STEVEN SHAPIN AND SIMON SCHAFFER

LEVIATHAN AND THE AIR-PUMP

HOBBES, BOYLE, AND THE EXPERIMENTAL LIFE

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INCLUDING A TRANSLATION OF THOMAS HOBBES,

DIALOGUS PHYSICUS DE NATURA AERIS,

BY SIMON SCHAFFER

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For our Parents

Every afternoon Father Nicanor would sit by the chestnut tree preaching in Latin, but José Arcadio Buendía insisted on rejecting rhetorical tricks and the transmutation of chocolate, and he demanded the daguerreotype of God as the only proof. Father Nicanor then brought him medals and pictures and even a reproduction of the Veronica, but José Arcadio Buendía rejected them as artistic objects without any scientific basis. He was so stubborn that Father Nicanor gave up his attempts at evangelization and continued visiting him out of humanitarian feelings. But then it was José Arcadio Buendía who took the lead and tried to break down the priest's faith with rationalist tricks. On a certain occasion when Father Nicanor brought a checker set to the chestnut tree and invited him to a game, José Arcadio Buendía would not accept, because according to him he could never understand the sense of a contest in which the two adversaries have agreed upon the rules.

GABRIEL GARCÍA MARQUEZ, *One Hundred Years of Solitude*

What a blessing to mankind, in himself and in his writings, was the ingenious, humble, and pious Mr. Boyle; what a common pest to society was the fallacious, proud, and impious Hobbes! Accordingly we find the former bad adieu to this world with the utmost serenity, honour, and hope; while the other went out of it in the dark, with an odium on his name, as well as with terrible apprehensions of an unknown future.

W. DODD, *The Beauties of History; or, Pictures of Virtue and Vice Drawn from Examples of Men, Eminent for Their Virtues or Infamous for Their Vices* (1796)

· CONTENTS ·

· ILLUSTRATIONS ·

· NOTES ON SOURCES AND CONVENTIONS ·

For citations of sources in footnotes we have adopted an economical convention similar to that employed in Elizabeth Eisenstein's *The Printing Press as an Agent of Change.* Bibliographic information is kept to a minimum in the notes, apart from the occasional addition of date of publication where that information is not given in the text and is germane. Full titles and publication details are provided in the Bibliography. Complete details of unpublished manuscript sources, seventeenth-century periodical articles, and items in state and parliamentary papers are, however, given in the notes and not repeated in the Bibliography.

We have made liberal use of correspondence and other material not published in the seventeenth century. Our major concerns have been with knowledge that was public or designed to be so, and this has affected the extent of our use of such sources. Where we are interested in material that was incompletely public or, possibly, intended to be restricted (as in chapter 6), our use of manuscript material is correspondingly greater.

During the period with which this book is concerned, the British Isles employed a calendar different from that used in most Continental countries, especially Catholic ones. The former used the Julian (old style) calendar, which was ten days behind the Gregorian (new style) calendar employed on the Continent. In addition, the British new year was reckoned to begin on 25 March. Because we deal in some detail with exchanges between England and Continental countries, we give all dates in both old and new style form, but we adjust years to correspond with a new year commencing 1 January. Thus, the English 6 March 1661 is given as 6/16 March 1662; the Dutch (who used the Gregorian calendar even though Protestant) 24 July 1664 is given as 14/24 July 1664; and so forth.

We have endeavoured, within reason, to preserve seventeenth-century orthography, punctuation, and emphases, and have dispensed with *sic* indications, save where absolutely necessary.

In our usage, "Hobbesian" refers to the beliefs and practices of Hobbes as an individual; "Hobbist" to the beliefs and practices of his real or alleged followers. We distinguish between religious Dissent (upper case) and intellectual and political dissent (lower case).

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A version of part of chapter 2 was published as "Pump and Circumstance: Robert Boyle's Literary Technology," in *Social Studies of Science* 14 (1984), 481-520. We thank Sage Publications Ltd. for permission to use this material. For permission to quote from manuscripts in their care we should like to thank the Syndics of Cambridge University Library and the Trustees of the British Library. For permission to reproduce pictorial material in their keeping, we thank the National Portrait Gallery, London (figure 5); the Sutherland Collection of the Ashmolean Museum, Oxford (figure 16); Cambridge University Library (figures 17, 20, 21, and 22); the British Library (figures 2 and 4); and Edinburgh University Library (figures 1, 3, 6, 7, 8, 9, 11, 12, 13, 14, 15, 18, 19, and the diagram in the translation of the *Dialogus physicus*). For permission to use the epigraph to chapter 1, we thank the holders of the original copyright to Umberto Eco's *The Name of the Rose*: Gruppo Editoriale Fabbri, Bompiani, Sonzogno, Etas S.p.A., Milan (American edition published by Harcourt Brace Jovanovich).

January 1985
Aulthucknall, Derbyshire

Leviathan and the Air-Pump

· 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,

and pays no attention to the actual text or to the attack on Boyle's air-pump programme. Lupoli gives a full and valuable exposition of Boyle's response to Hobbes in the *Examen.* He places the controversy in the context of the earlier pneumatic trials in Italy and France in the 1640s, notably the Pascal-Noël debate. Lupoli suggests that Hobbes attacked Boyle because of his "disappointment at being excluded from the new scientific association, but above all the disillusion and preoccupation with seeing his foundation of physical science ignored" (p. 324). Lupoli highlights Boyle's prolixity as a response to Hobbes's attack on the "rhetoric of ingenuity," and Boyle's tactic of point-by-point refutation of empirical claims as a means of avoiding a direct confrontation with Hobbes's whole physical programme (p. 329). But Lupoli is much more interested in Boyle's utterances on method and on experimental philosophy, and does not give any detailed account of the sources of Hobbes's own polemic. We are grateful to Agostino Lupoli for a copy of his paper (received after our manuscript was written): it is the only source we have found that cites the *Dialogus* in detail. Other major recent sources for Hobbes's natural philosophy do not treat the controversies with Boyle in any detail, nor do they examine the contents of Hobbes's *Dialogus physicus*; see, for example, Spragens, *The Politics of Motion*, esp. chap. 3; Reik, *The Golden Lands of Hobbes*, chap. 7; Goldsmith, *Hobbes's Science of Politics*, chap. 2, although each of these is valuable in other connections. In addition, there are many allusions to Hobbes's science by mainstream Hobbes scholars. They have tended to mine his philosophy of nature because of the generally high evaluation that historians of ideas have placed upon the significance of Hobbes's political and psychological theories and because of their conviction that there must be an overall pattern in his thought. Historians of science, given their low evaluation of Hobbes's natural philosophy and mathematics, have not tended to search for such a pattern.

> in spite of the invention of the air-pump, Hobbes still adhered to his view of the full world. Hobbes' last years were rather tragic. He did not well understand the great development of English empirical science that took place just at that time. . . . And when the members of the Royal Society adopted the experimental method of research . . . Hobbes could no longer keep abreast of them.[18]

Here we see the germ of a standard historiographic strategy for dealing with the Hobbes-Boyle controversy, and, arguably, for handling rejected knowledge in general. We have a dismissal, the rudiments of a causal explanation of the rejected knowledge (which implicitly acts to justify the dismissal), and an asymmetrical handling of rejected and accepted knowledge. First, it is established that the rejected knowledge is not knowledge at all, but error. This the historian accomplishes by taking the side of accepted knowledge and using the victorious party's causal explanation of their adversaries' position as the historian's own. Since the victors have thus disposed of error, so the historian's dismissal is justified.[19] Thus, L. T. More notes that Hobbes's "sneers" at Boyle were "a farrago of nonsense," and quotes Boyle's decisive riposte without detailing what Hobbes's position was.[20] McKie deals with the disputes simply by saying that "Boyle disposed very competently of Hobbes's arguments and very gracefully of his contentious and 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 be-

[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.

cause we have set ourselves the historical task of inquiring into *why* experimental practices were accounted proper and *how* such practices were considered to yield reliable knowledge. As part of the same exercise we shall be adopting something close to a "member's account" of Hobbes's anti-experimentalism. That is to say, we want to put ourselves in a position where objections to the experimental programme seem plausible, sensible, and rational. Following Gellner, we shall be offering a "charitable interpretation" of Hobbes's point of view.[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

[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.

· II ·

Seeing and Believing: The Experimental Production of Pneumatic Facts

. . . Facts are chiels that winna ding,
An' downa 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.

In the conventions of the intellectual world we now inhabit there is no item of knowledge so solid as a matter of fact. We may revise our ways of making sense of matters of fact and we may adjust their place in our overall maps of knowledge. Our theories, hypotheses, and our metaphysical systems may be jettisoned, but matters of fact stand undeniable and permanent. We do, to be sure, reject particular matters of fact, but the manner of our doing so adds solidity to the category of the fact. A discarded theory remains a theory; there are "good" theories and "bad" theories—theories currently regarded as true by everyone and theories that no one any longer believes to be true. However, when we reject a matter of fact, we take away its entitlement to the designation: it never was a matter of fact at all.

There is nothing so given as a matter of fact. In common speech, as in the philosophy of science, the solidity and permanence of matters of fact reside in the absence of human agency in their coming to be. Human agents make theories and interpretations, and human agents therefore may unmake them. But matters of fact are regarded as the very "mirror of nature."[1] Like Stendhal's ideal novel, matters of fact are held to be the passive result of holding a mirror up to reality. What men make, men may unmake; but what nature makes no man may dispute. To identify the role of human agency in the making of an item of knowledge is to identify the possibility of its being otherwise. To shift the agency onto natural reality is to stipulate the grounds for universal and irrevocable assent.

Robert Boyle sought to secure assent by way of the experimentally generated matter of fact. Facts were certain; other items of knowledge much less so. Boyle was therefore one of the most important actors in the seventeenth-century English movement towards a probabilistic and fallibilistic conception of man's natural knowledge. Before the mid-seventeenth century, as Hacking and Shapiro have shown, the designations of "knowledge" and "science" were rigidly distinguished from the category of "opinion."[2] Of the former one could expect the absolute certainty of demonstration, exemplified by logic and geometry. The goal of physical scientists had been to model their enterprise, so far as possible, upon the

[1] For a discussion of the historical origins of the correspondence theory of knowledge and the task of philosophy, see Rorty, *Philosophy and the Mirror of Nature*, esp. pp. 129ff.

[2] Hacking, *The Emergence of Probability*, esp. chaps. 3-5; B. Shapiro, *Probability and 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 celebrated 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. 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.[3] In this chapter we shall examine the means by which the experimental matter of fact was produced.

[3] 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. 95-96; B. Shapiro, *Probability and Certainty*, pp. 44-61.

The Mechanics of Fact-Making: Three Technologies

Boyle proposed that matters of fact be established by the aggregation of individuals' *beliefs*. Members of an intellectual collective had mutually to assure themselves and others that belief in an empirical experience was warranted. Matters of fact were the outcome of the process of having an empirical experience, warranting it to oneself, and assuring others that grounds for their belief were adequate. In that process a multiplication of the witnessing experience was fundamental. An experience, even of a rigidly controlled experimental performance, that one man alone witnessed was not adequate to make a matter of fact. If that experience could be extended to many, and in principle to all men, then the result could be constituted as a matter of fact. In this way, the matter of fact is to be seen as both an epistemological and a social category. The foundational item of experimental knowledge, and of what counted as properly grounded knowledge generally, was an artifact of communication and whatever social forms were deemed necessary to sustain and enhance communication.

We will show that the establishment of matters of fact in Boyle's experimental programme utilized three *technologies*: a *material technology* embedded in the construction and operation of the air-pump; a *literary technology* by means of which the phenomena produced by the pump were made known to those who were not direct witnesses; and a *social technology* that incorporated the conventions experimental philosophers should use in dealing with each other and considering knowledge-claims.[4] Despite the utility of distinguishing the three technologies employed in fact-making, the impression should not be given that we are dealing with distinct categories: each embedded the others. As we shall see, experimental practices employing the material technology of the air-pump crystallized specific forms of social organization; these valued social forms were dramatized in the literary exposition of experimental findings; the literary reporting of air-pump performances ex-

[4] Our use of the word *technology* in reference to the "software" of literary practices and social relations may appear jarring, but it is both important and etymologically justified, as Carl Mitcham nicely shows: "Philosophy and the History of Technology," esp. pp. 172-175. Mitcham demonstrates that Plato distinguished between two types of *techne*: one that consisted mainly of physical work and another that was closely associated with speech. By using *technology* to refer to literary and social practices, as well as to machines, we wish to stress that all three are *knowledge-producing tools*.

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 pneumatic facts, we must consider how each of the three technologies was used and how each bore upon the others.

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.[5] 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*.[6] The machine consisted of two main parts: a glass globe (or "receiver") and the pumping apparatus itself.

[5] 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].)

[6] 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 overall accounts remain the nineteenth-century essays of Wilson, both his *Religio chemici*, pp. 191-219, and, especially, his "Early History of the Air-Pump."

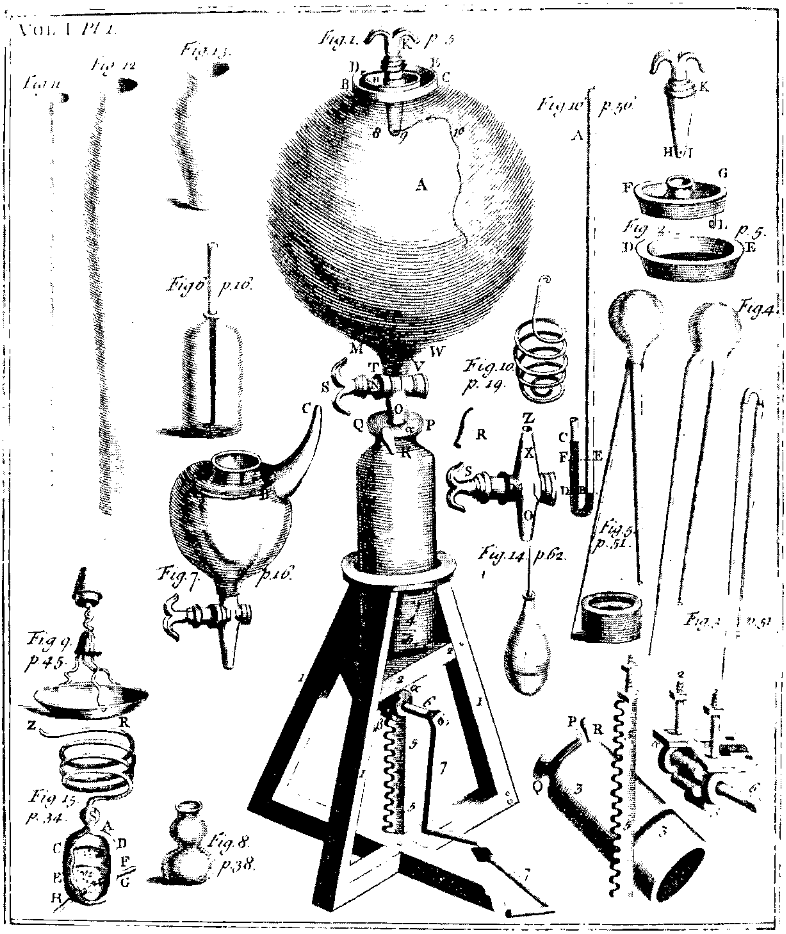

FIGURE 1
Robert Boyle's first air-pump, as it appeared in an engraving in New Experiments Physico-Mechanical *(1660). (Courtesy of Edinburgh University Library.)*

The receiver contained the space from which atmospheric air was to be removed. It was approximately thirty quarts in volume: although Boyle would, ideally, have liked a larger one, this was the limit of his "glass-men's" capabilities. In a few of his *New Experiments* Boyle used a variety of smaller receivers, some as small as one quart in volume, hoping (which proved to be untrue) that these would be easier to evacuate.[7] Experimental apparatus could be placed in

[7] Boyle, "New Experiments," p. 25.

