demanded that order was only to be ensured by erecting a demonstrative philosophy that allowed no boundaries between the natural, the human, and the social, and which allowed for no dissent within it.

In the concluding chapter we draw out some of the implications of this study for the history of science and the history of politics. We argue that the problem of generating and protecting knowledge is a problem in politics, and, conversely, that the problem of political order always involves solutions to the problem of knowledge.
Our previous usages of terminology such as "experimental space" or "philosophical space" have been twofold: we have referred to space in an abstract sense, as a cultural domain. This is the sense customarily intended when one speaks of the boundaries of disciplines or the overlap between areas of culture. The cartographic metaphor is a good one: it reminds us that there are, indeed, abstract cultural boundaries that exist in social space. Sanctions can be enforced by community members if the boundaries are transgressed. But we have also, at times, used the notion of space in a physically more concrete sense. The receiver of the air-pump circumscribed such a space, and we have shown the importance attached by Boyle to defending the integrity of that space. Yet we want to elaborate some notions concerning a rather larger-scale physical space. If someone were to be asked in 1660, "Where can I find a natural philosopher at work?", to what place would he be directed? For Hobbes there was to be no special space in which one did natural philosophy. Clearly, there were spaces that were deemed grossly inappropriate. Since philosophy was a noble activity, it was not to be done in the apothecary shop, in the garden, or in the tool room. He told his adversaries that philosophers were not "apothecaries," "gardeners," or any other sort of "workmen." Neither was philosophy to be withdrawn into the Inns of Court, the physicians' colleges, the clerics' convocations, or the universities. Philosophy was not the exclusive domain of the professional man. Any such withdrawal into special professional spaces threatened the public status of philosophy. Recall Hobbes's indictment of the Royal Society as yet another restricted professional space. He asked, "Cannot anyone who wishes come?" and gave the answer, "The place where they meet is not public."* We have seen that the experimentalists also insisted upon the public nature of their activity, but Boyle's "public" and Hobbes's "public" were different usages. Hobbes's philosophy had to be public in the sense that it must not become the preserve of interested professionals. The special interests of professional groups had acted historically to corrupt knowledge. Geometry had escaped this appropriation only because, as a contingent historical matter, its theorems and findings had not been seen to have a bearing on such interests: "Because men care not, in that subject, what be truth, as a thing that crosses no man's

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1 We are not aware of any specific debts for this usage. However, topographic

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*Lords and Commons of England, consider what Nation it is wherof ye are, and wherof ye are the Governours.
Milton, Areopagitica
ambition, profit or lust: Hobbes's philosophy also had to be public because its purpose was the establishment of agreement: settling the meanings and proper uses of words. Its public, not a witnessing public, not a public of erudite readers, but one of minds and tongues. In Boyle's natural philosophy there was to be a special space in which experiments were performed and witnessed. In the nascent laboratory, experiments were performed and witnessed. This was the nascent laboratory. Consider the experimental scene in Figure 2.1. This picture comes from Caspar Schott's Physica nova. What kind of experimental scene was this? How was knowledge being constituted? This was the nascent laboratory. It is a book that promoted Boyle's decision to begin the construction of an air-pump. It is a book that advertises Boyle's new machine. It is not shown actually working the pump; the pump is not shown in any special way. The machine is not dressed with any special dressing, it is not dressed with any special dressing. The architectural space in which the experiment is set is a courtyard or forum. We do not know whether it is meant that the experiment was actually done in public. The engraver was interested in artistic conventions familiar to him. The engraver was not interested in public space. The engraver was interested in his public, not the public. It is a picture showing the experimental scene in which knowledge was constituted.
practice. The public space insisted upon by experimental philosophers was a space for collective witnessing. We have shown the importance of witnessing for the constitution of the matter of fact. Witnessing was regarded as effective if two general conditions could be satisfied: first, the witnessing experience had to be made accessible; second, witnesses had to be reliable and their testimony had to be creditable. The first condition worked to open up experimental space, while the second acted to restrict entry. What in fact resulted was, so to speak, a public space with restricted access. (Arguably, this is an adequate characterization of the scientific laboratory of the late twentieth century: many laboratories have no legal sanction against public entry, but they are, as a practical matter, open only to "authorized personnel.") Restriction of access, we have indicated, was one of the positive recommendations of this new experimental space in Restoration culture. Either by decision or by tacit processes, the space was restricted to those who gave their assent to the legitimacy of the game being played within its confines.

