4. The Lineage and Limits of Science as "Community" in STS

In the foregoing chapter I have reviewed the major theses offered by Shapin and Schaffer in support of their central argument that modern scientific knowledge is the product of social construction within scientific communities. Where has this review left us? In the beginning we had a powerfully crafted case study, and we now face a tale full of messy if fascinating particulars. Now spilling around us is all the evidence that doesn't have a place in the "constructed" account: the (truly) plain style of modern scientific reportage; the co-emergence of Italian and French and other European scientific societies, the important role of the Paris Académie; the Republic of Letters; the emergence of scholarly journals; the emergence of symbolic algebra, and with it the tide of metrology, precision, number -- and communication in the absence of community; the elitist (and pre-romantic) individualism of Boyle; Hobbes's strong communitarian feeling; Hobbes's resolute materialism; Boyle's Christianity and belief in witches; the Royal Society's legitimate fear of being publicly associated with Hobbes; and more.

But does it matter if the "origins" aspect of the story of Hobbes and Boyle falters under scrutiny? Shapin and Schaffer's thesis that science is constructed in communities is actually quite independent of their historical arguments. This suggests that the overall form of Shapin and Schaffer's argument is something like the logical fallacy of assuming the consequent: that is, if community is *assumed* to be the defining feature of modern science, then any other historical premise (true or false) can lead us to it. This is a somewhat less generous way of restating the "defense" of Shapin and Schaffer given by Peter Dear:

> *Leviathan and the Air-Pump* was thus something of a morality play. It mattered little whether the portraits of Hobbes or, especially, Boyle were historically accurate, and it mattered even less whether their utterances on the meaning of experimental philosophy were representative of larger social groupings. .... Even in the case of Boyle, little would have been lost to the book's main purpose had the Royal Society never existed and Boyle written for a readership of one. The lessons about the implicit social meanings of experimental science would have held simply on the basis of the presentation of a Boylean epistemic utopia and the grounds of its critique by Hobbes (Dear, 1995, 451).

In this chapter I will therefore shift my focus from history to sociology, from the central *narrative* to the central *thesis* of *L&AP*. That thesis, for which the Hobbes-Boyle dispute was intended to serve as an origins story, is that the epistemological authority of science is dependent on, and

derived from the collective judgment and the agreement of what is referred to as "scientific community."[78]

The vagueness surrounding the meaning of "community" in *L&AP* is exacerbated by the fact that "community" has numerous referents both in sociological and historical literature. Individual scholars often invoke the word but rarely say exactly what *they* mean by it.[79] The use of the term "community" in the sociology of science can be seen as an instance of what Pickering has called the "almost principled refusal to interrogate key sociological concepts." Pickering's target was the concept "interest" -- but "community" will do as well. In Pickering's words, "Perhaps in SSK it is the social that has become sacred."[80]

How does Shapin and Schaffer's usage of the idea of "community" fit with the idea as it appears elsewhere in the sociology of science? To begin with, since the 1930's, the idea of scientific community has become deeply entrenched in the sociology, history, and philosophy of science:

> During the 1960s and 70s the view prevailed among metascientists that scientific knowledge is produced by communities of specialists....on various levels of generality, ranging from all the scientists active in a country....down to small groups of specialists. In the works of leading contemporary philosophers of science (Kuhn, Popper, Toulmin, Lakatos, Hacking et al.), as in

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[78] See Struan Jacobs and Brian Mooney (1997) for a critique of Kuhn's concept of community that has broad similarities to the critique of Shapin and Schaffer below. The reader will also note that, particularly in this chapter, I refer frequently to Jan Golinski's excellent study of constructivism and the history of science, *Making Natural Knowledge* (1998). I do not know a better book than his on the subject: it is, in my opinion, a true benchmark against which related scholarship may be measured.

[79] Of course there are many sociological terms with equally vague referents: we are all familiar with "forces," "interests," "sentiments," and "values." Such terms are not explanatory, of course, but, like "community," they represent sociological black boxes. Keith Stamm (1985), makes the point that, "in everyday language, 'community' is used with reference to a wide variety of things, events, and relationships--from villages, towns, neighborhoods, and cities to occupational and ethnic categories and the sharing of attitudes and lifestyles. Similar differences exist among researchers in their usage. One observer traced 90 definitions of community in the scientific literature, many of which seemed to have little in common.... The richness of meaning that gives the idea [of community] such compass and flexibility in everyday use presents obstacles for its scientific usage. In the scientific context richness of meaning can become surplus of meaning; a concept so broad, so encompassing that everyone seems to use it differently...." (Stamm, 1985, 10-13).

[80] Pickering 1994, fn. 35, 152. Ironically Pickering's footnote includes a quotation from Steven Shapin on just this point, that is, urging that "one must....welcome any pressure that urges analysts further to refine, define, justify and reflect upon their explanatory resources...." (this citation is from Shapin's 1988 review of Latour's *Science in Action*, at 549). Struan Jacobs (1987) emphasizes the rhetorical use of the word "community," and suggests it is a taxonomic category of the scientist rather than a self-identification of the participants. I will return to Jacobs and Mooney's (1997) analysis of Kuhn's sense of scientific community a little further on.

> many historical studies, the existence of scientific communities was assumed without question or argument (Jacobs, 1987, 266).

The idea of scientific community may have had its genesis in the work of Ludwig Fleck,[81] but in the Anglo-American world it has come to science studies primarily by way of Merton and Kuhn. Since then it has been amended and reinforced by Hagstrom in the 60's, critiqued and amended by anthropologists and microsociologists of science in the 70's and 80's (Collins, Latour, Traweek), and by philosophers of power and politics running from Foucault in the 70's and 80's to Longino and Rouse in the 80's and 90's.

The common thread running through all these very different accounts of science is that scientific knowledge "is" social knowledge: knowledge about which people agree, or at least, knowledge that is "communal" in some sense: scientific knowledge is made by "scientific communities." This does not mean there is no disagreement on how this works: one divisive issue is whether scientific communities exist because of a need for communication (Latour, Foucault, Habermas, Goldgar, Mulkay, Gilbert), or rather because they offer the potential for establishing an epistemic consensus (Merton, Kuhn). And some have suggested that it is incorrect to leave instruments, animals, and other features of the environment out of our understanding of science: according to scholars such as Law and Latour and Woolgar, we should think not in terms of scientific communities (which assume that human agency is central), but in terms of networks that embrace material agency more symmetrically.

One of the central problems in referring to scientific community is deciding on the scope of this elusive entity. In a 1974 methodological essay,[82] Shapin and Thackray addressed just this problem; they proposed a concentric model of community, operationally defined by the communicative strategies and positions of individuals. Shapin and Thackray's broad definition of "scientific community" -- at least in England -- embraced not only a dedicated and inventive association of experts, but also the interested and curious public, and amateurs.

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[81] A point nicely argued by Struan Jacobs, 1987, who traces the influence on Fleck of American pragmatism as well as work of Viennese philosophers of science. Jacobs points out the strong correlation between Fleck's ideas of thought collectives thought styles, and Kuhn's on communities and paradigms. See also Golinski, 1998, at 33, for a discussion of how Fleck handled the problem of the social origins of the epistemic authority of "facts."

[82] Steven Shapin and Arnold Thackray, 1974.

In *L&AP*, this broad notion of scientific community is replaced by a more formal, and restrictive, notion of a trusted association of practitioners striving for collective knowledge. Thereafter, in *Social History of Truth*, Shapin emphasized even more strongly the close association between the pursuit of knowledge and the ability of trusted communities to arrive at agreed-upon knowledge. A reviewer contrasted this trust-based community with earlier sociological characterizations of science: Shapin, he said, left unaddressed a "fascinating" idea -- that "communities" of scientists might operate in quite different ways -- some characterized by the need common to all distributed social interactions -- namely to maintain the "conversation" and hence engage in social practices of gentility and trust; but others (less localized? larger?) characterized by the "ungenteel skepticism of the kind which Merton thought was essential to scientific practice" (Bogen, 1995).

