SCIENCE AND SOCIETY: STEVEN SHAPIN'S SOCIOLOGICAL INTERPRETATIONS OF SCIENCE

ALEXANDER DALZIEL

SCIENCE AND SOCIETY:
STEVEN SHAPIN'S SOCIOLOGICAL INTERPRETATIONS OF SCIENCE

by

Alexander Dalziel

A thesis submitted to the

School of Graduate Studies

in partial fulfilment of the

requirements for the degree of

Master of Arts

Department of History, Faculty of Arts

Memorial University of Newfoundland

July, 2005

St. John's Newfoundland

Abstract

In his books Leviathan and the Air-Pump (1985) and A Social History of Truth (1994), Steven Shapin employed sociological interpretations of scientific knowledge. These books examined the embryonic scientific community of seventeenth-century England, particularly the role of Robert Boyle (1627-1691). Despite the constancy of his dedication to sociological tools, these two books display considerable differences in how society is used to interpret science. In Leviathan and the Air-Pump, Shapin and co-author Simon Schaffer contended that social tension and strife defined the development of science. In particular, they highlighted the role contemporary social and political struggles played in sparking controversy between the natural philosophies of Robert Boyle and Thomas Hobbes (1588-1679). In A Social History of Truth, Shapin argued that social factors such as credibility and trust played a fundamental role in natural science. Boyle assembled a strategy for establishing credibility using the tools that his local English and European society and culture provided him. This thesis will contend that, despite its many insights, Shapin's sociological agenda overreaches itself, and requires various philosophical and historical considerations to shore up its historiographical standing.

Acknowledgements

First, to Dr. Stuart Pierson - inspiration, example, mentor. I am still only starting to understand the depth and sweep of his erudition and appreciation of the intellect. Next, to Tina for constant support, patience and confidence. To my Mother, without whom I never would have embarked on such a quest. To Dr. Peter Trnka, for taking over half-way very admirably, and showing great fortitude, patience and insight in seeing this project through to completion. My thanks to Dr. Christopher English, for encouragement and keeping the history in a history thesis. To Dr. William Copeland, for encouragement. Thanks to Dr. Christopher Youé, for laughter and ready support. Kurt Korneski, for conversation along the way. To Fran Warren, my appreciation for fielding my many requests and making distance less of an issue. My gratitude to all those others in the History Department at Memorial University for their contributions, both great and small. To Deborah Inkpen and Tracy Dool, thanks for encouragement and flexibility. To Dr. Darren Hynes and Dr. Bernard Lightman, thanks for helpful and penetrating criticisms and suggestions. And finally, thanks to Steven Shapin, for a true and satisfying intellectual odyssey, and for prompt responses to my queries.

Table of Contents

Chapter 1
Introduction: Steven Shapin and the Sociological Historiography of Science

> In science convictions have no rights of citizenship . . . . Only when they decide to descend to the modesty of hypotheses, of a provisional experimental point of view, of a regulative fiction, they may be granted admission and even a certain value in the realm of knowledge . . . . But does this mean . . . that a conviction may obtain admission to science only when it ceases to be a conviction? Would it not be the first step in the discipline of the scientific spirit that one would not permit oneself any more convictions?
>
> Probably this is so; only we still have to ask: *to make it possible for this discipline to begin*, must there not be some prior conviction - even one that is so commanding and unconditional that it sacrifices all other convictions to itself? We see that science also rests on a faith; there simply is no science "without presuppositions."
>
> Friedrich Nietzsche, The Gay Science (1887).

The social history of science is an important component of contemporary historiography of science. It attempts to document and interpret the history of science by turning to the world of human interaction - society and culture - to explain why it is that scientists explain the natural world the way they do. As one might expect, historians of science who forward such interpretations seek ways to connect often abstract scientific concepts to various elements of the fluid social and cultural world.

Variety abounds in the historiography of science. Historians have advanced many different theses to explain the historical development of science. What makes the socially oriented historiographical approach so interesting is the manifold ties it exhibits with other bodies of scholarly and academic work, including sociology and philosophy. The body of work composed by historian and sociologist of science Steven Shapin is an excellent example of this phenomenon. Critical examination of two of Shapin's major scholarly accomplishments, Leviathan and the Air-Pump (LAP) and A Social History of

Truth (AST), published in 1985 and 1994 respectively, provides an excellent opportunity for the student to see both the strengths and weaknesses attendant to such a method.

What such critical examination reveals is that Shapin has shown a continuing dedication to employing sociological interpretations of episodes of science's history; in the case of the two books to be explored in depth in this thesis, episodes centring on the life and work of Robert Boyle (1627-1691). However, this dedication has changed over time. In LAP, Shapin used sociological tools that highlighted the role of social strife and tension in establishing scientific knowledge. More particularly, this social strife and tension was based on a sociology of group interaction and competition, where the economic, political and social resources of a social group came together to establish and promote a specific collection of scientific claims, methods and practices. Despite the fact that Shapin continued to believe that such economic, political and social foundations grounded scientific claims in AST, he retracted the sharp social conflict between groups in favour of an escalated importance for social cooperation in bringing about scientific knowledge. The defining features of this social cooperation were trust and credibility. Thus, the student of Shapin finds models of social tension versus social trust guiding Shapin's sociological interpretations of the history of science, and must work to understand the origins and consequences of Shapin's developing thought.

It is hardly novel to cite that the origins of Shapin's thought on the history of science find their immediate roots in the work of the Edinburgh Strong Programme. Its main proponents, Barry Barnes and David Bloor, articulated a series of arguments in favour of founding an understanding of science thoroughly sociological in character: all

human knowledge, including science and mathematics, could be explained in terms of the social structure of the communities whence they appeared. Another hardly novel observation is that Barnes and Bloor drew much of their immediate intellectual inspiration and stimulation from Thomas Kuhn's The Structure of Scientific Revolutions. They found a number of its concepts, such as the "paradigm," amenable to sociological reinterpretation.

What is more interesting to note is that the intellectual heritage from which Shapin emerged, whatever its stated objectives, gave great prominence to the role of history in understanding scientific knowledge. Kuhn explicitly tried to show the utility of a historical perspective in expanding the horizons of philosophy of science; the Strong Programme's insistence on the universal presence of social factors in scientific knowledge opened epistemic terrain for historical exploration, in many ways making history of science a specialized component of social history. These influences are omnipresent in Shapin's oeuvre. History of science no longer would be just a catalogue of scientific discoveries and achievements combined with interesting anecdotes about scientists, but would be able to contribute real insights into science's claims about the natural universe. In the hands of the Strong Programme and Shapin, the transient, historically describable elements of society gain precedence over the seemingly eternal, philosophically depicted roles of reason, objectivity and mathematics in science. For the Strong Programme and Shapin, *sociologized* history of science replaced *philosophized* history of science, with sociological categories of interpretation largely displacing

philosophically articulated concepts such as rationality, objectivity and other vaunted epistemological elements of traditional philosophy of science.

Over the course of the thesis, my critical argument will be that this goes too far, and that Shapin's claims regarding the social dependency of epistemic claims need to be moderated. To this end, the notions of "ontological equivalence" and "epistemological sensitivity" will be articulated. These ideas are not antithetical to a sociological approach, but show that the topic matter of scientific investigation, whether it be the celestial location of the planets or the chemical makeup of a flower, contain irreducible epistemological components which universally play a role in determining the knowledge-to-society relationship. Such universal components may be differently articulated or differently handled in different human communities. In anticipation, let it suffice to say that some knowledge claims, for instance those relating to matter theory, may be more dependent on the sociological structure of the scientific community than others, for instance the claims of observational astronomy. These two notions will then provide grounds for a sociologically satisfactory definition of the role of genius in science's history, described below as "intellectual awe." Audiences in all historical circumstances have the potential to respect the role of intellectual acuity and power, a claim that in no way downplays either the role of reason or of society in the history of science, but shows that they are in fact compatible.

The claims I have made above go some way toward explaining the "intellectualist" approach taken in this thesis. Although initially this may strike the reader as ironic, considering such intellectualism in the history of science is partially the sort of

thing that Shapin meant to counter, there are a number of justifications for it. As has been stated above, and will be fleshed out over the course of the argument, numerous strands of thinking derived from historiographical, sociological and philosophical sources set the course for Shapin's thinking. Thus, isolating the ideas upon which Shapin plotted this course is a pragmatic and worthwhile approach for a thesis of this scope and length.

Despite the fact that this allows fewer pages for intimate interaction with the historical primary sources upon which Shapin based his work, it provides a solid critical preparation for any such future interaction by revealing the advantages and disadvantages of Shapin's approach. Additionally, some of the philosophical strengths and weaknesses of Shapin's approach are of immediate consequence to his historiographical strengths and weaknesses. In particular, a measure of philosophical awareness and argument in many ways buttresses Shapin's sociological tendencies by showing places where these tendencies take Shapin off the rails and by offering philosophical considerations for how he might get back on track.

1.1 Thesis Outline

Chapter 2 offers an interpretation of the origins of Shapin's sociological approach with three main components. First, an examination of his description of the "internalism-externalism" debate will provide insight into how Shapin reacted to the idea that rational factors were internal to the history of science whereas social factors were external. Following this will be a brief examination of the important ideas of Thomas Kuhn and the possibilities they offered the sociologists of the Edinburgh Strong Programme. Kuhnian

ideas such as "paradigm," "normal science," "incommensurability," and "revolutionary science" were tantalizingly susceptible to the sociological reinterpretation of the Edinburgh Strong Programme. Once this examination is complete, the four main components of David Bloor's Knowledge and Social Imagery will be studied as a way to introduce the Edinburgh Strong Programme in the sociology of scientific knowledge. The four tenets of the strong programme - impartiality, causality, symmetry and reflexivity - will be described and critiqued. Because culture is a sociologically defined concept in the Strong Programme's view and crucial to understanding Shapin, mention will be made of Barry Barnes' notion of science as a form of culture. Shapin's thinking took form in this milieu.

Chapter 3 will analyse the model of social competition and strife between social groups found in Leviathan and the Air Pump (LAP), a work coauthored by Simon Schaffer. Here Shapin and Schaffer offered a bold and innovative interpretation of a particular moment in the history of science emphasizing the significance of social factors. Their philosophy of history proposed that science, as a form of knowledge, be seen as a component of the politics and economics of social groups struggling for survival and superiority within society. Importantly, Shapin and Schaffer introduced what has been called an "interest model" into their history of science, which is to say they described science as driven by political motives that aimed to consolidate and/or augment the social group's economic and political power. Science, far from being objective and neutral, was another weapon in the social struggle between different social groups. Loyalty to specific knowledge claims parallelled loyalty to a specific social group, political ideology,

economic model and so on. This approach to the history of science demanded that Shapin and Schaffer closely examine the political, social and intellectual history of the period they were studying, and not limit themselves to the biography of recognized scientists.

Chapter 4 will concentrate on the analysis and critique of Shapin's model of social credibility as found in A Social History of Truth (AST). Shapin's confidence in the intimate relationship between social factors and scientific knowledge did not slacken, but in AST he moved away from a strife-based conception of scientific knowledge to an interpretation of science as based on trust. In Shapin's mind, a knowledge claim became knowledge when a myriad of personal, biographical and social factors coalesced to make the claim credible in the eyes of contemporary audiences and peers. These myriad factors governed the selection of credible knowledge claims. This approach demanded that Shapin attend closely to how scientists cooperated amongst themselves, the sort of moral interactions they had, the socially tinted nuances of their thinking, and so on.

In conclusion, Chapter 5 will contend that examining the social foundations of trust and credibility provides a more promising line of inquiry than describing the dynamics of social tension and strife. The two approaches are not mutually incompatible; Shapin's account in LAP is sufficiently convincing to reach the conclusion that social tension *can* be relevant to the historian of science, but AST is even more successful in showing that the specific nature of trust and cooperation should *always* be relevant. Science can be seen as a weapon in social struggle, but even when so employed it will only be available as such because a nexus of trust and credibility created a system of claims that could be wielded in favour of other human activities. Additionally, the trust-

credibility thesis allows a role for other factors relevant to the history of science, such as the role of the natural world itself in shaping scientific debate and discovery. Consequently, historians of science should continue to articulate how knowledge is credible, while leaving the role of social conflict for those particular historical episodes requiring it.

Chapter 2
Shaping Shapin: The Intellectual Heritage of Steven Shapin's Thought

> The sociologist is concerned with knowledge, including scientific knowledge, purely as a natural phenomenon. The appropriate definition of knowledge will therefore be rather different from that of either the layman or the philosopher. Instead of defining it as true belief - or perhaps, justified true belief - knowledge for the sociologist is whatever people take to be knowledge.
>
> David Bloor, Knowledge and Social Imagery (1976).

Steven Shapin's scholarly oeuvre emerged from an intellectual heritage that sought to make history a central component of understanding scientific knowledge. In particular, this heritage aimed to move away from the tendency to see science as a rational product of the individual human mind - identified as typical of the history and philosophy of science - to a view of science as a product of the historical interactions of groups of human beings. What resulted was a Shapin who was completely dedicated to history as a tool for understanding scientific knowledge, a history which in turn was dedicated to the use of sociological tools and perspectives for achieving its interpretations.

Understanding this influence on Shapin can be approached from a number of angles. Three components will be isolated and examined in this chapter, starting with the so-called "internalism-externalism" debate. Shapin's analysis of this topic revealed his deeply historical appreciation of science. Next, Thomas Kuhn and his book The Structure of Scientific Revolutions provided a powerful motivation for historians of science to see their work differently, and Shapin and the Strong Programme were influenced by its arguments and ideas. Thirdly, the lead proponents of the Strong Programme, David Bloor

and Barry Barnes, partially inspired by Kuhn, established a research agenda for the history of science based on identifying social factors which established scientific knowledge claims. Outlining the program of their Strong Programme, which included four tenets and a particular concept of culture, will be requisite here, with an eye to the sort of historical inquiry in which such a program might result. Appreciating the role of these three elements in Shapin's thought will prepare the way for more in-depth examination of his historical work in subsequent chapters.

2.1 Shapin on Internalism-Externalism

Shapin defined the internalism-externalism debate in the historiography of science as the struggle between a camp of historians who wished to brand rational and scientific factors as "internal" and social, political, economic and cultural factors as "external." Importantly, Shapin cited such a dichotomy as central to the general historiographical tendency of concentrating on "intellectual" and "rational" factors to the detriment of "social" factors when explaining historical episodes of science.[1] Intellectual and rational factors might include the use of mathematics to describe a natural phenomenon; the use of experiment to test the predictions of a specific theory; the invention of scientific apparatus; and so on. Social factors might include the economic wealth of the scientist; his or her religious views; the possession of academic honours and entitlements; nationality; and so on. On the surface, such a division seems natural enough: a discipline

---

[1] This section is based on Steven Shapin, "Discipline and Bounding: The History and Sociology of Science as Seen Through the Externalism-Internalism Debate," History of Science (1992): pp. 333-369.

such as biology which employs sophisticated intellectual abilities and complex technologies would seem to be more influenced by the biologist's skill in employing those abilities and technologies in the laboratory than her social status or political beliefs outside of it.

A hypothetical example will help make this dichotomy of internal and external factors clearer. Consider, for instance, a biologist in her laboratory. Assume that she follows rigorous scientific method and protocols: her measurements are accurate, her observations astute, her scientific problems well defined, the scientific apparatus she employs in working order, and so on. Additionally, imagine that she is a member of her academic department's social club, volunteers weeknights at a soup kitchen, is a card-carrying member of a political party, and is a landscape painter of some ability. For the internalist historian of science, only the first set of factors, those involving her activities in the laboratory, are significant; what she does outside of the lab may have anecdotal interest, perhaps reflecting her vigorous curiosity and sense of personal responsibility, but in the end means little to her scientific achievements. It is what happens in the lab that is key. The externalist historian of science, however, would be inclined to consider those external, outside-of-the-lab characteristics of value too. It is exactly how they are of value that must be determined.

Shapin thought that this dichotomy was correct only in a facile sense. Obviously internal factors defined in this manner would always be primary for the historian of science, for it was in science that the historian was interested, and therefore the laboratory setting would always be important. Conversely, external factors would always be

secondary, as they failed to show how the science got done - clearly, going to the soup kitchen is not looking through a microscope. Shapin labelled this an "asymmetry" in the debate, stating that "[f]ormally, something like 'pure internalism' can exist and be practised while 'pure externalism' cannot without historical contradiction." The whole structure of the debate disbarred external, "social" factors from playing a meaningful role in understanding the history of science. Notably, Shapin saw this asymmetry as a problem in the debate: I will return to it after completing my examination of his presentation of internalism and externalism.[2]

In general, Shapin identified the internalism-externalism debate as a collection of various acts of drawing "boundaries." According to Shapin, by placing differing degrees of emphasis on internal and external factors, historians were implicitly or explicitly shrinking or expanding the domains of influence on science. Here, only purely rational factors mattered; there, social factors influenced the development of scientific ideas. Historiography of science becomes an attempt to define the proper extent of various influences on the development of scientific knowledge.[3]

To provide a fuller image of science, replete with historical and social detail, Shapin took on the problem from a different angle, effectively melting the external and internal into one on the basis of two considerations. Firstly, he noted that there is "society in science." The scientific community has social structure. For Shapin, science is a form of culture, and therefore participates in society, is acted upon by society, and even

---

[2] Shapin, "Discipline and Bounding," p. 347; quote also p. 347.

[3] Shapin, "Discipline and Bounding," pp. 333-335.

operates independently as a sub-society. Therefore, society is already an internal factor, and cannot be shunted aside without historiographical consequence. Shapin formalized this conclusion by propounding his idea of "cultural items." These were elements of the contemporary cultural setting which historical agents embraced or discarded as relevant to their actions and beliefs. This positing of an influential contemporary setting opens room for history to enter fully into the study of scientific knowledge.[4]

Secondly, Shapin based his argument on "historicism." Basically, historicism in the sense Shapin used it is a historiographical tool demanding that historians see the past in terms of how the historical agents they are studying saw it. Shapin defined it this way: "I take historicism in a loose sense, as the programme dedicated to analysing historical action in historical actors' terms." For instance, if a seventeenth-century English scientist in fact saw himself as a "natural philosopher," then it is the historian's responsibility to draw out the consequences of such a self-description, and avoid enforcing a view of how contemporary scientists see themselves on the past. Perhaps the seventeenth-century natural philosopher saw his work as a glorification of God, whereas his twenty-first century progeny sees religion as detrimental to an effectively working laboratory. The onus is on the historian to delineate and defend through evidence what historical agents conceived of as proper knowledge and what means were to be employed to secure it. Shapin's historicism entails interpretational flexibility - rigid strictures on what constitutes science, past, present and future, simply do not result in accurate history of

---

[4] Shapin, "Discipline and Bounding," pp. 349-350; 352-353 for cultural "items" or "resources."

science, but enforce a vision of what the historian thinks the past should look like in light of the present.[5]

Shapin's examination of the internalism-externalism debate is characterized throughout by a robust and worthy attempt to show the importance of historical analysis in detailing science's development. A thorough appreciation of the role of social factors almost necessarily entails a powerful role for history, for social factors change with place and time, and it is the job of historians in archives and with primary sources to relate such changing factors to changes in scientific knowledge. On the other hand, the internalist camp, at least at its extreme fringes, might find historical inquiry superfluous, contending that a purely *philosophical* understanding of rationality would show what makes science knowledge. Shapin was working via social means to firmly establish history's role in understanding science.

Despite Shapin's valuable insights into the internalism-externalism debate and the solid foundations he supplied for historical inquiry into science, there is a point of concern that needs addressing as it anticipates a number of critical arguments offered later in this thesis. This is the notion of asymmetry in the internalism-externalism debate raised above. Although Shapin was surely correct to point out the asymmetry which favours the internalist side of the debate, his handling of it is suspect for the following reason.

---

[5] Shapin, "Discipline and Bounding," p. 351; quote from p. 354. The sentiment expressed in the last sentence of the paragraph is echoed, in a Marxist vein, by historian Robert M. Young in his book Darwin's Metaphor: Nature's Place in Victorian Culture (Cambridge: Cambridge University, 1985): pp. 167-171. Young expressed how many historians were dissatisfied with the "separation of 'science' from social, philosophical, and theological issues," and began to seek out alternative historiographical approaches combining these elements. Notably, Young turned mostly to Marx for such an alternative.

Consider Shapin's comment on academic ownership: "It is a fact about our present academic arrangements that historians of science own the rights to talk about Newton rather than historians of politics. . . . And it is a further fact that the history of science, as it has been, presently is, and likely will remain, is primarily interested in Newton as mathematician and natural philosopher."[6] Shapin's point is that such academic configurations tend to favour an internalist approach, and his criticism of this narrow tendancy is well warranted. However, there is good reason why a figure such as Newton falls into the hands of historians of science as opposed to historians of politics, the simple fact being that his greatest contributions were to mathematics and natural philosophy and not to politics. This rather banal observation is nonetheless of great import, for it suggests that, in his attempt to draw a meaningful role for social factors in the history of science, he lost sight of the fact that natural science is ostensibly occupied with the study of the physical universe, not with the political rule and social guidance of human communities. Because it is in *knowing* the universe that scientists immerse themselves, problems of the relationship between the knower and the known obtain. If the historian is going to take the epistemic dimension of the scientist's labours seriously, then he or she must be prepared to integrate a coherent epistemological perspective into his or her historiographical tools that takes the knower-known interaction seriously. Shapin's talk of "present academic arrangements" casts doubts on whether he saw the importance this. To put it bluntly, such talk suggests that Shapin was guilty of overplaying sociology's role in understanding science, assuming that a socially sensitive history of science would have to

---

[6] Shapin, "Discipline and Bounding," p. 347.

be sociological in character, excluding a substantive role for philosophy. Thus, later I shall introduce ideas which speak to the problem of trying to do this without erasing either rational or social factors in the history of science, notably my notions of "ontological equivalence" and "epistemological sensitivity." Shapin's perspicuous appreciation of the internalism-externalism debate is not defeated by attention to this detail. Shapin's thinking can be fused with other, more intellectualist considerations - a point that will emerge below in the discussion of the Edinburgh Strong Programme as a *particular* constellation of ideas for promoting the social history of science.

The lesson to take away from Shapin's treatment of the internalism-externalism debate is that he was trying to establish a thoroughly historical vision of scientific knowledge through establishing a historiographical role for social factors, while challenging the very concepts of the "internal" and "external." Historians such as Shapin held an ardent conviction that social factors were integral to science's historical development, and therefore sought a theoretical perspective that would adequately express this.[7] A parallel effort to establish a central role for history in understanding scientific knowledge is found in the work of Thomas Kuhn, whose book The Structure of Scientific Revolutions attempted to show the importance of history to the philosophy of science. His work had a formative influence on the Strong Programme and Shapin.[8]

---

[7] For similarly strong convictions on the importance of social factors in science, see Robert Young, Darwin's Metaphor, passim.

[8] This influence is evident in Barnes' full-length scholarly publication T.S. Kuhn and Social Science (New York: Columbia University, 1982): passim. For useful commentary on the influence of Kuhn on historians of science keen to adopt social perspectives in their scholarship, see Jan Golinski, Making Natural Knowledge:

2.2 Thomas Kuhn and the Sociology of Science

Kuhn's work revealed the importance of historical inquiry in understanding the development of scientific knowledge. Philosophy of science alone was not sufficient. My attention here will be focussed on those elements of Kuhn which not only gave history a key role, but opened possibilities for sociological inquiry. Although Kuhn himself did not take a strong sociological line in his thinking, his arguments provided enticements which the Strong Programme later took up. Kuhn developed a sophisticated terminology to describe the historical development of scientific knowledge.[9] Therefore, the following pages will elucidate the Kuhnian ideas of "normal science," "paradigms," "revolutionary science," and "incommensurability," which the Strong Programme and Shapin would interpret in a sociological light.

"Normal science" was the daily ebb and flow of most scientific work. Kuhn characterized normal science as predominately puzzle solving which aimed to solve set problems with an established set of scientific tools and training. Both the problems and their solutions derived from what Kuhn called a "paradigm." This is one of the most

---

Constructivism and the History of Science (Cambridge: Cambridge University, 1998): pp. 13-27. For an alternate perspective, maintaining that because of his continuing internalism, Kuhn offered false hope for socially minded historians of science, see Robert Young, Darwin's Metaphor, p. 225.

[9] Ideas on science similar to Kuhn's had been anticipated and developed in the French philosophical tradition. Consider, for instance, Gaston Bachelard's concept of "epistemological rupture" and "sanctioned knowledge." Georges Canguilhem further articulated these ideas. Both were as keen as Kuhn to use history as a source for developing an accurate picture of science. See Mary Tiles, "Bachelard, Gaston," Routledge Encyclopedia of Philosophy, vol. 3, ed. Edward Craig (New York: Routledge, 1988): pp. 620-624; Gary Gutting, "French Philosophy of Science," Routledge Encyclopedia, vol. 4, pp. 779-786.

attractive and slippery of Kuhn's ideas, as his use of the term was not consistent and a number of possible definitions are available. A basic definition he offered was that it consisted of "some accepted examples of actual scientific practice - examples which include law, theory, application, and instrumentation together - [that] provide models from which spring particular coherent traditions of scientific research." Elsewhere he referred to the paradigm as a "constellation of group commitments" and as a collection of "shared examples." To simplify things, an acceptable, comprehensive definition of Kuhn's paradigm is as a communally accepted collection of particular preconceptions, ideas, and discoveries about nature, which were accompanied by and intimately related to formalized theories, scientific instrumentation, technical practices and educational techniques.[10]

What a paradigm does is provide a common ground for a group of scientists to proceed forward with normal science, solving a stubborn question or puzzle about nature, and in that success attracting followers and supplying the approach and tools to deal with further puzzles. Because of this, normal science, despite the arduous technical and intellectual labour involved, strove for the predictable in its findings: radical or novel results were not expected, and indeed would be a source of consternation and concern, possibly suggesting inadequacies and weaknesses in the paradigm. The Newtonian world-view was one such example. Newton provided a perspicuous and sophisticated solution to the prize problem of describing and predicting the orbits of the planets through a striking

---

[10] Thomas S. Kuhn, The Structure of Scientific Revolutions, 3rd ed. (Chicago: University of Chicago, 1996 [1st ed., 1962]): pp. 10-11, 176-191; quote on p. 10.

mathematical presentation of an idea known as gravitation. However, Newton went further, suggesting that all the universe was subject to the same mathematical formalization, a move which offered potential followers rich opportunities to apply the methods of the Newtonian paradigm to solve complex and demanding problems of planetary and stellar motion. Achieving a paradigm took great effort, and not all provinces of intellectual effort had attained one - for example, the social sciences. Pre-paradigmatic science, for instance celestial physics in the two hundred years immediately before Newton's accomplishments, saw practitioners fight over how to go about interpreting nature; achieving a paradigm meant providing the means to interpret some key natural problem in a powerful and persuasive way, and through it opening the door to explaining other, related phenomena. If the solution was convincing enough, it would attract able scientists away from other competing schools and reach a critical mass of practitioners so that competing schools gradually disappeared through attrition and the failure to win new disciples. At times, Kuhn was wont to think of a paradigm as a world view.[11]

For the purposes of this chapter, Kuhn's use of the words "group" and "sharing" in describing paradigms and normal science is particularly interesting. For instance, one of the "constellation" of group commitments was what Kuhn called "values," concepts such as accuracy, simplicity, compatibility with other theories, and so on. This talk of concepts valued, concepts shared, and concepts belonging to groups moved the understanding of science away from simply rational categories where concepts are proved

---

[11] Kuhn, Structure, pp.10-20, 30-32, 35-36, 111-112, 151.

or established by observation of nature to where they are part of the community's character. In short, the paradigm was not a completely rationally established entity, but a conglomerate of scientific success stories, theoretical dispositions, commonly held beliefs about nature, and so on.

Kuhn, however, was emphatic that no paradigm exhaustively accounted for all natural or experimental phenomena. To continue the Newtonian example, Newton's paradigm gave mathematical formalism a vaunted role, and it was so powerful in providing sought-after explanations of planetary motion that most natural philosophers were willing to put aside their misgivings regarding, for instance, its lack of a mechanism to account for gravity. For Kuhn, however, the very act of puzzle solving upon which normal science advanced contained the seeds of its own demise: eventually, scientists would reach a point where the application of their paradigm was so precise and detailed that increasingly more and more phenomena would fall outside of its purview; in other words, scientists would begin to hit upon more and more "anomalies:" findings that could not be adequately framed and explained in the context of the current paradigm. Kuhn was emphatic: "Normal science does not aim at novelties of fact or theory and, when successful, finds none. New and unsuspected phenomena are, however, repeatedly uncovered by scientific research, and radical new theories have again and again been invented by scientists."[12] Anomalies foment unease among practitioners, and an awareness that more and more anomalies are cropping up imparts the motivation for scientists to seek other explanations, resulting eventually in "paradigm change" or "shift"

---

[12] Kuhn, Structure, p. 52.