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 show-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 ("I").

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, *Religio chemici*, pp. 197-198; and see Boyle, "New Experiments," p. 36.

The evacuation of air from the receiver of Boyle's original air-pump was an extremely difficult business, as was maintaining that exhaustion for any length of time. Among the chief difficulties was the problem of leakage. Great care had to be taken to ensure that external air did not insinuate itself back into pump or receiver through a number of possible avenues. This is not at all a trivial and merely technical point. The capacity of this machine to produce matters of fact crucially depended upon its physical integrity, or, more precisely, upon collective agreement that it was air-tight for all practical purposes. Boyle detailed the measures he had taken to seal the machine against the intrusion of external air. For example, the aperture at the top of the receiver was sealed with a special cement called *diachylon*, a mixture "which . . . would, by reason of the exquisite commixtion of its small parts, and closeness of its texture, deny all access to the external air."[9] Boyle did not provide the recipe for diachylon, but it was probably a mixture of olive oil and other vegetable juices boiled together with lead oxide. He described how the stopcock was affixed and made good so that it did not leak, using a mixture of "melted pitch, rosin, and wood-ashes." And he took special pains to recount how the leather ring around the sucker was lubricated, both to facilitate its movement in the cylinder and to "more exactly hinder the air from insinuating itself betwixt it and the sides of the cylinder": a certain quantity of "sallad oil" was poured into both receiver and into the cylinder, and more oil was used to lubricate and seal the valve "R". Boyle noted that sometimes a mixture of oil and water proved a more effective seal and lubricant.[10] In addition, the machine was liable to more spectacular assaults upon its physical integrity. Given the state of the glass-blower's art (which Boyle continually lamented), receivers were likely to crack and even to implode. Small cracks were not, in Boyle's view, necessarily fatal. The greater external pressure could act to press them together, and he provided a recipe for fixing them if required: a mixture of powdered quick-lime, cheese scrapings and water, ground up into a paste "to have a strong and stinking smell," spread onto linen plasters and applied to the crack.[11] Finally, the brass cylinder might be bent by atmospheric pressure and the force required to move the sucker: this might also affect the goodness of the seal between washer and the inside of

[9] Boyle, "New Experiments," p. 7; but see p. 35 for Boyle's surmise that even diachylon was somewhat porous to air.

[10] Ibid., p. 9.

[11] Ibid., p. 26.

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."[12] 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.[13]

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

[12] 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."

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

admiration."[14] When in 1664 the King was to be received at the Society, it was anxiously debated what successor to the pump (by then well-known to His Majesty) could so well amuse and instruct the honoured guest. As Christopher Wren wrote from Oxford,

> The solemnity of the occasion, and my solicitude for the honour of the society, make me think nothing proper, nothing remarkable enough. It is not every year will produce such a master experiment as the Torricellian, and so fruitful as that is of new experiments; and therefore the society hath deservedly spent much time upon that and its offspring.

An experimental display adequate to such circumstances ought to be both edifying and spectacular, such as those conducted with the air-pump:

> And if you have any notable experiment, that may appear to open new light into the principles of philosophy, nothing would better beseem the pretensions of the society; though possibly such would be too jejune for this purpose, in which there ought to be something of pomp. On the other side, to produce knacks only, and things to raise wonder, such as Kircher, Schottus, and even jugglers abound with, will scarce become the gravity of the occasion. It must be something between both, luciferous in philosophy, and yet whose use and advantage is obvious without a lecture; and besides, that may surprise with some unexpected effect, and be commendable for the ingenuity of the contrivance.[15]

[14] The visit of the Danish ambassador is noted in Birch, *History*, vol. I, p. 16, and that of Margaret in ibid., pp. 175, 177-178. For Pepys' remark, see Pepys, *Diary*, vol. VIII, pp. 242-243 (entry for 30 May/9 June 1667); see also Nicolson, *Pepys' 'Diary' and the New Science*, chap. 3. Margaret had recently written of her strong preference for rationalistic, rather than experimental, methods in science. Her family were Hobbes's patrons, and her anti-experimentalism reflected his sentiments closely. See Cavendish, *Observations upon Experimental Philosophy* (1666), "Further Observations," p. 4 (also sig d1): ". . . our age being more for deluding Experiments than rational arguments, which some cal a *tedious babble*, doth prefer Sense before Reason, and trusts more to the deceiving sight of their eyes, and deluding glasses, then to the perception of clear and regular Reason. . . ." Cf. R. F. Jones, *Ancients and Moderns*, p. 315n.

[15] Wren to Brouncker, 30 July/9 August 1663, in Birch, *History*, vol. I, p. 288. Preparations for the King's reception were intense, going on from April 1663 to May 1664, but we have no evidence that the royal experimental performance ever took place; see also Oldenburg to Boyle, 2/12 July 1663, in Oldenburg, *Correspondence*, vol. II, pp. 78-79. At precisely the same time that Wren wrote his letter, Boyle

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 au-

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. 488, 490, 499, 501, 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. 158.

[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.

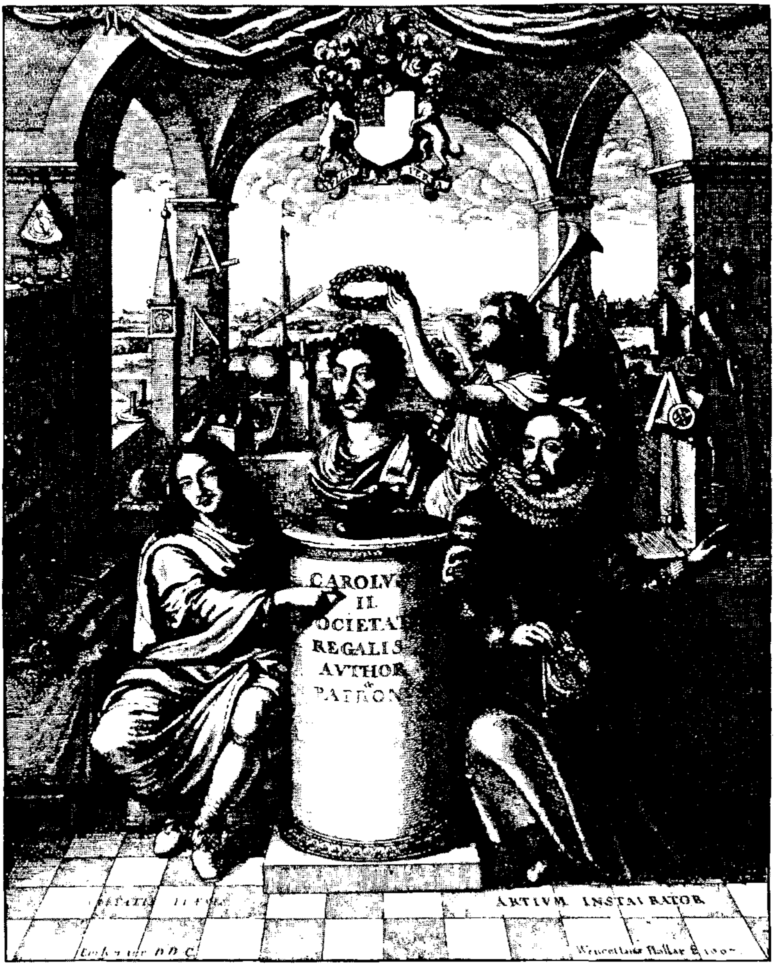

FIGURE 2

Frontispiece to Sprat's History of the Royal Society *(1667). Engraving by Wenceslaus Hollar, design probably by John Evelyn for John Beale in about 1666-1667, and transferred to Sprat's book later. Boyle's revised version of the air-pump is in the centre-left background (see also figure 17). The three figures in the foreground are the president of the Royal Society, Lord Brouncker (left); the King (bust, centre, being crowned by Fame); and Francis Bacon (right). (Courtesy of the British Library.)*

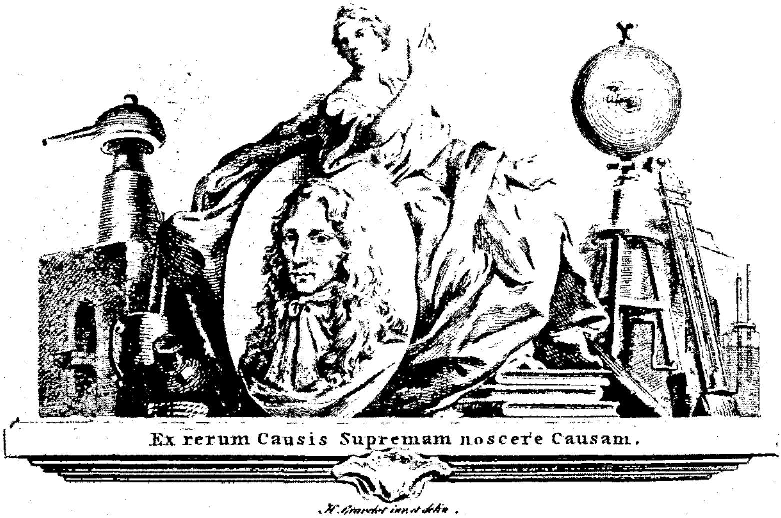

FIGURE 3

Vignette by Hubert François Gravelot Bourguignon for Thomas Birch's edition of Boyle's Works *(1744 and 1772), frontispiece to vol. I. (Courtesy of Edinburgh University Library.)*

thorship 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.[20] Some indication that the

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

assemblage of objects and the gesture had an institutionalized status is afforded by figure 4. This is the frontispiece of a 1679 French collection of experimental essays, including a series by Boyle on tastes and smells.[21] The female figure in this case is recognizably that of Athena, goddess of wisdom. The right hand, as in Gravelot's frontispiece, gestures to heaven, but the left holds a scroll inscribed "Nouvelles Experiences." (It is not clear whether this is a specific reference to the title of Boyle's pneumatic essays.) The female figure's feet rest on books, as they do in figure 3.

FIGURE 4

Frontispiece to anonymously edited collection of essays on natural philosophy: Recueil d'expériences et observations sur le combat, qui procède du mélange des corps *(Paris, 1679). (Courtesy of the British Library.)*

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thought and symbolic labour went into the preparation of philosophical iconography, and such images were intended to be de-coded and reflected upon in this manner. See, for example, the treatment of frontispieces in Webster, *From Paracelsus to Newton*; also Eisenstein, *The Printing Press as an Agent of Change*, esp. pp. 258-261; C. R. Hill, "The Iconography of the Laboratory."

[21] *Recueil d'expériences et observations sur le combat qui procède du mélange des corps* (Paris, 1679). Pp. 125-220 are "Expériences curieuses de l'illustre Mr. Boyle sur les saveurs et sur les odeurs." The anonymously edited collection also included essays by Nehemiah Grew and Leeuwenhoek.

THE PUMP AND THE "EMPIRE OF THE SENSES"

The power of new scientific instruments, the microscope and telescope as well as the air-pump, resided in their capacity to enhance perception and to constitute new perceptual objects. The experimental philosophy, empiricist and inductivist, depended upon the generation of matters of fact that were objects of perceptual experience. Unassisted senses were limited in their ability to discern and to constitute such perceptual objects. Boyle himself reckoned "that the Informations of Sense assisted and highlighted by Instruments are usually preferrable to those of Sense alone."[22] And Hooke detailed the means by which scientific instruments *enlarged* the senses:

> . . . his design was rather to improve and increase the distinguishing faculties of the senses, not only in order to reduce these things, which are already sensible to our organs unassisted, to number, weight, and measure, but also in order to the inlarging the limits of their power, so as to be able to do the same things in regions of matter hitherto inaccessible, impenetrable, and imperceptible by the senses unassisted. Because this, as it inlarges the empire of the senses, so it besieges and straitens the recesses of nature: and the use of these, well plied, though but by the hands of the common soldier, will in short time force nature to yield even the most inaccessible fortress.[23]

In Hooke's view, the task was one of remedying the "infirmities" of the human senses "with Instruments, and, as it were, the adding of artificial Organs to the natural." The aim was the "*inlargement of the dominion*, of the Senses."[24] Among the senses, the eye was paramount, but, " 'tis not improbable, but that there may be found many *Mechanical Inventions* to improve our other Senses, of *hearing, smelling, tasting, touching*."[25]

Things would be seen that were previously invisible: the rings of Saturn, the mosaic structure of the fly's eye, spots on the sun.

[22] Westfall, "Unpublished Boyle Papers," p. 115 (quoting Boyle, "Propositions on Sense, Reason, and Authority," Royal Society, Boyle Papers, IX, f 25); see also van Leeuwen, *The Problem of Certainty*, p. 97.

[23] Birch, *History*, vol. III, pp. 364-365 (entry for 13/23 December 1677).

[24] Hooke, *Micrographia* (1665), "The Preface," sig a2r; see also Bennett, "Hooke as Mechanic and Natural Philosopher," p. 44.

[25] Hooke, *Micrographia*, "The Preface," sig b2v.

And other things, essentially invisible, would be given visual manifestations: the pressure of the air, aqueous and terrestrial effluvia. As Hooke said, "There is a new visible World discovered."[26] This new visible world indicated not only the potential of scientific instruments to enhance the senses; it also served as a warning that the senses were inherently fallible and required such assistance as the experimental philosopher could offer. Glanvill took the telescopic discovery of Saturn's rings as an instance of the fallibility of both unassisted sense and of the hypotheses erected upon unassisted sense:

> And perhaps the newly discovered Ring about Saturn . . . will scarce be accounted for by any systeme of things the World hath yet been acquainted with. So that little can be looked for towards the advancement of natural Theory, but from those, that are likely to mend our prospect of events and sensible appearances; the defect of which will suffer us to proceed no further towards Science, then to imperfect guesses, and timerous supposals.[27]

Scientific instruments therefore imposed both a correction and a discipline upon the senses. In this respect the discipline enforced by devices such as the microscope and the air-pump was analogous to the discipline imposed upon the senses by reason. The senses alone were inadequate to constitute proper knowledge, but the senses disciplined were far more fit to the task. Hooke described the appropriate circulation of items from the senses to the higher intellectual faculties:

> The *Understanding* is to *order* all the inferiour services of the lower Faculties; but yet it is to do this only as a *lawful Master*, and not as a *Tyrant*. . . . It must *watch* the irregularities of the Senses, but it must not go before them, or *prevent* their information. . . . [T]he true Philosophy . . . is to *begin* with the Hands and Eyes, and to *proceed* on through the Memory, to be *con-*

[26] Ibid., sig a2v. There is a clear connection between these views of the role of scientific instruments and the epistemological problem of "transdiction" (inferring from the visible to the invisible) discussed by Mandelbaum, *Philosophy, Science, and Sense Perception*, chap. 2.

[27] Glanvill, *Scepsis scientifica* (1665), "To the Royal Society," sig b4v; also pp. 54-55. See also B. Shapiro, *Probability and Certainty*, pp. 61-62; for an account of the observational and theoretical issues at stake in the problem of Saturn's rings, see van Helden, " 'Annulo Cingitur': The Solution of the Problem of Saturn"; idem, "Accademia del Cimento and Saturn's Ring."

tinued by the Reason; nor is it to stop there, but to *come about* to the Hands and Eyes again, and so, by a *continual passage round* from one Faculty to another, it is to be maintained in life and strength, as much as the body of man is.[28]

Just as the reason disciplined the senses, and was disciplined by it, so the new scientific instruments disciplined sensory observation through their control of *access*.

