3. We Beg to Differ: Competing Histories in STS

One of my aims in this chapter will be to explore each of Shapin and Schaffer's major theses in some depth, indicating the kinds of concerns other scholars have expressed about them. My other aim will be to show that these theses are interdependent to the extent that a serious question about any one of them may cause critical damage to the overall structure of their argument. Shapin and Schaffer's first three theses relate to the three "technologies" they associate with Boyle and the Royal Society (social, literary, and experimental). The next two theses concern the interrelationship between tactics for achieving "peace" in the context of scholarly and scientific society, and in society at large. The following two theses relate to Hobbes's intellectual contributions and the reasons for his exclusion from the Royal Society; and the last thesis is a summary and synthesis of all the others, in the form of a broad argument that the invention of scientific community demonstrates the full continuity of science with politics.

3.1 The Royal Society

> *Thesis 1: The Royal Society represented itself as a unique social institution for the incubation and creation of a new, ideal type of community, one both public, and yet removed from other areas of social life, theology, politics, etc.; and as such it served as a model society for resolving problems of knowledge and as the basis for modern scientific practices*[29].

With this thesis, Shapin and Schaffer tread on ground made familiar by Merton many years before. The important difference between their version of this argument and Merton's is the irony they find in this self-representation by the Royal Society. Obviously, in Shapin and Schaffer's view, the Royal Society was (as Hobbes "correctly" perceived) neither public, nor removed from the social controversies and context of its time.

But in their view, the Royal Society was not only a politically necessary solution to the problem of social order, but also a unique and novel invention of its time. Shapin and Schaffer are followed to a large extent in this claim by Daniel Garber, although Garber struggles to distinguish his own view from theirs. He is particularly concerned about the "deflationary" consequences of adopting Shapin and Schaffer's interpretation of this history:

[29] For locations of this thesis in *L&AP* , see for instance 5; 283-319; also 340-341, esp. 341.

> My account of the view of experimental facthood in the Royal Society is, in a way, not particularly novel; *in essence, it is at the backbone of Shapin and Schaffer's important study, Leviathan and the Air-Pump*. But what I want to emphasize is that this *communitarian* view of experimental facthood was something quite new, a self-conscious innovation introduced by the Royal Society in the 1660's . One might possibly be able to find precedents for this, although I doubt it.....But what is important is that it is an idea that is not found in the important theorists of scientific practice in the generations immediately preceding the foundation of the Royal Society and was regarded as an innovation by the Royal Society itself.....*the social conception of experimental facthood is an idea with a history;* it arises at a particular time, in particular contingent circumstances.....I should emphasize that the questions that most interest me are different from the questions that Shapin and Schaffer attempt to answer in Leviathan and the Air-Pump. Their questions concern the history of experimental philosophy as such, why experimental philosophy as such arose in England when it did, and how and why it came to triumph over a different and nonexperimental conception of science, such as that represented by Hobbes. To answer this, they appeal to the political context of the debates and the way Hobbes's and Boyle's positions fit into that context. The answer they offer is interesting and worth taking seriously, but it is not an answer to the questions that interest me most. Shapin and Schaffer seem to take it for granted that the very idea of experimental science carries with it a social criterion of experimental facthood....My interest is in the circumstances under which this criterion arose (Garber, 1995, 196-7; n. 18; emphasis added).

Despite Garber's careful differentiation of his own interests from Shapin and Schaffer's, he really does share with them a keen interest in the same research question: that is, the circumstances under which the Royal Society and its new epistemic criteria arose. The essential difference in their approaches is that Garber identifies the key factor in these circumstances as the emergence of a critical mass of capable (educated, interested) experts; while Shapin and Schaffer see political circumstances as more significant causal factors. But both see a unique set of circumstances yielding a novel epistemic strategy based on social criteria.

Thus more trenchant critiques of this part of Shapin and Schaffer's argument come from scholars who suggest that the social organization "invented" by the Royal Society was not unique. First, some have argued that an important precedent for this form of organization was available in the English legal and economic social structures and mores of the time. They point to the social and economic roles played by the members of the Royal Society, both as lawyers and as landed gentry with strong agricultural interests, suggesting that these roles may indeed have been adapted to the purposes of the Royal Society, but were not *invented* by it. Second, the Royal Society emerged more or less alongside other formal academies in France and elsewhere, suggesting that the "invention" of the Royal Society was simply a uniquely English version of a much more general trend both in the institutionalization of certain practices and ideas, and in the encounter between the privileged classes, and natural philosophy.[30]

[30] This point demonstrates the difficulty of sorting out the argument cleanly: the issue of British originality (that is, the thesis of "England as crucible" for modern science) is dealt with further in section 3.5 below.

Rose-Mary Sargent has offered compelling evidence of the strong relationship between the strategies of the Royal Society and the tradition of common law. At the very least, Sargent suggests, those who do sociology of science also need to do "comparative study of the corresponding communities in other fields.... other bodies of knowledge and other occupational groups" (Sargent, 1989, fn. 11). Looking back to Bacon, she says, one already had indications that common law offered a strong model to the new science: Bacon argued that "Natural histories had to be constructed in the manner in which legal histories had been" and that reason alone cannot produce true theories (Sargent, 1989, 29). Sargent argues that distinction drawn by Boyle and the Royal Society between experimental, and mathematically-based science was anticipated by the contrast between the Roman law notion of "strict numerical probability," and the common law "model of demonstration in which experience was fundamental.....The experiential foundation of lawyers went beyond a mere accumulation of facts to a reasoned interpretation of the facts. Their notion of 'experience' relied heavily on the idea of an *expert*" (Sargent, 1989, 28). The law of precedent was a law based in an unsystematic accumulation of collective and consensual knowledge that placed little authority in individual judgment or individual cases.

The idea of a trusted community of witnesses that was at the foundation of the English common law system, was an immediate and explicit model for the "trying" of experimental fact. Although this relationship is touched on lightly by Shapin and Schaffer (*L&AP*, 327), it deserves much greater attention. At the very least it suggests that the particulars of the development of English law might have yielded a less-than-universal model for the conduct and epistemic standards of modern science.

It might also be helpful to look more closely at the English agricultural community and its organization and concerns, as a model for the Baconian flavor of the new experimental science. [31]

Suffice at this point to raise the question of the relative importance of the British, Italian, and French academies which all arose at approximately the same time.

[31] In her master's thesis (1993), Kathleen Whalen has argued that Shapin and Schaffer were wrong in leaving the distinct impression that experimentation and (especially) the literary technology used to describe it were new with the Royal Society. Whalen found methodological and literary precursors in the agricultural literature of the period. Her suggestion was that Boyle and the Royal Society adopted a set of empirical practices and literary strategies that were already functioning successfully in the culture and that were familiar to their gentry peers/audience. In a paper Whalen gave at the History of Science Society annual meeting in 1996, she suggested that the gentry in the seventeenth century were anything but 'disinterested'. Rather, in her view, the gentry acquired credibility because they were engaging explicitly

Here Garber is helpful in offering an extended comparison and contrast between the Baconian model of collective scientific endeavor, and that of the Royal Society (Garber, 1995). The strong pragmatic thrust of the new science, with its emphasis on the unique capability of man to control and manage nature, is surely worthy of greater attention by Shapin and Schaffer. It suggests that the social standing and economic interests of the Royal Society were nontrivial factors in the development of their experimental *and* communal approach to securing knowledge of the natural world. This is important because a pragmatic orientation to knowledge might be argued to have a logical, if indirect, relationship to the communal organization of scientific inquiry. It would require a serious detour to explore the connection adequately, but American *philosophical* pragmatist Joseph Rouse observes that there is an innate association between pragmatism and community consensus, through a

> ... dialogical model of rationality that stresses the practical, *communal* character of this rationality in which there is choice, deliberation, interpretation, judicious weighing and application of "universal criteria," and even rational disagreement about which criteria are relevant and most important (Rouse, 1987, 7).

But how much did the pragmatic thrust (in the informal, not formal philosophical, sense of "pragmatic"), and legal form of argument in early modern English experimentalism have to do with science? Was the pragmatic "form of life" in its Royal Society incarnation the first appearance of what we now know as “science”? It is remarkable that Shapin and Schaffer refer only tangentially to developments in the organization of science on the continent, and draw almost no attention to the importance of Hobbes’s interactions with significant continental scientists and philosophers, among them Galileo, Descartes, and Mersenne.[32] It is even more remarkable that this has drawn so little comment from Shapin and Schaffer’s critics.[33]

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with the problems of the time (most of which were economic), not because they were economically above the fray (personal communication, November 1998).

[32] These contacts are interesting in light of evidence that Hobbes's systematic philosophy is much more comfortable and interesting to those brought up in a continental philosophical tradition, than those with an Anglo-American philosophical background.

[33] Although I. Bernard Cohen did remark, in his brief review, that "the authors [Shapin and Schaffer] have not even ventured any speculations on the relations between scientific and civil polity in seventeenth-century France or in the Netherlands and Germany" (Cohen, 1987, 649).

For a little perspective on England's first major scientific society, one might turn to Roger Hahn's history of the Paris Académie Royale des Sciences, chartered six years after the Royal Society. Hahn describes his research in the following passage:

> This study illustrates the extent to which scientific institutions are shaped by the requirements of both science and society. ....[T]here is a need to recognize that [scientific institutions] are the main instrument for the formulation and the transmittal of scientific norms, and that these norms must harmonize with the dominant nonscientific modes of the time.....The institution's success is in large part determined by its ability to make the peculiar enterprise of science palatable to society while at the same time maintaining the advancement of the discipline itself (Hahn, 1971, Preface).

Given the crucial role that Shapin and Schaffer assign to the Royal Society, it is worth observing that Hahn never identifies the Académie as a French "knock-off" of the genuine article. Rather, Hahn's central point is the continuity between the social organization, activities, and "form of life" of the Académie—"the model institution of its time"—and the very French development of the Enlightenment—with its new secular religion and tremendous political implications.

In 1977 Everett Mendelsohn published an outstanding, if brief essay on "The Social Construction of Scientific Knowledge".[34] While he argues for a social understanding of scientific knowledge, Mendelsohn also speaks eloquently for the irreducible psychological and historical dimensions of that knowledge. He further notes that "the human approach to understanding, explaining and interacting with nature has certainly not been uniform through time, nor across cultures." He also stresses that what we call "the" scientific revolution might be understood as encompassing at least two generations—the first responding to a world of insecurity and anxiety, of instability of the old orders, and challenge to all authorities; the second a generation of "establishers and institutionalizers." Emblematic of the sequence of generations, Mendelsohn suggests, was the eventual succession in 1657 by the *Accademia del Cimento*, of the "Renaissance" *Accademia dei Lincei* of which Galileo was a member (Academy of the Lynx—an animal that sees in the dark) -- some decades after the latter's disappearance around 1630.

Any view of the Royal Society as the laboratory in which modern science was invented may also suffer from the "assumption of [cultural] convergence" that characterizes other studies of

[34] After reading just a few paragraphs I found myself asking, how was it possible to forget, by 1985, so much that we knew in 1977? See E. Mendelsohn, P. Weingart, and R. Whitley, eds., *The Social Production of Scientific Knowledge*, 3-26; Boston: D. Reidel.

modernization and modernity. Western scholarship concerning early modern European science is vast relative to that of either natural science or modernity in non-Western cultures. A 1998 issue of *Daedalus* devoted to "early modernities" (Eisenstadt, 1998) notes the "need for seeing modernity as something other than a single condition with a preordained future." In view of the international scope of modern science, it may be thoughtless to assume that this internationalization is proof that all modernities have converged on a single model.

It seems, in summary, that Shapin and Schaffer have adopted some fairly entrenched historical views of the Royal Society in assuming (really without argument) that it was novel and unique both as an organization for the pursuit of natural knowledge, and as a social form for settling disputed knowledge. But the broader evidence from other areas of the world and even of Restoration society in England would indicate that there was considerable mixing and borrowing of existing social forms in putting together the Royal Society.[35] Thus the origins story Shapin and Schaffer tell seems constructed—not in an essential and inevitable sense, but in the less satisfactory sense that it only makes sense of selected evidence.

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[35] One of the hazards of a project such as this is that important historical lines of inquiry can at most be suggested. There is of course a wonderfully rich and fairly long historical tradition of studies of Renaissance, early modern, Enlightenment, and later scientific organizations. Their period of great eminence was, at least one scholar has argued, bounded by the early nineteenth century: the current scene is complicated by the emergence not only of professional and specialist societies but by changes in the substance and patterns of contacts between scientific organizations, government, and industry (McClellan, 1985).

3.2 Literary Technology[36]

Thesis 2: Robert Boyle in particular, and the Royal Society in general, also introduced a new literary technology; and this technology, by creating the effect of virtual "witnessing," was capable of turning even isolated activities into community events or "matters of fact."

