Pump and Circumstance: Robert Boyle's Literary Technology

● *ABSTRACT*

Robert Boyle's experimental programme had as its end-product the generation of indisputable matters of fact. In this paper I analyze the resources used to produce these matters of fact, paying particular attention to linguistic practices. Experimental reports rich in circumstantial detail were designed to enable readers of the text to create a mental image of an experimental scene they did not directly witness. I call this 'virtual witnessing', and its importance was as a means of enlarging the witnessing public. The notion of a 'public' for experimental science is, I argue, essential to our understanding of how facts are generated and validated. In these episodes, circumstantial reporting was a technique for creating a public and for constituting authentic knowledge.

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The production of knowledge and the communication of knowledge are usually regarded as distinct activities. In this paper I shall argue to the contrary: speech about natural reality is a means of generating knowledge about reality, of securing assent to that knowledge, and of bounding domains of certain knowledge from areas of less certain standing. I shall attempt to display the conventional status of specific ways of speaking about nature and natural knowledge, and I shall examine the historical circumstances in which these ways of speaking were institutionalized. Although I shall be dealing with communication within a scientific community, there is a clear connection between this study and the analysis of scientific popularization. The popularization of science is usually understood as the extension of experience from the few to the many. I argue here that one of the major resources for generating and validating items of knowledge within the scientific community under study was this same extension of experience from the few to the many: the creation of a scientific public. The etymology of some of our key terms is apposite: if a *community* is a group

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sharing a common life, *communication* is a means of making things common.

The materials selected to address this issue come from episodes of unusual interest to the history, philosophy and sociology of science. Robert Boyle's experiments in pneumatics in the late 1650s and early 1660s represent a revolutionary moment in the career of scientific knowledge. In his *New Experiments Physico-Mechanical* (1660) and related texts of the early Restoration, Boyle not only produced new knowledge of the behaviour of air, he exhibited the proper experimental means by which legitimate knowledge was to be generated and evaluated. And he did so against the background of alternative programmes for the production of knowledge, the proponents of which subjected Boyle's recommended methods to explicit criticism. What was at issue in the controversies over Boyle's air-pump experiments during the 1660s was the question of how claims were to be authenticated as knowledge. What was to count as knowledge, or 'science'? How was this to be distinguished from other epistemological categories, such as 'belief' and 'opinion'? What degree of certainty could be expected of various intellectual enterprises and items of knowledge? And how could the appropriate grades of assurance and certainty be secured?[1]

These were all practical matters. In the setting of early Restoration England there was no one solution to the problem of knowledge which commanded universal assent. The technology of producing knowledge had to be built, exemplified and defended against attack. The categories of knowledge and their generation that seem to us self-evident and unproblematic were neither self-evident nor unproblematic in the 1660s. The foundations of knowledge were not matters merely for philosophers' reflections; they had to be constructed and the propriety of their foundational status had to be argued. The difficulties that many historians evidently have in recognizing this work of construction arise from the very success of that work: to a very large extent we live in the conventional world of knowledge-production that Boyle and his colleagues amongst the experimental philosophers laboured to make safe, self-evident and solid.

Robert Boyle sought to secure universal assent by way of the experimental *matter of fact*. About such facts one could be highly certain; about other items of natural knowledge more circumspection was indicated. Boyle was, therefore, an important actor in the probabilist and fallibilist movement of seventeenth-century

England. Before *circa* 1660, as Hacking and Shapiro have shown, the designations of 'knowledge' and 'science' were rigidly distinguished from 'opinion'.[2] Of the former one could expect the absolute certainty of *demonstration*, exemplified by logic and geometry. The goal of physical science had been to attain to this kind of certainty that compelled assent. By contrast, the English experimentalists of the mid-seventeenth century 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 necessary, as it was to mathematical demonstration; and physical science was, to varying degrees, removed from the realm of the demonstrative.[3] The probabilistic conception of physical knowledge was not regarded as a regrettable retreat from more ambitious goals; it was celebrated by its proponents as a wise rejection of failed dogmatism. The quest for necessary and universal assent to physical propositions was seen as improper and impolitic.

If universal 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.* The fact was the item of knowledge about which it was legitimate to be 'morally certain'. A crucial boundary was drawn around the domain of the factual, separating it from those items which might be otherwise and from which absolute and permanent certainty should not be expected. Nature was like a clock: man could be certain of its effects, of the hours shown by its hands; but the mechanism by which these effects were produced, the clock-work, might be various.[4]

It is in the understanding of how matters of fact were produced and how they came to command universal assent that historians have tended to succumb to the temptations of self-evidence.[5] It is the purpose of this paper to display the processes by which Boyle constructed experimental matters of fact and thereby produced the conditions in which assent could be mobilized.

The Mechanics of Fact-Making

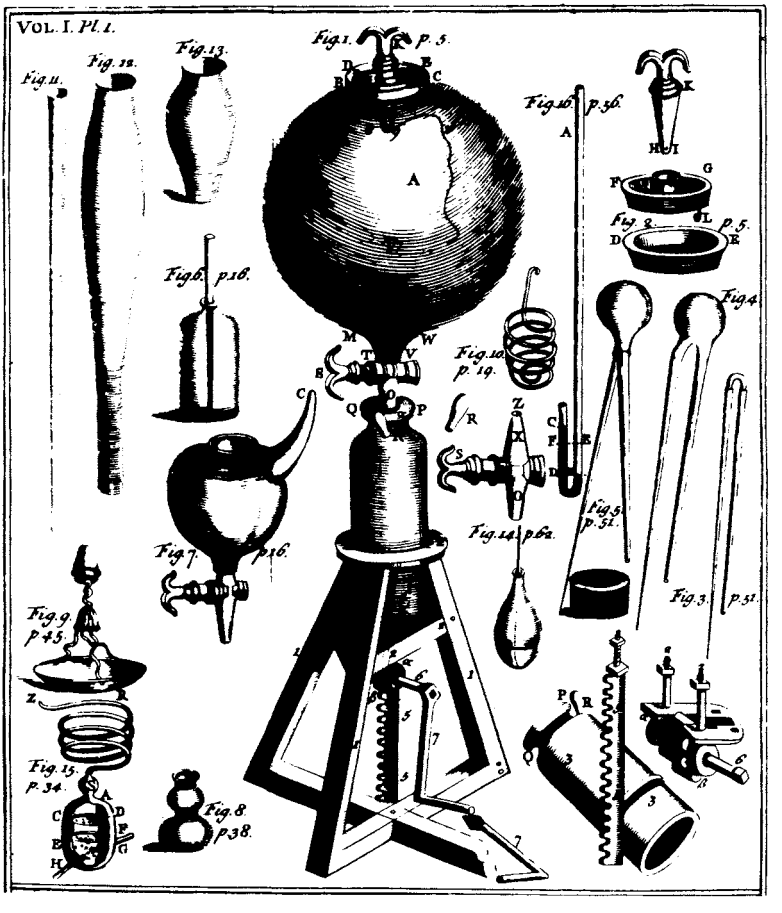
Boyle proposed that matters of fact be generated by a multiplication of the witnessing experience. An experience, even of

an experimental performance, that was witnessed by one man alone was not a matter of fact. If that witness 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 was at once an epistemological and a social category. The foundational category of the experimental philosophy, and of what counted as properly grounded knowledge generally, was an artefact of communication and of whatever social forms were deemed necessary to sustain and enhance communication. I argue that the establishment of matters of fact utilized three technologies: a *material technology* embedded in the construction and operation of the air-pump; a *literary technology* by means of which the phenomena produced by the pump were made known to those who were not direct witnesses; and a *social technology* which laid down the conventions natural philosophers should employ in dealing with each other and considering knowledge-claims.[6] Given the concerns of this paper, I shall be devoting most attention to Boyle's literary technology: the expository means by which matters of fact were established and assent mobilized. Yet the impression should not be given that we are dealing with three distinct technologies: each embedded the others. For example, experimental practices employing the material technology of the air-pump crystallized particular forms of social organization; desired forms of social organization were dramatized in the exposition of experimental findings; the literary reporting of air-pump performances provided an experience that was said to be essential to the propagation of the material technology or even to be a valid substitute for direct witness. In studying Boyle's literary technology we are not, therefore, talking about something which is merely a 'report' of what was done elsewhere; we are dealing with a most important form of experience and the means for extending and validating experience.

The Material Technology of the Air-Pump

We start by noting the obvious: Boyle's matters of fact were *machine-made*. In his terminology, performances using the air-pump counted as 'unobvious' or 'elaborate' experiments, contrasted to either the 'simple' observation of nature or the 'obvious' experiments involved in reflecting upon common artefacts like the gardener's watering-pot.[7] The air-pump (or

Figure 1
Boyle's Air Pump of 1660



(*Source:* from Boyle 'New Experiments Physico-Mechanical', op. cit. note 1)

'pneumatic engine') constructed for Boyle in 1659 (largely by Robert Hooke) was indeed an elaborate bit of scientific machinery (see Figure 1).[8] It consisted of a glass 'receiver' of about 30-quarts volume, connected to a brass 'cylinder' ('3') within which plied a wooden piston or 'sucker' ('4'). The aim was to evacuate the receiver of atmospheric air and thus to achieve a working vacuum. This was done by manually operating a pair of valves: on the downstroke, valve 'S' (the stop-cock) was opened and valve 'R' was inserted; the sucker was then moved down by means of a rack-and-pinion device ('5' and '7'). On the upstroke, the stop-cock was closed, the valve 'R' removed, and a quantity of air drawn into the cylinder was expelled. This operation was repeated many times until the effort of moving the sucker became too great, at which point a working vacuum was deemed to have been attained. Great care had to be taken to ensure that the pump was sealed against leakage, for example at the juncture of receiver and cylinder and around the sides of the sucker. Experimental apparatus could be placed into the receiver through an aperture at the top of the receiver ('B-C'), for instance a barometer or simple Torricellian apparatus. The machine was then ready to produce matters of fact. Boyle used the pump to generate phenomena which he interpreted in terms of 'the spring of the air' (its elasticity) and the weight of the air (its pressure).

