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Cultural History of Science: An Overview with Reflections

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The increased popularity of the label "cultural" within science studies, especially in relation to "cultural studies," invites consideration of how it is and can be used in historical work. A lot more seems now to be invested in the notion of "cultural history." This article examines some recent historiography of science as a means of considering what counts as cultural history in that domain and attempts to coordinate it with the sociologically informed studies of the past ten or fifteen years. The label "sociocultural" seems a more useful term by which to capture recent developments.

I.

The advent of "cultural studies" has created new connotations for the category of "cultural history," even though the latter is by no means necessarily the same as "cultural studies of the past." Recently, the official announcement of something called "the *new* cultural history" has reminded us that ideas of progress are still very much with us in this allegedly postmodern academic world (L. Hunt 1989). In the history of science, a new unit created at the University of California, Los Angeles was originally called Center for the Cultural History of Science. The name was subsequently modified to Center for Cultural Studies of Science, Technology, and Medicine. The intended resonances between cultural history and cultural studies were in place from the start. But the symbolic power of such labels is fraught with dangers.

Within the discipline of history, cultural history is a fairly well-understood item, whether or not qualified by the word "new." The idea, in its broadest terms, is to make something that appears strange to the reader look like something that in retrospect ought to have been expected. Robert Darnton's *The Great Cat Massacre* strategy illustrates the point: Darnton attempted, by appropriate symbolic elucidations, to normalize his readers' historical grasp of an eighteenth-century episode in which French printers captured and killed

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numerous neighborhood cats and regarded it all as great sport (Darnton 1984; see also LaCapra 1988 for trenchant comment). In many respects, the "normalization" view is quite a good way of thinking about the matter: for the historian of science it has resonances with Kuhnian prescriptions. Thomas Kuhn, wearing his historian's hat, said that, to understand a scientific article from the past, it is insufficient simply to read it and congratulate oneself on being able to grasp more or less the whole thing. Instead, one should focus on the apparent absurdity that, by contrast with everything else, seems not quite to fit. Only when that anomaly makes sense will everything else fall into place along with it, and an adequate understanding will be achieved (Kuhn 1977, xii). The procedure amounts to a hermeneutical trick.

Studies usually considered as cultural history routinely operate in such a fashion. One takes people doing things that to us look somewhat unexpected—or, crucially, can be *presented* as looking odd—and makes sense of their behavior by appropriate contextualization: finding out what made particular behaviors or ways of doing things look normal. In practice, the question is one of learning how to read the proper meanings of texts and the acts that they presuppose or that are witnessed through them (Chartier 1988). This kind of historical enterprise is not reductionist or foundationalist; it is not a matter of explaining something *in terms of* something else. Lynn Hunt proclaims a "new" cultural history along these somewhat vague lines in her recent collection (L. Hunt 1989). It seems implausible, however, that, to the extent that there are new approaches within cultural history, their novelty lies here. More precisely, perhaps, some novelty may lie in a form of representation of one's subject that asks the reader to participate; to reexperience the historian's own hermeneutical achievement. Even this, however, is not an accurate way of characterizing most cultural history, including cultural history of science.

Special difficulties arise from the central problem inherent in any kind of history of science that portrays itself as cultural. This problem becomes visible through a simple consideration of what we take culture to be: at its most fundamental, "culture" is a concept that simply designates whatever *is not* nature (Strathern 1992). Therefore, the problem becomes especially piquant in looking at the history of the making of knowledge about nature. Do we draw the implied ontological demarcation between culture and nature, so as to say that culture makes the knowledge, whereas nature is what the knowledge is *about*? This seems to be a kind of metaphysical realism that lacks any true cash value: it allows for an independent nature with definite properties but permits nature no role in the making of natural knowledge (see Feyerabend 1988, claiming to be a "metaphysical realist"). Or do we go all the way, maintaining that *everything* that humans do and say is by definition

culture, so that we have no need to speak about "nature" as anything other than a human construct? That move has certain attractions, but it leaves the notion of a specifically "cultural" history without any useful meaning: all history becomes cultural history.

In practice, however, these questions tend to be ignored in existing historiography (on a related theme, see Shapin 1992). There is frequently no reason why they should not be, unless a work parades itself as cultural history, in which case the reader might reasonably expect an explanation of what the writer has in mind. Moreover, some kinds of historiography of science—among them cultural history according to the working definition discussed above—call these questions forth by implication.

II.