A second thesis of Shapin and Schaffer's is that what they call "the experimental programme" of Robert Boyle and the Royal Society was, in Wittgenstein's phrases, a "language-game" and a "form of life" (*L&AP*, 22). The "language-game" required a "literary technology" capable of creating a virtual experience in its readers—one that would allow many individuals to share the sense of having witnessed the behavior of the "material technology" -- the experimental instrument and apparatus itself (*L&AP*, 25-6). Further, they claim that *Boyle himself* saw the critical role of an experiment as rhetorical, not "actual": that is, for Boyle an experiment was only powerful and valid if it obtained the assent of virtual witnesses. In order to create a *literary* facsimile of the experience of witnessing, Shapin and Schaffer say, Boyle invented a style of experimental account that is widely described as "prolix". This involves not only verbosity, but ornate sentence structure, conveying in a literary style the impression of countless circumstantial details. They contrast this style with Bacon's, and reference a number of other historians of science (Medawar, Jardine, Gillispie) to corroborate this judgment. Boyle's style of writing was intended not only to allow the reader the virtual experience of following the experimenter, but

[36] Following Shapin and Schaffer's principal argument, this section will be limited to addressing Boyle's literary technology and Shapin and Schaffer's claims about it. Not addressed here (though it is addressed at 92-98 and 143-146 in *Leviathan and the Air-Pump*) is Hobbes's own considerable rhetorical skill and distinctiveness. References on this include David Johnston, *Rhetoric of Leviathan: Thomas Hobbes and the Politics of Cultural Transformation* (Princeton: Princeton University Press, 1986); Raia Prokhovnik, *Rhetoric and Philosophy in Hobbes's Leviathan* (New York: Garland, 1991); and Robert P. Kraynack, *History and Modernity in the Thought of Thomas Hobbes* (New York: Cornell, 1993). Hobbes's style contrasted strongly with Boyle's of course. Samuel Mintz has characterized Hobbes's "powerful and subtle" prose as "by turns witty, ironical, didactic, and sententious," distinguished by an artistic "unity, order, and coherence" (Mintz, 1962 17-18). Hobbes's philosophy of language was quite explicit also, emphasizing the importance of explicit and consistent meanings. However, Sam Black has argued that to characterize Hobbes's civil philosophy as a "venture in rhetoric" (as Tom Sorell has done) is to mistake Hobbes profoundly: "What is especially objectionable about the rhetorical interpretation...is that it hopelessly muddles up the connection between ethics, science, and skepticism in the early modern period" (Black, 1997 203-204). Black is referring, for example, to Sorell's 1988 article, "The Science in Hobbes's Politics," where Sorell suggests that "Hobbes's civil science contains far more advocacy than explanation, far more moralizing....prescriptions and prohibitions" than science (Sorell, 1988, 78-79).

also to convey the importance of social attributes in the author which made him reliable:[37] honesty, modesty, patience, diligence, and ingenuity.

Although Shapin and Schaffer initially represent the material, literary, and social technologies employed by Boyle as equally important, they later suggest that the material and literary are in important ways merely props for the most important technology—the social: "The role of Boyle's literary technology was to create an experimental community." This community's primary identity was one of "objectivity" acquired by virtue of its collective and consensual approach to making knowledge claims (*L&AP*, 77). Thus the "literary technology" argument needs to be assessed not only in its own right but as one of the evidentiary props for Shapin and Schaffer's analysis of Boyle's "social technology."

Shapin and Schaffer's arguments with respect to Boyle's rhetoric have been supported by other scholars on these points. Latour observes that the construction of "fact" becomes "[t]he ratchet...in place that is going to give modern science its most spectacular feature: irreversible accumulation." What is crucial for Latour is that Boyle has limited the attempt to gain consensus to agreement on "facts," without requiring agreement on the interpretation of causes. Other scholars have agreed that the early Royal Society had a distinct—and novel—prose style;[38] Peter Dear has emphasized how historians of the period should pay attention to:

> ... language, rhetoric, and textual forms in understanding how science has been created...[and]...that language is not simply a transparent medium of communication, but a shaper (perhaps a realizer) of thought and an embodiment of social relations (Dear, 1991, 4).

In Dear's view, the new scientists sought to replace the authority of ancient texts with a new authority: the "text" or "book" of nature. This idea is not new with Shapin and Schaffer;[39] Dear writes that with the Royal Society's rhetorical style, "[a] *new source of authority* had in fact come

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[37] At least, Gillispie (1960, *The Edge of Objectivity*) found him so!, see *L&AP*, fn. 85, 63.

[38] "The most important recent addition to the continually growing body of literature on the early Royal Society and 'prose style' ....is Brian Vickers, 'The Royal Society and English Prose Style: A Reassessment,' " (at 3-76 in Brian Vickers and Nancy S. Struever, eds., *Rhetoric and the Pursuit of Truth: Language in the Seventeenth and Eighteenth Centuries.* Los Angeles: Clark Memorial Library). See Dear, 1991, *The Literary Structure of Scientific Argument,* 4.

[39] Dear, 1985 , "'Totius in Verba': Rhetoric and Authority in the Early Royal Society."

into being, a means of facilitating a cooperative enterprise dedicated to the expansion of natural knowledge. *This new authority overrode individual differences in ontological commitment*" (Dear, 1985, 262; emphasis added).

Henry Krips agrees that Boyle and the Royal Society invented a "new realist style" of scientific rhetoric: "Boyle was the inventor of a style that is the prototype for the modern scientific form of writing experimental accounts" (Krips, 1994, 54, fn. 2). But Krips also raises issues that don't sit well with the arguments pursued by Dear, or Shapin and Schaffer. Krips characterizes the "new realist style" as "free from rhetoric;" this new style is still with us today, says Krips: it is the model that allows scientists to "act as if particular styles in which they are trained to write are free from any rhetorical devices; in particular, they have continued to *accept the authority that accrues to texts by virtue of an alleged freedom from rhetoric*" (Krips, 1994, 53). Further, this style gave rise to the "double bind" of an empiricist epistemology "that made the reader's own experiences the ultimate source of reasons for belief, even while the reader had neither witnessed nor was able to reproduce the experiments" (Krips, 1994, 60).

But while acknowledging that the rhetoric of science is a worthy subject,[40] it is important to recall that Boyle's famously prolix style was anything but "free from rhetoric," and that the allegedly dry "realist" style is that which survives today. This underscores that the authority of science is credited to its "objectivity" -- and this standard was invented not by Boyle but by a later generation of scientists. What Boyle offered by way of a model for scientific rhetoric was rather the presentation of "facts" devoid of interpretation or causal argumentation. And this was Bacon's legacy, rather than an invention of Boyle or the Royal Society. Without examining the details of how, and why, Boyle's own literary style evolved over the course of his lifetime,[41] it

[40] Dear (1991) cites a number of important works associated with the "literary turn" in the history of science, among them Latour (1987), Greg Myers (*Writing Biology*, 1990); Woolgar, ed. (*Knowledge and Reflexivity*, 1988); and Mulkay and Gilbert (1984).

[41] Though to do so adds interesting new elements to the story: Boyle's early, pre-experimental literary style was extremely "literary" and fanciful. At around the time he began to set up a successful home laboratory, he also went through a period of illness which meant that he wrote more "plainly" -- because through an amanuensis. Later in life Boyle's rhetorical style was characterized, in part, by frequent rewriting and overlaying of earlier versions. See Hunter: "What is striking is that we see a writer who differed quite markedly from the mature Boyle, both in his preoccupations and his techniques. If one did not know that these writings were by the same person, one would have difficulty believing it .... There was a point at which Boyle changed quite dramatically from the early moralist to the later scientist..." (Hunter, 1995, 61, 65).

seems questionable whether Boyle's style of rhetoric can be taken as a crucial technology in the construction of the new science. While it is true that *Boyle* defended his own style in these terms, it is not so clear that his claim should be taken at face value.

Are there other individuals or institutions with an equally plausible claim to having forged a new scientific literary technology? Aside from Bacon, there are indeed other alternatives, pointing in the direction not of "virtual witnessing" of immediate, discrete experience, but rather to a style in which impersonal language, distance, selection, and summarization were prominent features. An obvious alternative is the Académie des Sciences. Frederic Holmes has noted that the members' papers read to and published by the Academy became, "by the early eighteenth century, the most consistent exemplars of the form that has persisted as the modern research paper" (Holmes, 1991, 166). Holmes associates this rhetorical style with the Academy's decision, from the time it was formed, to extend membership only to full-time scientific professionals. The Academy deliberately organized its scientific work along collective lines, "supposing that they could in this way attain more certain knowledge, freed from individual human biases and misjudgments" (Holmes, 1991, 166-167). Thus the Academy modeled a rhetorical style very different from Boyle's; while part of this difference might be attributed to the inherent limitations of Boyle's prolix style, Holmes' history suggests that since authority within the Academy derived from (or was reflected in) its formal membership, the need to establish authority rhetorically was correspondingly diminished.

Though Shapin and Schaffer identify the abstinence from interpretation and explanation closely with the rise of the new experimental science,[42] the association between the new science and this abstinence is not evident in the Academy's papers, even early in the eighteenth-century. The important role of interpretation and argument, even in literary contexts that incorporate experimental narratives and findings, suggests that it is naive to think that these can (or should) be omitted from scientific papers. A close study of Boyle's *personal* reasons for avoiding strong "philosophical" assertions in fact yields the picture of a man all too keenly aware of his own limitations as a scientist and philosopher (Hunter, 1995). It is not surprising, perhaps: the young moralist became impressed with the complexity of matters on which he had formerly been confident to speak, and acknowledged his own lack of academic training. Such considerations

[42] See *L&AP* at 66-68 on Boyle's deliberate separation of uninterpreted narratives and his "occational 'discourses' on their interpretation. One might then read the experiments and the 'reflexions' separately."

might well driven him to employ, and defend, this new rhetorical style.[43] But it does not follow that this style was critical for the establishment of a scientific community (peaceful or otherwise).[44]

Whatever influence Boyle's literary style may have had on other early modern scientists, that influence seems to have given way rather quickly in England to a second generation whose literary style was marked more by the professional distance and summarization of detail, noted as features of the Academy's style. Another interpretation might even find in Boyle's deliberate and exhaustively detailed accounts that passion for disciplining at a most "capillary" level that once again reminds us of Foucault's account of the essence of modernism. The detail could be suppressed, one might speculate along these lines, only once the communal discipline of detail had been sufficiently internalized to permit its overt exclusion[45]. Boyle's literary style, far from being midwife to a new science, may therefore have represented an odd mixture of the old and new during a time of transition. But transition to what? Timothy Reiss has suggested that there were important, and deep, changes afoot in the shift from language-based, to mathematically-based natural philosophy:

> ...the dilemmas posed from the early sixteenth century by a far older idea of language as tool for discovery began, as it were from within the language debates themselves, to push people towards a mathematically directed solution (Reiss, 1997, 130).

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[43]This uncertainty was an important theme of Boyle's. In 1681, in his "Discourse of Things above Reason, Enquiring Whether a Philosopher Should Admit There Are Any Such," Boyle writes,

> "I shall not scruple to acknowledge that, partly by my own experience, and partly by the confessions of others and by their unsuccessful attempts, I am induced to think that God....hath...made us men of a limited nature (in general), and of a bounded capacity" (cited in Pycior, 1997, at 176; original from *Selected Philosophical Papers of Robert Boyle*, ed. J. A. Stewart (Manchester: Manchester University Press, 1979, 238-239).

[44] As I have mentioned in the first chapter, Boyle's association both with law and agriculture -- as a landed gentleman -- also meant that his style of writing might not have been "invented" as a crucial technology of the new science, but simply be one to which he was accustomed from those personal associations. See Whalen, 1993.

[45] See the note by Conant (1961), on Boyle's "prolix" style: "While admiring Boyle's candor and his determination to report all the details, which set the standard for subsequent investigations, one must admit that unless a greater degree of selection had been made by later experimenters the literature of science would have become impossibly burdened with irrelevant details of inconclusive inquiries. Many generations of experimentalists have gradually evolved an unwritten code that governs the way in which experiments are now reported" (Conant, 1961, 57).

Echoing this observation, Theodore Porter contrasts seventeenth-century experimental philosophers such as Boyle, and their "great fondness for the odd happening," with the relatively austere and idealized accounts of Charles Dufay in the early eighteenth century. "[S]ingular events," writes Porter, "provided a poor basis for making communities of researchers," whereas the later style of experimental science and its reporting enhanced the "lawlikeness of nature....and spirit of public knowledge, at least within the specialist community, since close laboratory control offered the best chance for reproducing work at other sites" (Porter, 1994,18).

Thus it is questionable whether a rhetorical style and literary technology such as Boyle's was really critical or even influential in allowing the new experimental science to develop. It is equally plausible that Boyle's literary style reflected the contingencies of his own personal development; and that in developing his own personal style, he borrowed from other "forms of life" with which he was familiar, such as common law or agricultural research (see note 35 above). While Boyle's style may have reflected the peculiar social and political circumstances of England during the Restoration, it was not a style that was widely adopted, or used in scientific literature, even a few decades afterward. It could be that Boyle's literary technology was neither necessary nor necessarily influential in the emergence of experimentally-oriented scientific organizations and their communications in late seventeenth and early eighteenth centuries.

3.3 Experiment vs. Mathematics

Thesis 3: Boyle and the Royal Society also forged an experimental model for science, centered on a material technology (the experimental apparatus), in contradistinction to mathematically-based science, championed by Hobbes.