Boyle's air-pump was, as he said, 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 mightily to be a son of the Earl of Cork. Other natural philosophers, almost as well supplied with cash, shied away from the cost of having one built, 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. Air-pumps were not widely distributed in the 1660s. They were scarce commodities: Boyle's original machine was quickly presented to the Royal Society of London; he had one or two re-designed instruments 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, Cambridge 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 air-pumps that existed in the

decade after their invention.[9]

Thus, air-pump technology posed a problem of access. If knowledge was to be produced using this technology, then the numbers of philosophers who could produce it were limited. Indeed, in Restoration England this restriction was one of the chief recommendations of 'elaborate' experimentation: knowledge could no longer legitimately be generated by alchemical 'secretists' and sectarian 'enthusiasts' who claimed individual and unmediated inspiration from God. Experimental knowledge was to be tempered by collective labour and disciplined by artificial devices. The very intricacy of machines like the air-pump allowed philosophers, it was said, to discern which cause, amongst the many possible, might be responsible for observed effects. This was something, in Boyle's view, that the gardener's pot could not do.[10] However, access to the machine had to be opened up if knowledge-claims were not to be regarded as mere individual opinion and if the machine's matters of fact were not to be validated on the bare say-so of an individual's authority. How was this special sort of access to be achieved?

Witnessing Science

In Boyle's programme 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 an important distinction between actual experiments and what are now termed 'thought experiments'.[11] If knowledge was to be empirically based, as Boyle and other English experimentalists insisted it should, then its experimental foundations had to be attested to by eye-witnesses. Many phenomena, and particularly those alleged by the alchemists, were difficult to credit; in which cases Boyle averred 'that they that have seen them can much more reasonably believe them, than they that have not.'[12] 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 whatever?

Boyle insisted that witnessing was to be a collective enterprise. In natural philosophy, as in criminal law, the reliability of testimony depended crucially 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.[13]

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

> whether, seeing in all Countreys, that are govern'd by Laws, they expect no more, than the consent of two, or three witnesses, in matters of life, and estate; they will not think, they are fairly dealt withall, in what concerns their *Knowledg*, if they have the concurring Testimonies of *threescore or an hundred.*[14]

The thrust of the legal analogy should not be missed. It was not just 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 positive 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 counted as an active, and not just a descriptive, licence. Does 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 '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. The early air-pump trials were routinely performed in the Royal Society's ordinary public rooms, the machine being brought there specially for the occasion.[15] 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).'[16] Boyle's collaborator Robert Hooke worked to codify the 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 . . .'[17] And Sprat described the role of the 'Assembly' in 'resolv[ing] upon the matter of *Fact*' by collectively correcting individual idiosyncracies of observation and judgement.[18] In reporting experiments that were particularly crucial or problematic, Boyle named his witnesses and stipulated their qualifications. Thus, the experiment of the original air-pump trials that was 'the principal fruit I promised myself from our engine' was conducted in the presence of 'those excellent and deservedly famous Mathematic Professors, Dr *Wallis,* Dr *Ward,* and Mr *Wren* . . ., whom I name, both as justly counting it an honour to be known to them, and as being glad of such judicious and illustrious witnesses of our experiment . . .' Another important experiment was attested to by Wallis 'who will be allowed to be a very competent judge in these matters.' 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.[19] 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 knowledgeableness, '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 transitted the social and moral accounting systems of Restoration England.[20]

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. 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.'[21] Boyle wished to encourage young gentlemen to 'addict' themselves to experimental pursuits and, thereby, to multiply both experimental philosophers and experimental facts.

Replication, however, rarely succeeded, as Boyle himself recognized. When he came to prepare the *Continuation of New Experiments* seven years after the original air-pump trials, Boyle admitted that, despite his care in communicating details of the engine and of his procedures, there had been few successful replications:

> . . . in five or six years I could hear but of one or two engines that were brought to be fit to work, and of but one or two new experiments that had been added by the ingenious owners of them . . .[22]

This situation had not notably changed by the mid-1670s. In the seven or eight years after the *Continuation,* Boyle said that he heard 'of very few experiments made, either in the engine I used, or in any other made after the model thereof.' By this time a note of despair began to appear in Boyle's statements concerning the replication of his air-pump experiments. He

> was more willing to set down divers things with their minute circumstances; because I was of opinion, that probably many of these experiments would be never either re-examined by others, or re-iterated by myself. For though they may be easily read . . . yet he, that shall really go about to repeat them, will find it no easy task.[23]

The Literary Technology of Virtual Witnessing

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 actual replication: it is what I 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 its direct witness or its 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 amongst 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.

Prolixity and Iconography

In order to understand how Boyle deployed his literary technology of virtual witnessing we have to reorientate some of our common ideas about the status of the scientific *text*. We usually think of an experimental report as a narration of some prior visual experience: it points to sensory experience that lies behind the text. This is correct. However, we should also appreciate that the text itself constitutes a visual source. It is my 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 parts. This is not a picture of the 'idea' of an air-pump but of a particular existing air-pump.[24] The same applies to Boyle's pictorial representations of his particular pneumatic experiments: in one, 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 engravings, sometimes consulting directly with artist and engraver, sometimes by way of Hooke.[25] 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.'[26]

Thus, 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, the images 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 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.[27]

In the introductory pages of the *New Experiments,* Boyle's first published experimental findings, he directly announced his intention to be 'somewhat prolix'. His excuses were three-fold: first delivering things 'circumstantially' would, as we have already seen, facilitate replication; second, the density of circumstantial details 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'.[28] If one wrote an experimental report 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.[29] 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 are dispersed throughout his experimental reports of the 1660s, but he also composed a special tract on the subject of 'experimental essays'. Here Boyle offered extended apologia for his 'prolixity': 'I have,' he understated, 'declined that succinct way of writing'; he had sometimes 'delivered things, to make them more clear, in such a multitude of words, that I now seem even to myself to have in divers places been guilty of verbosity . . .' Not just his 'verbosity' but also Boyle's ornate sentence-structure, with appositive clauses piled on top of each other, was, he said, part of a plan to convey circumstantial details and to give the impression of verisimilitude:

> . . . I have knowingly and purposely transgressed the laws of oratory in one particular, namely, in making sometimes my periods [i.e., complete sentences] or parentheses over-long: for when I could not within the compass of a regular period comprise what I thought requisite to be delivered at once, I chose rather to neglect the precepts of rhetoricians, than the mention of those things, which I thought pertinent to my subject, and useful to you, my reader.[30]

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

Boyle was endeavouring to constitute himself as a reliable purveyor of experimental testimony and to offer conventions by means of which others could do likewise. The provision of circumstantial details of experimental scenes 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 performances, in which a wide range of contingencies might influence outcomes.[31] 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.[32] It is, however, vital to keep in mind that the contingencies proffered in Boyle's circumstantial accounts represent a selection of possible contingencies. There was not, nor can there be, any such thing as a report which notes all circumstances which 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 should make that modesty visible.

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 piece-meal reporting of experimental trials) was explicitly contrasted to the natural philosophical system. Those who wrote entire systems were identified as

'confident' individuals, whose ambition extended beyond what was proper or possible. By contrast, those who wrote experimental essays were 'sober and modest men', 'diligent and judicious' philosophers, who did not 'assert more than they can prove.' This practice cast the experimental philosopher into the role of intellectual 'under-builder', or even that of 'a drudge of greater industry than reason'. This was, however, a noble character, for it was one that was freely chosen to further 'the real advancement of true natural philosophy' rather than personal reputation.[33] The public display of this modesty was an exhibition that concern for individual celebrity did not cloud judgement 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'. He 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 character, and Boyle's presentation of self as a role model for experimental philosophers was powerful.[34]

Another technique for displaying 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, puritanical, unadorned (yet convoluted) style was identified as *functional*. It served to exhibit, 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'.[35]

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 crucial 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.[36]

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 model, 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 . . .'[37] The separation of modes of speech, and the ability of facts to make their own way, was 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' upon their interpretation. One might then read the experiments and the 'reflexions' separately.[38] Indeed, the construction of Boyle's experimental essays makes manifest the proper balance between the two categories: *New Experiments* consists of a sequential narrative of 43 pneumatic experiments; *Continuation* of 50; 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 this practice ran the risk of identifying the experimental philosopher as an ill-read philistine, it was, however, necessary: '. . . I could be very well content to be thought to have scarce looked upon any other book than that of nature.'[39] The injunction against 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.[40] A disengagement between experimental narrative and the authority of systematists served to dramatize the author's lack of

preconceived expectations and, especially, of theoretical investments in the outcome of experiments. For example, Boyle several times insisted that he was an innocent of the great theoretical systems of the seventeenth century. In order to reinforce the primacy of experimental findings, 'I had purposely refrained from acquainting myself thoroughly with the intire system of either the Atomical, or the Cartesian, or any other whether new or received philosophy . . .' And, again, he claimed that he had avoided a systematic acquaintance with the systems of Gassendi, Descartes, and even of Bacon, 'that I might not be prepossessed with any theory or principles . . .'[41]

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 the Community

I have said that the matter of fact was a social as well as an intellectual category. And I have argued that Boyle deployed his literary technology so as to make virtual witnessing a practical option for the validation of experimental performances. I want in this section 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 between individuals 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 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 form of social solidarity. In the following sections I will discuss the

ways in which Boyle's literary technology worked to create and maintain this social solidarity amongst experimental philosophers.