- the movement to a new set of theories, predispositions, etc. that successfully explains the anomalies and previous phenomena. Kuhn labelled the most extreme form of shift a "scientific revolution."[13]

Kuhn defined a scientific revolution in the following manner: "scientific revolutions are here taken to be those non-cumulative developmental episodes in which an older paradigm is replaced in whole or in part by an incompatible new one."[14] Their incompatibility was a crucial point, one that is explained via Kuhn's notion of incommensurability. Importantly, Kuhn emphasized that scientific revolutions were very similar in kind to political revolutions, a tantalizing assertion for sociologically inclined historians of science, and one that Kuhn's use of sociologically ripe terms like "institution" and "community life" made additionally seductive. It is worth quoting Kuhn again at this point:

> Political revolutions are inaugurated by a growing sense, often restricted to a segment of the political community, that existing institutions have ceased to meet the problems posed by an environment that they have in part created. In much the same way, scientific revolutions are inaugurated by a growing sense, again often restricted to a narrow subdivision of the scientific community, that an existing paradigm has ceased to function adequately in the exploration of an aspect of nature to which that paradigm itself has previously led the way.[15]

Kuhn immediately went on to add that the collapse of one paradigm and the competition to replace it occurs in an environment without clear rules or guidelines, which were lost with the previous paradigm's failure to maintain an environment of normal science, and

---

[13] Kuhn, Structure, pp. 17-18, 52-53, 64-65; 90-91.

[14] Kuhn, Structure, p. 92.

[15] Kuhn, Structure, p. 92.

which await re-institution, in a new shape and form, under a newly installed paradigm. An obvious example of a scientific revolution would be the paradigm shift from the Ptolemaic, earth-centred universe to the Copernican, heliocentric model. Importantly, the paradigm is a self-contained package, holding all the necessary conceptual and practical equipment necessary to sustain itself. This self-contained character predicates, however, its self-referential character, or as Kuhn put it, its "circularity." The rationality of a paradigm, so to speak, is contained within it, and does not span different paradigms in the revolutionary context. In an inconspicuous statement of great sociological significance, Kuhn stated that "there is no standard higher than the assent of the relevant community." This repeated use of terms such as community and institutions, juxtaposed with statements about the circularity of paradigms and the inability to mediate between them with reason or logic, offered tempting vistas of sociological interpretation for some historians of science.[16]

To flesh out this circularity-fraught struggle between paradigms, Kuhn employed his notion of "incommensurability." As with many of Kuhn's ideas, incommensurability is not easy to pin down with one, quick definition. Three main elements, however, may be identified. First, two competing paradigms will cite different problems within their field of study that demand solution. The example of the Newtonian shunting aside the need for a mechanism for universal gravitation shows this well: for Newtonians, the mathematical formalism of Newton's achievement was sufficiently persuasive, whereas more mechanistically inclined natural philosophers saw such a lack as a serious shortcoming.

---

[16] Kuhn, Structure, pp. 92-94; quote p. 94.

Different visions of what needs to be answered and what can be answered here clash. Second, competing paradigms are incommensurable with one another because they employ traditional vocabulary, terminology and even technology in different ways. Thus, criticisms of one by the other are never wrong in a facile sense, but are ineffectual because the competitors conceive of the terms being used in differing ways, a fact obscured by the use of the self-same terms. Kuhn used the example of Copernicus' heliocentric paradigm to elucidate this. Copernicus' Aristotelian-Ptolemaic critics were not wrong in a simple sense in rejecting his views, because for them the very definition of the earth was that it was the unmoving centre of the universe; to say, as Copernicus did, that the earth orbited the sun was semantically absurd for them, for they held to a whole system of physics that depended on the earth's placement at the centre of the universe. Third, and Kuhn cited this as the "most fundamental" sense, the incommensurability of competing paradigms meant that "the proponents of competing paradigms practice their trades in different worlds."[17] Different categories of understanding are employed; different interrelations between observed entities are posited; different standards are employed to assess those interrelations, and so on. In summary, Kuhn was aiming to show that paradigm shift could never be a clean, logical, rational development, but entailed a good deal of intellectual and scientific disputation, frustration and antagonism. Because paradigms were incommensurable, overthrow was what happened to bring one to preeminence and cast the other into obscurity. As Kuhn pointed out, a paradigm rarely

---

[17] Kuhn, Structure, p. 150.

achieved supremacy through convincing its foes - the proponents of the defeated paradigm generally died off, without any new adherents to carry on their fight.[18]

In effect, Kuhn's sophisticated model opens science to historical inquiry by suggesting how the more transient entities with which historical study generally concerns itself - culture, personality, politics, society - play a role in the actual epistemic activity of scientists. These transient elements are set in contrast to the seemingly eternal elements which philosophy of science seeks to establish and refine such as reason, reality and so on, that if exhaustively justified scientific knowledge would put the epistemic content of science beyond the scope of historical inquiry. For if there were one permanent, unchanging method for procuring knowledge, and science had discovered this method, then ephemeral social arrangements, political circumstances, cultural mores and so on would have but anecdotal interest, and the scholar really interested in finding out what makes science a form of knowledge would be better served by investing his or her efforts in philosophy.

This being said, it is necessary to acknowledge that Kuhn was not a thorough-going sociologist of science, as will be seen below by the simple fact that the Strong Programme needed to reinterpret and bolster his work in order to give it a solidly sociological footing. Part of the issue here is that Kuhn in The Structure of Scientific Revolutions had a strong metaphorical tendency in his thinking, and it is often difficult to assess how literally Kuhn himself took these metaphors. This tendency is well illustrated in his sections on the parallels between political and scientific revolutions, and his

---

[18] Kuhn, Structure, pp. 111-112, 148-151.

frequent and enthusiastic use of research in cognitive psychology, such as Gestalt switches and Bruner's and Postman's playing-cards experiments.[19] In the case of scientific and political revolutions, it is telling that Kuhn used the term "parallel" to describe the similarities, and did not state that they were one and the same in structure and evolution. In the case of the psychological references, Kuhn was more emphatically metaphorical, repeatedly using the word "suggest" and its cognates. It is also notable that for all of his talk of communities and groups, Kuhn rarely offered any sociological models explaining such structures or analysing them, at least not as explicitly as he did with psychology. Thus, it is a very good question indeed how far Kuhn intended to take any sociological reinterpretation of science.

The reason for this becomes clearer when one reminds oneself of what role Kuhn desired history to play in the vision of science he presented in The Structure of Scientific Revolutions. It is important to realize that, despite the importance he placed on history as a crucial tool in understanding scientific knowledge, Kuhn had in no way abandoned epistemology and philosophy of science. In fact, it would be more reasonable to say that Kuhn's aim was a properly *historicized* philosophy of science, which would allow historians and philosophers of science to work in concert to stimulate advances in each field. A few years after the initial publication of The Structure of Scientific Revolutions, Kuhn reflected on his work in the following manner:

> Traditional discussions of scientific method have sought a set of rules that would permit any individual who followed them to produce sound knowledge. I have tried to insist, instead, that, though science is practised by individuals, scientific

---

[19] Kuhn, Structure, pp. 62-64, 111-115.

> knowledge is intrinsically a group product and that neither its peculiar efficacy nor the manner in which it develops will be understood without reference to the special nature of the groups that produce it. In this sense my work has been deeply sociological, but not in a way that permits that subject to be separated from epistemology.[20]

In this way, Kuhn's thought ironically was not as revolutionary as some of his interpreters would have it. His own work was a smaller scale paradigm shift that modified the internal workings of his field, but did not entirely supercede it.

2.3 David Bloor and Barry Barnes: The Edinburgh Strong Programme

Unlike Kuhn, David Bloor and Barry Barnes, lead thinkers of the Edinburgh Strong Programme, were unequivocal in their support for the sociology of scientific knowledge. The "Strong Programme" was so labelled because it advanced a "strong" sociology of knowledge: all elements of scientific knowledge, whether mathematical, experimental, observational or theoretical, were open to sociological inquiry and explanation; as Bloor stated, "[t]here are no limitations [to the sociology of knowledge] which lie in the absolute or transcendent character of scientific knowledge itself, or in the special nature of rationality, validity, truth or objectivity."[21] For my purposes here what is important to recognize is that advancing this thesis simultaneously and intentionally made historical inquiry crucial to understanding scientific knowledge. To get a sense of this, an examination and analysis of the program of the Strong Programme will pave the way for

---

[20] Kuhn, "Preface," The Essential Tension: Selected Studies in Scientific Tradition and Change (Chicago: University of Chicago, 1977): pp. xx.

[21] David Bloor, Knowledge and Social Imagery, 2nd ed. (Chicago: University of Chicago, 1991): p. 3. The first edition was published in 1976.

an appreciation of the sociological roots of Shapin's scholarly oeuvre. This program can be approached through Bloor's seminal Knowledge and Social Imagery where he listed four "tenets" of the Strong Programme. Then, reference to Barnes' insistence on the importance of culture in the development of science will further help to show the historiographical implications of the Strong Programme.

What were the four tenets of the Strong Programme? They were: (a) causality, (b) impartiality, (c) symmetry, and (d) reflexivity. To varying degrees these build on and reinforce one another. All of them expressed Bloor's confidence that sociology was a perspicacious tool for understanding science. Bloor did not aim to elucidate their historiographical applicability, but to establish a theoretical foundation for such application in historical inquiry, as his brief forays into a variety of topics in the history of mathematics showed.[22]

To commence with (a) *causality*, Bloor contended that sociologists should attempt to reveal the causes that generated knowledge claims. Knowledge varies over time and place, and the sociologist needs to account for such variation. Using his expertise in describing and explaining society, the sociologist could account for collectively held belief through showing the nexus of social causation lying behind any knowledge claim.

Bloor even went so far as to argue that mathematics, seemingly the province of

---

[22] Particularly worthy of attention is David Bloor, "Polyhedra and the Abominations of Leviticus: Cognitive Styles in Mathematics," Essays in the Sociology of Perception, ed. Mary Douglas (London: Routledge and Kegan Paul, 1982): passim.

knowledge most distant from sociological description, was as much the product of socially generated consensus as any other form of knowledge.[23]

This talk of "social causation" requires further analysis. What does it mean to be socially caused? Bloor's use of the notion of cause is purposely loose and incompletely articulated, showing his lack of interest in philosophical methods. His proudly held "scientism" expressed his belief that the Strong Programme replicated scientific methodology in the sociology of knowledge.[24] Bloor defined knowledge as collective belief: "knowledge for the sociologist is whatever people take to be knowledge." The major problem of the sociology of scientific knowledge is the "variation" in human-held beliefs about the structure of the natural universe. For Bloor, the fact that different peoples have explained the universe differently demands sociological investigation. Thus, social causation for Bloor is the idea that the collectively held beliefs which human communities label "knowledge" spring from the social structures of the communities themselves. Bloor's primary conviction was that sociology could explain scientific knowledge. Writing early in the history of the Strong Programme, he explicitly left it for more empirically minded scholars such as Shapin to show its value through concrete inquiry.[25]

---

[23] Bloor, Knowledge and Social Imagery, p. 7; for his commentary on mathematics, see p. 3 and Chapter 5.

[24] Bloor, Knowledge and Social Imagery, pp. 5, 13, 160-161.

[25] Bloor, Knowledge and Social Imagery, pp. 4-7, 12. Bloor did qualify his belief in the universality of a social element in knowledge systems, stating in a number of places that other causes play a role, and that in some instances social causation might function as a "background condition;" see Knowledge and Social Imagery, pp. 7, 166. For more

Bloor intended this Strong Programme empiricism to stand in direct confrontation with the philosophy of science. Yet, as Michael Friedman points out, the Strong Programme itself advanced a blatant "philosophical agenda" of its own:

> [The] defenders of [the sociology of scientific knowledge] represent themselves as explicitly rejecting the aims and methods of traditional philosophy - not simply as leaving them out of account. They feel compelled, that is, explicitly to deny the philosophical theses underlying the traditional normative enterprise: for example, 'science is not a set of universal standards, sustaining true descriptions and valid inferences in different specific cultural contexts'; 'there is no sense attached to the idea that some standards or beliefs are really rational as distinct from merely locally accepted as such'; 'there are no context-free or super-cultural norms of rationality'; and so on. Moreover, it is precisely by insisting on such negative philosophical conclusions that defenders of [the sociology of scientific knowledge] adopt an explicitly philosophical agenda which itself goes beyond the bounds of purely descriptive empirical research.[26]

This is a poignant statement, for the Strong Programme and Shapin himself did not relinquish their anti-philosophical tendencies despite the burden of philosophical baggage

---

evidence that Bloor's style of presentation, where citing already existing examples of affiliated scholarship and encouraging others to pursue the Strong Programme's agenda to establish its empirical success was opposed to developing philosophically exhaustive and coherent arguments, was an explicit approach, see Bloor, "The Strengths of the Strong Programme," Philosophy of the Social Sciences 11 (1981): particularly p. 206. For more on the Strong Programme's philosophical component and its resulting openness to philosophical critique, see Larry Lauden, "The Pseudo-Science of Science?" Philosophy of the Social Sciences 11 (1981): pp. 173-198; and Michael Friedman, "On the Sociology of Scientific Knowledge and its Philosophical Agenda," Studies in History and Philosophy of Science 29 (1998): pp. 239-271.

[26] Michael Friedman, "Philosophical Agenda," p. 244. Friedman went on to argue that not only was this philosophical agenda unnecessary, but implausible, attempting to bend the purely normative aims of its philosophical hero Wittgenstein to empirical ends they were never intended to meet; see particularly pp. 251-256.

they felt obliged to carry into their scholarly labours.[27] The consequences of such a rejection of philosophy will be discussed throughout this thesis.

Next, Bloor recommended that sociologists be (b) *impartial* to whether subsequent generations or contemporary audiences saw the epistemic item under investigation as true or false. Strong Programme sociologists should simply allow a group's affiliation with some knowledge claim to be the justification for sociological interpretation; in other words, the sociologist should take the fact that a historical community held the belief as sufficient reason for study, no matter whether the society the sociologist inhabits considers the idea valid or not. The sociologist needs to take care not to discount ideas that his or her own society does not believe. Once the sociologist is impartial in this regard, his or her ascription of causes to differing bodies of knowledge can then be (c) *symmetric*. This dealt with a widely held tendency, known as the "arationality" principle in some circles, for critics to invoke sociological explanation only when a body of knowledge was perceived to be false, on the assumption that social factors could only distort knowledge formation. For these critics, what is based on reason and logic needs no explanation, being self-evidently correct, whereas what is erroneous requires the introduction of other factors, possibly social in character, to show where

---

[27] The persistence of this stance in Shapin's writing is evident in essays as temporally diverse as his "History of Science and its Sociological Reconstructions. "History of Science 20 (1981): passim, to his "Rarely Pure and Never Simple: Talking about Truth." Configurations 7 (1999): passim.

things went wrong. Bloor considered this asymmetry a false conceit, and widened the scope of sociological inquiry to include all communally held epistemic items.[28]

Reflection shows how intimately the first three tenets are interrelated. Once the sociologist defines knowledge as collective belief, then it flows from this that the sociologist should not import his or her own valuations of veracious versus erroneous into the study of knowledge claims. Further, once one accepts this definition of knowledge and additionally assumes that some form of social causation is at work in all forms of human belief, then naturally one should not limit his or her sociological attention only to epistemic claims deemed erroneous, as all knowledge will have an irreducible component of the social for the sociologist to identify and elucidate. Bloor's tenet of social causation predicates the impartiality and symmetry tenets, which in turn allow the sociologist to reveal the often sophisticated social causation lying behind epistemic claims.[29]

Finally, Bloor put forward the maxim of (d) *reflexivity*. Edinburgh Strong Programme sociology was no different than any other scientific discipline, or indeed from those epistemic communities the Strong Programmer investigated. Thus, sociology should be an expression of social causation too, something that the sociologist should be

---

[28] Bloor, Social Imagery, p. 7, 11-12.

[29] Bloor fleshed out a further defence showing the intimate interconnectedness of the causality, impartiality and symmetry postulates of these first three tenets in "Strengths of the Strong Programme," pp. 204-207.

prepared to explain when presenting his or her own interpretations of science.[30] This is the least important of the tenets for my purposes here, as it functioned mainly to shield the Strong Programme from accusations of an intellectual elitism that might suggest that the tools of the Strong Programme were capable of explaining all other forms of knowledge, while isolating themselves from the same critique. Should the sociologist so choose, he or she could plot a sociology of knowledge of the sociology of knowledge.

What are the historiographical implications of the Strong Programme's research agenda? The argument here is very similar to that presented above in the examination of internalism-externalism or Kuhn. For if knowledge can be seen as containing an ineradicable element of social causation, then the historian can study knowledge through examing the social circumstances that obtained in the historical setting where the knowledge appeared. Historical inquiry has the power to reveal the social scenarios historical agents encountered, and therefore can be extended to elucidating the knowledge systems through the same processes of study. The historian need not worry whether contemporary or historical audiences deemed the knowledge claim under examination spurious, for as long as a community held it to be true, then it is classified as knowledge and therefore should not be disdained as unworthy of historical inquiry. This is clearly evident in the brand of historicism which Shapin promoted as a partial solution to the

---

[30] Bloor, Social Imagery, p. 7. Elsewhere, Bloor pointed to his "inductivism" as grounding the scientistic tendency partially represented in his reflexivity postulate. By this, he seemed to be pointing to a kind of extrapolation from actual practice in science to the methods of the Strong Programme. It should also be noted that in making this argument Bloor indicated Kuhn as one of his authorities. See Bloor, "Strengths of the Strong Programme," pp. 206-207.

internalist-externalist controversy.[31] Finally, and perhaps most importantly, the historian need not set aside one toolbox and pick up another to deal with supposed true and false bodies of belief - under the symmetry postulate, knowledge systems are explicable using the same sociological tools, regardless of their supposed epistemic worth. The adoption of this tenet will be very clear in Shapin's work, as the failure of Hobbes' natural philosophy to defeat Boyle's experimentalism did not relegate the former to a secondary status in Shapin's mind - it was a full player in seventeenth-century English natural philosophy.

Besides these four tenets, the Strong Programme placed considerable emphasis on the role of culture in scientific knowledge. Barnes propounded a Strong-Programme notion of culture at some length. In short, the Strong Programme saw culture as a vehicle for social interests, a repository of historically established knowledge, and as a lens through which human agents perceived and understood reality. Each of these components requires analysis.

Interests for Barnes are thoroughly social entities. In its most visceral sense, an interest is something a social group pursues, defends and maintains in order to sustain or bolster the community's social position and strength. In the case of communities of knowledge-makers, the interest was primarily the need to produce accounts of their world that allowed for its prediction and manipulation. The scientific community posed certain questions to nature and found answers in the name of the social group's prosperity. The actualization of this interest was the ability to come up with results which the community had predicted and which resulted in the ability to use nature to forward the social group's

---

[31] See p. 13 of this chapter.

purposes. Interests, however, were not simply conscious strategies for guiding social activity, but actually shaped the human cognitive appreciation of nature; in Barnes' idiom, interests worked to "pre-organize" knowledge. He cited physiological diagrams of the human body as an example. According to Barnes, these images did not only aim to depict nature, but also to discipline and form the student's intellectual and cognitive abilities so as to understand nature physiologically. The specific interest of the community in this case was to build its strength through enlisting new adherents and succeeding in having them see nature in the manner of that community. Interests supplied the motive, direction, and structure of human knowledge.[32]

If interests played a key role in the composition of knowledge, then so too did history. For Barnes, all knowledge has a historical lineage, and this lineage is available to the practicing scientist in the form of "resources." This metaphor was crucial for the Strong Programme, as it gave a concreteness to culture necessary if it were to be something employable in sociological hands. The past had bequeathed the present a wide variety of methods, techniques, claims, counter-claims and so on for the practising scientist to take up, reworking them into seemingly new accomplishments; additionally, resources from other provinces of culture such as economics or art - seemingly unrelated to science - might be taken up and wielded in scientific discourse. In the form of resources, Barnes saw history as an inescapable contributor to the scientific endeavour, a fact which hints at the conservative nature of the knowledge-maker's enterprise: all

---

[32] Barry Barnes, Interests and the Growth of Knowledge (London: Routledge and K. Paul, 1977): pp. 6-7, 12-16, 18.

knowledge claims are based on an inherited past that they cannot escape - this theme is apparent throughout Shapin's oeuvre. The knowledge of the present owes its shape to the intellectual work of the past, and the present blends endlessly into the future, finality did not exist in knowledge. Knowledge is as history-laden as it was interest-laden.[33]

Closely connected to both the interest and historical elements of culture was its role as a lens for shaping both the appearance and interpretation of nature. In short, this is the argument that membership in a given community entails that the member will perceive and interpret reality in a communally shared manner. The basic philosophical contention behind this idea is that facts are the result of theory, and not vice versa. According to Barnes, the cognitive tendencies of an individual human tend to follow those of the community of which he or she is a part. If, for instance, a person belongs to a community which sees the universe as a mechanistic and non-teleological entity, then cognition will proceed to organize sensory input in a manner befitting this theoretical perspective; if, on the other hand, one believes the universe to be imbued with God-given purpose, the community will marshal reality so as correspond with this view. Obviously, Barnes recognized that there are limits to this: experience does not directly equip the community with detailed sensory evidence of mechanistic corpuscles or divine purpose. However, in his mind community membership structures experience so that it fits as closely as possible with basic assumptions, regardless of the absence of unequivocal evidence of the community's beliefs.[34]

---

[33] Barnes, Growth of Knowledge, pp. 11-12, 89.

[34] Barnes, Growth of Knowledge, pp. 17-18, 87-88.

Barnes attempted to make the experience-shaping characteristic of culture more concrete through associating it with language. For Barnes, human knowledge was a formation of language. All human knowledge was couched linguistically; and language, a human phenomenon, was open to sociological understanding. Language mediated all experience; the linguistic particularities of the community distilled the products of the senses in a pre-patterned manner. Thus, in Barnes' interpretation, language had important cognitive-psychological consequences, and the study of the specific language features through which humans justified their understandings of nature was an insightful way to mine the character of human knowledge.[35]

Barnes' theory of culture advances many exciting philosophical propositions: language and community structure cognition, theoretical predispositions determine facts, human interests and history structure epistemic beliefs, and so on. The Kuhnian influence here is clear, not only in Barnes' taste for using psychological references to develop his theory, but also in the paradigm-like vision of culture which emerges, where the basic explanatory elements of an epistemic system - for instance, the facts and experiments of the scientific world-view - only make sense in a matrix of theoretical assumptions embedded in language. Each community has its own socially derived semantics, a Barnesian sociological development of Kuhn's observation that those who share a paradigm understand terminology and theories differently than those inhabiting another

---

[35] Barry Barnes, Scientific Knowledge and Sociological Theory (London : Routledge & K. Paul, 1974): pp. 16, 18.

paradigm.[36] Throughout, Barnes attempts to recast elements of Kuhn's thinking in a sociological mould.

Shapin's contact with the ideas of the Strong Programme was immediate and intimate - he was a lecturer at the University of Edinburgh, alongside Barnes and Bloor, for much of the 1970s and 1980s. At that time he published a course bibliography for a course in "the social history of science," a course that ran alongside a "'philosophical' component." The aim of these courses was promoting the "sociology, philosophy, social history, and contemporary political aspects of science."[37] It can be seen from this that Shapin was deeply immersed in the Edinburgh Strong Programme, and he positioned himself intellectually as a voice that would promote historical application of the theoretical sociological insights of Barnes and Bloor. It is this attempt that will absorb my attention in the next two chapters.

---

[36] Barnes, Scientific Knowledge, pp. 10-11.

[37] Steven Shapin, "A Course in the Social History of Science," Social Studies of Science 10 (1980): pp. 231-258; all quotes from p. 231.

Chapter 3
Nature, Society and Social Strife in Leviathan and the Air-Pump

> As we come to recognize the conventional and artifactual status of our forms of knowing, we put ourselves in a position to know that it is ourselves and not reality that is responsible for what we know. Knowledge, as much as the state, is the product of human action. Hobbes was right.
>
> Steven Shapin and Simon Schaffer, Leviathan and the Air-Pump (1985).

Emerging from his years of teaching and research at the University of Edinburgh, Shapin's Leviathan and the Air-Pump, hereafter LAP, was a major contribution to the sociological study of the history of science. Coauthored with Simon Schaffer,[1] this book presented an interpretation of Robert Boyle's (1627-1691) experimentalist program in seventeenth-century England and Thomas Hobbes' (1588-1679) criticisms of it. The interpretation was boldly and thoroughly sociological in approach. Shapin and Schaffer

---

[1] This was Shapin and Schaffer's only major scholarly collaboration, representing for both their first major publication after their doctoral dissertations. This chapter will not attempt to differentiate their voices in LAP. Schaffer has continued to write extensively on the history of science, like Shapin remaining dedicated to the promotion and development of a sociological perspective. In particular, his curiosity has been attracted by seventeenth, eighteen, and nineteenth-century European astronomy. Examples of his writings include: Newtonian Cosmology and the Steady State, 1980 (Ph.D. thesis, Cambridge University); "Natural Philosophy and Public Spectacle in the 18th Century," History of Science 21 (1983): pp.1-43; "Authorized Prophets: Comets and Astronomers after 1759," Studies of 18th- Century Culture 17 (1985): pp. 45-74; "Comets and Idols: Newton's Cosmology and Political Theology," Action and Reaction: Proceedings of a Symposium to Commemorate the Tercentenary of Newton's Principia, Paul Theerman and Adele F. Seefe. (Newark: Delaware, 1993); "The Show that Never Ends: Perpetual Motion in the Early 18th Century," British Journal of the History of Science 28 (1995): pp.157-189.

emphasized how the differing social statuses of Boyle and Hobbes shaped each's natural philosophic outlooks.[2]

In LAP the sociological model depended heavily on notions of social strife and tension. This characteristic needs critical attention, as later in A Social History of Truth (AST), Shapin would acknowledge the limitations of relying on social strife and tension and propound an alternative detailing the role of credibility and trust in knowledge. It will be seen that, despite its strengths, their sociological approach faced problems generally in its downplaying of the significance of epistemological considerations and in its overly narrow conception of social structure. To moderate their sociological hand, I shall argue that a new philosophical awareness is required in the social history of science, an awareness expressed in the ideas of "ontological equivalence" and "epistemological sensitivity." It will also be seen that there are faults in the historiographical application of their sociological methods, particularly in how they handled their interpretation of Hobbes.

---

[2] Although some readers may be inclined to draw a sharp distinction between "science" and "philosophy," for the sake of historical accuracy "natural philosophy" or "philosophy" and their adjectival forms will be used throughout this thesis to describe the "science" of Boyle and Hobbes. They thought of themselves as natural philosophers, and I will respect that description. For the sake of the larger critical and historiographical perspective, however, it is safe to equate science and philosophy in the pages that follow, unless specified otherwise. Shapin's comments on knowledge apply quite broadly to any form of knowledge, be it philosophical or scientific.

3.1 Fundamental Notions of Shapin's Image of Science

Keeping with my intention to take largely an intellectualist tack by analysing the key ideas behind Shapin's thought, here I will present some of the ideas to which Shapin objected and the alternatives he proposed in LAP. Their origins in the intellectual heritage of Thomas Kuhn and the Edinburgh Strong Programme, described in the previous chapter, are manifest. First, Shapin doubted the following:

- that science has a privileged objectivity and conclusiveness in its methods;
- that observation, particularly as expressed in the methods of experiment, functioned as an independent, overarching control on theory;
- that the centrality of experiment in modern science confers decisiveness to science's epistemic claims;
- that science had succeeded in isolating itself from society, culture, and politics;

Shapin did not dispense with these factors entirely, but chose to reinterpret them sociologically. Thus, science's objectivity and experiment's centrality become community accomplishments, not the products of solitary contemplation and reflection; the interpretation of observation is handled in light of the interests and beliefs of Boyle's social community, not his individual intellectual acumen; and finally, science is portrayed as being an expression of social, political and cultural circumstances, a human activity that reflects how society is organized in both how it is done and the results it achieves. These features of Shapin's thought clearly express the tantalizing historical opportunities

Kuhn suggested in that they translate such notions as paradigms into sociological entities that carry a load of social, political and cultural elements.

It will help to see this from the other side and establish positively some features of science in Shapin's view. Shapin's sociological interpretation of scientific knowledge was coloured by ideas from the Strong Programme, particularly its first three tenets: causality, impartiality and symmetry. He grounded his interpretation on the following commitments:

- science, like all knowledge making, is a social activity, consisting of a collection of specific social conventions;
- science is a product of cooperative human activity;[3]
- all knowledge-claims, including those of science, have a definite social and political character, derived from the social milieus within which scientists work;
- the conventions which shape scientific knowledge are themselves shaped by the economic and social categories of those who speak about nature. Therefore, all knowledge transfers the distinctions of social class onto the natural world;
- scientific knowledge achieves solutions to "the problem of social order;" in other words, it is a way of forwarding the political agendas of those doing science.

