STEVEN

LEVIATHA

HOBBES, BOYLE

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January 1985
Ardhuckwell, Derbyshire

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William of Baskerville

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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.

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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, an 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. 273–274, argues that only by becoming a competent member of the community under study can one reliably test one's understanding.

along with Descartes and Gassendi."[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. 501; idem, *Philosophy and Science in the Scottish Universities*, pp. 8, 116, 259, 289, 313–317.

[11] Anon., "Hobbes"; Mackintosh, "Dissertation Second," pp. 310–383 (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.

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[10] Stephen, "Hobbes," esp
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[11] For the *horror vacui* claim
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[12] Watkins, *Hobbes's System*,
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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,[10]

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 experimental rituals 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 translation of physical science ignored" (p. 344). 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) as 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, Sprigge, *The Politics of Mirth*, esp. chap. 3; Reik, *The Golden Leads 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.

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[10] Brandt, *Hobbes' Mechanis*

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[11] L. T. More, *Life of Boyle*,
109) has even less to say abou

[7] McKie, "Introduction," p

[8] Laird, *Hobbes*, p. 117.

[6] Peters, *Hobbes*, p. 40.

[5] F. Jones, *Ancients and*
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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,"[34] 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."[35]

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."[36] M. A. Stewart refers to Boyle's pneumatics as leading "Hobbes into ill advised controversy on matters he did not understand."[37] Leslie Stephen and Groom 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.[38] (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-

[34] 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.

[35] B. Shapiro, *Probability and Certainty*, p. 79; cf. p. 68.

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

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

[38] Stephen, "Hobbes," p. 197; Robertson, "Hobbes," p. 565.

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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 created 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.[9] 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 regularizing social interaction within the scientific community. To this end, we

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

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.[14]

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

[14] 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*.

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[14] Alpers, *The Art of Describing*
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[15] Alpers, *The Art of Describing*
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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."[28] 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

[28] Alpers, *The Art of Describing*, pp. 72–73 (quoting Robert Hooke's *Micrographia* [1665], sig a2r).

partial assent at which Boyle aimed. Hobbes's assault identified the conventional nature of experimental facts.

In chapter 3 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 5 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.

· II ·

Seeing and Believing: The Experimental Production of Pneumatic Facts

. . . Facts are chiels that winna ding,
An' dauma be disputed.
Robert Burns, A Dream

Robert Boyle maintained that proper natural philosophical knowledge should be generated through experiment and that the foundations of such knowledge were to be constituted by experimentally produced matters of fact. Thomas Hobbes disagreed. In Hobbes's view Boyle's procedures could never yield the degree of certainty requisite in any enterprise worthy of being called philosophical. This book is about that dispute and about the issues that were seen to depend upon its resolution.

Hobbes's position has the historical appeal of the exotic. How was it possible for any rational man to deny the value of experiment and the foundational status of the matter of fact? By contrast, Boyle's programme appears to exude the banality of the self-evident. How could any rational man think otherwise? In this chapter we intend to address the problem of self-evidence by dissecting and displaying the mechanisms by which Boyle's experimental procedures were held to produce knowledge and, in particular, the variety of knowledge called "matters of fact." We will show that the experimental production of matters of fact involved an immense amount of labour, that it rested upon the acceptance of certain social and discursive conventions, and that it depended upon the production and protection of a special form of social organization. The experimental programme was, in Wittgenstein's phrases, a "language-game" and a "form of life." The acceptance or rejection of that programme amounted to the acceptance or rejection of the form of life that Boyle and his colleagues proposed. Once this point is made, neither the acceptance of the experimental programme nor the epistemological status of the matter of fact ought to appear self-evident.

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For a discussion of the his
edge and the rise of philosop
pp. 1ff.

• Hacking, *The Emergence of*
Certainty, esp. chap. 2.

demonstrative sciences and to attain to the kind of certainty that compelled absolute assent. By contrast, English experimentalists of the mid-seventeenth century and afterwards increasingly took the view that all that could be expected of physical knowledge was "probability," thus breaking down the radical distinction between "knowledge" and "opinion." Physical hypotheses were provisional and revisable; assent to them was not obligatory, as it was to mathematical demonstrations; and physical science was, to varying degrees, removed from the realm of the demonstrative. The probabilistic conception of physical knowledge was not regarded by its proponents as a regrettable retreat from more ambitious goals; it was relebrated as a wise rejection of a failed project. By the adoption of a probabilistic view of knowledge one could attain to an *appropriate* certainty and aim to secure *legitimate* assent to knowledge-claims. The quest for necessary and universal assent to physical propositions was seen as inappropriate and illegitimate. It belonged to a "dogmatic" enterprise, and dogmatism was seen not only as a failure but as dangerous to genuine knowledge.

