"When I am most profoundly myself, I join a forgotten community." --Eugene Ionesco *(Notes and Counter-Notes)*

1. Introduction: The "Black-Boxing" of Scientific Community

How are we to understand the relationship between scientific knowledge, and those who practice science -- the "scientific community?" In the last thirty years, the idea of scientific community has gone from a convenient sociological label for those who happen to practice or are interested in science, having now an almost definitional status: the claim that science *is* the scientific community, no more or less, is deeply philosophical and historical. The strongest expression of this late modern understanding of science is the scholarship of social constructivism, which first emerged about thirty years ago as an explicit and integrated research program, and which has developed today into a still-controversial but firmly entrenched approach to understanding what science is and how it is done.

Published in 1985, Shapin and Schaffer's *Leviathan and the Air-Pump* not only serves as an exemplar of contemporary social constructivist work on science, but its *subject* is the historical emergence of an explicit and clearly articulated relationship between scientific knowledge and scientific community. Shapin and Schaffer suggest that the emergence of science in early modern Europe was tied directly to the emergence of scientific community as the locus of authority for the claims of science and scientific practices. Specifically, Shapin and Schaffer's close historical study centers on the debate between Robert Boyle and the British Royal Society, and the English philosopher Thomas Hobbes. The significance of this debate, in their view, was that it revolved around the location and sources of authority in knowledge-seeking practices, at a time when the relationship between science and community was still an open question. They follow the debate up to the point where Boyle (apparently) prevailed, with the implication that modern science has been permanently shaped by this outcome.

Shapin and Schaffer's account is also interesting because of its strong structural and methodological resemblance to Foucault's *Discipline and Punish*. Like *Discipline and Punish*, *Leviathan and the Air-Pump* attempts to provide a macronarrative history of the practices of

modern science -- a task which has so far eluded social constructivist scholarship.[1] But where Foucault's emphasis on the emergence of modernity points toward a "post-modern" understanding of science as part of a broader cultural movement, Shapin and Schaffer's emphasis on the emergence of science points towards what one might almost say is a "post-scientific" understanding of modernity: social constructivism.

The claims of social constructivism -- Shapin and Schaffer's work being no exception -- have brought on counterclaims by philosophers, historians, and scientists themselves, many of whom have seen it as an attack on realism and on the claims of scientific knowledge to a superior (objective, reliable) epistemic standing. Some have objected that the mere fact that scientists make scientific knowledge in communities, does not undermine the claim of science to superior epistemic standing. Others, while maintaining a realist stance, have adopted the notion that the sociality of scientific knowledge-making tends, in the long run, to increase its reliability and accuracy. But to some degree the emergence of social constructivism has succeeded in shifting the terms of the debate to the extent that, despite the fury of the "science wars," virtually all contemporary scholarly views on science acknowledge that a crucial role is played by scientific communities in the negotiation and assessment of scientific claims.

While I will not intentionally downplay the strong antipathies and disagreements between constructivist and realist positions, this is not the focus of this thesis. Rather, I will argue that our own terms of debate have largely been defined by where we stand as (late) *modern* scholars. Thus I think Shapin and Schaffer were absolutely right that, in any examination of unresolved debates over the nature of science in the early modern period, it is important to ask in a fair amount of details what alternatives were still open. I agree too with Shapin and Schaffer that the debate between Boyle and Hobbes, which was their main subject in *Leviathan and the Air-Pump,* was a powerful and important one and worthy of careful reexamination. Where I disagree with Shapin and Schaffer is over their claim that the principle subject of that debate was the nature of *science*; I will argue that a broader contextualization of that debate reveals it to have been an extremely interesting debate over the nature of *modernity* in general, and modern social structures and cultural developments in particular.

[1] This reminds us that postmodernism, in some ways the child of constructivism, has rejected the possibility and desirability of macronarratives.

Social constructivism has presumed a rough equivalence between modernity and science, which, I will argue, is incorrect. By separating the social and cultural tactics and patterns of modernity from our understanding of science (something most easily done perhaps by looking at the edges of modernity in the late seventeenth and late twentieth centuries), we may discover new ways of thinking about the relationships between community and science and between science and modernity.

1.1 Re-Opening and Closing an Historical Black Box

"Hobbes was right." With this final flourish, Shapin and Schaffer suggested a paradoxical end to the seventeenth-century controversy over whether science is socially constructed. Yes, they affirmed, it was; so the case of Hobbes had clearly demonstrated. But at the same time it was Robert Boyle, whose particular social solutions to the "problem of knowledge" were victorious over Hobbes's. Given the heat of the original battles and the ironical outcome, it is fitting that the debate over the early modern negotiations about the scientific knowledge goes on, even three hundred years later. Since Shapin and Schaffer's book was published in 1985, contemporary science and technology scholars have contributed to a continuously expanding body of research centering on the early Royal Society and its major figures. Many of the supporting themes found in Shapin and Schaffer's work also flourish in science studies: among these are historical studies of rhetoric in science; studies of the social constraints, conditions, and conduct of (early modern) science; and, considerable new scholarship on the central place of experiment in early modern scientific knowledge. In light of this it might seem implausible to argue that anything was closed off by Shapin and Schaffer's bold rhetorical closure.

But one of my arguments will be that Shapin and Schaffer *did* effect a closure. I will not attempt to establish definitively that Shapin and Schaffer were wrong, or that they distorted the available evidence. I must note in fact that I can provide no new evidence nor any authoritative weighing of alternative views and criticisms made by distinguished scholars of the period. My challenge will be rather to present this other scholarship in a way that offers a wider context and to present suggestive evidence in support of two tasks. First, I believe reintroducing excluded evidence and interpretations will suggest that Shapin and Schaffer's interpretation of the events was itself cunningly "constructed". By reopening the case to which Shapin and Schaffer's book is devoted -- and by revisiting the evidence which they offer in their book -- it may be possible to recover some important insights and alternative interpretations concerning why this early modern

historical controversy was, in ways other than those Shapin and Schaffer identified, so important to our late modern understanding of science. Second, my aim will be to take a sufficiently broad view of contemporary science and technology scholarship that it will be possible to see how Shapin and Schaffer's analysis unintentionally illuminates those elements of the historical controversy between Boyle and Hobbes that contribute to our understanding of the emergence of *modernity* as opposed to the emergence of *science*.

Before proceeding, I will expand a little on both of these points. Shapin and Schaffer propose an understanding of the Royal Society and its formation, both as point-of-origin and archetype for scientific *community*, and effectively, as a vital *instrument* for the conduct of science.[2] Their understanding of *instrument* is richly represented in social constructivist literature. As described by Jan Golinski in the following discussion of Andrew Pickering's work, the process whereby an instrument in science comes into being (and hence is, in every sense, constructed), can be described as follows:

> [W]hen an instrument....assumes the status of an accepted means of producing valid phenomena, then it can be said to have become a "black box." The constructivist outlook suggests that this is not simply a matter of the configuration of the hardware, but also involves the creation of a consensus of how the hardware should be understood. Agreement about the factuality of the phenomenon is, at the same time, agreement that the apparatus has been appropriately used to produce it. .... The apparatus thus makes the transition from an object of investigation to an "instrument," properly speaking; henceforth, it is trusted -- at least provisionally -- to produce authoritative new knowledge. As Andrew Pickering puts it, the experimenters have achieved a stable fit between their model of how the apparatus works and their model of the phenomenon under investigation. Such a stabilization, initially a local and temporary accomplishment, constitutes both the "discovery" of a phenomenon and the black-boxing of a working instrument (Golinski, 1998, 140).[3]

In *Leviathan and the Air-Pump*, Shapin and Schaffer set about showing us a critical new piece of "hardware" -- the resources, activities, and products of the Royal Society. They consider it critical to localize the creation of a consensus about how the hardware should be understood, and how the new hardware (community) came to be trusted to produce authoritative scientific knowledge. Also typical of the constructivist method is Shapin and Schaffer's use of textual and

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[2] Joseph C. Pitt has suggested "technologies" as an alternative word to describe what I call "instruments" here, in his discussion of social arrangements such as those of the Royal Society, and also the N.I.H., the patent system, etc. See his *Thinking of Technology: Foundations of the Philosophy of Technology* (in press).

[3] One might well substitute "human software" for "hardware," and "origins" for "discovery".

rhetorical analysis. These tactics are used to help make the crucial argument that the Royal Society made novel use of literary technologies in establishing their own style and practice of science.