4.1 Merton's Normative Communities

Because of the contrast between Merton's and Shapin and Schaffer's understanding of community, and also because of Merton's enormous influence in science studies, it is useful to begin our look at the idea of scientific community with Robert Merton. Merton proposed a functionalist model of scientific community that emphasized the strong skeptical and normative function of the community in relation to its members. In this view,

> ...social integration in science is the result of consensus about strategic norms or standards (disinterestedness, universalism, organized scepticism, etc.). [Merton and his followers'] research consequently focused on rewards (citation, eponymy, prizes) to scientists for conforming to those norms, and on the stratification or unequal distribution of rewards and resources. In retrospect it is surprising how vague Mertonian sociology has been regarding the social unit(s) of science. Merton himself spoke of the 'social institution of science' , the singular form of which suggests that science exists as one vast undifferentiated social system (Jacobs, 1987, 267).

Merton's model of scientific community therefore emphasized the *rational* character of scientific behavior. On the other hand, as Shapin has pointed out, Merton's first (1938) accounts of the sociology of early modern science were concerned to "display the importance of 'nonrational' and 'nonlogical' considerations in social action. Sentiments were the nonrational and unconscious wellsprings of the social actions involved in sanctioning and pursuing science in seventeenth-century England" (Shapin, 1988, 598-599, 601). Whatever Merton meant by "sentiments" in his earlier work,[83] the Merton best known to later readers is the scholar whose work is the *locus classicus* of the idea of the scientific community as one that follows norms of universalism,

[83] Shapin suggests that Merton was using the concept in Pareto's also ill-defined sense.

communism, and disinterestedness. Some critics of Merton's work have asked whether these "norms" are really definitive of, or essential to, science;[84] others have criticized the teleological implications of Merton's account, and Merton's "fencing off" of science as if it were an autonomous social activity whose product (knowledge) was unaffected in substance by external influences.

4.2 Kuhn's Consensual Communities

Thomas Kuhn's classic work on the structure of scientific revolutions, originally published in 1962, represented a fundamental parting of the ways with Merton's concept of scientific community as it was understood in the 1950's. Where for Merton scientists were bound together in a community defined by shared values and producing knowledge for all time, Kuhn's scientific communities were forged by systems of traditional authority and pedagogy. Kuhn accounted for scientific change despite this institutional demand for conformity, by proposing cycles marked by periods of disruption. According to Kuhn, scientific communities conducted "normal" science in the context of a set of unquestioned views and assumptions that helped define allowable research problems, methods, and understandings ("paradigms"); but these fundamentally conservative social and intellectual formations were broken periodically by "extraordinary" science, during which fringe members or outsiders managed to effect a revolution or rupture, gradually attracting followers until a new consensus emerged and an epoch of "normal" science could settle in again.

For Kuhn, therefore, scientific communities were defined by the paradigms they held. True, these communities operated during periods of normal science somewhat like Merton's: as if they were autonomous and homogeneous associations that could both preserve and promulgate a current body of knowledge and technique. But for Kuhn, the scope and variations among scientific communities were shaped not by universal values and sociological categories but by the contingencies of history, discipline, and specialty.

Kuhn's theories on the dynamic of ruptures and intellectual revolution in science had a tremendous influence, extending far beyond the confines of science studies. Within science

[84] See for example Leslie Sklair, who writes, "[these norms] have a limited though important connection with science as such, but have a very great deal more to do with science as it is carried out in advanced capitalist society...The orthodoxy that I have been criticizing would have us believe that these norms are indispensable to the survival of science as such..." "The Political Sociology of Science: A Critique of Current Orthodoxies," 43-59 in: Halmos, Paul, ed., *The Sociology of Science*, 1972.

studies, Kuhn's influence was felt primarily in two ways: first, for those who accepted the Mertonian view, Kuhn's model of scientific communities seemed a recipe for epistemic relativism. At the very least, it was a strong brand of what has come to be called social constructivism. Kuhn proposed that communities with differing paradigms might be unable even to comprehend each other's research questions, methods, or findings: this was the problem of "incommensurability." But if scientific communities were not guided by reason and method, and if scientific knowledge was not constantly being improved from one generation to the next, how could science establish progressive, cumulative knowledge? The issue of scientific "progress and its problems" (to use Laudan's phrase) obsessed Kuhn's contemporaries, who struggled to resolve the phenomena Kuhn himself had never fully explained -- that is, how exactly paradigm shifts take place, and the dynamics of conflicts and controversies in science.

Kuhn's work therefore had the effect of inspiring the important genre of controversy studies in science. Among the scholars associated with this genre were Harry Collins (1985, *Changing order*); Martin Rudwick (1985, *The Great Devonian Controversy*), and James A. Secord (1986, *Controversy in Victorian Geology*), as well as Pickering (*Constructing Quarks*).[85] These scholars looked at scientific controversies across many different scales, from the very large to the very short-term. Quite obviously, the turn to controversy studies that was inspired at least in part by Kuhn, was also a major influence for Shapin and Schaffer's *L&AP*.

Since the boundaries around scientific communities were historical and contingent rather than universal, Kuhn was forced, eventually, to clarify whether a paradigm (and its corresponding community) was a large- or small-scale thing.[86] It seemed to many of Kuhn's readers that what he was describing (scientific communities and paradigms) were very large scale: large communities, broad disciplinary areas, long timeframes. But for many historians and other scholars, the dynamic Kuhn identified seemed a fairly inaccurate description of events on that scale. Entering the debate himself, Kuhn addressed this question in the 1970 second edition of the *Structure of Scientific Revolutions*; here he stated emphatically that paradigms and their communities were *not*

[85] I do not include Dorothy Nelkin (*Controversy: Politics of Technical Decisions*, 1978, 1984, 1991) in this listing, since her focus is not on controversy as an aspect of paradigm change in science, but on controversy as the focus for decision-making (risk analysis, social values) in cases that involve scientific research or technology.

[86] See for instance Bloor 1978, who developed a typology of paradigm communities in his essay, "Polyhedra and the Abominations of Leviticus," in *British Journal for the History of Science*, v. 11, 243-272.

large-scale phenomena, but relatively small-scale entities, which might involve no more than a few dozen specialists or a few small working groups, and which could be expected to undergo frequent changes.

Thus, despite the influential macrohistorical ambitions of Kuhn's work, paradoxically another important influence of Kuhn's was to encourage the development toward microhistories of science:

> Since Kuhn, "microhistories" of science have become the norm, in which a single controversy, institution, discipline, or research program, is scrutinized over a limited period. Although partly reflective of trends in other fields of history, ... this narrowing of focus is also the result of concentration on the themes of constructivism found significant in Kuhn. It was, however, prefigured by Kuhn's own failure to follow through his gestures toward a macronarrative of the history of science....such expectations were frustrated: Kuhn never provided such a narrative (Golinski, 1998, 26).

Once again it is possible to see that Shapin and Schaffer's *L&AP* strongly reflects Kuhn's influence. Their focus on understanding a critical paradigm shift, negotiated in and around a local community struggling to define its own boundaries, is almost pure Kuhn, and in Golinski's judgment *L&AP* was "a brilliant realization" of some of Kuhn's themes. But, Golinski continues, their approach "lacks a diachronic perspective ... There is little sense of development or resolution, and certainly no account of how the experimental "form of life" was sustained chronologically. *The task of synthesizing the results of this kind of local study into new large-scale historical narratives remains uncompleted*" (Golinski, 1998, 27). Shapin and Schaffer may have hoped to fill that gap: by weaving Merton's origins story with Kuhn's paradigm shift, they attempted the ultimate macrohistory: an ambitious and complex reconciliation of Mertonian sociology with Kuhnian constructivism. But Shapin and Schaffer's work also reflected a third approach to scientific community that has had a growing influence in science studies.

4.3 Community as Ethnos: The Anthropological Turn

Among the most significant new trends in science studies since Kuhn, has been what could be termed the "anthropological turn". The application of anthropological methods and concepts to the study of science in the last several decades has resulted in the unsettling confluence of mainstream social science with critical and postmodern theory.

