· 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.

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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 a2^{r}; see also Bennett, "Hooke as Mechanic and Natural Philosopher," p. 44.

[25] Hooke, *Micrographia*, "The Preface," sig b2^{v}.

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 $a2^v$. 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 $b4^v$; 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 put-

ting 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.

STEVEN SHAPIN & SIMON SCHAFFER

LEVIATHAN AND THE AIR-PUMP

HOBBES, BOYLE, AND THE EXPERIMENTAL LIFE

WITH A NEW INTRODUCTION BY THE AUTHORS

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