Boyle's and Hooke's air-pump was, in the former's terminology, an "elaborate" device. It was also temperamental (difficult to operate properly) and very expensive: the air-pump was seventeenth-century "Big Science." To finance its construction on an individual basis it helped greatly to be a son of the Earl of Cork. Other natural philosophers, presumably as well supplied with cash as Boyle, shied away from the expense of building a pneumatic engine, and a major justification for founding scientific societies in the 1660s and afterwards was the collective financing of the instruments upon which the experimental philosophy was deemed to depend.[29] Reading histories of seventeenth-century science, one might gain the impression that air-pumps were widely distributed. They were, however, very scarce commodities. We shall present further details concerning the location and operation of air-pumps during the 1660s in chapter 6. However, the situation can be briefly summarized: Boyle's original machine was soon presented to the Royal Society of London; he had one or two redesigned machines built for him by 1662, operating mainly in Oxford; Christiaan Huygens had one made in The Hague in 1661; there was one at the Montmor Academy in Paris; there was probably one at Christ's College, Cam-

[28] Hooke, *Micrographia*, "The Preface," sig b2[r]. For Hooke's stress on deductions from hypotheses, which differed from Boyle's approach, see Hesse, "Hooke's Philosophical Algebra"; idem, "Hooke's Development of Bacon's Method."

[29] The only hard evidence we have found concerning the cost of this air-pump indicates that a version of the *receiver* ran to £5: Birch, *History*, vol. II, p. 184. Given the expense of machining the actual pumping apparatus, and replacement costs for broken parts (probably considerable), an estimate of £25 for the entire machine might prove conservative. Thus this pump would have cost more than the annual salary of Robert Hooke as Curator of the Royal Society, who was the London pump's chief operator. Christiaan Huygens' older brother Constantijn, much the wealthiest of the three Huygens brothers, withdrew from a pump-building project, "being afraid of the cost": Huygens, *Oeuvres*, vol. III, p. 389. Cf. van Helden, "The Birth of the Modern Scientific Instrument," pp. 64, 82n-83n; and A. R. Hall, *The Revolution in Science*, p. 263: "Everyone wanted at least to have witnessed the experiments, though few could own so costly a piece of apparatus." In chapter 6 we present some evidence on the cost of later devices.

bridge, by the mid-1660s; and Henry Power may have possessed one in Halifax from 1661. So far as can be found out, these were all the pumps that existed in the decade after their invention.

Without doubt, the intricacy of these machines and their limited availability posed a problem of access that experimental philosophers laboured to overcome. Less obviously, the control of access to the devices that were to generate genuine knowledge was a positive advantage. The space where these machines worked—the nascent laboratory—was to be a public space, but a restricted public space, as critics like Hobbes were soon to point out. If one wanted to produce authenticated experimental knowledge—matters of fact—one had to come to this space and to work in it with others. If one wanted to see the new phenomena created by these machines, one had to come to that space and see them with others. The phenomena were not on show anywhere at all. The laboratory was, therefore, a disciplined space, where experimental, discursive, and social practices were collectively controlled by competent members. In these respects, the experimental laboratory was a better space in which to generate authentic knowledge than the space outside it in which simple observations of nature could be made. To be sure, such observations were reckoned to be vital to the new philosophy and were judged vastly preferable to trust in ancient authority. Yet most observational reports were attended with problems in evaluating *testimony*. A report of an observation of a new species of animal in, for example, the East Indies, could not easily be checked by philosophers whose credibility was assured. Thus all such reports had to be inspected both for their plausibility (given existing knowledge) and for the credibility and trustworthiness of the witness.[30] Such might not be the case with experimental performances in which, ideally, the phenomena were witnessed together by philosophers of known reliability and discernment. Insofar as one insisted upon the foundational status of experimentally produced matters of fact, one ruled out of court the knowledge-claims of alchemical "secretists" and of sectarian "enthusiasts" who claimed individual and unmediated inspiration from God, or whose solitary "treading of the Book of Nature" produced unverifiable observational testimony. It is not novel to notice that the constitution of experimental knowledge was to be a public process. We stress, however, that producing matters of fact through scientific

[30] For concern with evaluating testimony in the natural history sciences, see B. Shapiro, *Probability and Certainty*, chap. 4, esp. pp. 142-143.

machines imposed a special sort of discipline upon this public. In following sections of this chapter we shall describe the nature of the discursive and social practices that Boyle recommended for the generation of the matter of fact. Before proceeding to that task we need briefly to describe what a pneumatic experiment was and how its matters of fact were said to relate to their interpretation and explanation.

Two Experiments

The text of Boyle's *New Experiments* of 1660 consisted of narratives of forty-three trials made with the new pneumatic engine. In following chapters we shall see how critics of Boyle's experimental programme managed to deconstruct the integrity of both his matters of fact and explanatory resources. These deconstructions called into question almost every aspect of Boyle's practices and findings: from the physical integrity of the air-pump to the legitimacy of making experimental matters of fact into the foundations of proper natural philosophical knowledge. For the present, however, it will be useful to describe two of Boyle's first air-pump experiments as he himself recounted them. These two experiments have not been randomly chosen. There are three reasons for concentrating upon them. First, the phenomena produced were accounted paradigmatic by advocates and critics of Boyle's philosophy. They were prizes contested between mechanical and nonmechanical natural philosophers, and between varieties of mechanical philosophers in the seventeenth century. Second, they include a contrast between an experiment which Boyle reckoned to be successful and one which he admitted to be a failure: critics such as Hobbes, as we shall see, seized upon this admission of failure as a way to undermine the whole of Boyle's experimental programme. Third, both experiments were deemed by Boyle to have a particularly intimate connection with the legitimacy of his major explanatory items in pneumatics: the pressure and the "spring" of the air. The tactical relations between experimental matters of fact and their explanation is, therefore, especially visible in these instances.

The first experiment to be described is the seventeenth of Boyle's original series. He himself referred to it as "the principal fruit I promised myself from our engine." Arguably, the air-pump was constructed chiefly with a view to performing this experiment. We shall call it the "void-in-the-void" experiment. It consisted of putting the Torricellian apparatus in the pump and then evacuating the receiver.[31] The "noble experiment" of Evangelista Torricelli was first performed in 1644. A tube of mercury, sealed at one end, was filled and then inverted in a dish of the same substance. The resultant "Torricellian space" left at the top became a celebrated phenomenon and problem for natural philosophers. For a decade after its production, the phenomenon was associated with two questions of immense cosmological importance: the real *character* of that "space" and the *cause* of the elevation of the mercury in the glass tube. The centre of interest in these questions in 1645-1651 was France, where Mersenne reported on the Italian work, and where natural philosophers such as Pascal, Petit, Roberval, and Pecquet all gave their views and experimented with the Torricellian apparatus.

Two points about the state of this problem need to be made in this connection. First, the Torricellian phenomenon was discussed in terms of long-standing debates over whether or not a vacuum could exist in nature.[32] Was this experiment decisive proof that a vacuum did exist? In practice, all possible combinations of views were held on the Torricellian space and the elevation of the mercury. Scholastic authorities maintained that the space was not void, and that the height of mercury was determined by the necessary limit to the expansion of the air left above the mercury. For Descartes, the mercury was sustained by the weight of the atmosphere, but the Torricellian space was filled by some form of subtle matter. For Descartes' inveterate opponent Roberval, the Torricellian space was indeed empty, but the height of the mercury depended upon the limit of a natural *horror vacui*. Finally, both Torricelli and Pascal held that the space was empty, and that the mercury was sustained by atmospheric weight. This experiment was therefore given various descriptions in the course of a debate which centred on the choice between plenist and vacuist theories. Given the range of views actually maintained in the 1640s and 1650s, the Torricellian problem seemed a key example of scandal in natural philosophy.[33]

Second, it seemed to participants that experimental measures

[31] Boyle, "New Experiments," p. 33. Experiment 19 used a *water* barometer.

[32] For medieval and early modern controversies over the vacuum, see Grant, *Much Ado about Nothing*, esp. chap. 4.

[33] Schmitt, "Experimental Evidence for and against a Void"; idem, "Towards an Assessment of Renaissance Aristotelianism," esp. p. 179; de Waard, *L'expérience barométrique*; Middleton, *The History of the Barometer*, chaps. 1-2; Westfall, *The Construction of Modern Science*, pp. 25-50.

offered a path away from such indecisive controversy. In his own work Blaise Pascal tried to combine experimental modesty and demonstrative compulsion to sway his opponents and critics. In treatises published in 1647-1648 Pascal described what soon became celebrated experimental variants of the Torricellian performance that he tentatively proffered as convincing evidence for his hypothesis, including a report of the Puy-de-Dôme trial of September 1648. Pascal firmly argued against men like the orthodox but Cartesian philosopher Noël for their love of theory and their premature hypothesizing. Thus the Torricellian experiment was intimately associated with the claim of experiment to settle belief about nature, to end controversy, and to generate consensus.[34]

Boyle's void-in-the-void experiment, and his interpretation of it, indicates the depth of his commitment to the role of experiment in securing assent. No less importantly, it illustrates the extent to which Boyle broke with the natural philosophical discourse in which the Torricellian experiment and its derivatives had previously been situated. The contents of the Torricellian space, whether in the receiver or outside of it, were of little concern to him. Neither was it of interest to stipulate whether or not the exhausted receiver constituted a "vacuum" within the frame of meaning of existing vacuist-plenist controversies. He would create a new discourse in which the language of vacuism and plenism was ruled out of order, or at least managed so as to minimize the scandalous disputes that, in his view, it had engendered. The receiver was a space into which one could move this paradigmatic experiment. And the discursive and social practices in which talk about this experiment was to be embedded constituted a space in which disputes might be neutralized.[35]

This is what Boyle did: he took a three-foot-long glass tube, one-

[34] Guenancia, *Du vide à Dieu*, pp. 63-100. For the French context of this work, see also Lenoble, *Mersenne*; H. Brown, *Scientific Organizations*. For the transmission of this interest to England, and, particularly, to Boyle, see Webster, "Discovery of Boyle's Law," pp. 455-457; Hartlib to Boyle, 9/19 May 1648, in Boyle, *Works*, vol. VI, pp. 77-78. For a contemporary version of the history of experimental pneumatics, see Barry, *Physical Treatises of Pascal*, pp. xv-xx.

[35] For continuing English disagreements about the nature of the Torricellian space in the 1660s: Hooke, *Micrographia*, pp. 13-14, 103-105; idem, *An Attempt for the Explication* (1661), pp. 6-50 (rewritten in *Micrographia*, pp. 11-32); Power, *Experimental Philosophy* (1664), pp. 95, 109-111; John Wallis to Oldenburg, 26 September/6 October 1672, in Oldenburg, *Correspondence*, vol. IX, pp. 258-262; see also Frank, *Harvey and the Oxford Physiologists*, chaps. 4-5, where the context of overriding interest by Oxford researchers in the *nitre* is discussed.

quarter inch in diameter, filled it with mercury, and inverted it as usual into a dish of mercury, having, as he said, taken care to remove bubbles of air from the substance. The mercury column then subsided to a height of about 29 inches above the surface of the mercury in the dish below, leaving the Torricellian space at the top. He then pasted a piece of ruled paper at the top of the tube, and, using a number of strings, lowered the apparatus into the receiver. Part of the tube extended above the aperture in the receiver's top, and Boyle carefully filled up the joints with melted diachylon. He noted that there was no change in the height of the mercury before evacuation commenced.[36] (See figure 12 for a drawing of a later version of this experimental set-up.)

Pumping now commenced. The initial suck resulted in an immediate subsidence of the mercury column; subsequent sucks caused further falls. (Boyle's primitive attempt to measure the levels reached after each suck was unsuccessful, as the mercury descended below the paper gauge.) After about a quarter-hour's pumping (how many sucks is not recorded), the mercury would fall no further. Significantly, the mercury column did not fall all the way to the level of the liquid in the dish, remaining about an inch above it. The experiment was quickly repeated in the presence of witnesses, and the same result was obtained. Boyle further observed that the fall of the mercury could be reversed by turning the stopcock to let in a little air. However, the column did not quite regain its previous height even when the apparatus was returned to initial conditions. Variants of this basic protocol were also reported: the experiment was tried with a glass mercury-containing tube sealed at the top with diachylon to test the porousness of that plaster. Boyle found that diachylon did not provide a completely tight seal. It was tried with a smaller receiver to see whether a more efficient exhaustion, and therefore a more complete fall of the mercury column, could be obtained (it could not); and it was tried in reverse (the air in the receiver was condensed by working the pump backwards) to see whether the mercury could be made to stand higher than 29 inches (it could).

So far, the account we have given has been restricted to what Boyle said was done and observed, without any of the *meanings* he attached to the experiment. For Boyle, this experiment offered an exemplar of how it was permissible to interpret matters of fact.

[36] This summary derives from the account given in Boyle, "New Experiments," pp. 33-39.

The problems were those traditionally associated with the Torricellian experiment: the elevation of the mercury and the nature of apparently void space. Boyle came to the void-in-the-void experiment with definite expectations about its outcome. The purpose of putting the Torricellian apparatus in the receiver was to imitate, and to give a visible analogy for, the impossible task of trying "the experiment beyond the atmosphere." He surmised that the normal height at which the mercury column was sustained was accounted for by "an aequilibrium with the cylinder of air supposed to reach from the adjacent mercury to the top of the atmosphere." So, "if this experiment could be tried out of the atmosphere, the quicksilver in the tube would fall down to a level with that in the vessel." This expectation was accompanied by a preformed explanatory resource: the *pressure* of the air. If the mercury descended as expected, it would be because "then there would be no pressure upon the subjacent [mercury], to resist the weight of the incumbent mercury."[37] Another, related, explanatory resource was also implicated. When Boyle initially enclosed the Torricellian apparatus in the receiver, and before he began evacuating it, he noted that the column remained at the same height as before. The reason for this, he said, must be "rather by virtue of [the] spring [of the air enclosed in the receiver] than of its weight; since its weight cannot be supposed to amount to above two or three ounces, which is inconsiderable in comparison to such a cylinder of mercury as it would keep from subsiding." When pumping began, the mercury level fell because of the diminished pressure of air in the receiver. The observation that the mercury did not in fact fall all the way down was accounted for by slight leakage:

> . . . when the receiver was considerably emptied of its air, and consequently that little that remained grown unable to resist the irruption of the external, that air would (in spight of whatever we could do) press in at some little avenue or other; and though much could not thereat get in, yet a little was sufficient to counterbalance the pressure of so small a cylinder of quicksilver, as then remained in the tube.[38]

In the next section of this chapter we examine the ways in which Boyle used the concepts of the air's weight and its spring or elasticity. But, for the present, we note that weight and spring were

[37] Ibid., p. 33.
[38] Ibid., p. 34.

the two mechanical notions that circumscribed interpretative talk about this paradigmatic experiment.