The third technology that Shapin and Schaffer associate with Boyle's new scientific program is the "material technology" of the experimental apparatus. The air-pump and its setting (laboratory) represented a "disciplined space" in which individual sensory perceptions could be disciplined and collectively controlled. Hobbes's skepticism concerning the air-pump -- on many counts -- is taken as evidence for what Shapin and Schaffer pose as Hobbes's opposition to the experimental program generally, in favor of a rationalist "technology" grounded in mathematical reasoning. But beyond the obvious quarrel between Hobbes and Boyle concerning Boyle's apparatus, is this depiction accurate, of mathematics and experiment as broadly opposed

technologies from which experiment emerged as the heart of the new science? In what follows I will first look at how other scholars have viewed the characterization and contrast between Hobbes and Boyle, and then look at another important way of viewing their contrasting approaches to science that focuses not on the politics of experiment but on the relative strengths and contrasting implications of a science grounded in geometry and one centered on algebra.

HOBBES THE EXPERIMENTALIST? To begin with, we must decide what to make of Shapin and Schaffer's objection that "claims that Hobbes actually *approved of* experiment and accorded it a central place in ....philosophy" are inaccurate (*L&AP*, 126). Acknowledging that Hobbes too participated in experiments,[46] they cite as evidence of his opposition to experiment a particularly heated passage in which Hobbes appears to reckon experimenters working without benefit of philosophy, as mere apothecaries, gardeners, and the like (*L&AP*, 128-129).

Yet Hobbes's insistence that experiment in the absence of "philosophy" does not count as "philosophy" may be more a tautology, than the scalding rejection of experimentalism that Shapin and Schaffer make it sound. Hobbes's consistent materialist philosophy plainly contrasts with Boyle's belief in a non-material world, in which a God and witches and other spiritual entities all had their places, safely protected by fiat from materialist science. Hobbes's materialism regarding the air-pump may indeed have caused him to make some wild speculations to explain Boyle's experimental results; yet recent science suggests that there may be some redemption after all in Hobbes's stubborn materialism. Though moderns may have laughed for years at the seemingly primitive notions of "ether" and "plenum," the last laugh may be Hobbes's. There are physicists today who now suggest that "modern quantum field theory is a direct descendant of the ether," and that "even the vacuum of space, utterly devoid of matter or conventional forms of energy, actually seethes with activity," resulting in the fleeting existence of virtual particles which may become "real". [47] While it might seem foolish to suggest even an

[46]As Noel Malcolm has reminded us, Hobbes was "far from refusing to dirty his hands in practical science. When Descartes in his *Dioptrics* compared refracted light to a bullet fired at an angle into a solid surface, it was Hobbes who, as his Latin Optical Manuscript shows, performed experiments with an airgun to test the theory. In Paris in the 1640's he took a course in chemistry in the Jardin des Plantes; he also studied Vesalius and performed dissections with William Petty. It also seems likely that he had dissected deer with William Harvey. He read widely in contemporary physics and astronomy; and in 1648 he bought the Marquess of Newcastle's collection of telescopes and microscopes." Malcolm, 1988, 47-48.

[47] Malcolm W. Browne, reporting in the *New York Times* (D4, February 2, 1999), "Ether re-emerges as the *je ne sais quoi* of physics." Browne bases his story in part on an article published simultaneously by physicist Dr. Frank Wilczek in *Physics Today*.

indirect relationship between Hobbes's plenum and quantum "ether," I would argue that in this case at least, Hobbes's deeply consistent materialism proves to be the tortoise to Boyle's hare, and the race goes to the stubborn, not the swift.

BOYLE THE EXPERIMENTALIST? At the same time, Boyle's ideas about witchcraft demonstrate his tolerance for unscientific and non-materialist explanations; while his practice of alchemy reflects his interest in a mode of research that was at odds in many respects with the public experimental mode of the Royal Society. To insist that Boyle's method was *universally* experimental is therefore surely incorrect. It seems instead that Boyle felt a need to assign only certain activities and subjects of inquiry to experiment, leaving room for non-experimental approaches to other kinds of knowledge about the world.[48]

At any rate current scholarship accepts a complex understanding of Boyle and his research endeavors that undercuts Shapin and Schaffer's depiction of Boyle as "a poised and purposeful individual, best understood in terms of self-fashioning" (Feingold, 1996). According to Feingold, Michael Hunter "perceives him as a far more complicated human being, troubled by conscience and afflicted by doubts," and both Hunter and others (John Harwood and Lawrence Principe),

> ...demonstrate how Boyle's deep-seated ambivalence and complex personality manifested themselves in a convoluted literary style, endless apologies, and overpowering anxiety about criticism. ....Equally important is the necessity of viewing Boyle first and foremost as a *religious* natural philosopher. ...Linked to Boyle's profound religiosity was his pursuit of alchemy....We learn that Boyle was not as revolutionary as once thought and that, in fact, he shared as much with the medieval tradition as with the iatrochemists of the seventeenth century.....In his own way, and in certain areas, Boyle was not only secretive but also committed to a closed community of a few initiated adepts (Feingold, 1996,135).

Similarly, Sargent has argued that early advocates of experimentation like Bacon and Boyle, did not really live up to the empirical ideal, in which all knowledge is grounded in sense perception and actual and observable phenomena (Sargent, 1989, 34-39). She maintains that both Bacon

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[48]Again, one might make the argument that Boyle's alchemical pursuits might be "vindicated" at least as much as Hobbes's ideas about the ether, in the sense that in modern chemistry the transmutation of (some) elements is possible. The point of both "stretches" (Renaissance "ether" and transmutation of elements, to modern physics and chemistry), is only that there may be a kind of wisdom in certain scientific beliefs or commitments that take many centuries to understand or vindicate in a modern scientific way.

and Boyle made theoretical leaps, and she cites Boyle's corpuscularianism as evidence of this willingness to go beyond the observable. It seems then that the strict antagonism presented by Shapin and Schaffer between Hobbes and Boyle on the value of experiment and theory, might more accurately give way to a complex differentiation along many dimensions, both personal and philosophical.

HOBBES THE MATHEMATICIAN? Though Shapin and Schaffer do not dwell on Hobbes's mathematical endeavors at any length, their work reflects general scholarly knowledge of Hobbes's mathematical obsessions, and inadequacies, and especially Hobbes's long-lived and rancorous disputes with Royal Society member John Wallis (*L&AP*, 135-136). A reputation for stubbornly mistaken mathematics haunts Hobbes into the late twentieth-century, as can be seen for example in the work of Douglas Jesseph.[49] It is said that Hobbes persisted in mathematical errors long after his mistakes had been pointed out to him. This is a matter which some have attributed to Hobbes's vanity, and others to the dangers of systematic philosophy: that is, of a foolish consistency, driven by convictions that were too deeply embedded in Hobbes's entire philosophy for him to call into question without having the whole edifice crumble.

Some fairly sympathetic accounts of Hobbes's mathematics find this judgment to be fair, suggesting that it is ironic -- and nearly tragic -- that Hobbes the arch-rationalist could (in Jesseph's words) be "so resolutely wrong." The appearance of non-rational factors in Hobbes's arguments seems to lend some fuel to Shapin and Schaffer's argument that the "real" source of Hobbes's resolution was not mathematical, but reflected deeper social and political commitments and controversies, for which mathematics was simply an expressive vehicle (Jesseph, 1999b, 362). Indeed, the very scant attention paid by Shapin and Schaffer to Hobbes's mathematics itself suggests that his mathematics were subordinate to Hobbes's politics.

But Jesseph argues that the case for a purely sociological reading of Hobbes's mathematical controversies would be "seriously wrong." He argues against this on several grounds; first, he objects to the notion that Hobbes, who occupied a notably solitary intellectual space on the

[49] See Jesseph, "Hobbes and Mathematical Method," *Perspectives on Science*, 1993, v. 1 n. 2, 306-341; also Jesseph, forthcoming (1999a and 1999b). I am grateful to Prof. Jesseph for his generosity in sharing these latter two papers with me prior to publication; and to Prof. Richard Burian for his efforts to obtain them on my behalf.

landscape of his time, could be said to represent a “form of life” in the Wittgensteinian sense; and second, he points out that Hobbes’s mathematical opponents were thoroughly diverse in terms of religion, class, political affiliation, or any other social categories (Jesseph, 1999b, 371). Jesseph’s belief is that Hobbes’s mathematics failed for thoroughly internal reasons, "having to do with a fundamental incoherence in his philosophy of mathematics....brought on by his attachment to the methodological principle that all properly-formulated demonstrations must proceed from causes" (Jesseph, 1999a, 1).

But as also becomes clear in Jesseph’s account (and is not plain in Shapin and Schaffer's dismissive note on the matter), what was at stake in mathematical terms in the Hobbes-Wallis dispute concerned the relative merits of algebra and geometry. Hobbes’s arguments may thus usefully be understood in a different and broader context of the history of mathematics, not as wayward and isolated crankishness, but as a part -- if somewhat flawed representative -- of this important debate. For a perspective on that debate that is lacking from Shapin and Schaffer's work, Helena Pycior’s 1997 monograph on the development and reception of algebra in early modern England and Scotland is important.[50] Pycior explores in detail the important running struggle, and competition, between geometry and algebra; and the parallel struggle between a mathematics based in geometry and language, versus a symbolic and quantitative mathematics, that permitted negative and imaginary numbers, grounded in algebra.

In the middle- and late-sixteenth century in Europe, several important developments for the discovery and transformation of algebra took place. One of these was the publication, in 1545, of Girolamo Cardano’s *Great Art*, and François Viète’s *Introduction to the Analytic Art* in 1591. These two works broke new ground both in the area of imaginary and negative numbers, and in the use of arbitrary symbols and analytic method in algebra. Viète’s work had an important impact on English mathematicians William Oughtred and Thomas Harriot, each of whom published textbooks in 1631 using the new symbols. In England, Pycior points out, “a deep appreciation of the symbolical style came first; openness to the expanding universe of algebra [imaginary and negative numbers] followed slowly” (Pycior, 1997, 40). Oughtred played a major role not only in teaching the new symbolic style of algebra, but also in promoting mathematics as an academic discipline. Inspired in part by the efforts of John Collins, two major

[50] Helena M. Pycior, 1997, *Symbols, Impossible Numbers, and Geometric Entanglements: British Algebra through the Commentaries on Newton's Universal Arithmetick,* New York: Cambridge University Press. I thank D. Gold for recommending this reference.

English algebra texts were published near the turn of the century: Wallis's *Treatise of Algebra* in 1685, and Newton's *Universal Arithmetick* in 1707. Wallis's major contribution was to the development of algebra based not on geometry but arithmetic, and he contributed also to the acceptance of imaginary numbers by English mathematics.

In contrast, Pycior writes, "Hobbes's mathematical views were shaped by seventeenth-century respect for experience as a source of knowledge and his nominalist inclinations. Ironically, despite his animadversion toward early modern algebra, his nominalist approach potentially nudged algebra in the symbolical direction" (Pycior, 1997, 135). This tension, she writes, was never satisfactorily resolved by Hobbes, who "toyed with a new approach to arithmetic as a theory of names" (in this agreeing with both Wallis and Descartes on some points), while rejecting abstract general ideas as inconsistent with materialism (Pycior, 1997, 141-3). In one of Hobbes's most frequently quoted diatribes, he said of symbolic mathematics that one could find in it: "no knowledge, neither of quantity, nor of measure, nor of proportion, nor of time, nor of motion, nor of any thing, but only of certain characters, as if a hen had been scraping there."[51]

Pycior concludes her discussion of Hobbes with the observation that his rejection of early modern algebra—though on the face of it reactionary and conservative— actually reflected the "empiricist inclinations" of the English early modern scientific style. These same inclinations, she writes, were also demonstrated in the geometric optics of Barrow, Newton, and others. The older geometric tradition represented by Hobbes continued to strongly influence the teaching of English mathematics for several centuries. Some scholars have gone so far as to argue that even Hobbes's most notorious claim, that he had squared the circle, can be seen in this context not as "a splenetic eccentricity," but on its own terms as a legitimate mathematical procedure. In the context of Hobbes's famous dispute with Wallis, Hobbes's circle squaring actually "reveals the fundamental differences on issues which underpin their methods and philosophies" (Bird, 1996, 230-321). To summarize: the tremendous philosophical significance for Hobbes of geometrical mathematics, and the context in which Hobbes developed his ideas, is not evident in *L&AP*, except as these reflect on Hobbes's personal interactions with the Royal Society. This in turn reflects that an account of the origins of experimental science sits uncomfortably with a more continuous and pervasive account of developments in early modern mathematics.

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[51] Pycior, 1997, 147. She is citing Hobbes, *Six Lessons* in *English Works*, 7:188, 330.

BOYLE THE MATHEMATICIAN? Boyle is not known for his contributions—or even attempted contributions! -- to mathematics. But some comparison of Boyle's and Hobbes's ideas on mathematics will surely be as helpful as contrasting their ideas on experiment. Where Hobbes represented a geometrical tradition of mathematics, with deep resonances with materialism and nominalism, Boyle -- at least to some degree -- represented the new algebra, and its symbolic "plain" style. Considering various members of the Royal Society, Pycior points out that not only Boyle but Sprat and Hooke associated the ideal scientific rhetoric with "Mathematicall plainness" (Sprat); Hooke wrote that the model scientific report would share the brevity and disambiguity of "Geometrical Algebra, the expression of many and very perplex Quantities by a few obvious and plain Symbols".[52] Boyle himself was closely associated with a wider group of individuals (Wallis, Ward, Christopher Wren, Sprat, and students of Oughtred's) who strongly advocated "algebra, cryptography, terse scientific prose, ...or some combination" (Pycior, 1997, 114).