In chapter 5 we described differences in the engagements Boyle conducted with two sorts of adversaries: those who disputed moves within the experimental game and those who disputed the game. The latter could be permitted entry to the experimental community only at the price of putting that community's life at risk. Public stipulations about the accessibility of the experimental laboratory were tempered by the practical necessity of disciplining the experimental collective. This tension meant that Hobbes's identification of the Royal Society as a restricted place was potentially damaging, just as it is damaging in modern liberal societies to remark upon the sequesterment of science. Democratic ideals and the exigencies of professional expertise form an unstable compound.5 Hobbes's identification of restrictions on the experimental public shows why virtual witnessing was so vitally important, and why troubles in the experimental programme of physical replication were so energetically dealt with. Virtual witnessing acted to ensure that witnesses to matters of fact could effectively be mobilized in abstract space, while securing adequate policing of the physical space occupied by local experimental communities.

5 This has often been noted by historians dealing with widely differing settings; see, for example, Daniels, "The Pure-Science Ideal and Democratic Culture"; Erzah, "Science and the Problem of Authority in Democracy"; Fries, "The Ideology of Science during the Nixon Years"; Gillispie, "The Encyclopaedia and the Jacobin Philosophy of Science."

For Hobbes, the activity of the philosopher was not bounded: there was no cultural space where knowledge could be had where the philosopher should not go.6 The methods of the natural philosopher were, in crucial respects, identical to those of the civic philosopher, just as the purpose of each was the same: the achievement and protection of public peace. Hobbes's own career was a token of the philosophical enterprise so conceived. For Boyle and his colleagues, the topography of culture looked different. Their cultural terrain was vividly marked out with boundary-stones and warning notices. Most importantly, the experimental study of nature was to be visibly withdrawn from "humane affairs." The experimentalists were not to "meddle with" affairs of "church and state." The study of nature occupied a quite different space from the study of men and their affairs: objects and subjects would not and could not be treated as part of the same philosophical enterprise. By erecting such boundaries, the experimentalists thought to create a quiet and a moral space for the natural philosopher: "civil war" within their ranks would be avoided by observing these boundaries and the conventions of discourse within them. They would not speak of that which could not be mobilized into a matter of fact by the conventionally agreed patterns of community activity—thus the importance of legislation against speech about entities that would not be made sensible: either those that indisputably did exist (e.g., God and immaterial spirits) or those that probably did not (e.g., the aether). As a practical matter, Hobbes could hardly deny that the experimentalists had established a community with some politically important characteristics: a community whose members endeavoured to avoid metaphysical talk and causal inquiry, and which displayed many of the attributes of internal peace. But this community was not a society of philosophers. In abandoning the philosophical quest, such a group was contributing to civil disorder. It was the philosopher's task to secure public peace; this he could only do by rejecting the boundaries the experimentalists proposed between the study of nature and the study of men and their affairs.

The politics that regulated transactions between the philosophical community and the state was important, for it acted to characterize and to protect the knowledge the philosopher produced.