Warren Hagstrom's 1965 field research still falls within a mainstream sociological tradition, but it signals a turn toward anthropological methods and concepts. Hagstrom's functionalist perspective

owed more to Merton than to Kuhn; but his argument that, like other normative communities the scientific community is maintained by gift-based exchange, suggested an anthropological version of the Merton macronarrative of science, while opening up the study of science to decidedly non-Mertonian analyses.

An important consequence of applying anthropological methods and concepts to scientific community was the localization of scientific knowledge: the limitation of this tendency however is that it has made introducing a macronarrative dimension almost impossible: the "snapshot" quality of field work often presumes an ongoing stability in the adjustment of the culture to its environment, and implies that the patterns that can be found in that adjustment are common across cultures and across time.[87] It is difficult to make this assumption work in the context of significant changes in the content of scientific knowledge and its organization.

In 1983, the noted anthropologist Clifford Geertz suggested that "most effective academic communities are not that much larger than most peasant villages and just about as ingrown".[88] This observation was reflected in a new interest, throughout the late 70's and 80's, in producing "ethnographies" of laboratories and research sites. Among these were Latour and Woolgar's pioneering 1979 study of the Scripps Institute laboratories, and Sharon Traweek's widely-read 1988 ethnography of high energy physicists at the Stanford Linear Accelerator Center (SLAC). These works, which treat laboratories as the focus of "tribal" scientific communities, have also introduced other important themes. They have drawn conscientious attention to the interactions between the human actors and their *material* environment and instrumentation; they have also emphasized the various processes by which the system under study achieves both internal and external differentiation, along with a degree of coherence and stability. The underlying message of these and similar studies, is that the anthropological method is not only *possible* but actually *dictated* by the material: "the local character of [Traweek's] study is not simply a reflection of the limitations of fieldwork method but represents the inherently restricted range -- in time and space -- of human activities" (Golinski, 1998, 167).

Along with this emphasis on science as a local phenomenon, the anthropological genre in science studies has encouraged analysis of interactions within scientific communities in terms of

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[87] The emphasis on place (locale) and the abandonment of a temporal dimension by ethnographic approaches appeals to postmodern sensibilities, and perhaps mistakenly causes us to associate (for example) the work of ethnographer Sharon Traweek with the postmodern scholarship of Donna Haraway.

[88] Clifford Geertz, 1983, 157.

networks; it has also focused attention on material elements of the scientific "culture". This has resulted in many new studies on scientific instrumentation, as well as theorizing about the "agency" that can be attributed to material elements in the scientific network. The former idea is a signature of Latour's social studies of science, but it has also been developed by Harry Collins, who has emphasized informal connections among researchers who are situated in "networks of trust" (Collins 1985) and by Mulkay, Woolgar, and Gilbert (1975, 1984). Their theory of "research networks" identified communities with no inherent or natural boundaries, with transient memberships, and frequent migration in and out. Participants were bound, not by common training but by "concentrations of ties of interest arising from participants' perceptions of the relevance for their work of the research of others" (Jacobs, 1997, 476).

Like Latour and Woolgar, John Law has emphasized the symmetry of material and human agency in technological and scientific networks. Other recent work focused on instrumentation has included the 1995 volume edited by Norton Wise on the "values of precision," with much of that precision provided by instruments; and Adele Clark and Joan Fujimura's 1992 monograph *(The Right Tools for the Job*).

In a related development, starting in the late 1970's, Karin Knorr-Cetina began to propose arguments against functionalist definitions of "community" in favor of what she called "transcientific [or transepistemic] fields" or networks. The agents within such open fields, she argued, belonged to no particular logical class:

> Knorr-Cetina was denying autonomy, insulation, natural bounding, perimeters, and internal sources of integration....[She] saw these fields as a 'locus of struggle to establish and maintain 'resource-relationships,' which she explained as 'relations to which one resorts or on which one depends for supplies or support. Transepistemic arenas of symbolic action display themselves as the locus in which the establishment, definition, renewal, or expansion of resource-relationships is effectively negotiated" (Jacobs and Mooney, 1997, 477).

Schaffer's work, more than Shapin's, has reflected the material and technological dimension of science, and of scientific instruments in particular. Shapin, on the other hand, has (both in earlier and later work) emphasized the role of social networks. This theme is expanded greatly in Shapin's 1994 work, where he situates:

> ....networks of trust in relation to scientific knowledge in the seventeenth century....Like Collins, he claims that the perpetuation and extension of scientific knowledge is dependent upon practitioners' trust in one another's factual statements. .... [I]n the period when the scientific community was in the process of formation, however, these relationships cannot be supposed to have existed ready-made and isolated from society at large. Shapin therefore argues that relations

> of trust and credit had to be built up among natural philosophers in the seventeenth century, and that this was done by exploiting common assumptions about gentlemanly decorum and credibility..." (Golinski, 1998, 30)

One way of interpreting *L&AP* is as a confluence of these two approaches, one focused on the epistemological authority of the experimental tool;[89] and the other on the social strategies that helped forge the social network of the scientific community. But as we will see below, once social strategies for creating and sustaining a network come into play, it becomes difficult to avoid addressing specific strategies, including rhetoric and politics, for how those networks are managed. Whether or not what we refer to as "communities" are more accurately thought of as "networks," the study of rhetorical and communicative strategies in science represents an important focus in the analysis of the activities performed by scientists.[90] Similarly, the location of authority within the network -- where power is located, by what methods it is exercised -- is a central issue in contemporary science studies whether working from the model of science as "community" or as "network". *L&AP* thus reflects several related, widespread tendencies in science studies, both in suggesting that the "problem of knowledge" is (always) political; and in arguing that communicative and rhetorical strategies were central to the formation and maintenance of Boyle's fledgling scientific community.

4.4 Political Community: Knowledge as Power

In the earlier part of the twentieth century, many scholars saw the relationship between politics and scientific knowledge as one of mutual support and influence. This understanding is reflected to this day in both naive and sophisticated writing about how science allows human beings to "control" their environment and each other.[91] It is common to read about the positive or negative impacts that new scientific research might have on society or the environment, and one finds many expressions of the idea that scientists are socially and morally responsible for the political

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[89] The historical emergence of attributing scientific authority to instrumentation, Golinski claims, represented a "second scientific revolution" (Golinski, 1998, 137).

[90] Under the rubric of "inscriptions," Latour and Woolgar place statements and written communications into the realm of material agency in the technoscientific network; in fact, they claim that the authority acquired by these "communications" increases as they become distanced from the local, the contingent -- in short, from the "author".

[91] By "sophisticated" talk I include what Joseph Rouse calls the "new empiricism" of writers like Hacking, Cartwright, Hesse, and Laudan, which Rouse says "internalizes the connection between the development of science and the improvement of technical capability. The achievement of technical control over natural processes is seen not as a by-product of theoretical development but as its *raison d'etre*" (Rouse, 1987, 11).

effects and uses of their research. Around the middle of the century, Karl Popper made a case for the importance of political values and the political environment for the conduct of science. Contemporary writers in science studies tend to look at the relationship of science and politics as mutually influential: one thinks of Langdon Winner, for example, or Helen Longino, both of whom make eloquent philosophical arguments for the interpenetration of the activities of science with the social and moral and environmental context of scientific actors. But even these more contemporary approaches have assumed that science and politics are distinct, even if deeply interrelated and mutually influencing, human endeavors.[92] It was only with the work of Michel Foucault, in particular Foucault's *Discipline and Punish* (1977), that the relationship between power and scientific knowledge began to be analyzed very differently.

Foucault suggested that scientific knowledge is itself a form of political and social control, and a manifestation of a larger development that defines modernity: the development on the one hand of a culture of "discipline," defined by the compartmentalization, measurement, and detailed ("capillary") management of social processes; and on the other by the invention of the "normal" -- a fictional abstraction against which the world's "real" deviance is specified. The internalization of the "normal" as a self-measure creates a microlevel of political control, acting within each agent: it is thus a hidden but all the more powerful kind of control than mere external enforcement by an outside agent.

