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LEVIATHAN AND THE AIR-PUMP

HOBBES, BOYLE, AND THE EXPERIMENTAL LIFE

INCLUDING A TRANSLATION OF THOMAS HOBBES,

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*.[1] 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.

method."[2] 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.[3] 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.[4] 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

[2] 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 few more reliable ways of being expelled from a culture than continuing seriously to query its taken-for-granted intellectual framework.[5] 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."[6] If we pretend to be a stranger to experimental culture, we can seek to appropriate one great advantage the stranger has over the member in explaining the beliefs and practices of a specific culture: the stranger is in a position to *know* that there are alternatives to those beliefs and practices.[7] 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

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

[6] Schutz, *Collected Papers, Vol. II*, p. 104.

[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.[8] 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."[9] 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.[10] 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.[11] 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.[12] 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.

[10] Shepherd, "Newtonianism in Scottish Universities," esp. p. 70; idem, *Philosophy and Science in the Scottish Universities*, pp. 8, 116, 153, 167, 215-217.

[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."

[12] Kargon, *Atomism in England*, p. 54.

apparent than in classical history of science.[13] 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.[14]

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),[15] 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).[16] 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.[17] True, Hobbes's *Dialogus*

[13] 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, "Hobbes," esp. pp. 549-550 (cf. idem, *Hobbes*, pp. 160-185); A. E. Taylor, *Hobbes*, esp. 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, "Hobbes: Geometrical Objects"; idem, "Hobbes: The Art of the Geometricians"; 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 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, 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]

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.

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 splenetic 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-

18 Brandt, *Hobbes' Mechanical Conception*, pp. 377-378.

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.

23 Peters, *Hobbes*, p. 40.

24 R. F. Jones, *Ancients and Moderns*, p. 128; de Beer, "Some Letters of Hobbes," p. 197: Hobbes "failed to appreciate . . . the paramount value of experiment in deciding any question of natural philosophy."

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,"[25] 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."[26]

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."[27] M. A. Stewart refers to Boyle's pneumatics as leading "Hobbes into ill-advised controversy on matters he did not understand."[28] 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.[29] (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 because 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.[30] 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

[25] M. B. Hall, "Boyle," p. 379. Her *Boyle and Seventeenth-Century Chemistry* makes no mention of the Boyle-Hobbes disputes; cf. Burtt, *Metaphysical Foundations of Modern Science*, p. 169.

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

[27] Conant, "Boyle's Experiments in Pneumatics," p. 49.

[28] Stewart, "Introduction," p. xvi. Hobbes's "misunderstanding" of Boyle even creeps into accounts written for young people; see Kuslan and Stone, *Boyle: The Great Experimenter*, p. 26.

[29] Stephen, "Hobbes," p. 937; Robertson, "Hobbes," p. 552.

[30] Gellner, "Concepts and Society"; cf. Collins, "Son of Seven Sexes," pp. 52-54.

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.[31] 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

[31] For examples of empirical studies which assess method-statements in these terms, see P. B. Wood, "Methodology and Apologetics"; Miller, "Method and the 'Micropolitics' of Science"; Yeo, "Scientific Method and the Image of Science."

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.[32] 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.[33] 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

[32] 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.

[33] 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

[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*.

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,

[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 Incompleat 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 resources to the sociologically inclined historian of science include Baxandall's *Painting and Experience*, his *Limewood Sculptors of Renaissance Germany*, and Edgerton's *The Renaissance Discovery of Linear Perspective*.

[37] Alpers, *The Art of Describing*, pp. 45-46. Alpers alludes to Rorty's important survey of the development of mirror theories of knowledge: *Philosophy and the Mirror of Nature*, esp. chap. 3.

and the art of the experimentalist, was, therefore, to make representations 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 warranting knowledge. Secondly, Alpers adopts what we have termed 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 craft 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 conventions 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 consensus. 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 discuss the social and linguistic practices Boyle recommended to experimentalists; 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 a2^{v}).

and conflict. Our task here is to identify the conventions by which experimental knowledge was to be produced.