At the same time, Shapin and Schaffer depart to some extent from typical constructivist methods in providing an account of the historical and political contingencies, the manners and morals of Boyle's and Hobbes's day, that is remarkably close to Merton's understanding of early modern science. In other words, Shapin and Schaffer claim that the scientific *community* that emerged from the black-boxing of seventeenth century British politics and etiquette, is *definitive* of *science*. It is to this particular form of social organization that scientific knowledge owes its "special" epistemological status and claim to authority.[4] Thus Shapin and Schaffer's work embodies, however ironically, a Mertonian thesis concerning scientific community. Despite their constructivist method, what they look for in the black-box of early modern science are exactly the ingredients that gave rise to a scientific community in the Mertonian sense, as the primary and definitive locus of scientific authority.

Though originally shaped by historical conditions and contingencies, the Royal Society is offered by Shapin and Schaffer, just as it was by Merton, as the prototype for the most powerful "instrument" of all -- scientific community. They imply that this instrument has endured, in important ways unchanged, since the seventeenth century. Even Shapin and Schaffer's fiercest critics have rarely taken issue with Shapin and Schaffer's characterization of science *as* community: most controversies have concerned whether Shapin and Schaffer's study of scientific community has identified the correct time scale for understanding the constraints and duration of

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[4] It is surely not coincidental that Merton's early theoretical work on the sociology of science was also based on early modern science in Britain. Shapin (1988) has an interesting article that explicates and to a large extent defends Merton against his critics ("Understanding the Merton Thesis"). It is interesting in part because of Shapin's defense of the move to extrapolate boldly from microhistorical particulars -- a move Shapin has made himself. And it is perhaps even more interesting in Shapin's discussion of Merton's employment of the Paretan notion of "sentiments," to achieve a bridge -- in "black box" style -- between materialism and idealism. One might argue that for Shapin, "community" serves a similar role, bridging microhistory and macrosociology. At any rate, Segerstråle is correct in observing that Shapin and Schaffer's argument is "surprisingly close to the famous Merton thesis of the relationship between science and Puritanism in seventeenth-century England.....One looks in vain, however, both for an explicit comparison with Merton or even a reference to his work" (Segerstråle, 1987, 544-545).

the "experimental instrument" of community[5]. For example, Shapin and Schaffer's critics have challenged them on such issues as whether the particular form of science represented by the Royal Society was enduring (was it truly the original instrument, or simply an early foreshadowing of it); or whether the Royal Society is really paradigmatic of related inventions or instruments (the Royal Academy, Scottish or Italian scientific societies, contemporary universities, etc.). Was the instrumental phenomenon of the Royal Society indeed of very short-term duration, in the sense that it changed significantly from its origins before becoming more influential on the longer-term?

By looking back inside the black box of community at the substance of the disputes between Boyle and Hobbes, it may be possible to recover a sense of what were at the time beautifully explicit and almost epic, if now somewhat obscure, disputes concerning science. Certainly Hobbes and Boyle tangled over the immediate facts and claims of the new experimental science as well as its social arrangements. But behind these, I suggest, there was a underlying tension that from the vantage point of late modern science may take more work to bring out. One side of this tension was the assumption (represented by Boyle) that, through the performance of scientific acts we can stand next to God's world, the divine order of nature. The other side was the (Hobbesian) view, that human actors are physical beings in a more or less disorderly world, participating in the work of creating an ordered understanding of the world. Obviously in this opposition there are undercurrents of the complex and many-leveled tensions between dualism and monism, idealism and realism, constructivism and realism, experiment and theory. (Hobbes presents an interesting case to the modern scholar in having embraced some of these apparently incompatible views, to wit his firm stance as both a materialist and a constructivist.)

Thus, I am suggesting we might reverse the flow of Shapin and Schaffer's argument (that the solution to the problem of society drove the solution to the problem of knowledge), by saying that deep differences in ideas about the purpose and limits of human knowledge had profound implications for the *sociology* of science. If one begins (as the Royal Society did) with the assumption of a divine order in nature, then the world can be understood as made of parts that skilled and experienced scientists can learn to isolate in pure form, using instruments that limit degrees of freedom in the world. Once these parts are identified, scientists can collect them

[5] There are close similarities between this analysis and Galison's description of long-, medium-, and short-term constraints on experimental activity. Galison in turn references Braudel's separation of historical time into "geographical time," "social time," and "individual time" (Galison, 1987, 246, 247-255).

together and use them to reconstruct an underlying created order of things. It is easy to argue that the proper social instrument for accomplishing a science based on this assumption is an orderly democratic group, committed to the repetition of experiments and a consensual evaluation of their claims, which in turn constitute the *authority of science*.

On the other hand, if one begins (as Hobbes did) with a view of the world as "naturally" disorderly, so that human intervention is needed to create names for things -- and, by using those names, to create order by reasoning about the world -- then any systematic understanding of the world can be assumed to be the result of effort and artifice. In this scenario, the proper social instrument for scientific understanding may be the individual agent, or perhaps a deeply integrated small group of individuals. Either of these, it might be argued, would be better suited to attempt synthetic understanding of the world, since that understanding would have to be achieved through what is essentially a creative act of imagination, and the actions that demonstrate aesthetic and moral commitment. For such an agent, community might be thought of as an existential *context* (a context of *others*), not an *authority*.

The presumption that science concerns an underlying but preexisting order is so pervasive in our understanding of modern science, that it is difficult to imagine the alternative. Our sense that science is "about" an order which we firmly believe exists, is both reflected and constituted by our notions of objectivity, metrology, and the "discovery" of "natural laws." All we need to do to find it is to pare away subjectivity, deviance, and anomaly. Even the celebrated cutting edges of scientific thought at the end of this twentieth century tend toward these suppositions: from Kauffman's celebration of "order for free" -- the spontaneous emergence of complex order from simple, seemingly random processes; to the continuing search for Theories of Everything.[6]

It is fitting that the immediate instrumental conflict between Boyle and Hobbes (in the conventional sense of "instrument") concerned the plausibility of creating a physical vacuum. The air-pump was the epitome of instrumentation that would create perfect physical control, a perfect reduction of the degrees of freedom in the universe of the evacuated tube. It was, one might fancy, the material metaphor for the ideal to which the Royal Society subscribed: a human

[6] Similarly, concessions of this century to a less generalized (more pluralistic, more discipline-specific, and more practice-oriented) understanding of science, have not undercut the implied metaphysical commitment to a fundamentally ordered universe. In other words, scientific holism and reductionism, though opposed to each other, both presume that the identification of objects, the states they "possess", and "objectivity," are more fundamental than process, change, and instability.

society in which sentiment, philosophy, social standing, and more, were ostensibly evacuated. No wonder the air-pump was Boyle's trademark and Hobbes's target.

Yet it is quite common for scientific instruments -- including scientific community -- to be thought capable of "isolating a fragment of preexisting reality, separating a 'signal' from the background 'noise'." The alternative is to think of them as "instrumental system(s) [that] produce[s] certain entities and destroy[s] others. Thus, 'an experimental system is a labyrinth, whose walls, in the course of being erected, in one and the same movement blind and guide the experimenter'" (Golinski, 1998, 321). This view, I believe, is one with which Hobbes might have sympathized.

Below I will set out an argument that the scientific community, understood in this way as an *instrument,* carried with it a deep assumption of natural order. The idea of scientific community and its assumptions have profoundly guided modern science; but at the same time they have blinded us to a full understanding of what science is and may be. We must ask: is the meaning -- the signal -- of science to be found as much in the noise of the universe as in its "laws;" as much in the constructive syntheses of individual human actors, as in the democratic or meritocratic rule of communities? If we ask these questions, then the difficult Thomas Hobbes, even with his stubborn ideas about the nonexistence of vacuums and the squaring of circles, holds a most important place at the table of science, if not of the Royal Society.

In order to get to these arguments, I will first summarize a dozen or so important themes in Shapin and Schaffer's book, and review those themes in the context of other scholarly work -- including their own earlier and later scholarship. I will look not only at direct reviews and critiques offered of their 1985 work, but also at the impact the book has had in the way of opening up -- and closing off -- certain avenues of research and research questions.