---

[3] To provide philosophical backing for these first two points, Shapin assumed Wittgenstein's phraseology: science was a "form of life" and a "language game;" see, for instance, Simon Schaffer and Steven Shapin, Leviathan and the Air-Pump: Hobbes, Boyle and the Experimental Life (Princeton: Princeton University, 1985): pp. 15, 22. Shapin's assumed his understanding of Wittgenstein under the mantle of Strong Programme mentor David Bloor, who propounded an approach to the sociology of knowledge based on Wittgensteinian philosophy; see his Wittgenstein: A Social Theory of Knowledge (New York: Columbia University, 1983): passim. There, Bloor defined "form of life" as a "pattern of socially sustained boundaries;" see p. 140.

For Shapin, in line with his Strong Programme mentors, scientific knowledge contained an irreducible element of the social. All knowledge claims had a causal source amid the social communities of historical agents; thus, the same tools were to be applied to scientific knowledge as to social structures, regardless of whether the knowledge was widely held to be true or was only seen as such by a small community. As with Kuhn, Shapin carried out further acts of translation in applying the Strong Programme to historical interpretation. For instance, the Strong Programme definition of knowledge as collectively held belief is made historiographically applicable in the role of social conventions in knowledge - epistemic claims which the community holds must have concrete social expression, and that expression was in the form of conventions.[4]

Perhaps what is most interesting here is that Shapin immersed science in a vision of society where tension and strife between different, class-like social groups, imparted the motivation to do science, not as a purely intellectual investigation, but as a crutch for political machinations, a legitimating tool to ground social ambitions and political power.[5] Shapin and Schaffer stated in the concluding chapter of LAP that,

> . . . the contest among alternative forms of life and their characteristic forms of intellectual product depends upon the political success of the various candidates in insinuating themselves into the activities of other

---

[4] Bloor described conventions on a more normative, theoretical level, describing them as founded on language games and argued that human "conventional behaviour" was the source of the human imposition of meaning on the environment. See Bloor, Wittgenstein, pp. 49, 137.

[5] Schaffer and Shapin, LAP, pp.156, 171, 192, 199.

> institutions and other interest groups. He who has the most, and the most powerful, allies wins.[6]

Science's success resides not in the perspicacity of its vision into the essential character of nature, but in its success in forging the right political allegiances. Is it necessary to take this approach if one wishes to take up a sociological approach to the history of science? No. Before coming to a detailed justification for this conclusion, however, these radical pronouncements on science and society shall be examined in depth.

3.2 Three Core Ideas

To do so, I will analyze three key ideas that emerge from my reading of LAP: (a) nature is not knowable in one, final, conclusive way; (b) scientific knowledge is a product of social conventions; and (c) science is a social-political tool. Some may not grant a distinction between the second and third ideas, but it is crucial to understanding later developments in Shapin's thinking about science, particularly as expressed in A Social History of Truth. This is because one does not have to see social conventions as originating in social tension and strife, but may turn to other characteristics of human communities, such as trust and credibility, to find their origins.

Before moving into hard issues of interpretation, a brief historical outline of exactly what sort of knowledge Boyle sought in his experiments will be useful. Boyle's main philosophical interest resided mainly in what would now be considered chemistry,

---

[6] Schaffer and Shapin, LAP, p. 342.

but which might more insightfully be considered matter theory. One factor to keep in mind is that most natural philosophers of the first half of the seventeenth century did not consider chemical researches a proper area for natural philosophic curiosity. The "alchemists" and "iastrochemists" mainly saw chemistry as a way to make medicines. This was known as the Paracelsian tradition. The then dominant form of chemistry, it conceived of chemistry in a very organic sense, and most of its concepts seem completely unrelated to the discoveries of modern chemistry. As mechanical explanation gained more ground in natural philosophy, some began to advance mechanistic explanations for chemical phenomena. Boyle was an eager innovator in this push because of his confidence that experimentally based chemical researches would provide the mechanistic natural philosophy with a proper matter theory. Although Boyle continued many Paracelsian traditions, he firmly embraced moving corpuscles as the fundamental constituents of matter. He used numerous experiments to articulate and defend his corpuscularianism,[7] and it is to Shapin and Schaffer's scepticism about the Boylean conception of experiment which we will now turn.

The (a) inconclusiveness of knowledge based on experiment is a major theme of LAP. Shapin and Schaffer contended that Boyle saw "matters of fact" as incontestable, whereas causal explanations were sources of division within the philosophical community. Boyle wanted to rejuvenate human knowledge by building a natural

---

[7] Richard S. Westfall, The Construction of Modern Science: Mechanisms and Mechanics (Cambridge: Cambridge University, 1971): pp. 65-81.

philosophic system that relied only on matters of fact, eschewing causal explanations. Experiment would build a large store of facts about nature, without demanding that each experimenter advance causes for these facts, thus freeing the experimentalist natural philosophy from divisive internal dispute. Shapin and Schaffer gave detailed examples of some of Boyle's experiments to show that "matters of fact" were indeed contestable. Because they conjectured that all observation was predicated on having some set of theoretical predispositions, they contended that matters of fact were indistinguishable from causal explanations. Theoretical predispositions structured how the observer saw the experiment, and thus tainted the matters of fact with a large dose of causal reasoning. For Shapin and Schaffer, Boyle's was an impossible program - any experiment was susceptible to various interpretations.

Boyle's "void-in-the-void experiment" is an excellent example of this. Here Boyle elucidated his own answer to the famed "Torricellian space." Evangelista Torricelli noted in 1644 that when a full tube of mercury was tipped upside down into a vessel of the same substance, the mercury failed to empty completely from the tube. A space was left between the top of the tube and the remaining mercury, which levelled off some distance above the level of the mercury in the vessel. Boyle tried the experiment inside the glass receiver of his air-pump. As the process of exhausting the receiver of air progressed, the mercury in the tube fell closer and closer to the level of the mercury in the dish. Although it did not drop to exactly the same level, Boyle extrapolated from this experiment that air had pressure and "spring." In conditions outside of the pump, the weight of the airy

atmosphere exerted a pressure on the mercury in the dish, preventing all the mercury in the tube from draining. Once sealed inside the receiver of the air-pump, but before Boyle's lab workers pumped the air out, the level of the mercury remained the same, Boyle explained, because the air had a springiness which compensated for the loss of atmospheric pressure. With the air removed, the mercury could then drain freely into the dish. Boyle claimed that he had overcome air's pressure and spring through his experiment.[8]

It was at this point, however, that Shapin and Schaffer asked a sticky question: what were the pressure and spring of the air? Were they matters of fact or hypotheses? They noted that, despite Boyle's exhortations to avoid causal explanation and rely only on matters of fact, air pressure and spring were as much causal as factual. Boyle, they said, failed to explain how he arrived at these ideas from observing the fall of the mercury in the pump's receiver. Although Boyle refused to indicate the causes of the spring and pressure of the air, he *treated* them as causes. Shapin and Schaffer saw this as epistemologically suspect. If matters of fact were different from causes, then they had to be different, and Boyle's matters of fact had clear causal functions for Shapin and Schaffer.[9]

Boyle's ostensible antipathy to causal explanation created another problem, one concerning the ontological status of the Torricellian space. The main reason it was of

---

[8] Schaffer and Shapin, LAP, pp. 40-49.

[9] Schaffer and Shapin, LAP, pp. 52-55, 220-224.

interest to philosophers was because many claimed it was proof of vacuity in nature - space could be completely emptied of matter. If not, what exactly was in the space between the surface of the mercury and the top of the tube? The plenists, however, were convinced that the universe was stuffed full of matter, with no vacuous spaces. Therefore, somehow the Torricellian space was replenished with matter. Boyle, however, said little on this head - he felt it to be the sort of questioning inappropriate for the experimentalist. Experiment, he assured, could never answer this sort of metaphysical debate, and this was proper: if experiment could not decide the matter, then the question was futile and not worth posing. Boyle redefined the question to suit experimentalism - a vacuum had indeed been created during the experiment, but not necessarily one devoid of all matter, only devoid (or almost devoid) of normal air. So he used the term "vacuum", but in a way that evaded its traditional meanings.[10] Again, according to Shapin and Schaffer, predispositions decided how observers were to interpret experiment - only certain sorts of questions were admissible. Shapin and Schaffer did, however, suggest that Boyle leaned towards the vacuist side of the metaphysical debate. As some scholars have noted, matter theory was contentious because it involved the ontological status of God.[11] Boyle, being much concerned with showing himself a good Christian, wanted God to fit into a

---

[10] An excellent example of Shapin and Schaffer employing Kuhnian incommensurability; see Chapter 2 of this thesis.

[11] Edward Grant, Much Ado about Nothing: Theories of Space and Vacuum from the Middle Ages to the Scientific Revolution (Cambridge: Cambridge University, 1981): pp. 259-264.

mechanistic universe. Many anti-mechanistic criticisms of the Cartesian and Hobbesian ontologies charged that in a fully mechanistic plenum, everything was deterministic, and thus God could not exert his will, at least not after he gave the initial impetus that put the universe in motion. By permitting the possibility of vacuum, Boyle espoused an ontology compatible both with a corpuscular mechanist view of matter and a universe fully under the sway, at all times and places, of an omnipotent God. Theoretical predispositions, this time of a theological-ontological character, governed the interpretation of experiment.[12]

Shapin and Schaffer drove home the point that experimentation was not epistemologically conclusive by introducing Hobbes' criticisms of Boyle's work.[13] Hobbes interpreted the "void-in-the-void" experiment very differently, using his own mechanistic and plenist account of what had happened to the mercury. For Hobbes, the whole universe was filled with matter - it was a plenum. No empty space - or vacuum - existed. The Torricellian space was filled not just with matter, but air, straight from the

---

[12] Schaffer and Shapin, LAP, pp. 141, 202-207.

[13] It must be emphasized that the interpretation found in this chapter of Hobbes and his thinking, particularly regarding the relationship between his political philosophy and his vision of knowledge, is Shapin and Schaffer's reading of Hobbes. They attempted an innovative fusion of the political with the epistemological to present Hobbes as an absolutist in knowledge as well as in the polity. There are, however, other interpretations of Hobbes which do not accept such a fusion. For instance, Hobbes advised his absolutist monarch to exercise moderation in regulating his subjects' beliefs, due to the volatility and variety found amongst those beliefs; forbearance on the part of the monarch in matters epistemic would allow the preservation of absolute power in the political sphere. Shapin and Schaffer presented a radical reading of Hobbes, and the reader should keep this in mind. Tom Sorell, "Thomas Hobbes," Routledge Encyclopedia of Philosophy, vol. 4, ed. Edward Craig (New York: Routledge, 1998): pp. 459-476.

atmosphere. When Boyle had air withdrawn from the air-pump's glass receiver, according to Hobbes' interpretation, it went into the atmosphere, thus increasing the amount of matter in that vicinity. This resulted in a circulation of air that immediately redressed the emptying receiver. Because of the overload in the plenum, the exiting air forced an opposing stream of air back into the receiver. This occurred violently at points of leakage, such as around the tube through which the outward flowing air passed. Leakage, according to Hobbes, was inevitable, because the infinitely divisible air was bound to find some compromise in the structural integrity of the air-pump. The Torricellian space itself was filled with this air, which "penetrated" the mercury to return to the emptied space by the same process explained above. The space was achieved because of a certain balancing act in nature - the downward flow of the mercury was eventually halted because the air passing back up through it into the Torricellian space resisted the flow. Shapin and Schaffer emphasized that Hobbes adhered to the belief that for a proposition to be philosophical knowledge, it had to contain a causal explanation. Without causality, knowledge could not be philosophical - it would be craft. He thus expounded his explanation in completely causal terms.[14]

For Shapin and Schaffer, Hobbes saw the "void-in-the-void" experiment very differently from Boyle because of his differing theoretical attachment to mechanistic plenism and the causal nature of philosophical knowledge, which combined to create a forceful argument against experimentalism as a philosophical project in general.

---

[14] Schaffer and Shapin, LAP, pp. 83, 89-91, 139-143.

Experiment in and of itself was not decisive. Hobbes did not allow Boyle to dodge judgment of the ontological character of the Torricellian space - to do so was a fault, not a strength, of Boyle's philosophy. Epistemologically, he demanded that Boyle either deem the spring a cause, or else determine one. If not, experimentalism was no better than some sort of craft, and could not be honoured with the name philosophy. Ignoring Boyle's protests, he treated the spring of the air as a causal explanation, and tripped up Boyle by stating that the springy air smelled of self-moving matter - definite anathema to Boyle as it conflicted with both his mechanistic and his religious convictions: matter that moved itself would not need a God to help it do so.[15]

In short, Hobbes denied Boyle's matters of fact outright. The same sensory episodes sparked very different intellectual responses. A number of consequences flow from Shapin and Schaffer's presentation: far from being incontestable, one person's "matters of fact" collapsed completely in the face of another's different epistemological and ontological perspectives; experiment in no way relieved the observer of the burden of his or her theoretical predispositions; it is a fundamental characteristic of the human epistemological condition that episodes of sensory experience are open to many interpretations. This observation ties in directly with the Edinburgh Strong Programme's ideas of social causation and culture described in Chapter 2: Hobbes and Boyle were, according to Shapin and Schaffer, fighting for different epistemic claims due to their different social placements.

---

[15] Schaffer and Shapin, LAP, pp. 121, 141-142, 204.

Shapin and Schaffer concluded LAP with the statement "Hobbes was right." What he was right about in their minds is of the utmost bearing to the question at hand. For them, Hobbes was right in acknowledging that reality can never determine human belief. It is a mistake to think that experiment (or reality) creates knowledge - agency in knowledge belongs to humans. Experimentation is not a privileged epistemological tool; it does not allow nature to express itself in some special way. It is merely another way that humans poke and prod at nature. To let oneself assume that one's philosophy allows nature to speak for itself is to march down the path to rampant inconsistencies and absurdities. For Shapin and Schaffer, Boyle followed that path through trying to do the impossible: allowing nature to dictate its own character. What he achieved was to let a certain human image of nature run loose, uncontrolled, in the guise of an incontestible reality. Nature was a human idea. Understanding it hinged on recognizing its origins in human agency, not in an ineffable external agency. And so, "Hobbes was right." Knowledge for Hobbes had to be made by humans from the ground up. Shapin and Schaffer agreed. This expresses the inconclusiveness of experiment in LAP's image of science most strikingly.[16]

Now the second core idea: (b) the role of social convention in knowledge. The person who thought that experiment was a superior knowledge-gathering method is left with a conundrum: if nature fails to shape human knowledge, then what exactly does the work of convincing a human that a particular way of looking at the world is the right one?

---

[16] Schaffer and Shapin, LAP, pp. 149-154, 344.

Here Shapin and Schaffer's Strong Programme credentials are blatant. They responded that one's social environment fulfilled this role, a move which directly fulfilled the first directive of Bloor's Strong Programme. Because the causative character of nature was limited, social causation was needed to undergird knowledge systems. Thus for Shapin and Schaffer, social factors stabilize facts, making them institutions. Let us look at how, according to Shapin and Schaffer, Boyle did this in his specific historical circumstances, particularly those of the Royal Society.

Boyle's membership in the Royal Society is a crucial element in Shapin and Schaffer's argument, and thus warrants some historical description. Starting circa 1645 primarily as an informal meeting of philosophers and thinkers at Gresham College in London, the Royal Society was meeting formally by 1660. It was granted Royal assent in 1662. Boyle, Henry Oldenburg, Christopher Wren and Robert Hooke were among its early members. Its appearance was part of a trend across Europe towards organizing natural philosophic groups during the seventeenth century, partially because universities tended to be inhospitably disposed towards the new, non-Aristotelian natural philosophy; the *Académie Royale des Sciences* in France, established in 1666, is another example. Despite the trappings of formality signified by the appellation "Royal," the Royal Society remained quite informal, and became popular with educated society at large. Many of its members had rather flimsy scientific credentials. Nonetheless, it served as a hub for the communication of seventeenth-century philosophic discovery and thought for much of Europe. The Royal Society was a place where those sympathetic to the experimental

natural philosophy could exchange views, see experiments performed, and find out what new discoveries were on the horizon.[17]

Shapin and Schaffer recast this interpretation in the following manner. To support his view of nature, Boyle established and led a community - the Royal Society - whose members would attest to the actuality and validity of the facts he espoused. In Shapin and Schaffer's interpretation, facts needed a human community to survive, and there was a strong parallel between social membership and epistemic conviction. Through appealing to the socially founded moral and cultural sensitivities of Society members, Boyle could recruit members to espouse his claims, for instance, about the spring of the air. In Shapin and Schaffer's terminology, Boyle used these social mores to design "technologies" to convince others of the worth of experimentalism; these technologies were the "literary technology" of his writings, the "material technology" of the air-pump, and a "social technology" that ordered and managed the experimentalist community. A humble and detailed writing style showed Boyle's unbiased, fastidious approach to knowledge-creation; the sophistication of the air-pump could be wielded flexibly to garner assent; and the avoidance of causal explanation fostered tolerance among experimentalists, who needed only admit the matters of fact which the pump presented them to gain membership.[18]

---

[17] Westfall, Construction of Modern Science, pp.105-114.

[18] Schaffer and Shapin, LAP, pp. 25-26, 65-69, 76-79.

The point about literary technology is relatively straightforward: Boyle used a particular argumentative style in his writing to instill confidence in his gentle audience. Shapin and Schaffer propounded this notion largely through the idea of "virtual witnessing."[19] If immediate observation of an experiment was the route to epistemic confidence, then a way was needed to communicate experiments far and wide - obviously not every possible observer could be present at the scene of the experiment itself. Boyle accomplished virtual witnessing by sprinkling his texts with engravings of experimental scenes, loading his pages with "prolix" prose, and comporting himself literarily as a modest and unpretentious author. His personal *modesty* appealed to the criteria which his social peers in the upper echelons used to discern the valid from the spurious. Being modest involved speaking without undue confidence and candour, cultivating a disinterestedness towards both one's own and others' philosophical claims, being civil and courteous to philosophic opponents, and piously following proper Protestant Christian belief. Importantly, the matter of fact was presented confidently, but causes cautiously. According to Shapin and Schaffer, Boyle intended to make the reader feel as if he were present at the experiment, and to build confidence in the reader through showing how experimentalist philosophers politely interacted with one another.[20] We will continue discussion of the social technology that the literary technology promoted later. For now,

---

[19] The concept of "virtual witnessing" is one to keep in mind, because it is the root of the more sophisticated notions of trust and credibility that Shapin later developed in AST.

[20] Schaffer and Shapin, LAP, pp. 55-72.

however, it is important to remember that via "literary technology" Shapin and Schaffer were pointing out the rhetorical character of Boyle's experimentalism, a feature they contended was a concomitant of any knowledge-making practice.

Because Boyle, and many subsequent historians, saw the experimentalist enterprise as hinging on tools like the air-pump, it is necessary to examine closely Shapin and Schaffer's concept of material technology. The air-pump was a physical entity of some engineering complexity. Its technical sophistication made it a rallying point for the experimental community - it was a symbol. Further, the fact that its design was not perfect - it constantly faced problems of leakage - gave Boyle an additional way to argue for and against claims. This was an important claim in LAP's approach, because for Shapin and Schaffer it showed the lack of conclusiveness in experiment. When Boyle deemed an experiment successful, he asserted that the pump worked; when an experiment failed, he often sought failure in some feature of the air-pump to redeem the knowledge claim and suggest that, had the pump worked correctly, then the results of the experiment would have matched expectations.[21] Shapin and Schaffer interpreted historical episodes recorded in Boyle's canon of published experiments to reveal how humans can interpret those experiments in any number of ways. The example of the Torricellian space and Hobbes' and Boyle's differing interpretations of it described above shows this contention. Boyle's belief was that promoting the proper literary and social technologies would lead to a

---

[21] Schaffer and Shapin, LAP, pp. 30, 44, 48, 180-181.

proper appreciation of the phenomena which the material technology of the air-pump exhibited experimentally.

Boyle's social technology, according to Shapin and Schaffer, aimed to promote consensus and peace within the experimental community. Central to this task was the matter of fact's dominance epistemologically, argumentatively, observationally, and conversationally; in Shapin and Schaffer's turn of phrase, it was the "currency of intellectual exchange."[22] By using matters of fact as the focal point for talk about nature, Boyle believed experimentalists would be protected from reproach; if their intellectual views were unpalatable, the matter of fact was at fault, not the human being. All that was needed was that each member accept the matter of fact - causal explanations could take whatever form they wished. With the possibility of personal attack removed, civility would reign, even in the face of disagreement about causes. Talk of causes was very worrisome for Boyle.[23] Shapin and Schaffer transformed a seemingly purely epistemological factor, Boyle's notion of the matter of fact, into a social factor as well.

Here the reader should take pause to consider what I argued earlier regarding the importance of society in Shapin's picture of science in LAP. Scientific knowledge, according to Shapin, owes its acceptance to social conventions. In this view, it is the human presentation of the claim that is more important than the actual epistemic content of the claim itself. Yet how is it that these social conventions are persuasive? The idea

---

[22] Schaffer and Shapin, LAP, p. 73.

[23] Schaffer and Shapin, LAP, pp. 65-69.

that knowledge gains shape and acceptance through social conventions needs to be linked to the third component idea of this reading of LAP: that (c) knowledge is intimately involved with the achievement of particular social and political aims. To do so, a number of steps are required. First, the reader needs a sense of the historical backdrop to Boyle's experimentalism. That backdrop was Restoration England. This then needs to be connected to Shapin and Schaffer's interpretation of experimentalism's political and social agendas. Next, a brief analysis of the particular features Shapin and Schaffer projected on society and their possible intellectual precursors in the thought of Karl Marx will indicate the sociological specificity of LAP's interpretation: the model of society in LAP was but one possible sociological approach. Finally, focus on the moral imperative in scientific knowledge that emerges from this sociological construal will be required, thus revealing a Shapinian mechanism constructed to meet the Strong Programme demand for showing the "social causation" behind knowledge claims. The social structure of Boyle's world made certain political goals imperative, and the force that guided the members of the community towards those goals was morality.

In LAP, Shapin and Schaffer honed in on how Boyle and Hobbes acted out their historical roles during the Restoration (1660-1685). Two key themes in this period were the fear of absolutist tyranny and the challenge of establishing religious tolerance. Taking place in the second quarter of the seventeenth century, the English Civil War, fought between Royalists and Parliamentarians, led to the regicide of Charles I in 1649 and the subsequent creation of the British Republic, divided into the Commonwealth (1649-1653)

and the Protectorate (1653-1660) periods with Oliver Cromwell the leading political and military figure. This was followed by the reestablishment of the Stuart dynasty, with Charles II taking the throne in May 1660. As one historian has commented, the Restoration was a difficult time, for it was an attempt to reestablish a monarchical system in a society that had spilled much blood to get away from just such a system. In particular, one of the great fears driving the Parliamentarians in the Civil War had been the perceived absolutism of Charles I and his system of "Personal Rule," a system which bypassed and evaded the English Parliament. Therefore, Charles II needed to maintain great respect for Parliament and handle carefully the monarch-Parliament relationship. In general, the political culture was tense, with the astute Charles II keeping the situation contained. Further, a key flash point throughout the seventeenth century was the issue of religion, particularly the management of the variety of Protestant sentiment within the Church of England and dealing with the widespread fear of Roman Catholicism. Fear of absolutism and varying attitudes toward religious pluralism cut across society; for instance, both the Parliamentarian and Royalist camps in the Civil War consisted of a mix of nobility, gentry and other social and economic classes. Religious sentiment fired both camps, and the proliferation of religious fundamentalism was seen as one reason why civil war had raked the country.[24]

---

[24] See David L. Smith, A History of the British Isles: The Double Crown, (Oxford: Blackwell, 1998): passim.

It was in the political and social milieu of Restoration England that Shapin and Schaffer's answer for Boyle's antipathy to causal explanation lay. They provided the following interpretation. Boyle was a noble. During the Civil War, the nobility had been torn into those who wanted the kingship abolished and those who supported absolutist royal authority. Many members of the nobility realized that their social status was predicated on a monarchical society, but realized too that the power they enjoyed could exist only under a king whose rule was less than absolute. Further, the nobility was also deeply frightened by the rifts which religious debate had caused in English society. They wanted to maintain their Christianity, while avoiding religious divisions. So, to preserve their social status the nobility had three key interests: have a king on the English throne; make sure the king did not exercise absolute power; and restore the power of the Church. Shapin and Schaffer argued that Boyle was devoted to these interests.[25]

In this context, Shapin and Schaffer keyed their discussion on the issue of dissent. The political and religious leadership of the nobility perceived that the strife of the Civil War originated mainly in the proliferation of "subjects' beliefs."[26] Differing views of religion and politics led to radicalism, which in turn led to strife. Nobles like Boyle strove to find a way to accommodate differing opinions, but within confines that would prevent civil conflict from erupting. To this end, ideas of discipline and limited forms of tolerance became popular among some segments of the nobility. Variations in Protestant Christian

---

[25] Schaffer and Shapin, LAP, Chapter VII, passim.

[26] Schaffer and Shapin, LAP, p. 298.

belief, for instance, as long as they exhibited a general acceptance of God's existence and authority, as well as a hatred of Catholicism, were permissible. There was space for a degree of pluralism within the *Protestant* religious community. Under certain restrictions, a variety of opinions were permissible. If individuals did not accept a Protestant God, however, they were banished from discussion and their ideas suppressed.[27]

According to Shapin and Schaffer, experimentalism and its accompanying doctrine of probabilism worked in this direction. Like the relaxed confirmism demanded of the individual vis-à-vis a Protestant God, the experimentalist demanded only that the individual acknowledge the matter of fact as the supreme element in knowledge. Boyle asserted that nature was God's creation, and that those things witnessed in experiment were expressions of his will. Shapin and Schaffer maintained that Boyle and his fellows were trying to move agency away from human individuals to a divinely created physical world. This move founded Boyle's causal pluralism. According to his reasoning, God could have structured a plethora of causes behind one perceived effect - only He knew the causes. Humans were not to step on His epistemological turf by suggesting that they had absolute knowledge of causes. So Boylean experimentalists were to avoid ardent attachment to causes, for such had brought about the internecine conflagrations of seventeenth-century Britain. It simply was not the experimentalist's duty to advance causal argument in his work (although he might indulge himself in such speculation in his

---

[27] Schaffer and Shapin, LAP, pp. 283-284, 289-290, 298-310. Also see "Latitudinarianism," The Oxford Dictionary of the Christian Church, ed. F.L. Cross (Oxford: Oxford University, 1997).

spare time). The experimentalist's fear of absolutism in knowledge was an epistemological parallel to the noble's fear of the king wielding absolute power. Such power removed the possibility of the way of life of the nobility. For Shapin and Schaffer, this correlation between experimentalist rhetoric and the aristocratic social propaganda was more than just a coincidence: experimentalism was a "solution to the problem of social order." By articulating and enforcing a doctrine that placed the ultimate power over human ends outside humans in God and His Creation, the nobility could defeat the idea that individual opinion should guide behaviour. Experimentalism was to show that political stability and diversity of opinion could peacefully coexist. In short, "[n]o isolated powerful individual authority should impose belief" on the philosophical community or on the political community. Boyle had specific political goals that would protect his social position, and his experimentalist philosophy served those ends.[28]

Consider some features of Shapin and Schaffer's strife-ridden society, where historical agents struggle to maintain or overthrow "social position" and advance "political goals." In its most basic characteristics, it is tense, competitive, and antagonistic. The antagonism is founded on groups of individuals who share common social origins and therefore strive for common political ends; the existence of different social groups holding differing levels of political power and influence fires this antagonism. Importantly, the tension and strife of this Shapinian-Schafferian society emerges from the battle for political dominance. Knowledge is but one component in a

---

[28] Schaffer and Shapin, LAP, pp. 139-140, 147, 298-319; quote p.298.

larger social-political battlefield, and for Shapin and Schaffer its vicissitudes and development melded cleanly into the large-scale backdrop. Although Shapin mainly forged his thought under Strong Programme tutelage, the sociology of knowledge pedigree goes back further yet (and beyond the scope of this thesis) at least to Karl Mannheim, and through him to the influence of Karl Marx, who Isaiah Berlin called the "true father . . . of modern sociology."[29] This observation is relevant because it speaks profoundly to the character of the project Shapin and Schaffer set themselves and shows the specific tradition in which their understanding of sociology operated. Two points need to be made to draw out the significance of this observation: first, a basic examination of the relationship between society and thought in the Marxian universe; and second, a general, circumstantial argument that Shapin was well positioned to be influenced by this atmosphere of Marxist critique during the formative years of his intellectual training.