The Linguistic Boundaries of the Experimental Community

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 physics 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 most of its good jokes by parodying the convoluted language of Sir Nicholas Gimcrack (Boyle).[42] The practice of experimental philosophy, despite what numerous historians have assumed, was not overwhelmingly popular in Restoration England.[43] 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.[44] 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 of the

Aristotelians) frequently 'do not evince what they are alleged to prove', the former could be accepted into the experimental philosophy by stripping away the theoretical language with which they happened to be glossed. As Carneades (Boyle's mouthpiece) said,

> . . . your hermetic philosophers present us, together with divers substantial and noble experiments, theories, which either like peacocks feathers make a great shew, but are neither solid nor useful; or else like apes, if they have some appearance of being rational, are blemished with some absurdity or other, that, when they are attentively considered, make them appear ridiculous.[45]

Thus, those alchemists who wished to be incorporated into a legitimate philosophical community were instructed what linguistic practices could secure their entry. The same principles were laid down 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.'[46] By arguing that there was only a contingent, not a necessary, connection between the language of matters of fact and theoretical language, Boyle was defining the linguistic terms upon which existing communities could join the experimental enterprise. They were liberal terms, which might serve to maximize potential membership.[47]

There were other natural philosophers Boyle despaired to recruit. Hobbes, notably, was the kind of philosopher who, on no account, ought to be admitted, for he denied the value of systematic and elaborate experimentation, the foundational status of the matter of fact, and the distinction between causal and descriptive language. Of Hobbes's *Dialogus physicus,* Boyle asked 'What new experiment or matter of fact Mr *Hobbes* has therein added to enrich the history of nature . . .?' In his criticisms of Boyle's experiments Hobbes 'does not, that I remember, deny the truth of any of the matters of fact I have delivered.' According to Boyle, both Hobbes and another critic, the Jesuit Franciscus Linus, had not 'seen cause to deny any thing that I deliver as experiment.'[48] One could not be regarded as a competent member of the experimental community if one failed to communicate experimental matters of fact, or if one did so in a manner that failed to recognize the linguistic boundaries between factual and causal locutions.

Linguistic Boundaries within the Experimental Community

Just as linguistic categories were used to manage entry to the experimental community, distinctions between the language of facts and that of theories were deployed to regulate discourse within it. In broad terms, Boyle insisted upon a separation between 'physiological' and 'metaphysical' languages: experimental discourse was to be confined to the former. One of the central categories of Boyle's 'new pneumatics' also happened to be a major preoccupation of the old physics — namely, vacuism versus plenism, and the judgement whether a vacuum was possible in nature. How was it proper to speak of the contents of the receiver of an evacuated air-pump? And how did this speech relate to traditional usages of the term 'vacuum'?

A practical problem was posed by the fact that the lexicon of the new philosophy was largely compiled out of the usages of old discursive practices. Old words had to be given new meanings. Thus, it was proper to apply the term 'vacuum' to the contents of the exhausted receiver, but it was improper to take this to mean that the space was absolutely devoid of all matter. Such an absolutely void space was the 'vacuum' of metaphysical discourse. What Boyle meant by the air-pump's 'vacuum' was 'not a space, wherein there is no body at all, but such as is either altogether, or almost totally devoid of air.'[49] If contemporary plenists maintained that this vacuum might be filled by a subtle form of matter, or 'aether', Boyle could reply with a series of experiments which showed that such an aether could not be made 'sensible', that is, it had no physical manifestations. And speech of entities that were not amenable to sensible experimentation was not permissible within experimental philosophy.[50]

The separation of 'physiological' from 'metaphysical' language was most crucial to Boyle's strategy for dealing with causal inquiry in physical science. In keeping with his probabilist conception of knowledge, Boyle wished to bracket off speech about matters of fact, about which one might be certain, from speech of their physical causes, which were at best probable. In terms of Boyle's air-pump programme, the most important instance of this bracketing concerned the notion which was the main product of these experiments: the 'spring of the air'. Boyle said 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.' The cause of the air's elasticity *might* be accounted for variously: by Cartesian vortices, or by the real physical existence in the corpuscles of the air of 'slender springs' or of a fleecy structure.[51] The job of the experimental philosopher was to speak of experimentally-produced matters of fact, not to conjecture further than that.[52]

Boyle had considerable problems in diffusing this new mode of speech. Plenist critics persisted in understanding Boyle to be using 'vacuum' in its metaphysical sense, and Boyle was obliged persistently to reiterate its proper usage.[53] Other writers either refused to conceive of a natural philosophy that bracketed off causal speech, or reckoned that Boyle must be committed to some (illegitimate and unacknowledged) causal account of the spring of the air.[54] So far as the 'spring of the air' was concerned, Boyle's stipulation that it had been made experimentally 'manifest' and his disinclination to speak of its cause had an interesting effect. By putting the spring on the other side of the boundary from causal locutions, Boyle constituted the spring, for all practical purposes, into a matter of fact. When it came to labelling the epistemological status of the spring, Boyle variously referred to it as an 'hypothesis' or even as a 'doctrine'. However, by making the spring into something that was made manifest through experiment, and by protecting it from the uncertainties that afflicted epistemological items like causal notions, Boyle treated this 'hypothesis' in the same way that he treated other matters of fact.[55]

The vital difference between matters of fact and all other epistemological categories was the degree of assent one might expect to them. To an authenticated matter of fact all men will assent. In Boyle's system that was taken for granted because it was through the technologies that multiplied witness that matters of fact were constituted. General assent was what made matters of fact, and general assent was therefore mobilized around matters of fact. With 'hypotheses', 'theories', 'conjectures', and the like, the situation was quite different. These categories threatened that assent which could be crystallized in the institution of the matter of fact. Thus, the linguistic conventions of Boyle's experimental programme separated speech appropriate to the two categories as a way of drawing the boundaries between that about which one was to expect certainty and assent and that about which one could expect uncertainty and divisiveness. The idea was not to eliminate

dissent or to oblige men to agree to all items in natural philosophy (as it was for Hobbes); rather, it was to manage dissent and to keep it within safe bounds. An authenticated matter of fact was treated as a mirror of nature; a theory, by contrast, was clearly man-made and could, therefore, be contested. Boyle's linguistic boundaries acted to segregate what could be disputed from what could not. The management of dispute in experimental philosophy was crucial to protecting the foundations of knowledge.

Manners in Dispute

Since natural philosophers were not to be compelled to give assent to all items of knowledge, dispute and controversy was to be expected. How should this be dealt with? The problem of conducting dispute was a matter of intense practical concern in early Restoration science. During the Civil War and Interregnum the divisiveness of 'enthusiasts', sectarians and hermeticists threatened to bring about radical individualism in philosophy. Nor did the various sects of Peripatetic natural philosophers display a public image of a stable and united intellectual community. Unless the new experimental community could exhibit a broadly-based consensus and harmony within its own ranks, it was unreasonable to expect it to secure the legitimacy within Restoration culture that its leaders desired. Moreover, that very consensus was vital to the establishment of matters of fact as the foundational category of the new practice.

By the early 1660s Boyle was in a position to give concrete exemplars of how disputes ought to be conducted; three critics published their responses to his *New Experiments,* and he replied to each one: Linus, Hobbes and Henry More. But even before he had been engaged in dispute, Boyle laid down a set of rules for how controversies were to be handled by the experimental philosopher. For example, in *A Proëmial Essay* (composed 1657), Boyle insisted that disputes should be about findings and not about persons. It was proper to take a hard view of reports which were inaccurate but most improper to attack the character of those who rendered them: 'for I love to speak of persons with civility, though of things with freedom'. The *ad hominem* style must at all costs be avoided, for the risk was that of making foes out of mere dissenters. This was the key point: potential contributors of matters of fact, however

wrong they may be, must be treated as possible converts to the experimental philosophy. If, however, they were bitterly treated, they would be lost to the cause and to the community whose size and consensus validated matters of fact:

> And as for the (very much too common) practice of many, who write, as if they thought railing at a man's person, or wrangling about his words, necessary to the confutation of his opinions; besides that I think such a quarrelsome and injurious way of writing does very much misbecome both a philosopher and a Christian, methinks it is as unwise, as it is provoking. For if I civilly endeavour to reason a man out of his opinions, I make myself but one work to do, namely, to convince his understanding; but, if in a bitter or exasperating way I oppose his errors, I increase the difficulties I would surmount, and have as well his affections against me as his judgment: and it is very uneasy to make a proselyte of him, that is not only a dissenter from us, but an enemy to us.[56]