I will then attempt to demonstrate that, if you take all of the key critiques together, they act as a set of filters that collectively reveal something that has been missed, not only by Shapin and Schaffer, but also by their critics. On first reading, the skillful contemporary scholarship in this area has given us a wonderful but conflicting tangle of stories. The student and the scholar alike may be excused some confusion over what to make of this welter of subtle differentiations and fine-tuning, of sympathetic but crucial revisions, not to mention vehement disagreements. But I will attempt to show that, if one takes these critiques collectively, applying not one but all the

filters at once, what is revealed as uniquely important in this book is the story of the controversy between Hobbes and Boyle.

This means, first of all, that we have Shapin and Schaffer most emphatically to thank for bringing this story back to scholarly life. But second, the reason this matters is that the story of Hobbes and Boyle represents the basic tension between notions of authority based on consent, consensus, and social-political agreement; and *authority* in the etymologically older sense of being identified with a particular agent, and author or "auctor" (one who is a source of growth or increase). It also represents, as we will see further on, the tension between the notion of "maker's knowledge" (a notion of Mersenne's adopted by Hobbes) and "observer's knowledge" -- the knowledge of witnesses.[7]

What is implied, I argue, by these tensions, is the need for a third leg, responsive to the broad traditions of natural history and natural philosophy. Such an enterprise might be termed *natural sociology*, but perhaps better -- since "sociology" retains the sense of distinction between human life and other natural phenomena -- might be *natural reason.*[8] The need for such a "third leg" is the need for a naturalized conception of collective and intellectual human life, with biological, ecological, and cognitive dimensions. This enterprise might retain ideas like community as short-hands of convenience, but would recognize that such terms do not (as many in contemporary science studies would have us think) have clear meanings, unambiguous referents or explanatory powers.

The development of such a science might well transform our understanding of science, and scientific community. If so, it might also transform some of the most famous myths that surround the emergence of early modern science. The discussion which follows is a kind of case study in how we think about science and scientific community. My hope is that it will shed some light on what can be learned from the history of science about the larger patterns and dynamics of science in which we are enmeshed.

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[7] There is a parallel here with the tension between the idea of *experiment*, as a concrete object and event; and *experience*, as an encounter and process. This distinction is clear in English, not so clear perhaps in other languages; in French, *expérience* is used for both meanings.

[8] The parallel between my suggestion and the systematic philosophy of Hobbes' *Leviathan* -- also consisting of three parts -- is probably not coincidental. I owe the suggestion of "natural reason" as preferable to "natural sociology" to Prof. Joseph C. Pitt, who proposed it in the course of my thesis defense in May 1999.

2. *Leviathan and the Air-Pump* as a Case Study in Modern STS

Shapin and Schaffer's *Leviathan and the Air-Pump* (*L&AP)* was first published in 1985 to good reviews. It appeared almost twenty-five years after Kuhn published his hugely influential argument on paradigms and revolutions in science. Although the analogy fails in many important respects, science studies in the last decade has circled around Shapin and Schaffer's book in something of the way that social understandings of science were developed with reference to Kuhn in the 1960's and 1970's. Like Thomas Kuhn's work before it, Shapin and Schaffer's has served as both a touchstone and a source of continuing controversy in science studies.

Of course *Leviathan and the Air-Pump* is radically different from Kuhn's work in many respects: Shapin and Schaffer's book boasts a skeptical, ironical tone; a microscopic historical scope (if one with macrohistorical implications); reliance on close textual analysis; and strong sociology. But as Jan Golinski has pointed out, like Kuhn's *Structure of Scientific Revolutions, Leviathan and the Air-Pump* was a new attempt to resolve the tension between macro and micro levels of analysis in the history of science. The difference, Golinksi notes, was that *Leviathan and the Air-Pump* lacked any attempt at the "diachronic macronarrative" that Kuhn had promised but was himself unable to deliver (Golinski, 1998).[9]

2.1 The Argument of *Leviathan and the Air-Pump*

A central claim of Shapin and Schaffer's -- echoing Merton's more cautious arguments made almost fifty years before -- was that modern science was born in the unique political and cultural milieu of seventeenth-century England. It is somewhat surprising that this claim in itself is not challenged more often on account of what is surely unconscious ethnocentrism. Any Anglo-American claiming that a tiny organization of the socially privileged in seventeenth-century England was the cradle for any human endeavor as huge as "science" faces this challenge. If, on the other hand, we break "science" apart from "scientific community" and consider the invention of the latter more on par with the invention of the cotton gin than the invention of music or philosophy, the fault is not so egregious. Another perhaps more convincing way of parsing the two concepts *science* and *community* apart is to suggest that the invention of this kind of

[9] That is, a narrative that represents not only a "big picture" account of large-scale developments and changes, but one developed via historical events across a relatively long period of time.

"community" was not peculiar to science but was peculiar to what we think of as modernism. To affect this conceptual separation is of course my aim.

What defined "modern" science, Shapin and Schaffer maintained, was the emergence of a scientific community. This entity, which reserved to itself a new kind of epistemological authority, was a normatively constituted social group, working with an avowedly experimental and performative methodology; marked by a communal etiquette and identity; and declaredly free from other authority, whether political, religious, or philosophical.[10]

The organization in which community emerged was the British Royal Society. Its authority was historically new, and contrasted with a disputational model of scientific practice that had preceded it. That earlier mode of practice had drawn on the "ancient authority" of accepted texts, and the synthesis of "experience." It was subjected neither to the tests of experiment, nor to the collective "witnessing" of such manipulated events.

And yet, Shapin and Schaffer claimed, the real source of the Society's epistemic authority was not experiment, as the Society maintained at the time, but rather politics: "The solution to the problem of knowledge is political: it is predicated upon laying down rules and conventions of relations between men in the intellectual polity" (*L&AP*, 342). To help sharpen this point, Shapin and Schaffer focused on a neglected part of the history of the Royal Society: a heated controversy that took place before the "black box" of scientific community closed.[11] This controversy was between a central figure in the Royal Society, Robert Boyle; and the philosopher Thomas Hobbes. Hobbes was of course a renowned and notorious figure in British mathematics, science and natural philosophy, as well as a preeminent political philosopher. But he was, at least to some degree marginalized by the Royal Society, a condition which provoked Hobbes's considerable literary ire.

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[10] Their position is shared by other constructivist scholars, among them Peter Dear, for whom the "consolidation" of the scientific revolution in the latter part of the seventeenth century was both cause and evidence of the development of a *community of practitioners* (Dear, 1985, 145).

[11] "There is not very much written about these disputes [between Boyle and Hobbes]... It is possible that part of the reason for these errors, and for the general neglect of the Hobbes-Boyle controversies, is documentary....Hobbes *Dialogus* has never been translated from the Latin original, and this may go some way to explain its neglect" (*L&AP*, 9).

The Boyle-Hobbes controversy over the Royal Society's air-pump experiments has the superficial appearance of a textbook case of scientific progress: correct modern science vanquishing the seemingly ludicrous objections of an old guard unable to understand or accept new ideas. Why then should it hold interest for late modern researchers? According to Shapin and Schaffer, it was precisely the resolution of this controversy in favor of the Royal Society that should interest us. It was, they argue, to the resolution of this controversy that we owe the now familiar contours of our current understanding of what science is, and how it is done. "Our present-day problems of defining our knowledge, our society, and the relationships between them centre on the same dichotomies between the public and the private, between authority and expertise, that structured the disputes" between Boyle and Hobbes (*L&AP*, 343). Shapin and Schaffer conclude that this history thus demonstrates the "conventional and artifactual status of our forms of knowing.....it is ourselves and not reality that is responsible for what we know" (*L&AP*, 344).

Shapin and Schaffer's argument is the more powerful in being strongly argued by Boyle's own adversary, Thomas Hobbes himself. For Hobbes, the political nature of the Society was abundantly clear. The great irony of the story of course is that despite Hobbes's accuracy and perceptiveness on this point, it was the "conventional and artifactual" Society that emerged as both the immediate -- and long-term -- victor in the war for the definition of "science". Despite their relatively generous treatment of Hobbes, and the bold *envoi* affirming Hobbes's correctness (in their mind, by asserting the constructivist position several hundred years before it became popular) -- Shapin and Schaffer did not seriously attempt to restore Hobbes *as a scientist*: "Of course," they wrote, "our ambition is not to rewrite the *clear judgment of history:* Hobbes's views found little support in the English natural philosophical community" (*L&AP*, 13, emphasis added).