While it was permissible, even obligatory, to speak of the cause of the mercury's elevation in such terms, the treatment of the question of a void was handled in a radically different manner. This was to be made, so far as possible, into a nonquestion. Was the Torricellian space a *vacuum*? Did the exhausted receiver constitute a vacuum? The platform from which Boyle elected to address these questions was *experimental*: the way of talking appropriate to experimental philosophy was different in kind to existing natural philosophical discourse. Boyle recognized that his experiment would be deemed relevant to the traditional question posed of the Torricellian experiment, "whether or no that noble experiment infer a vacuum?" Was the exhausted receiver a space "devoid of all corporeal substance?" Boyle professed himself reluctant to enter "so nice a question" and he did not "dare" to "take upon me to determine so difficult a controversy." But settling the question of a vacuum was not what this experiment was about, nor were questions like this any part of the experimental programme. They could *not* be settled experimentally, and, because they could not, they were illegitimate questions. Plenists, those who maintained, either on mechanical or nonmechanical grounds, that there could not be a vacuum, had taken their reasons

> not from any experiments, or phaenomena of nature, that clearly and particularly prove their hypothesis, but from their notion of a body, whose nature, according to them, consisting only in extension . . . [means that] to say a space devoid of body, is, to speak in the schoolmen's phrase, a contradiction in adjecto.

But such reasons and such speech had no place in the experimental programme; they served "to make the controversy about a vacuum rather a metaphysical, than a physiological question; which therefore we shall here no longer debate. . . ."[39]

The significance of this move must be stressed. Boyle was not "a vacuist" nor did he undertake his *New Experiments* to prove a vacuum. Neither was he "a plenist," and he mobilized powerful arguments against the mechanical and nonmechanical principles adduced by those who maintained that a vacuum was impossible.[40]

[39] Ibid., pp. 37-38. The notion of body attacked here was that of Cartesian plenists.
[40] For example, ibid., pp. 37-38, 74-75; cf. C. T. Harrison, "Bacon, Hobbes, Boyle, and the Ancient Atomists," pp. 216-217 (on Boyle's "belief in the vacuum").

What he was endeavouring to create was a natural philosophical discourse in which such questions were inadmissible. The air-pump could not decide whether or not a "metaphysical" vacuum existed. This was not a failing of the pump; instead, it was one of its *strengths*. Experimental practices were to rule out of court those problems that bred dispute and divisiveness among philosophers, and they were to substitute those questions that could generate matters of fact upon which philosophers might agree. Thus Boyle allowed himself to use the term "vacuum" in relation to the contents of the evacuated receiver, while giving the term experimental meaning. By "vacuum," Boyle declared, "I understand not a space, wherein there is no body at all, but such as is either altogether, or almost totally devoid of air."[41] Boyle admitted the *possibility* that the receiver exhausted of air was replenished with "some etherial matter," "but not that it really is so."[42] As we shall see in chapter 5, during the 1660s Boyle rendered the question of an aether into an experimental programme, partly in response to plenist critics of his *New Experiments*. However, even in that research programme, the *existence* of an aether in the receiver, and therefore of a plenum, was not decided, but only whether such an aether had any experimental consequences.

Boyle's "vacuum" was a space "almost totally devoid of air": the incomplete fall of the mercury indicated to him that the pump leaked to a certain extent. The finite leakage of the pump was not, in his view, a fatal flaw but a valuable resource in accounting for experimental findings and in exemplifying the proper usage of terms like "vacuum." The "vacuum" of his exhausted receiver was thus not an experiment but a space in which to do experiments and generate matters of fact without falling into futile metaphysical dispute.[43] And it was an experimental space about which new discursive and social practices could be mobilized to generate assent.

The second of Boyle's *New Experiments* we describe can be treated more briefly. This was the thirty-first of the series, and again it dealt with a theoretically important and much debated phenome-

[41] Boyle, "New Experiments," p. 10. This was a definition apparently so novel, and so difficult to comprehend within existing philosophical discourse, that Boyle was obliged continually to repeat it in his subsequent disputes with Hobbes and Linus (see chapter 5).

[42] Ibid., p. 37.

[43] Compare the reaction of the German researchers Schott and Guericke to leakage in Boyle's pump (discussed in chapter 6). They said that their pump (in which one could not perform experiments) was therefore better than Boyle's: Schott, *Technica curiosa sive mirabilia artis* (1664), book II, pp. 75, 97-98.

non, that of *cohesion*. Two smooth bodies, such as marble or glass discs, can be made spontaneously to cohere when pressed against each other. This common phenomenon had long been a centre-piece of vacuist-plenist controversies. Lucretius used it to prove the existence of a vacuum; in the Middle Ages it was appropriated by both vacuists and plenists to support their cases; and it occupied a prominent place in Galileo's work on the problems of rigidity and cohesion. (In following chapters we shall discuss the work that Boyle did on cohesion prior to *New Experiments*, Hobbes's treatment of the phenomenon in his *De corpore* of 1655, and the continuing disputes between the two that dealt with this problem.) The fact that such surfaces displayed spontaneous cohesion was not in doubt; the proper explanation of that cohesion and of the circumstances attending their forcible separation was, however, intensely debated. It was agreed by all that it was difficult, yet possible, to separate cohered very smooth bodies by exerting a force perpendicular to the plane of their cohesion. Lucretius had argued that, since the velocity of the air rushing in from the sides to fill the space created by their separation must be finite, therefore a vacuum existed at the moment of separation. Scholastic plenists tended to stress the difficulty of separation, attributing this to the *horror vacui*. Various glosses were put upon the act of separation, all tending to establish the reality of a plenum.[44]

Boyle's idea, as with the Torricellian experiment, was to insert this phenomenon into his new experimental space. He would thus subject it to his new technical and discursive practices and use it to exemplify the effects of the air's pressure. Again, Boyle came to the experiment with an expectation of its outcome and with explanatory resources equipped to account for the outcome. If two "exquisitely polished" marble discs were laid upon each other, "they will stick so fast together, that he, that lifts up the uppermost, shall, if the undermost be not exceedingly heavy, lift up that too, and sustain it aloft in the free air." "A probable cause" of this cohesion was at hand:

> . . . the unequal pressure of the air upon the undermost stone; for the lower superfices of that stone being freely exposed to the air, is pressed upon by it, whereas the uppermost surface,

[44] See, for example, Grant, *Much Ado about Nothing*, pp. 95-100; Lucretius, *On the Nature of the Universe*, p. 12; Galileo, *Dialogues concerning Two New Sciences*, pp. 11-13; Millington, "Theories of Cohesion." Boyle used the terms "cohesion" and "adhesion" more or less interchangeably in referring to this phenomenon. As "adhesion" now suggests viscous sticking, we shall consistently use "cohesion."

> being contiguous to the superior stone, is thereby defended from the pressure of the air; which consequently pressing the lower stone against the upper, hinders it from falling.

Boyle conjectured that cohered marbles placed in the receiver that was then evacuated would fall apart as the air's pressure diminished.

This is what he did: he took marble discs 2⅓ inches in diameter and between ¼ and ½ inch thick; he then tried to make them cohere in free air. Immediately, there were problems: he could not obtain marbles ground so smooth that they would stay together for more than several minutes. Since it would take longer than that to exhaust the receiver, these were clearly unsuitable. So he moistened the interior surfaces of the pair with alcohol. This would, he reckoned, serve to smooth out residual irregularities in the marbles. Having got the marbles to cohere, he then attached a weight of four ounces to the lower stone ("to facilitate its falling off"), lowered the set by means of a string into the receiver, and commenced pumping. (For a later version of this experiment, see figure 9.) The marbles did not separate, and the experiment was accounted unsuccessful. Yet Boyle was ready with a reason why this experimental failure should not occasion the abandonment of his hypothesis: the pump leaked. That quantity of residual air, allowed in by the porousness of diachylon or by the looseness of the fit between sucker and cylinder, kept the marbles stuck together. The same leakage that permitted Boyle to offer an experimental meaning of the "vacuum" now provided a reason to hold fast to the theory of the air's pressure in the face of apparent counterevidence. In this sense, the experiment was not a failure at all.[45]

One other striking circumstance of this experiment needs to be noted. The trial was reported as a test and exemplification of the pressure of the air. In the quite brief narrative that constituted

[45] Boyle, "New Experiments," pp. 69-70. Boyle alluded here to earlier experiments on cohesion, published a year later in *The History of Fluidity and Firmness*; we discuss these in chapter 5. Readers of a realist bent, who might wish to know "what really happened" in these experiments, will necessarily be disappointed. We cannot reconstruct with any confidence what specific physical factors operated in Boyle's trials. From the point of view of modern scientific knowledge, a range of factors would have to be considered here. These include: (1) the isotropic pressure gradient on different surfaces of the marbles (as Boyle said); (2) short-range contact forces (not considered by Boyle); and (3) the phenomenon of *adhesion* due to the viscosity of the various lubricants Boyle employed (which he considered he had sufficiently allowed for).

Boyle's thirty-first experiment there was no allusion of any kind to the discursive tradition in which the phenomenon of cohesion had been paradigmatic. The phenomenon was not treated here as having any bearing upon the question of a vacuum versus a plenum. Having argued against the legitimacy of this philosophical discourse in experiment 17, Boyle now showed how one of its centrepieces could be handled as if that discourse did not exist.[46]

Facts and Causes: The Spring, Pressure, and Weight of the Air

Boyle's *New Experiments* did not offer any explicit and systematic philosophy of knowledge. It did not discuss the problem of justifying inductive inference, propose formal criteria for establishing physical hypotheses, nor did it stipulate formal rules for limiting causal inquiry. What *New Experiments* did do was to *exemplify* a *working* philosophy of scientific knowledge.[47] In a concrete experimental setting it showed the new natural philosopher how he was to proceed in dealing with practical matters of induction, hypothesizing, causal theorizing, and the relating of matters of fact to their explanations. Boyle sought here to create a *picture* to accompany the experimental language-game and the experimental form of life. He did this largely by *ostension*: by showing others through his own example what it was like to work and to talk as an experimental philosopher.

Boyle's epistemological armamentarium included matters of fact, hypotheses, conjectures, doctrines, speculations, and many other locutions serving to indicate causal explanations. His overarching concern was to protect the matter of fact by separating it from various items of causal knowledge, and he repeatedly urged caution in moving from experimental matters of fact to their physical explanation. How, in practice, did Boyle manage this boundary? And

[46] We shall see that Boyle's adversaries, Hobbes and Linus, refused to allow this phenomenon to pass into the new, "nonmetaphysical" experimental discourse. Boyle's responses to them commented upon vacuist-plenist discourse and its legitimacy in this case.

[47] For an attempt to identify Boyle's "coherent and sophisticated view of scientific method," see Laudan, "The Clock Metaphor and Probabilism," pp. 81-97, esp. p. 81. We have no substantial disagreements with Laudan on Boyle's methods, but we dissent from his assessment of Boyle's philosophy as coherent and systematic. Cf. also Wiener, "The Experimental Philosophy of Boyle," and Westfall, "Unpublished Boyle Papers."

how, in practice, did he move between matters of fact and ways of accounting for them? Our best access to these questions is through an examination of Boyle's major explanatory resources in *New Experiments* and in his subsequent essays in pneumatics: the spring, pressure, and weight of the air.

The first thing to note is that the epistemological status of spring, pressure, and weight was never clearly spelt out in *New Experiments* or elsewhere. For example, in reporting the first of his *New Experiments,* the spring of the air was simply referred to as a "notion": it was "that notion, by which it seems likely, that most, if not all [his pneumatical findings] will prove explicable. . . ."[48] In other places Boyle chose to label the status of the spring an "hypothesis" or a "doctrine."[49] And, as we shall show in chapter 5, Boyle operationally treated the spring of the air as a matter of fact. In the twentieth of the *New Experiments* Boyle supposed that the fact "that the air hath a notable elastical power" has been "abundantly evinced" from his researches, "and it begins to be acknowledged by the eminentest naturalists."[50]

It would be easy to conclude, if one wanted, that Boyle was a poor formal philosopher of knowledge and a deficient formulator of scientific methodology. That is not a point we wish to make; nevertheless, there are several aspects of his procedures we need to note in this connection. First, Boyle did not detail the steps by which he moved from matters of fact to their explanation. He did not, for example, say in what ways the air's "elastical power" had been "evinced" and established; he merely announced that this had been accomplished. Second, he did not clearly discriminate between the air's spring and pressure as hypothetical causes of experimental facts and as matters of fact in their own right. Certainly, by the early 1660s (especially in his controversies with critics) Boyle was treating these explanatory items as if they were matters of fact and not hypotheses: their real existence had been *proved* by experiment, and he entertained no doubt on that score. While continuing to warn experimentalists to be circumspect in their hypothesizing and

[48] Boyle, "New Experiments," p. 11.

[49] See, for example, Boyle, "Examen of Hobbes," p. 197; idem, "Defence against Linus," pp. 119-120, 162 (and note the full title referring to the "doctrine" of the air's spring and weight). For discussion of the senses in which Boyle used the term "hypothesis," see Westfall, "Unpublished Boyle Papers," pp. 69-70: "Boyle evidently considered all generalizations in natural science to be hypotheses"; "To Boyle 'hypothesis' meant a supposition put forth to account for known facts . . ."

[50] Boyle, "New Experiments," p. 44.

to regard causal items as provisional, he treated *these* hypotheses as certainly established. And yet the criteria and rules for establishing hypotheses were not given. Third, Boyle made an unexplained distinction between the assurance we can have about the air's spring and pressure as causes and the assurance we can have about *their* causes. There was a strong boundary placed between speech about the spring as an explanation of matters of fact and speech about explanations of spring. Thus, in the first of the *New Experiments*, Boyle claimed that his "business [was] not . . . to assign the adequate cause of the spring of the air, but only to manifest, that the air hath a spring, and to relate some of its effects." Possible causes of this spring were arrayed, Boyle professing himself "not willing to declare peremptorily for either of them against the other." For instance, one might conceive of the spring as caused by the air having a real texture like that of wool fleece or sponge; or one might account for it in terms of Cartesian vortices; or one could posit that the air's corpuscles actually were "congeries of little slender springs."[51] Not only was it impossible to decide, it was, in Boyle's view, impolitic to try to decide which was the real cause. He warned against any such attempt as futile, and he never worked to specify the cause of the spring. The spring and the spring's cause were therefore treated as fundamentally different explanatory items: the former was "evinced" by the experiments; the latter was not, and, in practice, could not be. But they were both causes, and Boyle proffered no criteria for identifying in what way they were entitled to such radically different treatments. (The cause of the air's weight was, however, more straightforwardly accounted for: it was a function of the height and density of the atmospheric cylinder bearing upon any given cross-section.)

Our point may be summarized this way: the language-game that Boyle was teaching the experimental philosopher to play rested upon implicit acts of boundary-drawing. There was to be a crucial boundary between the experimental matter of fact and its ultimate physical cause and explanation. Viewed naively, or as a stranger might view it, it is unclear why the spring of the air, as the professed cause of the observed results, should be treated as a matter of fact rather than as a speculative hypothesis. Indeed, we have hinted here (and shall describe in detail in chapter 5) how the idea of the spring moved from outside to within the class of matters of fact.

[51] Ibid., pp. 11-12, 50, 54. Boyle explicitly labelled these various causal notions as "hypotheses." See also idem, "The General History of the Air," pp. 613-615.

It is also unclear upon what bases Boyle distinguished between his treatment of the spring and the cause of the spring. These are the grounds upon which one might wish to criticize Boyle as epistemologist and methodologist. However, our conclusions are not these: rather, we note that Boyle's criteria and rules for making his preferred distinctions between matters of fact and causes have the status of *conventions.* Causal talk is grounded in conventions which Boyle's reports exemplify, just as the construction of the matter of fact is conventional in nature (as we shall show in the following sections of this chapter). The ultimate justification of convention does not take the form of verbalized rules. Instead, the "justification" of convention *is* the form of life: the total pattern of activities which includes discursive practices.[52] This observation is supported by our later discussions of the ways in which Boyle's critics attempted to subvert his justifications of experimental practice and the ways in which Boyle replied.