This is not to say that Boyle and the Royal Society were comfortable with the idea of mathematics as the cornerstone of their scientific practice. As Peter Dear has pointed out, the work of John Wallis and Christopher Wren that was published in the early volumes of the Society's *Philosophical Transactions*, was the exception and "fitted only uneasily" into the Society's work. Dear concludes that "the Royal Society could not comfortably accommodate together the disparate traditions and criteria of experimental and mathematical researches."[53] But the foregoing discussion suggests once again that to insist on this dichotomy may obscure important aspects of the intellectual currents and positions on both "sides" of the ostensible boundary.

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[52]Pycior, 1997, 46-47. Her citations are from Thomas Sprat, *The History of the Royal-Society of London, for the Improving of Natural Knowledge* (1667, 1958 reprint, edited by J. I. Cope and H. W. Jones, St. Louis: Washington University Press, 113); and Robert Hooke, "A General Scheme, or Idea of the Present State of Natural Philosophy, and How its Defects May be Remedied," 1705, from *The Posthumous Works of Robert Hooke* (reprint edited by Richard S. Westfall, 1969, New York: Johnson Reprint, 63-64).

[53] Peter Dear, 1997, "The Early Royal Society," 270-271. Dear cites Thomas Kuhn as possibly corroborating this "discomfort" in his 1976 essay, "Mathematical versus Experimental Traditions in the History of the Physical Sciences," reprinted in Kuhn, 1977, *The Essential Tension* (Chicago: Chicago University Press).

Posing the history of the Hobbes-Boyle dispute as one of mathematics versus experiment runs the danger of losing insight into the encounter between algebra and empiricism in England. The mathematics vs. experiment formulation also suggests the contrast was between "winning" and "losing" approaches to science. Yet as we have also seen, such a contrast is somewhat shaky: the "loser" (Hobbes) was by many accounts not a particularly strong representative of mathematics; and Boyle's experimentalism was only one facet of a multi-faceted knowledge-seeking strategy that also included alchemy and corpuscularianism, and was in some senses dominated by theology. To address scientific history in terms of "winners" and "losers," "right" and "wrong," may be part of the problem. As Allen Debus has said, the challenge to scholars is neither to hide nor expose but rather to understand the "heterodoxy" of their subjects: "[T]o uncover the internal reason and justification for their presence in the mind of the savant and their organic coherence with his scientific ideas" rather than "side-stepping" or excusing on the basis of the backwardness of the past.[54]

But the argument that Hobbes and Boyle were radically opposed in their views on the role of experiment and mathematics in natural philosophy, is crucial to *L&AP*. As Latour has pointed out, looked at closely, Hobbes and Boyle,

> ....agree on almost everything. They want a King, they want a disciplined Parliament, they want a disciplined unified Church, and they are all for a 'mechanistic' philosophy. Although they are both firmly attached to the rationalist tradition, they nevertheless differ in a few crucial ways on what to expect from experiment, from scientific reasoning and from the air-pump. Hobbes' and Boyle's disagreements in the middle of Revolutionary England are turned into the "fruit flies" of the new social theory of science the authors [Shapin and Schaffer] develop (Latour, 1990, 148).

But were Hobbes and Boyle such "fruit flies" ? It could be that Shapin and Schaffer have misidentified the root controversy between Hobbes and Boyle. A controversy of at least equal, and possibly greater, significance than the one between experiment and mathematics for the emergence and development of modern science may have been between the ancient and modern traditions of mathematics: the former grounded in geometry; the latter in symbolic algebra. Scholarship on *this* controversy has suggested, intriguingly, that the emergence of "scientific community" crystallized not around experiment, witnessing, and trust, but rather around the capability of abstract number to permit a new impersonal kind of community in the absence of

[54] Allen Debus (1972). *Science, Medicine and Society in the Renaissance,* New York, v. 1.

other social bases for the trustworthiness of knowledge claims bases (such as proximity, or shared class).[55] In this view, it is quantification and number, and not collective witnessing, that is the source of the “objectivity” that distinguishes modern science. This alternative framing of the significance of Boyle's "material technology" and Hobbes's objections to it, is a serious challenge to Shapin and Schaffer’s interpretative program.

With these first three theses, Shapin and Schaffer developed their argument that Boyle and the Royal Society invented three mutually embedded "technologies" (social, literary, and experimental), which together provided the basis for claims by the new scientists to "matters of fact" as the brickwork of scientific authority. In the preceding three sections I have suggested that the relationship between each of these technologies and the emergence of early modern science is more complicated and ambiguous than Shapin and Schaffer have represented. The social technology represented by the Royal Society had important and competing precedents and parallels across other professions and in other nations. The peculiar social form of the Royal Society was an amalgam of pre-existing social forms; and it was only one of many closely related forms of social organization that constituted the networks of scientific communication and practice in early modern Europe. Indeed the "literary technology" most closely associated with the emergence of the new science was not characteristic of the early Royal Society, nor was it associated with the virtual reproduction of experimental witnessing. Rather, it reflected a move away from literary to symbolic accounts of experimental events and findings: a very different dynamic with a more subtle and complex meaning for the role of community in modern science. Similarly, the third technology of the Royal Society (the experimental apparatus or material technology), did not itself demarcate the line of opposition between Boyle and Hobbes. Instead, their conflict illustrated a more intricate dynamic at work between geometrical and algebraic forms of mathematics, and materialist and Christian philosophies; the long-standing and long-running nature of those developments and controversies argue strongly against the Royal Society and Boyle as the point of origin for early modern science.

3.4 Trust and sociality

[55] See especially Theodore Porter, 1995, *Trust in Numbers*, and Timothy Reiss, 1997. Mordechai Feingold also identifies a strong continuity in the development of mathematics both in terms of the conventional "periodization" and in terms of the location of mathematical work within and outside the universities (1984, *The Mathematician's Apprenticeship*).

Thesis 4: The Royal Society's strategy for achieving epistemic consensus was not only literary, and experimental, but also a matter of etiquette, trust, and sociality.

This thesis and the next (3.5) are both keys to Shapin and Schaffer's argument that the origins of experimental science were local, and contingent on the particular social and political context in which Hobbes and Boyle found themselves. Very briefly, that argument suggests that the strategies devised by the Royal Society to establish common epistemological ground, were stimulated by the critical problem of achieving social peace in Restoration England.

Thesis 4 is really composed of two dependent assertions, and it will be helpful to address them separately. The first is that consensus was a deliberate and conscious goal of the Royal Society; the second is that trust, civility, and etiquette—purely social strategies for managing disputes—were critical to the Royal Society in achieving this consensus among themselves. The former argument rests in part on the belief that in turbulent political and religious times, protracted controversy of all kinds had revealed such ugly and violent consequences that finding peaceable ways to disagree—or grounds, however narrow, for agreement—had become a matter of self- and social preservation. The latter argument is less historically conditioned. But as presented in *L&AP* , the relationship between these two claims comes close to a chicken-or-the-egg question: was the Royal Society interested in consensus because of its social standards, or did it develop, borrow, or promulgate these social standards in order to achieve badly wanted epistemic consensus?

How significant, for the early Royal Society, was the "problem" of dissent and of forging consensus? Shapin and Schaffer follow other historians[56] in claiming it was "a matter of serious practical concern." Their account follows this basic story: the source of the concern lay with the perceived threat of "enthusiasts," sects, and others who by asserting the primacy of individual or "private" judgment, undermined the delicately balanced authority of the church and state (*L&AP*,

[56] They do not necessarily agree in all respects of course, but among those who have also drawn attention to the special significance for early modern science of the raging seventeenth-century English religious and political controversies, particularly in the Restoration, are James R. Jacob and Margaret C. Jacob, 1980, "The Anglican Origins of Modern Science," *Isis* 70, 251-267; Christopher Hill, 1972, *The World Turned Upside Down*; P. M. Rattansi, 1972, "The Social Interpretation of Science in the Seventeenth Century," in P. Mathias, ed., *Science and Society, 1600-1900*, Cambridge; Shapin, 1980, "Social Uses of Science, 1660-1800," in R. S. Porter and G.S. Rousseau, eds., *The Ferment of Knowledge*; and Michael Heyd, 1981, "The reaction to enthusiasm in the seventeenth century," *Journal of Modern History* 53, 258-280.

72-3). Scholastic science had a reputation for “litigiousness”—arbitrary and stubborn protraction of controversy. The Royal Society wanted to distinguish itself from the old science; it could not appear divided within itself and at the same time carve out the politically protected status it sought. Dispassionate and quiet conversation, having no part of the “madness of that dismal Age,” was the chief aim of those who originally drew together to create the Society (*L&AP*, 76). In particular, their conversation was to exclude religion or politics.[57] But just as important, the Society was to attempt through a variety of strategies to replace the authority of individual experience, insight, and belief, with cooperative and collective authority.

According to Shapin and Schaffer's interpretation, achieving consensus was a crucial function of the Royal Society; and it was to be achieved by constraining the scope of the Society's concerns to "matters of fact" which could be collectively witnessed via the performance of experiments. Other scholars have disputed this point:

> Reading *Leviathan and the Air-Pump*, one might assume that there existed in the Royal Society an unchallenged consensus about the respective roles of hypothesis and experiment in philosophical enquiries....This one-dimensional picture is, however, highly misleading, for there were disagreements among the early Fellows over the proper uses of hypotheses, the legitimacy of the natural history programme, and the epistemological standing of matters-of-fact, which were quite as serious as those between Boyle and Hobbes.....There was thus much less agreement about the protocols of the ‘experimental life’ than Shapin and Schaffer imply.... (Wood, 1988, 107).

Shapin and Schaffer may have created some confusion by identifying Boyle's social style and preferences so closely with those of the entire Royal Society. Boyle may indeed have represented a "fundamental contrast" with Hobbes on this matter: for Boyle, “knowledge was constituted when all believed alike,” whereas for Hobbes, “belief and opinion were items pertaining to individuals, and, as such, could not be manipulated into bases for public order” (*L&AP*, 105). But if there was not actually shared belief within the Royal Society, *even on the knowledge-constituting role of experiment*, this deeply undermines the claim Shapin and Schaffer make, that the Royal Society in practice considered epistemic "peace" their highest goal.

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[57] In the often-quoted words of the statutes of the Society as drafted by Hooke (1663), "The Business and Design of the Royal Society is: To improve the knowledge of natural things, and all useful Arts, Manufactures, Mechanics, Practices, Engynes and Inventions by Experiments (not meddling with Divinity, Metaphysics, Moralls, Politicks, Grammar, Rhetoric, or Logick)." Cited by M. Ornstein, 1938, *The Role of Scientific Societies in the Seventeenth Century*, third edition, London, Archon Books, 1963 reprint, at 108.

What then of Shapin and Schaffer's related argument, that the Royal Society attempted to reduce scientific controversy through a variety of careful social strategies? Shapin and Schaffer point to Boyle's rules for the management of scientific dispute, as published in his *Proëmical Essay*: there must be no *ad hominem* attacks; sectarian views should be ignored and therefore discouraged; and individuals must be willing to publicly renounce positions later shown to be erroneous (*L&AP*, 73-4). Boyle's ostentatiously "civil" behavior and his personal concern for the social conduct of scientific controversies are a matter of record.[58] The Royal Society's founders had good reasons for setting up expectations and social constraints that would assure that controversy and disagreement were (by mutual agreement) carefully expressed and deliberately, if informally, managed.

But does Hobbes really stand against the Royal Society here? He agrees on most of these counts (*L&AP*, 106), if not on the advisability of turning the other cheek. It has often been suggested that Hobbes's exclusion from the Royal Society (which I will examine further in section 3.7, below) was due mostly to his refusal to abide by these standards of civility.[59] True, as an intellectual enemy, Hobbes was contentious, acid-tongued, and stubborn; and he responded to *ad hominem* attacks both vehemently and in kind; and out of stubbornness or vanity or both, he steadfastly refused to admit when he was wrong. Given such qualities, it is easy to forget that Hobbes was also broadly regarded with the deepest affection and loyalty, and that his vehemence and delight in such sparring contests were not so very unusual for his time.

If the negative characterization of Hobbes above is correct, it is clear however that Hobbes and Boyle had little in common in terms of their *private* standards for philosophical and scientific conduct. But once again, it is not so clear that these standards constituted an implied behavioral boundary around the Royal Society; or that these personal differences were much more than artifactual aspects of the deeper intellectual controversy between Hobbes and Boyle.

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[58] Though it should also be said that incivility was not rare within the Royal Society, to wit Newton's incivility to Hooke.

[59] Quentin Skinner and Michael Hunter have both contended (Skinner more strenuously) that Hobbes was excluded from the Royal Society for reasons of personality and personal antagonisms between Hobbes and individual Society members. See Skinner, 1969; and Hunter (1982, 1994).

But setting aside considerations of Hobbes and Boyle, the question remains: was the notion of civility in scientific discourse critical to the epistemic authority of the new science? Was it necessary to have a peaceable scientific community, with strong social standards for interaction, in order to establish and maintain the new science? In his 1994 monograph on "trust" and "civility" in the period, Shapin asserts emphatically that the notion of "truth"—including epistemic criteria for scientific truth—was tightly bound to the gentlemanly notion of "trust"—the standards by which a source was deemed "credible" or reliable. This notion of "credibility," Shapin argues, was essential in order for the social experience of experimental witnessing to scale up and out, that is, beyond the geographic confines of the laboratory and the temporal confines of a single event. In his essay review on Shapin's book, Peter Dear summarizes Shapin's view, that "any piece of scientific knowledge, as with knowledge in general, is saturated with trust-relationships....[A] scientist accepts on trust nearly all the background assumptions required to test a new knowledge-claim.... Believing what others maintain, and trusting their probity, is what hold the enterprise together—*sine qua non*" (Dear, 1995, 452).