A small body of recent work in the history of sixteenth- and seventeenth-century science holds the most secure claim to the label "cultural" history of science by virtue of the fact that it is a direct application of preexistent early modern European cultural history. Examining sixteenth- and seventeenth-century court culture, especially in Italy, young historians such as Paula Findlen, Mario Biagioli, Pamela Smith, and Jay Tribby have done valuable work on court-cultural norms and practices relating to the making of natural knowledge, portraying experimental work as performance, museum collection as self-fashioning, and philosophical authorship as the management of patronage systems (Biagioli 1993; Findlen 1990, 1993, 1994; Tribby 1991; Smith 1991, 1994; the pioneering study is Westman 1980). There is a strong temptation to see this work as the upshot of a casual relativism that does not ask about the truth or falsity of knowledge claims, but simply accepts them as whatever they are understood to be in a local cultural context. Jay Tribby's work is particularly aggressive in this regard. Discussing the "experiments" of the Accademia del Cimento in Tuscany in the late 1650s and 1660s, which historians of science have traditionally used to illustrate the growth of experimental science in Europe in the second half of the seventeenth century, Tribby is unconcerned about whether these activities can be usefully described in any kind of language associating them with modern notions of experiment in science. For him, they have to do with the establishment and reproduction of a Tuscan national identity, not the creation of impersonal knowledge about nature according to a modernist creed (Tribby 1994; cf. Biagioli 1992). Some of Findlen's recent work also sounds this theme (Findlen 1993). But there is another side to the matter, which becomes clearer

when the historian purports to speak within the auspices of the history of science rather than the history of early modern court culture.

In a recent article on the scientific revolution in Italy, Mario Biagioli observes in passing that Italian astronomers after Galileo had trouble emulating Galileo's own career strategies "partly because of the way the solar system happened to be" (Biagioli 1992, 22). Thus, in Biagioli's account of early modern astronomy, an independently existing, noncultural solar system enters into an explanation of the efficacy of seventeenth-century Italian patronage networks. The move is familiar to historians of science: it amounts to an implicit reestablishment of an "internal/external" division, this time built along a "culture/nature" axis (Shapin 1992).[1] There is, however, no reason why new directions in the cultural history of science ought of necessity to encounter this difficulty. The dichotomous division between culture and nature always stands in the wings, to be sure, and invites historiographical practices that take science itself to be about an independently existing world rather than being an enterprise that thoroughly constitutes that world as a determinable entity. But Galileo did not just stumble across things that were prepackaged moons of Jupiter. He needed to *make* those things. The degree of difficulty attending that accomplishment depended on the forms of practice currently sanctioned by the wider astronomical culture and the confederations within it established by Galileo and others. Galileo at the same time successfully sold the accomplishment to the Tuscan court.

In the history of science, instead of speaking about culturally laden interpretations of the nature of things, we can speak of the determination of the existence of those things in the first place. Bruno Latour has made much of this idea, arguing that the determination of the properties of something and the establishment of its existence are coextensive processes; the thing *is* the concatenation of its supposed properties (Latour 1987). Things, such as moons, stars, and planets, are culture too. A point that emerges with particular clarity from this example is that the major components of a proper understanding are social—the structuring and operations of identifiable social groups. In discussing new directions in cultural history, Roger Chartier (1988, 14) has stressed that the social world is made through representations. Similarly, Latour argues for a symmetry between knowledge of the social and natural world (Latour 1993). Little good is served by mere disembodied talk of "meanings." Chartier's own work indicates the need to tie representational or, more generally, discursive practices to the social positions of those who engage in them; meanings cannot interact solely with one another without losing all significance. The central focus is the accomplishment of some sort of *social* understanding of how people do what they do and are what they are.

The problem is to avoid being straitjacketed by unwieldy, quasi-deterministic models of social structure that exist outside the discursive practices that serve to represent the social world. It may be more helpful, therefore, to use a different term, namely "sociocultural" history of science, with respect to which one can locate those historical studies that invite the label "cultural."[2]

If cultural history as a genre is understood as rejecting reduction to purportedly more fundamental elements—such as one might find in explanations of the French Revolution that invoked the idea of a crisis in the mode of production, for example—then sociocultural history denotes a distinct approach: the prefix implies the provision of a social, or perhaps sociologically informed, account of the workings of a society or identifiable social group in the making of meaning. Most historians of science nowadays resist restricting themselves to studies of the structure of scientific institutions or disciplines in which either the ostensible content of the science is left aside, in the manner of a Mertonian sociologist, or the historical growth of science is detailed as if by chroniclers of the development of a biological organism. The major topics of investigation in more recent history of science are laboratory practice, theoretical discourse, and, in general, anything that might go to make up scientific knowledge. The more flexible term "sociocultural," as a designator of such historiography, carries the implication of a degree of foundationalism, insofar as we here typically have to do with social (but not necessarily "sociological") explanations of what one might call "scientific behaviors"—pieces of scientific culture. Such foundationalism, at what is often a highly mediated level, is perhaps inevitable in the bootstrap procedures of making explanations.