6 According to Hobbes, men "cannot have any idea of [God] in their mind, answerable to his nature" ("Leviathan," p. 92), and, for that reason, theology was explicitly excluded from the philosophical enterprise ("Concerning Body," p. 10).
The experimental polity was said to be composed of free men, freely acting, faithfully delivering what they witnessed and sincerely believed to be the case. It was a community whose freedom was responsibly used and which publicly displayed its capacity for self-discipline. Such freedom was safe. Even disputes within the community could be pointed to as models for innocuous and managed conflict. Moreover, such free action was said to be requisite for the production and protection of objective knowledge. Interference with this form of life and you will interfere with the capacity of knowledge to mirror reality. Mastery, authority, and the exercise of arbitrary power all acted to distort legitimate philosophical knowledge. By contrast, Hobbes proposed that philosophers should have masters who enforced peace among them and who laid down the principles of their activity. Such mastery did not corrode philosophical authenticity. The Hobbesian form of life was not, after all, predicated upon a model of men as free-acting, witnessing, and believing individuals. Hobbesian man differed from Boylean man precisely in the latter's possession of free will and in the role of that will in constituting knowledge. Hobbesian philosophy did not seek the foundations of knowledge in witnessed and testified matters of fact: one did not ground philosophy in "dreams." We see that both games proposed for natural philosophers assumed a causal connection between the political structure of the philosophical community and the genuineness of the knowledge produced. Hobbes's philosophical truth was to be generated and sustained by absolutism. Boyle and his colleagues lacked a precise vocabulary for the polity they were attempting to erect. Almost all of the terms they used were highly contested in the early Restoration: "civil society," a "balance of powers," a "commonwealth." The experimental community was to be neither tyranny nor democracy. The "middle ways" were to be taken.9

Scientific activity, the scientist's role, and the scientific community have always been dependent: they exist, are valued, and supported insofar as the state or its various agencies see point in them. What sustained the experimental space that was created in the mid-seventeenth century? The nascent laboratory of the Royal Society and other experimental spaces were producing things that were widely wanted in Restoration society. These wants did not simply preexist, waiting to be met; they were actively cultivated by the experimen-

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9 The phrase is Hooke's: ibid., sig b1 v, similar locutions typify much Royal Society publicity.
talists. The experimentalists' task was to show others that their problems could be solved if they came to the experimental philosopher and to the space he occupied in Restoration culture. The experimentalists could effectively cultivate and satisfy these wants, the legitimacy of experimental activity and the integrity of laboratory and scientific role would be ensured. The wants addressed by the experimental community spread across Restoration economic, political, religious, and cultural activity. Did gunners want their artillery pieces to fire more accurately? Then they should bring their practical problems to the physicists of the Royal Society. Did brewers want a more reliable ale? Then they should come to the chemists. Did physicians want a theoretical framework for the explanation and treatment of fever? Then they should inspect the wares of the mechanical philosopher. The experimental laboratory was advertised as a place where practically useful knowledge was produced. But the laboratory could also supply solutions to less tangible problems. Did theologians desire facts and schemata that could be deployed to convince otherwise obdurate men of the existence and attributes of the Deity? They, too, should come to the laboratory where their wants would be satisfied. Through the eighteenth century one of the most important justifications for the natural philosopher's role was the spectacular display of God's power in nature. Theologians could come to the place where the Leyden jar operated if they wanted to show cynics the reality of God's majesty; natural theologians could come to the astronomer's observatory if they wanted evidence of God's wise and regular arrangements for the order of nature; moralists could come to the natural historian if they wanted socially usable patterns of natural hierarchy, order, and the due submission of ranks. The scientific role could be institutionalized and the scientific community could be legitimized insofar as the experimental space became a place where this multiplicity of interests was addressed, acquired, and drawn together. One of the more remarkable features of the early experimental programme was the intensity with which its proponents worked to publicize experimental spaces as useful: to identify problems in Restoration society to which the work of the experimental philosopher could provide the solutions.

There was another desideratum the experimental community sought to mobilize and satisfy in Restoration society. The experimental philosopher could be made to provide a model of the moral citizen, and the experimental community could be constituted as a model of the ideal polity. Publicists of the early Royal Society stressed that theirs was a community in which free discourse did not breed dispute, scandal, or civil war; a community that aimed at peace and had found out the methods for effectively generating and maintaining consensus; a community without arbitrary authority that had learnt to order itself. The experimental philosophers aimed to show those who looked at their community an idealized reflection of the Restoration settlement. Here was a functioning example of how to organize and sustain a peaceable society between the extremes of tyranny and radical individualism. Did civic philosophers and political actors wish to construct such a society? Then they should come to the laboratory to see how it worked.