If universal and necessary assent was not to be expected of explanatory constructs in science, how then was proper science to be *founded*? Boyle and the experimentalists offered the matter of fact as the foundation of proper knowledge. In the system of physical knowledge the fact was the item about which one could have the highest degree of probabilistic assurance: "moral certainty." A crucial boundary was constructed around the domain of the factual, separating matters of fact from those items that might be otherwise and about which absolute, permanent, and even "moral" certainty should not be expected.[1] In the root metaphor of the mechanical philosophy, nature was like a clock: man could be certain of the hour shown by its hands, of natural effects, but the mechanism by which those effects were really produced, the clockwork, might be various.[2] In this chapter we shall examine the means by which the experimental matter of fact was produced.

[1] The usual form in which Boyle phrased this was that God might produce the same natural effects through very different causes. Therefore, "it is a very easy mistake for men to conclude that because an effect may be produced by such determinate causes, it must be so, or actually is so." Boyle, "Usefulness of Experimental Natural Philosophy," p. 45; see also Laudan, "The Clock Metaphor and Probabilism"; Rogers, "Descartes and the Method of English Science"; van Leeuwen, *The Problem of Certainty*, pp. 55–91; B. Shapiro, *Probability and Certainty*, pp. 44–61.

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[1] Our use of the word *technol*
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esp. pp. 172–175. Mitcham dem
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as well as to machines, we wish

tended an experience that was regarded as essential to the propagation of the material technology or even as a valid substitute for direct witness of experimental displays. If we wish to understand how Boyle worked to construct scientific facts, we must consider how each of the three technologies was used and how each bore upon the others.[27]

The Material Technology of the Air-Pump

We start by noting the obvious: matters of fact in Boyle's new pneumatics were machine-made. His mechanical philosophy used the machine not merely as an ontological metaphor but also, crucially, as a means of intellectual production. The matters of fact that constituted the foundations of the new science were brought into being by a purpose-built scientific machine. This was the air-pump (or "pneumatical engine," or, eponymously, the *machina Boyleana*), which was constructed for Boyle by the instrument maker Greatorex and, especially, by Robert Hooke in 1658-1659. We have to describe how this machine was put together and how it worked in order to understand its role in fact-production.

Boyle intended to improve upon the design of Otto von Guericke's device, described by Caspar Schott in his *Mechanica hydraulico-pneumatica* of 1657. According to Boyle, this earlier machine (see figure 22) had several practical disadvantages: (1) it needed to be immersed in a large volume of water; (2) it was a solid vessel, such that experimental apparatus could not be inserted in it; and (3) it was extremely difficult to operate, requiring, as Boyle observed, "the continual labour of two strong men for divers hours" to evacuate it.[28] Boyle and Hooke sought to overcome these practical problems. Figure 1 is an engraving of their first successful machine, that was used to produce the forty-three experiments of *New Experiments Physico-Mechanical*.[29] The machine consisted of two main parts: a glass globe (or "receiver") and the pumping apparatus itself.

28 Boyle, "New Experiments," pp. 6-7. (Many of Boyle's essay titles began with "New Experiments . . ."; we use this short title to refer exclusively to the "New Experiments Physico-Mechanical, touching the Spring of the Air" [1660].)

29 This account is drawn largely from that provided by Boyle in "New Experiments," pp. 6-11. One of the best modern descriptions of this pump and its operation is Frank, *Harvey and the Oxford Physiologists*, pp. 129-130. The best technical accounts remain the nineteenth-century essays of Wilson: *Religio chemici*, pp. 186-219; and, especially, his "Early History of the Air-Pump."

Robert Boyle's first air pump, ...
Physico-Mechanical (16...

The receiver contained ...
to be removed. It was ...
though Boyle would ...
limit of his "glass men's ...
Boyle used a variety of ...
in volume, hoping (wh...
be easier to evacuate. ...

Boyle, "New Experiments ...

the receiver through an aperture of about four-inch diameter at the top ("B-C"), and special arrangements could be made for instruments, like the Torricellian experiment, which were taller than even the big receiver, in which cases part of the apparatus extended through the sealed aperture above the receiver.

The receiver narrowed at its base so as to fit into a brass device ("N") containing a stopcock ("S"). This in turn was connected to a hollow brass cylinder ("3") about 14 inches long and about three inches in internal diameter. At the upper lip of the cylinder there was a small hole into which a brass valve ("R") could be inserted as required. Within the cylinder was a wooden piston (or "sucker") topped with "a good thick piece of tanned shew-leather" ("4"), which provided for an exceedingly tight fit between piston and the inside of the cylinder. The piston was worked up and down by means of an iron rack ("5") and pinion ("7") device, the whole machine resting upon a wooden frame ("F").