In a classic article that represents cultural history of science as usually understood, Larry Owens (1985) attempted to lay out a broad-based understanding of the inauguration of laboratory science education at the new Johns Hopkins University in the immediate postbellum period. He searches for deeply rooted cultural explanations of laboratory pedagogy by bringing into the picture the gymnasium built at Harvard and the growth of intercollegiate football, especially centered on Yale. Owens's account uses Huizinga's image of *homo ludens*: the idea of a game and the space within which its conduct is contained informs his reading of the Hemenway Gymnasium, the Yale playing fields, and Henry Newell Martin's physiological laboratory at Hopkins. All operate within "a place apart," governed by their own rules and devoted to the creation of an artificial order. To make us understand the new Hopkins laboratory teaching, Owens shows us that functionally analogous "games" were being inaugurated at other institutions of higher education in the United States at the same time. The cultural meanings of all these innovations cluster around the common theme of "pure and sound government": the consolida-

tion in the wake of the Civil War of new forms of governing an increasingly fragmented and heterogeneous body politic.

However, Owens's argument makes sense only in relation to its social correlates: the things that were done or built are meaningful only because of the sorts of people who did them or built them. Those people are understood by reference to their social positions: the central characters were almost all members of the north-eastern ruling elite, and Baltimore was a city emerging from a divided and scarring period of conflict. Symbols do not float free of the society that displays them; their very meanings as symbols depend on the social locations that arrange them—and vice versa. It would ordinarily have been adequate to speak loosely of Owens's article as cultural history; now, with the mutation and drift of labels, it can be seen more precisely for what it, and practically all such history of science, really is: sociocultural.

Examples could be multiplied, but the point could only really be made effectively by adducing an example of a piece of historiography that could *not* reasonably be called anything other than "cultural." This is hard because history in general operates according to criteria of intelligibility that routinely create cultural understanding by reference to the society that creates that culture. How else could it be?

III.

In most areas of general history, a practical distinction between cultural and social history has long been in place. Social history has been taken to refer to studies of demographics, family structure, and other numerically relatable phenomena. Cultural history has been thought to deal with qualitative aspects of behavior not obviously (apparently) determined by givens of human existence such as reproduction or starvation, or by "rational" considerations such as political machinations. Now, on the one hand, the history of science has certainly not traditionally been concerned with things such as demographics; on the other hand, notions of the rational have themselves become problematic in recent years. In studies using sociology of knowledge approaches to the making of scientific knowledge—including knowledge that once would have been seen as based on straightforward rational inferences—the labels "social" and "cultural" have truly gone askew.

Thus recent explicit attention to cultural history of science must be explained in some other manner. The appropriate reference seems to be cultural studies, and the reconstitution of the value of the word "cultural" that has issued from it. Semantic slippage, not functional labeling, has resulted in the present rather confused state of affairs.

In a recent article, Joseph Rouse (1992) has discussed cultural studies of science, including historical work. One of the surprising things about the article is that both Robert Marc Friedman and Donna Haraway are included in a list of historian-practitioners of this endeavor. Any historian familiar with their respective writings would not find this a natural grouping. If Rouse's label has any value, it is only by exclusion, therefore, not inclusion: anyone who does not clearly fit into the groups that are contrasted with cultural studies of science is, ipso facto, a practitioner of the latter. Perhaps that is what Rouse really means by saying, at the beginning of the article, that his intention is not to "reify cultural studies of science"; on any other reading, that would be precisely what he does. The most readily defined group of outsiders are the sociologists of scientific knowledge (my colleague Trevor Pinch is quoted to provide a convenient group characterization of sociology of scientific knowledge [SSK]). This becomes an astonishing demarcation when viewed from the historian's perspective, because much recent work in the history of science dealing most effectively with the culture of science is directly and deeply indebted to the work of SSK. In a world not otherwise in thrall to cultural studies, this work can readily be called cultural history.