2.2 The Authors: Shapin's and Schaffer's Related Work

In 1985, Shapin and Schaffer were writing within the newly emergent perspective of social constructivism. This tradition (if it can be called that after thirty years) was itself strongly influenced by Kuhn, and especially by Kuhn's treatment of four issues: (1) the treatment of idea that practical reasoning in science as governed by accepted conventions; (2) the role of authority in scientific pedagogy; (3) the nature of scientific controversies; and (4) the definition of scientific communities (Golinski, 1998, 13). These four issues are tightly interwoven not only in Kuhn's but in subsequent constructivist accounts of science. The social theory that informs such accounts is based on an implicit theory of social organization and interaction that aims for

validity that transcends prior or future historical events. Thus the irony is that while providing an ambitiously "macro" view of the phenomena, such accounts are effectively at odds with proposed historical macronarratives (such as those proposed by Marx or Foucault).

For Kuhn, scientific community and its conceptual, practical, and didactic paradigms, were mutually defining, but this begged the crucial questions concerning the dynamics of that process. This meant that an account of scientific controversy and conceptual change in science were central to a complete account of scientific practice and knowledge. Kuhn handled this problem through his famous, and famously problematic, distinction between "normal" and "revolutionary" science. Many scholars since Kuhn have turned their attention to patterns in, and details of, scientific controversies, in order to help flesh out a process that seemed convincing in Kuhn's account at the broadest level, but which seemed to get murky in actual historical cases.

Shapin and Schaffer's *L&AP* was in this genre of controversy studies, but it rose to the surface with particular flamboyance because their selected case was not only *a* study of scientific controversy, but was put forward as *the* turning point in the history of how scientific controversies are conducted and resolved. These historical events, persons, and institutions laid the origins of modern experimental science, by introducing the new authority of the scientific community to decide such disputes. Though historical, their study was even more:

> ...a sociological study, one anchored in the empirical studies of controversies....It began with disputes over "technical" facts and argued *outward* to the broader issues that were revealed to be at stake, rather than arguing from social context *inward* to technical content. The authors' debt to the controversy studies, and -- through them -- to Kuhn's account of the incommensurable debates between competing paradigms is clear (Golinski, 1998, 21-22).

In the section that follows I will examine the scholarly reception of *L&AP* and, insofar as they are relevant, reviews of Shapin's and Schaffer's subsequent work. But before doing so, in the present section I will look briefly at the context Shapin and Schaffer have *themselves* provided: the work that each produced prior to their collaboration; and the work they have separately produced following it.[12]

Simon Schaffer's primary disciplinary identity is as a social historian of science and technology. His work is eclectic with respect to the scientific disciplines and periods of time with which he has been concerned. He has studied eminent individual scientists (including Newton, Darwin,

[12] Although each has collaborated with other writers, for whatever reason, to the best of my knowledge they have not collaborated since 1985 with each other.

and Babbage), but he has also shown particular interest in social class and its relationship to experimental and other scientific instruments, ranging from the "electric planetarium" to Babbage's calculating engine. Since completing *L&AP*, Schaffer has been devoted to the analysis of (other) instruments, experiments, and the relations of experimental practice and experimenters to their instruments. He has also worked on the relationship between science and the "public" (an area which Shapin also addressed early in his career): a recent article by Schaffer consists of an analysis of the "performance" aspect of early eighteenth-century perpetual motion machines (Schaffer, 1995).

In all his work Schaffer has been identified with the "strong programme," devoted to the sociological interpretation of scientific practices. But while that program has seen its share of controversy, Schaffer's own work has not been particularly controversial. Among his many collaborations since working with Shapin, Schaffer has co-edited (with SSK scholars Trevor Pinch and David Gooding) a book entitled *The Uses of Experiment* (1989). This consisted of an edited collection of articles, putting forth a view of experiment, in the words of the publisher, that "emphasizes that experiments always involve choice, tactics, and strategy in persuading audiences that the world conforms to the picture that the experimenter has created." Currently at the University of Cambridge (Department of History and Philosophy of Science), Schaffer has recently returned to work involving the Royal Society, but in the 1730's, publishing a study of the "electric planetarium" and problems arising out of social relations between artisans and gentry in producing those experimental effects. The continuity, overall, between Schaffer's 1985 work, his work with Gooding and Pinch, and his later work, seems fairly smooth. His general approach could be characterized as developing cross-case replications of the 1985 argument.[13]

It is probable, then, that it is to Steven Shapin that we can attribute *L&AP's* broad Mertonian historical claims, its focus on literary technologies, and its strongly argued theoretical position on the social construction of scientific knowledge. So it is fitting that since the publication of *L&AP*, Shapin, and not Schaffer, has drawn the most fire from dissenting scholars. Shapin was at the University of Edinburgh in the 1970's and 1980's, when it was headquarters of the strong programme in SSK. Since the mid 1980's he has continued to hone and revisit his original arguments for many years, from a position in the Sociology Department and Science Studies program at the University of California in San Diego. Each of his numerous articles and several

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[13] My bibliography includes citations to many of Schaffer's scholarly publications.

books has added new layers and perspectives to the original *L&AP* history.[14] His later books include an analysis of the "social history of truth" and a brief but ambitiously macro-level analysis of the scientific revolution.

An article published in 1992 illustrates the breadth of Shapin's arguments, and also contains some useful if somewhat self-conscious notes on the "young(ish) Steven Shapin" of Edinburgh days.[15] In this essay Shapin traces the externalism-internalism debate in the history and sociology of science from its lineage in Merton and Sorokin and 1940's and 1950's Marxism, through work by Guerlac, Rosenberg, and Laudan, to the "eclectic" scholarship that followed, up to the work of Barry Barnes and Bruno Latour. Shapin sees himself as moving, under the influence of Latour, Collins, and Barnes, toward "bolder" positions; he represents his own work, both in *L&AP* and afterward, as a contribution to Barnes' "programme for empirical research into the constitution and consequences of scientific boundaries....We wanted to trace the contours of a cultural practice in which parties disagreed about what a proper natural philosophical practice was and where its internal and external boundaries ought to be located" (Shapin, 1992, 353).

In articles appearing in the ten years following the publication of *L&AP,* Shapin pursued his fundamental argument for social constructivism, defending it as an important antidote to conventional historical, philosophical, and social individualism:

> *....there is little that we can say with very great confidence about the domain of individual meanings and the causes of individual careers and creativity ....* The pull of the association between individuality and epistemic value is powerful, yet it can be defied. And the most effective way of resisting it is to question the dichotomy which invites us normatively to choose between isolated knowers fabricating their culture out of thin air or individual circumstance and 'cultural dopes' doing nothing more than reflecting existing knowledge or social predicament (Shapin, 1993, 335-345, emphasis added).[16]

Shapin's 1994 monograph took a theme from the his 1985 work on the Royal Society's early history and expanded it. In this book Shapin argued that an essential aspect of the early scientific community was the set of habits, social practices, and values that related "trust-worthiness" and "truth-telling" in seventeenth-century England, to the status of being a gentleman. As

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[14] Citations for many of Shapin's scholarly publications are found in my bibliography.

[15] Steven Shapin, 1992. "Discipline and Bounding: The History and Sociology of Science as Seen Through the Externalism-Internalism Debate," *History of Science,* v. 30 n. 4, 333-369.

[16] Steven Shapin, 1993. "Essay Review, Personal Development and Intellectual Biography: The Case of Robert Boyle," *British Journal for the History of Science,* v. 26, 335-345.

summarized by Mordechai Feingold in an unsparing critical review, Shapin's argument is that the scientific revolution came about at a time when traditional epistemic authorities, and the authority of direct experience, had both been recognized as inadequate and unreliable as sources of knowledge. At this time, according to Feingold, Shapin argued that,

> ...scientific discourse was refashioned along the model of civil conversation--where too-assertive claims, contradictions, and disputes were scrupulously avoided--in order to safeguard the cohesiveness of the community and protect the knowledge it produced. The immediate result was that the unswerving pursuit of truth was dispensed with as too indecorous, and in its place a temperate probabilism, as well as a markedly lower threshold of verity, was instituted (Feingold, 1997, 131).[17]

In 1996, Shapin brought out what is in one sense his broadest macronarrative to date, a book whose subject is the Scientific Revolution in general. In this book he deemphasizes the climactic historical nature of the "revolution" in favor of a "critical synthesis" in which the "heterogeneity, and ... contested status, of natural knowledge in the seventeenth century" (Shapin, 1996, 162) are foregrounded in a historically and socially complex tale told for the nonspecialist. By addressing his arguments to a popular audience, Shapin may have deflected scholarly criticism, since he is a bit vague as to *who* actually thinks *what* about the scientific revolution: he cites references only in a subject-oriented "Bibliographic Essay;" there are no *bibliographic* footnotes or endnotes. For example, Shapin writes broadly of "some recent historical work [that] has claimed that the seventeenth century, and especially the English setting, witnessed remarkable innovations in the modes of identifying, securing, validating, organizing, and communicating *experience*," and that he wants his "survey to reflect the significance of those claims." Since Shapin is one of the foremost claimants of this position, this is not surprising to the scholar, but for the general reader this reference carries an air of objectivity that could be misleading (Shapin, 1996, 12).[18]

At the same time, Shapin's appreciation of the debates in this area of scholarship is clear, and he has incorporated many of them to one degree or another, not only in the extended "Bibliographic Essay" but elsewhere. For example, Shapin acknowledges at one point that there are diverse national histories of "the" scientific revolution, and that the complex history of intellectual and

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[17] We will see additional views of Feingold on Shapin in the following chapter.