Furthermore, it was impolitic to acknowledge the existence of 'sects' in natural philosophy. One way by which one could hope to overcome sectarianism was to decline public recognition that it existed: 'it is none of my design,' Boyle said, 'to engage myself with, or against, any one sect of Naturalists . . .' The experiments will decide the case. The views of these 'sects' should be noted only insofar as they are founded upon experiment. Therefore, it was right and politic to be harsh in one's writings against those who do not contribute experimental findings, for they have nothing to offer to the constitution of matters of fact. Finally, the experimental philosopher must show that there was point and purpose to legitimately conducted dispute. He should be prepared publicly to renounce positions that were shown to be erroneous. Flexibility followed from fallibilism. As Boyle wrote, 'till a man is sure he is infallible, it is not fit for him to be unalterable.'[57]

The conventions for managing dispute were dramatized in the structure of *The Sceptical Chymist.* These fictional conversations (between an Aristotelian, two varieties of hermeticists, and 'Carneades' as mouth-piece for Boyle) took the form, not of a Socratic dialogue, but of a *conference.*[58] They were a little piece of theatre that exhibited how persuasion, dissensus and, ultimately, conversion to truth ought to be conducted. Several points about Boyle's theatre of persuasion can be briefly made: first, the 'symposiasts' are imaginary, not real. This means that opinions can be confuted without exacerbating relations between real philosophers. Even Carneades, although he is manifestly 'Boyle's

man', is not Boyle himself: Carneades is made actually to quote 'our friend Mr *Boyle*' as a device for distancing opinions from individuals. The author is insulated from the text and from the opinions he may actually espouse. Second, truth is not inculcated from Carneades to his interlocutors; rather it is dramatized as emerging through the conversation.[59] Everyone is seen to have a say in the consensus which is the dénouement.[60] Third, the conversation is, without exception, civil: as Boyle said, 'I am not sorry to have this opportunity of giving an example, how to manage even disputes with civility . . .'[61] No symposiast abuses another; no ill temper is displayed; no one leaves the conversation in pique or frustration.[62] Fourth, and most importantly, the currency of intellectual discourse, and the means by which agreement is reached, is the experimental matter of fact. Here, as I have indicated, matters of fact are not treated as the exclusive property of any one philosophical sect. Insofar as the alchemists have produced experimental findings, they have minted the real coins of experimental exchange. Their experiments are welcome, while their 'obscure' speculations are not. Insofar as the Aristotelians produce few experiments, and insofar as they refuse to dismantle the 'arch'-like 'mutual coherence' of their philosophical system into facts and theories, they can make little contribution to the experimental conference.[63] In these ways, the structure and the linguistic conventions of this imaginary conversation make vivid the rules for real conversations proper to experimental philosophy.

Real disputes followed hard upon the imaginary ones of *The Sceptical Chymist,* providing Boyle with valuable opportunities of putting his principles into practice. Linus was the adversary who experimented but who denied the power of the 'spring of the air'; Henry More was the adversary whom Boyle wished to be an ally — offering what he regarded as a theologically more appropriate explanation of Boyle's pneumatic findings; and Hobbes was the adversary who denied the value of experiment and the foundational status of the matter of fact. Each carefully crafted response that Boyle produced was labelled as a model for how disputes should be managed by the experimental philosopher.[64]

First, all public disputes had to be justified: the experimental philosopher should be loath to engage in controversy. As Boyle claimed, '. . . I have a natural indisposedness to contention . . .'[65] The justification was not the defence of one's reputation but the

protection of what was vital to the collective practice of proper philosophy: the value of systematic experimentation, the matters of fact that experiment produced, the boundaries that separated those facts from less certain epistemological items, and the rules of social life that regulated discourse in the experimental community. As we have seen, Boyle took care to identify the *object* of controversy as interpretations of facts, not the facts themselves. Neither Linus nor Hobbes, he said, denied 'any thing that I deliver as experiment . . ., so that usually . . . they are fain to fall upon the hypotheses themselves.' This was a crucial stipulation, because, if it was accepted, then the arena of disagreement could be so defined as to protect the status of matters of fact. The very phenomenon of public disputation about 'hypotheses' could be contrasted to the absence of controversy about that which Boyle 'deliver[ed] as experiment'.[66]

The importance of protecting experimental practice is evident in the differing tones of Boyle's responses to Linus and to Hobbes. While Linus attacked the spring of the air, the major interpretative resource of Boyle's pneumatics, 'he takes no exceptions at the experiments themselves, as we have recorded them.' Boyle concluded that this 'is no contemptible testimony, that the matters of fact have been rightly delivered . . .' The Jesuit was congratulated for essaying to experiment himself and for his diligence in understanding what Boyle had written.[67] He was a good adversary and was dealt with as a potential convert. With Hobbes the situation was quite different. This adversary, 'not content to fall upon the explications of my experiments, has (by an attempt, for aught I know, unexampeled) endeavoured to disparage unobvious experiments themselves, and to discourage others from making them.'[68] Hobbes was a dangerous adversary; there was no possibility of recruiting such a man to the experimental programme, and his objections had to be publicly exploded.

For all that, Hobbes, no less than Linus and More, had to be dealt with civilly. Boyle aimed, he said, 'to give an example of disputing in print against a provoking, though unprovoked, adversary, without bitterness and incivility . . .' He hoped that his own *Examen* 'will not be thought to have less of reason for having the less of passion . . .'[69] Managing a dispute with Hobbes was a hard case, and, if it could be conducted in a decent tone, it would offer a model of the language of controversy appropriate to a moral community of experimental philosophers. Boyle did not

have far to look to find examples of improper disputation, in which the language of controversy acted to exacerbate divisions in natural philosophy. From the mid-1650s Hobbes's natural philosophy and geometry had been attacked by the Oxford professors John Wallis and Seth Ward. Wallis, one of the toughest street-fighters of the new philosophy, had not only shown his adversary's notions to be erroneous, he had punned upon the plebian origins of Hobbes's name and insinuated improper political affiliations and motivations. Hobbes, who professed himself concerned for maintaining good manners in dispute, showed his foes the sharp side of his tongue:

> So go your ways, you *Uncivil Ecclesiastics, Inhuman Divines, Dedoctors of morality, Unasinous Colleagues, Egregious pair of Issachars, most wretched Vindices and Indices Academiarum . . .*[70]

And again, summing up the value of one of Wallis's criticisms,

> . . . all error and railing, that is, stinking wind; such as a jade lets fly, when he is too hard girt upon a full belly.[71]

This is what Boyle wished to avoid. It was not merely a matter of Boyle's individual 'modest' temperament or what he reckoned was owing to fellow Christian philosophers. What was at issue was the creation and preservation of a calm public space in which natural philosophers could heal their divisions, collectively agree upon the foundations of knowledge, and, thereby, establish their credit in Restoration culture. Such a calm space was vital to achieving these goals. As Boyle reminded his readers in the introduction to his *New Experiments,* published in that 'wonderful pacifick year' of the Restoration of the monarchy, 'the strange confusions of this unhappy nation, in the midst of which I have made and written these experiments, are apt to disturb that calmness of mind and undistractedness of thoughts, that are wont to be requisite to happy speculations.'[72] And Sprat recalled the circumstances of the Oxford group of experimentalists that spawned the Royal Society: 'Their first purpose was no more, then onely the satisfaction of breathing a freer air, and of conversing in quiet one with another, without being ingag'd in the passions, and madness of that dismal Age.' He described the difference between 'humane affairs', which 'may affect us, with a thousand various disquiets', and the experimental

study of nature: '*that* gives us room to differ, without animosity; and permits us to raise contrary imaginations upon it, without any danger of a *Civil War*.'[73]

This calm space that experimental philosophy was to inhabit would be created and maintained through the deployment within the moral community of appropriate linguistic practices.[74] An appropriate language had to perform several functions. First, it had to be a resource for managing dissent and conflict in such a way as to make it possible for philosophers to express divergent views while leaving the foundations of knowledge intact, and, in fact, buttressing these foundations. We have seen this in the linguistic separation Boyle wished to make between speech of matters of fact and speech of explanatory items. Second, it had to facilitate reconciliation amongst existing sects of philosophers, mobilizing that reconciliation so as to reinforce the foundational status of matters of fact. We have seen this in Boyle's distribution of authentic matters of fact amongst groups with divergent theoretical commitments and in his identification of experimental matters of fact as the medium of exchange in the new practice. Third, such a language had to constitute a vehicle whereby matters of fact could effectively be generated and validated by a community whose size was, in principle, unlimited. And this we have seen in the role played by Boyle's literary technology in multiplying the witnessing experience.

Scientific Knowledge and Exposition: Conclusions

I have shown that three technologies were involved in the production and validation of Boyle's experimental matters of fact: the material, the literary and the social. Although I have concentrated here upon the literary technology, I have also suggested that the three technologies are not distinct: the working of each depends upon and incorporates the others. I want now briefly to develop that point by showing how each technology contributes to a common strategy for constituting matters of fact.

What makes a fact different from an artefact is that the former is not perceived to be man-made. What men make, men may unmake, but a matter of fact is taken to be the very mirror of nature. 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 agency on to natural reality is to stipulate the grounds for universal assent. Each of the three technologies works to achieve the appearance of matters of fact as *given* items: each functions as an objectifying resource.