The nearest thing to a coherent instantiation of cultural studies of science is found in feminist studies of science, which are shaped by a deliberately critical stance. However, most feminist historiography of science does not clearly fit the cultural-studies rubric. Some feminist scholarship that attempts to "reclaim" women of the past as significant "contributors" to science has been deftly criticized on feminist historiographical grounds by Dorinda Outram (1991). Although they often fit the cultural history rubric, historical studies of women in science and of views of women underwritten by scientific authority (e.g., Schiebinger 1989; Russett 1989), do not seem to be what Rouse has in mind; neither does historiography on the cultural history of sex (e.g., Laqueur 1990; Foucault 1990). Instead, Rouse appropriately singles out Haraway's (1989) *Primate Visions*, which appears to come closest to his ideal type; Evelyn Fox Keller (1985) may also be mentioned in this regard. Such professional historiography, however, is comparatively rare (see Jordanova 1993 for a valuable overview and discussion); the feminist focus is usually, not unreasonably, squarely on the present.

A more manageable approach than that attempted by Rouse might lie in the fact that much historiography of science is now devoted to studying the cultures of science. This can mean the cultures of relatively easily demarcated disciplinary communities—which is not to discount the essential, and in some measure constitutive, part played by the world "outside" these communities (e.g., Kohler 1994; Kay 1993)—or it can mean the cultural world of scientific practices not so readily demarcated from other practices, such as

technological work; political, religious, and other forms of social behavior; pedagogy; or even ballet (e.g., B. Hunt 1991b; Cannon 1978; Shapin and Schaffer 1985; Iliffe 1992; Gooday 1991; Feldhay forthcoming). This work points toward a much more sensitive understanding of how to approach the conformations and behaviors of scientific communities—the culture of science, taken in a sociologically structured, anthropological sense of the term "culture."

Much of this work owes a considerable methodological debt to the British sociology of scientific knowledge developed in the 1970s by, above all, David Bloor, Barry Barnes, and Harry Collins (Barnes 1974, 1977; Bloor 1991; Collins 1992).[3] Their work was guided by the idea that even the most apparently unproblematic conceptual steps in the conduct of a science—such as had routinely been represented by the majority of philosophers of science as straightforward matters of empirical test and replication—were, in fact, the outcomes of complex processes of debate and consensus within the relevant groups of scientists that could be sociologically investigated. Meanings of experimental results and field observations, or even the question of whether an experiment was competently performed or not, were shown to be routinely debated and contested. For the historian, this opened the possibility of studying how scientific communities were constituted and how they operated in specific times and places to produce knowledge.

One of the cultural locations most widely investigated from this perspective, certainly in a methodologically self-conscious way, is Victorian Britain. Martin Rudwick and James Secord have developed detailed pictures of the English geological community that produced two notable controversies in early Victorian geology (Rudwick 1985; Secord 1986). As a result, not only do we know more about the central figures in this community but we see how the certified knowledge that resulted from their activities and interactions came to be made and what it was made *of*. Both historians endeavor (1) to present the geological community as an articulated social organism rather than a homogeneous thought-collective (cf. Fleck 1979) and (2) to explain the way in which this organism and its parts functioned ecologically—that is, within the greater society within which it lived. The two studies are by no means clones, however. Rudwick's book is focused more squarely on the development and closure of a scientific dispute, tracking the dynamics of the creation of new knowledge about the world: it stands as a contribution to science studies as much as to history and throws down the gauntlet to any who would challenge the representativeness of the case for science as a whole (Rudwick 1985, 16). Secord's book places itself in a context of Victorian cultural history. Geology happens to be its particular focus, but its intention is to contribute to a wider understanding of the Victorian world. Nonetheless,

it makes important statements about the proper way to study science. Secord wants to show the conventionality and culturally determined character of the drawing of boundaries in nature, here exemplified by stratigraphy. Both books present scientific knowledge as a cultural product, not just because of a contingent mode of its production, but in its very nature. And the whole enterprise involves sticking closely to a social analysis of the community's workings and makeup.