Consider also the language Boyle used to describe his principal ontological concern: the air and its properties of spring, weight, and pressure. As we have noted, Boyle announced that the function of his pneumatic researches was "only to manifest that the air hath a spring, and to relate some of its effects."[53] Adversaries were defined by Boyle in terms of their alleged attitude to the spring of the air as a matter of fact. He argued that "the Cartesians," for example, need not grant a vacuum, nor need they abandon their notion of some form of subtle matter that could penetrate glass, but they must "add, as some of them of late have done, the spring of the air to their hypothesis." Boyle confessed in 1662 that it was more difficult to deal with adversaries, such as the Jesuit Franciscus Linus, who allowed a limited spring in the air, than it was to deal with those who denied it altogether, such as Hobbes. So in his response to Linus he claimed that "we have performed much more by the spring of the air, which we can within certain limits increase at pleasure, than we can by bare weight."[54] This comment suggests that Boyle distinguished systematically between spring and weight. He did not. Typically, he used the term "pressure" to describe

[52] This account has obvious resonances with Wittgenstein's treatment of language as secondary to patterns of activity. Language makes sense as embedded within those patterns: Wittgenstein, *Blue and Brown Books*, pp. 81-89; idem, *On Certainty*, props. 192, 204.

[53] Boyle, "New Experiments," p. 12.

[54] Boyle, "Examen of Hobbes," p. 191; idem, "Defence against Linus," pp. 121, 133.

these attributes of the air, distinguishing the specific cause of pressure only when it fitted a specific polemical purpose. In future references we shall follow Boyle in using the term "pressure" generically.

But Boyle's terminology was by no means consistent. He referred to the "pressing or sustaining force of the air," or to the "sustaining power of the air." In *New Experiments* he discussed the apparent heaviness of the cover of the receiver when evacuated, using the terms "spring of the external air," "force of the internal expanded air and that of the atmosphere," and "pressure" interchangeably. In early experiments in this text the term "protrusion" is used alongside that of "pressure."[55] These usages were no more consistent in subsequent essays on pneumatics and the air-pump trials. In the *Continuation of New Experiments* of 1669 and in later texts written against Hobbes, "pressure" referred to both weight and spring.[56] And in the central void-in-the-void experiment 17 of *New Experiments* Boyle reported that the insertion of the Torricellian apparatus in the sealed receiver did not produce a fall in the height of the mercury in the barometer. He attributed this to the "spring" of the air inside the still-unevacuated receiver, which was not affected by its removal from the "weight" of the atmosphere. Thus trials that computed the relation between the height of this mercury and the number of strokes of the sucker were interpreted as testing the relation between the air's "pressure" and its "density." "Pressure" thus embraced spring and weight.[57]

Two important moments in Boyle's exposition made this terminology highly sensitive to interpretation. First, we have introduced Boyle's experiment on the cohesion of smooth marbles *in vacuo.* This was, as we shall describe in chapter 5, a continuation of a sustained series of earlier trials in free air. In *The History of Fluidity and Firmness,* composed in 1659 and published in 1661, such cohesion was attributed to "the pressure of the atmosphere, proceeding partly from the weight of the ambient air . . . and partly from a kind of spring." This suggested that, since cohesion was due to the "pressure of the air" or "the sustaining power of the air," the removal of the air from the receiver of the air-pump would

[55] Boyle, "History of Fluidity and Firmness," p. 409; idem, "New Experiments," pp. 11, 15-18, 69, 76.

[56] Boyle, "Continuation of New Experiments," p. 276; idem, "Animadversions on Hobbes," p. 111.

[57] Boyle, "New Experiments," pp. 33-34. Compare Webster, "Discovery of Boyle's Law," p. 470: ". . . the spring of the air, which [Boyle] now terms its pressure."

produce the separation of the cohering marbles. This trial failed, but the evidence of this failure was later used to demonstrate "the spring of the air even when rarified." In 1661 and 1662 Boyle continued to use "pressure" to embrace spring and weight in this experimental context. In *The History of Fluidity and Firmness* this usage was important, because Boyle offered an account of the cohesion of marbles that relied upon "the spring of the air" pressing upon the marbles isotropically, and *also* an account which relied upon "the pressure of the air considered as a weight." Yet Boyle used the term "pressure" for both.[58] In his response to Hobbes, Boyle still wrote that "the spring of the air may perform somewhat in the case proposed," though he emphasized that the weight of the air was more important, and continued to use the term "pressure of the fluid air" for the cause of cohesion.[59]

Second, Boyle used his term "pressure" when contesting the Scholastic argument from the *horror vacui.* Here "pressure" functioned as the *sole* alternative to an *unacceptable* mystification, whereas in the trials with marbles it functioned as a term that covered a *multiplicity* of *acceptable* explanations of a single phenomenon. In *New Experiments,* therefore, "the supposed aversation of nature to a vacuum" was presented as "accidental" and attributed to "the weight and fluidity, or at least flexility of the bodies here below; and partly, perhaps principally, of the air, whose restless endeavour to expand itself every way makes it either rush in itself or compel the interposed bodies into small spaces."[60] Finally, the spring and the weight of the air could not be easily disentangled, since one produced the other. Boyle wrote in *New Experiments* that the effects of spring were due to the release of compressed particles, and that this compression was itself due to the weight of the air. This claim was applied repeatedly in the accounts of the air-pump trials, and in each case the term "pressure" was used. In the later *Continuation* Boyle outlined the distinction between weight and pressure in a systematic fashion, *for the first time* in print. He attacked "the school-philosophers" and their use of *horror vacui;* he distinguished between the "gravity" and "the bare spring of the air," "which latter I now mention as a distinct thing from the other." Boyle acknowledged that his trials had *not* separated weight from spring, "since the weight of the upper parts of the air does, if I may so speak,

[58] Boyle, "History of Fluidity and Firmness," pp. 403-406.
[59] Boyle, "Examen of Hobbes," p. 227.
[60] Boyle, "New Experiments," p. 75.

bend the springs of the lower." Referring to the work in *New Experiments,* Boyle announced his intention of displaying the practically identical, but theoretically distinct, effects of "the pressure of all the superincumbent atmosphere acting as a weight" and "the pressure of a small portion of the air, included indeed (but without any new compression) acting as a spring." So "pressure" was to be read as an embracing term, and its ambiguities and variation of meaning were themselves a resource that Boyle used in debating the air-pump trials, notably those of the cohering marbles and of the enclosure of the mercury barometer in the receiver.[61]

Witnessing Science

We have begun to develop the idea that experimental knowledge production rested upon a set of *conventions* for generating matters of fact and for handling their explications. Taking the matter of fact as foundational to the experimental form of life, let us proceed to analyze and display how the conventions of generating the fact actually worked. In Boyle's view the capacity of experiments to yield matters of fact depended not only upon their actual performance but essentially upon the assurance of the relevant community that they had been so performed. He therefore made a vital distinction between actual experiments and what are now termed "thought experiments."[62] If knowledge was to be empiri-

[61] Ibid., pp. 13, 16; idem, "Continuation of New Experiments," pp. 176-177.

[62] See, for instance, Boyle, "Sceptical Chymist," p. 460: here Boyle suggested that many experiments reported by the alchemists "questionless they never tried." For an insinuation that Henry More may not actually have performed experiments adduced against Boyle's findings, see Boyle, "Hydrostatical Discourse," pp. 607-608. Compare the response of Boyle to Pascal's trials of the Puy-de-Dôme experiment ("New Experiments," p. 43); and by Power, Towneley, and himself ("Defence against Linus," pp. 151-155). Yet Boyle doubted the reality of Pascal's other reports of underwater trials; see "Hydrostatical Paradoxes," pp. 745-746: ". . . though the experiments [Pascal] mentions be delivered in such a manner, as is usual in mentioning matters of fact; yet I remember not, that he expressly says, that he actually tried them, and therefore he might possibly have set them down, as things that *must* happen, upon a just confidence, that he was not mistaken in his ratiocinations. . . . Whether or no Monsieur Pascal ever made these experiments himself, he does not seem to have been very desirous, that others should make them after him." For the report by Pascal that drew Boyle's censure, see Barry, *Physical Treatises of Pascal,* pp. 20-21; for the role of thought experiments in the history of science: Koyré, *Galileo Studies,* p. 97; Kuhn, "A Function for Thought Experiments"; Schmitt, "Experience and Experiment."

cally based, as Boyle and other English experimentalists insisted it should, then its experimental foundations had to be *witnessed*. Experimental performances and their products had to be attested by the testimony of eye witnesses. Many phenomena, and particularly those alleged by the alchemists, were difficult to accept by those adhering to the corpuscular and mechanical philosophies. In these cases Boyle averred "that they that have seen them can much more reasonably believe them, than they that have not."[63] The problem with eye witnessing as a criterion for assurance was one of *discipline*. How did one police the reports of witnesses so as to avoid radical individualism? Was one obliged to credit a report on the testimony of any witness whatsoever?

Boyle insisted that witnessing was to be a collective act. In natural philosophy, as in criminal law, the reliability of testimony depended upon its multiplicity:

> For, though the testimony of a single witness shall not suffice to prove the accused party guilty of murder; yet the testimony of two witnesses, though but of equal credit . . . shall ordinarily suffice to prove a man guilty; because it is thought reasonable to suppose, that, though each testimony single be but probable, yet a concurrence of such probabilities, (which ought in reason to be attributed to the truth of what they jointly tend to prove) may well amount to a moral certainty, *i.e.*, such a certainty, as may warrant the judge to proceed to the sentence of death against the indicted party.[64]

And Sprat, in defending the reliability of the Royal Society's judgments in matters of fact, inquired

> whether, seeing in all Countreys, that are govern'd by Laws, they expect no more, than the consent of two, or three witnesses, in matters of life, and estate; they will not think, they are fairly dealt withall, in what concerns their *Knowledg*, if they have the concurring Testimonies of *threescore or an hundred?*[65]

The thrust of the legal analogy should not be missed. It was not merely that one was multiplying authority by multiplying witnesses

[63] Boyle, "Unsuccessfulness of Experiments," p. 343; idem, "Sceptical Chymist," p. 486; cf. idem, "Animadversions on Hobbes," p. 110.

[64] Boyle, "Some Considerations about Reason and Religion," p. 182; see also Daston, *The Reasonable Calculus*, pp. 90-91; on testimony, see Hacking, *The Emergence of Probability*, chap. 3; on evidence in seventeenth-century English law, see B. Shapiro, *Probability and Certainty*, chap. 5.

[65] Sprat, *History*, p. 100.

(although this was part of the tactic); it was that *right action* could be taken, and seen to be taken, on the basis of these collective testimonies. The action concerned the voluntary giving of assent to matters of fact. The multiplication of witness was an indication that testimony referred to a true state of affairs in nature. Multiple witnessing was accounted an active licence rather than just a descriptive licence. Did it not force the conclusion that such and such an action was done (a specific trial), and that subsequent action (offering assent) was warranted?

In experimental practice one way of securing the multiplication of witnesses was to perform experiments in a social space. The experimental "laboratory" was contrasted to the alchemist's closet precisely in that the former was said to be a public and the latter a private space.[66] Air-pump trials, for instance, were routinely performed in the Royal Society's ordinary assembly rooms, the machine being brought there specially for the occasion. (We shall see in chapter 4 that one of the ways by which Hobbes attacked the experimental programme was to deny the Society's claim that this *was* a public place.) In reporting upon his experimental performances Boyle commonly specified that they were "many of them tried in the presence of ingenious men," or that he made them "in the

[66] The terms "laboratory" and "elaboratory" (etymologically: a place where the work is done) were very new in seventeenth-century England. The first use of the former recorded in the *Oxford English Dictionary* was in Thomas Timme's edition of DuChesne's *Practise of Chymicall and Hermeticall Physicke* (1605), part 3, sig Bb4^r (where the reference was to a place for keeping things secret); the first use of the latter was in John Evelyn's *State of France as It Stood in the IXth Year of Lewis XIII* (1652). It is plausible that the usage entered England from French and German iatrochemistry, and, thus, at least initially, that it had Paracelsian resonances. For Timme (or Tymme) as the leading ideologue of Paracelsian theory, see Debus, *The English Paracelsians*, pp. 87-97. For an exemplary use of "laboratory" to refer to a closed, private space, see Gabriel Plattes, "Caveat for Alchymists," in Hartlib, *Chymical, Medicinal and Chyrurgical Addresses* (1655; composed 1642-1643), p. 87: "A Laboratory, like to that in the City of *Venice*, where they are sure of secrecy, by reason that no man is suffered to enter in, unless he can be contented to remain there, being surely provided for, till he be brought forth to go to the Church to be buried." Compare Geoghegan, "Plattes' Caveat for Alchymists." For the "universal laboratory" developed in London by Hartlib, Clodius and Digby, see Hartlib to Boyle, 8/18 May and 15/25 May 1654, in Boyle, *Works*, vol. VI, pp. 86-89, and Clodius to Boyle, 12/22 December 1663, in Maddison, *Life of Boyle*, p. 87. For a list of the new open laboratories established in London in the 1650s and 1660s, including that of the King at Whitehall, see Gunther, *Early Science in Oxford*, vol. I, pp. 36-42; also Webster, *The Great Instauration*, pp. 48, 239, 302-303. Thomas Birch praised Boyle because "his laboratory was constantly open to the curious," while noting that Boyle suppressed his own work in poisons and on invisible or erasable ink: Boyle, *Works*, vol. I, p. cxlv.

presence of an illustrious assembly of virtuosi (who were spectators of the experiment)."[67] Boyle's collaborator Hooke codified the Royal Society's procedures for the standard recording of experiments: the register was "to be sign'd by a certain Number of the Persons present, who have been present, and Witnesses of all the said Proceedings, who, by Sub-scribing their Names, will prove undoubted Testimony."[68] And Thomas Sprat described the role of the "Assembly" in "resolv[ing] upon the matter of *Fact*" by collectively correcting individual idiosyncrasies of observation and judgment. The Society made "the whole process pass under its own eyes."[69] In reporting experiments that were particularly important or problematic, Boyle named his witnesses and stipulated their qualifications. Thus the experiment of the original air-pump trials that was "the principal fruit I promised myself from our engine" was conducted in the presence of "those excellent and deservedly famous Mathematic Professors, Dr. *Wallis*, Dr. *Ward*, and Mr. *Wren* . . . , whom I name, both as justly counting it an honour to be known to them, and as being glad of such judicious and illustrious witnesses of our experiment."[70] Another important experiment was attested to by Wallis "who will be allowed to be a very competent judge in these matters."[71] And in his censure of the alchemists Boyle generally warned natural philosophers not "to believe chymical experiments . . . unless he, that delivers that, mentions his doing it upon his own particular knowledge, or upon the relation of some credible person, avowing it upon his own experience." Alchemists were recommended to name the putative author of these experiments "upon whose credit they relate" them.[72] The credibility of witnesses followed the taken-for-granted conventions of that setting for assessing individuals' reliability and trustworthiness: Oxford professors were accounted more reliable witnesses than Oxfordshire peasants. The natural philosopher had no option but to rely for a substantial part of his knowledge on the testimony of witnesses; and, in assessing that testimony, he (no less than judge or jury) had to determine their credibility. This necessarily involved

[67] Boyle, "New Experiments," p. 1; idem, "History of Fluidity and Firmness," p. 410; idem, "Defence against Linus," p. 173.