This book has been concerned with the identification of alternative philosophical forms of life, with the display of their conventional bases, and with the analysis of what hinged upon the choice between them. We have not taken as one of our questions, "Why did Boyle win?" Obviously, many aspects of the programme he recommended continue to characterize modern scientific activity and philosophies of scientific method. Yet, an unbroken continuum between Boyle's interventions and twentieth-century science is highly unlikely. For example, the relationship between Boyle's experimental programme and Newton's "mathematical way" is yet to be fully explored. Nevertheless, modern historians who find in Boyle the "founder" of truly modern science can point to similar sentiments among late seventeenth-century and eighteenth-century commentators. Despite these qualifications the general form of an answer to the question of Boyle's "success" begins to emerge, and it takes a satisfyingly historical form. This experimental form of life achieved local success to the extent that the Restoration settlement was secured. Indeed, it was one of the important elements in that security.

Insofar as we have displayed the political status of solutions to problems of knowledge, we have not referred to politics as some-

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10 For this section we are deeply indebted to recent work by Bruno Latour, especially his "Give Me a Laboratory" and Les microbes: guerre et paix.

11 From the best modern historical research it now appears that none of the utilitarian promissory notes could be, or were, cashed in the seventeenth century; see Westfall, "Hooke, Mechanical Technology, and Scientific Investigation"; A. R. Hall, "Gunnery, Science, and the Royal Society." If science did not deliver technological utility, it becomes even more important to ask about its other perceived values, including social, political, and religious uses.

12 See particularly Schaffer, "Natural Philosophy"; idem, "Natural Philosophy and Public Spectacle."
thing that happens solely outside of science and which can, so to speak, press in upon it. The experimental community vigorously developed and deployed such boundary-speech, and we have sought to situate this speech historically and to explain why these conventionalized ways of talking developed. What we cannot do if we want to be serious about the historical nature of our inquiry is to use such actors’ speech unthinkingly as an explanatory resource.

The language that transports politics outside of science is precisely what we need to understand and explain. We find ourselves standing against much current sentiment in the history of science that holds that we should have less talk of the “insides” and “outsides” of science, that we have transcended such outmoded categories. Far from it; we have not yet begun to understand the issues involved. We still need to understand how such boundary-conventions developed: how, as a matter of historical record, scientific actors allocated items with respect to their boundaries (not ours), and how, as a matter of record, they behaved with respect to the items thus allocated. Nor should we take any one system of boundaries as belonging self-evidently to the thing that is called “science.”

We have had three things to connect: (1) the polity of the intellectual community; (2) the solution to the practical problem of making and justifying knowledge; and (3) the polity of the wider society. We have made three connections: we have attempted to show (1) that the solution to the problem of knowledge is political; it is predicated upon laying down rules and conventions of relations between men in the intellectual polity; (2) that the knowledge thus produced and authenticated becomes an element in political action in the wider polity; it is impossible that we should come to understand the nature of political action in the state without referring to the products of the intellectual polity; (3) that the contest among alternative forms of life and their characteristic forms of intellectual product depends upon the political success of the various candidates in insinuating themselves into the activities of other institutions and other interest groups. He who has the most, and the most powerful, allies wins.

We have sought to establish that what the Restoration polity and experimental science had in common was a form of life. The practices involved in the generation and justification of proper knowledge were part of the settlement and protection of a certain kind of social order. Other intellectual practices were condemned and rejected because they were judged inappropriate or dangerous to the polity that emerged in the Restoration. It is, of course, far from original to notice an intimate and an important relationship between the form of life of experimental natural science and the political forms of liberal and pluralistic societies. During the Second World War, when liberal society in the West was undergoing its most virulent challenge, that perception was formed into part of the problematic of the academic study of science. What sort of society is able to sustain legitimate and authentic science? And what contribution does scientific knowledge make to the maintenance of liberal society? The answer then given was unambiguous: an open and liberal society was the natural habitat of science, taken as the quest for objective knowledge. Such knowledge, in turn, constituted one of the sureties for the continuance of open and liberal society. Interfere with the one, and you will erode the other.