The same generalizations apply to articles produced by what can be called "the Cambridge school," consisting of Simon Schaffer and his students (notable examples include Schaffer 1992; Gooday 1991; Morus 1992; Warwick 1992-93). They focus on Victorian (and early twentieth-century) physics and accomplish results similar to those of Rudwick and Secord in geology. Their articles and the similar earlier work by Bruce Hunt (1991a, 1991b) and by Crosbie Smith and N. Norton Wise (1989, esp. pt. 4) paint a portrait of a scientific community, or communities, working hard to create stable procedures and concepts in electromagnetism. In this sociological and cultural history, the distinction between "wider" culture and the cultures of specialist groups of academic physicists and electrical engineers is contingent and perennially, actively reconstructed as part of the historical stories themselves (see also Desmond and Moore 1992). Technical achievements, such as well-entrenched, and hence reliable, units of electrical resistance, are shown to be the same as the achievements of architects or theologians: made by social groups as characteristic cultural attainments.

IV.

Steven Shapin has done a considerable amount of work in this vein on the seventeenth century, and the book he wrote in collaboration with Simon Schaffer, *Leviathan and the Air-Pump* (Shapin and Schaffer 1985), has become a classic in the history of science. His work, like the work just considered, illustrates once again the vacuity attending the overserious use of the term "cultural history of science." If Joseph Rouse's characterization of cultural studies of science, including the historical, were to be applied here, only paradox would result. Rouse claims that the characteristic feature of cultural studies of science, in contrast to the SSK mode, is that the former intends reflexively to involve itself in scientific endeavor; to be "critically and epistemically engaged" (Rouse 1992, 20). Quite what this means in practice is less clear, although a passage from Donna Haraway is invoked to support it. By contrast, SSK is said to be concerned with attempting to place social science at the same level with natural science, as part of its ambition

to understand natural science sociologically. It is, said Rouse, "uncritical of scientific practices" (Rouse 1992, 22), and satisfied with describing them and accounting for them accurately.

But if one wished to distinguish between Shapin's work on the seventeenth century and other historical scholarship that presents itself as "cultural" history (including Schaffer 1992), one would find that Shapin's is the most self-consciously sociological, insofar as he typically motivates his studies with reference to, and discussion of, sociological theory (especially Shapin 1994). He does so in order to coordinate his findings with issues relating to present-day science and the concerns that arise with respect to it. Shapin, who is the most unremittingly indebted to SSK in his historical work, is also the most unremittingly concerned with modern science and is emphatically not "uncritical of scientific practices" insofar as he wishes for a much greater public awareness of, and hence potential to address and shape, what scientists do (Shapin 1989, 1993).

Students of the seventeenth century, Shapin being the most prominent, who have studied the culture of scientific communities have done so in a peculiarly fraught arena. A focus on the natural philosophy and mathematical sciences of early modern Europe has created, by necessity, a sense of the clear distinctions at many levels between the practices of that time and place and those of modern science. It would now be possible to maintain, as Andrew Cunningham has done most vociferously, that science in its modern-day sense simply did not exist before the nineteenth century (Cunningham 1988, 1991; Cunningham and Williams 1993; Jardine 1991, 102; also more generally Schuster and Watchirs 1990). Arguable as this position is, it remains a minority view, and for good reason: if the knowledge-making practices of the early modern period were not admitted as part of the history of modern science, then historical understanding of the character and creation of modern science itself would become impossible.[4]

Modern science did not appear de novo, out of nothing, and its very character must depend in crucial ways on those conditions that permitted and brought about its appearance—the cultural precursors, to speak teleologically, that provided models for the development of new ideologies of knowledge in the nineteenth century.

Thus, to take an example, although the emergence of experimental practice in the seventeenth century does not necessarily characterize the experimental sciences of the nineteenth and twentieth centuries, understanding the former does permit a greater understanding of the meanings, presuppositions, and prerequisites of a form of knowledge-making that played a crucial role in the gradual appearance of nineteenth-century experimentalism. By analogy, the liberal nation-state of the nineteenth century differed from the European

states of the eighteenth century and earlier, but the creation of the former can only be understood in terms of the latter, and the creation of the former only takes on meaning as an expansion of the structural possibilities inherent in the latter. Similarly, the peculiarities of the modern science that arose during the past two centuries can only be understood by reference to the meanings of the science (as one might just as well refer to it in this context) of the previous couple of centuries. However problematic it might be, to disallow the connection simply invites a nominalistic erasure of historical explanation.