Following the argument in Chapter 2 that the efforts of Kuhn, the Strong Programme, and Shapin were directed towards giving history, via sociology, a central role in understanding science, then it should be no surprise that the imprint of Marx is apparent in a book such as LAP. Marx's sociological doctrine of "historical materialism" gave a huge push to social history, particularly in drawing attention to the influence of economics on the social structure. The most important point of this doctrine is that those who control economic power - often referred to as the "means of production" - form a

---

[29] Isaiah Berlin, Karl Marx: His Life and Environment, 3rd ed. (New York: Time, 1963): p. 130.

social group or "class" which in turn controls political power; in the case of capitalism, the economic system which Marx identified as currently obtaining, the "bourgeoisie" was the group in command. Those who were outside of this class were the exploited masses, a "proletariat" (in the capitalist phase) who the bourgeoisie used for their labour and the accumulation of wealth. The social-political structure in this model is completely economically determined, a fact which is of the utmost relevance to those interested in the history of ideas. In short (and grossly simplified for my purposes here), Marx saw the play of economics and class as the material of history, whereas the thought of human beings formed a "superstructure" atop this material foundation. For Marx, the majority of this thought was the imposition of the rationalizations or "mystifications" of the ruling elite on the masses in order to stabilize the contemporary economic order of things in their (the dominant class's) favour; combined with the ruling class's political institutions, habits, morals and other ways of living, a whole "ideology" existed that, if adopted universally, would allow the ruling class to maintain its ascendence. The oppressed classes needed to recognize the economic system that determined their position in society and their ensuing exploitation if they were to build an intellectual system - i.e. one based on the dictates of Marxism itself - that would aid them in fomenting revolution and overthrowing their oppressors. In this schema, then, ideas have little value in and of themselves, but are expressions of the desire to gain or maintain power, depending on one's social location.[30]

---

[30] Berlin, Marx, pp. 101-130. It should be pointed out that Marx primarily attacked economics, the tool bourgeois intellectuals applied most ruthlessly or ineptly (depending on their consciousness of the nature of capitalism) to extend their mental sway; he saw

It seems that Marx did not intend this analysis to be applied to science immoderately, as he cited science and human rational faculties as tools in the struggle against exploitation and class dominance.[31] However, the universality with which he applied the above critique to all other elements of bourgeois culture meant that, in the hands of sociologists of knowledge, science - an activity plied primarily by the moneyed classes, those with the leisure time to invest in such investigations - too might prove amenable to such a critique. I do not want to say Shapin or even the Strong Programme therefore practised a strict Marxian form of history; reading more vulgar Marxian accounts, such as a Boris Hessen, promptly makes it clear that the Strong Programme saw greater variety in society than simply two predominant classes of oppressed and oppressors.[32] However, the parallels are striking: Boyle, a member of Restoration England's power elite, attempted to buttress institutions, such as the Church and noble

---

other fields of knowledge as less significant in capitalist exploitation. For some interpreters, this meant that the dominant *mentalité* did not affect all fields of intellectual endeavour equally profoundly - those farther from the means of controlling the system were less touched by class politics and consciousness. Such a remark has bearing on how deeply the social structure might affect esoteric fields of science. See Robert J. Antonio, "Materialism," Encyclopedia of Sociology, Vol. 3, 2nd ed., eds. Edgar F. Borgotta and Rhonda J. V. Montgomery (New York: Macmillan, 2000): p. 1783.

[31] Berlin, Marx, p. 116. This thrust in Marx's thinking was somewhat countered by a conviction that bourgeois culture could not but affect science in some way. See Richard W. Miller, "Marxist Philosophy of Science," Routledge Encyclopedia of Philosophy, Vol. 6, Ed. Edward Craig (London: Routledge, 1998): p. 147.

[32] Boris Hessen, The Social and Economic Roots of Newton's 'Principia' (New York: Howard Fertig, 1971): passim. (Originally published in Science at the Cross Roads, 1931).

privilege, that advanced his material interests through his natural philosophy; whereas Hobbes, on the other hand, used his natural philosophic ideas to attack those institutions from which his social standing disbarred him. Most importantly, a sort of Marxian antagonism obtained throughout LAP, as a gutsy, no-holds-barred attitude reigned between the various groups of society. What can be said comfortably is that Shapin and Schaffer were inspired by a Marxian ethic in LAP.[33]

Such inspiration was readily available during Shapin's formative intellectual years. Circumstantial evidence for this is clear in Robert Young's call for a "radical" historiography of science in an essay that implicitly sheds some light on the origins of Shapin's sociological thinking. As Young pointed out, many intellectuals in the Western democracies questioned the liberal institutions around them during the late 1960s and 1970s. Shapin's intellectual mentality springs from this milieu: he completed his graduate work in the early 1970s; further, in both in LAP and elsewhere, he described science as a liberal institution, and described some of the influences to which he turned to develop an intellectual standpoint to express his dissatisfaction with contemporary science.[34] Young

---

[33] It is also worth noting that Marx's belief in the materialistic determination of human thought - that humanity's material conditions determine their consciousness and not vice versa - also provides some mechanism for the Bloorian notion of "social causation." For Marx, the superstructures of human thought that grow atop the social-economic foundations of human existence could arise either consciously or unconsciously. Perhaps to some degree, Bloor and subsequently Shapin failed to articulate a mechanism for social causation as they presupposed such a materialist social influence on human thought. See Berlin, Marx, p. 110.

[34] For instance, see the remarks questioning liberal institutions in LAP, pp. 343-344, and his remarks in the foreward to Daniel S. Greenburg, The Politics of Pure

also identified the Marxian origins of much socially founded history of science, and announced the need that existed in the 1970s for a more sophisticated articulation of how class and society, traditionally framed as external factors, affected the actual contents of scientific knowledge.[35] The Strong Programme and Shapin's subsequent scholarship were responses to the same urge.[36] Shapin's earlier scholarship handled notions of class and class struggle much more explicitly,[37] and provides grounds for seeing LAP in a similar light. What resulted in LAP were social groups that resembled social classes. For science was a result of social struggle, whether it was between Hobbes and Boyle, Mechanic's Institutes and a wayward proletariat, or moral philosophers and radical phrenologists.[38]

Science, New edition (Chicago: University of Chicago, 1999): pp. xv-xxi, particularly xv, xvii-xviii, xx-xxi.

[35] Robert M. Young, Darwin's Metaphor: Nature's Place in Victorian Culture (Cambridge: Cambridge University, 1985): pp. 164-247, and particularly pp. 212-213 for Young's comments on scholarly doubts regarding liberal institutions, and pp. 170, 200-202 for his words on the need for new historiographical tools.

[36] Should any doubt exist on this head, please note Barnes' sympathetic treatment of Marx and Marxian notions of social structure in Interests and the Growth of Knowledge (London: Routledge and K. Paul, 1977): p. 47; and also Young's comments in Darwin's Metaphor, pp. 244-245, on the Marxist potential in the writings of Mary Douglas, to which the Strong Programme often turned for motivation and leadership.

[37] For instance, see Barry Barnes and Steven Shapin, "Science, Nature and Control: Interpreting Mechanics' Institutes," Social Studies in Science 7 (1977): passim, or Steven Shapin, "The Pottery Philosophical Society, 1819-1835: An Examination of the Cultural Uses of Provincial Science," Science Studies 2 (1972): passim.

[38] The criticisms of G.N. Cantor are particularly apposite to understanding the social theory underlying Shapin's work on phrenology, and this can be extended to LAP. See Cantor, "A Critique of Shapin's Social Interpretation of the Edinburgh Phrenological Debate," Annals of Science 33 (1975): pp. 245-256.

Shapinian scientists' views of the natural world were mediated by social status. Shapin was not an explicit Marxist, but the ideas that he developed in LAP reveal a Marxian tincture.

LAP resembled the work of social and intellectual historian Christopher Hill in this regard. Hill's scholarship showed how the seventeenth century in England was populated with all sorts of exotic-sounding political entities struggling to gain supremacy in the conditions of bourgeois revolution. Diggers, Fifth Monarchists, Levellers, Presbyters, Anabaptists - all struggled to defend their social interests.[39] Shapin and Schaffer added the experimentalists to this list.[40] Many of Shapin's earlier writings had discussed how scientific societies directly expressed class interests in the setting of social and economic tumult. The middle class of North Staffordshire in the early nineteenth century gathered together under the aegis of the Pottery Philosophical Society to talk science, a hobby which they hoped would emphasize and enliven their leisured existence. Science let them focus their minds on God's wondrous nature, while also allowing them to learn about new ways to build wealth through scientific innovation. It also socialized the middle class, giving them a forum to muster their strength against the old-guard of

---

[39] Christopher Hill, The World Turned Upside Down: Radical Ideas During the English Revolution (New York: Viking, 1972): passim; and Intellectual Origins of the English Revolution (Oxford: Clarendon, 1965): passim.

[40] The origins of this placement are visible in the writings of James R. Jacob and Margaret Jacob. See, for example, J. R. Jacob, "The Ideological Origins of Robert Boyle's Natural Philosophy," Journal of European Studies 2 (1972): passim.

aristocrats.[41] Similarly, the middle class, from around 1825 onwards, established "Mechanics' Institutes" to focus, control and harness the working class. Science would smooth out the rough edges of the hard-drinking and morally derelict proletariat, improve their productivity, and keep them distracted from movements that sought to work them into a revolutionary, anti-bourgeois fervour.[42] In LAP, the Royal Society took on a similar role. It was a rallying point for the Protestant gentry and nobility. It served as a vehicle for the promotion of the ideals that protected their social, economic and political position. Science grew out of social needs, and the model of social needs Shapin and Schaffer devised gave precedence to the role of social strife and tension between economically differentiated social groups.

With this background, Shapin saw an interconnection between philosophical/scientific systems and social-political stratagems. Historians unsympathetic to a sociological history of science might describe such parallels as mere coincidence and classify them as external factors that did not influence (in the case of true knowledge claims) the internal epistemic content of science, as was discussed in Chapter 2.[43] Shapin, however, was inclined to see them as more substantial. Building on the work of social and cultural anthropologists like Mary Douglas, Shapin turned the coincidence interpretation

---

[41] Shapin, "Pottery Philosophical Society," pp. 311-315, 318-320, 328, 335-336.

[42] Barnes and Shapin, "Interpreting Mechanics' Institutes," pp. 38, 40.

[43] An example of the "arationality" principle at work, with which the symmetry postulate of Bloor's four tenets of the Strong Programme aimed to deal.

on its head and turned parallels into "homologies:" such parallels were not coincidences, but shared a common social origin. Because of this, such homologies between philosophy and social identity should be expected. Doing science, in his view, was a way of developing and reinforcing one's social image. Through making particular knowledge claims, usually in opposition to some other group's beliefs, the social group solidified their unity. It was very much a team-versus-team view, and knowledge exercises like science aimed at reshaping the world in the image of each social group's assumptions about what the world was like, and how human beings should live in regard to that reality.[44]

This particular sociological background and theoretical equipment convinced Shapin and Schaffer of the third component we are analysing: that science was a social and political tool. This allowed them to argue that Boyle's experimentalism had an explicit political and moral dimension. The importance of this must be emphasized, for it is the clearest manifestation of Bloor's tenet of social causation in human knowledge that was mentioned in Chapter 2. Shapin and Schaffer argued that Boyle construed his material, literary and social technologies in such a way as to make it a moral responsibility for Boyle's audience to acquiesce to the matter of fact. This morality was shaped around the cultural mores of the noble Boyle and his socially elite peers. Thus, those most likely to accept the matter of fact would come from Boyle's social class. Boyle

---

[44] Steven Shapin, "Homo phrenologicus: Anthropological Perspectives on an Historical Problem," Natural Order: Historical Studies in Scientific Culture, eds. Steven Shapin and Barry Barnes (Beverly Hills: Sage, 1979): pp. 46, 60.

had active political goals; experimentalism expressed these goals; those who shared Boyle's standards of moral comportment would accept the social conventions of his philosophical clique, thus making the experimentally generated matter of fact the epistemic champion of the nobility. Thus united, the nobility could carve out a "calm space"[45] within Restoration society. The resulting tranquillity was to be both epistemological and social. Boyle the philosopher could continue to make his knowledge-claims, unthreatened by pesky non-experimentalists like Hobbes, while Boyle the aristocrat could continue to live his privileged material existence, without being subject to the overthrow of the mob or the stifling absolutism of the king.[46]

Hobbes' opposition to Boylean experimentalism was cast in terms of social causation as well. He was an outsider, whose ideas the nobles widely scorned as those of an atheist, a materialist and a threat to the peace. To call someone a "Hobbist" in the decades following the Civil War in England was a grave accusation.[47] According to Shapin and Schaffer, Hobbes opposed Boyle on matters of the vacuum and causation of visible phenomena because he was not part of Boyle's social set, and therefore did not share his social interests. Most poignant for Shapin and Schaffer was Hobbes' tireless fight against Church power in the secular political sphere. Hobbes contended that peace would only come once the king ruled absolutely and all obeyed his will. For Shapin and

---

[45] Schaffer and Shapin, LAP, p. 76.

[46] Schaffer and Shapin, LAP, Chapter VII, passim.

[47] Smith, Double Crown, p. 240.

Schaffer's Hobbes, there could be no variation of opinion on how to rule if one wanted peace, and variation could be avoided through universal submission to one ruler. Hobbes had witnessed Church figures attempting to gain earthly political power during the Civil War and after. Thus, Boyle's attempt to give the weakened Church a new crutch through experiment was anathema to Hobbes. According to Shapin and Schaffer, Hobbes invested the monarch with absolute power in his ideal polity - even the power to determine the structure of knowledge of nature. While Boyle put human fate in the hands of God, spirits and other unearthly, immaterial entities, Hobbes put the prosperity and hope of humanity in the hands of its ruler. Hobbes subjected theology and natural philosophy to the philosophy of the polity. Hobbes saw all knowledge as human-made, and a ruler, once properly invested with absolute authority and the complete submission of the people, could establish a natural philosophy to explain all causes which would brook no dissent, for to do so would be to challenge the unchallengeable authority of the king. Human knowledge of the natural world depended on civic order, and civic order would never emanate from the natural world, only from the social world. Knowledge was a product of society - society was not a product of knowledge. This hierarchy was a central ingredient of Shapin and Schaffer's view of scientific knowledge.[48]

To help elucidate the consequences of this interpretation, a step-by-step recipe for Shapinian knowledge-making in LAP is useful. First, humans have social needs, which they struggle to achieve through creating political systems; they are thus goal-oriented

---

[48] Schaffer and Shapin, LAP, pp. 92-107, 150-154, 310-319, 320-331.

creatures, and this goal-orientation takes shape in a context where different groups of people hold different levels of power and wealth. The second stage in the process is that each group constructs its particular knowledge systems from the views it has about the world and how humans fit into it. Knowledge is formed to help achieve these social and political ends. Each group within society has conventions about the way society and nature are/should be. These "cultures of conventions" shape behaviour, and it is on this basis that members within that culture build and accept what they take as valid knowledge. This gives knowledge its conventional character - a knowledge claim must reflect these conventions, or risk being ignored. The end of the process is reached when the sponsoring social clique wins out in the social-struggle lottery, then its system of knowledge becomes an institution, its claims becoming "reality." Its survival is a result of the success of the social group that propounded it. The role of nature is completely secondary, even seemingly inconsequential, in the success of a knowledge claim in the pages of LAP. Knowledge for Shapin and Schaffer was a social rallying point, somewhat similar to a team sport. Social needs determined epistemic choices. Like other brands of knowledge - philosophical, religious, folkloric or mythological - features of science such as truth and objectivity, are seen as "accomplishments, as historical products, as actors' judgements and categories."[49] Shapinian-Schafferian science has little to distinguish it as a unique and privileged form of human epistemic activity.

---

[49] Schaffer and Shapin, LAP, p. 14.

3.3 LAP's Image of Science: A Critical Appraisal

It becomes increasingly evident when reading LAP that Shapin and Schaffer wanted to show that the challenges Hobbes presented to Boyle's experimentalism are fundamental to all knowledge at all times, in all places, and in all cultures: knowledge is a constellation of social conventions that human agents use as a tool to accomplish social ends. This idea appears explicitly in the final chapter.[50] To better approach this conclusion, it is worth examining the core ideas of LAP critically.

In portraying experiment as an inconclusive way of knowing, Shapin made a philosophical claim.[51] Thus, it is appropriate to subject it to some philosophical consideration. To that end, the notions of "ontological equivalency" and "epistemological sensitivity" will be introduced to throw light on the problem. What will appear is that social factors have varying degrees of influence on scientific problems, depending on the very nature of the question being asked and object being studied. With the inconclusiveness of experiment thus amended, I will proceed to a critical examination of the sociological component of Shapin and Schaffer's work. A number of problems with fusing the social and the epistemic will be brought out. I will contend that Shapin's portrayal of society is limited by his tendency to see social factors as largely strife-ridden confrontations between different social communities. Then I will examine the social

---

[50] Schaffer and Shapin, LAP, Chapter VIII, passim.

[51] Michael Friedman, "On the Sociology of Scientific Knowledge and its Philosophical Agenda," Studies in History and Philosophy of Science 29 (1998): pp. 239-242. Please see Chapter 2 for more details.

environments which Boyle and, in particular, Hobbes faced to show the difficulties of applying sociological explanations to all epistemic items. In the end, it will be seen that intellectual obstinance - the human agent's unwillingness to alter epistemic viewpoints based on his or here social interests - is fundamental to the world view Shapin and Schaffer espoused in LAP.

First there is the issue of the epistemological inconclusiveness of experiment. One of the key argumentative themes of LAP was the disagreement between Boyle and Hobbes over the use of experiment in philosophy; this disagreement involved both the interpretation of specific experiments and the legitimacy of experiment as a general route to securing knowledge of the physical universe. The results Boyle extracted from his experimental handling of the Torricellian space were not the same as Hobbes'. According to Shapin and Schaffer, because Hobbes demanded that causes of phenomena be posited, whereas Boyle felt the search for causes led to ungovernable dispute, they approached the interpretation of experiments differently, and therefore differing interpretations followed. Thus, Hobbes argued that the air-pump could never be conceived as making a vacuum because his natural philosophic system did not allow for such; Boyle, on the other hand, found the plenum-vacuum debate tiresome and divisive, and therefore cast his interpretation in terms of the spring of the air, pushing aside final answers on the existence or non-existence of the vacuum. More generally, and more profoundly, Shapin and Schaffer argued that Hobbes, based on a familiarity with experiment and his own causally based method, saw experiment as something distinct from philosophy. For them,

"[w]hat Hobbes was claiming was that the systematic doing of experiments was not to be equated with philosophy: going on in the way Boyle recommended for experimentalists was not the same thing as philosophical practice."[52] Thus, hypothetically, even if Boyle and Hobbes had agreed on an interpretation of some experiment, such agreement would have been limited, as Hobbes saw the role of experiment in deciding natural philosophic debates as limited.

What the reader needs to take into consideration when examining this theme is Shapin's Strong Programme background. He and Schaffer were looking at knowledge as a historical, sociological phenomenon: if a group of people believed something, then it could count as knowledge, and thus the historian could probe the social foundations of the group to establish historical interpretations of that knowledge system. The historiographical consequence of this approach was a broadening of the historian's scope. Previously ignored scientific arguments or systems, such as Hobbes', were in fact relevant to the history of science. From this, the idea that experiment was inconclusive followed almost axiomatically, for on sociological grounds the mere persistence of a group's objections to a knowledge claim would constitute grounds for deeming the claim indecisive.

With this in mind, the question of the necessity of philosophical considerations in historiographical practice arises. For Shapin and Schaffer, as well as their Strong Programme mentors, philosophy was too inflexible to be of historiographical value.

---

[52] Schaffer and Shapin, LAP, p. 129.

Knowledge systems vary with place and time, yet the general thrust of much philosophy of science was to show why one particular set of knowledge claims is better than others. This results in a chauvinism for those who have inherited the culture that such a philosophy of science aims to justify. Shapin and Schaffer addressed this problem eloquently in their talk of "member's" versus "stranger's accounts."[53] Yet it is not so clear that profoundly philosophical considerations need be so inflexible. In fact, they need be no more timeless and universal than the claim that all knowledge can be analyzed in terms of the social environments where it was born. Such considerations might integrate with a moderated sociological approach and result in a powerful form of expression for the history of science.

Those sympathetic to Shapin and Schaffer's sociological approach might not see the need for such philosophical revision. Nonetheless, it is required, if only to moderate the seemingly inevitable conclusion that the authors intended their readership to reach: Boyle's natural philosophic project survived because of the social preeminence of his allies and the support they gave him. Is this in fact the only reason? Anticipating the Strong Programme qualification that social causes are not the only causes, can it therefore be said that such social factors were, as Shapin and Schaffer intended, the primary reasons? What will be seen is that a pair of philosophically devised historiographical tools, "ontological equivalence" and "epistemological sensitivity," suggest a significant

---

[53] Schaffer and Shapin, LAP, pp. 4-7. This clearly speaks to the impartiality requirement of the Strong Programme - see Chapter 2.

role for epistemological factors. Consider Boyle's and Hobbes' differing interpretations of the Torricellian space. Could Hobbes' rejected theory on the Torricellian space be granted the same legitimacy as Boyle's? Certainly from a purely scientific standpoint this question poses difficulties, as debates of the plenum versus the vacuum have yet to be answered, and, counter to Boyle's exhortations, are crucial, for instance, to twenty-first-century discussions of cosmology.[54]

To start the argument, I will put the problem this way: both Hobbes and Boyle are looking at the same historical event "before their eyes," so to speak.[55] Yet their interpretations of that event differ. Shapin and Schaffer argued on sociological grounds that Boyle and Hobbes had different social aims and backgrounds, which resulted in different natural philosophical outlooks, and which therefore ultimately ended in different interpretations of what they have witnessed.

Can their differences be seen in another light? Yes, and this is where the notion of "ontological equivalence" comes in. This notion turns on the idea that the experience of an event and the interpretation of it are two ontologically distinct entities; put simply, one

---

[54] Consider, for instance, the concept of "vacuum energy;" see Peter Coles, Cosmology: A Very Short Introduction (Oxford: Oxford University, 2001): pp. 91-92.

[55] Historically, of course, Hobbes was not present at any such exercise of the air-pump, but for argumentative purposes, this need not concern us, for as Shapin and Schaffer argued, Hobbes did have a very good understanding of experimental practice and had taken considerable pains to familiarize himself with Boyle's work; even had he been present, Shapin and Schaffer's interpretation suggests they still would have interpreted the experiment differently, because of their different natural philosophic beliefs and political agendas.

is a mass of organized sensory data, whereas the other is a linguistic assemblage, which for the sake of convenience may be labelled description. Description is a different sort of thing than the sensory experience from which it derives. Consider a seemingly simple example: no one would mistaken a poem about a flower for the flower it aims to describe - they are obviously two different entities, whatever their descriptive interrelationships. But what about a scientific description of that flower? It seems often that such a scientific description is taken to describe what a flower is, and is not simply seen as a reaction to a flower. However, from the perspective of ontological equivalence, it as much an entity distinct from the flower itself as the poem. A example from LAP will help bring this point forward. For instance, consider Shapin's insistence that acquiescence to Boyle's spring of the air was not the same as observing the changing level of the mercury in the void-in-the-void experiment. This is sound because to observe something is not the same as describing what is observed. In effect, it places a "gap" between thing interpreted and interpretation, a gap which human agents attempt to bridge with descriptions. Such a gap precludes a final, conclusive association of one interpretation with one event; in other words, the lack of ontological equivalence between the two things means that the thing observed does not inexorably lead to a particular interpretation of it, but permits a multiplicity of plausible descriptions. To some degree, this too can explain the epistemic variety evident in human history - sociology is not alone in this regard. Further, the problem of the lack of ontological equivalence between the thing observed and the linguistically couched description of that observational experience gives credence to

Shapin's challenge to Boyle's experiment-theory arrangement, but not solely on sociological grounds: the consequence is that the gap between observation and description may be seen as a universal feature of human experience, not reducible simply to sociological terms, and therefore open to other forms of analysis such as normative, philosophical analysis. Finally, and not least, the reader must be wary about Shapin and Schaffer's intentions - were they saying, or at times intimating, that the scientific endeavour was political endeavour? Such a thesis would be untenable for anyone who takes the notion of ontological equivalence seriously, and certainly would not privilege sociology over other intellectual tools.

What needs to be seen next is whether the gap between the thing interpreted and its interpretation itself might vary. For if, to invoke a spatial metaphor, the gap between the two can vary in distance, then maybe one might plausibly state that one interpretation better fits the event being interpreted than another. It is on this head that the shortcomings of Shapin and Schaffer's approach become manifest. This is because the Strong Programme in its own way accepts such a lack of ontological equivalence, but takes it for granted that it has the same effect on all systems of knowledge, i.e. rendering them all equally open to redefinition as bodies of collectively held belief that can be compared to one another in terms of the power of the social groups that prop them up. Yet if the size of gap between thing interpreted and an interpretation of that thing varies, then possibly the Strong Programme sociological approach might have run into a serious limitation. Such a variance in distance might be intellectually appreciable for human agents, and they may

take closeness or distance as further means to examine a claim and decide upon its worth. Such a critical ability on the part of human individuals would result in a severe reduction of the potency of purely sociological interpretation of the history of science.

My intent is not to resurrect a dichotomy between the social and the intellectual. In fact, it is my contention that it is Shapin and Schaffer who have erected such a barrier. To put this in sharper terms, it seems Shapin and Schaffer saw nature as having a very limited rôle in the generation of ideas. Consider it this way: nature presents a wonderful variety of events, but clearly there are boundaries to its experiential bounty. For instance, objects dropped on planet Earth do not fall up, they fall down (helium-filled balloons excepted); humans cannot live under the sea without technical aids; the sun rises and sets without consulting humans; whether it rains or shines is not my decision, nor yours. Additionally, human intellectual and sensory equipment do not determine the course of nature, although human technologies may have a limited or haphazard effect (potentially disastrous, as in the case of climate change or nuclear war) on nature. To put all agency in the creative hands of human communities seems gratuitous - mundane experience makes humans poignantly aware that nature lays constraints upon the descriptions observers generate, and the interface of humanity with nature seems to be at least as significant as the interface between humans in creating knowledge. Something, some external agency, is required upon which human descriptive powers may set to work. However, the lack of ontological equivalence between description and thing-described seemingly leaves the field open for draining reality of its epistemic significance: if no description can ever

exhaustively account for some phenomenon or entity, if there is not some one description which nature generates for human inquisitiveness to uncover, then how can one description, perhaps a *scientific* description, be said to be better or worse than others?

One way to look at this is whether the interpretation is attempting to answer some question, and if so, what sort of question. For example, someone may want to know what is the relationship between the movement of the sun and the planets. It seems clear that the best candidate response to this question is that the planets orbit the sun. However, a poem that places the Earth at the centre of the solar system, even of the universe, is not wrong in any simple way, unless it pretends to answer questions of celestial dynamics. Not all descriptions are equal in all circumstances - they all attempt to satisfy different human questions, different human longings. The absence of ontological equivalence between experience and interpretation provides humanity with an almost infinite capacity to interpret; this does not mean that it provides humanity with infinite experiential variety. One consequence of this for the historian is that he or she must determine exactly what the historical communities under examination were after. Sociological examination obviously has some lucid things to say on this head; for instance, on levels ranging from the surreptitious to the blatant, Boyle and Hobbes may have been attempting to use knowledge to achieve different social ends. However, on a more obvious level, it seems Hobbes and Boyle were likely after the same thing: the best possible explanation of the experiment in order to best describe natural phenomena. Achieving such an explanation in and of itself might help achieve other, associated goals; nonetheless, historians need to

recognize that in terms of human knowledge, Hobbes and Boyle had set themselves the same task, and therefore epistemic variety was not their aim, but a convincing, persuasive solution to a question about nature.

Therefore, to supplement ontological equivalence a conception of "epistemological sensitivity," is required. This is basically a philosophical idiom with historiographical application that gives historians of science a way to deal with the breach between the historical event and its interpretation which the lack of ontological equivalence opens up. There are varying degrees of descriptive persuasiveness and conclusiveness. Consider again the description that says the known planets in our solar system orbit the sun. If one wants to know the interaction between the planets and the sun, this is the best description going. Others are possible and even useful. This description is not obvious, depending on sophisticated theoretical and observational techniques. Yet it is ludicrous to suggest its validity resides primarily in the power of a particular group of people to enforce it upon other groups. Or, from another angle, to say that the solar system, including the sun, orbits the Earth is just plain wrong. Using a number of techniques, theories, observations, and ultimately arguments, all but the most obstinate humans could be shown the accuracy of this description. In other words, the description is not extremely epistemologically sensitive.[56]

---

[56] The work of Otto Neugebauer provides a good reference to this issue. His exploration of ancient astronomy and mathematics, and the numerous descriptions of the heavens which are still accurate to this date, stands as the ultimate example of the persistence of certain elements of knowledge through long stretches of history and across a broad variety of cultures, and indeed throughout a variety of differing astronomical

Other elements of the current scientific outlook are not so secure. Acceptance of the Big Bang theory, for instance, is contested within the field of cosmology, and this very fact suggests that the relationship between available experimental and observational data and the theory are plagued by a rather serious ontological inequivalence. This is not simply a lack of data, however, as with any such large-scale event - large scale in both its occurrence in time and in overall cosmological significance - there is almost always going to be the possibility for serious disagreement over fundamental definitions, entities and so on. Thus, there are numerous sound strategies for arguing against the Big Bang in ways that there are not for arguing against a heliocentric solar system. It seems very clear that the conclusion that the earth orbits the sun will not change, no matter what crises the physics community experiences in coming decades; however, the survival of the Big Bang theory as it is now understood given, say, the disavowal of quantum physics as a theoretical cornerstone, is very questionable indeed. The case is similar with genetics, plate tectonics, and evolution by natural selection, each exposing varying degrees of epistemological sensitivity. In this case there is a relatively high level of epistemological sensitivity - without the theory, the "matters of fact," as Boyle would have called them, would dissolve. The matter of fact of the planets orbiting the sun is not so epistemologically sensitive.

