SEEING AND BELIEVING: THE EXPERIMENTAL PRODUCTION OF PNEUMATIC FACTS

Excerpts from: Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle and the Experimental Life*, Princeton; Princeton University Press, 1985, Chapter 2, pp. 22-79.

Excerpts – DO NOT QUOTE FROM THIS TEXT

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 text 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.'" 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. … 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. In this chapter we shall examine the means by which the experimental matter of fact was produced.

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.

Boyle's experimental programme utilized three technologies: a material technology embedded in the construction and operation of the airpump; 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. 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 extended 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

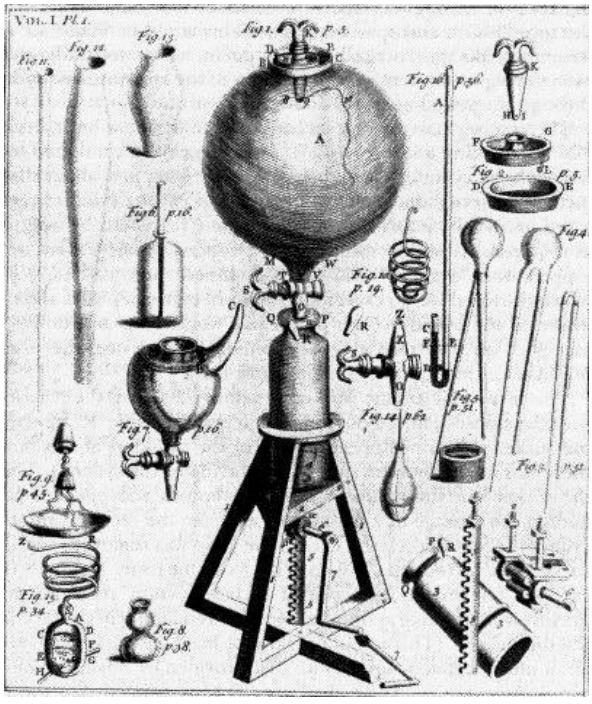
pneumatic facts, we must consider how each of the three technologies was used and how each bore upon the others.

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.

Figure 1 is an engraving of the Boyle-Hooke 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.

FIGURE I



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. 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 differed radically from Boyle's.)

The evacuation of air from the receiver of Boyle's original airpump 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. … 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. … Three points should be noticed: (1) both the engine's integrity and its limited leakage were important resources for Boyle in validating his pneumatic findings and their proper interpretation; (2) the physical integrity of the machine was vital to the perceived integrity of the knowledge the machine helped to produce; and (3) 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. … 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 … that depicted Boyle together with his pneumatic engine. … 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). 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. The authorship 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 … 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.

FIGURE 2

Frontispiece to Sprat 's History of the Royal Society (1667). Engraving by Wenceslaus Hollar, design probably hy 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.)

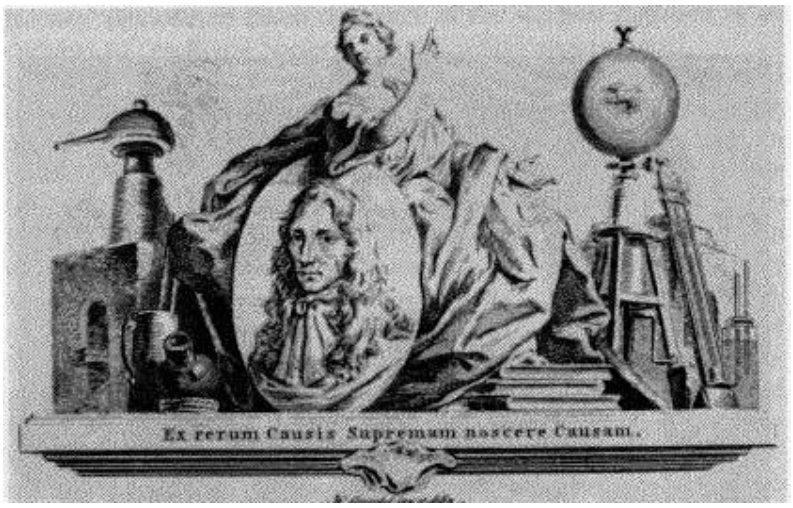


FIGURE 3

Vignette by Hubert François Gravelot Bourguignon for Thomas Birch's edition of Boyle's Works (I 744 and I 772), frontispiece to vol. 1. (Courtesy of Edinburgh University Library.)

Two Experiments

The text of Boyle's *New Experiments* of1660 consisted of narratives of forty-three trials made with the new pneumatic engine. Critics of Boyle's experimental programme 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. It will be useful to describe two of Boyle's first air-pump experiments as he himself recounted them. 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 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. 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.

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. 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 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.

Second, it seemed to participants that experimental measures 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-l648 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. … Thus the Torricellian experiment was intimately associated with the claim of experiment to settle belief about nature, to end controversy, and to generate consensus.

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.

This is what Boyle did: he took a three-foot-long glass tube, one 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 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. This is a figure 12 for a drawing of a later version of this experimental set-up:

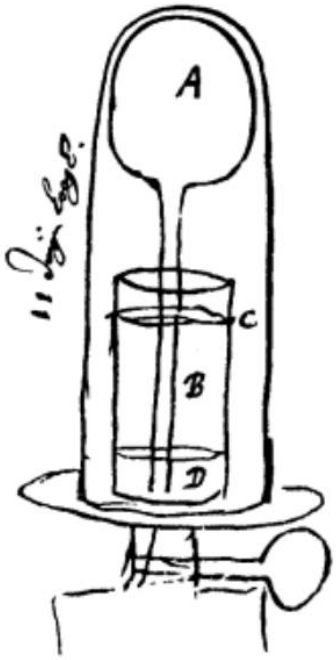


FIGURE 12

Huygens' diagram of his trial of the void-in-the-void experiment with his new pump (December 1661). A: flask full of water; D: water in outer vessel B; C: water level in both A and B after exsuction of air from receiver. From Huygens, Oeuvres, *vol.* XVII, *p. 317 (figure 39). (Courtesy of Edinburgh University Library.)*

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. 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."

But for Boyle, the treatment of the question of a void 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 not 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.

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. *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." Boyle admitted the possibility that the receiver exhausted of air was replenished with "some etherial matter," "but not that it really is so."

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. 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 phenomenon, 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 centrepiece 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. … 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.

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. …

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. 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 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. . . ." In other places Boyle chose to label the status of the spring an "hypothesis" or a "doctrine." And 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."

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 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." 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. 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. 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.

Also 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." 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. 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. …

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. Let us proceed now 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." If knowledge was to be empirically 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." 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.

The thrust of the legal analogy should not be missed. It was not merely that one was multiplying authority by multiplying witnesses (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. 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. … (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 presence of an illustrious assembly of virtuosi (who were spectators of the experiment)." 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." 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." In reporting experiments that were particularly important or problematic, Boyle named his witnesses and stipulated their qualifications.

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. 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 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 Engiand.

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...." The *History of Colours* was designed "not barely to relate [the experiments], but . . . to teach a young gentleman to make them." 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. This situation had not materially changed by the mid-l670s. 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."

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

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.

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." 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 proceedings. He would be recruited as a witness and be put in a position where he could validate experimental phenomena as matters of fact. 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."

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. 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. 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 experiment. 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 "underbuilder," 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.As The public display of this 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.

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."

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 locutions 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." 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."

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." 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. Indeed, the construction of Boyle's experimental essays 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." 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."

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 as a demonstrative discipline." 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. 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. 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.

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." 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.

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 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. 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." 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 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." 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.

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.