That, then, is the mission, explicit or implicit, of the historical studies presently under consideration. Older approaches to the so-called scientific revolution of the sixteenth and (especially) seventeenth centuries tended to play down the differences between the "science" of different times and places, regarding science as a self-contained intellectual tradition that could be studied and understood without serious reference to the social realities that constituted its conceptual form. Appreciation of the paramount importance of local differences in time and space, and the fact that such differences are not merely accidental but constitutive, lies at the heart of recent studies of science that may be called sociocultural. This kind of work rejects what might be called the "Platonic" view of the history of science, a view wherein the cognitive content of science exists in a Popperian "Third World" and develops independently of the human actions that manifest it (Popper 1972, esp. chaps. 3, 4). Thus the old story of the scientific revolution (central to the entire field of the history of science until about twenty years ago) traced the "origin of modern science" from Copernicus to Kepler to Galileo to Newton (Hall 1983 and Cohen 1985 exemplify the approach). The ideas of one figure fed into and were developed naturally by the next, until the views and approaches implicit at the start became fully explicit at the end, the self-evident culmination of the "revolution." Such history could not be characterized in any way as cultural or sociocultural because it allowed too little contingency: only accidental matters of timing and personality were contingent, whereas what happened, sooner or later, was always there, ready to appear. The philosopher Imré Lakatos, although perhaps annoying some historians in the 1960s and early 1970s with his "rational reconstructions of the history of science," nonetheless caught the spirit of the enterprise very well—better, in fact, than most historians engaged in it, who would have rejected the above characterization indignantly (Lakatos 1978).

The newer approaches create their effect by promoting a sense of strangeness. Their aims are advanced when they allow the subject to be "science," despite the aforementioned considerable differences between the enterprises of the seventeenth century and modern science; given that, however, the science, especially the more apparently familiar aspects of it, made to appear

as much contextually bound, and contextually endowed with its authentic meaning, as possible. The central focus in this literature is the experiment, or experimentalism in general. Shapin and Schaffer (1985) attempted to localize the invention of experimental science, using Robert Boyle and the early Royal Society as their subjects and assessing them in relation to the arguments and positions of Thomas Hobbes, who worked hard to undermine the legitimacy of the experimental philosophy that Boyle tried industriously to establish. Shapin's latest book (1994), described as being tantamount to the "first" book, of which *Leviathan and the Air-Pump* can be regarded as the "sequel," extends these themes into a more general examination of the prerequisites of an experimental science. Shapin looks in particular at the creation of "matters of fact" through the medium of accredited belief—an unmistakably social matter that is realized through investigation of the culture of the relevant groups. Here the central group is that of "gentlemen."

Other historians have examined similar themes in the early modern period. Experimentalism routinely holds an important place in their establishment of the appropriate historical problematic (Pumfrey 1989; Golinski 1989; Dear 1991). The advantage of studying experiment and experimentalism is that experimental activity is visible and, in some obvious respects, easily attributed a sociocultural meaning because of its brute practical dimensions. If one focuses on theoretical or methodological ideas rather than material practices, "socializing" them often reduces to the presentation of isomorphisms between scientific ideas and social ideologies.[5] Instrumental uses of ideas about nature, or methodological precepts, are the most effective means of accomplishing the task convincingly, but the actual performance of experimental work speaks most dramatically to the concerns of those who would exempt genuine "science" from "social" determination—"nature" as opposed to "culture."

Steven Shapin's work encapsulates the crucial perspective of the sociocultural approach quite neatly: he deploys, and demonstrates, the aphorism that "knowledge about things is also knowledge about people" (Shapin 1994, chap. 6). This assertion has the virtue of locking together the special subject of the history of science, namely knowledge of nonhuman things in the world, and the usual subject of history, namely, human beings and their behavior. It also shows the point of the connection between "socio" and "cultural." There are no definitive "things" without concomitant judgments of people to establish those things. Shapin provides seventeenth-century examples of objects such as comets or icebergs of which the characteristics, or very existence, depended on a network of personal relations of trust and interpersonal assessment comprehensible only through the tools of social history. That is why his new book is *A Social History of Truth*.