[68] Hooke, *Philosophical Experiments and Observations*, pp. 27-28.

[69] Sprat, *History*, pp. 98-99, 84; see also B. Shapiro, *Probability and Certainty*, pp. 21-22; Glanvill, *Scepsis scientifica*, p. 54 (on experiments as a corrective to sense).

[70] Boyle, "New Experiments," pp. 33-34.

[71] Boyle, "Discovery of the Admirable Rarefaction of Air," p. 498.

[72] Boyle, "Sceptical Chymist," p. 460.

their moral constitution as well as their knowledgeability, "for the two grand requisites, of a witness [are] the knowledge he has of the things he delivers, and his faithfulness in truly delivering what he knows." Thus the giving of witness in experimental philosophy traversed the social and moral accounting systems of Restoration England.[73]

Another important way of multiplying witnesses to experimentally produced phenomena was to facilitate their *replication.* Experimental protocols could be reported in such a way as to enable readers of the reports to perform the experiments for themselves, thus ensuring distant but direct witnesses. Boyle elected to publish several of his experimental series in the form of letters to other experimentalists or potential experimentalists. The *New Experiments* of 1660 was written as a letter to his nephew, Lord Dungarvan; the various tracts of the *Certain Physiological Essays* of 1661 were written to another nephew, Richard Jones; the *History of Colours* of 1664 was originally written to an unspecified friend.[74] The purpose of this form of communication was explicitly to proselytize. The *New Experiments* was published so "that the person I addressed them to might, without mistake, and with as little trouble as possible, be able to repeat such unusual experiments. . . ."[75] The *History of Colours* was designed "not barely to relate [the experiments], but . . . to teach a young gentleman to make them."[76] Boyle wished to encourage young gentlemen to "addict" themselves to experimental pursuits and thereby to multiply both experimental philosophers and experimental facts.

In Boyle's view, replication was rarely accomplished. When he came to publish the *Continuation of New Experiments* more than eight years after the original air-pump trials, Boyle admitted that, despite his care in communicating details of the engine and his procedures, there had been few successful replications.[77] This situation had not

[73] Boyle, "The Christian Virtuoso," p. 529; also B. Shapiro, *Probability and Certainty*, chap. 5, esp. p. 179. For the role of social accounting systems in the evaluation of observation reports, see Westrum, "Science and Social Intelligence about Anomalies: The Case of Meteorites."

[74] M. B. Hall, *Boyle and Seventeenth-Century Chemistry*, pp. 40-41.

[75] Boyle, "New Experiments," p. 2.

[76] Boyle, "The Experimental History of Colours," p. 663. Certain "easy and recreative experiments, which require but little time, or charge, or trouble in the making" were recommended to be tried by ladies (p. 664).

[77] Boyle, "Continuation of New Experiments," p. 176 (dated 24 March 1667 [o.s.]; published 1669). In chapter 6 we discuss some interesting problems of replication involving Huygens' air-pump in Holland during the 1660s.

materially changed by the mid-1670s. In the seven or eight years after the *Continuation*, Boyle said that he had heard "of very few experiments made, either in the engine I used, or in any other made after the model thereof." Boyle now expressed despair that these experiments would ever be replicated. He said that he was now even more willing "to set down divers things with their minute circumstances" because "probably many of these experiments would be never either re-examined by others, or re-iterated by myself." Anyone who set about trying to replicate such experiments, Boyle said, "will find it no easy task."[78]

Prolixity and Iconography

The third way by which witnesses could be multiplied is far more important than the performance of experiments before direct witnesses or the facilitating of their replication: it is what we shall call *virtual witnessing*. The technology of virtual witnessing involves the production in a *reader's* mind of such an image of an experimental scene as obviates the necessity for either direct witness or replication.[79] Through virtual witnessing the multiplication of witnesses could be, in principle, unlimited. It was therefore the most powerful technology for constituting matters of fact. The validation of experiments, and the crediting of their outcomes as matters of fact, necessarily entailed their realization in the laboratory of the mind and the mind's eye. What was required was a technology of trust and assurance that the things had been done and done in the way claimed.

The technology of virtual witnessing was not different in kind to that used to facilitate actual replication. One could deploy the same linguistic resources in order to encourage the physical replication of experiments or to trigger in the reader's mind a naturalistic image of the experimental scene. Of course, actual replication was to be preferred, for this eliminated reliance upon testimony altogether. Yet, because of natural and legitimate sus-

[78] Boyle, "Continuation of New Experiments. The Second Part," pp. 505, 507 (1680).

[79] We prefer this term to van Leeuwen's "vicarious experience": we wish to preserve the notion that virtual witnessing is a positive action, whereas vicarious experience is commonly held not to be proper experience at all; see van Leeuwen, *The Problem of Certainty*, pp. 97-102; Hacking, *The Emergence of Probability*, chaps. 3-4.

picion among those who were neither direct witnesses nor replicators, a greater degree of assurance was required to produce assent in virtual witnesses. Boyle's literary technology was crafted to secure this assent.

In order to understand how Boyle deployed the literary technology of virtual witnessing, we have to reorient some of our common ideas about the scientific text. We usually think of an experimental report as a narration of some prior visual experience: it points to sensory experiences that lie behind the text. This is correct. However, we should also appreciate that the text itself constitutes a visual source. It is our task here to see how Boyle's texts were constructed so as to provide a source of virtual witness that was agreed to be reliable. The best way to fasten upon the notion of the text as this kind of source might be to start by looking at some of the pictures that Boyle provided alongside his prose.

Figure 1, for example, is an engraving of his original air-pump, appended to the *New Experiments*. Producing these kinds of images was an expensive business in the mid-seventeenth century and natural philosophers used them sparingly. As we see, figure 1 is not a schematized line drawing but an attempt at detailed naturalistic representation complete with the conventions of shadowing and cut-away sections of the parts. This is not a picture of the "idea" of an air-pump, but of a particular existing air-pump.[80] And the same applies to Boyle's pictorial representations of his pneumatic experiments: in one engraving we are shown a mouse lying dead in the receiver; in another, images of the experimenters. Boyle devoted great attention to the manufacture of these images, sometimes consulting directly with the engraver, sometimes by way of Hooke.[81] Their role was to be a supplement to the imaginative witness provided by the words in the text. In the *Continuation* Boyle expanded upon the relationships between the two sorts of exposition; he told his readers that "they who either were versed in such kind of studies or have any peculiar facility of imagining, would well enough conceive my meaning only by words," but others required visual assistance. He apologized for the relative poverty of the images, "being myself absent from the engraver for a good

[80] For studies of engraving and print-making in scientific texts, see Ivins, *Prints and Visual Communication*, esp. pp. 33-36; Eisenstein, *The Printing Press as an Agent of Change*, esp. pp. 262-270, 468-471. We briefly treat Hobbes's iconography in chapter 4.

[81] Hooke to Boyle, 25 August/4 September and 8/18 September 1664, in Boyle, *Works*, vol. VI, pp. 487-490, and Maddison, "The Portraiture of Boyle."

part of the time he was at work, some of the cuts were misplaced, and not graven in the plates."[82]

So visual representations, few as they necessarily were in Boyle's texts, were mimetic devices. By virtue of the density of *circumstantial detail* that could be conveyed through the engraver's laying of lines, they imitated reality and gave the viewer a vivid impression of the experimental scene. The sort of naturalistic images that Boyle favoured provided a greater density of circumstantial detail than would have been proffered by more schematic representations. The images served to announce, as it were, that "this was really done" and that "it was done in the way stipulated"; they allayed distrust and facilitated virtual witnessing. Therefore, understanding the role of pictorial representations offers a way of appreciating what Boyle was trying to achieve with his literary technology.[83]

In the introductory pages of *New Experiments*, Boyle's first published experimental findings, he directly announced his intention to be "somewhat prolix." His excuses were threefold: first, delivering things "circumstantially" would, as we have already seen, facilitate replication; second, the density of circumstantial detail was justified by the fact that these were "new" experiments, with novel conclusions drawn from them: it was therefore necessary that they be "circumstantially related, to keep the reader from distrusting them"; third, circumstantial reports such as these offered the possibility of virtual witnessing. As Boyle said, "these narratives [are to be] as standing records in our new pneumatics, and [readers] need not reiterate themselves an experiment *to have as distinct an idea of it*, as may suffice them to ground their reflexions and speculations upon."[84] If one wrote experimental reports in the correct way, the reader could take on trust that these things happened. Further, it would be as if that reader had been present at the

[82] Boyle, "Continuation of New Experiments," p. 178.

[83] Compare Alpers, *The Art of Describing*, which analyzes the purposes and conventions of realistic pictures in seventeenth-century Holland, demonstrating substantial links between English empiricist theories of knowledge and Dutch picturing. Evidently, the Dutch were trying to achieve by way of picturing what the English were attempting through the reform of prose.

[84] Boyle, "New Experiments," pp. 1-2 (emphases added). The function of circumstantial detail in the prose of Boyle and other Fellows of the Royal Society is also treated in B. Shapiro, *Probability and Certainty*, chap. 7; Lupoli, "La polemica tra Hobbes e Boyle," p. 329; Dear, "*Totius in verba*: The Rhetorical Constitution of Authority in the Early Royal Society"; and Golinski, *Language, Method and Theory in British Chemical Discourse*. We are very grateful to Dear and Golinski for allowing us to see their typescripts.

proceedings. He would be recruited as a witness and be put in a position where he could validate experimental phenomena as matters of fact.[85] Therefore, attention to the writing of experimental reports was of equal importance to doing the experiments themselves.

In the late 1650s Boyle devoted himself to laying down the rules for the literary technology of the experimental programme. Stipulations about how to write proper scientific prose were dispersed throughout his experimental reports of the 1660s, but he also composed a special tract on the subject of "experimental essays." Here Boyle offered an extended *apologia* for his "prolixity": "I have," he understated, "declined that succinct way of writing"; he had sometimes "delivered things, to make them more clear, in such a multitude of words, that I now seem even to myself to have in divers places been guilty of verbosity." Not just his "verbosity" but also Boyle's ornate sentence structure, with appositive clauses piled on top of each other, was, he said, part of a plan to convey circumstantial details and to give the impression of verisimilitude:

> . . . I have knowingly and purposely transgressed the laws of oratory in one particular, namely, in making sometimes my periods [i.e., complete sentences] or parentheses over-long: for when I could not within the compass of a regular period com-

[85] There is probably a connection between Boyle's justification of circumstantial reporting and Bacon's argument in favour of "initiative," as opposed to "magistral," methods of communication; see, for example, Hodges, "Anatomy as Science," pp. 83-84; Jardine, *Bacon: Discovery and the Art of Discourse*, pp. 174-178; Wallace, *Bacon on Communication & Rhetoric*, pp. 18-19. Bacon said that the magistral method "requires that what is told should be believed; the initiative that it should be examined." Initiative methods display the processes by which conclusions are reached; magistral methods mask those processes. Although Boyle's inspiration may, plausibly, have been Baconian, the "influence" of Bacon is sometimes exaggerated (e.g., Wallace, *Bacon on Communication & Rhetoric*, pp. 225-227). It is useful to remember that it was Boyle, not Bacon, who developed the literary forms for an actual programme of systematic experimentation; it is hard to imagine two more different forms than Bacon's aphorisms and Boyle's experimental narratives. See also a marvellously speculative paper on the *Cartesian* roots of contrasting styles of scientific exposition: Watkins, "Confession is Good for Ideas," and the better-known Medawar, "Is the Scientific Paper a Fraud?" For modern testimony to Boyle's success in winning readers' assurance, see Gillispie, *The Edge of Objectivity*, p. 103: "Truly experimental physics came into its own with Robert Boyle. He spared his reader no detail. No one could doubt that he performed all the experiments he reported . . . , bringing to his laboratory great ingenuity, incomparable patience, and that simple honesty which makes experiment really a respectful inquiry rather than an overbearing demonstration."

> prise what I thought requisite to be delivered at once, I chose rather to neglect the precepts of rhetoricians, than the mention of those things, which I thought pertinent to my subject, and useful to you, my reader.[86]

Elaborate sentences, with circumstantial details encompassed within the confines of one grammatical entity, might mimic that immediacy and simultaneity of experience afforded by pictorial representations.

Boyle was endeavouring to appear as a reliable purveyor of experimental testimony and to offer conventions by means of which others could do likewise. The provision of circumstantial details was a way of assuring readers that real experiments had yielded the findings stipulated. It was also necessary, in Boyle's view, to offer readers circumstantial accounts of *failed* experiments. This performed two functions: first, it allayed anxieties in those neophyte experimentalists whose expectations of success were not immediately fulfilled; second, it assured the reader that the relator was not wilfully suppressing inconvenient evidence, that he was in fact being faithful to reality. Complex and circumstantial accounts were to be taken as undistorted mirrors of complex experimental outcomes.[87] So, for example, it was not legitimate to hide the fact that air-pumps sometimes did not work properly or that they often leaked: ". . . I think it becomes one, that professeth himself a faithful relator of experiments not to conceal" such unfortunate contingencies.[88] It is, however, vital to keep in mind that in his circumstantial accounts Boyle proffered only a *selection* of possible contingencies. There was not, nor can there be, any such thing as a report that notes *all* circumstances that might affect an experi-

[86] Boyle, "Proëmial Essay," pp. 305-306, 316; cf. idem, "New Experiments," p. 1; Westfall, "Unpublished Boyle Papers." According to one literary historian, "though [Boyle] aims, like Dryden, to write as a cultured man would talk, his style is hurried and careless, and his sentences rattle on without form or elegance." (Horne, "Literature and Science," p. 193.)

[87] Boyle, "Unsuccessfulness of Experiments," esp. pp. 339-340, 353. Recognizing that contingencies might affect experimental outcomes was also a way of tempering inclinations to *reject* good testimony too readily: if an otherwise reliable source stipulated an outcome that was not immediately obtained, one was advised to persevere; see ibid., pp. 344-345; idem, "Continuation of New Experiments," pp. 275-276; idem, "Hydrostatical Paradoxes," p. 743; Westfall, "Unpublished Boyle Papers," pp. 72-73.

[88] Boyle, "New Experiments," p. 26; and recall Boyle's reporting of the failed experiment 31 (discussed above). In chapter 5 we return to the problem of success and failure in experiment.

ment. Circumstantial, or stylized, accounts do not, therefore, exist as pure forms but as publicly acknowledged moves towards or away from the reporting of contingencies.

The Modesty of Experimental Narrative

The ability of the reporter to multiply witnesses depended upon readers' acceptance of him as a provider of reliable testimony. It was the burden of Boyle's literary technology to assure his readers that he was such a man as should be believed. He therefore had to find the means to make visible in the text the accepted tokens of a man of good faith. One technique has just been discussed: the reporting of experimental failures. A man who recounted unsuccessful experiments was such a man whose objectivity was not distorted by his interests. Thus the literary display of a certain sort of morality was a technique in the making of matters of fact. A man whose narratives could be credited as mirrors of reality was a *modest man*; his reports ought to make that modesty visible. In treating the moral tone of experimental reporting we are therefore beginning to understand the relationship between Boyle's literary and social technologies. How experimentalists were to talk with each other was an important element in specifying the social relations that could constitute and protect experimental knowledge.