[18] Not that Shapin isn't aware of what he is doing: "To enable this book to address a general readership most effectively, and to make the exposition flow as smoothly as possible, I chose--with some concern about setting aside conventions traditionally observed in works by specialist scholars written for other specialists--not to burden the text with dense citations of relevant secondary literature" (Shapin, 1996, *Acknowledgements*, xiii).

social currents and counter-currents make any generalizations about "revolutions" difficult. But though Shapin admits that his "interpretative generalizations" are to some degree based on arbitrary selections of evidence, he justifies it as the price to be paid for the "overall understanding" and (once again) the audience at which he aims:

> Stories of endless complexity, endlessly qualified, hedged about with modifications and surrounded by a moat of literature citations, are unlikely to be read by any but specialists. And though such accounts can further our stock of factual knowledge about the past, they are less likely to be coherent enough to advance our overall understanding (Shapin, 1996, 11).

It is worth some thought, however, to ask whether the dichotomous choice Shapin poses here is in fact exclusive. It might be argued that some qualifications -- particularly those that cast doubt on the assertion "qualified" -- are more equal than others. This will be a theme sounded fairly often in the critical and scholarly response both to *L&AP,* and to Shapin's later work, to which I turn now.

2.3 Critical Reception and Reviews of *Leviathan and the Air-Pump*

Although reviewed widely in the major science studies journals, scholarly treatment of this work has not been limited to review literature. Other important monographs in science and technology studies[19] have defined their positions, in part, by way of interpreting Shapin and Schaffer's work. Nor have reviews appeared only in the initial one-to-five years since publication: several quite critical pieces have appeared over a dozen years afterward.[20] This would indicate that discussion is far from closed on their work; yet I think it is reasonable to summarize the range of responses to date as clustering around three positions.

There are many scholars whose response to *L&AP* has been thoroughly positive. They have gone on record that the book is important, and that its arguments are worthy of even bolder extension. These reviewers acknowledge that some historical errors may be found in the work; and that there may be gaps in its argument that are worth trying to fill. A number have suggested that a different twist on Shapin and Schaffer's interpretation would be helpful -- and would bring Shapin

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[19] E.g., Bruno Latour's *We Have Never Been Modern*; Philip Kitcher's *The Advancement of Science*; Howard Margolis' *Paradigms & Barriers*; and more critically, Levitt and Gross's *Higher Superstition*.

[20] Christopher Norris, 1997, "Why Strong Sociologists Abhor a Vacuum;" Cassandra Pinnick, 1998, "What is Wrong with the Strong Programme's Case Study of the 'Hobbes-Boyle Dispute'?"; Margaret C. Jacob, 1998, "Reflections on Bruno Latour's Version of the Seventeenth Century."

and Schaffer's arguments more in line with their own research emphases. It is almost as though the book offers a compelling surface in which these reviewers see themselves reflected -- albeit with unwanted distortions.

Among the reviewers in this cluster I would place Peter Dear, whose own extensive scholarship has stressed a literary approach, with emphasis on the importance of rhetoric in the development of early modern science. Dear has also addressed the broader European context of early modern English science. Another in this group would be Mordechai Feingold, who in his 1991 review found the book "a major contribution," though lacking in analysis of the mathematical debates between Hobbes and the Royal Society (Feingold's own scholarly contributions had emphasized the important role of mathematics, particularly inside the English universities).[21] The book, Feingold wrote, "[is] important" and

> clearly demonstrates, the seventeenth-century debate between Robert Boyle and Thomas Hobbes resulted in the creation of a most powerful image of what constitutes consensus within a scientific community.... I missed a discussion of the other dimension of the debate between Hobbes and the Royal Society, the one over mathematics. .. [Stylistic] cavils in no way lessen my respect for the major contribution this book makes to our understanding of the genesis of the 'new science' of the seventeenth century" (Feingold 1991, 187-188).

Scholars more squarely in the social constructivist camp -- though with their own special emphases -- have found much to praise in Shapin and Schaffer's work. Among these Bruno Latour figures prominently; Latour devoted considerable attention to *Leviathan and the Air-Pump* in his own 1993 work (*We Have Never Been Modern*) as well as responding to it in a 1990 review and a 1993 interview.[22] Though acknowledging some areas of disagreement with Shapin and Schaffer, including disagreement over Latour's interpretation of their work ("some say it is a case

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[21] This juxtaposition is ironic of course, since Dear and Feingold (and Shapin) exchanged heated remarks later in the decade in the wake of Feingold's review of Shapin's 1994 *A Social History of Truth*, with Dear coming to Shapin's defense as soon as he was no longer obliged, as review editor for *Isis*, to behave neutrally. Feingold's quite severe judgment of Shapin's 1994 work make his placement in this first group surprising, but not wholly so when one realizes the importance Feingold places on the development of "community" in his own history of the period. Also, the 1994 review reflects methodological concerns of an historian about the work of a sociologist, whereas the 1985 work by Shapin and Schaffer was a collaboration of an historian and a sociologist.

[22] Shapin and Schaffer themselves both authored substantive reviews of Latour's work. See Schaffer 1991, "The Eighteenth Brumaire of Bruno Latour," reviewing Latour's *The Pasteurization of France*; and Shapin 1988, "Following Scientists Around," a review of Latour's 1987 *Science in Action*. Schaffer is sharply critical, accusing Latour of promoting the very errors of which he accuses others. Shapin is somewhat more generous, acknowledging that "Latour's work gives students of science and technology major resources to extend their interpretative projects" even while arguing against Latour's advocacy of "radically restricted" use of those tools (Shapin 1988d, 547-548).

of kidnapping!") Latour is one of their strongest advocates: "I think *Leviathan and the Air-Pump* is the most important book published about the relations of science and society today ..... Their book is important because it shows the history of power as well as of science" and also as "a critique of the notion of critical theory....denouncing and unveiling is [not] the only work to do" (interview by Crawford 1993, 280-281). It will be important to look more closely at these and similar scholarly responses, and I will do so below.

In addition to these "engaged" and, on balance, positive professional reviews within the science studies literature, Shapin and Schaffer's book also received excellent reviews from publications with large audiences (the *Times Literary Supplement, Science, American Historical Review*). In contrast to the first group, these reviews outside the STS journals tended to keep some distance from the arguments. While reviewer complaints did not raise doubts as to the quality of the scholarship, they did ask whether the handling of evidence was balanced, or whether problematic questions were raised and left unresolved. Thus a second cluster of responses emerged that, though positive, suggested that the argument left room for improvement. They did not see a convincing case made for new research programmes or methods; nor did they claim Shapin and Schaffer's work represented any sort of danger to scholarly standards or the epistemic status of science. Within this cluster I would count reviews by I. Bernard Cohen (1987), Charles Webster (1986), and Thomas Hankins (1986).

"Their scholarship is difficult to fault," wrote Webster, but, he noted, there was "too much Boyle, too little Hobbes" and the rhetoric was "over-didactic, repetitive, and prolix." Hankins found "much food for thought" but the terminology was "confusing" and (again) the displacement of discussion of Hobbes's philosophy to the end of the book was inappropriate -- it would have been more effective near the beginning. Cohen found it "rich ... meticulous" but observed that Shapin and Schaffer's general thesis was more convincing than their contention that the histories of science and politics were, in this instance, as closely allied as they claimed.