But Dear goes on to question whether the Royal Society's civility in handling disputes over matters of fact, reflected a deep, socially structured process of achieving epistemic resolution; or whether it was simply a social gloss used to preserve reputations and ensure ongoing friendly relations among respected peers. Mordechai Feingold echoes this objection, suggesting that there is nothing substantive or constitutive about similarities between the practices of the "new science," and the social mores, rituals, and practices of the genteel classes. Feingold objects that Shapin has overstated the supposed demarcation in gentility between the new scientists and the academy, claiming that "[Shapin] downplays the nearly *universal* attribution of gentle status to scholars and authors" (Feingold, 1996, 133 , emphasis in original). And Feingold also argues that Shapin has misrepresented the degree to which the Royal Society expected or enforced "polite" discourse on scientific subjects. Conversation and interaction surely took place on multiple levels, Feingold argues, some of which *were* purely social; and accounts of the Society's deliberations, including records of debates and dissent, were surely "edited" for public consumption (Feingold, 1996, 134).

Another important social formation in the seventeenth century and after was the loosely structured international literary and intellectual network known as the "Republic of Letters." Historians of the "Republic" have suggested that there was a set of social practices which were

(and still are!) useful in constituting and maintaining a flow of intellectual and literary discourse among participants; and that the importance of these practices lies neither in the *content* of that discourse, nor as a means of securing consensus. Instead, this set of practices ensures individuals' continued access to the resources they require to participate in, and pursue, their chosen work. Intellectual and cultural exchange, access, inspiration, correction -- and not consensus -- were surely among the benefits hoped for by participants (like Hobbes) in informal scholarly circles like those that centered on Marin Mersenne.

Lorraine Daston and Dena Goodman have published important work on this topic, and a recent book by Anne Goldgar offers a detailed study of the "Republic of Letters" centering on its Huguenot members, that contrasts in striking ways with the "Republic of Science" as presented by Shapin and Schaffer.[60]

Goldgar offers an international perspective on the genesis of the Royal Society: the formation of academies and learned societies of all kinds began in northern Europe around the middle of the seventeenth century, and these were often accompanied by the emergence of literary journals. Even more than the academies, these journals signaled the emergence of a largely informal but highly influential social organization. It existed neither by constitution nor royal edict, but rather in the minds of its members, "its regulations and even its membership .... nebulous at best" (Goldgar, 1994, 2). It operated according to largely unexpressed and unwritten rules of (social) conduct. These rules underscored the feeling of the members of the Republic of Letters that they belonged to an association that in important ways was separate from the rest of society:

> Seventeenth- and eighteenth-century scholars felt that, at least in the academic realm, they were not subject to the norms and values of the wider society. Unlike their non-scholarly counterparts, they thought they lived in an essentially egalitarian community, in which all members had equal rights to criticize the work and conduct of others. In practice this egalitarian ideal was not so solid, but the hierarchical structure the community did take on was based on different standards from society at large. ....The learned world was one world, international and nondenominational, rising above the petty concerns of church and state. Or so, at least, the scholars claimed (Goldgar, 1994, 3).

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[60] Anne Goldgar, 1995, *Impolite Learning: Conduct and Community in the Republic of Letters, 1680-1750*. Yale University Press. See also Daston, Lorraine, 1991, *Science in Context*, "The Ideal and Reality of the Republic of Letters in the Enlightenment," 4: 2, 1991, 367-86; and Dena Goodman, 1994, *The Republic of Letters: A Cultural History of the French Enlightenment*, Ithaca, Cornell University Press.

Goldgar emphasizes the "self-selection" of members of the Republic of Letters, and the critical role played by personal interactions both in creating and sustaining the Republic. Located everywhere and nowhere, it was located,

> ...in the relations among its members and among other scholars, the quarrels, gossip, dinners, lending of books and sharing of information. Most important, the Republic was located in the values and attitudes which shaped these interactions and formed *the concept of community* in the minds of its members....the most important common concern of members of the Republic of Letters was their own conduct. In the conception of its own members, ideology, religion, political philosophy, scientific strategy, or any other intellectual or philosophical framework were not as important as their own identity as a community. ...Communal bonds were the focus of community; the Republic of Letters was a reflexive event (Goldgar, 1994, 6).

At one point Goldgar's thesis resonates with Warren Hagstrom's classic account of scientific community:[61] she, like Hagstrom, claims that science is sustained by a gift-based cultural exchange network—a strongly cooperative mode of conduct with "considerable practical value" for its participants, whose broader purpose is to "establish and strengthen social relations" (Goldgar, 1994, 20-22). Yet Goldgar also notes that the role played in the Republic of Letters by scholarly journals forced a sometimes uncomfortable encounter between the social rules of the Republic and the personal goals of its members:

> Could a journal be both *honnete* and *utile*? were these compatible virtues? .... This conflict lay behind much of the uneasiness about the role of journals in the community.... [T]he operation of journals, their reading, and the interaction of their audience and their authors, suggest a tangled web of ideas about how such an institution ought to behave. Individual goals conflicted with group aspirations, and both clashed internally with themselves. At the most basic level, the difficulty lay in separating the professional from the personal..... Even its institutionalization was accomplished in terms of the individual, as the journal became supreme arbiter in the community on the basis of its 'personal' attributes (Goldgar, 1994, 110-114).

Coming back to Shapin and Schaffer from this perspective, it is difficult not to suspect that, to the degree politeness and the virtues of polite society were advocated by the Royal Society, this band of English gentlemen was reflecting, rather than originating, the establishment in the seventeenth-century of far-reaching international *informal* relations among individuals with common intellectual and cultural interests. The currency needed for participating in those relations was not so much *truth* as access to communications. The tensions noted in the Republic of Letters, between managing controversy and expressing disagreement, suggest that such tensions were not, and could not be, "settled" within the Royal Society. Rather, such tensions were distinguishing

[61] Warren Hagstrom, 1965, *The Scientific Community* (New York: Basic Books).

features of the innate conflicts between individual needs and desires, and the importance of maintaining access to other individuals in a more or less organized and continual way.

This fact of informal intellectual life, and not the requirement of the new science for establishing epistemic consensus, helped define the virtuous scholar (or scientist) as demonstrating:

> ....openness with information, the willingness to lend books, assistance in other people's research, instructive conversation, anxiousness to be of service, gentleness with opponents, and even assisting indigent men of letters. .... If they failed or refused to recognize these fundamentals, great scholars were therefore not so great after all..... [I]n large part it was the way a scholar behaved in the community, his acknowledgement of its nature *as* a community, which was an important means of judging his worthiness in the hierarchy (Goldgar, 1994, 154).

To summarize the implications for Shapin and Schaffer's work, Goldgar and other scholars working on the Republic of Letters have raised important issues concerning relationships between formal and informal networks of scholarly association in the seventeenth century. A missing piece in Shapin and Schaffer's account is a discussion or contrast of Hobbes's full participation in a distinguished informal international network of scientists and philosophers, and indeed a fuller account of how the Royal Society's practices fit into those of the rich network of preexisting international scholarly and scientific interactions. Goldgar's work suggests strongly that the social technologies adopted by the Royal Society were widely understood and valued by scholars, not because they generated consensus, but because the "peace" they kept permitted the expression of dissension and difference, without loss of access to the benefits of continued association.

3.5 England the Crucible

Thesis 5: The particular circumstances of politics in Restoration England required a solution to the problem of achieving epistemic consensus and managing epistemic controversy.

As noted above, formal and informal associations that promoted the exchange of information among the intellectuals, scholars, and scientists were emerging throughout the late seventeenth century in northern and western Europe, in particular Italy and France as well as England. Further, even among scientific societies, official academies and societies developed in parallel with private and informal societies, in one scholar's terms, "essentially no different from their

more established brothers and sisters, except for the lack of official status and legal recognition" (McClellan, 1985, 2).

Further, there is no recognition in Shapin and Schaffer of the broader genre of learned societies of which the Royal Society was perhaps exemplary, but by no scholarly account, generative. A study of early scientific societies published in the same year as *L&AP* cites considerable prior research in support of this understanding:

> It is to be emphasized that the scientific societies themselves constitute only one subset of eighteenth-century learned societies in general. Indeed, the learned society as a type of institution was *the* characteristic form for the organization of culture throughout Europe and the West in the eighteenth century. Social and cultural enterprises of all sorts were typically incorporated by ancien régime states and social orders into learned societies, several scores of which dealt with language and philology, literature and belles lettres, painting and the fine arts, history and archaeology, antiquities, architecture, philosophy, medicine, agriculture, economics, the mechanical arts, as well as science .... The learned society movement in science in the eighteenth century was clearly international in character. That character is of special importance, given the ideological notion peculiar to the eighteenth century of an international Republic of Letters uniting the forces of Enlightenment (McClellan, 1985, 3, 5).

The social mechanisms these associations employed to manage a balance between civility and debate included standards of genteel conduct (honesty, trust, modesty, courtesy, exchange of service); but Goldgar's study has argued persuasively that these social formations did not do away with dissent and controversy, but rather established a social space in which both exchange and controversy could coexist. What was critical in the further development and management of this space, was the ability of the participants to maintain a prolonged exchange of views that went far beyond what could take place within a local academy or society. Early literary and scientific journals played an important role in supporting such associations, embodying an ideal of neutrality that made exchange "safe," even while they conveyed the potentially divisive substance of exchange and debate.

The usefulness of having a neutral space for intellectual exchange was not lost on the Royal Society, and Shapin and Schaffer have made much of the fact that Boyle and his colleagues claimed to have achieved such a space by leaving their "Politicks" and "Metaphysik" at the door. Was this kind of neutrality really possible? Shapin and Schaffer claim that it was not: that the Society was able to achieve only a *simulacrum* of neutrality by following a rule of consensus that effectively "neutralized" the individualism that each member brought to the group. This new kind

of authority based on consensus and collectivity would transcend the subjectivity of individuals by producing group “objectivity”.

So Shapin and Schaffer propose. But the reduction of the "objectivity" of science to a matter of consensus is of course precisely the reduction that Shapin and Schaffer’s critics have resisted so vehemently. As Daniel Garber has written, Shapin and Schaffer have plainly suggested that "the establishment of experimental facts can be explained entirely in sociological terms....[as] simply a matter of social negotiation among members of the relevant community." In the most general sense, he admits, there is some (a priori) truth that attaches to the idea that,

> .... belief in experimental facts, as in everything, must simply be a function of some communal agreement or other, explicit or tacit; belief, one might claim, as with the language in which it is framed, is by its nature social.....the thesis would seem to be grounded in very, very general facts about language, belief, and society (Garber, 1994, 202).

But he adds, this has "obvious deflationary consequence(s)" for our understanding of science. Are there are alternatives to this view of how, and why, science manages consensus, that go beyond naive realism or naive relativism?

A strong alternative has already been suggested above, though it is only faintly present in Shapin and Schaffer’s account. What I have in mind is the gradual emergence throughout the sixteenth, seventeenth, and eighteenth centuries, of numerical objectivity and algebraic mathematics. Theodore Porter has presented a compelling account of this history in his 1995 monograph, *Trust in Numbers*. Porter suggests that the transition from geometrically-centered mathematics to algebra and quantification enabled the emergence of modern science -- but through a mechanism that made a *trust*-based scientific community unnecessary. Quantification permitted a kind of scientific communication to spread, and flourish, that did not *depend* on trust but rather made it possible to communicate in the absence of personal communication, trust, *or* community. Far from depending on the social values of genteel culture in the Restoration, the new science actually held within it the seeds of a kind of socialism, through

> ....the elevation of general rules and social values over subjectivity and the selfish desires of the individual. This exaltation of the objectivity of science is often confused with elitism. As defined here, though, it is anything but elitist (Porter, 1995, 75).

Not only was the new mathematics *not* about trust, but it was also *not* about truth: numerical objectivity,

> ....implies nothing about truth to nature. It has more to do with the exclusion of judgment, the struggle against subjectivity. This impersonality has long been taken to be one of the hallmarks of science..... [T]his, more than anything else, accounts for the authority of scientific pronouncements in contemporary political life..... In science, as in political and administrative affairs, objectivity names a set of strategies for dealing with distance and distrust (Porter, 1995, ix).

Thus Porter offers a strong challenge to Shapin and Schaffer's contention that the early foundations of modern science were literary, instrumental, and consensual. Quantification is another candidate technology on which we might discover the new science rested. But it created no Boylean communities of trust. The technology of quantification instead explains the capability of science to promote shared discourse, even in the absence of agreement, trust, shared culture, or other supportive social technologies:

> ...the relative rigidity of rules for composing papers, analyzing data, even formulating theory, ought to be understood in part as a way of generating a shared discourse, of unifying a weak research community. ... Methodological strictness serves as an alternative to shared beliefs and as a check on the expression of idiosyncratic personal opinions....(Porter, 1995, 229).