Thus Foucault proposes a surprising continuity between the notion of discipline in its punitive, and educative, senses. Even the seemingly elevated notion of "discipline" in the sense of a set of scientific practices, according to Foucault's analysis, is embedded in larger "formations of power." These formations consist of a pervasive practice of making "separations" and categorizations. These contribute to the larger disciplinary process that characterizes modern life:

> Discipline formation requires the consolidation of a community that shares a particular model of practice, which in turn implies modes of regulating behavior....The ambiguity of the word "discipline" is crucial here: It refers both to a form of instruction to which one submits and to a means of controlling behavior.... (Golinski, 1998, 69).

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[92] Richard Rorty has pointed out that this distinction is not merely linguistic but deeply philosophical, with profound results: "We are the heirs of three hundred years of rhetoric about the importance of distinguishing sharply between science and religion, science and politics, science and art, science and philosophy, and so on. This rhetoric has formed the culture of Europe. It made us what we are today" (Rorty 1979, 330-331).

Disciplinarity is thus a mode of thought that both promotes, and undermines, the fiction of the "sovereignty of the subject." In a kind of pervasive and universal extension of Kuhn's notion of incommensurability, Foucault argued that disciplinarity limits the range of discourse such that "outside the range of a discipline [there are] statements that are not so much false as categorically unthinkable..." (Golinski, 1998, 70).[93]

Other influential work on the relationship between science and politics has also emphasized, if less radically than Foucault, the unity of political and scientific practice.[94] Joseph Rouse has argued that "scientific skills, practices, and techniques [are] the place where this kind of power is developed and operates....To understand scientific knowledge, we need .... a positive account of the skills and practical know-how that construct and stabilize phenomena and that enable scientists to intervene and to manipulate them in informative ways. Only in such skills and practices do knowledge and power come together" (Rouse, 1987, 21). Ultimately, Rouse argues, the distinction between scientific knowledge and politics is not tenable: "I think it is no longer possible, ... to sustain a political distinction between the exercise of power over human bodies and the development and use of capacities to control and manipulate things" (Rouse, 1987, 244).

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[93] Golinski's discussion of this paradox or illusion of individual "creativity," is helpful: "Even originality can be read, through Foucault's eyes, as an outcome of discipline. But the historian of modern science can readily point to examples of individuals who have fashioned themselves outside existing disciplines, at the crossover points between disciplines, or in opposition to the prevailing currents within their discipline.... These individuals' fashioning of their identities cannot be explained as a result of hegemonic structures of the disciplinary power. Rather, some recognition of the capacity of individuals for autonomous self-expression seems to be required. Notwithstanding the powerful sway of disciplinarity over the personal formation of scientific practitioners, these cases remind us that individuals can construct their own professional identities by creatively manipulating the resources they find around them. Even in the age of disciplines, then, an understanding of scientists' identities may require reference to such notions as virtuosity and bricolage" (Golinski, 1998, 78).

[94] Rouse acknowledges Foucault's influence throughout his 1987 book; see, for instance, 185: "Scientific practices belong to what Foucault has called the realm of 'government': ... The political character of scientific interpretation can show itself at both the macro and micro levels. To begin with the former, consider the seventeenth-century developments....Everyone grants that this revolution fundamentally changed the understanding of the natural world. But it did not just substitute one position for another in response to a common issue; it changed what physics was *about*, what was at issue in understanding the natural world.....It introduced new 'ideals of natural order', stripped places of their physical significance....burst the closure of the world to reveal an infinite universe, and substituted the categories of force, mass, and instantaneous velocity for qualitative and teleological ones in the comprehension of matter.... These were not only intellectual changes. They were connected to transformations in political relations and institutions, the creation of new religious practices, and the emergence of new forms of economic behavior and social interaction." Also see much of chapter 7 in this same book.

In a variation on the principle of symmetry espoused by Latour, Woolgar, and Law (that is, regarding both human and non-human entities "symmetrically" as agents in scientific networks), Rouse argues that no real boundary exists between the "natural world" and the "social world":

> We do not need to ascribe interests, values, rights, or a preferred "natural" order to material things to recognize their integral engagement in the operation of power (Rouse, 1996, 191).[95]

Rouse's work stands here for a more thoroughly and consciously political approach to the study of science as a source of cognitive, social, and political authority. It is representative of what has been referred to as the

> ...late twentieth-century project of uncovering the hidden operations of power, questioning once seemingly fixed and 'essential' categories such as race, gender, and ethnicity, asserting the importance of interpretation, denying universal claims to truth, and rejecting simple, linear narratives like the one of inevitable scientific progress. Far from being singled out for special attention, science stands as one social and cultural construction ...among many... (Walters *et al.*, 1997, 8).

Questioning the authority and autonomy of the "scientific community," these analyses see science as part of a larger cultural landscape, where authority, if it exists at all, has an almost Kuhnian cast: it is authority that is at the same time local, particular (that is, internalized), and consensual: it is cultural authority, the authority of the "ethnos". These cultural-political studies of science focus a great deal of attention on the microboundaries that define particular scientific or epistemic groupings.

There is only way for the pendulum to swing from here, and swing it has: as David Hollinger writes, "No sooner do we ask, How wide the circle of the we? than we ought to ask, What identifies the we? and How deep the structure of power within it? and How is the authority to set its boundaries distributed?" Hollinger has proposed that we are entering a "postethnic" period in which microboundaries will no longer fascinate us as barriers to be overcome, but will become the vehicle for understanding their creative, analytical, and constructive functions (Hollinger, in Walters *et al.*, 1997, 26; 31, and fn. 64).[96]

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[95] The invocation of "symmetry" in this sense needs to be distinguished from the Strong Programme's symmetry principle that contrasts with Whiggish history of science. In this different sense, "symmetry" really refers to the neutral stance of the observer, who should strive to understand the events without reference to eventual outcomes or subsequent knowledge.

[96] Another influential contribution to the return-swing of the pendulum has been Peter Galison's concept of epistemic "trading zones," complete with "pidgins," in which transfers across the microboundaries within and between scientific communities are negotiated. I think however that Galison's approach represents an

In many respects Shapin and Schaffer's *L&AP* falls comfortably into this cultural-political understanding of the idea of scientific community. Their work strongly echoes Foucault's ideas[97] and as I have suggested earlier, *L&AP* seems to adopt Foucault's literary technology. Foucault's *Discipline and Punish* was structured around the vivid and detailed retelling of a fairly obscure historical event, with which Foucault associated the emergence of a "modern" understanding of knowledge. *L&AP* offers a less gruesome, but nevertheless "capillary" account of the somewhat obscure wrangling between Hobbes and Boyle over the air-pump trials. The central events in both *L&AP* and in Foucault's work are presented as a *catastrophe* -- the point at which the balance of medieval and modern was tipped in favor of the modern.

More than any other recent work in science studies, Shapin and Schaffer have argued that scientific *knowledge* and politics are inseparable, and the boundary erected (from Boyle to the present) between science and politics is socially "constructed":

> We have made three connections: we have attempted to show (1) that the solution to the problem of knowledge is political... (2) that the knowledge thus produced and authenticated becomes an element in political action in the wider polity....[and] (3) 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 institutions and other interest groups (*L&AP* , 342).

Shapin and Schaffer champion Hobbes because they believe he shared this analysis. But I will argue below (in section 4.6) that Hobbes differed from Foucault in at least one critical respect, one obscured by Shapin and Schaffer's adoption of a received interpretation of Hobbes's views on free will that is fundamentally mistaken (*L&AP*, 339). Where Foucault saw the exercise of "discipline" on the community as lodging, ultimately, within the mind itself, Hobbes did not: he insisted on the freedom of the individual to *believe* as they will, in defiance of authority or custom.[98] This point is actually well-developed in *L&AP*, but it is never reconciled with Shapin and Schaffer's characterization of Hobbes' s opposition to "free-will:"

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intellectual historian's borrowing from the anthropological turn, above, rather than a contribution to the cultural-political genre of science studies.