Finally, a third set of scholarly responses is not only critical but agitated and even offended by Shapin and Schaffer's method, argument, quality, implications, or conclusions. The points of attack are varied: some contend the work is bad sociology; some that it is misleadingly incorrect history; and others that it is representative of a widespread and insidious failure in science studies to understand the nature and activities of science.

In this third group, on the milder end is Daniel Garber (1995), who acknowledges the work as "important" but disputes fairly strongly what he takes as Shapin and Schaffer's claim 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." It is this point, also, that makes Shapin and Schaffer's work a target in Paul Gross and Norman Levitt's critique of constructivist and post-modern science and technology studies. Though acknowledging that *Leviathan and the Air-Pump* was "exhaustively and meticulously researched" Gross and Levitt add that "the ideological perspectives of its authors make it an exercise in tunnel vision." They suggest another view of the history according to which Hobbes was excluded from the Royal Society not for his politics but for his "grotesque failures as a would-be geometer," a "mathematical dilettante and bumbler [who] simply does not belong in the same pantheon with Descartes, Huygens, Newton, Leibniz, and Bernoulli. His misadventures are tiresome, and, in the last analysis, uninstructive." They accuse Shapin and Schaffer of ignoring the "existence of sound, objective reasons for deciding at least some scientific controversies," [23] including the controversy over whether a vacuum could exist.

An even harsher blast lands late in the game, from Christopher Norris in 1997. Norris takes Shapin and Schaffer's 1985 book as the archetype of the "strong" programme in sociology.[24] Norris notes approvingly the "great scholarly erudition" as well as consistency and vigor of Shapin and Schaffer's work, but finds their contention that "things might have gone otherwise" particularly objectionable given the offensiveness of Hobbes's political absolutism. "Hobbes's approach had nothing to commend it," Norris asserts, "whether as a purported contribution to science or as a means of securing that common peace which is mankind's chief concern. Such has indeed been the verdict of most commentators since," Norris adds, suggesting that the offense lies partly in resurrecting Hobbes's long-dead intellectual corpse. Shapin and Schaffer, despite their profession of "symmetry," are guilty of Hobbesian bias with an overlay of Foucault. For Norris they also represent the problems of the anti-realist view of science, and, more generally, the relative poverty of sociological knowledge compared with physical science.

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[23] Gross and Levitt 1994, *Higher Superstition*, 63-69.

[24] This target-shooting is already a touch anachronistic in the sense that the "strong programme" is best identified with work done by the Edinburgh school a good ten or twenty years ago, i.e. even before Shapin and Schaffer's 1985 book. For Norris to refer in 1997 to the "strong programme" as "burgeoning" would have been more accurate even in the 1980's, but is not so appropriate in 1997 (Norris, 1997, 10, fn. 2).

Norris' charges are made from the vantage of political philosophy; several historians of science have attacked Shapin and Schaffer's book from the perspective of historical expertise, among them the Boyle scholar and seventeenth-century historian Rose-Mary Sargent; and philosopher-historian Cassandra Pinnick. Sargent, who develops an alternative explanatory thesis relating the history of English common law to the development of early modern science in England, criticizes Shapin and Schaffer's book for "its oversimplification of the issues with which the experimentalists and the lawyers of 17th-century England were concerned." Pinnick is more strident: Shapin and Schaffer's historical account is "wrong," and "the failure of philosophers of science to engage the historical basis of the Shapin and Schaffer case study has allowed flawed history and historiographical technique to assume iconic proportions, in science studies and in philosophy....It's not just the sociology [in the book] that belongs in the dust bin. It's the historical goods too." Among this third group of critics, one gathers a distinct sense of being informed that the emperors have no clothes. The air of resentment creeps in with arguments that Shapin and Schaffer have attempted to pull the historical, scientific, rhetorical, or sociological wool over one's eyes -- and (pace Pickering) a sense of frustration at the complications of sorting out these thoroughly *mangled* strands of evidence.

Certainly, whatever the other effects of Shapin and Schaffer's work, they struck a number of chords among science studies scholars. One cannot, probably, attribute the contemporary flourishing of seventeenth-century history of science to them, but their work has surely added the stimulus of provocative rhetoric and argument that have contributed to a revival of biographical and historical studies of Boyle, the Royal Society and its associates, and (to a lesser extent) of Hobbes. Thus the scholarly context and influence of *Leviathan and the Air-Pump* must include not only theoretical and methodological disputes among various approaches to the study of science, but also an increasingly rich and detailed scholarly record concerning the many historical issues implicit or explicit in their book: from the grand questions of the precedents and causes of the seventeenth-century "scientific revolution," to a range of special but crucial questions. These include the nature of Hobbes's scientific and mathematical contributions; the details of early modern experimental practice; the histories of other members of the Royal Society; Robert Boyle's alchemical and theological practices and beliefs; the relations between Hobbes and the Royal Society at large; and the roles played by the universities, scholarly societies, and other social institutions and practices in the development of the "new science."

2.4 Competing Local Views: Hobbes, Boyle, and the Royal Society

It will be helpful to have a sense of the broader scholarly landscape into which, in the last ten years, Shapin and Schaffer's work has come to settle. Seventeenth-century British history has for many decades occupied an important place in the history of modern science; while it is important to recall this strong tradition and background, I will emphasize the work done in these subjects by Shapin and Schaffer's contemporary cohort of seventeenth-century historians (as well as sociologists and philosophers) of science. This will keep my arguments focused on the role and impact of Shapin and Schaffer's work within contemporary science studies.

HOBBES

Turning first to the subject of Shapin and Schaffer's "principals," Hobbes and Boyle: Hobbes has continued to attract much wider scholarly attention for his political philosophy than for his work in science or mathematics. The judgment of history (as noted by Norris, above), has been difficult to dislodge. It is hard to dissuade those relatively unfamiliar with Hobbes that he was not an authoritarian apologist for absolute monarchy, an atheist -- and, of course, a scientific and mathematical crank. Fortunately Hobbes continues to attract new scholarship, and contemporary writing on Hobbes demonstrates the potential for rich and perhaps novel interpretations of Hobbes's life and thought.

Notable among recent scholars who have concerned themselves with Hobbes' scientific and mathematical thought are Hardy Grant, Douglas Jesseph, Noel Malcolm, Jan Prins, G. A. J. Rogers, Tom Sorell, and Richard Tuck. In addition, there is a steady stream of work relating more closely to Hobbes' s political philosophy and to the rhetoric, interpretation, theology, and natural philosophy of what is traditionally counted as Hobbes's most important work, *Leviathan*. In addition, a number of scholars have addressed and even tried to explain the remarkably lasting ambivalence that Hobbes has inspired. G. A. J. Rogers has noted that Hobbes "bears comparison with the enormous impact of Descartes," yet that Hobbes "has always been controversial....It was not....easy to acknowledge debts to Hobbes, and much safer to attack him" (Rogers, 1988, 3). Among others, Grant, Jesseph, and Prins have contributed work that offers new insight into the quite seriously interesting if sometimes idiosyncratic mathematical and scientific ideas that Hobbes offered. And while these ideas are often thought to be wholly idiosyncratic (and further evidence of Hobbes's isolation from the benefits of a scientific community), recent scholarship

has helped point out the significant influences of major continental figures, including Galileo and Mersenne, on the younger Hobbes's scientific philosophy (D. W. Hanson, 1990). Hanson places Hobbes within a vital skeptical philosophical tradition;[25] coupled with a vision of Hobbes's vigorous constructivist political and moral philosophy,[26] this perspective provides a very different view indeed from the received one of Hobbes.

Noel Malcolm, who later edited a collection of Hobbes's correspondence, further suggests that Hobbes's reputation was deliberately distorted by his own contemporaries, with lasting consequences. "Hobbes was becoming an increasingly disreputable figure," writes Malcolm about Hobbes just before the establishment of the Royal Society, "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" (Malcolm, 1988, 60). Those closest to Hobbes, says Malcolm, were the "nervous" publicists of the Royal Society. That the "Monster of Malmesbury" of late seventeenth-century England still carries the burden of his contemporaries' fears, is an interesting point that helps get at both what was surfaced, and what remains unaddressed, in Shapin and Schaffer's revival of the Hobbes-Boyle disputes.