This is how the engine worked to remove air from the receiver: with the stopcock in the closed position and the valve "R" inserted, the sucker was drawn up to the top of the cylinder; at this point there was no air between sucker and the top of the cylinder. Then the sucker was drawn down and the stopcock was opened, permitting the passage of a quantity of air from the receiver into the cylinder. The stopcock was closed, the valve was removed, and the sucker was forced up, thus expelling that quantity of air to the exterior. The process was repeated, each "exsuction" requiring progressively more force as the amount of air remaining in the receiver was diminished. (This account of how the machine worked to *remove* air, it must be noted, agrees with that provided by Boyle and modern commentators. As we shall see, Hobbes claimed that the receiver remained always full; therefore his view of how the pump operated, to be detailed in chapter 4, differed radically from Boyle's.) Later air-pumps of the 1660s and 1670s (described in chapters 5 and 6) differed from this original design in several respects: the cylinder and receiver were indirectly connected, and, after Denis Papin's innovation of 1676, there were two pumping cylinders with self-acting valves. Although we shall be almost exclusively concerned here with Boyle's air-pump as a rarefying engine, it could also be used to condense air in the receiver, simply by reversing the operations by which air was withdrawn.[8]

[8] As noted, for example, by Wilson, *Robert Boyle*, pp. 197–198, and see Boyle, "New Experiments," p. 56

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[9] Boyle, "New Experiments
diachylon was somewhat
[10] Ibid., p. 9.
[11] Ibid., p. 56.

the cylinder. The reasons for our detailed treatment of the physical integrity of the air-pump and the steps Boyle took to guarantee it will become clear below. For the present, we simply note three points: (1) that both the engine's integrity and its limited leakage were important resources for Boyle in validating his pneumatic findings and their proper interpretation; (2) that the physical integrity of the machine was vital to the perceived integrity of the knowledge the machine helped to produce; and (3) that the lack of its physical integrity was a strategy used by critics, particularly Hobbes, to deconstruct Boyle's claims and to substitute alternative accounts.

The Air-Pump as Emblem

Boyle's machine was a powerful emblem of a new and powerful practice. As Rupert Hall has noted:

> The air-pump was the unfailing pièce de résistance of the incipient scientific laboratory. Its wonders were inevitably displayed whenever a grandee graced a scientific assembly with his presence. After the chemist's furnace and distillation apparatus it was the first large and expensive piece of equipment to be used in experimental practice.

It was "the cyclotron of its age."[18] Similarly, Marie Boas Hall:

> . . . Boyle's air-pump together with Hooke's microscope constituted the show pieces of the [Royal] Society; when distinguished visitors were to be entertained, the chief exhibits were always experiments with the pump.[19]

As early as February 1661 the Danish ambassador "was entertained with experiments on Mr. Boyle's air-pump," and in 1667 Margaret Cavendish, Duchess of Newcastle, probably the first woman to be admitted to a meeting of the Royal Society, was treated to a similar display. According to Pepys, Margaret "was full of admiration, all

[18] A. R. Hall, *From Galileo to Newton*, p. 254, and idem, *The Revolution in Science*, p. 262; see also Price, "The Manufacture of Scientific Instruments," p. 636: the pneumatic pump "was the first large and complex machine to come into the laboratory."

[19] M. B. Hall, *Boyle and Seventeenth Century Chemistry*, p. 185.

admiration."[20] When i
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[20] The visit of the Danish a
that of Margaret in ibid., pp
vol. viii, pp. 242–243 (entry fo
and the New Science, chap. 3. M
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Boas," p. 4 (also sig. d1r ...
rationalizing minds, which ...
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the perception of clear and re...
p. 270).

[21] When to Brouncker, 30
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May 1664, but we have no ev...
ton's place; see also Oldenbur...
ence, vol. ii, pp. 78–79. At pre...

No new device had taken the place of the *machina Boyleana* as an emblem of the Royal Society's experimental programme.