[97] A fact which makes the solitary and dismissive footnote concerning Foucault in *L&AP* somewhat surprising.

[98] This may seem to fly in the face of Hobbes's deserved reputation as one of the preeminent spokesmen for determinism. Samuel Mintz devotes a chapter to Hobbes's controversy with Bramhall and Cudworth over free will (Mintz, 1962, 110-133). The gist of Hobbes's argument is that a thoroughly consistent materialism can only be consistent with determinism, but that has no effect on the experience that we name "free will".

> [Hobbes's] strategy was one of behavioral control, not one of internal moral control. It was not that the control of belief was wrong; it was that such control was impractical and an inadequate surety for public order...Individual states of belief were in principle uncontrollable because they were in practice unreachable. I cannot know what you believe; you may be lying. I can force you to make a profession, but I cannot guarantee that this profession corresponds to your state of belief (*L&AP*, 105).

4.5 Discursive Community: "Totius in Verba"[99]

Regardless of whether a community is defined in terms of shared norms, shared beliefs, shared practices, shared culture, or the exercise of power, the challenge of identifying a "community" has frequently turned to the tools, artifacts, and activities of communication. Why is this? Perhaps the most important reason is that communication is (as Hobbes was pointing out) the only empirically "reachable" phenomenon that reflects the internal reality of social bonds.

A substantial scholarly literature exists that operationalizes scientific community in terms of such communicative phenomena as citation and co-citation, conference attendance, formal publication, and informal conversation. But the varieties of definition of community have meant widely varying approaches to studying its operationalization in terms of scientific communication.

This variety can be illustrated with a few examples. Latour handles the material artifacts of communication as nodes within his technoscientific network, and as "inscriptions" that help stabilize and "enroll" others into a particular system or subsystem. Knorr-Cetina identifies the process of communication as one of obtaining or offering access to resources. The cultural turn in science studies has directed attention to communications as "technologies" with important rhetorical capabilities and implications, deliberately managed by their authors: this is the sense used by Shapin and Schaffer in discussing Boyle's literary style. Others have studied communications as more or less autonomous entities, having their own fate independent of their author's intentions:

> Texts defy the efforts of their authors to control them in large measure because the tropological and rhetorical dimensions of language....ensure a multiplicity of meanings and the possibility of continual reinterpretation (Bono, 1990, 61).

[99] The reference is to Peter Dear's take-off on the Royal Society's motto, "Nullius in Verba" (Dear, 1985).

And once started down the path of communication, one is led quickly to literary and linguistic studies. Science studies has a substantial literature analyzing scientific metaphor[100] and other rhetorical devices and features of scientific literature. Other scholars, including Shapin and Schaffer, have turned to discourse analysis and the notion that certain "language games" (in Wittgenstein's terms) define "forms of life" and "communities of language users." The considerable body of scholarship in science studies devoted to the rhetoric of science includes work on scientific "narratives", genres, and metaphor.[101] These studies, writes Dear, "indicate a growing awareness by historians of science of the potentialities of attention to language, rhetoric, and textual forms in understanding how science has been created....[T]hat language is not simply a transparent medium of communication, but a shaper (perhaps a realizer) of thought and an embodiment of social relations" (Dear, 1991, 4).

There is however, a distinct bias in these analyses of communications as artifacts and products of scientific communities. All of them begin with an assumption of communities, and proceed to examine the "role" of communications in those communities, asking whether communications are independent of their creators; whether they are transparent or manipulative, formal or informal, and so on. The sociologist Keith Stamm has pointed out that this approach derives from the sociological traditions we can trace to two early American sociologists: Robert Park -- who considered communication a mechanism for maintaining community; and Robert Merton, for whom community relationships are the antecedent and communication the consequence (Stamm, 1985, 3-4; 5). The problem with these views, Stamm argues, is that the critical link or "tie" in any community is the activity of the individual. This "tie" may be spatial (a byproduct of physical location); cognitive (the result of identification of an individual with something); affective (the effect of psychological or emotional ties to something); or communicative (constructed by communication). Thus "community ties" are specifically those things which connect individuals to each other across a variety of gaps, which may be spatial, social, linguistic, political, informational, etc. The "community tie" is then a connection that is established through activities that Stamm suggests can be categorized as follows:

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[100] A starting point for metaphor would be George Lakoff and Mark Johnson's *Metaphors We Live By* (1980, University of Chicago), and their subsequent volume, *Philosophy in the Flesh* (1998, Basic Books).

[101] Among them are Charles Bazerman (1988), *Shaping Written Knowledge: The Genre and Activity of the Experimental Article in Science* (Madison, University of Wisconsin Press); *Gillian* Beer's 1983 work on *Darwin's Plots: Evolutionary Narrative in Darwin, George Eliot and Nineteenth Century Fiction*, cited by Jan Golinski (1998); and Peter Dear's edited volume, *The Literary Structure of Scientific Argument*: *Historical Studies* (1991), with articles by Thomas Broman, Peter Dear, Frederic Holmes, Bruce J. Hunt, Lynn K. Nyhart, Lissa Roberts, and Lisa Rosner.

- attending: keeping informed of & aware of others
- orienting: individual thinks about, has ideas about others
- agreeing: individual shares concerns and views of others
- connecting: talking to, listening to, getting together with others
- manipulating: working to have an effect on others

The significance of Stamm's analysis for the present subject is that he offers an analysis that is closer to Latour and Woolgar's in attending to the informal establishment and maintenance of "ties," but he has provided a more structured set of categories with which to identify those activities apart from any explicit, formal "communications" (in the sense of written arguments, narratives, or reports) that may accompany them. The point is that the formal products and instruments of communication do not, themselves, constitute community.[102]

Struan Jacobs and Brian Mooney have noted other problems with the operationalization of community in terms of communications. They argue that numerous citation studies have failed to clearly identify or describe scientific communities. This does not mean that "scientific communities" (otherwise operationalized) do not exist; but it does mean that another view of the "community ties" of scientists (in Stamm's sense above), is that these ties are the *activities* scientists undertake in order to ensure continued access to needed, or valued, resources. Karin Knorr-Cetina has proposed that a more complete account of scientific communication would not limit itself to purely "cognitive" interactions, or even to the transfer of resources (credit, reward, money, and so on) that figure importantly in traditional sociology as well as in Latour and Woolgar's descriptions of technoscientific networks. Knorr-Cetina argues that the study of science should take in "transepistemic fields," [understood] as the "locus of [the scientist's] struggle to establish and maintain 'resource-relationships,'....'relations to which one resorts or on which one depends for supplies or support' " (Jacobs and Mooney, 1997, 477).

What this might mean for science studies may be illustrated by turning back once again to Anne Goldgar's account of the Republic of Letters. Goldgar begins not with community, but with the

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[102] As Goldgar has written in the context of her historical work on the Republic of Letters, "*The text has been thought to contain all meaning*, even if that meaning was not intentional. But the meaning, or the identity, of an intellectual community is not necessarily to be found only in its writings. To assume otherwise is to beg an important question, and to impose our own discourse about intellectual life on another era. We must read between the lines, and ask whether scholars' world view was really entirely structured around the subject matter they discussed. We must consider whether *the establishment of meaning was in fact the dominant motivation for members of the Republic of Letters*" (Goldgar, 1995, 4-5, emphasis added).

formal and informal exchanges and interactions that constituted it: "[T]he scholarly community was foremost *a collection of people*. This collection was tied together by certain shared values which were created by the interactions among its members. Thus, more than the people themselves, or their works, or their specific connections, it is important for us to look at the *interactions of citizens* of the Republic of Letters to understand what the community really was" (Goldgar, 1995, 4, emphasis added).