Finally, if the technology of quantification is as important as Porter and others[62] have claimed, then it is difficult to sustain the argument that England, and a peculiarly English set of political circumstances, had some unique claim to generating early modern science in Europe. Yet Shapin and Schaffer have done exactly this, claiming that uniquely English political considerations were "constituents of the evaluation of rival natural philosophical program" (*L&AP*, 283). They maintain that there was a fundamental equivalence between the newly established Restoration polity, and newly constituted associations promoting experimental science (*L&AP*, 342). For Shapin and Schaffer, it is this co-establishment that constitutes "the origins of a relationship between our knowledge and our polity that has, in its fundamentals, lasted for three centuries" (*L&AP*, 343).

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[62] Related work includes Timothy Reiss's *Knowledge, Discovery, and Imagination in Early Modern Europe: The Rise of Aesthetic Rationalism* (1997), and Alfred Crosby's *The Measure of Reality: Quantification and Western Society, 1250-1600* (1997), as well as Norton Wise's edited work, *The Values of Precision* (1995, Princeton: Princeton University Press). But see also Thomas Kuhn's essay in *The Essential Tension* (1977), "Mathematical Versus Experimental Traditions in the Development of Physical Science," 31-65, Chicago: Chicago University Press.

This "origins" story falters as it becomes clear that social and intellectual events and developments in a variety of settings and over a long period of time have very strong rival claims on explaining the emergence of early modern science. These developments included the replacement of the trivium by the quadrivium, of geometry by algebra, and the pursuit of an ordered human "form of life" based on an ideal of rationality. Timothy Reiss has called this slower evolution the rise of "aesthetic rationalism." Ironically, this formulation puts Hobbes rather than Boyle at the center of the "new" science, reminding us both of Hobbes's emphasis on rational politics, and his powerful sense of the relationship between rational politics and rational mathematics:

> This 'harmony', 'numbered' or 'mathematical' order, marked not just the system of the *material* world, but one's perception of it and one's *rational attunement* to it. It was what made reasonable analysis and understanding, ethical behavior, *and* political and social stability possible at all. It was a thoroughly optimistic view, and that it may now strike 'us' as largely incomprehensible or false is not because 'we' do not credit its grounding premises (as Taylor observes), but because we are at a loss to know how to square them, not with injustices to 'others' , with which we mostly have little difficulty, but with horrors committed upon ourselves (Reiss, 1997, 197-198).

Hobbes's insistence on individual belief, his thoroughly consistent materialism, and his insistence on the crucial role of mathematics in providing secure knowledge, make us see Hobbes not as a curmudgeonly royalist but as a major voice for views with radically "modern" implications for the meaning and conduct of science.

Having now reviewed some of the problems with Shapin and Schaffer's theses that situate the goals and strategies of the new science in English political and social history, I will now turn to their major theses addressing why Thomas Hobbes might be an important and underestimated figure in early modern science.

3.6 Hobbes's Importance

Thesis 6: Hobbes's most significant and lasting contribution to early modern science was his analysis of the political foundations of human knowledge, and not his scientific or mathematical ideas, which were driven by and secondary to his political commitments.

Based solely on a reading of Shapin and Schaffer, it would be easy to underestimate Hobbes's involvement in the development of early modern science. The Hobbes that emerges from their pages is an argumentative, stubborn, and thoroughly political codger: Shapin and Schaffer dismiss Hobbes's association with some of the most important individuals and developments in the history of early modern science in a few paragraphs (*L&AP*, 82-87). After reading their account of Hobbes, one could easily come away with the impression that Hobbes, though a gifted political philosopher, was primarily a polemicist, a courtier who had outlived his court, and a scientific amateur whose contacts with the likes of Galileo, Mersenne, Gassendi, and Descartes were due to his social connections and not his scientific ideas.

Anyone who has had the opportunity of reading further about Hobbes's long and interesting life might on the other hand, echo the critic who wished that Shapin and Schaffer had offered more Hobbes, and less Boyle.[63] That they did not may, in part, reflect the remarkable fact that, to this day, Hobbes remains a kind of black sheep—someone whose influence others are reluctant to acknowledge, lest they be labelled "Hobbist" and their ideas discounted.

Before turning in section 3.7 to the question of why Hobbes was excluded from the Royal Society, I will first examine Shapin and Schaffer's characterization of Hobbes's significance in the history of science. Were these ideas really were so trivial and amateur in relation to his political ones? Did Hobbes's science (and, some would say, his blindness to what should have been convincing experimental evidence) in fact *derive* from his political convictions?

Of course one of the most important features of Hobbes's philosophy was that his natural and social philosophy were of a piece; this was the "systematic" aspect of his thinking to which Boyle in particular objected (as "immodest"). An interesting suggestion that some have made is that it is this systematic quality of his philosophy that makes Hobbes more acceptable to French and continental European scholars than to those raised in an English philosophical tradition.

But the Hobbes Shapin and Schaffer present to us is a Hobbes whose political philosophy and commitments were primary, and whose political convictions, to a large extent, shaped and fueled

[63] Charles Webster, in his review in the *Times Literary Supplement*, complained that *L&AP* contained "too much Boyle and too little Hobbes." March 13, 1987, 281.

his natural philosophy. But there are several important features of Hobbes's philosophy that suggest that it is incorrect to derive one aspect of his philosophy from the other; they were, truly, of a piece.[64]

By way of background to justify this claim, I will first review some of what we know about Hobbes's early thought in science and philosophy. Schooled at Oxford from 1603 to 1608, Hobbes's first important association with early modern science was probably his service as secretary to Francis Bacon, probably for a brief time in the 1620's, when Hobbes was in his 30's or 40's. He had some contacts with scientific circles in Paris and London in this period. It was during his third journey to France and Italy in the 1630's that Hobbes met and began his correspondence with Galileo, Mersenne, Gassendi and Roberval. In the late 1630's, Hobbes composed *De Corpore*, one of the early mechanistic natural philosophies, though it was not published until 1655. It is the fact that *De Cive* was published before *De Corpore*, appearing in 1642 in Paris as a privately printed book; and that even *Leviathan* was published earlier (in 1651), that makes some scholars question whether Hobbes's natural philosophy drove his social philosophy, or the other way around. The publishing history does not solve the question either way.

Hobbes lived abroad, in Paris, between 1640 and 1651. In 1640-1 Hobbes frequented the circle presided over by Marin Mersenne, and through Mersenne Hobbes proposed his objections to Descartes' *Meditations*, as one of the elite group of intellectuals invited to offer objections for response and for publication by Descartes. Hobbes was deeply interested in optics, adopting at first a corpuscular theory of light, which he later changed to one that treated light as motion

[64] Usually the debate insists on one or the other: for example, it is Sam Black's view that "it is scarcely possible to make sense of Hobbes's moral theory when it is viewed independently from his natural philosophy" (Black, 1997, 175). But it is easy to become mixed up in the chicken-or-egg questions about which ideas -- social or natural -- came first. In resolving this question scholars have sought to determine whether the early publication of the political part of Hobbes's philosophy undercuts the ostensibly natural philosophical foundation of that work. On the other hand D. W. Hanson argues against the "received view of method in the philosophy of Hobbes [that] Hobbes...was inordinately preoccupied with issues of proper method; that his methodological ideas came to affect the content and structure of his philosophy; that his more or less unfortunate infatuation with geometry led him to attempt the impossible project of producing a rigorously deductive general system....." (Hanson, 1990, 587). One of the more satisfying accounts of the interrelations between Hobbes's natural and social philosophy may be William Lynch's. Lynch discounts on the one hand allegations of fully integrated "super-rationality" and simple explanations that label one or the other as primary. Rather, Lynch argues "we should think of Hobbes's political agenda as a kind of heuristic that a bounded rational agent could use to impose order on disparate fields of inquiry" (Lynch, 1991, 319).

transmitted through a medium. He composed three treatises on optics, one of which Mersenne published in his *Tractatus Opticus*. Much of *De Homine*, 1658, was devoted to optics.[65]

Hobbes's associations with Galileo and Mersenne were extremely important in the development of his natural philosophy. Hobbes was profoundly affected by his encounter with Galileo, whose influence can be seen in the primacy Hobbes gave to laws of motion in his philosophy. Mersenne's influence was also crucial for Hobbes. A friar who served as an unofficial clearinghouse for many of the early modern scientific ideas, Mersenne defended Descartes and Galileo against theological criticism and struggled to expose the pseudo-sciences of alchemy and astrology. He continued some of Galileo's work in acoustics and some scholars have suggested that Mersenne's work stimulated some of Galileo's own later discoveries. He proposed the use of the pendulum as a timing device to Huygens, thus inspiring the first pendulum clock. In 1633 Mersenne published *Traité des Mouvements*, and in 1634 he published *Les Méchanique de Galilée* which was a version of Galileo's lectures on mechanics. He translated parts of Galileo's *Dialogo* into French and in 1639 he published a translation of Galileo's *Discorsi*. It is through Mersenne that Galileo's work became known outside Italy.[66]

At this point it becomes clearer why it doesn't make sense to privilege Hobbes's political views over his natural philosophy, or to assume that his now-obscure natural philosophy derived from his notorious political ideas. More helpful in sorting out the relationship between these areas of his philosophy is the recollection that Hobbes shared with Mersenne the conviction that there were precious few bases for true intersubjective knowledge; and that only a being that had created a phenomenon, could ever have full understanding of it. It followed, therefore, that only mathematics and civil society, each of which *were* human constructions, *could* be truly knowable by humans. Hobbes insisted that there is "nothing accessible to us in ...natural objects themselves" which could settle our disputes about them (Black, 1997, 182-4). This deep, consistent skepticism during the early modern period stands in remarkable contrast to the intellectual background of the day: the "luxuriant growth of a vast collection of esoteric

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[65]On Hobbes's contributions to optics, see: A.E. Shapiro, 1973, "Kinematic Optics: A Study of the Wave Theory of Light in the Seventeenth Century," *Archive for History of Exact Sciences*, 11, 134-266; and H. R. Bernstein, 1980, "Conatus, Hobbes, and the Young Leibniz," in *Studies in History and Philosophy of Science*, 11, 25-37. These citations are from Lynch, 1991.

[66] A fairly recent source on Mersenne is Peter Dear's 1988 *Mersenne and the Learning of the Schools*, Ithaca: Cornell University Press.

movements of thought ... . at once religious, mystical, magical and social, in the fields of astrology, alchemy, cabalism, natural, and indeed, demonic magic..." (Hanson, 1990, 594-9).

The task which Hobbes and Mersenne took on was no less than the problem of building knowledge and science on the arid ground of this deep skepticism: a philosophy of "maker's knowledge," holding that we cannot know true causes of things unless we make them ourselves. For Hobbes, mathematical knowledge was necessarily the only "natural" knowledge that could be settled absolutely among men. But, as Hanson has said, only Hobbes made the "remarkable leap" of applying this same principle to social life: Hobbes "not only adopted the principle of maker's knowledge, applying it to mathematics as Mersenne had done, but took the extraordinary step of extending it to civil science, that is, we make civil society, the commonwealth, ourselves" (Hanson, 1990, 624).

It is widely recognized that Hobbes's ideas about the commonwealth and its constructed character were (and are) important, and at the time were also highly original. But the fundamental importance of the principle of maker's knowledge in the *scientific* revolution has been argued by Amos Funkenstein, who said of Hobbes:

> ....no other thinker of the seventeenth century argued as consistently as he did for the constructive character of all human manifestations—language, science, political order. No one stressed more forcefully that all knowledge is knowledge by doing (Funkenstein, 1986, 327).

Hobbes also offered a convincing and consistent, but at the time thoroughly unacceptable alternative to the Cartesian dualism that has infested (or more charitably, influenced) western science for over three hundred years.[67] From the perspective of late twentieth-century neuroscience, Hobbes's materialist and nominalist ideas about the corporeality of human thinking and the crucial role of language in philosophy seem remarkably fresh. For Hobbes, Descartes's "conflation of strong belief and logically demonstrated truth" was unacceptable: it was inconceivable for an act to exist without its subject: the thinking self must be a corporeal self

[67]Samuel Mintz writes, "Hobbes banished spirit from the universe; Descartes separated it from matter, but never denied that spirit was real.....Cartesian dualism remains a view of the world essentially different from the Hobbesian view. It is a difference which helps also to define the Cartesian and Hobbist spheres of influence. The great philosophic problem of seventeenth-century science was to find a way of working out its mechanical principles without abandoning spirit and God. Cartesianism offered just the kind of support the burgeoning scientific movement needed, namely, a rigorously mechanical view of the material world, accompanied always by the certainty that the spirit-world also exists" (Mintz, 1962, 10-11).

(Lynch, 1991, 311). Our ideas, even about God, must be material: "we give the name of 'angel' to this thing which we believe in or suppose to exist. But the idea by means of which I imagine an angel is composed of the ideas of visible things" (Lynch, 1991, 310-311). But to disbelieve in the separate reality of the spiritual world (whether God, angels, or witches) was more radical in that time than we can almost imagine in our present world. The small comfort Hobbes offered, that we may still *believe* in God, even though we cannot know him, or even *conceive* of him, was hardly enough to reconcile Hobbes with his contemporaries.[68]

As unique as his philosophical system is, and remains, it may now be more clear that Hobbes's importance as a philosopher can only be reduced to his eminence as a *political* philosopher if one already accepts the essential independence of the categories of political and natural philosophy (which Hobbes did not). To treat Hobbes on his own terms, Shapin and Schaffer might have dealt more "symmetrically" with Hobbes's natural and political philosophy. What they have done instead is to insist on the *priority* of political over natural philosophy: which is not, of course, the same thing.