Now we live in a less certain age. We are no longer so sure that traditional characterizations of how science proceeds adequately describe its reality, just as we have come increasingly to doubt whether liberal rhetoric corresponds to the real nature of the society in which we now live. Our present-day problems of defining our knowledge, our society, and the relationships between them centre on the same dichotomies between the public and the private, between authority and expertise, that structured the disputes we have examined in this book. We regard our scientific knowledge as open and accessible in principle, but the public does not understand it. Scientific journals are in our public libraries, but they are written in a language alien to the citizenry. We say that our laboratories constitute some of our most open professional spaces, yet the public does not enter them. Our society is said to be democratic, but the public cannot call to account what they cannot comprehend. A form of knowledge that is the most open in principle has become the most closed in practice. To entertain these doubts about our science is to question the constitution of our society. It is no wonder that scientific knowledge is so difficult to hold up to scrutiny.

In this book we have examined the origins of a relationship between our knowledge and our polity that has, in its fundamentals, lasted for three centuries. The past offers resources for understanding the present, but not, we think, for foretelling the future. Nevertheless, we can venture one prediction as highly probable.

**15 Merton, The Sociology of Science, chaps. 12-15; Needham, The Grand Tiptation; Zilsel, Die sozialen Ursprünge der neuesten Wissenschaft.**
The form of life in which we make our scientific knowledge will stand or fall with the way we order our affairs in the state.

We have written about a period in which the nature of knowledge, the nature of the polity, and the nature of the relationships between them were matters for wide-ranging and practical debate. A new social order emerged together with the rejection of an old intellectual order. In the late eighteenth century that settlement is, in turn, being called into serious question. Neither our scientific knowledge, nor the constitution of our society, nor traditional statements about the connections between our society and our knowledge are taken for granted any longer. 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. Knowledge, as much as the state, is the product of human actions. Hobbes was right.

**Appendix**

**Hobbes's Physical Dialogue (1661)**

**Translated by Simon Schaffer**

This is a virtually complete translation of Thomas Hobbes’s response to Boyle’s *New Experiments Physico-Mechanical* of 1660. To our knowledge this is the first translation from the original Latin. Two editions of the *Dialogus physicus* appeared in Hobbes’s lifetime. The first was published in London in August 1661 by Andrew Crooke; the other was included as the sixth part (separately paginated) of the 1668 Amsterdam edition of Hobbes’s *Opera philosophica*, published by Johan Blaeu. In Molesworth’s *Latin Works of 1839-1845*, the *Dialogus physicus* appears as pp. 233-296 of volume IV. Molesworth pages are indicated in the margins; page breaks are signalled by a stroke in the text. The accompanying item, *De duplicatione cubi*, is not translated.

Differences between 1661 and 1668 editions were slight. Most differences were grammatical and were reconciled by Molesworth, whose transcription is quite accurate. A few substantive differences between earlier and later editions, especially in the dedication to Sorbière, are indicated in the translation.

The annotations to this translation point out those passages that correspond or relate to passages in *De corpore* (1655, 1656), *Problemata physica* (1662), and *Decameron physiologicum* (1678). These are especially frequent in the later sections of the *Dialogus* on heat and hydrostatics.

Certain Latin terms are indicated where the original has particular significance. *Conatus* is consistently rendered as “endeavour,” following standard usage in Hobbes’s own versions of his work. *Aer purus* and *antitupia* (pure air and resistance or spring) are technical terms whose significance is discussed in chapters 4 and 5. *Pondus* (weight) is always distinguished from *gravitas* (gravity), since Hobbes makes a distinction between the two in *Dialogus physicus* (although this is by no means consistent elsewhere). *Experientia* is rendered “experience” and distinguished from *experimentum* (experiment), though this distinction was not common in the period. Hobbes makes liberal use of the verb *supponere* (to suppose): this is mainly used as an axiom of demonstration that may have no veracity, contrasted to