But Goldgar's exploration of the informal institutions of the Republic of Letters led her to look beyond communications in the form of correspondence and publishing, to what remained in the way of evidence of personal relations and friendships, "quarrels, gossip, dinners, lending of books and sharing of information." She found that these helped to forge a concept of community that was "not as important as their own identity as a community." Communal bonds "were the focus of community; the Republic of Letters was a reflexive event" (Goldgar, 1995, 6). She emphasizes that the community was *not* important to its members because of its shared intellectual paradigms -- controversy and disagreement were common and tolerated. But the community was managed at least sufficiently to ensure its members continued access to the resources each might offer the others: these ranged from the conveniences of mutual assistance to the contrastive benefits of encountering differing opinions and ideas. Thus the "gentlemanly" virtues that Shapin identified as a mechanism for attaining intellectual consensus and authority, operated, according to Goldgar, in a much more general and instrumental way as a matter of private interest: the social virtues looked for in scholars included, as we saw in the last chapter, openness, assistance, willingness to share information and resources (Goldgar, 1995, 154).

4.6 Hobbesian Community: The Creative Construction of Individuals

> He drew a circle that shut me out --
> Heretic, rebel, a thing to flout.
> But Love and I had the wit to win:
> We drew a circle that took him in.[103]

The discussion above brings us back to issues that are central to Hobbes's thinking, and also central to his controversy with, and exclusion from, the Royal Society. Let us take a quick look at what Hobbes's ideas about community might mean for science in contrast to the views presented above. Why do people engage in activities that form communities? Why are some

[103] The poem is Edwin Markham, 1950; it is quoted by David Hollinger, at 26 in Walters et al., 1997, chapter 1, "How Wide the Circle of the 'We'?"

people taken in and others left out? And, to what extent does it make sense to assume that what people think, believe, and "know", is an artifact of the communities with which they are identified?

Hobbes's tremendous originality as a social philosopher -- even today -- consists in his having addressed the problem of sociality as a fundamentally individual problem. Coupled with his consistent materialism, this meant that his understanding of community was at the same time rigorously material, and (individually) constructivist. Community for Hobbes was a profoundly important collaborative invention. It was a "solution" to the fundamental social problem of reconciling the needs and desires of individuals with the reality of other individuals' needs and desires. But its authority was not separate from the individuals who constituted it: rather, its authority derived from its unique ability to serve critical individual interests. According to Hobbes, the acts whereby individuals consent to create, or join, a community, are highly rational (if not always consciously so). But while the community may be rational, this does not mean that the activity of the mind is: philosophy, Hobbes acknowledged, is "a Lust of the mind" paling the "short vehemence of any carnall Pleasure" (Hobbes, *Leviathan*). The individual members of communities are not bound by their membership to any particular set of inner thoughts and beliefs. With the exception of the few subjects about which we may reasonably hope for full understanding (geometry and politics), Hobbes maintains that in other areas of science and knowledge we cannot expect to gain full understanding, whether through experience, experiment, *or* agreement.

This would seem to be a recipe for endless controversy in large areas of science. Where in our late modern era Kuhn's account of scientific controversy stressed that epistemic controversy was the exception, Hobbes's approach developed in the early modern era, stressed that it was the norm. For contemporary STS scholars the Hobbesian notion is still uncomfortable. While, (as we saw above) they are greatly interested in scientific disputes, these are studied in somewhat the way doctors study of disease and irregularity.

A Hobbesian view of scientific controversy suggests that scientific controversy is not anomalous, but rather is the inevitable expression of the persistence of individual belief and the inherently *human* situation of human actors in a thoroughly material world. For a Hobbesian, community is a powerful, necessary invention of humans, one that enables them to pursue their interests and purposes, in which they are keenly, personally, invested.

Placing oneself in the context of a trusted community is thus not a matter of giving up authority to the "group". It is a matter of facing the rational necessity of living in an inherently complex and even chaotic environment. Similarly, conducting the pursuit of knowledge in the context of a trusted community is not a matter of placing epistemic authority with that community. It is a matter of practical self-interest to conduct this pursuit with the support of others -- and the most important "support" of all may be the strong opposition another presents to us![104] And finally, the pursuit of knowledge cannot have as its goal absolute or comprehensive knowledge of the material world: the point is not to find "the" order of the world, but to so construct "an" order in the world that it becomes meaningful to us. In this sense science is natural *philosophy*.

[104]One thinks, for example, of Paul Feyerabend's poignant dedication of *Against Method* to Imré Lakatos, "Friend, and fellow-anarchist", who died before it was published. "[E]very wicked phrase [that Feyerabend's 'essay'] contains was written in anticipation of an even more wicked reply from the recipient [Lakatos]. It is also clear that as it stands the book is sadly incomplete. It lacks the most important part: the reply of the person to which it is addressed" (1975, London: NLB).

5. Conclusion: At the Margins of Modern Science

Leviathan and the Air-Pump represents a particularly complex challenge to the reader who attempts to pin down what Shapin and Schaffer mean by "community". In their usage, "community" is a pastiche of most of the meanings of community considered in the chapter above. The reader's difficulty in sorting out those meanings is exacerbated by the ironical and reflexive quality of the book, which make it difficult to separate Boyle's and Hobbes's understandings from Shapin's and Schaffer's.

5.1 Shapin and Schaffer: Science as Community?

According to Shapin and Schaffer, the scientific community that Boyle was attempting to create was both Mertonian and Kuhnian. It had Mertonian pretensions (to objectivity, universality, democracy, skepticism); *and* it was struggling to establish the scientific community -- Kuhnian consensus -- as the source of epistemic authority in science. It was both a long-term and hugely successful "solution" to the problem of knowledge, and an historically contingent outcome that could have easily gone otherwise. This is somewhat confusing.

Meanwhile Shapin and Schaffer's methods reflect the anthropological turn, with its emphasis on the shaping significance of the local. According to Shapin and Schaffer, the story of Hobbes and Boyle and its outcome were strongly conditioned by very specific historical and cultural contingencies: by the civil war in England, and by the cultural and normative inclinations of a tiny band of well-off Christian gentlemen living out a particular late seventeenth-century brand of British culture. At least at first, they take no sides: their pose is one of (simulated) ethnographic observation, evenhanded neutrality. They pay special attention to scientific instruments (the air-pump, in this case), with their own peculiarities, limitations, and powers to persuade and to transfer knowledge. But after all, it turns out that the "ethnography" is much more than a local story: it is also the origins story for the way modern science is done: they have pulled a macro-rabbit out of a micro-hat.[105]

[105] This adds another reflexive layer to their story: in most ethnographies, origins stories are analyzed as reflections or expressions of the community's cosmology, not as real historical accounts. In their account of Boyle and Hobbes, Shapin and Schaffer present us with an origins story *qua* history, embedded within an ethnography of our own (modern) culture of science.

In Shapin and Schaffer's narrative the political nature of scientific community is a central point. In part, this reflects their sense of how Boyle's and Hobbes's divergent political views account for their respective ideas about how science should be conducted. Their argument for a necessary relationship between science and politics also reflects Hobbes's political philosophy, that put political and natural science on an equal footing. As for Shapin and Schaffer's own ideas, they go beyond the fairly tame idea that science and politics are affected by each other, to the much larger assertion that "the solution to the problem of knowledge is political" (*L&AP*, 342). They do not go so far as Foucault in suggesting that the content and methods of modern science *are* inherently exercises of social power. But this hesitation seems to stem in part from a residual Kuhnian sense that "science as we know it" is managed by consensual communities; and in part from a nod to Merton's contention that members of scientific communities share values that help produce useful results. Despite their symmetric exposition of Hobbes's science and politics (this time in the Strong Programme sense of "symmetric"), Shapin and Schaffer leave the reader with the sense that when Hobbes was banished from the house of the Royal Society, the black box of modern science also closed. This impression might have been dispelled had Shapin and Schaffer reflected on what was taking place outside the Royal Society, in the salons and country houses and universities, and outside of England.

Shapin and Schaffer's definition of scientific community also embraces the strategy of operationalizing community in terms of communication. They treat "literary technology" and rhetorical strategies as central elements in the negotiation of community membership, structure, and boundaries. They devote a very large portion of their book to close analyses of texts that they take to represent the full significance of the debates between Boyle and Hobbes. On the other hand, their analysis of community does not really depend on any acts of communication having taken place: rather, the protagonists' respective texts (as Dear noted above) could be usefully juxtaposed even if they had never been published, or even if Boyle and Hobbes had lived at different times. And while Shapin and Schaffer argue that Boyle's literary technology had a specifically persuasive intention, their argument is not bolstered by evidence of how this communication actually constituted or affected a "community."