3.7 Hobbes's Exclusion

Thesis 7: Hobbes was excluded from the Royal Society because he stood in opposition to their new (experimental, hence consensual and apolitical) model of science.

In many ways Hobbes was a highly anomalous figure in English philosophy: he was famously heterodox in his religious views. To modern readers he seems at once politically radical, liberal, and conservative, a combination that we (Americans, at any rate) find confusing. His personal life was relatively modest and serene, and throughout his long life he remained more or less continually under the financial and political protection of English nobility. Despite a reputation that put him in the company of Descartes, Mersenne, Bacon, and Galileo, this important philosopher was deliberately excluded from membership in the Royal Society. How could this be?

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[68] William Lynch writes that "This [Hobbes's materialism] does not mean we cannot believe in God, but that 'we have no idea or image corresponding to the sacred name of God. And this is why we are forbidden to worship God in the form of an image; for otherwise we might think that we were conceiving of him who is incapable of being conceived" (Lynch, 1991, 310-311).

This is a critical point in Shapin and Schaffer's book: much turns on their explanation, and they are not shy in asserting that "we now have an answer to the question" why Hobbes was excluded from the Royal Society. They argue that it is not necessary to distinguish the "personal" from the "philosophical" reasons for this exclusion, since they are mutually implicated: "The social order implicated in the rationalistic production of knowledge threatened that involved in the Royal Society's experimentalism....Hobbes's anti-experimentalism....gave grounds for his exclusion" (*L&AP*, 139).

Other scholars have contended that Hobbes's exclusion could be explained differently, and fully, by the personal antipathy between Hobbes and important members of the Royal Society to whom he had given offense: especially Robert Boyle and John Wallis. This unfortunate personal history combined with his religious heterodoxy meant that Hobbes did not "fit in" comfortably with the Society's other members' social behavior or expectations. Some recent scholars, among them Quentin Skinner and Michael Hunter, suggested that Hobbes's exclusion was due not to these particular antipathies, but to his being more generally avoided as a "club bore" -- someone tending to express their own ideas repetitiously, at great length, and without listening.[69]

But once the full context and vehemence of anti-Hobbism is recognized, the reasons for Hobbes's exclusion, including the reasons why this exclusion had strongly mixed philosophical, social, and personal dimensions, begin to come more sharply into focus. Samuel Mintz has provided ample evidence that it was Hobbes's reputation as an atheist that was at the bottom of his exclusion. To be an atheist was simultaneously to be philosophically, socially, and personally at odds with -- indeed threatening to -- the prevailing social, philosophical, and personal interests not only of the members of the Royal Society, but also of far more numerous individuals in all ranks of society.

To most of his contemporaries, Mintz has pointed out, Hobbes was "the *pontifex maximus* of infidelity." The longevity and hysterical tenor of anti-Hobbes literature spoke to a huge debate surrounding Hobbes during life and after death, at the heart of which was Hobbes's supposed atheism:

> It was atheism then which was at the heart of the controversy about Hobbes, the source of all the fears and seething indignation which Hobbes's thought inspired, the single charge which is most persistently made, and to which all other differences between Hobbes and his contemporaries can be reduced. Fearing the spread of atheism, and seeing it writ large in the works of a very acute

[69] See P. B. Wood, 1988, "Behemoth v. the Skeptical Chymist," 108.

> thinker, Hobbes's enemies were able to sink their political and sectarian differences, and unite in what they conceived to be a common defence of the Christian faith (Mintz, 1962, 46-47).

Hobbes inspired lavish polemics, even among those who sought to avoid such excesses; Seth Ward admitted his limbs were "numb with horror and indignation" when he contemplated Hobbes's impiety (Mintz, 1962, 55). Hobbes was "the Malmesburian Hydra...the hideous monstrosity....the Pugilist of impious death, the Insipid Venerator of a Material God..." in one sermon, "a pandor to bestiality" and the force behind the debauchery of his generation, in another. These and similar assessments raged on the continent as well as throughout England. Some even said that Hobbes was responsible for the Great Fire and Plague of London. Given the pitch of emotion and fear that surrounded people's ideas about him, the remarkable thing is that Hobbes was not put to death: it was a fate from which he was very probably saved by friends and admirers at court.

Hobbes also inspired and participated in more elevated but equally fierce discussions of his ideas, debating issues of materialism and free-will with Cudworth, More, Glanville, and others. But after reviewing even these debates carefully (as Mintz has done), it becomes more difficult to think of Hobbes's chief point of controversy with the Royal Society as stemming from anything other than his materialism -- which was then (and for centuries afterward) taken to be equivalent to atheism. It is much more difficult to imagine that so subtle a point as the proper role for experiment in natural philosophy had anything like the same effect. As Wood writes (with some understatement, I think),

> Shapin and Schaffer's thesis is correct as far as it goes, but they tend to underestimate the importance of Hobbes's supposed infidelity as a factor contributing to his exclusion from the company of the Greshamites. .... [I]t could be argued that even Hobbes's friends within the Society would have realized that, institutionally speaking, it was impossible to make Hobbes a member, since this would have almost certainly caused irreparable damage to the Society's public image, and perhaps even destroyed its very existence (Wood, 1988, 106).

The picture of Hobbes as a curmudgeonly outsider, ranting plaintively and interminably against the Society even while rattling the doors locked against him, hardly does him justice. It is critical that we not lose sight of Hobbes's reputation among his contemporaries, for as Mintz claims, that background of intense opposition sets "his boldness and originality...in high relief" (Mintz, 1962, 155).[70]

[70] By way of contrast, Shapin and Schaffer's account of Hobbes's "heresy" and the public response appears in *L&AP* at 293-298; 313, and interspersed elsewhere.

By far the most convincing and consistent account available to us today of why Hobbes was excluded from the Royal Society is that, to be identified with Hobbes was tantamount to being identified as "an atheist and a lecher." This meant that the Royal Society had good reasons to distance itself from Hobbes *because of Hobbes's reputation* and not because of Hobbes's beliefs -- whatever their specific content. It also means that even people who were influenced by Hobbes or found his ideas helpful, were unlikely to admit to his influence. For a time extending far past Hobbes's death, one can follow the path of Hobbes's influence only by assuming that it was more likely to be "hidden" than acknowledged. This largely circumstantial argument is one that G. A. J. Rogers has attempted, finding striking evidence of unacknowledged influence by Hobbes on both Locke and More (Rogers, 1988, 189-205).

Additional evidence for this explanation of Hobbes's exclusion from the Royal Society is that, prior to the Restoration, Hobbes's place in the informal circles of the new science was never questioned; he counted over a dozen of the early Fellows among his good friends, admirers, or associates. Even some of the accounts of Hobbes's "dogmatism" that crop up in the earliest days of the Royal Society seem to be a little trumped up. Noel Malcolm argues that during the Restoration, something close to a campaign took place by the publicists of the Society, to distance themselves and the views generally prevalent in the Royal Society from Hobbes's:

> Hobbes was becoming an increasingly disreputable figure, both politically and theologically; and the people who felt that it was most in their interests to blacken his reputation further were the ones who were vulnerable to embarrassing comparisons between his position and their own. Most modern descriptions of the early years of the Royal Society still fail to give a sufficient sense of just how nervous of criticism the publicists of the Society were (Malcolm, 1988, 60).

Hobbes was not only a dangerous but also a convenient target for the rhetoric of the liberal theologians. Where they offered rational belief, Hobbes represented atheism. Where they offered the sociable virtues of moderation, humility, and devotion to peace, they stood in contrast to the "dogmatic" Hobbes (Malcolm, 1988, 62). The middle way they offered steered a course between twin dangers of the age.

But what Hobbes represented to the Royal Society was not opposition to their experimental "form of life"-- or even to their form of social organization. Indeed Hobbes was very much interested in the creation of such organizations, about which he corresponded with Sorbière just prior to the

constitution of the Royal Society.[71] Rather, Hobbes represented a threat to their own viability as a public organization. The irony of course was that this meant that the "public" organization had to be closed to Hobbes in order for them to stay open for business. Hobbes had to be excluded, not because he disagreed with certain views concerning experiment, but because the Society ran a publicity risk in being identified with him. If, in the wake of these events, Hobbes also complained that Boyle's experiments were not truly public, he was not only insisting that an organization could not be both public and private at the same time, but he was also speaking the truth. The fact that Hobbes also objected that truth could not be arrived at intersubjectively via experiment, was a theoretical point that should be balanced with the interest Hobbes also demonstrated, both in experiment, and in the subjective *and* rational discourse of philosophers and scientists.

3.8 Science as Politics and Community

> *Thesis 8: The consensus of a scientific community provides the source of authority of modern scientific knowledge, and thus there has been, since the establishment of consensual scientific community with Royal Society, a seamless continuity of science and politics.*

This brings me to the capstone thesis of *L&AP*, and to a brief examination of the implications for this thesis for the questions raised above about theses 1-7. In my final chapter I will step outside the confines of *L&AP* to take a more extended look at the idea of scientific community. In that last chapter I will ask whether it might be more accurate to identify *modern* science with the emergence of disciplined, consensual scientific communities, than to look to the communal organizations of early modern science as the seed from which science, and the scientific revolution, arose.

How important to Shapin and Schaffer is the identification of community with science? One will not find an entry for "community" in the index of *L&AP*; but this only underscores the extent to

[71] Their correspondence concerned Montmor's academy -- a group of French scientists with whom Hobbes had been associated, and who had recently adopted a formal constitution. Malcolm suggests that Hobbes may have even been party to the conversations or other dealings that helped establish the Royal Society; and this, as Malcolm says, "would help to explain his deep resentment at being excluded from it" (Malcolm, 1988, 59-60).

which the idea of scientific community pervades this book and Shapin and Schaffer's approach to their subject.[72]

Although Schaffer is an historian first, albeit with a strong interest in the social aspects of scientific activity, Shapin is a sociologist by training and profession. There is a strong and probably inevitable tension therefore between the notion of local, or historically situated, categories for describing social phenomena, and categories that at least have some claim to universal meaning and application. In sociological usage, "community" implies a common referent, whether we are talking about seventeenth-century England, twelfth-century Italy, or twentieth-century Chicago. But "community" in an historian's usage might be presented as an idea, or category, with a much narrower referent. This tension, I believe, is to some degree unresolved in the work at hand.

Several of Shapin's early published essays deal with the theory of scientific community, and with the problem of how (and where) to discover the demarcation of scientific community from the (non-scientific) public. Among these is Shapin's 1974 article with Arnold Thackray on the "British Scientific Community".[73] It is a methodological piece, in which they advocate that historians of science question the demarcation. "Scientist," and "scientific community," Shapin and Thackray write, are social constructs. In the eighteenth or nineteenth centuries, the idea of "scientific community" did not mean a strictly professional group. Rather, they say, there was a congruence between the "perceived rules and structure of the scientific community as a social system and the vision of a changed and rationally reformed society". "Science" in this meaning related more to utility, and to "a vision of a new, rational order," than to any "passionate commitment to the literal truth" of scientific rhetoric (Shapin and Thackray, 1974, 5; 10). Shapin

[72]A key section in Chapter II of *L&AP* , for instance, is titled "Scientific discourse and community boundaries" (69-72); this section discusses a "community of experimental philosophers," a "moral community of experimentalists," "the crystallization of an experimental community centered on Gresham College," and a "legitimate philosophical community," all of which were essentially equivalent, and which managed their membership through more or less articulated "rules" for admission and retention, "entry fees," and practices of "recruitment." Clearly what is implied is a fairly deliberate kind of "community," almost in the sense of a "club." Indeed, one of the issues glossed over in *L&AP* may be the relationship between a more informal notion of community and the notion of community as a relatively formal association (like the Royal Society). The "gloss" is illustrated later in the same chapter, where Shapin and Schaffer present the relatively informal strategy of "linguistic practices" as the key technology that kept Boyle's "community from fragmenting" and "bound its discourse internally and externally" (*L&AP*, 77).

[73] Steven Shapin and Arnold Thackray, 1974, "Prosopography as a Research Tool in the History of Science: The British Scientific Community 1700-1900," *History of Science*, 12, 1-28.

and Thackray go on then to suggest that the early British scientific community might best be understood not as an association of self-identified "scientists," but as made up of people who participated in scientific communication in different ways: either by contributing directly to scientific discourse; by participating in identified scientific associations or teaching or disseminating the contributions of others; or by "patronizing" or otherwise disseminating scientific principles without identifying primarily with scientific teaching or research institutions (Shapin and Thackray, 1974, 12-13).

In 1982 Shapin published another article that is important in understanding his notion of community: "One can either debate the possibility of the sociology of scientific knowledge [SSK] or one can do it," he wrote[74], launching into a methodological and substantive review of work he judged to represent important contributions to SSK *praxis*.[75] Here Shapin argues that common threads throughout scholarly work in SSK includes the "contingency of scientific judgments about experimental findings and matters of fact," and also that "such judgments may well be structured by wider social interests" (Shapin, 1982, 191). This "empirical" or "instrumental" approach to the sociology of science that he surveys -- and to which both he and Schaffer contribute in *L&AP* -- is one that views knowledge, including scientific knowledge, as "goal directed....produced and judged to further particular collectively sustained goals" (Shapin, 1982, 197).