---

systems. His works, however, do not negate the possibility of these different elements being imbued with different meanings at different times and places. See O. Neugebauer, The Exact Sciences in Antiquity, 2nd ed. (New York: Harper, 1962): passim.

Applied to the interpretation found in LAP, epistemological sensitivity casts light on issues of matter theory which Boyle and Hobbes contested. Indeed, they prove to be very epistemologically sensitive. The entities with which Hobbes and Boyle took the universe to be populated - corpuscles, fluid ethers, springs of the air and so on - did not describe anything readily observable. You see the sun in the sky and stand on the Earth regardless of their interrelations, but one could never hold the seventeenth-century mechanist's corpuscle in one's hand, or even see it under a microscope. The spring of the air, the fluid ether - these were extrapolations, highly dependent on a particular theory that attaches to observations in the laboratory. This point must be emphasized: no seventeenth-century natural philosopher literally saw (to employ a visual metaphor) the spring of the air or a corpuscle.[57] Thus, matter theory is very epistemologically sensitive: the niceties of theory impinge far more heavily upon matter theory than they do, for instance, on positing a sun-orbiting solar system.

Concerning the Torricellian space, Boyle and Hobbes were dealing with an epistemologically sensitive problem; it is laudable for historians of science to not side with Boyle as "obviously correct" in the debate - Boyle was not "obviously" correct. However, epistemological sensitivity suggests that something more was going on than the simple imposition of Boylean natural philosophic conclusions on the populace due to the power of his social clique vis-à-vis Hobbes'. The niceties differentiating Hobbes'

---

[57] My thinking on this matter owes much to Shapin's discussion of metonymic relationships in "Cordelia's Love: Credibility and the Social Studies of Science," Perspectives on Science: Historical, Philosophical, Social 3 (1995): pp. 261-266.

interpretation from Boyle's obviously did not provide much for interested audiences to show one bridged the ontological gap between interpretation and experiment more successfully than the other. However, Boyle's success in inventing and manipulating experiments to show theoretical concepts "in action" seems more supple than Hobbes' rationalistic enterprise. A hint of the failure of Shapin and Schaffer to appreciate a concept such as epistemological sensitivity in LAP can be found in this suppleness - admittedly not a philosophically decisive term - is a theme they gave considerable significance in their Strong Programme sociology of science, which I shall call "intellectual obstinacy." Before we move onto this component of the argument, let me conclude that the very possibility of recasting the Boyle-Hobbes controversy in terms of epistemological sensitivity shows the possibilities of a historiographical sensibility that takes both philosophy and sociology into consideration. And the importance of this will become clear below when both social and intellectual factors are shown to be crucial to Hobbes' failure to gain ultimate acceptance for his natural philosophic claims in seventeenth-century England.

The issue of intellectual obstinancy is the most serious historiographical consequence of Shapin and Schaffer's lack of attention to epistemological sensitivity. What do I mean by intellectual obstinacy? By this I refer to the consequence of Shapin's Strong Programme-derived maxim of studying knowledge - again, defined as collectively held belief - as a natural phenomenon. This entails that any acts that result in the creation of knowledge - assent, denial, argument, etc. - are worthy of study simply because they

appeared in human epistemic history. There is not much objectionable here - surely all episodes have some intrinsic worth for scholarly study. When it becomes clear to the reader, however, that the obstinate refusal to alter one's intellectual standpoint in the face of overwhelming evidence to the contrary is seen as an epistemic act that speaks to the legitimacy of the overwhelming evidence's value, then it becomes a concern, for such obstinance was probably noticeable also to contemporary audiences who would judge such behaviour accordingly. This is exactly the case in LAP, where Shapin and Schaffer happily described Hobbes' obstinate refusal to accept the results of an experiment that he had earlier cited as, if successful, an argument against his natural philosophy and in favour of Boyle's.

When describing the idea of a crucial experiment, Shapin and Schaffer indicated how Hobbes still withheld assent despite Boyle's apparent success in proving a contentious experimental result. After many failed attempts, Boyle finally succeeded in having two pieces of marble separate from one another in the air-pump, which he demanded Hobbes acknowledge as proof of the pump's ability to create a vacuum and simultaneously of the power of the spring of the air to keep the two pieces of marble together in normal atmospheric conditions. Hobbes had explicitly stated that, should Boyle succeed in getting the marbles to separate, there would be no possibility of denying Boyle's hypothesis on air's spring. Shapin and Schaffer described Hobbes' reaction to Boyle meeting this challenge as follows:

> However, Hobbes did not recant. In the *Decameron physiologicum* of 1678, Hobbes still did not make any mention of Boyle's "successful" experiments on

> cohesion *in vacuo*. He continued unconvinced and unrepentant. He still cited the cohesion of marbles as paradigmatic support for the plenist account.[58]

No reason was cited by which Hobbes might have justified this move. Shapin and Schaffer saw this as an example of the failure of experiment to generate assent - Hobbes could, and did, deny the "conclusive" proof Boyle provided.

True to their Strong Programme roots, Shapin and Schaffer were following Bloor's maxim of studying knowledge "purely as a natural phenomenon."[59] In their minds, the sort of obstinacy that characterized Hobbes' response to Boyle's experimental "success" was simply an observed component of human epistemological behaviour. It is not difficult to grant them this point - pride, folly, stubbornness seem universal features of the human character. What is more difficult to grant them is that it somehow shows any sort of failure in Boyle's method: if Hobbes was unwilling to accept the terms of a deal he himself articulated, then Hobbes was the worst for it. In terms of historical evidence to mark this as a socially recognized failure of Boyle's natural philosophy, Shapin and Schaffer failed to provide examples of any advocates of Hobbes' stepping forward to defend this act of intellectual obstinacy (which itself hints at the problem of Hobbes' exact social constituency, to be raised below), which, had they presented it, might go some ways towards showing the accuracy of their sociological model. Obviously others were persuaded of Boyle's success and Hobbes' failure, for Boyle's natural philosophic

---

[58] Schaffer and Shapin, LAP, p. 198.

[59] David Bloor, Knowledge and Social Imagery, 2nd ed. (Chicago: University of Chicago, 1991): p. 5.

project went on to thrive, whereas Hobbes' disappeared into obscurity. In the end, this notion of intellectual obstinacy boils down to little more than an observation that pride, ignorance, and mean self-interest play a large role in human affairs.

Intellectual obstinacy had a traceable pedigree in Shapin's writings. Scottish phrenologists and idealists both saw the same brains dissected, yet they refused to vindicate the other's interpretation.[60] This quarrel had social origins, thus suggesting a fundamental opposition between epistemic views from different parts of the social structure. This, for Shapin, was the key to the argument: one need not bend to another's claims about reality because those claims depended on a set of social experiences foreign to one's own experience, and, because of the strife-ridden competition between social groups, possibly even detrimental to it. The implications of this stance extended not just to judgements based on observation, but to all types of judgement: mathematical, logical, necessary, and so on. According to Shapin, "the sociology of knowledge is built upon an appreciation of the contingent circumstances affecting the production and evaluation of scientific accounts."[61] These "contingent circumstances" were generally the judgement-maker's social and political milieu, and particularly the interests one acquired through occupying a certain position in those milieus. Interests structured cognition; the scientist's

---

[60] Steven Shapin, "The Politics of Observation: Cerebral Anatomy and Social Interests in the Edinburgh Phrenology Disputes," On the Margins of Science: The Social Construction of Rejected Knowledge, ed. Roy Wallis (Keele: Keele University, 1979): pp.149-157.

[61] Steven Shapin, "History of Science and its Sociological Reconstructions," History of Science 20 (1981): p.159.

"theoretical" interests (i.e. in proving the theory he or she espoused) determined the success or failure of experiments; "professional vested interests" sorted good from bad theories and methodologies; interests in, for example, religion, made the boundary between science and non-science fluid; ideas for social and political organization guided the lines along which interpretation developed.[62] Further, scientists might take ideas from other fields of knowledge, such as economics, and apply them to the natural world, using them as "resources" to fuel their explanations.[63] From another angle, natural philosophers of the seventeenth century used knowledge about nature "to comment upon specific political events or the proper order of society."[64] What this reliance of judgement on social and cultural contingencies means is that no claim about nature has a necessary character. Many things can be said about nature, but in Shapinian science, there simply is no overarching canon of rationality or of intellectual conduct that makes a judgement universal, so there is no reason why one must acquiesce to a particular account of nature.

Through connecting this back into the discussion of epistemological sensitivity above, an interesting conclusion emerges: possibly, historical audiences may note obstinacy such as Hobbes' and "success" such as Boyle's and be swayed accordingly.[65]

---

[62] Shapin, "Sociological Reconstructions," pp.159, 164-165, 169-171, 188-189.

[63] Shapin, "Sociological Reconstructions," p.177.

[64] Shapin, "Sociological Reconstructions," p.181.

[65] Certainly, convincing portrayals of Hobbes' intellectual vicissitudes point to seventeenth-century audiences' assessments of his "extreme" materialism and refusal to acknowledge mathematical error. See Douglas M. Jesseph, Squaring the Circle: The War

What makes this interesting is that both philosophy and sociology come out well in this conclusion: audiences may use rational *and* social criteria to assess knowledge claims; it is even possible that the rational tools employed may be socially derived. Thus, giving intellectual obstinacy an epistemological role as Shapin and Schaffer did distracted them from intellectually significant factors in the acceptance of knowledge claims. Stubborn refusal to assent cannot be placed on the same intellectual plain as attempts to directly address opposing arguments. Such a conclusion is of the utmost importance to the success of the historian's craft.

The philosophical sensibility above sheds critical light on the structure of society proposed in LAP. For Shapin and Schaffer, its seems that the use of sociology in the history of science is predicated on a particular theoretical view of society as composed of mutually hostile social groups. Shapin saw knowledge as emerging from a strife-ridden social structure, and being subservient to the needs of the groups within that structure. Part of the Shapinian recipe is that nature itself plays a limited role in knowledge's growth - as has been described above, nature as shown through observation and experiment has a subsidiary role in the constitution of scientific knowledge: two individuals representing different social groups can look at the same experiment and conclude different things. Unfortunately, there is a tension here, a tension between the human ability to know society and to know nature. For if society is to have formative power on the epistemic

---

between Hobbes and Wallis (Chicago: University of Chicago, 1999): pp. 276-277; and Noel Malcolm, "Hobbes and the Royal Society," Perspectives on Thomas Hobbes, eds. G. A. J. Rogers and Alan Ryan (Oxford: Clarendon, 1988): passim.

contentions of human beings, then there must be some mechanism that allows humans to ascertain, consciously or unconsciously, their social environs and to shape their knowledge claims accordingly. However, the Shapinian epistemology, relying as described in Chapter 2 on the Strong Programme's confidence in social causation, results in inconsistencies. If humans are barred from a perspicacious, intimate apprehension of the characteristics of nature, then how do they *know* the characteristics of the social structure? Unfortunately, no mechanism is supplied. Further, if they are in the same deficient position regarding knowing society as knowing nature, then how much significance can one attribute to the sociologically leaning historian's view that the structure of knowledge follows the structure of society? After all, humans are as much a part of the natural world as they are the social world - perhaps their knowledge follows both? Shapin and Schaffer took detailed knowledge of the social structure for granted in a way that they would not let scientists take a detailed knowledge of physical reality for granted.

Shapin and Schaffer premised LAP on a social structure which featured widespread competition and tension between different social groupings, while encouraging the reader to avoid correlating a particular description of nature with nature itself. Nowhere did Shapin and Schaffer prove that the seventeenth century had this particular social structure; even if one were willing to grant them this as proven,[66] they

---

[66] As some of the historical background above has shown, homogenous class unity, for instance, was in no way a feature of the English Civil War; see Smith, Double Crown, pp. 129-130. For incisive commentary on the difficulties in pinning down class

nowhere explained how the historical agents in question, Hobbes and Boyle, themselves identified this structure. If the events Boyle and Hobbes observed in the air-pump were open to various interpretations, then why are their assessments of their personal locations in the social structure not subject to a similar variety of interpretations? Shapin and Schaffer assumed a finely attuned, accurate faculty of social knowledge, but denied such a faculty of natural knowledge. The reader needs to ask: how is knowing society different from knowing nature? Is it not possible that the process of extrapolation from the observed raw reality of human beings interacting with one another to a theoretical, sociological interpretation stating that such interaction is based on a class-based dynamic of social strife and tension may be the same as moving from the observed Torricellian space to the theoretical/factual claim of the spring of the air?[67]

---

and its role in early modern British history, consult J. H. Hexter, "The Myth of the Middle Class in Tudor England" and "Storm Over the Gentry," Reappraisals in History: New Views on History and Society in Early Modern Europe (New York: Harper and Row, 1961): pp. 71-116 and 117-152 respectively.

[67] The ontological equivalence and epistemological sensitivity concepts may help clarify this problem. That humans interact with one another is undeniable, but that they interact in a setting of class struggle is something entirely more difficult to establish conclusively. In short, one can confidently assert that humans interact; one can confidently assert that there are such things as cooperation and strife amongst humans; one can even say with assurance that some communities are better positioned in the struggle for survival than others. However, the particular, well-knit, class-based social world that Shapin and Schaffer assumed was a sophisticated entity indeed, one that is as dependent on a multiplicity of social and intellectual factors as was Boyle's notion of the spring of the air. In other words, social interaction is not ontologically equivalent to a class-based social paradigm, and ascribing such a paradigm to a human community is much more epistemologically sensitive than simply identifying that there are such things as human communities.

These remarks echo earlier criticisms, notably those made by G. N. Cantor in response to Shapin's attempt to use sociological tools to explain the debate between phrenologists and moral philosophers in early nineteenth-century Edinburgh. Cantor was "very dubious whether the social realm is more actively known than the cognitive," challenging Shapin's "intuitive" understanding of "social conflict." Further, Cantor identified the lack of a "translational theory linking the social and cognitive realms" - in other words, the absence of a direct causal link between society and (in this case) phrenology.[68] That such problems continued in LAP suggests one of the fundamental difficulties of the Strong Programme and Shapin's image of science: positing a human capacity for knowing society while undermining an equivalent capacity for knowing nature.

In the end, Shapin and Schaffer were reluctant to leave any ground for real epistemological problems, fearing it might weaken the potency of their sociological interpretation. This is unfortunate, as it steals some of the wind from the sails of Shapin and Schaffer's historiographical revision of the Boyle-Hobbes confrontation. How so? By applying an inconsistent epistemological scheme that made the social universe more knowable than the physical universe, Shapin and Schaffer subtracted from the interpretational richness of factors that drive scientific creativity. The addition of a sociological gear need not annihilate all other approaches, but may augment and illuminate a multifarious engine of factors - cultural, personal, epistemological, and

---

[68] G.N. Cantor, "Critique of Shapin's Social Interpretation," pp. 246-248.

intellectual. LAP sidesteps the question of how we know by substituting one reality for another. As Douglas Jesseph has pointed out, this smacks of sociological reductionism.[69] In any case, such a reduction casts an oversimplifying light on how characters, such as Boyle, Hooke, Wallis and other members of the Royal Society, a group of diverse social origins, could come together in one intellectual fold. It also commits an error of historical accuracy, making, for instance, the non-noble Hooke and noble Boyle seem to have more in common socially than Hooke and Hobbes or Boyle and Hobbes. Excessive zeal was part of the problem: Shapin and Schaffer were arguing an often-ridiculed interpretation of scientific knowledge, and therefore adopted a combative, give-no-ground style of presentation.

Regardless of the absolute merit of Shapin's sociological theory and its application, there are also questions regarding the consistency of how he and Schaffer applied it in interpreting Boyle's and Hobbes' natural philosophic views. First, as Jesseph has noted, one problem with Shapin and Schaffer's sociological interpretation of Hobbes is that it is difficult to find a social group Hobbes actually represented.[70] This is a crucial problem from a Strong Programme perspective because of its definition of knowledge as collectively held belief:[71] if only Hobbes held these beliefs, then they remained just beliefs. There are two possible dimensions to this problem, methodological and

---

[69] Jesseph, Squaring the Circle, pp. 343-356.

[70] Jesseph, Squaring the Circle, p. 351.

[71] Bloor, Knowledge and Social Imagery, p. 5. Also Chapter 2 of this thesis.

programmatic/historical in character. First, if the lack of identification of Hobbes' particular social group was an oversight, it was a serious one, for they should have spent time outlining the Hobbesian clique's interests and aims if they truly wanted to show the Boyle-Hobbes encounters as clashes between different social groups. Shapin and Schaffer provided ample evidence of the sort of social group to which Boyle belonged, specifically the Restoration nobility as it attempted to cut a line between an all powerful king and an unruly mob. They do not, however, provide Hobbes with any such group. Second, and potentially more devastating, is if Shapin and Schaffer failed to identify a particular social group to which Hobbes belonged because no such group existed. If this is the case, then it is hard to see how Hobbes' system was anything more than idiosyncratic belief, which clearly fails to meet the definition of knowledge as collectively held belief, and therefore casts doubt on the whole project - surely group acknowledgement had to count for something in a sociological study of knowledge? Moreover, if a lone individual can concoct natural philosophic systems as sophisticated as Hobbes', then how can knowledge be said to be a sociological accomplishment?

Looked at more deeply, Hobbes' social position is not one for which Shapin and Schaffer's rigid sociological model can easily account. Hobbes certainly did have social support, generally emanating from those aristocrats who backed the king during the English Civil War - in other words, the Royalist camp. The character of this support was generally in the form of friendship and professional appointments as opposed to all out partisan backing. Hobbes was intimate with the Cavendish family, which included the

Earls of Newcastle and Devonshire; through the exiled English court in France (1651-1660) he became tutor to the Prince of Wales, and gained the support of Henry Bennet, who became Secretary of State Lord Arlington after the Restoration. During the 1650s, Hobbes began to encounter considerable harassment because of his perceived religious views, more specifically his supposed "atheism." Nonetheless, he still managed to make friends in influential circles, including lawyers such as John Seldon, physicians like the famed William Harvey, even succeeding in befriending some Cromwell sympathizers, such as John Hall. Even more interesting is the variety of friends he had in the ranks of Boyle's Royal Society. Noel Malcolm has pointed out that Hobbes could count a significant minority of Fellows of the Royal Society as friends (some 15 of a total membership of 46 in 1661).[72] Hobbes certainly was not without social support.

However, no clear group formed around Hobbes, and he was both in personality and intellect very much an individual. As the 1650s progressed, the views Hobbes espoused in Leviathan were more and more widely and viciously castigated as atheism. Atheism was a serious charge in seventeenth-century England, which connoted not simply disbelief in the existence of God, but also possibly unorthodox religious views, belief in materialistic ontologies, secular attitudes, or the placement of human civil authorities above human religious authorities. Hobbes was very open to such charges, regardless of their truth, as these features characterized Hobbes' philosophy. Although Hobbes never professed atheism openly, many audiences felt the thrust of his natural philosophy was

---

[72] Malcolm, "Hobbes and Royal Society," p. 51 and passim.

obvious, and the allegation of atheism forged alliances against Hobbes between groups that otherwise were at odds. In particular, the clergy of various, often antipathetic denominations and sects, and the universities came together to attack Hobbes due to his sharp anti-clerical arguments and his attacks on the universities as outdated bastions of Scholasticism working in support of the clergy.[73] Thus, even at the zenith of his natural philosophic career and status as a leading English man of letters, Hobbes had few supporters in England who vocally defended him, for fear of their own reputations. His notoriety also made him a convenient target for those who felt it necessary to buttress theirs. As one historian has argued, even potential allies of Hobbes were often vociferously outspoken against him because of the similarities in their thought; rivals such as Boyle endorsed a mechanistic natural philosophy similar to Hobbes, and therefore feared allegations of atheism would besmirch them too. In this way, attacks on Hobbes were a convenient way to protect one's own mechanistic theories from accusations of atheism. Additionally, as Hobbes' reputation declined from the 1650s on - partially due to his failed attempts to square the circle and the resulting heated rivalry with Wallis - his friends had even more reason to hesitate to endorse Hobbes vigorously in public, which possibly explains his failure to be invited to join the Royal Society, despite his many friends there.[74]

---

[73] For more on this topic, see Samuel L. Mintz, The Hunting of Leviathan: Seventeenth-Century Reactions to the Materialism and Moral Philosophy of Thomas Hobbes (Cambridge: Cambridge University, 1969): pp. 44-45, 47-49.

[74] Malcolm, "Hobbes and Royal Society," pp. 60, 62-64.

Regarding the sort of absolutism that Shapin and Schaffer ascribed to him, it is important to keep in mind that the political situation in the British Isles was very fluid during Hobbes' life, and that absolutism was problematic in the political context of Restoration England. Certainly, the experience of Charles I's "Personal Rule," characterized by his absolutist shunting aside of the Houses of Lords and Commons and relying solely on the advice of a few hand-picked counsellors, had enraged the Parliamentarians. As Charles I attempted to centralize control of taxes, government policy, and religion in the hands of the king and Privy Council, tensions grew until the Civil War erupted. Despite the post-civil war settlement that saw a king again sitting supreme on the British throne, the British aristocracy and gentry demanded that their views and policies be the political basis on which the king ruled; even the lower classes that were largely ignored politically in the Restoration had shown little stomach for absolutism.[75] Thus, Hobbes seems to have had few natural allies when it came to the absolutist component of his political philosophy; to put it more exactly, possibly many of those who favoured absolutism were hesitant to speak out for Hobbes, for fear of upsetting the Restoration balance. His system may have been coherent and consistent from an intellectual standpoint, with a lasting legacy in political and social philosophy, but contemporary audiences in England handled Hobbes derisively or with care, but rarely

---

[75] This interpretation of Charles I's rule and the subsequent civil wars is based largely on Smith, Double Crown, particularly Chapters 4-6. Although the civil wars were fought by all levels of society, a well-known thesis argues that the Restoration settlement was heavily biassed towards the gentry and aristocracy; see Christopher Hill, The Century of Revolution, 1603-1714 (London: Nelson, 1961): pp. 307-311.

without outspoken adoration, due to the perceived extremism of his political and religious views.

LAP was a bold attempt to show the importance of social agency in the constitution of scientific knowledge. Shapin and Schaffer's development of the idea of the role of social conventions in the formation of science was an important step in understanding what it means to say that science is a human activity. Whether they went too far in their revisionist zeal is a worthwhile question. Indeed, subscribing solely to sociological means to interpret the history of science proves inadequate, needlessly brushing away rich philosophical issues which inform supple and probing historical interpretation. Thus, the historical picture loses some of its accuracy. Shapin and Schaffer failed to account sufficiently for the paradigmatic promise of Boyle's experimentalism - a feature Kuhn no doubt would have identified as key to Boyle's success. From another angle, using social means to solve the problem of how humans secure knowledge of the natural world really only replaces one problematic means for another. Knowledge of society in many ways seems no less fraught with epistemological trouble than knowledge of the natural world. In the end, they simply failed to apply their critical concerns universally, inexplicably absenting the social sphere. The criticisms in this chapter have tried to argue the value of other sensibilities, sensibilities less sociologically rigid and more philosophically tolerant.

Shapin's later scholarly work responded, possibly unconsciously, to such considerations. As the reader will see in the next chapter, he continued to subscribe to a

sociological interpretation of science in A Social History of Truth. However, his depiction of how society works to found knowledge became more supple, and thus more historiographically satisfying. A society that forged knowledge in the fires of social tension and struggle transformed into a society ripe with change and uncertainty, where social agents attempted to assess how best to fashion knowledge using the cultural means available to them. Now let us turn to A Social History of Truth.

Chapter 4
The "Social" Scientist: Individual versus Collective Knowledge in A Social History of Truth

> There is a massive mismatch between dominant characterizations of the sources of our factual knowledge and the ways in which we actually secure that knowledge.
>
> Steven Shapin, A Social History of Truth (1994).

> Man does not create gods, in spite of appearances. The times, the age, impose them on him. Man can serve his age or rebel against it, but the target of his cooperation or rebellion comes to him from the outside.
>
> Stanislaw Lem, Solaris (1961).

After Leviathan and the Air-Pump (LAP), Shapin continued to employ sociological tools and interpretations. However, there were changes in approach. The most significant of these was the almost complete disappearance of social strife as a key factor in scientific development, and the seeming abandonment of the idea of science as a political tool. Instead, Shapin turned to a sociologically grounded notion of credibility that exhorted the historian to look to the scientist-philosopher's local culture for explanations as to why his pronouncements about the universe were accepted as knowledge. In particular, this notion of credibility was predicated on trust - the ability of social agents to create a foundation for mutual cooperation and interaction. The primary consequence of this change is that the historian no longer need seek out the fault lines between different social groups, but instead those cultural factors which allow historical agents to trust one another. Shapin propounded this alternative in A Social History of Truth (AST), his next major work.

4.1 The Role of Society in Scientific Knowledge

One of the primary tasks Shapin set himself in AST was to contrast the power of a sociological idiom to that of an individualist idiom in the history of science; I will label this ethic of the individual in science as "epistemological individualism." According to this ethic, society should be a non-factor in knowledge, and when present can only blight the integrity of knowledge. In subscribing to this notion, a number of responsibilities fall to the scientist: he bows to no authority other than his own; he takes nothing on trust, but subjects all claims about the universe to his perspicacious mathematical-experimental technique; equal competence using these tools means that he has equal right to speak among his peers. The social dimension of the scientist consists mainly in the command he possesses in the public sphere due to his qualifications, and in the public nature of the results (they are not to be sequestered away from a public that demands the fruits of science). Epistemological individualism precludes a constructive role for society in making scientific knowledge.[1] Shapin sums up this popular image of science succinctly:

> In ideal-type empiricist schemata, an isolated individual, conceived as free, pure, and unconstrained, confronts a natural reality, conceived as transparent to his gaze. Within such a framework, the particular expectations, conventions, and interests transmitted to the individual from society can act only as sources of contamination and corruption.[2]

---

[1] Steven Shapin, A Social History of Truth: Civility and Science in Seventeenth Century England (Chicago: University of Chicago, 1994): pp. 5, 16; Adi Ophir and Steven Shapin, "The Place of Knowledge: A Methodological Survey," Science in Context 4, 1 (1991): p. 5.

[2] Ophir and Shapin, "Place of Knowledge," p. 5.

Shapin described this association between individuality and epistemic integrity as a long-established belief in Western cultures. One reason he postulated for this was that many people saw solitude as a sign of intellectual worth - that which the human does in isolation is more likely to be truth than that which is done in the company of others. This idea attached to prophetic religious experience, art, literature, and, through philosophy, to science.[3] The idea gained shape in seventeenth-century England, where Boyle and his ilk reiterated at length the importance of doing experiments oneself, and not turning to others to form one's conclusions. For them, this stance constituted a break with the authority-ridden Aristotelian philosophy of nature, a philosophy they saw as exhorting one to bow down to the words found in a book as opposed to the experience of one's own eyes. The best way to find out about the universe was to study it oneself.[4]

In opposition to epistemological individualism, Shapin posited a thoroughly social model of science, open to sociological examination. Let us call this model "epistemological collectivism." The main fruit of this was AST. What are the core ideas in AST? There are two, but each contains a number of threads to be drawn out and elucidated. First, it contains the idea that the primary constituent of science is trust.

Second is the idea that knowledge is always a result of community cooperation. As a

---

[3] Steven Shapin, "'The Mind is Its Own Place': Science and Solitude in Seventeenth-Century England." Science in Context 4 (1991): pp. 192-194, 202-206; Steven Shapin, "The Invisible Technician," American Scientist 77 (1989): p. 561; Shapin, AST, pp. 150, 416.

[4] Shapin, "Invisible Technician," pp. 556, 561; Shapin, AST, pp. xxv, 5, 16, 201-202.

counter to epistemological individualism, AST advances an epistemology built around a social dynamic. Science is not disembodied knowledge, but depends as crucially on the culture in which it formed as on the universe it studies. In contrast to the antagonistic model Shapin developed in LAP, this dynamic of trust depends less on political allegiances and the struggle for dominance than on the need to conform knowledge to a culture's canons of respectability and believability. Truculence as a motor for scientific change recedes, and social cooperation advances. Over the following pages we will evaluate the model of trust and social cooperation in order to set up a comparison of the two approaches in this thesis' Conclusion in Chapter 5.