In summary, the community that Shapin and Schaffer argue was to one extent or another *invented* by the Royal Society, and which became the *social technological* foundation for modern scientific authority and practice, is a fairly familiar sociological one: an amalgam of Kuhn's and Merton's. But it is made at least confusing and perhaps incoherent by Shapin and Schaffer's use

of the techniques of post-Kuhnian sociology of science to support this quasi-Mertonian, quasi-Kuhnian understanding of science.

Ironically, since they did so much to bring Hobbes to the attention of science studies, I believe that what Shapin and Schaffer may be missing is an appreciation of a radical alternative offered by Hobbes. This was not so much an alternative *politics*, as an alternative *science of human knowledge*: what one might term, in parallel with "natural philosophy" and "natural history," *natural reason*.

5.2 Natural Reason

The historical encounter between Hobbes and Boyle serves us better not as an origins story, but as an exemplar of an important encounter in the history of human thinking. That encounter was with us long before Hobbes and Boyle and persists to this day: it is the struggle between different ways of viewing the relationship between human beings and the world. Boyle insisted on fencing off what we would now call the "human" sciences from the study of the "natural" world (*L&AP*, 337); Hobbes developed a comprehensive science that not only included humans, but which took the individual mind as the generative agent (in the full sense of *auctor* or *authority*). When Boyle withdrew from the risks of personal belief and experience to the "humbler" circle of experiment and collective opinion, he also withdrew from the authority of personal -- and rational -- belief; while Hobbes relied "stubbornly" on his own mind as a source of authority in the face of almost universal disagreement.

Thus one of the important things going on in the debate between Hobbes and Boyle is a struggle over the status of the "social" and "human" sciences relative to the "natural" sciences. It was indeed, in a sense, the great triumph of early modern science to reverse the directionality of scientific investigation. Frances Yates, in a study of Giordano Bruno, wrote that,

> The basic difference between the attitude of the magician to the world and the attitude of the scientist to the world is that the former wants to draw the world into himself, whilst the scientist does just the opposite, he externalizes and impersonalizes the world....Hence, may it not be supposed, when mechanics and mathematics took over from animism and magic, it was this internalisation, this intimate connection of the *mens* with the world, which had to be avoided at all costs. And hence, it may be suggested, through the necessity for this strong reaction, the mistake arose of allowing the problem of mind to fall so completely out of step and so far behind the problem of matter in the

> external world and how it works....This bad start of the problem of knowledge has never quite been made up.[106]

It is hard to improve on Yates' formulation, but I will try to elaborate a little: the path between mind and nature is just that, and there have *always* been two potential directions for the investigative arrow between these. The modern scientific turn has (Yates argues) been defined by its discovery of the flow of reason from mind to nature, that is, of externalising and impersonalizing the world. Those modern scientists who have followed the nature-to-mind direction (drawing the world into themselves, interpreting it according to personal, systematic philosophies), have at times found themselves marginalized on that account.[107]

We have come to expect an externalization of knowledge claims, whether via community (the sociological view), or the disciplining of knowledge (Foucault), or through a pervasive translation of ideas, observations, and the like into measurable quantities (Porter). But there are alternatives; the larger point here is that however we define modernity, we should be careful about identifying modernity with science.

For Hobbes, the "problem of mind" *was* a "problem of matter." But the separation of the social and human sciences from the natural sciences which Boyle's "new science" represented, and which Shapin and Schaffer identify with "modern" science, not only *creates* the "problem of mind": it also results in a set of difficult problems at the macro- micro interface of the "social" and the "human":

> The area (of social science) that needs work most urgently involves theories about micro-macro linkages, especially the development of theories about change, and even more especially, theories about change at the micro level affecting the macro level (Entwisle, 1991, 286).

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[106] Frances A. Yates, *Giordano Bruno and the Hermetic Tradition*, London, 1964, Vintage Book edition, 1969, 454-455.

[107] This is the subject for an entirely separate thesis, but it is my belief that many marginalized modern scientists have been marginalized in part because they were perceived as relying on a personal systematic philosophy, both as a stimulus to discovery and as a standard for their own belief. I have in mind as examples the modern American biologist Robert Rosen; and the Swedish astrophysicist Hannes Alfvén. This is quite different from the tolerance shown to scientists who develop an explicit, systematic, but often speculative set of knowledge claims *post hoc* (often following major success in their career, or near the end of their lives) -- one thinks here (in contrast) of the physicist David Bohm, or the biologist Stuart Kauffman.

What is needed to provide such linkages? Kuhn struggled with this problem without success. Many recent scholars have thought they could forge them by working at lower levels (microhistories, microethnographies, microsociologies). Others have shifted the focus to the artifacts (instruments, mostly) that promise the richness of individuation without the risks of introducing individuals' minds. But the problems with these and most other micro-macro linkages in the human sciences stem from the reluctance of social scientists to push their micro analysis beyond external behaviors, into the "inner" realms of the mind and the self. It is as though the determination of the Royal Society that certain topics should be forbidden to science (politics, religion) persists to this day, in the form of a reluctance to admit mind (because inaccessible to collective witnessing) as a proper subject for science. Until the relatively recent emergence of cognitive science and neuroscience as full-fledged research areas, "cultural" scientists have gotten as close as anyone to admitting mind, by admitting communication -- the "behavior" used to bridge the gap between minds -- as a subject. The gap between communication and mind is forgotten at our peril. Communications -- whether as observed behaviors or as "inscriptions," tracings of past behaviors -- are to mind what the shimmering paths left by snails in a morning garden are to the snails. What is needed is a "thoroughly modern mentalism"[108]. Modern science has already laid important groundwork for this, through neuroscience, linguistics, and cognitive psychology, and it is highly possible that these developments will make the *modernist* boundary between nature and mind fully permeable -- *within* the bounds of *science*.

What do these "inner" sciences of the mind (as opposed to the still "external" sciences of behavior, and communication) portend for science studies? It could be that they will force a new encounter with a subject that has almost the status of a taboo: the semi-sacred subject of our individuality, and our "free-will." It is a moral taboo -- because we deeply believe in the utter uniqueness of our individual lives, and this is difficult to reconcile with the idea of a "science" of individuals.

A "science" of individuals is also a philosophical and scientific taboo. Our modernity (in Foucault's sense) tells us that science is about finding actually-existing, naturally- given order in the world. The world's chaotic and disorderly features -- including that capillary *dis*order that is

[108] This phrase, attributed to Jerry Fodor, appears in the entry for "mentalism" in the *Cambridge Dictionary of Philosophy*, 482-483 (1995), which contrasts the behaviorism of Skinner with explicitly materialistic cognitive science.

found at the level of individuals -- are (according to this "modern," "scientific" way of thinking) deviant. As far as modern science has gone toward developing a common understanding of external deviance (deviance measured in terms of abnormal behaviors and communications), it has not yet penetrated to the inner layer of individual identity that is "self". As individuals, we are defined by our departure from the "normal," hence we are defined *as* deviant. We might fear that if a science of the (inner) mind were possible -- if we could create the conditions for collective witnessing of our "selves" -- the result would be to force us to frame our own self-understanding in terms of deviance.

Although this way of phrasing the potential relationships between science and individualism reflects Foucault's influence, studies of the spread of abstract mathematical capability make similar suggestions. The pre-modern aesthetic was one in which the mind's power provided creative, simplifying abstractions that made it possible, for example, to estimate the grain capacity of a set of distinctly-sized barrels; while the modern aesthetic was one that relied on standard-sized barrels and precise measures, having developed the "habit" of seeing, and looking for, "grounded rule underlying all observable reality.....the ...*habit of seeing sure and clear mathematical rule under seeming vagary, of <u>discovering</u> universally applicable order in the real*" (Crosby, 1997,143).