Boyle found a number of ways of displaying modesty. One of the most straightforward was the use of the *form* of the experimental essay. The essay, that is, the piecemeal reporting of experimental trials, was explicitly contrasted to the natural philosophical *system*. Those who wrote entire systems were identified as "confident" individuals, whose ambition extended beyond what was proper or possible. By contrast, those who wrote experimental essays were "sober and modest men," "diligent and judicious" philosophers, who did not "assert more than they can prove." This practice cast the experimental philosopher into the role of intellectual "under-builder," or even that of "a drudge of greater industry than reason." This was, however, a noble character, for it was one that was freely chosen to further "the real advancement of true natural philosophy" rather than personal reputation.[89] The public display of this

[89] Boyle, "Proëmial Essay," pp. 301-307, 300; cf. idem, "Sceptical Chymist," pp. 469-470, 486, 584. Within a year, Henry Power was quoting Boyle's formulations back to him: "I beseech you to looke upon us [Yorkshire experimentalists] as Countrey-Drudges of *much greater Industry than Reason.*" Power to Boyle, 10/20 November

modesty was an exhibition that concern for individual celebrity did not cloud judgment and distort the integrity of one's reports. In this connection it is absolutely crucial to remember who it was that was portraying himself as a mere "under-builder." Boyle was the son of the Earl of Cork, and everyone knew that very well. Thus, it was *plausible* that such modesty could have a noble aspect, and Boyle's presentation of self as a moral model for experimental philosophers was powerful.[90]

Another technique for showing modesty was Boyle's professedly "naked way of writing." He would eschew a "florid" style; his object was to write "rather in a philosophical than a rhetorical strain." This plain, ascetic, unadorned (yet convoluted) style was identified as *functional.* It served to display, once more, the philosopher's dedication to community service rather than to his personal reputation. Moreover, the "florid" style to be avoided was a hindrance to the clear provision of virtual witness: it was, Boyle said, like painting "the eye-glasses of a telescope."[91]

The most important literary device Boyle employed for demonstrating modesty acted to protect the fundamental epistemological category of the experimental programme: the matter of fact. There were to be appropriate moral postures, and appropriate modes of speech, for epistemological items on either side of the important boundary that separated matters of fact from the lo-

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1662, in British Library Sloane MSS 1326 f33^{v}. For natural philosophical textbooks, see Reif, "The Textbook Tradition in Natural Philosophy."

[90] Several of the less modest personalities of seventeenth-century English science were individuals who lacked the gentle birth that routinely enhanced the credibility of testimony: for instance, Hobbes, Hooke, Wallis, and Newton. The best source for Boyle's social situation and temperament is J. Jacob, *Boyle*, chaps. 1-2.

[91] Boyle, "Proëmial Essay," pp. 318, 304. For the importance of the lens and the perceptual model of knowledge in seventeenth-century theories of knowledge, see Alpers, *The Art of Describing*, chap. 3. For Boyle, as for many other philosophers concerned with the reform of language, the goal was "plain-speaking." For the linguistic programme of the early Royal Society and its connections with experimental philosophy, see Christensen, "Wilkins and the Royal Society's Reform of Prose Style"; R. F. Jones, "Science and Language"; idem, "Science and English Prose Style"; Salmon, "Wilkins' *Essay*"; Slaughter, *Universal Languages and Scientific Taxonomy*, esp. pp. 104-186; Aarsleff, *From Locke to Saussure*, pp. 225-277; B. Shapiro, *Probability and Certainty*, pp. 227-246; Hunter, *Science and Society*, pp. 118-119; Dear, "*Totius in verba*: The Rhetorical Constitution of Authority in the Early Royal Society." For Boyle's attack on the "confused," "equivocal," and "cloudy" language of the alchemists, see "Sceptical Chymist," esp. pp. 460, 520-522, 537-539; and, for his criticisms of Hobbes's expository "obscurity," see "Examen of Hobbes," p. 227, and our discussion in chapter 5.

cutions used to account for them: theories, hypotheses, speculations, and the like. Thus, Boyle told his nephew,

> . . . in almost every one of the following essays I . . . speak so doubtingly, and use so often, *perhaps, it seems, it is not improbable*, and such other expressions, as argue a diffidence of the truth of the opinions I incline to, and that I should be so shy of laying down principles, and sometimes of so much as venturing at explications.

Since knowledge of physical causes was only "probable," this was the correct moral stance and manner of speech, but things were otherwise with matters of fact, and here a confident mode was not only permissible but necessary: ". . . I dare speak confidently and positively of very few things, except of matters of fact."[92] Boyle specifically warned readers who expected physical statements to possess "a mathematical certainty and accurateness": ". . . in physical enquiries it is often sufficient, that our determinations come very near the matter, though they fall short of a mathematical exactness."[93]

It was necessary to speak confidently of matters of fact because, as the foundations of proper philosophy, they required protection. And it was proper to speak confidently of matters of fact because they were not of one's own making: they were, in the empiricist language-game, discovered rather than invented. As Boyle told one of his adversaries, experimental facts can "make their own way," and "such as were very probable, would meet with patrons and defenders."[94] The separation of moral modes of speech and the ability of facts to make their own way were made visible on the printed page. In *New Experiments* Boyle said he intended to leave "a conspicuous interval" between his narratives of experimental findings and his occasional "discourses" on their interpretation. One might then read the experiments and the "reflexions" separately.[95] Indeed, the construction of Boyle's experimental essays

[92] Boyle, "Proëmial Essay," p. 307; on "wary and diffident expressions," see also idem, "New Experiments," p. 2. Cf. Sprat, *History*, pp. 100-101; Glanvill, *Scepsis scientifica*, pp. 170-171. For treatments of Boyle's remarks in the context of probabilist and fallibilist models of knowledge, see B. Shapiro, *Probability and Certainty*, pp. 26-27; van Leeuwen, *The Problem of Certainty*, p. 103; Daston, *The Reasonable Calculus*, pp. 164-165.

[93] Boyle, "Hydrostatical Paradoxes," p. 741. Boyle was chastising Pascal in this context.

[94] Boyle, "Hydrostatical Discourse," p. 596.

[95] Boyle, "New Experiments," p. 2.

made manifest the proper separation and balance between the two categories: *New Experiments* consisted of a sequential narrative of forty-three pneumatic experiments; *Continuation* of fifty; and the second part of *Continuation* of an even larger number of disconnected experimental observations, only sparingly larded with interpretative locutions.

The confidence with which one ought to speak about matters of fact extended to stipulations about the proper use of authorities. Citations of other writers should be employed to use them not as "judges, but as witnesses," as "certificates to attest matters of fact." If such a practice ran the risk of identifying the experimental philosopher as an ill-read philistine, it was, for all that, necessary. As Boyle said, "I could be very well content to be thought to have scarce looked upon any other book than that of nature."[96] The injunction against the ornamental citing of authorities performed a significant function in the mobilization of assent to matters of fact. It was a way of displaying that one was aware of the workings of the Baconian "idols" and was taking measures to mitigate their corrupting effects on knowledge-claims.[97] A disengagement between experimental narrative and the authority of systematists served to dramatize the author's lack of preconceived expectations and, especially, of theoretical investments in the outcome of experiments. For example, Boyle several times insisted that he was an innocent of the great theoretical systems of the seventeenth century. In order to reinforce the primacy of experimental findings, "I had purposely refrained from acquainting myself thoroughly with the intire system of either the Atomical, or the Cartesian, or any other whether new or received philosophy." And, again, he claimed that he had avoided a systematic acquaintance with the systems of Gassendi, Descartes, and even of Bacon, "that I might not be prepossessed with any theory or principles."[98]

[96] Boyle, "Proëmial Essay," pp. 313, 317.

[97] On the "idols" and fallibilism, see B. Shapiro, *Probability and Certainty*, pp. 61-62.

[98] Boyle, "Some Specimens of an Attempt to Make Chymical Experiments Useful," p. 355; idem, "Proëmial Essay," p. 302; on the corrupting effects of "preconceived hypothesis or conjecture," see idem, "New Experiments," p. 47, and, for doubts about the correctness of Boyle's professed unfamiliarity with Descartes and other systematists, see Westfall, "Unpublished Boyle Papers," p. 63; Laudan, "The Clock Metaphor and Probabilism," p. 82n; M. B. Hall, "The Establishment of the Mechanical Philosophy," pp. 460-461; idem, *Boyle and Seventeenth-Century Chemistry*, chap. 3; idem, "Boyle as a Theoretical Scientist"; idem, "Science in the Early Royal Society," pp. 72-73; Kargon, *Atomism in England*, chap. 9; Frank, *Harvey and the*

Boyle's "naked way of writing," his professions and displays of humility, and his exhibition of theoretical innocence all complemented each other in the establishment and the protection of matters of fact. They served to portray the author as a disinterested observer and his accounts as unclouded and undistorted mirrors of nature. Such an author gave the signs of a man whose testimony was reliable. Hence, his texts could be credited and the number of witnesses to his experimental narratives could be multiplied indefinitely.

Scientific Discourse and Community Boundaries

We have argued that the matter of fact was a social as well as an intellectual category, and we have shown that Boyle deployed his literary technology so as to make virtual witnessing a practical option for the validation of experimental performances. In this section we want to examine the ways in which Boyle's literary technology dramatized the social relations proper to a community of experimental philosophers. Only by establishing right rules of discourse could matters of fact be generated and defended, and only by constituting these matters of fact into the agreed foundations of knowledge could a moral community of experimentalists be created and sustained. Matters of fact were to be produced in a public space: a particular physical space in which experiments were collectively performed and directly witnessed and an abstract space constituted through virtual witnessing. The problem of producing this kind of knowledge was, therefore, the problem of maintaining a certain form of discourse and a certain mode of social solidarity.

In the late 1650s and early 1660s, when Boyle was formulating his experimental and literary practices, the English experimental community was still in its infancy. Even with the founding of the Royal Society, the crystallization of an experimental community centred on Gresham College, and the network of correspondence organized by Henry Oldenburg, the experimental programme was far from securely institutionalized. Criticisms of the experimental way of producing physical knowledge emanated from English philosophers (notably Hobbes) and from Continental writers committed to rationalist methods and to the practice of natural philosophy

Oxford Physiologists, pp. 93-97. Our concern here is not with the veracity of Boyle's professions but with the reasons he made them and the purposes they were designed to serve.

as a demonstrative discipline.[99] Experimentalists were made into figures of fun on the Restoration stage: Thomas Shadwell's *The Virtuoso* dramatized the absurdity of weighing the air, and scored many of its jokes by parodying the convoluted language of Sir Nicholas Gimcrack (Boyle). The practice of experimental philosophy, despite what numerous historians have assumed, was not overwhelmingly popular in Restoration England.[100] In order for experimental philosophy to be established as a legitimate activity, several things needed to be done. First, it required recruits: experimentalists had to be enlisted as neophytes, and converts from other forms of philosophical practice had to be obtained. Second, the social role of the experimental philosopher and the linguistic practices appropriate to an experimental community needed to be defined and publicized.[101] What was the proper nature of discourse in such a community? What were the linguistic signs of competent membership? And what uses of language could be taken as indications that an individual had transgressed the conventions of the community?

The entry fee to the experimental community was to be the communication of a candidate matter of fact. In *The Sceptical Chymist*, for instance, Boyle extended an olive branch even to the alchemists. The solid experimental findings produced by some alchemists could be sifted from the dross of their "obscure" speculations. Since the experiments of the alchemists (and the few experiments of the Aristotelians) frequently "do not evince what they are alleged to prove," the former might be accepted into the experimental philosophy by stripping away the theoretical language with which they happened to be glossed. As Carneades (Boyle's mouthpiece) said,

[99] For a major Continental critique, see R. McKeon, *Philosophy of Spinoza*, chap. 4; A. R. Hall and M. B. Hall, "Philosophy and Natural Philosophy: Boyle and Spinoza"; and, for an English attack related to Hobbes's, see J. Jacob, *Stubbe*, esp. pp. 84-108.

[100] For the extent to which experimental philosophy was "popular," see Hunter, *Science and Society*, esp. chaps. 3, 6. Shadwell's play was performed in 1676; as we shall see in chapter 4, Charles II, the Society's royal patron, was also said to have found the weighing of the air rather funny, and Petty was aware of pneumatic satire in the early 1670s: A. R. Hall, "Gunnery, Science, and the Royal Society," pp. 129-130. There is some evidence that Hooke believed *he* was Gimcrack: Westfall, "Hooke," p. 483.

[101] This is not intended as an exhaustive catalogue of the measures required for institutionalization. Clearly, patronage was necessary and alliances had to be forged with existing powerful institutions.

your hermetic philosophers present us, together with divers substantial and noble experiments, theories, which either like peacocks feathers make a great shew, but are neither solid nor useful; or else like apes, if they have some appearance of being rational, are blemished with some absurdity or other, that, when they are attentively considered, make them appear ridiculous.[102]

Thus those alchemists who wished to be incorporated into a legitimate philosophical community were instructed what linguistic practices could secure their admission. Boyle laid down the same principles with respect to any practitioner: "Let his opinions be never so false, his experiments being true, I am not obliged to believe the former, and am left at liberty to benefit myself by the latter."[103] By arguing that there was only a contingent, not a necessary, connection between the language of theory and the language of facts, Boyle was defining the linguistic terms on which existing communities could join the experimental programme.

They were liberal terms, which might serve to maximize potential membership. Boyle's way of dealing with the Hermetics drew on the views of the Hartlib group of the late 1640s and 1650s. By contrast, there were those who rejected the findings of late alchemy (e.g., Hobbes) and those who rejected the process of assimilation (e.g., Newton). The debt to the Hartlib group is important. *The Sceptical Chymist* was drafted before summer 1658 as "Reflexions" on Peripatetic and Paracelsian chemical theory. Precedents existed for the style and tone of the dialogue in Mersenne's *Vérité des sciences* (1625), a conversation between a Christian philosopher, a sceptic, and an alchemist in which an *open* alchemical college was proposed; in Plattes' *Caveat for Alchymists* (1655), published along with Boyle's invitation to open communication in alchemy and physic, where Plattes referred to attempts to demonstrate transmutation before Parliament; and in Renaudot's *Conference concerning the Philosopher's Stone*, published in the same Hartlibian volume, in which seven men—some sceptics, some believers—publicly disputed the possibility of transmutation. Boyle distanced himself somewhat from the group in 1655-1656 when he moved to Oxford to initiate the work on air and saltpetre. But he continued his commitment to the absorption of alchemy within the rules of experimental discourse. The contrast with Newton is instructive. He behaved in an appro-

[102] Boyle, "Sceptical Chymist," pp. 468, 513, 550, 584.
[103] Boyle, "Proëmial Essay," p. 303.

priate but totally distinct manner in alchemy and in experimental philosophy, while Boyle laboured to bring alchemy into the public domain: hence Boyle's 1670s *publications* on alchemy and Newton's criticisms of Boyle's decision to publish.[104]

There were other natural philosophers Boyle despaired to recruit and to assimilate. As we shall see, Hobbes was the sort of philosopher who on no account ought to be admitted to the experimental companionship, for he denied the value of systematic and elaborate experimentation as well as the foundational status of the fact and the distinction between causal and descriptive language. The experimental and the rationalistic language-games were perceived to be radically incompatible. There could be no rapprochement between them, only a choice between the one and the other.

Manners in Dispute

Since experimental philosophers were not to be compelled to give assent to all items of knowledge, dispute and disagreement were to be expected. The task was to manage such dissensus by confining it within safe boundaries. Disagreement about causal explanations might be rendered safe insofar as it was accepted that such items were not foundational. What was neither safe nor permissible was dispute over matters of fact or over the rules of the game by which matters of fact were experimentally produced.