How could Shapin say that the primary component of scientific knowledge is trust? This provocative claim arose from Shapin's characterization of knowledge as a "collective good" and as a "social institution." What this means is that knowledge is a product of human beings working together. No one ever gains knowledge on his or her own, but forms scientific claims in a group environment and then submits them to a greater community, which exercises either approbation, modification, or rejection. Shapin was outspoken on this head: knowledge is never an individual activity. The glue that Shapin proposed unites the multifarious components of the knowledge community is the "moral bond" of trust.[5] In AST, Shapin built his concept of trust in light of its antithesis, scepticism. Understanding this concept demands attention be paid to what he termed "the natural attitude" or "mundane reason," concepts taken from various strains of

---

[5] Shapin, AST, pp. 5-7.

phenomenology. Understanding how he applied this idea to historical interpretation of the world of Robert Boyle and seventeenth-century English natural philosophy will be imperative.

That trust is constitutive of all knowledge is about as close as Shapin will ever come to a universal generalization. Shapin described how many generations of scholars identified the importance of trust in a functioning society. He was aware, however, that few theorists had placed much confidence in trust as a tool for establishing reliable systems of knowledge, a state of affairs he sought to remedy. To make his point, he compared the role of trust to the role, highly-vaunted in scientific rhetoric, of scepticism. Shapin envisaged the popular justification of the potency of scientific knowledge as reliant on the "solitude, passivity, and impersonality" of its gathering; "[k]nowledge is supposed to be the product of a sovereign individual confronting the world; reliance upon others produces errors."[6] Shapin entered the very laboratory of the scientist to show the inadequacy of this image. Returning to his days as a student of genetics, Shapin scrutinized the claim that cytosine is a constituent of DNA. He took the reader through the process he followed in the laboratory to analyse the DNA. What he concluded was that at any number of points he could have taken a sceptical stance regarding the materials he was using, the process itself, or the conclusions based upon it. Instead, overall he opted for credulity. He trusted the measurements of thermometers and the centrifuges; he trusted the label which told him that the liquid he was using was ethanol; he was confident

---

[6] Quotes from AST, pp. 5, 17.

without additional research that the substance he was analysing was a rat's liver; and so on. All of these assumptions, Shapin said, could have been subjected to rigorous scepticism, but each sceptical move would have relied on new assumptions, ad infinitum. The whole idea of scepticism, then, depends on there being large components of trust already in place. Shapin contended that for him to espouse radical scepticism in the laboratory would likely lead his colleagues to dismiss him from that very place of endeavour - a serious *social* consequence. So scepticism plays a much more limited role and trust a much larger role in the modern scientific endeavour than much rhetoric would suggest.[7]

Shapin's next move was to expound a loose epistemology founded upon the dynamic of trust.[8] This loose epistemology owed much to his reading of Wittgenstein, pragmatism, and phenomenology. He offered the phenomenologists' idea of the "natural attitude" as the unreflective general trust humans hold that there is an external universe, consistent in its behaviour and structure, and that other human beings experience this

---

[7] Shapin, AST, pp. 5, 8-22.

[8] Shapin continued to be quite critical of the discipline of epistemology in AST, maintaining this Strong Programme theme; for instance, see his disparaging remarks about epistemology in Steven Shapin, "Rarely Pure and Never Simple: Talking about Truth," Configurations 7 (1999): passim. For Shapin, it seems epistemology and philosophy of science can only be strictly rationalistic, realist and individualist. I consider this an unwarranted stereotype which, even if true, need not be the case. My definition is that anything that describes how knowledge is made and warranted can and should be called epistemology. It should also be noted that Shapin himself was more attentive to epistemology in AST, using sociologically modified philosophical concepts such as those described in the following pages to solve practical problems related to human knowledge.

world in a consistent manner. Upon this basic assumption humans form their "moral/social"and "cognitive" orders. Without this assumption, human action would be impossible, for it is difficult to imagine an environment of social cooperation where each individual thought they experienced a universe they themselves ordered. The natural attitude is the field upon which limited scepticism can play and upon which knowledge of things and people is made and destroyed. Because humans regularly do experience variation in interpretation of phenomena, the mechanism of "mundane reason" evolved. By mundane reason Shapin was indicating the collection of explanations humans use to explain away divergent experiential accounts while maintaining social order. Humans in daily social intercourse will qualify claims, attribute different views to differing perspectives, and so on, without concluding from divergent views that each human experiences a different world. The natural attitude and mundane reason, Shapin emphasized, were both quotidian, functional concepts. Because of their fundamental character as assumptions, Shapin dismissed proving them through intellectual means. The justification of their worth was in the broad success of their use.[9]

Elsewhere, Shapin talked of the "mundaneness postulate." Care must be taken to distinguish "mundane reason" and the "mundaneness postulate." Mundane reason is a process for dealing with the world and other humans who would make claims about it. The mundaneness postulate points to elements of the scientist's environment that the

---

[9] Shapin, AST, pp. 27-34. This last point is a tip of the hat to the later Wittgenstein and the Strong Programme.

canons of epistemological individualism do not take to have any bearing on the truthfulness of knowledge. The latter concept emphasized that the cognitive and social processes that go on in the laboratory under the head of "scientific reason" are no different from the collective of modes of reasoning applied in ordinary human action. Shapin enumerated quite a few potential targets for the historian using the mundaneness postulate: "conversing . . . coercing . . . making marks on blackboards . . . gesturing . . . grunting . . . retrieving routine social knowledge about the standing and identity of those making claims . . . ."[10]

Shapin fleshed out the notion of mundane reason through examining the events of Robert Boyle's life. Key to this examination was Boyle's standing as a "gentleman." According to Shapin, the gentleman was the trustworthy agent of seventeenth-century England. This conclusion went back to two considerations: the nature of the "free agent," and the possession of "virtue." Boyle was a free agent because he had the means of a gentleman: noble birth (he was a son of the Earl of Cork), freedom from remunerative labour, political standing, and so on. Had he been in a position where he had to ply his skills to earn a living, he would not have been a free agent. On this foundation of social status and economic means the gentleman established his virtue. Because he had wealth and power, and because he was a man (being a woman in seventeenth-century England, even one of gentle upbringing, generally meant a low degree of credibility), he did not

---

[10] Please see Shapin, "Rarely Pure," pp.7-8 for the quote, and pp. 7-10. There Shapin was very explicit that the mundaneness postulate guided the production of AST.

need to lie to attain reputation and standing; because he did not labour for a master, and was indeed a master himself, he was free from the taint of serving another's interests, and spoke only for his own. Shapin also added flavours of Christianity and the secular warrior code into the gentle constitution: the seventeenth-century English gentleman who was seen to espouse Christian piety while defending his honour from slights was seen as virtuous. Moving further, Shapin wanted to make a special point about the gentleman: that truthfulness was his identifying characteristic, both from his own perspective and from the perspective of others in society. The gentleman was bombarded by this belief from all sides: the traditions of chivalry encouraged it; Christ and God demanded it; courtiers wrote long tracts about it; Classical secular literature defined and polished it. The importance of being honoured with the epithet of honesty was symbolized in the ultimate response to an accusation of lying - the duel. One should risk one's life defending his honour, and defence of honour almost always meant defence against an accusation that one had committed an ill or unjust act. To say a gentleman lied was to remove from him his warrant to the title "gentleman." Shapin's conclusion was that if any one sort of individual was going to be trusted as a truth-maker in seventeenth-century England, it was going to be the gentleman.[11]

Being gentlemanly, however, was not simply something that applied once and for all to a person - it was something that an individual worked at inculcating in his popular image, and a living concept that others applied to individuals. With conscious purpose, an

---

[11] Shapin, AST, Chapters Two and Three, passim.

individual could accentuate and define his gentility.[12] Shapin very much portrayed Boyle as someone who blatantly embarked on a strategy of highlighting his gentle status; to put it another way, Boyle recognized the epistemic potential in his social identity and accentuated those features which would amplify his credibility in the social circles he frequented. His strategy included the following elements: a certain social self-description; a particular self-modelling of Christian characteristics; and a self-fashioned gentle scholarly identity. Each of these deserves treatment in turn.

Even though his peers recognized him as a gentleman, Boyle worked to accentuate those features of his gentility that would vivify his truthfulness. For instance, he stressed his love for his father - although it is likely that he rarely saw the man. Boyle described his status as a younger brother as an asset, because it meant that he did not carry the burden of responsibility of land and power associated with being eldest. Boyle claimed, according to Shapin, that he was "so precisely positioned at the golden mean of basic social identities" as to occupy a perfect position for contemplating the universe. His particular social placement meant that the worries of rule or property did not assail him, but that the nobility of his birth and the comfort of his means encouraged his taste for philosophy. Although most writers on gentlemanly conduct ridiculed the distance scholarly sorts maintained from society, Boyle thought learning a mark of gentility, suggesting discipline amidst the manifold corruptions of social life. Thus, Boyle

---

[12] Shapin, AST, pp.126-130. Also see Shapin, "Personal Development and Intellectual Biography: The Case of Robert Boyle," British Journal for the History of Science 26 (1993): passim.

propounded a mixture of "solitary disengagement" and "civic duty" for the gentle philosopher. Interestingly, Boyle did nothing to hide his persistent sickliness, for it too was a sign of high-mindedness. Scholarship took a toll on the body, so the thinking went, and Boyle used his unstable physical constitution as a tool to build up trust in his philosophical doctrines.[13]

Public display of his Christianity also underpinned his reputation for intellectual ability. Boyle described himself as someone who had been touched by God's grace, having been saved miraculously from likely fatal injury a number of times as a youth. As when he yoked learning to the secular ideal of the gentleman, he also defended the idea that the study of nature was a sign of Christian devotion. Further, when one articulated truths about the natural universe, he displayed to others God's favour, for only God could constitute one so as to find such truths. Combined with other statements about his religiosity, and his advertised tendency towards Protestant Christian virtues such as chastity and introspection, Boyle made his Christianity a pillar of his truthful image.[14]

As alluded to above, Boyle made a great effort to combine learning and scholarly pursuits with prominent notions of gentility. For Shapin, this was Boyle's most creative

---

[13] Shapin, AST, pp. 135-137, 141, 147, 149-156. For more on the role of the body in founding credibility, see Shapin, "The Philosopher and the Chicken: On the Dietetics of Disembodied Knowledge," Science Incarnate: Historical Embodiments of Natural Knowledge, eds. Christopher Lawrence and Steven Shapin (Chicago: University of Chicago, 1998) pp. 21-55; and Shapin, "Descartes the Doctor: Rationalism and Its Therapies," British Journal for the History of Science 33 (2000): pp. 131-154.

[14] Shapin, AST, pp. 157-160, 163-165.

achievement, for although many younger sons of noble parentage pursued philosophy, Boyle was unique in his attempt to make the philosophical enterprise a component of the gentle identity - others eschewed gentle living after their avowal of learning. Philosophy for Boyle was a fortress, where one could productively develop virtue, holding out from the social ills prevalent in gentle circles. Simultaneously, however, Boyle had to dissolve the perceived irascibility of scholarly living.[15] Most gentle folk saw philosophers as arrogant and tending toward unwholesome dispute, attributes incompatible with genteel interaction. Boyle's philosophical appearance, that of the "experimental philosopher," attempted to remedy this by making a greater show of humility and modesty, which he accomplished partly through following traditional Christian and gentle norms. One of the key features of an experimental philosopher not shared by the more traditional philosopher was a dedication to sharing knowledge with the public. This helped convince his peers that Boyle was a different type of philosopher, for the theme of secretiveness was key to the traditional view of the scholar, particularly in the form of the alchemist. When he published a text, Boyle stated his reticence to publish, suggesting a deeply set humility, but stating that he had decided to do so because of its felicitous benefits for the public, thus showing his generosity of spirit. His publications generally stressed the author's fallibility. Boyle wanted to show he was no over-confident braggart, introducing

---

[15] A detailed look at the culture that disparaged the pedantry and social clumsiness of the scholar can be found in Shapin, "'A Scholar and a Gentleman': The Problematic Identity of the Scientific Practitioner in Early Modern England," History of Science 29 (1991): pp. 279-327.

his works with many statements indicating the potential errors in his conclusions, resulting from the many weaknesses of the human condition. Shapin contended that Boyle tried to appear as someone unaccustomed to making philosophical statements. This technique reduced the perceived personal investment Boyle had in his work, making it appear less a vocation than an honourable hobby done to honour God and uphold truth. He was a free agent, not a person dependent on the academy or the priesthood for his living; the desired conclusion was that, "[Boyle] was the sort of man who had no reason to misrepresent how matters stood in nature."[16] Fellow Englishmen would see him as honest because of his disinterest and disassociation from the conclusions he advanced. His attempted disassociation was so complete that he even underlined his supposed general philosophical ignorance as a strength: he was unfamiliar with other philosophies, so they did not unfairly prejudice his conclusions; he saw nature with an innocent clarity impossible for someone overwhelmed with bookish theories.[17]

To summarize my reading of the above, the salient point for Shapin was that Boyle's credibility was a social phenomenon. The persona Boyle radiated gave his claims a credible lustre. The inherent intellectual value of his claims was usually out of the reach of his audiences; what made the experimental philosophy attractive for them had more to do with the upstanding figure Boyle cut in seventeenth-century English aristocratic and gentle circles than with the perspicuity of his claims. The deeper conclusion is that

---

[16] Shapin, AST, p. 180.

[17] Shapin, AST, pp. 175-182.

knowledge claims depend greatly on the image of the person making them; rational analysis of the claims itself is only a small part of the total assessment.

However, even that small, rational component of knowledge-making was founded on social organization. Simply believing in someone was not enough - one needed ways to judge claims about what sorts of entities populated the universe. This situation was particularly critical in seventeenth-century Europe, as European explorers visited parts of the world previously unknown to them and as technologies such as the telescope and microscope revealed new worlds to human observation. Because one's own experience was limited, one needed a way to deal with the multiplicity of new claims. Shapin described the situation as one of "ontological openness." As claims of new entities arose, entities not described by Aristotle and other ancient philosophers, doubt seeped into some minds about whether the last-word authority of the ancients was deserved. Traditional philosophical method turned on what classical philosophy pronounced - for many scholars, if it was not in Aristotle's works, then it was not to be found in the universe. Those who were sceptical, however, voiced the need for some intellectual latitude as to what existed - thus ontological openness.[18]

Perceived epistemic anomalies within the Aristotelian model of nature encouraged many to dispose of Aristotelian methods of inquiry and seek new techniques for

---

[18] Shapin, AST, pp. 194-202.

understanding the universe.[19] The characteristic features critics discerned in the Aristotelian epistemology were authority and testimony, and these two methods for collecting knowledge became the primary targets of modernist derision. Such critics advanced a wide-ranging personal experience as a more reliable path to knowledge. One could trust oneself, but not others; trust in authority and testimony led to "corrupt[ion] by credulity."[20]

What Shapin observed in their pronouncements, however, was a certain way of "managing" trust, for, according to his thinking, trust could never be entirely abandoned. The management of testimony involved "epistemological decorum" and "prudential maxims." Here human thinking attains a distinctly circumstantial flavour. Epistemological decorum united how people know what is socially proper for them to know and how they are to go about knowing socially. Shapin used the idea of epistemological decorum to explain how people distinguish differing types of knowledge, and devise different evaluations of certainty to apply to them. It would be socially impractical to reject any epistemic claim based solely on its absence from one's own limited experience. It would also be patently unreasonable for an individual to demand

---

[19] At this point in the discussion, Shapin qualified his talk of radical change in philosophical understandings of nature by pointing to the fact that no individual or group can ever entirely eschew a former world view, for then one would find oneself completely unanchored when trying to evaluate the new realm of entities. This is a striking example of the conservative theme in Shapinian knowledge. See Shapin, AST, p. 200.

[20] Shapin, AST, pp. 195, 200, 202.

certainty in equal degrees in all fields of knowledge.[21] Shapin presented the common argument of seventeenth-century English philosophers and theologians that, despite the fact that Christ's existence could not be proven mathematically or even experimentally, and was really conveyed only by testimony, it was still very much the individual's responsibility to know that there was a man named Christ, whose actions were historical, not fictional. In short, there was a *moral* obligation to assent to the truth of Christ's historical reality, despite the fact that no seventeenth-century Englishman had ever laid eyes upon him. Testimony thus could be a formative element in human knowledge. Shapin consistently aimed to tie knowledge to a social foundation, and in AST Shapin's conception of the social never wandered far from the moral.[22]

The "prudential maxims" provided a fulcrum upon which epistemological decorum turned. Concentrating mostly on the work of John Locke, Shapin delineated how thinkers in seventeenth-century England recommended one deal with testimony. Locke propounded a system where the person encountering testimony was to judge it by its plausibility, number of sources, consistency, immediacy of the testifier to the event, expertise of the testifier, the comportment of the testifier while presenting his testimony,

---

[21] These statements are Shapin's sociological attempt to deal with the package of intellectual issues I branded under the head "epistemological sensitivity" in the previous chapter. This sociological engagement with epistemological issues shows the increased maturity of Shapin's thinking in AST, and reveals a change of tact in his thought, if not in his rhetoric. The consequences of this change will be explored more fully in Chapter 5.

[22] Shapin, AST, pp. xxix, 202-211; in particular pp. 209-210 for the example of religion.

and the "acknowledged integrity and disinterestedness" of the testifier. Shapin emphasized that there were flexible "maxims" to guide behaviour, not iron-clad "rules" that dictated it. Epistemological judgement varied from place to place, from situation to situation. The important thing was that social consensus exist at the end of the epistemic process. A historical agent like Boyle might invoke the prudential maxims of testimony differently in different situations, including occasionally resorting to their "counter-maxims," which also had thrift in the intellectual economy of seventeenth-century England. For instance, although having a plethora of sources making the same epistemic claim was a prudential maxim to judge testimony, it might also be prudent to disavow claims made by a broad selection of individuals. Such breadth conferred banality upon the claim, and banality might suggest epistemic worthlessness.[23]

Fluidity was a primary trait of Shapinian scientific reasoning in AST. This fluidity was clear in the historical episodes Shapin explored. Shapin aimed to show that knowing about the entities that populated the universe was intimately tied to knowing about the people who described them. Before an individual could establish anything about the characteristics of, for instance, comets, he or she had to establish something about the character of the person making claims about comets. Indeed one of the most gripping historical accounts Shapin provided was about a dispute between Adrien Auzout and Johannes Hevelius over the celestial location of comets appearing in 1664 and 1665. A

---

[23] Shapin, AST, pp. 212-227, 232-233, 238-240; for examples of other "countermaxims," see pp. 233-238.

closer examination of this episode will help frame the issues to which Shapin was most drawn in his thinking.

Observations of two comets were involved in the debate. The first, which Shapin called "C1", presented itself to Europeans in late 1664 and disappeared from view in March 1665. It attracted widespread attention from the elite of seventeenth-century European natural philosophy: Christiaan Huygens in Holland, Hevelius in Poland, Isaac Newton in England, and others. With their eyes turned to the heavens, they spotted a second comet ("C2") in March 1665; by mid-April it faded from view. Great excitement stirred amongst the learned, for comets were a celestial treat that posed serious philosophical challenges and potentially great philosophical rewards in helping to collapse the Aristotelian cosmology. As Shapin indicated, for these philosophers to be able to plot the course of a comet in advance and with accuracy would be an important accomplishment. Auzout offered just such a plot. Within a short time natural philosophers all over Europe, including in England, accepted Auzout's work as legitimate. Similar events transpired over the more transient C2. Although he prevaricated somewhat regarding the accuracy of the predictions, Auzout also produced a plot for this comet.[24]

Controversy arose once Hevelius published his observations of the two comets. Auzout pointed out that Hevelius' observation of the last celestial point C1 occupied varied greatly from what others had predicted or observed. Hevelius refused to recant his observation, and asked the Royal Society to adjudicate the matter. Unfortunately, upon

---

[24] Shapin, AST, pp. 266-272.

further consultation, the members of the Royal Society in England found that apparently only Hevelius made observations of C1 on the date in question - the rest of the community only had Auzout's predictions, as well as some other independently achieved plots, to consider. Eventually, in 1666, because the weight of the philosophical community was behind Auzout's prediction and there was no corroboration of Hevelius' observation, the Royal Society decided that the latter's observation was an error. Prediction won out over observation.[25]

Hevelius remained obstinate, refusing to renounce his claim. At this point, things became thorny. Boyle and his fellows in the Royal Society, as well as their natural philosophical peers in the rest of Europe, had built parts of their edifice with contributions from both Auzout and Hevelius. Both were important members of the community who were woven into its epistemic fabric. In short, the Royal Society could not insist Hevelius atone for his mistake, but they also did not want Auzout to see their not taking his claim of Hevelius' error seriously. Hevelius' protestations led mathematician John Wallis and secretary of the Royal Society Henry Oldenburg to placate him with statements that the issue would be reconsidered and offering a possible epistemic exit for the controversy: that Hevelius had indeed observed a comet, but not C1 - he possibly saw the early appearance of C2. This said, the Royal Society gradually let the matter fall from the table of debate, and Shapin could find no further statements relating to the matter.[26]

---

[25] Shapin, AST, pp. 273-280.

[26] Shapin, AST, pp. 280-287.

Shapin interpreted this whole episode as primarily a social and moral exercise, with little of the rational contributing to the actors' epistemic decisions. Pride, integrity and social stability were the major issues, not the overarching goal of truth. Further, epistemic decisions such as 'What constitutes an accurate observation/prediction?' were negotiated socially. Each individual's set of observations/predictions were discrepant, but only Hevelius breached the corridor of variation which the majority of actors had agreed upon, whether tacitly or explicitly. Because Auzout and Hevelius were both part of the epistemic and social fabric of the community, adjudicators sought to find a courteous way to solve the problem. In their initial solution, they offered Hevelius a way to humble himself without denigration, in fact a way that emphasized his social merits and overall importance to the community. When this proved too much for him, the community took another strategy: silence. By putting the matter aside, they, without words, asked Hevelius and Auzout to move away from acrimonious debate to continued productive research. Social and epistemic stability won out over an unswerving dedication to rationality and truth.[27]

Shapin drew a number of conclusions about how seventeenth-century English experimental natural philosophy operated from his empirical investigations. First, close sceptical scrutiny and detailed processes of verification did not accompany the admission of new bits of knowledge into the catalogue of the experimentalist school. Episodes such as the one described above were, according to Shapin, deviations from the norm. The aim

---

[27] Shapin, AST, pp. 286-291.

was to build the stock of knowledge and this in the main involved building the human community. Knowing, thus, was primarily a social act in both means and ends.[28] What critiquing the natural philosophical community actually carried out was more a process of social analysis, based upon what Shapin called "systems of recognition." In general, those who did natural philosophy knew each other personally. Intermediaries worked to introduce new members to the community, recommending them as trustworthy individuals. Shapin's last conclusion was that there was a certain reciprocity between members of the natural philosophical community. Unbounded scepticism entailed risks for the sceptic because such unruly behaviour might result in an equal degree of scepticism towards his knowledge claims. The overall tenor of Shapin's treatment of these episodes was that accepted members of the community treated each other amicably, or at least aimed to do so, while the claims of those from outside of the community might be dismissed with relative ease.[29]

Shapin made explicit the epistemological consequences of this orientation towards amicability through a study of Boyle's attitude towards the use of mathematics in natural philosophy. The crux of Shapin's argument was that Boyle advised against the use of mathematics as a language for describing nature because it was *too* precise. This idea returns us to the matter of how closely a measurement or description corresponds to

---

[28] Shapin, AST, pp. 303-304.

[29] Shapin, AST, pp. 303-309; for an example of those who Boyle dismissed, see Shapin's discussion of the divers and pressure, AST, pp. 258-266, 289.

reality is a matter for social negotiation. Shapin hammered on the point that Boyle was neither intellectually nor categorically against the use of mathematics in natural philosophy: on a number of occasions he lauded the power of mathematics.[30] Despite the deluge of intellectual reasons why a natural philosopher who espoused a mechanical ontology might turn to mathematics as the language of nature, Boyle declined to do so for social reasons. Mathematics smacked too much of the arcane, and, if made a centrepiece of Boyle's experimentalist natural philosophic endeavour, would render it opaque to wider audiences. Inclusive membership in the community demanded a less obscure way of expression. The idiom Boyle opted for was analogical, not mathematical. This strategy aimed for "intelligibility," which "was defined by the public character of philosophical language."[31] The gentlemanly natural philosopher would use familiar objects to describe the world of corpuscles and other esoteric entities. Sophisticated mathematical language rendered this impossible:

> Boyle understood mathematics to encompass an abstract, esoteric, and private form of culture. That was a major reason why he worried about its place within experimental natural philosophy. If experimental philosophy was to secure legitimacy and truth by implementing a public language, then the incorporation of mathematical culture might threaten a new privacy.[32]

---

[30] Shapin, AST, pp. 317-322.

[31] Shapin, AST, p. 336.

[32] Shapin, AST, p. 336.

It will be remembered that at least a degree of sociability was crucial to the gentleman's identity, while reclusiveness described the traditional scholar. Boylean natural philosophy depended on as big a community as possible for its epistemic justification, so overly mathematized descriptions of nature just would not do.[33]

Besides the historical curiosity of Boyle's position, Shapin wanted another point to emerge from his treatment of Boyle and mathematics, namely that science is a form of conversation. Boyle saw mathematics as disruptive because it was overly precise, useful for abstract, esoteric reasoning, but obstructive for immediate, public experimentation. Insistence that reality show mathematical exactitude and constancy would bog down the philosophic community, or, in Shapin's idiom, inhibit the flow of conversation. Pragmatic reasoning in science was key. Absolute, unswerving trust at every historical moment was unnecessary; in fact it was even detrimental to the philosophic enterprise, because it would undermine a general, more ineffable atmosphere of trust that allowed knowledge-making to proceed. Properly expressed doubt reenforced a general sense of trust. Natural philosophy, and science by extension, was a form of engagement with both nature and society, and thus might be seen as a type of conversation.[34]

The final major sortie against the notion of individualism in scientific knowledge in AST was Shapin's exposure of the master-servant relationship in Boyle's natural philosophical laboratory. Although it is widely said that Boyle made such and such an

---

[33] Shapin, AST, pp. 333-338.

[34] Shapin, AST, pp. 351-354.

experiment or discovered such and such a property of nature, a sophisticated culture underlaid such claims. Boyle himself did few experiments, but assigned experimental tasks to "laborants," "operators," "assistants," "amanuenses," "workmen," and "domestics" - terms used to describe remunerated staff and servants at the time. Sometimes Boyle was present, sometimes not. Nonetheless, his role was crucial, not just as financier and source of inspiration, but as epistemological authority. Boyle had authority to speak about nature, because of his social position and because of the way he portrayed himself to contemporary society. Some veins of political theory in seventeenth-century England stated that the free gentleman represented not just himself, but those indebted to him for their living; servants had no say in the state themselves, but supposedly had their interests protected and voiced in the person of their gentleman master. Whereas servants had no legitimacy as speakers about nature, and remunerated specialists might be seen as interested in achieving a certain epistemic outcome to make a living, Boyle faced no such limitations as a wealthy gentleman. Gentleman Boyle purportedly possessed the capacity to understand what it was his lab workers had done - such understanding did not necessarily exist in those labourers themselves. What is interesting here was the extension of mundane, general culture into the sphere of natural philosophical endeavour. Just as gentlemen had servants and assistants to handle most of the daily needs of the household, they also staffed their laboratories with servants and assistants to explore the mysteries of nature. Doing natural philosophy was not different in

this way from tending the garden or cleaning the laundry or cooking the master's dinner.[35] Far from being the solitary effort of Boyle, his natural philosophy was the work of a team. Because an array of epistemological biases attended the work of non-gentle folk, however, Boyle needed to command the situation and make the links of labour and work invisible, placing himself as the sole "author" of the work. The work would thus attain credibility in the eyes of the rest of the experimentalist establishment.[36]

Shapin's "epistemological collectivism" thoroughly integrated science into the general culture. He carried out this integration by making a convincing case that scientific knowledge depends greatly on the same canons of judgement humans employ in their social interactions; the effect of this argument is to bring a moral flavour to scientific knowledge: how one judges a knowledge claim often depends on how one judges the maker of the claim. Another result of this is that science takes on a less revolutionary, ground-breaking character, and becomes a much more fixed element of the cultural landscape, relying on the Barnesian cultural "resources" described in Chapter 2: previous

---

[35] Indeed there is a considerable element of humour in Shapin's account of Boyle's relationship with his servants and assistants: Boyle's servants, plucked off some mundane task, running about in the winter cold to carry out what must have seemed to them another quirky request of their pensive master, to see whether hot water would turn to solid ice more quickly than cold water, thus smiting another blow to the Aristotelian conception of nature. See Shapin, AST, pp. 387-388.

[36] Shapin, AST, pp. 362-365, 374, 382-383, 403-406; also see Shapin, "Invisible Technician," pp. 557, 560, particularly for Shapin's emphasis that morally both gentlemen and servants saw nothing wrong in the fact that the former took most of the credit and the latter did most of the work.

understandings of the world provide the blueprints for future knowledge making.[37] The following section will provide a critical appraisal of some of AST's signature arguments and claims that underlie this image of science.