But a Hobbesian "natural sociology" might offer a way of handling the deviance -- or autonomy and uniqueness -- of individuals as a deep premise, rather than as an anomaly.[109] In such a science, the study of how relationships (historical, accidental, intentional, economic, geographic, communicative, political) are forged by individuals might become the basis for a new "Leviathan." The most fruitful methods for such study might include biography,[110] ecology,[111] and neuroscience.

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[109] Richard Burian has noted the resonance of this notion with the Darwinian idea that variation is not deviance, but a necessary condition for survival, growth, and change (personal communication, March 1999).

[110] I prefer to think in terms of "biography;" others have preferred the term "actor-oriented." An actor-oriented approach to the history of science, emphasizing the "life-course-structure," would represent a potential blending of microsociology and biography, of ecology and intellectual history (Rosenberg, 1988).

[111] Although, according to Golinski, the suggestion of an ecological approach to studying science made by Adele Clark and Joan Fujimura, is as a way of pursuing the "hidden ordering principles" behind the "apparent confusion" of things and activities of science; and the "importance of relations of mutual interdependence between the different elements, like the connections between the various organisms that inhabit an ecological system" (Golinski, 1998, 139-140). I would put it differently, that one can have a serious ecology without "hidden ordering principles."

Some of these new sciences are already, I believe, driving us toward a most interesting encounter the like of which we have not seen plainly for three hundred years: the reconciliation of a thoroughly material understanding of reality, with our stubborn insistence that there is a reality -- one that we refer to with the language of spiritual experience -- that has had only a marginal and uncomfortable relationship with Western science. It is this sense of a very long-awaited breakthrough that creates excitement in Western philosophy when we read, in Lakoff and Johnson's latest book,

> If there is no disembodied mind or Soul, then what is the locus of the real spiritual experience that people have in cultures around the world? This experience can only be embodied. It must be a consequence of what is happening in our bodies and brains..... The environment is not an 'other' to us. It is not a collection of things that we encounter. Rather, it is part of our being... Embodied spirituality is ... an ethical relationship to the physical world (Lakoff and Johnson, 1998, 566).

Shapin and Schaffer have rightly said that the problem dealt with in the struggle between Hobbes and Boyle, is a problem of order; and that the struggle for order is not only external (political, cultural, communicative), but also a struggle for *cognitive order*. But are the two the same? Shapin and Schaffer's thesis underscores the extent to which contemporary STS has presumed that the cognitive order won by modern science defines the cognitive order *possible* for science. I have tried to suggest above that the underlying battle for cognitive order had as much or more to do with *modernity* as with science, and hence requires not so much an origins story as an account of broadly complex social and cultural changes that would themselves profoundly change what we count as natural knowledge and to whom we look for scientific authority. At the point where the battle had not yet been won for Boyle and the new experimentalists, Hobbes argued the point with the passion and directness of someone who knew that far more than personal egotism was at stake. He argued, of course, that the two are *not* the same: that the problem of social order is *not* the same as the struggle for cognitive order, even though social order may provide the condition for pursuit of cognitive order.

Even in Foucault's formulation, which appears (like Hobbes's) on the surface either to advocate or acknowledge a nasty sort of universal social discipline -- community is not consensus. For Foucault, community is "an inventive discursive formation within which oppositional agents must find creative means of exchange or be relegated to silence" (Brahm and Driscoll, 1995, 151-152).

Both Hobbes's and Foucault's formulations strongly imply that the survival of community depends on an ongoing individual struggle to create, and then to translate inner order to outer order. That struggle in turn depends on the existence of "oppositional agents;" it depends on those agents having a generative capability ("creative means"); and it depends on those agents being successful in conveying messages that will be returned (exchanged).

A close examination of the controversy between Boyle and Hobbes is fruitful for historians and other scholars, and Shapin and Schaffer's provocative resurrection of the ideas and events surrounding both of these individuals have likewise stimulated important discussions. But contrary to Shapin and Schaffer's claim, the importance of this microhistory is not that it reveals the inner workings (local factors) that tipped a kind of macrohistorical balance. Rather, Hobbes's and Boyles's controversy is significant even at this distance, as a myth that helps illustrate and stand for a deep and important tension still very much present in science: this is the tension between the authority of shared beliefs, and authority "against community." By authority "against community," I mean authority that is demonstrated through the systematic thinking and work of individual scientists; and which comes neither from ancient texts nor public demonstration, but from creative and lived commitments to ever-changing combinations of beliefs, experiences, and possibilities. By "systematic" I mean the implicit or explicit insistence on finding order and coherence in ideas and discoveries of science, a predilection for which Hobbes was known.

In the place of a science that posits natural order and lawfulness, and looks in the world for simplicities that can only exist through the careful taming of degrees of freedom, we might have a science that presumes a kind of dialectic between the generation of simplicity, natural complexity and disorder: each bringing about the other.[112] In the place of consensual agreement, the "agreement" of science, a modern Hobbes might suggest, is the compelling but open-ended generative structure of the individual mind. In the place of *modern* science, struggling to mirror some transcendent truth through human order, we might instead understand science as an ingenious, creative, interpretative achievement that with any luck will be continually circulating and continually renewed.

[112] A social theorist who hints in this direction is Niklas Luhmann. See for example his *Social Systems*, 1995.

Appendix 1: Selecting *Leviathan and the Air-Pump*

Shapin and Schaffer's work is distinguished in several important ways from otherwise equally important books that promote the constructivist perspective, that the epistemic authority of science is a product of the social, or social and material, interactions around which the practices of science are organized. There is Latour and Woolgar's *Laboratory Life*, for instance, or Pickering's *Constructing Quarks*. But Shapin and Schaffer's work is distinguished from these and other constructivist works by virtue both of its broad historical argument, and its attempt to provide the very same kind of "virtual witnessing" (in this case, of the birth of modern science) that they associate with early modern experimental practices. In contrast, Latour and Woolgar, though avowedly "non-modern," adopt the *post-modern* temporal modality: that is, one essentially without a temporal dimension. Pickering, whose work on physical theory deals primarily with very recent science history, has no reason to invoke a deep historical dimension to his arguments.

Shapin and Schaffer's work in fact resembles Foucault's *Discipline and Punish* in important ways: both books attempt to recover the history of epistemic practices by revisiting and recreating a crucial moment, allowing the reader to observe an alleged historical black box in the making.[113] Like Foucault, Shapin and Schaffer put forward broad and provocative claims. They present their views neither as cautiously as Merton presented *his* bold (but related) claims in the 1930's, nor as ambivalently and ambiguously as Kuhn thirty years after Merton. Their historical arguments are not built on relatively obscure moments of techno-scientific history (cf. Latour but also Law, Bijker, et al., 1985); nor around a single important individual (cf. Biagioli 1993). Instead they make strong claims about familiar subjects, thus providing a larger and more interesting target for interpretative as well as technical historical disputes. They have drawn criticism from other scholars -- historians in particular -- who find the crucial "moment" Shapin and Schaffer have identified to be not nearly as clear-cut as they have represented it. Some critics have argued that mid- to late-seventeenth-century British politics and social history did not represent, as Shapin and Schaffer claimed, an "origins" story for the development of modern theory and practice of experimental science. Equally important developments, they argue, took place elsewhere in other European venues, or during much earlier times. Others have accused Shapin or Schaffer of mishandling the historical evidence in order to bolster sociological arguments.

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[113] This resemblance has been noted, e.g. by Christopher Norris, 1997. Norris is sympathetic neither to Foucault nor to Shapin and Schaffer, noting the latter's "Hobbesian bias."

Such criticisms are reminiscent of questions raised about Foucault's historiography: how accurate is his reconstruction of the events and texts? How accurate is the assertion that *this moment* was one when a new social and epistemological model was formed? Is *this moment* definitive of science -- and does it retain the sense that science is a "special" kind of knowledge, or does it flatten the epistemic landscape so that science has no special epistemic status? Is there science without discipline (in Foucault's sense) or community (in Shapin and Schaffer's)?

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