Take, for example, the role of the air-pump in the production of matters of fact. As I have noted, pneumatic facts were machine-made. The product of the pump was not, as it is for the modern scientific machines studied by Latour, an 'inscription': it was a visual experience that had to be transformed into an inscription by a witness.[75] However, the air-pump of the 1660s has this in common with the gamma counter of the present-day neuroendocrinological laboratory: 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 cognitive or moral faults in the human being, nor is a 'good' observation his personal product. It is the machine that has generated the finding. A striking instance of this usage arose in the 1660s when Christiaan Huygens offered a matter of fact produced by his pump which appeared to conflict with one of Boyle's central explanatory resources. Boyle did not impugn Huygens's integrity or his perceptual and cognitive competences. Instead, he suggested that the fault lay with the machine: '[I] question not his Ratiocination, but only the staunchness of his pump.'[76] The machine constitutes a resource that may be used to factor out human agency in the intellectual product: 'it is not I who says this; it is the machine that speaks,' or 'it is not your fault; it is the machine's.'

Boyle's social technology constituted an objectifying resource by making the production of knowledge visible as a collective enterprise: 'it is not I who says this; it is all of us.' As Sprat insisted, collective performance and collective witness served to correct the natural working of the 'idols': the faultiness, the idiosyncracy or the bias of any individual's judgement 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 everyone could come in; not everyone's testimony was of equal worth; not everyone was equally able to influence the official voice of the institution. 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 philosophers.

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 — each individual setting himself up as the ultimate judge of knowledge — would destroy the conventional basis of knowledge, while the disciplined collective social structure of the experimental language game 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 space. No one man was to have the right to lay down what was to count as knowledge. Legitimate knowledge was 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 generation and evaluation. Human coercion was to have no visible place in the experimental way of life.[77]

It was the function of the literary technology to create that communal way of life, to bound it, and to provide the forms and conventions of social relations within it. The literary technology of virtual witnessing supplemented the public space of the laboratory by extending 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 served to protect items of knowledge to which one could expect universal assent from items which produced divisiveness. Similarly, Boyle's stipulations concerning proper manners in dispute worked to guarantee that social solidarity which generated assent to matters of fact and to rule out of order those imputations which would undermine the moral integrity of the experimental way of life.

I have attempted to display these linguistic practices in the making, and, within restrictions of space, I have alluded to sources of seventeenth-century opposition to these practices. It is important to understand two things about these ways of expounding scientific knowledge and securing assent: that they are historical constructions and that there have been alternative practices. It is

particularly important to understand this because of the problems of givenness and self-evidence that attend the institutionalization and conventionalization of these practices. Just as the three technologies operate to create the illusion that matters of fact are not man-made, so the institutionalized and conventional status of the scientific discourse that Boyle helped to produce makes the illusion that scientists' speech about natural reality is simply a reflection of that reality. In this instance, and in others like it, the historian has two major tasks: to display the man-made nature of scientific knowledge, and to account for the illusion that this knowledge is *not* man-made. It is one of the recommendations of the sociology of knowledge perspective that analysts often attempt to accomplish these two tasks in the same exercise.[78]

In the late twentieth century scientific papers are rarely, if ever, written with the depth of circumstantial detail which Boyle's reports contained. Why might this be? The answer to this question leads us to the study of linguistic aspects of scientific institutionalization and differentiation. In discussing the characteristics of a *Denkkollektiv,* Ludwik Fleck noted that such a group cultivates 'a certain exclusiveness both formally and in content':

> A thought commune becomes isolated formally, but also absolutely bonded together, through statutory and customary arrangements, sometimes a separate language, or at least special terminology . . . The optimum system of a science, the ultimate organization of its principles, is completely incomprehensible to the novice [or, Fleck might have added, to any non-member].[79]

Fleck was suggesting that the linguistic conventions of a body of practitioners constitute an answer to the question 'Who may speak?' The language of an institutionalized and specialized scientific group is removed from ordinary speech, and from the speech of scientists belonging to another community, both as a sign and as a vehicle of the group's special and bounded status. Not everyone may speak; the ability to speak entails the mastering of special linguistic competences; and the use of ordinary speech is taken as a sign of non-membership and non-competence. Such a group gives linguistic indications that the generation and validation of its knowledge does not require the mobilizing of belief, trust and assent outwith its own social boundaries. (Yet, when external support or subvention is required, special *occasional* modes of speech may be resorted to, including the various languages of 'popularization'.)

By contrast, Boyle's circumstantial reporting was a means of involving a wider community and soliciting its participation in the making of factual experimental knowledge. His circumstantial language was a way of bringing readers into the experimental scene, indeed of making the reader an actor in that scene. The reader was to be shown not just the products of experiments but their mode of construction and the contingencies affecting their performance, *as if he were present*. Boyle aimed to accomplish this, not by inventing a totally novel language (although it was novel to the natural philosophical community of the time), but, it could be argued, by incorporating aspects of ordinary speech and lay techniques of validating knowledge-claims. The language of early Restoration experimental science was, in this sense, a public language. And the use of this public language was, in Boyle's work, essential to the creation of both the knowledge and the social solidarity of the experimental community. Trust and assent had to be won from a public that might crucially deny trust and assent.

● *NOTES*

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1. R. Boyle, 'New Experiments Physico-Mechanical, touching the Spring of the Air . . .', in Boyle, *Works,* ed. T. Birch, 6 Vols. (London, 1772), Vol. I, 1–117. (All subsequent references to Boyle's writings are to this edition and will be cited as *RBW*.)

2. I. Hacking, *The Emergence of Probability: A Philosophical Study of Early Ideas about Probability, Induction and Statistical Inference* (Cambridge: Cambridge University Press, 1975), esp. Chapters 3–5; B.J. Shapiro, *Probability and Certainty in Seventeenth-Century England: A Study of the Relationships between Natural Science, Religion, History, Law and Literature* (Princeton, NJ: Princeton University Press, 1983), esp. Chapter 2.

3. Newton's place in the development of a probabilist view of physical science is ambiguous. Certain of his critics thought that he aimed at the necessary assent which most English natural philosophers had agreed to eschew; see Z. Bechler, 'Newton's

1672 Optical Controversies: A Study in the Grammar of Scientific Dissent', in Y. Elkana (ed.), *The Interaction between Science and Philosophy* (Atlantic Highlands, NJ: Humanities Press, 1974), 115–42.

4. The usual form in which Boyle phrased this was the statement that God might produce the same effects in nature 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, 'Some Considerations touching the Usefulness of Experimental Natural Philosophy', *RBW*, Vol. II, 1–201, at 45 (orig. publ. 1663). See also L. Laudan, 'The Clock Metaphor and Probabilism: The Impact of Descartes on English Methodological Thought, 1650-65', *Annals of Science*, Vol. 22 (1965), 73–104; G.A.J. Rogers, 'Descartes and the Method of English Science', ibid., Vol. 29 (1972), 237–55; H.G. van Leeuwen, *The Problem of Certainty in English Thought 1630-1690* (The Hague: M. Nijhoff, 1963), 95–96; Shapiro, op. cit. note 2, 44–61.

5. This is especially evident in historians' treatment (or lack thereof) of criticisms of seventeenth-century experimentalism by philosophers who denied both the central role of experimental procedures and the foundational status of the matter of fact. For example, insofar as Thomas Hobbes's criticisms of Boyle's experimental programme have been discussed, historians have preferred to conclude that he 'misunderstood' Boyle, or that he 'failed to appreciate' the power of experimental methods: see, among others, F. Brandt, *Thomas Hobbes' Mechanical Conception of Nature* (Copenhagen: Levin & Munksgaard, 1928), 377–78; M.B. Hall, 'Boyle, Robert', in *Dictionary of Scientific Biography*, Vol. 2 (New York: Charles Scribner's Sons, 1970), 379; L.T. More, *The Life and Works of the Honourable Robert Boyle* (London: Oxford University Press, 1944), 97, 239. Hobbes's anti-experimentalism is fully treated in S. Shapin and S. Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle and the Experimental Life* (Princeton, NJ: Princeton University Press, forthcoming).

6. The use of the word 'technology' in reference to the 'software' of literary practices and social relations may appear jarring, but it is in fact etymologically justified, as Carl Mitcham nicely shows: C. Mitcham, 'Philosophy and the History of Technology', in G. Bugliarello and D.B. Doner (eds), *The History and Philosophy of Technology* (Urbana, Ill.: University of Illinois Press, 1979), 163–201, esp. 172 ff. The Greek *techne* has behind it the Indo-European stem *tekhn*, probably meaning 'woodwork' or 'carpentry'. However, in early Plato *techne* was also conceived as a kind of knowledge. In *Gorgias* Socrates distinguishes two types of *techne*: one which consists mainly of physical work and another which is closely associated with speech. By using 'technology' to refer to social and literary practices, as well as to hardware, I wish to stress that all three are *knowledge-producing tools*.