4.2 A Critical Appraisal

AST is highly recommended because of its engaging interpretation and well-employed historicism. The sense which Shapin's notion of historicism carried was described in Chapter 2, and AST was an excellent realization of it. To some degree, Shapin's increased sensitivity to epistemological factors mitigates the concerns I expressed in regards to LAP in the last chapter. Nonetheless, there are problems. Primarily, these relate to the difficulty of relying solely on gentlemanly codes as the foundations of credibility in seventeenth-century English science. An overly heavy reliance on a particular sociological interpretation detracts from the importance of the scientist's actual epistemic interest: the natural world. What is needed is a concept of "intellectual awe," which I will sketch over the course of this critical section. In short, this concept allows rational faculties and social factors to work together historiographically to show how historical agents establish communal acceptance for particular epistemic claims. Let us examine these issues in detail.

With its probing vision and mass of well-organized data, Shapin's AST provides an excellent account of how science's history (if only one particular episode) unfolded.

---

[37] Shapin, AST, p. 200.

All historians of science are exposed to the failures of epistemological individualism in the actual historical episodes of science's past. The very nature of the historian's enterprise entangles him or her in the vagaries of actual action, the failure of life to remain consistent, the meanderings of the human mind from the ideal path of reason. Interestingly, Shapin's epistemological collectivism ameliorates the situation, providing a social lens that cuts right to the epistemological heart of science. Shapin's fusion of the social with the scientific is thus a seminal achievement, and makes for stimulating reading.

What most recommends Shapin's epistemological collectivistic interpretation of the history of science is its historicism. Shapin's account of Boyle is immersed in the events, even the minutiae, of the past. There is a strong sense of continuity in Shapin's account: Boyle and his peers do not simply break with the past in a moment of revelation, but rework and manipulate their intellectual, social and historical environments to effect change. Although this is possibly less romantic and less ambitious than showing how a scientific revolution took place in these years, it does resonate plausibility.[38] Boyle's period was one of vigorous change in how human beings (predominantly Europeans) looked at the world. Such change took hard work, and had to be based on some traditions. From another angle, Shapin's historicism respects the past. For instance, consider a statement made by Shapin in characterizing a common internalist tendency in the history

---

[38] This theme was taken up by Shapin in his book on the Scientific Revolution. See Shapin, The Scientific Revolution (Chicago: University of Chicago, 1996): pp. 1-4.

of science: "Instead of discerning the internal and external in historical actors' terms, analysts have been overwhelmingly content to speak of scientific boundaries *as if they were those obtaining or normative in present-day science*."[39] Shapin in AST certainly did show a great respect for the epistemic culture of seventeenth-century England. This dedication to writing the history of science with the aim of showing what the historical agents in question saw as legitimate knowledge is laudable, and helps to moderate if not correct the common tendency towards presentism: the interpretation of the past purely in terms of the present. The discovery of scientific truth indeed does have a history, and social factors played a role in how this history unfolded. A sociological approach to the history of science can result in a deep appreciation of the power of events and the historical dimension of human stories about truth.

The main problem with Shapin's interpretation in AST is that too much pressure is put on sociological factors to explain the rise of the scientific establishment. Shapin made an insightful argument in indicating the importance of gentlemanly codes in establishing the origins of scientific credibility in seventeenth-century England, but the primacy of this code can be exposed to criticism that challenges Shapin's social image of science. Shapin stated that "the setting" he was describing was a place "where many aspects of what now count as reliable truth-generating practices were put in place and institutionalized . . . "[40] This is no doubt true. Judging by the focus of the rest of the book, it is clear that Shapin

---

[39] Shapin, "Discipline and Bounding," p. 351. Shapin's italics.

[40] Shapin, AST, p. 5.

was contending that Boyle "put in place and institutionalized" these "practices." While Boyle's significance in this regard might be somewhat exaggerated, there is no doubt that he was an important figure.[41] Through drawing in considerations of Sir Isaac Newton's prominence, an argument in favour of a notion of "intellectual awe" can be articulated, a notion that might temper and contribute to Shapin's epistemological collectivism. First, I will set up a Newtonian counterpoint to Shapin's Boyle-based interpretation, my basic argument being that Newton's scientific accomplishment helps see Shapin's claims in broader terms, and shows some of the historical weaknesses attendant to them. Then, I will reexamine a theme prevalent in the last chapter: that of the role of nature and the intellect in human knowledge. In the case of AST, Shapin's tendency was to drain historical agents of real intellectual significance, ascribing all seemingly intellectual accomplishments to social and personal features.

Shapin consistently treated Boyle as if he were a gloss on a considerable portion of subsequent scientific practice. Boyle was no doubt important to the young scientific establishment in England specifically, and in Europe more generally. His sustained defence of the role of experiment in studying nature and his constant attempt to arrive at

---

[41] It is worth noting the individualistic significance Shapin ascribed to Boyle, a tendency ironic in the face of his sociological, anti-individualistic leanings. Even if Boyle built his credibility from social factors, it was nonetheless done by an individual. From a historiographical perspective, individuals seem to play an irreducible role in science. Depending on the individualism invoked, this need not necessarily assault a sociological sensibility, but is nonetheless a notable feature of AST, considering Shapin's overall assault on individualism. Interestingly, less sociologically ambitious historians have avoided this pitfall. For instance, see Michael Hunter, Science and Society in Restoration England (Aldershot: Gregg Revivals, 1992): Foreword, pp. 32-58.

experimental solutions to stubborn natural questions indeed meant that he rose to prominence and made real contributions to the development of a scientific mentality and community. Newton subsequently built upon Boyle's contributions. Boyle reached his ascendancy in the 1670s, and died in the last decade of that century. Newton had risen to prominence, even dominance, in the Royal Society by the late seventeenth century, with the publication of the Principia in 1687 being a watershed. Boyle was alive and philosophically active during the same period as Newton. Boyle lived to see Newton become the dominant intellectual figure in England, and in much of Europe. Newton's influence was strongest in the Royal Society, the very vehicle Boyle had used to promote his own vision of natural philosophy. Newton came to dominate the same community Boyle inhabited. Certainly, Boyle's significance as an exemplar of the new natural philosophic practice was considerable in the rest of Europe; regarding Newton, there is absolutely no doubt in this regard, particularly after the publication of the Principia.[42] These considerations show the combined importance of the Boylean-Newtonian achievement.

However, it should be pointed out that there were considerable differences between the Boylean and Newtonian conceptions of natural philosophy, and that Shapin was not unaware of this concern. On at least a pair of occasions in AST, he alluded to the considerable sea change that occurred in English natural philosophy with the rise of Isaac

---

[42] Richard S. Westfall, Never at Rest: A Biography of Isaac Newton (Cambridge: Cambridge University, 1980): pp. 472-473.

Newton.[43] Elsewhere, Shapin contended that the Boylean and Newtonian models of scientific practice remained current side by side, characterizing them as different games to which scientists might turn to describe their work in different situations.[44] There is no doubt some truth to this, both philosophically and historically. From a philosophical perspective, the probabilistic epistemology which Boyle favoured offered some respite amidst sharp and persistent intellectual debates, while Newton's more absolutist intellectual streak offered a persuasiveness and strength that might also appeal to intellectuals. However, judging from Newton's vitriolic exchanges, he did not entirely embrace Boyle's gentlemanly practice - the eminent philosopher and mathematician Gottfried Leibniz was but one of the foes that Newton actively and bellicosely worked to besmirch with allegations of error and ill intent.[45]

Society trumps intellect in Shapin. He revealed much about Boyle's social origins: his nobility, his wealth, his political power, and so on. Shapin did not want to suggest Boyle's position within the natural philosophical community was some sort of deceit, based on influence and power. Shapin did want to convince the reader, however, that Boyle's social standing had formative consequences for his way of looking at the natural

---

[43] Shapin, AST, pp. 185, 312.

[44] Shapin, Scientific Revolution, pp. 111-117.

[45] Many historians and sociologists of science point to the relative stability within science, and the rarity of sharp, tumultuous dispute; for example see Shapin, AST, pp.19-21. For more on Newton's battles with Leibniz, see A.R. Hall, Philosophers at War: The Quarrel between Newton and Leibniz (Cambridge: Cambridge University, 1980): passim.

world, for promoting his views, and seeing them institutionalized. Boyle's gentlemanly conduct in society was imported into his activity in natural philosophy. Shapin's argument was that people trusted Boyle because of his social and cultural comportment. For the most part, AST contains no attempts to show that Boyle possessed formidable intellectual capabilities, powers of observation and description, intense concentration or any other such skills. This ability is taken for granted; it is as if Shapin considered the power of intellect, dismissed it, and sought for other means with which one might convince others of epistemic worth. This pessimistic assessment of the power of argument and intellect to persuade, emerging from Bloor's notion of social causation described in Chapter 2, is a central theme of Shapin's epistemological collectivism, but it is unwarranted. The social characteristics of Newton help elucidate this conviction.

The fact that Newton represented the pinnacle of natural philosophic achievement for centuries afterwards demands that the historian modify and extend the explanatory methods of Shapin's epistemological collectivism. Consider Newton's relatively humble social origins: he came from a yeoman family, not ranked among the upper echelons of society, and certainly far beneath Boyle on the social ladder. He did not have the financial means of the gentleman, nor the social comportment. Philosophy was his career, the major source of his income. His epistemological tendencies were not probabilistic and tolerant, nor did they eschew the identification of causes. He used the sophisticated language of mathematics, including "fluxions," the calculus he himself devised, to describe nature. Finally, Newton waged open and vicious battle with eminent intellectual

rivals, including Boyle's protégée Robert Hooke, the first Astronomer Royal John Flamsteed, and Leibniz. In other words, Newton conducted himself with little humility, little decorum, and a very unBoylean degree of confidence.[46] Shapin's Boyle, on the other hand, played up to his fellows through accentuating his nobility, showed his disinterestedness and diffidence towards philosophy, and construed philosophy as the study of observable phenomena, eschewing causes. Boyle saw mathematics as a threat to the philosophic community, while Newton used his mathematized natural philosophy to conquer the very community to which Boyle propounded this argument. Boyle attempted with genteel graces to use natural philosophy to facilitate conversation, while Newton tried with vitriol to use it to crush religious and intellectual opponents. If Boyle was very much the gentlemanly noble, then Newton was very much the disputatious scholar. The reader is left with one conclusion: that if Shapin has accurately described how credibility manifested itself in seventeenth-century England, then Newton should not have been a significant figure in that nation's intellectual history. That he indeed was recommends that the historian develop new concepts to rectify this short-coming of Shapin's epistemological collectivism.

In particular, Shapinian historical agents need revamped intellectual capacities merged with their social skills. Shapin downplayed intellectual perspicacity in the name of social adroitness. While almost infinite social variation exists, in Shapin's universe

---

[46] In fact a visage hiding considerable inner doubt. See Westfall, Never at Rest, pp. 699

human agents seem to be pretty much intellectually uniform. Yet Boyle was obviously of a more intellectually alert mind than many of his contemporaries: his philosophical writings and experimental activities point to this. Many of Boyle's contemporary peers lagged behind his abilities, and they were duly impressed with his powers of mind and expression. Although his prominence cannot solely be ascribed to his intellectual acumen or his social dexterity, a combination of the two seems likely. To use a more extreme example, Newton's powers of observation, mathematical skill, intellectual creativity, focus of mind, intensity of concentration, scope of imagination - in short, his intellect - leave little doubt on this head: they were awesome, beyond the pale of most of his contemporaries, and indeed many subsequent generations.[47] It may be true that all of these would mean little in the face of a concerted effort on the part of society to ignore or suppress the intellectual prowess of a great mind; it may be true that a complete social deviant or bumbler possessing such prowess might not end up a recognized philosophical/scientific authority; but in the case of Newton it seems plausible that these abilities were crucial in founding his credibility through the respect they garnered. Newton had few of the cultural tools, but plenty of the intellectual skills, to accomplish great things in a natural philosophical community primarily inhabited by gentle and noble

---

[47] For famous examples of the products of Newton's intellect, see Isaac Newton, Principia, trans. Andrew Motte and Florian Cajori (Berkeley: University of California, 1946): passim; Opticks, trans. Silvanus P. Thompson (Toronto: Encyclopedia Britannica,1952): passim. For an example of the difficulties of contemporaries in grasping Newton's thought, consider John Locke's approaching Huygens for assistance with Principia - see Ed. I. Bernard Cohen, Newton's Papers and Letters on Natural Philosophy, 2nd ed. (Cambridge: Cambridge University, 1958): p. 402.

folk. Even in the face of his most obvious deficiencies for living in gentle society, others pushed aside their concerns, or at least allowed them to cohabit with their respect for his philosophic abilities.

The sociologically based epistemology Shapin relied on makes the intellect purely a matter of social form over intellectual content. As has been said in the previous chapter, Shapinian historical agents therefore operate in a world drained of epistemic significance. Their persuasive capacities reside in social image, their intelligence appears in their dress and mannerisms, their success resides in making strong buddy-networks that avoid scathing intellectual censure. Rarely do perspicuous observations, creative intellectual solutions, and the like come into play. What Shapin has done is draw our attention to the fact that social, cultural and personal factors do reside in our appreciations of intellectual worth; in his zeal, however, he has downplayed real intellectual ability to the point where its need seems obviated.[48] This is unwarranted. To fuse social and cultural factors with the history of science properly does not require that the intellect be abandoned. Let me be emphatic on this point: my reading of Shapin is that he saw intellect and society as incompatible. Reacting to earlier historiographical trends that degraded or ignored the social in favour of the intellect in science, Shapin's revision went too far in the opposite direction, making it seem as if social factors, conceived of as being fundamentally non-

---

[48] For instance, consider Shapin's caveat when introducing his examination of Boyle's attitudes towards mathematics: "Nothing that I want to say about these episodes depends upon deciding the real state of Boyle's mathematical skills." This statement nicely summarizes Shapin's tendency to shunt aside issues pertaining to the power of the intellect. Shapin, AST, p. 314.

intellectual, were the most pertinent elements. Some historians had dispensed with the reality of social influence in science - Shapin responded by dispensing with the reality of the perspicacious intellect. However, the sociological study of science need not entail this conclusion. Both may be real, and the division of the two may be a widespread product of the human ability to analyse his or her situation; to employ Shapin's own terminology, the division may be an institution, a boundary established over centuries, if not millennia, of cultural discourse. Their mutual reality, therefore, may mean they are mutually dependent, and both may require elucidation in a manner which shows this.

What is needed is a notion that combines the two. I advance the notion of "intellectual awe," which suggests that both the historical actor's intellectual power and his audience's ability to access and interpret that power are crucial to writing effective history of science. Audiences of thinking, critical individuals can recognize intellectual ability and acknowledge it, no matter the dress or mannerisms of the person presenting knowledge claims. Without this intellectual component, historically unsatisfying accounts of the history of human knowledge are inevitable, accounts that fail to describe why someone like Newton was so prominent in human history, or unfairly portray Boyle as more schmoozer than natural philosopher. In Shapin's terms, Newton should have been a profoundly *incredible* speaker about nature. That audiences did not consider him in this way speaks to oversights in Shapin's interpretation. The awe of Boyle's and Newton's audiences in the face of their intellects was a massively important factor in founding

natural philosophical credibility, an awe around which audiences may have constructed social explanations to make the philosophers acceptable to society.[49]

Newton's success challenges two key components of Shapin's epistemological collectivism: the idea of the free agent; and the idea that epistemic legitimacy is a function of social conformity. The key component of Shapin's sociology of knowledge is the role of the disinterested, free agent. Shapin emphasized that Boyle was a free agent, financially free to pursue his academic pursuits as hobbies, and that this disinterest established the foundations of his epistemic legitimacy. Yet Newton was not noble, not rich. His wealth largely came from his truck in philosophy. The whole culture Boyle founded quite clearly did not contain much for Newton to build upon, but yet he seized the philosophic upper-hand convincingly in Boyle's lifetime, in the very same philosophical community. The character of Newton's ascendance may lead the reader to ask: did that culture of credibility which Shapin used to explain Boyle's success exist at all? His neglect of the intellect in the Boylean achievement may leave just such a residue of doubt in the reader's mind.

---

[49] A successful social and cultural portrayal of Newton does not seem impossible, although it would not fit the gentlemanly model Shapin developed; possibly a rectified vision of the scholar might be the path to such an interpretation. A number of distinctly social, cultural and moral tethers could attach to Newton's credibility. Robert Iliffe, for instance, has examined the importance of Newton's self-portrayal as an absent-minded, melancholy, sickly philosopher as a social signifier of his epistemic credibility. See Rob Iliffe, "Isaac Newton: Lucatello Professor of Mathematics," Science Incarnate: Historical Embodiments of Natural Knowledge, eds. Christopher Lawrence and Steven Shapin (Chicago: University of Chicago, 1998): pp. 121-155.

Some final remarks. AST continued many of the themes that Shapin had developed jointly with Schaffer in LAP. Society is still the primary factor in the scientist's endeavours. It shapes his attitudes, methods, and conclusions. Knowledge remains highly anthropocentric, speaking equally if not more about the people who make epistemic products as about the natural phenomena those epistemic products ostensibly describe. Nonetheless, there is discord. The most noticeable change was a significant downplaying of the idea that science had a distinct and dominating political ethic. Boyle's political goals are far less pronounced in AST; Shapin has translated the political energy that charged LAP into the motivation to establish oneself as credible and trustworthy, with such attributions seeping across social categories. In short, in AST Shapin was more concerned with showing how science is a cultural and moral collective enterprise based on trust, as opposed to a *political*, cultural and moral collective enterprise based on social strife. Shapin wanted to show how knowledge is made and justified in society, not necessarily how knowledge aims to shape and mould society. Shapin was generally successful in executing this thesis, a more subtle and palatable option than that of LAP.

Despite the many social factors that weigh on the scientist, despite the possibility of these social factors shaping both the production and reception of knowledge claims, canons of intellectual distinction still exist. The implication of Shapin's writing in AST, despite the fact he provided means to escape the prevalent dichotomy of intellect versus social, was that the role of society excluded the role of the intellect. My aim through the notion of intellectual awe has been to show that part of the solution is that the foundations

of scientific knowledge are a constant interplay between scientist and audience, thus meaning there are irreducible elements of both the social and the intellectual, should we wish to continue to honour this dichotomy. Their social character will always be influenced by the external universe, and the determination with which one pursues defining and describing that universe. Whatever the case, Shapin is guilty of overemphasizing the universal importance of social factors at the expense of the human intellect.

Chapter 5
Conclusion: Sociological Continuity, Interpretational Variety: Society in the Thinking of Steven Shapin

Margrethe Physics, yes? Physics.

Bohr This is physics.

Margrethe It's also politics.

Heisenberg The two are sometimes painfully difficult to keep apart.

Michael Frayn, Copenhagen (1998).

Shapin's writings on science provide insight into the relationship between society and science, and ambitiously attempt to show how the former fundamentally shapes the latter. The previous chapters have shown Shapin's unyielding dedication to providing sociological explanations for science, and in both Leviathan and the Air-Pump (LAP) and A Social History of Truth (AST) it is clear that this dedication results in important findings. Primarily, they both show that reasonable cases can be made for the idea that social considerations permeate science to its very core. Nonetheless, the approaches found in LAP and AST do differ, particularly in what social factors they think significant for the historian's attention. LAP emphasized elements of the social struggle between classes, whereas AST delved deeply into a society's culture, ethics and canon of interpersonal interactions, with less emphasis on the politics of these attributes. Probing these differences and unravelling their consequences will conclude this exploration of Shapin's thought and provide a critique of Shapin's approach to writing the history of science.

What is perhaps most interesting is the *historicizing* of scientific knowledge that results from the sociological treatment. Obviously, the very existence of the academic

discipline of "history of science" suggests the possibility of a historical contribution to understanding science. However, an interpretation such as Shapin's brings history right to the heart of scientific knowledge itself, providing historically specific explanations for specific knowledge claims. As described in Chapter 2, Shapin, basing his move on the thoughts of Thomas Kuhn and the Edinburgh Strong Programme, was part of a tradition of trying to bring historical elucidation to the core of scientific activity itself, erasing borders that placed society "outside" science. In LAP and AST, Shapin showed such historicization to be a result of a sociological approach. Displaying the power of historical inquiry in such a manner cannot but excite historians.

The particular character of such sociologically founded historicization depends, of course, on the particular character of the sociological tools employed - it is hardly surprising that sociology is not one, unified set of methods and approaches. LAP uses sociological techniques to explain science, but what vision of society did Shapin employ to elucidate science's success and power? As indicated in Chapter 3 themes of a society fraught with social strife and competing social groups run through LAP. Shapin largely distinguished Hobbes and Boyle by their membership in different social classes: Boyle is clearly a member of the English nobility, while Hobbes less clearly belongs to some sort of middle class. When their intellectual worlds collide, their social worlds collide, with their respective memberships rallying around their man. From the standpoint of the history of ideas, the Marxian tincture of Shapin's thought is more pronounced: ideas matter less than actions; social, economic and political power decide scientific questions,

not trenchant arguments and well-couched criticisms. Class loyalties matter more than intellectual acuity. Power derived from social factors governs intellectual debate.

AST too is dedicated to providing a sociological explanation of science, but the vision of society that it chooses hinges less on the politics of a class society than the dependence of intellectual credibility on local cultural attributes. Here Shapin examined how one makes oneself believable, how one fits an intellectual viewpoint into a general cultural scheme that will lend the ideas credibility and therefore power. The power of belief rests not in the intellectual consistency of an idea, but in the possibility of rendering that idea amenable to popular notions of decorum, propriety, and knowledge. Boyle's contemporaries approved of him as polite, generous, authoritative - in short, noble - which gave credit to his ideas, for the most part in Shapin's presentation regardless of their intellectual worth. The Shapin of AST imbued ideas with socially-founded credibility, not class-based power.

The differences in these two sociological approaches have significant consequences, not the least of which is that the historian who adopts the approach of LAP will find his attention drawn to quite different features of society than the historian following AST's techniques. The LAP-style historian would want to find fault lines in the social structure of the community of people practising science, and then trace how variance and controversy in scientific ideas closely parallelled these differences. On this model, intellectual conflict represents the surface of deeper socially based, interest-driven conflict. In the AST camp, the historian would want to isolate the cultural community of

the proponent of a certain scientific idea, and then describe the cultural resources available at that time and place to make the idea credible. The internal dynamics of the social group are crucial here, as the historian is at pains to show how the idea in question gains currency amongst the social actors in a community. Notably, the actors that make up an epistemic community need not be all of the same class, and although the historian must note this variation in status, AST provides tools to explain what dynamics allow for different social actors to form one epistemic community. LAP is not so flexible.

These difference resulted in two different books. LAP and AST certainly read quite differently, LAP being much more pessimistic about the foundations of modern science, whereas AST offers a more optimistic alternative to traditional realist-individualist epistemologies of science. Primarily, these different feels can be ascribed to the fact that the Shapin of LAP constantly shone light on the besmirched ideas of Hobbes, which Boyle and his scientist progeny seem to have given an unfairly bad rap. In AST, however, Shapin matter of factly described how Boyle went about building credibility in the only way open to him or any other scientist: through the cultural means available to him in his society. The difference here is that the light of an older epistemology occasionally pierced the clouds of LAP: the belief that intellectual worth means something independent of what society may have to say about it. Ironically, the world of LAP still has vestiges of a realist epistemology, where the worth of ideas can be measured against something extra-social, but where such measurements are quashed by those whose only epistemology is fundamentally a brutal calculus of power and self-aggrandizement.

There are the fumes of conspiracy in the pages of LAP: Boyle and his minions crushed the scientific notions of Hobbes using a social stratagem, not a frontal intellectual assault.

AST, on the other hand, seems more comfortable with the social interpretation, cogently offering arguments for the social foundations of Boyle's knowledge system. AST's world has a completely socialized epistemology, where historical agents forge ideas using the materials of culture and society available to them in their particular juncture of time and space. There is a far reduced sense in AST of any intellectual wrong-doing on the part of Boyle and company, because, in the intellectual universe they inhabited, ideas were exchanged in the coinage of culture. There was no realist "answer" veiled behind a screen of aristocratic intrigue and manipulation. Thus, should a culture survive, then likely its ideas would survive with it. There can be no controversy in this world about whether Hobbes was right or Boyle was right, because social acceptance is the only factor. Shapin thoroughly purged realism from the pages of AST in a way he failed to do in LAP. The notion that science might be "reformed" once its suspect social origins are revealed lurks in LAP; such hopes of reform found no place in AST.[1] Shapin was at peace with a thoroughly social interpretation in AST, a peace which lessened his

---

[1] Ian Hacking's reflections on the "gradations of constructionist commitment" help elucidate Shapin's attitudes in LAP and AST. Hacking listed six grades defining the ends of constructionist thinking such as Shapin's sociological interpretation. These were historical, ironic, reformist, unmasking, rebellious, and revolutionary. The Shapin of LAP can be described as rebellious, if not revolutionary, whereas the AST-Shapin is more ironic-reformist. See Hacking, The Social Construction of What? (Cambridge: Harvard University, 1999): pp. 19-21.

scepticism towards modern science, an irony which no doubt should give many of his "realist" critics pause to think.[2]

Does this mean that the interpretations put forth in LAP and AST are incompatible? Superficially, no. The interpretation of LAP is a case study in the dynamics between conflicting ideas. AST attempts to establish the social foundations of credibility. Shapin could plausibly argue that the processes outlined in AST create the ideas that then come head to head in the arena of conflict described in LAP. Both Hobbes' and Boyle's ideas had credibility in their respective social circles, but when they met in a competitive environment, the superior social power of Boyle and his associates decided the day.[3]

Go a little deeper, however, and this argument for compatibility needs to be qualified. The matter hinges on the concept of trust and the concept of society. AST explicitly aimed to show that trust is the founding component of all knowledge. Shapin did this quite adroitly. LAP wanted to dissolve simplistic notions of scientific truth by extending social competition into the heart of scientific practice. There is considerable tension here. The question that must be asked is: how compatible are Shapin's strife-oriented view of society in LAP and his trust-based view of science in AST? The fact is

---

[2] Ralph Estling, "Is Science Concerned with Truth?" Skeptical Inquirer 22, 4 (July/August 1998): pp. 55-56. Although not specifically concerned with Shapin, this article nonetheless reveals one of the general "pro-science" criticisms of the type of scholarly work Shapin pursues.

[3] Although, as pointed out in Chapter 2, exactly what social clique Hobbes represented is unclear. See Douglas Jesseph, Squaring the Circle: The War between Hobbes and Wallis (Chicago: University of Chicago, 1999): p. 351.

that AST considerably toned down the strong parallel between social-economic class with epistemic loyalty. Social origins still play a role, but one that is more productive and less provocative than in LAP, showing that the boundaries of social classes are porous to trust. AST made a strong case for how various members of the scientific community interacted, and this included Anglo-Irish nobles like Boyle, French scholars like Auzout, non-noble experimentalists like Hooke, Boyle's servants and so on. Hobbes and Boyle may have occupied different social spaces, but they were not radically different, and their social spaces certainly overlapped in a number of places. Social tensions were either absent or managed quite successfully in this interpretation, lacking the manipulative, even bellicose force of LAP. Further, if strict class breakages were present in LAP, then the lack of internal divisions is also notable. Shapin failed to show any competition within the nobility to attain the mantle of philosophic supremacy. Why not? Did not some of Boyle's social equivalents develop their own ideas about nature, ideas which ran counter to Boyle's? A weakness of many socially minded histories is that they downplay individual characteristics to the point of non-existence, and the Shapin of LAP headed too far in this direction. Neither is AST strong on this head, but at least it made some strides with its examination of the Hevelius-Auzout controversy, even if this still lent the impression of a strict unity within the English philosophic community. It is not novel to remark that pride and competition can indeed be present within communities, and these can have deleterious effects on the abilities of one individual to lead intellectually. The flow of trust

and struggle is not limited to the borderlands of communities, it may simply be more pronounced there.

So which of the two approaches is more appropriate for the historian of science? LAP certainly makes for exhilarating reading. The interpretation tackles head on the issue of society's role in scientific knowledge. There is great drama in the struggle of opposing scientific views as each side marshals its forces. Its boldness is striking: Shapin vigorously took sociology to the heart of historical explanation of scientific knowledge, revealing a great confidence in the completeness and accuracy of this approach, a confidence which cannot but impress the reader.

Nonetheless, the interpretation Shapin crafted in AST provides a better starting point for the historian of science, primary because it is a cool-headed and flexible approach. LAP's exhilarating character has much to do with its vigorous espousal of the sociology of science over realist schools of explanation. AST, despite its toned-down language, is much more careful, and makes the case for the sociology of historical events in science more through detailed argument than daring attack. Shapin established AST's sociological mechanism much more thoroughly, directly bringing to bear an array of sociological and philosophical sources and original thought in a unique synthesis. The care with which Shapin built his theoretical platform and then extended it to his actual episodes of historical interpretation may not be as thrilling as the assertive approach embodied in LAP, but it does clearly present the strengths and weaknesses of the sociology of science.