This leads to several observations: First, it is helpful to realize that Shapin and Schaffer approach their subject in 1985 with an emphatically articulated methodological and theoretical toolkit in hand. But second, the story they tell of Boyle and Hobbes is *not* simply another case study in the tradition described by Shapin in 1982. It is more ambitious: it is the "origins" story precisely *for* this (socially interested, goal-directed, consensual) understanding of science.

The drama of their account of the conflict between Hobbes and Boyle stems from the opposition Shapin and Schaffer identify between a pre-modern, rationalist, individualistic model for

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[74] A call to arms which he repeated in the first chapter of *L&AP*: "One can either debate the possibility of the sociology of knowledge, or one can get on with the job of doing the thing" (*L&AP,* 15).

[75] Shapin, 1982, "History of Science and its Sociological Reconstructions," *History of Science* , 20, 157-211.

attaining natural knowledge; and a "modern" way pioneered by the Royal Society, privileging shared knowledge and group consensus over individual knowledge, reason, or belief. The "experimental life" described as the key feature of the Royal Society's scientific activity stands in *L&AP* for "experimental community." Shapin and Schaffer effectively equate modern science *with* that community. "Our subject is experiment," they say, and their research is intended to determine whether "experiment [is] a privileged means of arriving at consensually agreed knowledge of nature, or are other means possible?" (*L&AP*, 3). Shapin and Schaffer conclude that it is the "clear judgment of history" that the "experimental way of life" is epistemically privileged; but with the caveat that the privilege is granted by politics, not by nature. The privilege of experiment is thus an historical outcome, and both socially and politically contingent: "[T]here was nothing self-evident or inevitable about the series of historical judgments...which yielded a natural philosophical consensus in favour of the experimental programme" (*L&AP*, 13).

It was a only set of historical and political contingencies that led to the "judgment of history," making "properly grounded knowledge generally" one with "artifact[s] of communication and whatever social forms were deemed necessary to sustain and enhance communication" (*L&AP*, 25). Boyle is important, in their analysis, because he was a prominent and influential voice for this new philosophy. Boyle's view was that the critical element in the performance of an experiment was not the performance itself but "the assurance of the relevant community that they had been so performed" (*L&AP*, 55). This in turn required that the experiments be performed in a "social space," initially through a collective witnessing of an experimental process, but then through "virtual" social space such as highly credible written public accounts, or the performance of closely identical experiments before a variety of geographically distant audiences (replication).

The defining feature of the "community" Boyle advocated was in direct conflict with Hobbes's: where for Hobbes community was useful as a form of cooperation to achieve a common purpose or interest, for Boyle community was not just the vehicle but the authority for that common purpose: "the public constitution and validation of knowledge." Such knowledge might begin with "individuals' acts of seeing and believing," but it was only "completed when all individuals voluntarily agreed with one another about what had been seen and ought to be believed" (*L&AP*, 78). Nature was to enforce this agreement, but agreement there must be, or there would be no knowledge!

For Shapin and Schaffer, Hobbes represented a type of knowledge that was *not* defined by the community, but was arrived at according to (agreed-on, authoritative, but individually-practiced) "rationalist methods and....demonstrative discipline" (*L&AP,* 69-70). Therefore from Boyle's point of view, Hobbes was, if not socially then philosophically "unclubbable." Hobbes was the kind of natural philosopher "Boyle despaired to recruit and assimilate" (*L&AP*, 72). Hobbes represented "radical individualism -- the state in which each individual set himself up as the ultimate judge of knowledge."[76]

What was this "individualism"? For Hobbes, "to end with agreement, one must start with agreement;" and agreement could only be enforced one of two ways: first, by deriving rational knowledge about human constructions (geometry, society); or second, by "settling the significations of ... words" and deriving knowledge rationally from those definitions. Thus experimental "knowledge" could not enforce agreement but would always have the status of "belief" (*L&AP,* 102-106). To secure the peace, people might submit to authority in order to attain civil agreement on any matter; but this did not secure "knowledge" or enforce individual "belief". This was the extent of Hobbes's "individualism:" the preservation of the irreducible validity of individual belief, even in the face of political authority or the cumulation of public "evidence".

Because they have constructed their account of community on the microhistorical "event" of the controversy between Hobbes and Boyle, their thesis therefore requires that the major point of contention between Hobbes and Boyle was over the meaning of the socially-constructed demarcation between a scientific community and the "public" (with its religious, political, economic, and other extraneous concerns. For Boyle, they say the "meaning" of this demarcation was the creation of a realm of knowledge arrived at by consensus. For Hobbes, they say, consensus could never be a sure source of knowledge, and as far as natural philosophy was concerned, all individuals must ultimately rely on their own beliefs. The outcome of the Hobbes-Boyle controversy -- victory of Boylean "demarcation", along with a presumption for the superior epistemic authority of consensual scientific knowledge was, Shapin and Schaffer maintain, one

[76] Some scholars have suggested that this, and not only his stubbornness or vanity, may be why Hobbes might be called a "dogmatist" (*L&AP*, 78, but see also 136-9 where Shapin and Schaffer suggest these varieties of dogmatism are of a piece.

that has persisted into our present. Because this is such a crucial point, it is important to ask, how valid is this contrast between Boyle's and Hobbes's views?

BOYLE THE INDIVIDUALIST?

Although the "social technology" Boyle developed was socially managed and directed at social ends, Shapin and Schaffer point out that "there were massive [individual, as well as collective] rewards from a successful exercise in boundary maintenance. Within this community debate was free: hence the remarkable authority experimenters claimed." Boyle crowed that the freedom he exercised in his own opinion was not subject to the judgment of "the multitude of hands, through which [that opinion] has passed unsuspected" (*L&AP*, 303). Presented as a clever, if somewhat paradoxical solution to the political dilemmas and conditions of the Restoration, one may still question whether the core political value here is consensus, or rather the benefits derived from membership in an exclusive "club" that would protect the special claim to "free inquiry" by its members.

Theodore Porter has suggested that there has always been an inherent contrast between the claims of the socially privileged, and the claim by science to "objectivity":

> [Objectivity] implies nothing about truth to nature. It has more to do with the exclusion of judgment, the struggle against subjectivity. This impersonality has long been taken to be one of the hallmarks of science..... [T]his, more than anything else, accounts for the authority of scientific pronouncements in contemporary political life. ... In science, as in political and administrative affairs, objectivity names a set of strategies for dealing with distance and distrust. If the laboratory, like the old-regime village, is the site of personal knowledge, the discipline, like the centralized state, depends on a more public form of knowing and communicating. Quantification is preeminent among the means by which science has been constructed as a global network rather than merely a collection of local research communities (Porter, 1995, xi).

The power of objectivity is also, Porter says, a challenge to subjectivity: and this

> ...has important consequences that are not often recognized. The strong self generally belongs to a social elite.... The educational formation of personal identity was always, implicitly or explicitly, the formation of a culture, usually an elite culture. An insistence on quantification tends to break that culture down, or to compensate for its absence (Porter, 1995, 76).

The price of admission to the experimental community, say Shapin and Schaffer, was the "matter of fact." But that is not the whole story: as Shapin and Schaffer knew well, a *hidden* price of

admission was also social status: Hooke had a very different position in the community than the gentleman Boyle. Other evidence presented outside of *L&AP* suggests further that the value attributed to agreement seemed to exist in parallel with the contrasting belief that solitude was an important source of epistemic and philosophical legitimation. Shapin himself has elaborated this interesting contradiction in a 1991 article.[77] Here Shapin points to Newton's brilliant output in enforced solitude, and Descartes's radical philosophical solitude as canonical evidence that the modern scientific tradition reserved a very important role for solitude in achieving scientific knowledge.

Shapin suggests that one solution to the tension between the virtues of solitude and those of collectivity, was that "solitude" was itself simply a social pose. The "rhetoric of solitude," he says, did not signify actual solitariness or social "vacuum" but rather disengagement from contaminating sectors of society and social interest, often via identification with a different social institution, itself removed from other venues. That institution might be a monastery or a rural estate. (One might ask, could it also be a Royal Society?) Even Boyle, Shapin notes, had a reputation for engaging in the rhetoric and behavior of solitude: "[T]hroughout the 1660's Boyle publicly yearned for solitude," but "[it] was a retreat to a redefined and relegitimized solitude," that was not, in actual fact, "solitary" (Shapin, 1991, 202-203). Faced with the powerful scientific fruits of Newton's radical (and real) solitude, Shapin suggests that a kind of truce was struck by followers of Boyle, that acknowledged solitude as the proper condition for scientific discovery, while retaining the belief that only public demonstration could properly serve as justification: "Individual belief had to be turned into knowledge; witness had to be shared....[P]rivate contexts of discovery had to be linked with public contexts of justification" (Shapin, 1991, 207).

Shapin understands both points of view as complementary *social* "repertoires" rather than as philosophically opposed views concerning the proper sources of epistemic authority. Granted that mathematical sciences did not seem to require the same degree of sociality even in their justification contexts, Shapin maintains that the need for public justification, for the separation of the "back" from the "front," of private reality from performance, is a kind of cultural universal in Western culture, stretching back to the Greeks and up to the present. A cultural universal is a social matter, Shapin implies, not a philosophical position: hence, "the study of the social uses of

[77] Shapin, 1991, " 'The Mind in its Own Place': Science and Solitude in Seventeenth-Century England," *Science in Context* 4, 191-218.

solitude adds further support to the notion that problems of knowledge and problems of social order are solved together" (Shapin, 1991, 191). But in what sense are such problems ever "solved"?

Another view of Boyle's approach to community is that he accommodated at least a partially individualist philosophy within an ostensibly communitarian social framework. Once again, a closer look at the dichotomies and issues presented in *L&AP* suggest a more complex and longer-range dynamic at work.

HOBBES THE COMMUNITARIAN?

As for Hobbes's views on community, Shapin and Schaffer present Hobbes as an "overweening rationalist" for whom the individual is the ultimate arbiter of knowledge, and for whom philosophy, not experiment or experience, is the only source of epistemic obligation (*L&AP,* 127, 143). Granted,

> ...as a practical matter, Hobbes could hardly deny that the experimentalists had established a community with some politically important characteristics: a community whose members endeavored to avoid metaphysical talk and causal inquiry, and which displayed many of the attributes of internal peace. But this community was not a society of *philosophers* . In abandoning the philosophical quest, such a group was contributing to civil disorder (*L&AP,* 337).

This passage hints at but does not do justice to the very real likelihood offered by other scholars that Hobbes was strongly interested in and even favored the establishment of societies where communal philosophical efforts and exchanges could be sustained. But the passage also implies that the chief virtue Hobbes would see in such a society was "political." Thus the authors sustain a distinction between "the study of nature and the study of men and their affairs," even while, in the next sentence, they acknowledge that Hobbes himself would have contested that distinction (*L&AP,* 337).

But this formulation doesn't do honor to the primacy of philosophy over politics in Hobbes's thought. Despite the historical circumstances in which *Leviathan* was written, political peace itself may not have been the highest goal of philosophy for Hobbes. Another reading might be that the effort to establish and maintain peace was a paramount derivative or secondary goal, arrived at via the pursuit of truth. If so, Hobbes's objection to the Royal Society might well be

understood as based in his philosophical argument with its epistemological premises, and not because Hobbes genuinely believed the Society was capable of fermenting or contributing directly to civil disorder.

Similarly, it is not clear that Hobbes's objection to the Society was based on a rejection of the value of community in attaining common goals. Tom Sorell has contrasted Hobbes's views on this point with Descartes, arguing that,

> Hobbes seems never to have shared Descartes's preference for the work of individuals over the products of groups. On the contrary, he wrote that the greatest human construction, a thing that would indeed parallel in its own way the world created by God, namely an enduring commonwealth, was a thing that could only be constructed by many people acting together. In the case of the reform of science Hobbes was too impressed by the results that could be achieved by a research group, such as the once centered on Mersenne, to have been attracted to Descartes's highly individualistic approach. In England, too, he favoured institutions which enabled scientists to work together. For example, he praised Gresham College .... and when members of it formed the Royal Society, he was greatly disappointed not to be elected as a Fellow (Sorell, 1988, 517-518).

So in the end we have two relatively complex characters in Hobbes and Boyle, rather than the more conveniently polarized ones we met in *L&AP*. Although the scholarly literature on each figure is vast, and I have only scraped at the surfaces of it, the result is already, in important ways, at odds with the history Shapin and Schaffer have given us. Considering the ongoing readership and attention their history receives among students and scholars of science, this is unsettling. If history is to provide us with case studies that illustrate ("empirically") the importance of certain ideas about what science is and how it is done, then it is somewhat unnerving, when at the microhistorical level, the more we know, the closer we get, the more the evidentiary story seems to shimmer and dissolve. Hiram Caton laments somewhat peevishly -- but with some justice -- "The ideal of accumulated knowledge [in history] is supposed to fill [each historian's blank spaces of ignorance] as each generation adds its light. But....even a well-defined subject, such as the thought of Thomas Hobbes, has generated a secondary literature so extensive that few scholars even attempt to encompass it. The ideal of accumulated knowledge gives way to the recirculation of ignorance" (Caton, 1988, 104).