BOYLE

As for Robert Boyle, he has been a familiar figure on the landscape of the history of science for some time. Never quite cutting the commanding figure of a Galileo or a Newton, Boyle has still been portrayed as a demi-god in the pantheon of early modern scientists. Over twenty years prior to the appearance of Shapin and Schaffer's book, Thomas Kuhn wrote an essay that is essentially a complex "debunking" of Boyle's role in the intellectual history of chemical theory (Kuhn, 1952). In 1977 James R. Jacob published a major new study of Boyle.[27] These works are only

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[25] See Hanson, 1990. The crucial but underappreciated role of skepticism in early modern and modern philosophy, especially as it has developed in contrast to varieties of positivism, has also been a theme of Richard Popkin's recent *Columbia History of Western Philosophy* (1998) as well as Popkin's earlier work.

[26] Surprisingly this "constructivism" is quite close in meaning to the sense of "social constructivism" in science studies. Hanson, 1990, writes: "Funkenstein remarks of Hobbes, rightly I am inclined to think, that '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.'" Hanson is citing Funkenstein (1986), *Theology and the Scientific Imagination*, 327 (Princeton: Princeton University Press).

[27] *Robert Boyle and the English Revolution: A Study in Social and Intellectual Change*. New York: Burt Franklin.

representative of an ongoing scholarly interest in both Boyle, and the Royal Society that Boyle championed. But what emerges from this pre-Shapin and Schaffer period of scholarship is the image of Boyle as a skilled political figure, exercising strong intellectual leadership in fashioning an entirely new model for the conduct and understanding of science. Jacob's analysis, for example, emphasized the political contingencies, in particular Boyle's concern over sectarian movements, that shaped Boyle's natural philosophy.

A 1994 volume edited by historian Michael Hunter, sought to substantially revise the understanding of Boyle that Jacob and others had offered. In particular, current research has emphasized Boyle's interests and activities that place him far outside the role of experimentalist "adept": Boyle's romantic early literary works; his interest in, and practice, of alchemy; and his "overriding concerns with personal virtue and piety" underscore what scholars both agree about and usually fail to emphasize: that Boyle's primary identification during his lifetime was as a Christian.

Hunter has made controversial claims regarding Boyle's ambivalent and even "dysfunctional" personality -- based in large part on Boyle's literary traits. Several authors in the Hunter volume take issue with Shapin and Schaffer's characterization of Boyle -- not only Hunter, but also Malcolm Oster and Rose-Mary Sargent. Sargent, who in 1995 published her own monograph on Boyle (*The Diffident Naturalist*), objects to Shapin and Schaffer's representation of Boyle as motivated by externalities, seeking "to reinstate Boyle as a sophisticated writer on the problems of the experimental method" (Golinski, 1997, 213). In an essay review of this volume, Golinski concludes that,

> The vision of Boyle as a philosopher and theologian that emerges from these papers is a fairly consistent one: he appears as a critic of Cartesianism from within the general ambit of the mechanical philosophy. He sees human reason as limited in its ability to reduce phenomena to mechanical explanations; he insists that final causes are to be accorded a greater importance in natural philosophy than Descartes seemed willing to allow; and he specifies that miraculous, entirely non-mechanical, events do occur in nature (Golinski, 1997, 216).

The most recent monograph to be published on Boyle is Lawrence Principe's *The Aspiring Adept* (1998), which follows Principe's earlier research of Boyle's early literary work. Principe's revision of the standard account of Boyle emphasizes the extent to which Boyle was involved with the relatively closed world of alchemy, in parallel with his highly public and publicized work with the new science and its experimental method. The issue of Boyle's alchemical work has also been addressed in Hunter and Newman: in the Hunter volume, Golinksi writes,

"reversing the terms of the standard argument -- that Boyle used corpuscular mechanism to break with alchemy -- [Newman] suggests that Boyle's corpuscularianism is in fact alchemical, rather than mechanical, in its origins" (Golinksi, 1997, 214).[28]

Hunter, who in the last two decades has undertaken extensive and admired archival and editorial work both with Boyle's papers and with those of the Royal Society, has also addressed Boyle's "conversion" to modern science. His very careful textual analysis (1995, "How Boyle Became a Scientist") suggests an important discontinuity between the early Boyle and Boyle the scientist. Hunter's speculation is that, at a pivotal period in Boyle's life, during which he experienced long periods of illness, Boyle became exposed to an unusually wide range of scholarly works. These, Hunter notes, may have strongly suggested to Boyle the hitherto unrealized intellectual complexity of many philosophical issues. At this time Boyle also experienced his first success in creating a home laboratory. This combination of experiences seemed, Hunter claims, to convince Boyle in a very vivid way of the importance of experiment as a corrective and check on excessive rationalism.

THE ROYAL SOCIETY

In many ways Shapin and Schaffer allow Boyle to stand for and represent the Royal Society -- but it is useful to note that there is a substantial body of historical research concerning the Society itself and its other principals. Among recent scholars whose work has emphasized the Royal Society are Peter Dear, Mordechai Feingold, Daniel Garber, Michael Hunter, and the many scholars who have focused on other important figures in the Royal Society: correspondent Thomas Sprat; Christiaan Huygens; the great experimenter, Robert Hooke; and John Wallis. This literature is far too vast and complex for me to do justice to it here; but a few notes will help demonstrate that when Shapin and Schaffer wrote, they were entering a context already rich with historical, philosophical, and sociological work.

Peter Dear, whose approach to this history is in many ways close to Shapin and Schaffer's, has placed greater emphasis on the literary and rhetorical aspects of the Society's activities, and less on experiment and place. In a major 1985 article, Dear argues that "the style of science espoused by the Fellows of the Royal Society was more important than the substance of that

[28] This revelation of deep involvement with alchemical investigations has also been made about Newton. Modern historians are now assimilating the fact that the greatest heroes of early modern science cannot be understood without acknowledging their roots in medieval science, philosophy, and theology.

science." Dear also suggests that the specificities of the English context as presented by Shapin and Schaffer, and particularly Schaffer's emphasis on "matters of fact," are valuable; but that "matters of fact" were not an invention of the Royal Society; rather:

> What Schaffer calls the 'matter of fact' was, we have seen, constituted by a literary form which simultaneously redefined the role of experience in natural philosophy and provided a new basis for authority. That the Royal Society could itself exploit this redefinition of authority is suggested by Schaffer's remarks, but to understand its genesis requires a broader perspective (1985, 160).

Dear's work on the Royal Society follows on an important, and well-known earlier literature -- not only Merton's influential work on seventeenth-century English science, politics, and religion; but also Dorothy Stimson's 1948 *History of the Royal Society*; James R. Jacob's 1977 work on Boyle, mentioned above; and Hunter's 1982 monograph on the Royal Society.

Following the publication of Shapin and Schaffer's book, a number of scholars have addressed the history of the idea of the Royal Society and its relationship with the pragmatic Baconian ideal. In 1995 Daniel Garber provided an analysis contrasting Bacon's fictional "House of Solomon," with the Royal Society. Garber's understanding appears very close to Shapin and Schaffer's, in that he regards Bacon's, as well as Descartes', notions of experimental facthood as "radically individualistic." In contrast, he says, the early Royal Society held a "self-consciously social conception of experimental facthood," which could only occur at this historical moment because it was only in the latter half of the seventeenth century that a critical mass of expertise: "a community entitled to make judgments necessary to establish such facts" -- first emerged. Garber differs with some previous writers (e.g. Shapiro 1983) in claiming that the Royal Society's social philosophy of science did not follow from, but rather reversed, Bacon's:

> [In the Royal Society] we ... have an important transformation in Bacon's conception of the scientific community. Sprat's appeal to community is a repudiation of Bacon's conception of experimental science. Bacon saw experimental philosophy as a cooperative venture. But for Bacon, the main advantage of the numerous investigators working together is that more facts can be collected for one's natural history and consequences derived more expeditiously.... (Garber, 1995, 195).

On the other hand, Garber also argues against the idea he believes Shapin and Schaffer have infused with this history, namely 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" (Garber, 1995, 202). This view, Garber continues,

> ...has some historical limitations. One might take it for an almost a priori truth: 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....But if the sociological claim is taken to be a thesis with real content and relevance for the historian of philosophy and science, then I think that, at best, it can only be an account that holds [since the 17th c]....But, even when ...suitably historicized, I have my doubts. The thesis that the world of facts established by science is simply a matter of social agreement has an obvious deflationary consequence....It would be a great irony if the social criterion of experimental facthood that, in a sense, marks the beginning of modern experimental science also marks the beginning of its demise (Garber, 1995, 203).