The problem of conducting dispute was a matter of serious practical concern in early Restoration science. During the Civil War and Interregnum "enthusiasts," hermeticists and sectaries threatened

[104] Compare Boyle, "Experimental Discourse of Quicksilver Growing Hot with Gold" (1676) and "An Historical Account of a Degradation of Gold" (1678) with Newton to Oldenburg, 26 April/6 May 1676, in Newton, *Correspondence*, vol. II, pp. 1-3. For Boyle's intention to compose "a short essay concerning chemistry," and a comment on the degradation of gold, see Hartlib to Boyle, 28 February/ 10 March 1654, in Boyle, *Works*, vol. VI, p. 79. For Boyle and the Hartlib group: O'Brien, "Hartlib's Influence on Boyle's Scientific Development"; Rowbottom, "Earliest Published Writing of Boyle"; Webster, "English Medical Reformers"; Wilkinson "The Hartlib Papers." Dobbs, *Foundations of Newton's Alchemy*, p. 72, writes that Boyle and Hartlib moved alchemy "into the area of public dialogue where assumptions underlying alchemical theory could be subjected to a critical analysis. . . . And conceptual scrutiny was being paralleled elsewhere in the group by a more open communication of empirical information." For sources of *The Sceptical Chymist*, see M. B. Hall, "An Early Version of Boyle's 'Sceptical Chymist'," which dates the "Reflexions" to 1657, and Webster, "Water as the Ultimate Principle of Nature," which gives the latest date as summer 1658.

to bring about a radical individualism in knowledge: a situation in which "private judgment" eroded any existing authority and the credibility of any existing institutionalized conventions for generating valid knowledge. Nor did the various sects of Peripatetic natural philosophers display a public image of a stable and united intellectual community. The "litigiousness" of Scholastic philosophers was commonly noted by their experimentalist critics.[105] Unless the experimental community could exhibit a broadly based harmony and consensus within its own ranks, it was unreasonable to expect it to secure the legitimacy within Restoration culture that its leaders desired. Moreover, that very consensus was vital to the establishment of matters of fact as the foundational category of the new practice.

By the early 1660s Boyle was in a position to give concrete exemplars of how disputes in natural philosophy ought to be managed. Three adversaries entered the lists, each objecting to aspects of his *New Experiments*. In chapters 4 and 5 we shall see what their objections were and how Boyle responded to each one: Hobbes, Linus, and Henry More. But even before he had been publicly engaged in dispute, Boyle laid down a set of rules for how controversies were to be handled by the experimental philosopher. For example, in *Proëmial Essay* (published 1661, composed 1657), Boyle went to great lengths to lay down the moral conventions that ought to regulate controversy. Disputes should be about findings and not about persons. It was proper to take a hard view of reports that were inaccurate but most improper to attack the character of those that rendered them, "for I love to speak of persons with civility, though of things with freedom." The *ad hominem* style must at all costs be avoided, for the risk was that of making foes out of mere dissenters. This was the key point: potential contributors of matters of fact, however misguided they might be, must be treated as possible converts to the experimental form of life. If, however, they were harshly dealt with, they would be lost to the cause and to the community whose size and consensus validated matters of fact:

> And as for the (very much too common) practice of many, who write, as if they thought railing at a man's person, or wrangling about his words, necessary to the confutation of his

105 On Peripatetic litigiousness, see, for example, Boyle, "The Christian Virtuoso," p. 523, and Glanvill, *Scepsis scientifica*, pp. 136-137; on opposition to the sectaries' individualism, see J. Jacob, *Boyle*, chap. 3; and, for general background, see Heyd, "The Reaction to Enthusiasm in the Seventeenth Century."

> opinions; besides that I think such a quarrelsome and injurious way of writing does very much misbecome both a philosopher and a Christian, methinks it is as unwise, as it is provoking. For if I civilly endeavour to reason a man out of his opinions, I make myself but one work to do, namely, to convince his understanding; but, if in a bitter or exasperating way I oppose his errors, I increase the difficulties I would surmount, and have as well his affections against me as his judgment: and it is very uneasy to make a proselyte of him, that is not only a dissenter from us, but an enemy to us.[106]

Furthermore, even the acknowledgment that natural philosophical sects in fact existed might be impolitic. Excessive talk about sects might work to ensure their survival: "It is none of my design," Boyle said, "to engage myself with, or against, any one sect of Naturalists." The *experiments* would decide the case. The views of sects should be noticed only insofar as they were founded upon experiment. Thus it was right and politic to be severe in one's writings against those who did not contribute experimental findings, for they had nothing to offer to the constitution of matters of fact. Yet the experimental philosopher must show that there was point and purpose to legitimately conducted dispute. He should be prepared publicly to renounce positions that were shown to be erroneous. Flexibility followed from fallibilism. As Boyle wrote, "Till a man is sure he is infallible, it is not fit for him to be unalterable."[107]

The conventions for managing disputes were dramatized in the structure of *The Sceptical Chymist*. These fictional conversations (between an Aristotelian, two varieties of Hermetics, and Carneades as mouthpiece for Boyle) took the form, not of a Socratic dialogue, but of a *conference*.[108] They were a piece of theatre that exhibited how persuasion, dissensus and, ultimately, conversion to truth ought to be conducted. Several points about Boyle's theatre of persuasion can be briefly made: first, the symposiasts are imaginary, not real. This means that opinions can be confuted without exacerbating relations between real philosophers. Even Carneades, although he is manifestly "Boyle's man," is not Boyle himself: Carneades is made actually to quote "our friend Mr. *Boyle*" as a device for distancing opinions from individuals. The *author* is insulated

[106] Boyle, "Proëmial Essay," p. 312.
[107] Ibid., p. 311.
[108] See Multhauf, "Some Nonexistent Chemists."

from the text and from the opinions he may actually espouse.[109] Second, truth is not inculcated from Carneades to his interlocutors; rather it is dramatized as emerging through the conversation. Everyone is seen to have a say in the consensus which is the *dénouement.*[110] Third, the conversation is, without exception, *civil*: as Boyle said, "I am not sorry to have this opportunity of giving an example, how to manage even disputes with civility."[111] No symposiast abuses another; no ill temper is displayed; no one leaves the conversation in pique or frustration.[112] Fourth, and most important, the currency of intellectual exchange, and the means by which agreement is reached, is the experimental matter of fact. Here, as we have already indicated, matters of fact are not treated as the exclusive property of any one philosophical sect. Insofar as the alchemists have produced experimental findings, they have minted the real coins of experimental exchange. Their experiments are welcome, while their "obscure" speculations are not. Insofar as the Aristotelians produce few experiments, and insofar as they refuse to dismantle the "arch"-like "mutual coherence" of their system into facts and theories, they can make little contribution to the experimental conference.[113] In these ways, the structure and the linguistic rules of this imaginary conversation make vivid the rules for real conversations proper to experimental philosophy.

In subsequent chapters we discuss the real disputes that followed hard upon the imaginary ones of *The Sceptical Chymist.* Franciscus Linus was the adversary who experimented but who denied the power of the spring of the air; Henry More was the adversary whom Boyle wished to be an ally: More offered what he reckoned to be a more theologically appropriate account of Boyle's pneumatic

[109] Boyle, "Sceptical Chymist," p. 486. Boyle said in the preface that he would not "declare my own opinion"; he wished to be "a silent auditor of their discourses" (pp. 460, 466-467).

[110] The consensus that emerges is very like the position from which Carneades starts, but the plot of *The Sceptical Chymist* involved disguising that fact. Interestingly, the consensus is not *total* (as Jan Golinski has pointed out): Eleutherius indicates reservations about Carneades' arguments, and Philoponus (a more "hard-line" alchemist who is absent for the bulk of the proceedings) might not, in Eleutherius's opinion, have been persuaded. In later chapters we draw the contrast between the form and use of the dialogue by Boyle's anti-experimentalist adversary Hobbes.

[111] Boyle, "Sceptical Chymist," p. 462.

[112] Actually, the great bulk of the talk is between Carneades and Eleutherius. The other two participants inexplicably absent themselves during much of the symposium. This is possibly an accident of Boyle's self-confessed sloppiness with his manuscripts; see Multhauf, "Some Nonexistent Chemists," pp. 39-41.

[113] Boyle, "Sceptical Chymist," p. 469.

findings; but Hobbes was the adversary who denied the value of experiment and the foundational status of the matter of fact. Each carefully crafted response that Boyle produced was labelled as a model for how disputes should be managed by the experimental philosopher. In each response Boyle professed that his concern was not the defence of his reputation but the protection of what was vital to the collective practice of proper philosophy: the value of systematic experimentation (especially that employing "elaborate" instruments such as the air-pump), the matters of fact that experiment produced, the boundaries that separated those facts from less certain epistemological items, and the rules of social life that regulated discourse in the experimental community. The object of controversy, in Boyle's stipulation, was not fact but the interpretation of fact. And the moral tone of philosophical controversy was to be civil and liberal.

What was at stake in these controversies was the creation and the preservation of a calm space in which natural philosophers could heal their divisions, collectively agree upon the foundations of knowledge, and thereby establish their credit in Restoration culture. A calm space was essential to achieving these goals. As Boyle reminded his readers in the introdution to *New Experiments* (published in that "wonderful, pacifick year" of the Restoration), "the strange confusions of this unhappy nation, in the midst of which I have made and written these experiments, are apt to disturb that calmness of mind and undistractedness of thoughts, that are wont to be requisite to happy speculations."[114] And Sprat recalled the circumstances of the Oxford group of experimentalists that spawned the Royal Society: "Their first purpose was no more, then onely the satisfaction of breathing a freer air, and of conversing in quiet one with another, without being ingag'd in the passions, and madness of that dismal Age."[115]

Three Technologies and the Nature of Assent

We have argued that three technologies were involved in the production and validation of matters of fact: material, literary, and social. We have also stressed that the three technologies are not distinct and that the workings of each depends upon the others. We can now briefly develop that point by showing how each of

[114] Boyle, "New Experiments," p. 3. The phrase "wonderful pacifick year" is from Sprat, *History*, p. 58.

[115] Sprat, *History*, p. 53.

Boyle's technologies contributes to a common strategy for the constitution of the matter of fact. In the first section of this chapter we argued that the matter of fact can serve as the foundation of knowledge and secure assent insofar as it is not regarded as man-made. Each of Boyle's three technologies worked to achieve the appearance of matters of fact as *given* items. That is to say, each technology functioned as an *objectifying resource.*

Take, for example, the role of the air-pump in the production of matters of fact. Pneumatic facts, as we have noted, were machine-made. One of the significant features of a scientific machine is that it stands between the perceptual competences of a human being and natural reality itself. A "bad" observation taken from a machine need not be ascribed to faults in the human being, nor is a "good" observation his personal product: it is this impersonal device, the machine, that has produced the finding. In chapter 6 we shall see a striking instance of this usage. When, in the 1660s, Christiaan Huygens offered a matter of fact that appeared to conflict with one of Boyle's explanatory resources, Boyle did not impugn the perceptual or cognitive competences of his fellow experimentalist. Rather, he was able to suggest that the machine was responsible for the conflict: "[I] question not [his] Ratiocination, but only the stanchness of his pump."[116] The machine constitutes a resource that may be used to factor out human agency in the product: as if it were said "it is not I who says this; it is the machine"; "it is not your fault; it is the machine's."

The role of Boyle's literary technology was to create an experimental community, to bound its discourse internally and externally, and to provide the forms and conventions of social relations within it. The literary technology of virtual witnessing extended the public space of the laboratory in offering a valid witnessing experience to all readers of the text. The boundaries stipulated by Boyle's linguistic practices acted to keep that community from fragmenting and to protect items of knowledge to which one might expect universal assent from items of knowledge that historically generated divisiveness. Similarly, his stipulations concerning proper manners in dispute worked to guarantee that social solidarity that produced assent to matters of fact and to rule out of order those imputations that would undermine the moral integrity of the experimental form of life. The objectivity of the experimental matter of fact was an

[116] Boyle to Moray, July 1662, in Huygens, *Oeuvres*, vol. IV, p. 220. Compare Boyle's accounting for Linus's deviant findings in his attempted replication of the Puy-de-Dôme experiment: "Defence against Linus," pp. 152-153, and chapter 5 below.

artifact of certain forms of discourse and certain modes of social solidarity.

Boyle's social technology constituted an objectifying resource by making the production of knowledge visible as a collective enterprise: "It is not I who says this; it is all of us." As Sprat insisted, collective performance and collective witness served to correct the natural working of the "idols": the faultiness, the idiosyncrasy, or the bias of any individual's judgment and observational ability. The Royal Society advertised itself as a "union of eyes, and hands"; the space in which it produced its experimental knowledge was stipulated to be a *public space.* It was public in a very precisely defined and very rigorously policed sense: not everybody could come in; not everybody's testimony was of equal worth; not everybody was equally able to influence the institutional consensus. Nevertheless, what Boyle was proposing, and what the Royal Society was endorsing, was a crucially important *move towards* the public constitution and validation of knowledge. The contrast was, on the one hand, with the private work of the alchemists, and, on the other, with the individual dictates of the systematical philosopher.

In the official formulation of the Royal Society, the production of experimental knowledge commenced with individuals' acts of seeing and believing, and was completed when all individuals voluntarily agreed with one another about what had been seen and ought to be believed. This freedom to speak had to be protected by a special sort of discipline. Radical individualism—the state in which each individual set himself up as the ultimate judge of knowledge—would destroy the conventional basis of proper knowledge, while the disciplined collective social structure of the experimental form of life would create and sustain that factual basis. Thus the experimentalists were on guard against "dogmatists" and "tyrants" in philosophy, just as they abominated "secretists" who produced their knowledge-claims in a private and undisciplined space. No one man was to have the right to lay down what was to count as knowledge. Legitimate knowledge was warranted as objective insofar as it was produced by the collective, and agreed to voluntarily by those who comprised the collective. The objectification of knowledge proceeded through displays of the communal basis of its generation and evaluation. Human coercion was to have no visible place in the experimental form of life.[117]

[117] Sprat, *History*, pp. 98-99 (for the individual and the collective); ibid., p. 85, and Hooke, *Micrographia*, "The Preface," sig a2^{v} (for "eyes and hands" and "a sincere

If the obligation to assent to items of knowledge was not to come from human coercion, where did it come from? It was to be nature, not man, that enforced assent. One was to believe, and to say one believed, in matters of fact because they reflected the structure of natural reality. We have described the technologies that Boyle deployed to generate matters of fact and the conventions that regulated the knowledge-production of the ideal experimental community. Yet the transposition onto nature of experimental knowledge depended upon the routinization of these technologies and conventions. The naturalization of experimental knowledge depended upon the institutionalization of experimental conventions. It follows from this that any attack upon the validity and objectivity of experimental knowledge-production could proceed by way of a display of its conventional basis: showing the work of production involved and exhibiting the lack of obligation to credit experimental knowledge. It might also exhibit an alternative form of life by which assent might more effectively be achieved, one which would yield a superior sort of obligation to assent. In his criticisms of Boyle's programme, Hobbes endeavoured to do just this. Hobbes maintained that the experimental form of life could not produce effective assent: it was not *philosophy*.

Hand, and a faithful Eye"); Sprat, *History*, pp. 28-32 and Glanvill, *Scepsis scientifica*, p. 98 (for "tyrants" in philosophy). For the disciplining of the Royal Society's public: J. Jacob, *Boyle*, p. 156; idem, *Stubbe*, pp. 59-63; also some highly perceptive remarks in Ezrahi, "Science and the Problem of Authority in Democracy," esp. pp. 46-53.