The powerfully emblematic status of the air-pump is manifested in its contemporary iconography. Boyle and Hooke took an active interest in the production of drawings and engravings by William Faithorne that depicted Boyle together with his pneumatic engine (see figure 16b).[16] During the mid 1660s the Somerset virtuoso John Beale was sedulously involved in celebrating the Baconian works of the Royal Society, encouraging John Evelyn to produce an appropriate iconographic drawing which, after various vicissitudes, eventually appeared as a frontispiece in some copies of Sprat's *History of the Royal Society* (1667) (see figure 2).[17] This engraving (by Wenceslaus Hollar) shows a redesigned version of Boyle's pump in the left background. (See figure 17 for an enlargement.) Through the later seventeenth and eighteenth centuries the Faithorne image was continually adapted and modified. Perhaps the richest in iconographic significance eventually appeared on the title page of the collected editions of Boyle's *Works* in 1744 and 1772 (figure 3).[18] This vignette by Hubert François Gravelot Bourguignon incorporated the Faithorne likenesses of Boyle and his original pump. The power of the pump is indicated by the conjunction of the Latin motto and the gesture of the classical female figure. Her left hand points to the air-pump while her right points to the heavens. The significance of the gesture is reinforced by the motto: "To know the Supreme Cause from the causes of things." It is the operation of the pneumatic engine, among all the scientific apparatus displayed in the engraving, that is going to enable the philosopher to approach God's knowledge.[19] The ex-

was using similar language about "jugglers" and royal displays: "The works of God are not like the tricks of jugglers, or the pageants that entertain princes, where concealment is requisite to wonder; but the knowledge of the works of God proportions our admiration of them." Boyle, "Usefulness of Experimental Natural Philosophy," p. 30 (1663).

16 For a full account of seventeenth- and eighteenth-century images of Boyle, see Maddison, "The Portraiture of Boyle." For correspondence relating to the Faithorne work, see Boyle, *Works*, vol. vi, pp. 498, 499, 501, 502, 503.

17 A detailed treatment of the circumstances attending the production of this image is in Hunter, *Science and Society*, pp. 194–197.

18 See Maddison, "The Portraiture of Boyle," p. 159.

19 Such a motto might have been regarded as inappropriate by many mid-seventeenth-century experimental philosophers: its apparently immodest sentiments seem to belong more to the mid-eighteenth century. Boyle agreed that one could move in understanding "from Nature up to Nature's God," yet we shall see that he set strict limits on the possibilities of causal knowledge.

Frontispiece to Sprat's Histo…
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1667, and transferred to Spr…
is in the centre left backgr…
foreground are the president…
(bust centre, being crowned …

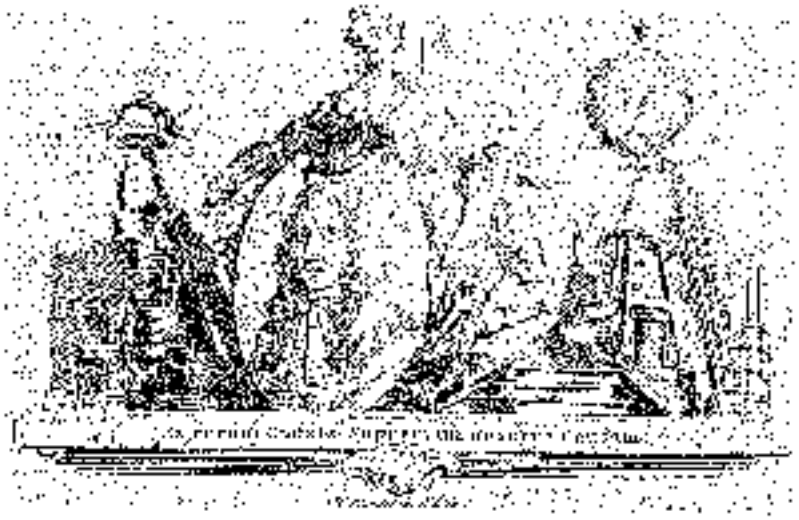

FIGURE 5
Vignette by Robert François Gaucher after Bourguignon for Thomas Birch's edition of Boyle's Works (1744 and 1772), frontispiece to vol. 1. (Courtesy of Edinburgh University Library.)

Dorship of the pump is further symbolized by the line from the heaven-pointing hand to Boyle himself. Note further the spatial separation of the various items of philosophical instrumentation. On the right are instruments for experimenting on the nature of the air. The pump, a two-branch mercury barometer (leaning on the pump), and a double capillary manometer. All these are modern experimental devices, just as Boyle's pneumatics was paradigmatic of modern experimental philosophy. On the left are instruments for experimenting with fire: notably a furnace with an alembic. All these are medieval in origin, being the apparatus employed by alchemists and practitioners of the old philosophy. The female figure faces away from these, indicating not Boyle's rejection of these (since he employed them himself) but the relative value of the two programmes and their resulting intellectual products. Furthermore, those products take the form of *writings*, and the figure's feet rest upon a pile of books (the embodiment of the quest for knowledge) that belong to the assemblage of pneumatic instruments. There are no books on the left.[29] Some indication that the

[29] It is, of course, possible that our interpretation of this image is incorrect; but it is unlikely that, in its general form, it is overambitious. An immense amount of