7. See, for example, Boyle, 'An Examen of Mr. T. Hobbes his Dialogus Physicus de Natura Aëris . . .', in *RBW*, Vol. I, 186–242, at 241 (orig. publ. 1662); Boyle, 'Animadversions upon Mr. Hobbes's Problemata de Vacuo', in *RBW*, Vol. IV, 104–28, at 105 (orig. publ. 1674). The explication of the behaviour of liquids in the gardener's pot was a set-piece in the mid-seventeenth-century contest between rival physical systems; see T. Hobbes, 'Concerning Body', in *The English Works of Thomas Hobbes*, ed. Sir William Molesworth, 11 Vols. (London, 1839-1845), Vol. I, 414–15 (orig. publ. 1656); compare Boyle, 'Examen of Hobbes', 191–93.

8. Boyle described his pump in 'New Experiments', op. cit. note 1, 6–11. One of

the best accounts of the original pump and subsequent designs is still G. Wilson, 'On the Early History of the Air-Pump in England', *Edinburgh New Philosophical Journal*, Vol. 46 (1849), 330–54; see also R.G. Frank, Jr, *Harvey and the Oxford Physiologists: A Study of Scientific Ideas* (Berkeley, Calif.: University of California Press, 1980), 128–30.

9. The only information we have concerning the cost of the Boyle pump indicates that a version of the *receiver* ran to £5: T. Birch, *The History of the Royal Society of London*, 4 Vols. (London, 1756-1757), Vol. II, 184. Given the expense of machining the actual pumping apparatus, an estimate of £25 for the entire engine might be conservative. Thus, an air-pump would have cost more than the annual salary of the Curator of the Royal Society, Robert Hooke, who was the London pump's chief operator. Christiaan Huygens's elder brother Constantijn pulled out of a pump-building project, 'being afraid of the cost': Christiaan Huygens, *Oeuvres complètes*, 22 Vols. (The Hague: M. Nijhoff, 1888-1950), Vol. III, 389. The Accademia del Cimento in Florence did not even try to build a *Machina Boyleana*, even though they had the necessary texts at hand: W.E. Knowles Middleton, *The Experimenters: A Study of the Accademia del Cimento* (Baltimore, Md: The Johns Hopkins University Press, 1971), 263–65. Full details of the career of the air-pump in the 1660s are in Shapin and Schaffer, op. cit. note 5, Chapter 6.

10. Boyle, 'Examen of Hobbes', op. cit. note 7, 193. Both 'elaborate' and systematic experimentation were also recommended as the bases for constructing well-framed theories. Those theories 'that are grounded but upon few and obvious experiments, are subject to be contradicted' by new findings; see Boyle, 'A Proëmial Essay . . . with Some Considerations touching Experimental Essays in General', in *RBW*, Vol. I, 299–318, at 302 (orig. publ. 1661).

11. See, for example, Boyle, 'The Sceptical Chymist', in *RBW*, Vol. I, 458–586, at 460 (orig. publ. 1661): here Boyle suggests that many 'experiments' reported by the alchemists 'questionless they never tried'. For an insinuation that Henry More may not actually have performed experiments adduced against Boyle's findings, see Boyle, 'An Hydrostatical Discourse, Occasioned by the Objections of the Learned Dr. Henry More', in *RBW*, Vol. III, 596–628, at 607–08 (orig. publ. 1672). Compare the response of Boyle to Pascal's trials and their reporting. Boyle reported the replication of the Puy-de-Dôme experiment in 'New Experiments', op. cit. note 1, 14, 43; and by Power, Towneley and himself in 'A Defence of the Doctrine touching the Spring and Weight of the Air . . . against the Objections of Franciscus Linus', in *RBW*, Vol. I, 118–85, at 151–55 (orig. publ. 1662). Yet Boyle doubted the reality of Pascal's other reports of underwater trials; see 'Hydrostatical Paradoxes, made out by New Experiments . . .', in *RBW*, Vol. II, 738–97, at 745–46 (orig. publ. 1666): '. . . though the experiments [Pascal] mentions be delivered in such a manner, as is usual in mentioning matters of fact; yet I remember not, that he expressly says, that he actually tried them, and therefore he might possibly have set them down, as things that *must* happen, upon a just confidence, that he was not mistaken in his ratiocinations . . . Whether or not Monsieur Pascal ever made these experiments himself, he does not seem to have been very desirous, that others should make them after him.' For the role of thought experiments in the history of science, see A. Koyré, *Galileo Studies* (Atlantic Highlands, NJ: Humanities Press, 1978), 97; T.S. Kuhn, 'A Function for Thought Experiments', in Kuhn, *The Essential Tension* (Chicago: The University of Chicago Press, 1977), 240–65; C.B. Schmitt, 'Experience and Experiment: A Comparison of Zabarella's View with Galileo's in

De motu', *Studies in the Renaissance*, Vol. 16 (1969), 80–137.

12. Boyle, 'Two Essays, Concerning the Unsuccessfulness of Experiments', in *RBW*, Vol. 1, 318–53, at 343 (orig. publ. 1661); Boyle, 'Sceptical Chymist', op. cit. note 11, 486. Cf. Boyle, 'Animadversions on Hobbes', op. cit. note 7, 110: here Boyle rejected Hobbes's claim to have observed a phenomenon that Boyle regarded as implausible; Hobbes 'does not here affirm, that he, or any he can trust, has seen the thing done . . . Wherefore, till I be better informed of the matter of fact, I can scarce look upon what Mr. Hobbes says . . . as other than his conjecture . . .'

13. Boyle, 'Some Considerations about the Reconcileableness of Reason and Religion', in *RBW*, Vol. 1V, 151–91, at 182 (orig. publ. 1675); see also L.J. Daston, *The Reasonable Calculus: Classical Probability Theory, 1650-1840* (unpublished PhD dissertation, Harvard University, 1979), 90–91; on testimony: Hacking, op. cit. note 2, Chapter 3; on evidence in seventeenth-century English law, see Shapiro, op. cit. note 2, Chapter 5; S. Schaffer, 'Making Certain (essay review of Shapiro), *Social Studies of Science*, Vol. 14 (1984), 137–52, esp. 146–47 (for the legal analogy of scientific witnessing).

14. T. Sprat, *History of the Royal Society* (London, 1667), 100.

15. One of the ways by which Hobbes attacked the experimental programme was to insinuate that the Royal Society was *not* a public place: not everyone could come to witness experimental displays; see T. Hobbes, 'Dialogus physicus de natura aeris . . .', in Hobbes, *Opera philosophica*, ed. Sir William Molesworth, 5 Vols. (London, 1839–45), Vol. 1V, 233–96, at 240 (orig. publ. 1661): 'Cannot anyone who wishes come, since as I suppose they meet in a public place, and give his opinion on the experiments which are seen as well as they? Not at all . . . the place where they meet is not public.' (Translation by Simon Schaffer.) Thomas Birch praised Boyle because 'his laboratory was constantly open to the curious'; see *RBW*, Vol. I, cxlv.

16. Boyle, 'New Experiments', op. cit. note 1, 1; Boyle, 'The History of Fluidity and Firmness', in *RBW*, Vol. 1, 377–442, at 410 (orig. publ. 1661); Boyle 'Defence against Linus', op. cit. note 11, 173.

17. R. Hooke, *Philosophical Experiments and Observations* (London, 1726), 27–28.

18. Sprat, op. cit. note 14, 98–99; see also Shapiro, op. cit. note 2, 21–22.

19. Boyle, 'New Experiments', op. cit. note 1, 33–34; Boyle, 'A Discovery of the Admirable Rarefaction of Air . . .', in *RBW*, Vol. 1I1, 496–500, at 498 (orig. publ. 1671); Boyle, 'Sceptical Chymist', op. cit. note 11, 460.

20. Boyle, 'The Christian Virtuoso', in *RBW*, Vol. V, 508–40, at 529 (orig. publ. 1690); see also Shapiro, op. cit. note 2, Chapter 5 (esp. 179). For a study of social accounting systems in the evaluation of observation reports, see R. Westrum, 'Science and Social Intelligence about Anomalies: The Case of Meteorites', *Social Studies of Science*, Vol. 8 (1978), 461–93. Explicit concern for the quality of testimony was much more intense in natural history than it was in experimental philosophy. In the latter, access to experimental devices was disciplined by their cost and location; thus, not everyone could *in practice* offer experimental testimony, while those that did were of known character, reliability and probity. By contrast, the offering of observation reports was almost completely undisciplined, and the reliability of such testimony was a matter of fundamental concern.

21. M. Boas [Hall], *Robert Boyle and Seventeenth-Century Chemistry* (Cambridge: Cambridge University Press, 1958), 40-41; Boyle, 'New Experiments', op. cit. note 1, 2; Boyle, 'The Experimental History of Colours', in *RBW*, Vol. I,

662–778, at 633 (orig. publ. 1663). Cf. 664, where certain 'easy and recreative experiments, which require but little time, or charge, or trouble in the making' were recommended to be tried by ladies. Richard Jones was the 'Pyrophilus' to whom other essays were addressed.

22. Boyle 'A Continuation of New Experiments Physico-Mechanical, touching the Spring and Weight of the Air', in *RBW*, Vol. III, 175–276, at 176. This was written in 1668 and printed a year later. Boyle was not being entirely straightforward here: Huygens's air-pump in The Netherlands had in 1662 produced a matter of fact — the so-called anomalous suspension of water — that seriously troubled Boyle's explanatory schema. Boyle never referred to this finding in print; see Shapin and Schaffer, op. cit. note 5, Chapter 6; S. Schaffer, 'Aethers, Air Pumps and Anomalous Suspension', *British Journal for the History of Science* (forthcoming).