Both approaches, however, suffer from an inability to adequately integrate the role of nature and intellect into the social image of science. Fundamentally, natural science is about the universe: atoms, genes, meteors, clouds, stars, rocks, bones - all those things which constitute the physical universe. Shapin and his Edinburgh Strong Programme mentors made knowledge a matter of collective belief, a move which was hostile to the naive realism of most scientists and the laity. Part of this is a necessary component of their struggle against a realism which disallows a functional, positive role for human society in the development of science. Yet, the pendulum has swung too far to the social side of its arc, and some re-balancing is in order to show exactly what sort of constraints "reality" places on historical human actors. I will not pretend to have an answer for this stubborn question, but the notions of epistemological sensitivity and intellectual awe do offer, I believe, some hope to the historian of unifying an insightful sociological vision of science with a concept of nature that evades traditional problems of induction and realism.

A final, quite unintentional consequence of Shapin's sociological approach is that of intellectual complacency. The arguments of LAP and AST may suggest that, because either power or culture determines the course of knowledge no standard exists by which to measure knowledge, and therefore scientists and academics may continue on in their own veins of thinking without ever taking criticism seriously. Shapin, I am confident, would not embrace the complacent thinking that backs this sort of pawn-shop relativism. The poverty of this should be obvious to all. The high calibre of the research and argument found in Shapin's oeuvre itself should suggest to the reader that the intellectual culture of

which we partake is capable of generating high standards by which scholarship can be measured, whatever the "final" foundations of our knowledge systems. Intellectual integrity may be rare, but it does exist, as an eye to Shapin's own fastidiousness of evidence and argument should adequately prove.

In the end, what the differences between LAP and AST show is that faith in the ability of sociological characteristics to explain science does not result in a uniform interpretation of science. Indeed, the historian must choose what sort of society will be employed to explain science. He or she sets the social standard. This choice carries all the biases, strengths, limitations and insights of any choice. Some choices, however, are clearly better than others. The choices Shapin made in AST are the more promising ones for the history of science.

Steven Shapin Bibliography

Barnes, Barry and Steven Shapin.1977a. "Science, Nature and Control: Interpreting Mechanics' Institutes." Social Studies in Science 7: pp. 31-74.

-----. 1977b. "Where is the Edge of Objectivity?" British Journal for the History of Science 34, 10: pp. 61-66.

-----. 1979a. "Darwin and Social Darwinism: Purity and History." Natural Order: Historical Studies of Scientific Culture. Beverly Hills: Sage. pp. 125-142.

-----, eds.1979b. Natural Order: Historical Studies of Scientific Culture. Beverly Hills: Sage.

Collins, Harry and Steven Shapin. 1989. "Experiment, Science Teaching and the New History and Sociology of Science." Teaching the History of Science. Eds. Michael Shortland and Andrew Warwick. Oxford: Blackwell.

Lawrence, Christopher and Steven Shapin, eds. 1998. Science Incarnate: Historical Embodiments of Natural Knowledge. Chicago: University of Chicago.

-----. "Introduction: The Body of Knowledge." 1998. Science Incarnate: Historical Embodiments of Natural Knowledge. Eds Christopher Lawrence and Steven Shapin. Chicago: University of Chicago. pp. 1-19.

Ophir, Adi and Steven Shapin. 1991. "The Place of Knowledge: A Methodological Survey." Science in Context 4, 1: pp. 3-21.

Schaffer, Simon and Steven Shapin. 1985. Leviathan and the Air-Pump: Hobbes, Boyle and the Experimental Life. Princeton: Princeton University.

Shapin, Steven. 1972a. "The Pottery Philosophical Society, 1819-1835: An Examination of the Cultural Uses of Provincial Science." Science Studies 2: pp. 311-336.

-----. 1972b."The Royal Society of Edinburgh: A Study in the Social Context of Hanoverian Science." Dissertation Abstracts International 32: 6868-A. (Doctoral Thesis, University of Pennsylvania, 1971; Supervisor Arnold Thackray)

-----. 1974. "The Audience for Science in 18th-century Edinburgh." History of Science 12: pp. 95-121.

-----. 1974b."Property, patronage, and the politics of science: The founding of the Royal Society of Edinburgh." British Journal of History of Science 7: pp. 1-41.

-----. 1975."Phrenological Knowledge and the Social Structure of Early 19th-century Edinburgh."Annals of Science 32: pp. 219-243.

-----. 1979a."Homo phrenologicus: Anthropological Perspectives on an Historical Problem." Natural Order: Historical Studies in Scientific Culture. Eds. Steven Shapin and Barry Barnes. Beverly Hills: Sage. pp. 41-71.

-----. 1979b. "The Politics of Observation: Cerebral Anatomy and Social Interests in the Edinburgh Phrenology Disputes." On the Margins of Science: The Social Construction of Rejected Knowledge. Ed. Roy Wallis. Keele: Keele University Press. pp. 139-178.

-----. 1980a. "A Course in the Social History of Science," Social Studies of Science 10: pp. 231-258.

-----. 1980b."Social Uses of Science." The Ferment of Knowledge: Studies in the Historiography of Eighteenth-Century Science. Eds. George S. Rousseau and Roy Porter. Cambridge: Cambridge University. pp. 93-139.

-----. 1981."History of Science and its Sociological Reconstructions."History of Science 20: pp.157-211.

-----. 1982."Of gods and kings: Natural Philosophy and Politics in the Leibniz-Clark Disputes." Isis 72: pp.187-215.

-----. 1983a."The Historical Role of the Experiment." Proceedings of the International Conference on Using History of Physics in Innovatory Physics Education. Pavia: University of Pavia.

-----. 1983b."'Nibbling at the Teats of Science': Edinburgh and the Diffusion of Science in the 1830s." Metropolis and Province: Science in British Culture, 1780-1850. Eds. Ian Inkster and Jack Morvell. London: Hutchinson.

-----. 1984a."Pump and Circumstance: Robert Boyle's Literary Technology." Social Studies of Science 14: pp. 481-520.

-----. 1984b. See Isis 75: pp.125-130, for Shapin's comments.

-----. 1987."O Henry [essay review of Oldenburg, Correspondence]." Isis 78: pp. 417-424.

-----. "Closure and Credibility in 17th-Century Science." Conference proceedings of the Joint Conference of History of Science Society and British Society for the History

of Science. pp. 147-154.

-----. 1988a."The House of Experiment in Seventeenth-Century England." Isis 79: pp. 373-404.

-----. 1988b."Robert Boyle and Mathematics: Reality, Representation and Experimental Practice." Science in Context 2: pp. 23-58.

-----. 1988c."Understanding the Merton Thesis." Isis 79: pp. 594-605.

-----. 1989a."The Invisible Technician." American Scientist 77: pp. 554-563.

-----. 1989b."Who was Robert Hooke?" Robert Hooke: New Studies. Woodbridge: Boydell, pp. 253-285.

-----. 1990. "Science and the Public." Companion to the History of Modern Science. Eds. G.N. Cantor, J.R.R. Christie, M.J.S. Hodge, R.C. Olby. Routledge: London, pp. 990-1007.

-----. 1991a."'The Mind is It's Own Place': Science and Solitude in Seventeenth-Century England." Science in Context 4: pp. 191-218.

-----. 1991b."'A Scholar and a Gentleman': The Problematic Identity of the Scientific Practitioner in Early Modern England." History of Science 29: pp. 279-327.

-----. 1992a."Discipline and Bounding: The History and Sociology of Science as Seen through the Externalism-Internalism Debate." History of Science 30: pp. 333-369.

-----. 1992b. "Why the Public Ought to Understand Science-in-the-Making." Public Understanding of Science 1: pp. 27-30.

-----. 1993."Personal Development and Intellectual Biography: The Case of Robert Boyle." British Journal for the History of Science 26: pp. 335-345.

-----. 1994. A Social History of Truth: Civility and Science in Seventeenth Century England. Chicago: University of Chicago.

-----. 1995a."Cordelia's Love: Credibility and the Social Studies of Science." Perspectives on Science: Historical, Philosophical, Social 3: pp. 255-275.

-----. 1995b."Here and Everywhere: Sociology of Scientific Knowledge." Annual Review of Sociology 21: pp. 289-321.

-----. 1995c. "Truth, Honesty, and the Authority of Science." Society's Choices: Social and Ethical Decision Making in Biomedicine. Eds. Ruth Ellen Bulger, Elizabeth Meyer Bobby, Harvey V. Fineberg. Washington: National Academy, pp. 388-408.

-----. 1996. The Scientific Revolution. Chicago: University of Chicago.

-----. 1998a. "The Philosopher and the Chicken: On the Dietetics of Disembodied Knowledge."Science Incarnate: Historical Embodiments of Natural Knowledge. Eds. Christopher Lawrence and Steven Shapin. Chicago: University of Chicago. pp. 21-50.

-----. 1998b."Sailing Scientist." London Review of Books 2 July: pp.12-13.

-----. 1999a. "Foreward to the 1999 Edition." in Daniel S. Greenberg, The Politics of Pure Science. New Edition. Chicago: University of Chicago, 1999. pp. xv-xxi.

-----. 1999b. "Rarely Pure and Never Simple: Talking about Truth." Configurations 7: pp. 1-14.

-----. 2000. "Descartes the Doctor: Rationalism and Its Therapies." British Journal for the History of Science 33: pp.131-154.

-----. 2001. "How to be Anti-Scientific." The one culture? A Conversation about Science. Eds. Harry Collins and Jay A. Labinger. Chicago: University of Chicago, 2001. pp. 99-115.

-----. 2001. "Truth and Credibility: Science and the Social Study of Science." International Encyclopedia of the Social and Behavioural Sciences. Eds. Neil J. Smelser and Paul B. Baltes. Oxford: Elsevier Science. pp. 15926-15932.

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

Edinburgh Strong Programme Bibliography

Barnes, Barry. About Science. Oxford: B. Blackwell, 1985.

-----. "Alternative Modernity: The Technical Turn in Philosophy and Social Theory." American Journal of Sociology 102, 1 (1996): pp. 258-259.

-----. "A Case of Amnesia." Social Studies of Science 13, 4 (1985): pp.175-176.

-----. "The Conventional Component in Science and Cognition." Society and Knowledge. New Brunswick: Transactions, 1984.

-----. "Cultural Change: The Thought Styles of Mannheim and Kuhn." Common Knowledge 3, 2 (1994), pp. 65-78.

-----. "Durkheim's Philosophy of Science and the Sociology of Knowledge: Creating an Intellectual Niche." Isis 87, 4 (1996): pp. 743-744.

-----. The Elements of Social Theory. Princeton: Princeton University, 1995.

-----. "Expert Evidence: Interpreting Science in the Law." Social Studies of Science 20, 4 (1990): pp. 757-760.

-----. "How Not to do the Sociology of Knowledge." Annals of Scholarship 8 (1991), pp. 321-335.

-----. "Ideas of Difference: Social Spaces and the Labour of Division." Sociological Review 46, 4 (1998): pp. 849-851.

-----. Interests and the Growth of Knowledge. London: Routledge and K. Paul, 1977.

-----. "More Heat than Light: Economics as Social Physics, Physics as Nature's Economics." Philosophy of the Social Sciences 22, 1 (1992): pp. 112-121.

-----. The Nature of Power. Cambridge: Polity, 1988.

-----. "On Authority and Its Relationship to Power." Sociological Review Monograph 32 (1986): pp. 180-195.

-----. "On the Conventional Character of Knowledge and Cognition." Science Observed. Michael Mulkay, ed. London: Sage, 1983.

-----. "On the Implications of a Body of Knowledge." Knowledge 4 (1982), pp. 7-144.

-----. "On the Role of Interests: Scientific Change."On the Margins of Science. Roy Wallis, ed. Keele: Keele University, 1979.

-----. "Oversimplification and the Desire for Truth: Response to Mermin." Social Studies of Science 28, 4 (1998): pp. 636-640.

-----. "Power." Sociological Review 35, 3 (1987): pp. 634-637.

-----. "Power and Conflict: Toward a General Theory." Sociology 24, 4 (1990): pp. 723-724.

-----. "Power: Its Forms, Bases and Uses (2nd Edition)." Canadian Journal of Sociology /Cahiers canadiens de sociologie 15, 1 (1990): p. 119.

-----. "Problems of Intelligibility and Paradigm Instances." Scientific Rationality: The Sociological Turn. James R. Brown, ed. Dordrecht: Reidel, 1984.

-----. "The Science-Technology Query." Social Studies of Science 12 (1982), pp. 166-171.

-----. Scientific Knowledge and Sociological Theory. London : Routledge & K. Paul, 1974.

-----. "Status Groups and Collective Action." Sociology 26, 2 (1992): pp. 259-270.

-----. "Theories of Collective Knowledge: Downs, Olson and Hirsch." Sociological Review 38, 4 (1990): pp. 783-785.

-----. T.S. Kuhn and Social Science. New York: Columbia University, 1982.

-----. "Vicissitudes of Belief." Social Studies of Science 9, 2 (1979): pp. 247-263.

Barnes, Barry, David Bloor, and John Henry. Scientific Knowledge : A Sociological Analysis. Chicago: University of Chicago, 1996.

Barnes, Barry and David Edge, eds. Science in Context : Readings in the Sociology of Science . Cambridge: MIT, 1982.

Barnes, Barry and Donald MacKenzie. "Scientific Judgement: The Biometry-Medelism Controversy." Natural Order. Steven Shapin and Barry Barnes, eds. 1979.

------. "Darwin and Social Darwinism: Purity and History." Natural Order: Historical Studies of Scientific Culture. Beverly Hills: Sage, 1979. pp.125-142.

------. "Science, Nature and Control: Interpreting Mechanics' Institutes." Social Studies in Science 7 (1977): pp. 31-74.

Bloor, David. "Changing Axes: Response to Mermin." Social Studies in Science 28, 4 (1998): pp. 624-635.

-----. "Durkheim and Mauss Revisited: Classification and the Sociology of Knowledge." Society and Knowledge: Contemporary Perspectives in the Sociology of Knowledge. New Brunswick: Transactions, 1984.

-----. "Hamilton and Peacock on the Essence of Algebra." Social History of 19th Century Mathematics. Boston: Birkhäuser, 1981.

-----. Knowledge and Social Imagery. London: Routledge & K. Paul, 1976.

-----. "Left and Right Wittgensteins." Science as Practice and Culture. Andrew Pickering, ed. Chicago: Chicago University, 1992.

-----. "Polyhedra and the Abominations of Leviticus." British Journal of the History of Science 11 (1978), pp. 245-272.

-----. "Professor Campbell on Models of Language Learning and the Sociology of Science." The Cognitive Turn: Sociological and Psychological Perspectives on Science. Steve Fuller et. al., eds. Dordrecht: Kluwer Academic, 1989.

-----. "Rationalism, Supernaturalism, and the Sociology of Knowledge." Scientific Knowledge Socialized. Imre Hronszky, Marta Fehér and Balazs Dajka, eds. Dordrecht: Kluwer Academic, 1988.

-----. "Social Influences in Matter-Theory." The Physical Sciences Since Antiquity. Rom Harré, ed. New York: St. Martin's, 1986.

-----. "Some Determinants of Cognitive Style in Science." Cognition and Fact: Materials on Ludwig Fleck. Dordrecht: Reidel, 1986.

-----. "The Strengths of the Strong Programme." The Strong Programme in the Sociology of Knowledge in Philosophy of the Social Sciences 11 (1981), pp. 173-243.

-----. Wittgenstein, Rules and Institutions. London: Routledge, 1997.

-----. Wittgenstein : A Social Theory of Knowledge. London : MacMillan, 1983.

Schaffer, Simon. "Authorized Prophets: Comets and Astronomers after 1759," Studies of 18th- Century Culture 17 (1985): pp. 45-74.

----. "Comets and Idols: Newton's Cosmology and Political Theology," Action and Reaction: Proceedings of a Symposium to Commemorate the Tercentenary of Newton's Principia, Paul Theerman and Adele F. Seefe.. Newark: Delaware,

1993.

----. "Natural Philosophy and Public Spectacle in the 18th Century," History of Science 21 (1983): pp. 1-43.

----. Newtonian Cosmology and the Steady State, 1980 (Ph.D. thesis, Cambridge University).

----. "Regeneration: The body of Natural Philosophers in Restoration England." Science Incarnate: Historical Embodiments of Natural Knowledge. Eds. Christopher Lawrence and Steven Shapin. Chicago: University of Chicago,1998. pp. 83-120.

----. "The Show that Never Ends: Perpetual Motion in the Early 18th Century." British Journal of the History of Science 28 (1995): pp. 157-189.

Secondary Sources

Aaron, Richard I. John Locke. 3rd ed. Oxford: Clarendon, 1971.

Abell, George O., David Morrison, and Sidney C. Wolff. Exploration of the Universe. 6th ed. Philadelphia: Saunders College, 1993.

Agassi, Joseph. Towards a Historiography of Science. Gravenhage: Mouton, 1963.

Antonio, Robert J. "Materialism." Encyclopedia of Sociology. Vol. 3. 2nd ed. Eds. Edgar F. Borgotta and Rhonda J. V. Montgomery. New York: Macmillan, 2000. pp. 1780-1786.

Ariew, Roger and Peter Barker, "Introduction." Revolution and Continuity: Essays in the History and Philosophy of Early Modern Science. Eds. Roger Ariew and Peter Barker. Washington: Catholic University of America, 1991. pp. 1-19.

Auguste Comte and Positivism: The Essential Writings. Ed. Gertrud Lenzer. New York: Harper and Row, 1975.

de Beer, E.S., ed. The Correspondence of John Locke. Vol. IV. Oxford: Clarendon, 1976.

Ben-Chaim, Michael. "Doctrine and Use: Newton's 'Gift of Preaching.'" History of Science 36 (1998): pp. 269-298.

Berlin, Isaiah. Karl Marx: His Life and Environment. 3rd ed. New York: Time, 1963.

Butterfield, H. The Origins of Modern Science, 1300-1800. Rev. ed. London: Bell, 1957.

Cantor, Geoffrey N. "A Critique of Shapin's Social Interpretation of the Edinburgh Phrenology Debate." Annals of Science 32 (1975): pp. 245-256.

----. "The Edinburgh Phrenology Debate: 1803-1828." Annals of Science 32 (1975): pp. 195-218.

Clark, G.N. Science and Social Welfare in the Age of Newton. Oxford: Oxford University, 1970.

Cohen, H. Floris. The Scientific Revolution: A Historiographical Inquiry. Chicago: University of Chicago, 1994.

Cohen, I.B, ed. Newton's Papers and Letters on Natural Philosophy, 2nd ed. Cambridge: Cambridge University, 1958.

----. Revolution in Science. Cambridge: Harvard University, 1985.

Coles, Peter. Cosmology: A Very Short Introduction. Oxford: Oxford University, 2001.

Cunningham, Andrew. "Getting the Game Right: Some Plain Words on the Identity and Invention of Science." Studies in the History and Philosophy of Science 19 (1988): pp. 365-389.

Duhem, Pierre. To Save the Phenomenon: An Essay on the Idea of Physical Theory from Plato to Galileo. Trans. Edmund Doland and Chaninah Maschler. Chicago: University of Chicago, 1969.

-----. Le Système du Monde: Histoire des Doctrines Cosmologiques de Platon à Copernic. 10 vols. Paris: Hermann, 1988.

The Encyclopedia of Philosophy. Ed. Paul Edwards. New York: Macmillan, 1967.

Estling, Ralph. "Is Science Concerned with Truth?" Skeptical Inquirer 22, 4 (July/August 1998): pp. 55-56.

Field, J.V. and Frank A.J.L. James. "Introduction." Renaissance and Revolution: Humanists, Scholars, Craftsmen and Natural Philosophers in Early Modern Europe. Eds. Field, J.V. and Frank A.J.L. James. Cambridge: Cambridge University, 1993.

Frayn, Michael. Copenhagen. New York: Anchor, 1998.

Friedman, Michael. "On the Sociology of Scientific Knowledge and its Philosophical Agenda." Studies in History and Philosophy of Science 29 (1998): pp. 239-271.

Gardiner, Samuel R. History of the Commonwealth and Protectorate, 1649-1656. Vols. 1-4. Gloucestershire: Windrush, 1989. (Originally published 1903)

----. History of the Great Civil War, 1642-1649. Vols. 1-4. London: Longmans, Green and Co., 1893.

Gibson, James. Locke's Theory of Knowledge and its Historical Relations. Cambridge: Cambridge University, 1931.

Gillispie, Charles Coulston, ed. Dictionary of Scientific Biography. 16 vols. New York: Charles Scriber's Sons, 1970.

Golinski, Jan. Making Natural Knowledge: Constructivism and the History of Science. Cambridge: Cambridge University, 1998.

Grant, Edward. Much Ado about Nothing: Theories of Space and Vacuum from the Middle Ages to the Scientific Revolution. Cambridge: Cambridge University, 1981.

Greenberg, Daniel S. The Politics of Pure Science. New Edition. Chicago: University of Chicago, 1999. (Originally published in 1967)

Gutting, Gary. "French Philosophy of Science." Routledge Encyclopedia of Philosophy. Vol.4. Ed. Edward Craig. New York: Routledge, 1998. pp. 779-786.

Hacking, Ian. The Social Construction of What? Cambridge: Harvard University, 1999.

Hall, A.R. Philosophers at War: The Quarrel between Newton and Leibniz. Cambridge: Cambridge University, 1980.

-----. The Revolution in Science, 1500-1800. London: Longman, 1983. (Based on his The Scientific Revolution, 1954)

Henry, John. "Calling for a Ceasefire in the Science Wars." Science 395, 6702 (8 October 1998): pp. 557-558.

Hessen, Boris. The Social and Economic Roots of Newton's 'Principia'. New York:

Howard Fertig, 1971. (Originally published in Science at the Cross Roads, 1931).

Hexter, J. H. Reappraisals in History: New Views on History and Society in Early Modern Europe. New York: Harper and Row, 1961.

Hill, Christopher. The Century of Revolution 1603-1714. London: Nelson, 1961.

-----. Change and Continuity in Seventeenth-Century England. Rev. ed. London: Yale University, 1991.

-----. Intellectual Origins of the English Revolution. Oxford: Clarendon, 1965.

-----. The World Turned Upside Down: Radical Ideas During the English Revolution. New York: Viking, 1972.

Hunter, Michael. "The Conscience of Robert Boyle: Functionalism, 'Dysfunctionalism' and the Task of Historical Understanding." Renaissance and Revolution: Humanists, Scholars, Craftsmen, and Natural Philosophers in Early Modern Europe. Eds. J.V. Field and Frank A.J.L. James. Cambridge: Cambridge University, 1993. pp. 147-159.

----. Science and Society in Restoration England. Aldershot: Gregg Revivals, 1992.

Hutton, Ronald. The British Republic, 1649-1660. 2nd ed. New York: St. Martin's, 2000.

Iliffe, Rob. "Isaac Newton: Lucatello Professor of Mathematics." Science Incarnate: Historical Embodiments of Natural Knowledge. Eds. Christopher Lawrence and Steven Shapin. Chicago: University of Chicago, 1998. pp. 121-155.

Jacob, J. R."The Ideological Origins of Robert Boyle's Natural Philosophy." Journal of European Studies 2 (1972): pp. 1-21.

Jesseph, Douglas M. Squaring the Circle: The War between Hobbes and Wallis. Chicago: University of Chicago, 1999.

Koyré, Alexandre. Galileo Studies. Trans. John Mepham. New Jersey: Humanities, 1978.

-----. Newtonian Studies. Cambridge: Harvard University, 1965.

Kuhn, Thomas S. The Essential Tension: Selected Studies in Scientific Tradition and Change. Chicago: University of Chicago, 1977.

-----. The Structure of Scientific Revolutions. 3rd ed. Chicago: University of Chicago, 1996.

Lauden, Larry. "The Pseudo-Science of Science?" Philosophy of the Social Sciences 11 (1981): pp. 173-198.

Lem, Stanislaw. Solaris. Trans. Joanna Kilmartin and Steve Cox. New York: Harcourt Brace, 1961.

Lindberg, David C. "Conceptions of the Scientific Revolution from Bacon to Butterfield: A Preliminary Sketch." Reappraisals of the Scientific Revolution. Eds. David C. Lindberg and Robert S. Westfall. Cambridge: Cambridge University, 1990. pp. 1-26.

Lindberg, David C. and Robert S. Westman. "Introduction." Reappraisals of the Scientific Revolution. Eds. David C. Lindberg and Robert S. Westman. Cambridge: Cambridge University, 1990. pp. xvii-xxvii.

Locke, John. An Essay Concerning Human Understanding. Ed. Peter H. Nidditch. Oxford: Clarendon, 1975.

-----. Two Treatises of Government. Ed. Peter Laslett. Student ed. Cambridge: Cambridge University, 1988.

Malcolm, Noel. "Hobbes and the Royal Society." Perspectives on Thomas Hobbes. Eds. G.A.J. Rogers and Alan Ryan. Oxford: Clarendon, 1988. pp. 43-66.

----. "A Summary Biography of Hobbes." Cambridge Companion to Hobbes. Ed. Tom Sorell. Cambridge: Cambridge University, 1996. pp. 13-44.

Mannheim, Karl. Ideology and Utopia: An Introduction to the Sociology of Knowledge. Trans. Louis Wirth and Edward Shils. New York: Harcourt, Brace, 1936.

Manuel, Frank. A Portrait of Isaac Newton. Cambridge: Harvard University, 1968.

Marshall, John. John Locke: Resistance, Religion and Responsibility. Cambridge: Cambridge University, 1994.

Merton, Robert K. Science, Technology and Society in Seventeenth-Century England. New York: Harper and Row, 1970.

Mintz, Samuel L. The Hunting of Leviathan: Seventeenth-Century Reactions to the

Materialism and Moral Philosophy of Thomas Hobbes. Cambridge: Cambridge University, 1969 (Originally published in 1962).

Neugebauer, O. Astronomy and History: Selected Essays. New York: Springer-Verlag, 1983.

-----. The Exact Sciences in Antiquity. 2nd ed. New York: Harper, 1962.

Newton, Isaac. Principia. Trans. Andrew Motte and Florian Cajori. Berkeley: University of California, 1946.

-----. Opticks. Trans. Silvanus P. Thompson. Toronto: Encyclopedia Britannica, 1952.

-----. Correspondence. 7 vols. Ed. H.W. Turnbull. Cambridge: Cambridge University, 1959-1977.

Nietzsche, Friedrich. The Gay Science. Trans. Walter Kaufmann. New York: Vintage, 1974 (Originally published in German in 1887).

Okasha, Samir. Philosophy of Science: A Very Short Introduction. Oxford: Oxford University, 2002.

-----. "Politics and the English Language." Essays. London: Penguin, 2000.

Osler, Margaret J. "Mixing Metaphors: Science and Religion or Natural Philosophy and Theology in Early Modern Europe." History of Science 36, 1(1998): pp. 91-113.

The Oxford Dictionary of the Christian Church. Ed. F.L. Cross. Oxford: Oxford University, 1997.

Pierson, Stuart. "Corpore Cadente." Perspectives on Science 1 (1993): pp. 627-658.

-----. "Johnston's Smallwood," Newfoundland Studies 14, 2 (1998): pp. 282-300.

-----. "Two Mathematics, Two Gods: Newton and the Second Law." Perspectives on Science 2 (1994): pp. 231-253.

Popper, Karl R. The Logic of Scientific Discovery. Rev. ed. London: Hutchinson, 1968.

Rogers, G.A.J. "Locke's *Essay* and Newton's *Principia*." Journal of the History of Ideas 39, 2(1978), pp. 217-232.

The Routledge Encyclopaedia of Philosophy. Ed. Edward Craig. London: Routledge, 1998.

Sagan, Carl. Contact. New York: Pocket, 1985.

Sarton, George. Sarton on the History of Science. Ed. D. Stimson. Cambridge: Harvard University, 1962.

Smith, David L. A History of the British Isles: The Double Crown. Oxford: Blackwell, 1998.

Sorell, Tom. "Thomas Hobbes." Routledge Encyclopedia of Philosophy. Vol. 4. Ed. Edward Craig. New York: Routledge, 1998. pp. 459-476.

Thompson, Graham R. and Jonathan Turk. Earth Science and the Environment. Philadelphia: Saunders College, 1993.

Tiles, Mary. "Bachelard, Gaston." Routledge Encyclopedia of Philosophy. Vol. 3. Ed. Edward Craig. New York: Routledge, 1988. pp. 620-624

Weber, Max. The Protestant Ethic and the Spirit of Capitalism. Trans. Talcott Parsons. New York: Charles Scribner's Sons, 1958. (Originally published 1904-5).

Weinberg, Steven. "The Revolution that Didn't Happen." The New York Review of Books XLV, 15 (8 October 1998): pp. 48-52.

Westfall, Richard S. The Construction of Modern Science: Mechanisms and Mechanics. Cambridge: Cambridge University, 1977.

----. Never at Rest: A Biography of Isaac Newton. Cambridge: Cambridge University, 1980.

Whewell, W. History of the Inductive Sciences, from the Earliest to the Present Time. 3rd. ed. 3 vols. London: Parker, 1857.

Wittgenstein, Ludwig. Philosophical Investigations. 3rd ed. Trans. G.E.M. Anscombe. Englewood Cliffs: Prentice Hall, 1958.

Young, Robert M. Darwin's Metaphor: Nature's Place in Victorian Culture Cambridge: Cambridge University, 1985.