In chapter 3 we discuss the state and objects of Hobbes's natural philosophy before the publication of Boyle's *New Experiments* of 1660. Our major object here is to read *Leviathan* (1651) as *natural* philosophy and as epistemology. As a treatise in civic philosophy *Leviathan* was designed to show the practices that would guarantee order in the state. That order could be, and during the Civil War was being, threatened by clerical intellectuals who arrogated to themselves a share of civic authority to which they were not entitled. Their major resources in these acts of usurpation were, according to Hobbes, a false ontology and a false epistemology. Hobbes endeavoured to show the absurdity of an ontology that posited incorporeal substances and immaterial spirits. Thus, he built a *plenist* ontology, and, in the process, erected a materialistic theory of knowledge in which the foundations of knowledge were notions of *causes*, and those causes were matter and motion. An enterprise entitled to the name of philosophy was causal in nature. It modelled itself on the demonstrative enterprises of geometry and civic philosophy. And, crucially, it produced assent through its demonstrative character. Assent was to be total and it was to be enforced.

Hobbes's philosophy, both in *Leviathan* and in *De corpore* (1655) was already in place when Boyle's experimental programme became public in the year of the Restoration. He immediately replied to Boyle's radical proposals. The analysis of Hobbes's *Dialogus physicus* forms the framework for chapter 4. In this text, Hobbes attempted to explode Boyle's experimentalism on several grounds: he argued that Boyle's air-pump lacked physical integrity (it leaked) and that, therefore, its putative matters of fact were not facts at all; he used the leakage of the pump to offer an alternative physical explanation of Boyle's findings. The pump, far from being an operational vacuum, was always full of a fraction of atmospheric air. Plenist accounts of the pump were superior to Boyle's, and Hobbes attacked Boyle as a vacuist despite the latter's professions of nescience on the vacuist-plenist debates of the past. Of greater epistemological importance was Hobbes's attack on the generation of matters of fact, the constitution of such facts into the consensual foundations of knowledge, and Boyle's segregation of facts from the physical causes that might account for them. These attacks amounted to the assertion that, whatever Boyle's experimental programme was, it was not *philosophy*. Philosophy was a causal enterprise and, as such, secured a total and irrevocable assent, not the partial assent at which Boyle aimed. Hobbes's assault identified the conventional nature of experimental facts.

In chapter 5 we show how Boyle replied to Hobbes and to two other adversaries in the 1660s: the Jesuit Franciscus Linus and the Cambridge Platonist Henry More. By examining the different nature and style of Boyle's responses, we identify that which Boyle was most concerned to protect: the air-pump as a means of generating legitimate philosophical knowledge and the integrity of the rules that were to regulate the moral life of the experimental community. Boyle treated Hobbes as a failed experimentalist rather than as someone proposing a quite different way of constructing philosophical knowledge. He used the opportunities provided by all three adversaries to exhibit how experimental controversy could be managed, without destroying the experimental enterprise itself—indeed, to show how controversy could be used to buttress the factual foundations of experimental knowledge.

In chapters 2, 4, and 5 we discuss the central role of the air-pump in the experimental programme and how critics might use imperfections in its working to attack experiment itself. In chapter 6 we attempt to do two things. First, we look at how the pump itself evolved as a material object in the 1660s, arguing that these changes embodied responses to earlier criticisms, especially those offered by Hobbes. We uncover information about the small number of pumps that were successfully built in that decade, and we show that, despite Boyle's reporting practices, no one was able to build a pump and make it operate without seeing the original. This poses problems of *replication* of greater interest than historians have previously recognized. Replication is also central to the second task of this chapter. In chapter 2 we argue that the constitution of matters of fact involved the multiplication of witnesses, and that Boyle exerted himself to encourage the reiteration of his experiments. However, shortly after the *New Experiments* appeared, another philosopher, Christiaan Huygens in the Netherlands, produced a finding (the so-called anomalous suspension of water) that seemed to invalidate one of the most important of Boyle's explanatory resources. We examine how this important anomaly was treated, and we conclude that the successful working of the air-pump was calibrated by previous commitments to whether or not such a phenomenon could exist. We analyze response to anomaly as a manifestation of the experimental form of life and of the conventions employed in the experimental community to protect itself from fatal internal discord.

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.