23. Boyle, 'A Continuation of New Experiments, Physico-Mechanical . . . The Second Part', in *RBW*, Vol. IV, 505–93, at 505, 507 (orig. publ. 1680).

24. This practice can be contrasted with the iconography of the anti-experimentalist Hobbes whose natural philosophy texts included only a few images of experimental systems, and these very simple and highly stylized. In giving his account of the air-pump and how it worked, Hobbes deliberately scorned the use of pictures; see Hobbes, op. cit. note 15, 235, 242. For studies of engraving and print-making in scientific texts, see W.M. Ivins, Jr, *Prints and Visual Communication* (Cambridge, Mass.: MIT Press, 1969), esp. 33–36, and E.L. Eisenstein, *The Printing Press as an Agent of Change* (Cambridge: Cambridge University Press, 1979), esp. 262–70, 468–71.

25. Hooke to Boyle, 25 August and 8 September 1664, in *RBW*, Vol. VI, 487–90, and R.E.W. Maddison, 'The Portraiture of the Honourable Robert Boyle, FRS', *Annals of Science*, Vol. 15 (1959), 141–214.

26. Boyle, 'Continuation of New Experiments', op. cit. note 22, 178.

27. Unfortunately, this paper was completed before I was able to read Svetlana Alpers's brilliant *The Art of Describing: Dutch Art in the Seventeenth Century* (London: John Murray; Chicago: The University of Chicago Press, 1983). Alpers analyzes the purposes and the conventions of realistic pictures in seventeenth-century Holland, demonstrating substantial links between English empiricist theories of knowledge and Dutch picturing. Her Chapter on 'The Craft of Representation' is a superb examination of the pictorial conventions for generating realist responses. Evidently, the Dutch were trying to achieve by way of picturing what the English were attempting by way of the reform of prose.

28. Boyle, 'New Experiments', op. cit. note 1, 1–2 (emphases added). The role of circumstantial detail in Boyle's prose and in that of other early Fellows of the Royal Society is treated in Shapiro, op. cit. note 2, Chapter 7. See also two excellent unpublished papers: P. Dear, '*Totius in verba*: The Rhetorical Constitution of Authority in the Early Royal Society', typescript, Program in History of Science, Princeton University (a version will shortly appear in *Isis*); and J.V. Golinski, 'Robert Boyle: Scepticism and Authority in Seventeenth-Century Chemistry', paper delivered to conference on Linguistic Aspects of Science, Leeds University, 10-11 January 1984. I am very grateful to Dear and Golinski for allowing me to see their materials.

29. There is probably a connection between Boyle's justification for circumstantial reporting and Bacon's argument in favour of 'initiative' (as opposed to 'magistral') methods of communication in science: see, for example, D.L.

Hodges, 'Anatomy as Science', *Assays*, Vol. 1 (1981), 73–89, esp. 83–84; L. Jardine, *Francis Bacon: Discovery and the Art of Discourse* (Cambridge: Cambridge University Press, 1974), 174–78; K.R. Wallace, *Francis Bacon on Communication & Rhetoric* (Chapel Hill, NC: The University of North Carolina Press, 1943), 18–19. The magistral method, as Bacon said, 'requires that what is told should be believed; the initiative that it should be examined.' Initiative methods display the processes by which conclusions were reached; magistral methods mask those processes. Although Boyle's inspiration may, plausibly, have been Baconian, the 'influence' of Bacon is sometimes much exaggerated (for example, Wallace, 225–27). It is useful to remember that it was Boyle, not Bacon, who actually developed the literary forms of experimental communication; it is hard to imagine two more different forms than Bacon's aphorisms and Boyle's experimental narratives. See also a marvellous speculative paper on the *Cartesian* roots of contrasting styles of scientific exposition: J.W.N. Watkins, 'Confession is Good for Ideas', in D. Edge (ed.), *Experiment: A Series of Scientific Case Histories* (London: BBC, 1964), 64–70, and the better-known paper in the same collection by P.B. Medawar, 'Is the Scientific Paper a Fraud?' (7–12).

30. Boyle, 'Proëmial Essay', op. cit. note 10, 305–06; cf. Boyle, 'New Experiments', op. cit. note 1, 1: R.S. Westfall, 'Unpublished Boyle Papers relating to Scientific Method', *Annals of Science*, Vol. 12 (1956), 63–73, 103–17.

31. Boyle 'Unsuccessfulness of Experiments', op. cit. note 12, 339–40, 353; Recognizing that contingencies might affect experimental outcomes was also a way of tempering inclinations to reject good testimony too readily. If an otherwise reliable authority stipulated an outcome that was not immediately obtained, one was advised to persevere; see ibid., 344–45; Boyle, 'Continuation of New Experiments', op. cit. note 22, 275–76; Boyle, 'Hydrostatical Paradoxes', op. cit. note 11, 743; Westfall, op. cit. note 30, 72–73.

32. Boyle, 'New Experiments', op. cit. note 1, 26. For an example of Boyle reporting an experimental failure, see ibid., 69–70. A critic like Hobbes could capitalize upon Boyle's reported failures, or, more interestingly, deconstruct Boyle's reported successes by identifying further contingencies which affected experimental outcomes; see, for instance, Hobbes, op. cit. note 15, 245–46.

33. Boyle, 'Proëmial Essay', op. cit. note 10, 300–01, 307; cf. 'Sceptical Chymist', op. cit. note 11, 469–70, 486, 584. Several of the less modest personalities of seventeenth-century English science were individuals who lacked the gentle birth that routinely enhanced the credibility of testimony: e.g., Hobbes, Hooke, Wallis and Newton.

34. The best source for Boyle's social situation and temperament is J.R. Jacob, *Robert Boyle and the English Revolution: A Study in Social and Intellectual Change* (New York: Burt Franklin, 1977), Chapters 1–2.

35. Boyle, 'Proëmial Essay', op. cit. note 10, 318, 304. For the importance of the lens and the perceptual model of knowledge in seventeenth-century epistemology, see Alpers, op. cit. note 27, Chapter 3. The goal for Boyle, as for many other philosophers concerned with linguistic reform, was *plain-speaking*. For the linguistic programme of the early Royal Society and its connections with experimental philosophy, see F. Christensen, 'John Wilkins and the Royal Society's Reform of Prose Style', *Modern Language Quarterly*, Vol. 7 (1946), 179–87, 279–90; R.F. Jones, 'Science and Language in England of the Mid-Seventeenth Century', *Journal of English and Germanic Philology*, Vol. 31 (1932), 315–31; Jones, 'Science and

English Prose Style in the Third Quarter of the Seventeenth Century', *Publications of the Modern Language Association of America*, Vol. 45 (1930), 977–1009; V. Salmon, 'John Wilkins' *Essay* (1668): Critics and Continuators', *Historiographica Linguistica*, Vol. 1 (1974), 147–63; M.M. Slaughter, *Universal Languages and Scientific Taxonomy in the Seventeenth Century* (Cambridge: Cambridge University Press, 1982), esp. 104–86; H. Aarsleff, *From Locke to Saussure: Essays on the Study of Language and Intellectual History* (London: Athlone Press, 1982), 225–77; M. Hunter, *Science and Society in Restoration England* (Cambridge: Cambridge University Press, 1981), 118–19; Shapiro, op. cit. note 2, 227–46; and the sources cited in notes 28 and 29. For Boyle's attack on the 'confused', 'equivocal' and 'cloudy' language of the alchemists, see 'Sceptical Chymist', op. cit. note 11, 460, 520–22, 537–39; and, for his criticisms of Hobbes's expository 'obscurity', see 'Examen of Hobbes', op. cit. note 7, 227.

36. Boyle, 'Proëmial Essay', op. cit. note 10, 307 (emphases in original). On 'wary and diffident expressions', see also 'New Experiments', op. cit. note 1, 2; and compare Sprat, op. cit. note 14, 100–01; J. Glanvill, *Scepsis scientifica* (London: Kegan, Paul, Trench, 1885; orig. publ. 1665), 200–01. For discussions of Boyle's remarks in the context of probabilist and fallibilist models of knowledge, see Shapiro, op. cit. note 2, 26–27; van Leeuwen, op. cit. note 4, 103; Daston, op. cit. note 13, 164–65.

37. Boyle, 'Hydrostatical Discourse', op. cit. note 11, 596.

38. Boyle, 'New Experiments', op. cit. note 1, 2.

39. Boyle, 'Proëmial Essay', op. cit. note 10, 313, 317.

40. On the 'idols' and fallibilism, see Shapiro, op. cit. note 2, 61–62.

41. Boyle, 'Some Specimens of an Attempt to Make Chymical Experiments Useful to Illustrate the Notions of the Corpuscular Philosophy. The Preface', in *RBW*, Vol. I, 354–59, at 355 (orig. publ. 1661); Boyle, 'Proëmial Essay', op. cit. note 10, 302. On the corrupting effects of 'preconceived hypothesis or conjecture', see Boyle, 'New Experiments', op. cit. note 1, 47; and for doubts about the correctness of Boyle's professed unfamiliarity with the writings of Descartes and other systematists, see Westfall, op. cit. note 30, 63; Laudan, op. cit. note 4, 82n.; M. Boas [Hall], 'Boyle as a Theoretical Scientist', *Isis*, Vol. 41 (1950), 261–68; Boas, 'The Establishment of the Mechanical Philosophy', *Osiris*, Vol. 10 (1952), 412–541, at 460–61; Frank, op. cit. note 8, 93–97. My concern here is not with the veracity of Boyle's professions but with the reasons why he made them.