2.5 Competing Macronarratives

A great many eminent scholars of science and technology have, at one point or another in their careers, devoted attention to some early modern subject. Thus Shapin and Schaffer join a long line and excellent company in making their own contributions to the study of this period. Although it is clear from the sections above that research on the English contributions and activities in early modern science is itself a small scholarly industry, it is important to sketch the broad outlines of work that has been done and that continues to grow in a variety of directions. At the very least, this will provide a more concrete sense that in choosing to locate the origins of modern science in the Hobbes-Boyle debate, Shapin and Schaffer have selected one set of evidence, and one interpretation among many. Their black box, however tightly constructed, may begin to look quite different in the context offered by the complex social, political, and intellectual architecture of their peers' work.

Whether in the history, sociology, or philosophy of science, the early modern period in Europe has been a fertile subject. The notion that there was a critical historical point at which what we refer to as "science" emerged --a "scientific revolution" -- and that it occurred in Europe during the sixteenth and seventeenth centuries -- has been a core one in the study of science. The fact that the very idea of a "scientific revolution" is itself disputed only feeds the tremendous intellectual energy that has been devoted to understanding the many facets of this period.

A number of alternative ideas have emerged as important to understanding and contrasting the specifically English side of this history. One of these has been the emergence and role of various forms of scholarly association and organization throughout Europe during this period. And one of the most important contributions in this area was Roger Hahn's 1971 study of the Académie Royale des Sciences, which addresses many of the themes taken up a decade later by Shapin and

Schaffer. Hahn suggested that, in contrast to earlier studies focused on the history of scientific ideas, there was growing realization that, "....wherever the spirit of scientific inquiry penetrated, new patterns of social organization sprang up." Though no one could accuse Hahn of social constructivism, this work, like others around the same time, acknowledged "that the relationship of thought to social organization was in fact considerably more complex [than had been recognized]. .... [T]he organizational revolution must now be considered an essential component of the Scientific Revolution rather than its by-product. Their relationship was concurrent and symbiotic rather than sequential" (Hahn, 1971, 1). One of the other interesting things about this work is the evidence it presents about the socialization that existed within -- and which helped to define -- these early scientific institutions. It would be easy to mistake the following for Shapin in the 1990's, not Hahn twenty years earlier:

> There was no longer room for the dilettante, the virtuoso, the eccentric, the undisciplined mind. Ostentation, vanity, and pomposity were to be replaced by earnest unaffectedness. Simplicity, hard work, and unadorned logic were to characterize the new academician.....the Academy was establishing behavioral norms that actually defined the scientific community (Hahn, 1971, 31).

But this is France, not England. The international quality of this socialization is not addressed by Shapin and Schaffer in their local study of the Royal Society and its social mores.

In addition to other earlier works on European scientific societies, another historical account published in the same year as *L&AP* offers a sweeping view of the development of early scientific organizations, though with greater emphasis on the eighteenth century. James McClellan's *Science Reorganized* (1985) acknowledges that "the history of organized science in all its dimensions begins in antiquity," and notes that the "dominion of the scientific societies in the eighteenth century....represents a distinct historical stage, *following on the Scientific Revolution*..." (McClellan, 1985, xviii-xix, emphasis added). Further, McClellan notes that while scientific societies predominated over other modes of organization, those modes were diverse and important, embracing informal social organizations as well as observatories, botanical gardens, the journals associated with these organizations, and at least some universities. Though it is not his purpose, one might even detect in McClellan's far-ranging account of the increasingly pervasive formal scientific organization, multiplication of specialized scientific and organizational roles, and internalization of organizational norms, the "capillary" reach of modernism as Foucault has represented it.

A close cousin of the emerging scientific community was the Republic of Letters. Lorraine Daston, and more recently Anne Goldgar, have written extensively on the Republic of Letters in

continental Europe. It is Goldgar's contention that to a large degree the intellectual products and activities of this informal, extended community were of secondary importance *to its participants*, compared with its purely social function. Not only, says Goldgar, was the Republic of Letters constituted primarily through *informal* institutions -- "quarrels, gossip, dinners, lending of books and sharing of information," but, she maintains,

> ...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, 1995, 6).

Goldgar goes on to acknowledge that "there seems to be something of a *Zeitgeist* in historical thinking about the importance of civility to academia. The ideas for [Goldgar's] 1990 thesis .... were developed in the archives, but some of them have been arrived at independently by other scholars" -- among them, of course being Shapin, whose 1994 book centers on civility in the culture of early modern science (Goldgar, 1995, n. 28, 253).

Other themes of importance in the study of science in this period have been the relationship between developments in mathematics, and the emerging experimental or "new" science. This relationship is presented somewhat thinly by Shapin and Schaffer, with an implicit opposition represented by the mathematically-minded Hobbes on one side and the arch-experimentalist Boyle on the other. Presumably they base this opposition on the close association, at the time, between the study of mathematics and the Schools and universities, which might be plausibly associated with Hobbes's mathematical approach. If mathematics and experiment truly occupied opposing camps, then Shapin and Schaffer would be correct perhaps in presenting the English universities and the Royal Society as standing in opposition to each other.

But it is not clear that this would be correct; other scholars have in fact disputed this: Peter Dear, for one (who has also written about Mersenne -- someone strongly connected intellectually with the younger Hobbes), has written about "the mathematical way in the scientific revolution," suggesting that the Boylean version of early modern science was not "the high road to modern experimentalism; it was a detour. .... [T]he event-centered empiricism of the early Fellows of the Royal Society stood somewhat at odds with the dominant modes of experience in contemporary philosophy of nature" (Dear, 1995, 3, 8). Further, Dear argues that this reinstates the universities squarely at the center of the scientific revolution. "[P]hysical science in the seventeenth century ...is rooted in a milieu of academic scholarly endeavor" (Dear, 1995, 9).

Just as Dear weaves together the issues of mathematical philosophy and academic training, Mordechai Feingold has done the same in his 1984 study of English universities during the Restoration. Feingold claims that the gap between the major scientific figures and "unenlightened science" as carried out in the universities was not as large as has been thought, and that many "new" scientists were far more embedded in the academy than has been acknowledged. All parts of the contemporary scientific community should be accounted for by historians, he argues, and not merely those outside the universities (Feingold, 1984, 7, 21). The universities too should be counted as contributing to a deep-rooted and long-standing set of intellectual developments that led to the scientific revolution.

In the last ten or fifteen years, literature on the role of mathematics, measurement, and number in early modern science has included books by Theodore Porter (1995), who has explored the relationship between mathematics and ideas of objectivity; by Karl Crosby (1997), who has written about quantification in the Western tradition prior to the Scientific Revolution; and by Timothy Reiss (1997), who has analyzed the shift in emphasis of scholarly training from grammatical to mathematical logic; and the strong relationship between the impact of mathematical thinking in the arts and in natural science. Related to this literature is the extensive work by Shapin and Schaffer's contemporaries in the philosophy and epistemology of the early modern period, including close study of early ideas concerning probability (e.g. Shapiro, 1983).

Finally, a strong showing of biographical and individual studies in early modern science has provided increasingly rich and often surprisingly complex new views of some of the most familiar figures in the scientific revolution -- widely ranging and important studies on Newton, Galileo, and Descartes, as well as new work on Brahe, Leibniz, Bruno, Bacon, and others. In addition to contributing new perspectives on the intellectual and personal lives of these individuals, a number of scholars have emphasized the significance of their social roles, including the important role of patronage in shaping those biographical, sociocultural, and scientific histories; Richard Westfall's 1985 article and Mario Biagioli's 1993 monograph about Galileo are among the well-known accounts of scientific patronage.

In summary, Shapin and Schaffer's tale of Boyle and Hobbes has settled into a complex and restless scholarly landscape. Their most sweeping thesis is that "knowledge, as much as the state, is the product of human actions." But this claim caps a detailed argument based on many particulars. Their scholarly "house" rests on the equipoise of many strands of argument and

evidence that collectively justify their contention that modern scientific knowledge is the product of an ideal Restoration community, invented by Boyle and his contemporaries.

My task in the following chapter will therefore be to parse Shapin and Schaffer's argument carefully: to identify each of the main strands of evidence and argument that they have woven together, and to address each in turn. This strategy will, I trust, make their broad arguments more tractable to scrutiny; but by keeping in mind how these particulars depend on each other, I will hope also to keep my concerns from being reduced to a critique of particulars.