42. Shadwell's play was performed in 1676. There is some evidence that Hooke believed *he* was the model for Gimcrack; see R.S. Westfall, 'Hooke, Robert', in *Dictionary of Scientific Biography*, Vol. VI, 481–88, at 483. Charles II, the Royal Society's patron, was also said to have found the weighing of the air rather funny.

43. For the extent to which experimental philosophy was, in fact, popular, see Hunter, op. cit. note 35, Chapters 3, 6.

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

45. Boyle, 'Sceptical Chymist', op. cit. note 11, esp. 468, 513, 550, 584.

46. Boyle, 'Proëmial Essay', op. cit. note 10, 303.

47. Boyle's way of dealing with the hermetics drew on the views of the Hartlib group of the 1640s and 1650s. By contrast, there were those who rejected the findings of late alchemy (e.g., Hobbes) and those who rejected the process of assimilation (e.g., Newton).

48. Boyle, 'Examen of Hobbes', op. cit. note 7, 233, 197; Boyle, 'Defence against Linus', op. cit. note 11, 122.

49. Boyle, 'New Experiments', op. cit. note 1, 10.

50. Boyle, 'Continuation of New Experiments', op. cit. note 22, 250–58. Note that in other contexts Boyle encouraged speech of immaterial entities such as spirits; what he said was that such items ought to be purged from the routine discourse of experimental philosophy; see, for example, 'Hydrostatical Discourse', op. cit. note 11, 608.

51. Boyle, 'New Experiments', op. cit. note 1, 11–12; cf. Boyle, 'The General History of the Air . . .', in *RBW*, Vol. V, 609–743, at 614–15 (orig. publ. 1692).

52. These problems were structurally similar to those afflicting Newton later in the century. Newton said that he wished to speak of gravitation as a mathematical regularity, without venturing an account of its physical cause. Newton's allies and enemies alike found it difficult to accept such mathematical statements as the end-product of physical inquiry; see A. Koyré, *Newtonian Studies* (Chicago: The University of Chicago Press, 1968), 115–63, 273–82.

53. Boyle, 'Defence against Linus', op. cit. note 11, 135, 137; Boyle, 'Examen of Hobbes', op. cit. note 7, 191, 207; Boyle, 'Animadversions on Hobbes', op. cit. note 7, 112.

54. Hobbes, op. cit. note 15, 271, 273, 278; for Boyle's reply, see 'Examen of Hobbes', op. cit. note 7, 193–94.

55. Cf. Boas [Hall], 'Boyle', op. cit. note 41; Boas, 'Establishment', op. cit. note 41, 475–77.

56. Boyle, 'Proëmial Essay', op. cit. note 10, 312.

57. Ibid., 311.

58. See R.P. Multhauf, 'Some Nonexistent Chemists of the Seventeenth Century: Remarks on the Use of the Dialogue in Scientific Writing', in A.G. Debus and Multhauf, *Alchemy and Chemistry in the Seventeenth Century* (Los Angeles, Calif.: William Andrews Clark Memorial Library, 1966), 31–50.

59. Boyle, 'Sceptical Chymist', op. cit. note 11, 486. In the preface Boyle says that he will not 'declare my own opinion'; he wishes to be 'a silent auditor of their discourses' (460, 466–67).

60. The consensus that emerges is very like the position from which Carneades starts, but the plot of *The Sceptical Chymist* involves disguising that fact. Interestingly, the consensus is not total (a point nicely made by Golinksi, op. cit. note 28): Eleutherius indicates reservations about Carneades's arguments; and Philoponus (a more 'hard-line' alchemist who is absent for the bulk of the symposium) might not, in Eleutherius's opinion, have been persuaded. The obvious contrast is with the form and function of the dialogue in the writings of Boyle's anti-experimentalist adversary Hobbes, especially the *Dialogus physicus, Problemata physica* and *Decameron physiologicum*. Boyle strongly disapproved of Hobbes's dialogues, in which the 'Hobbes' character demanded, and secured, absolute assent from his interlocutor; see Boyle, 'Animadversions on Hobbes', op. cit. note 7, 105.

61. Boyle, 'Sceptical Chymist', op. cit. note 11, 462.

62. Actually, the great bulk of the talk is between Carneades and Eleutherius. The other two participants inexplicably absent themselves from most of the proceedings. This is possibly an accident due to Boyle's self-confessed sloppiness with his manuscripts; he was continually apologizing for losing pages of his drafts.

63. Boyle, 'Sceptical Chymist', op. cit. note 11, 469.

64. Boyle's responses to his adversaries are closely examined in Shapin and Schaffer, op. cit. note 5, Chapter 5.

65. Boyle, 'Examen of Hobbes', op. cit. note 7, 190; cf. Boyle, 'Hydrostatical Discourse', op. cit. note 11, 596–97; Boyle, 'Animadversions on Hobbes', op. cit. note 7, 104–05; Boyle, 'Defence against Linus', op. cit. note 11, 118–21.

66. Boyle, 'Defence against Linus', op. cit. note 11, 122; cf. Boyle, 'Examen of Hobbes', op. cit. note 7, 197, 208, 233.

67. Boyle, 'Defence against Linus', op. cit. note 11, 163, 120.

68. Boyle, 'Examen of Hobbes', op. cit. note 7, 186.

69. Ibid., 188; cf. 190.

70. Hobbes, 'Six Lessons to the Professors of the Mathematics . . . in the University of Oxford', in Hobbes, op. cit. note 7, Vol. VII, 181–356, at 356 (orig. publ. 1656).

71. Hobbes, 'Considerations upon the Reputation, Loyalty, Manners, and Religion of Thomas Hobbes', in Hobbes, op. cit. note 7, Vol. IV, 409–40, at 440 (orig. publ. 1662).

72. Boyle, 'New Experiments', op. cit. note 1, 3. The phrase 'wonderful pacifick year' is from Sprat, op. cit. note 14, 58.

73. Sprat, op. cit. note 14, 53, 56.

74. The focus of the paper has been upon an individual, yet its purpose is not individualistic. Boyle was a major innovator and practitioner of the new linguistic technologies. Nevertheless, he proposed them as routine practices for a *community*, and it is clear that Boyle's proposals were widely applauded and implemented, especially in the early Royal Society. His sentiments on this subject were, as I have briefly indicated, echoed by Sprat, Glanvill and Hooke, among others. For further details on the relevant English community of language users, see Shapiro, op. cit. note 2, Chapter 7.

75. B. Latour and S. Woolgar, *Laboratory Life: The Social Construction of Scientific Facts* (Beverly Hills, Calif.: Sage, 1979), Chapter 2; and, for a fine study of the role of instruments in scientific observation reports, see T.J. Pinch, 'Towards an Analysis of Scientific Observation: The Externality and Evidential Significance of Observational Reports in Physics', *Social Studies of Science*, Vol.15 (1985), in press.

76. Boyle to R. Moray, July 1662, in Huygens, op. cit. note 9, Vol. IV, 217–20; cf. Boyle, 'Defence against Linus', op. cit. note 11, 152–53.

77. Sprat, op. cit. note 14, 85 (for 'eyes and hands'), 98–99 (for the individual and the collective), 28–32 (for 'tyrants' in philosophy). For the disciplining of the Royal Society's public, see Jacob, op. cit. note 34, esp. 156, and J.R. Jacob, *Henry Stubbe, Radical Protestantism and the Early Enlightenment* (Cambridge: Cambridge University Press, 1983), 59–63; also some brilliantly perceptive remarks in Y. Ezrahi, 'Science and the Problem of Authority in Democracy', in T.F. Gieryn (ed.), *Science and Social Structure: A Festschrift for Robert K. Merton, Transactions of the New York Academy of Sciences*, Series II, Vol. 39 (New York: New York Academy of Sciences, 1980), 43–60, esp. 46–53.

78. See especially the work of Collins whose metaphor of completed and consensual scientific knowledge as 'the ship in the bottle' nicely crystallizes this point: for example, H.M. Collins, 'The Seven Sexes: A Study in the Sociology of a Phenomenon, or the Replication of Experiments in Physics', *Sociology*, Vol. 9 (1975), 205–24; Collins, 'Son of Seven Sexes: The Social Destruction of a Physical

Phenomenon', *Social Studies of Science*, Vol. 11 (1981), 33–62. Cf. Latour and Woolgar, op. cit. note 75, 176: 'Our argument is not just that facts are socially constructed. We also wish to show that the process involves the use of certain devices whereby all traces of production are made extremely difficult to detect.' For an historical study making a similar point, see S. Shapin, 'The Politics of Observation: Cerebral Anatomy and Social Interests in the Edinburgh Phrenology Disputes', in R. Wallis (ed.), *On the Margins of Science: The Social Construction of Rejected Knowledge, Sociological Review Monograph* No. 27 (Keele, Staffs: Keele University Press, 1979), 139–78.

79. L. Fleck, *Genesis and Development of a Scientific Fact*, trans. F. Bradley and T.J. Trenn, eds Trenn and R.K. Merton (Chicago: The University of Chicago Press, 1979), 103, 105 (orig. publ. in German, 1935).

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