Chemistry has been another focus for studying the cultures of science. Owen Hannaway's (1975) classic study, *The Chemists and the Word*, is an exemplary and influential piece of cultural history; it is not reductionist but it is not mystificatory either. The book interprets divergent ways of construing a subject by divergently located historical actors and provides an understanding of the making of a scientific (or pedagogical) discipline in the years around 1600. Jan Golinski has written largely on British chemistry in the eighteenth and early nineteenth centuries from a perspective heavily influenced by SSK, whereas Lissa Roberts has adopted techniques of investigation that are less rooted in a social analytic fabric and are more concerned with the behavioral complexion of chemical practice, both linguistic and experimental (Golinski 1992; Roberts 1991, forthcoming; Christie and Golinski 1982). Such studies and the related work in the history of eighteenth-century medicine—for example, Porter and Porter (1982)—are characterized above all by their naturalism (Shapin 1980 on "contextualism"). This would be unremarkable in any area of history other than the history of science. In some cases it is even questionable whether such studies would be recognized as cultural history (as opposed to social history) if they were considered as part of general history. Naturalistic historical studies of science are regarded as methodologically noteworthy only because of the epistemological issues implicated in the practice of the history of science as a whole. This is the paradox of cultural history of science.

V.

The most disciplined use of the word "culture" that has found its way into the historiography of science is that borrowed from anthropology. Few explicit examples can be adduced, but those few are noteworthy, not least because of their intersection with certain examples of established cultural history. Some social anthropologists, most notably Robin Horton in the 1960s, devoted attention to how non-Western peoples produce knowledge about the world in their cosmologies (Horton 1971). They drew explicit comparisons with modern Western science. In a review of the relevant literature, Steven Shapin (once again, 1979) examined the alternative approaches considered by anthropologists and their possible value to historians of science. Shapin argued that the anthropological discussants all assumed a straightforward, rational-empiricist view of science and developed their analytic approaches to the cosmologies of other cultures in that light. He suggested that some of their interpretive ideas might profitably be applied to Western scientific practices themselves. The common anthropological under-

standing of culture as an effect of social organization interpretable in terms of social exigencies (symbolic, functional, or whatever else the particular theory might require) gave Shapin's approach a good deal of intellectual discipline.

In a somewhat similar vein, David Bloor (1978) suggested that the "grid/group" theory developed by the cultural anthropologist Mary Douglas might be an effective tool, or at least research heuristic, in understanding the knowledge-making practices of scientific or mathematical communities. Grid/group analysis relates the manner in which a community classifies nature (whether in terms of experimental results, natural history, or mathematical theorems) to its social structure. A "high-group" community does not freely admit outsiders, whereas a "high-grid" community is internally structured in a very inflexible, usually hierarchical, system; low group and low grid designate the opposite characteristics. Douglas had argued that these features of a society, arranged along orthogonal axes to designate low and high group along one, and low and high grid along the other, mapped out regions that corresponded to particular forms of organizing proper classificatory behavior. Bloor borrowed the idea as a hypothesis applicable to the knowledge-making classificatory practices of the sciences. In the early 1980s, historians such as Martin Rudwick and Ken Caneva also applied this idea; its possible value to the historian of science was endorsed more recently by David Oldroyd (Douglas 1982; Oldroyd 1986). Grid/group analysis creates the possibility of understanding knowledge practices through their relation to the society or community that engages in them, rather than leaving those practices unaccounted for. Thus a high-group, low-grid community ought to exhibit the intellectual style called "monster barring," whereby deviations from the norm are ruthlessly rejected. Bloor's aim was to make sense of the inside of scientific knowledge with reference to the particular social structures of particular scientific communities. Bloor also confronted the practical problem, crucial for the working historian, of how to move between the statements of individual historical actors and the characterization, developed by the analyst, of their community's thought style. The approach is sociocultural, or even social-scientific, rather than cultural-historical, precisely to the extent that it is concerned with a social contextual understanding of the content of scientific thought. It is crucial, however, to recognize the explicitly conjectural status of its sociological assumptions.

Another anthropologically related historical style derives, in part, from anthropological borrowings by historians of general culture, especially early modernists. Robert Darnton's (1984) essay on "The Great Cat Massacre" is explicitly built on ideas promulgated by the anthropologist Clifford Geertz (1973). The central notion is the well-known one of "thick description,"

which carries a slightly misleading air of technicality with it: the notion incorporates the view that the only way to understand an aspect of an alien culture (such as a cat massacre in eighteenth-century France) is to triangulate on it from many directions so as to uncover as many relevant connotations of the event or behavior as reasonably possible (the process could in principle continue forever). The intention is to create an understanding by traversing those parts of the network of meaning that create the event or behavior as a discrete entity to begin with. Few historians of science have used Geertz's methodological perspective explicitly. Lissa Roberts, however, has recently used it as a supplement to a more traditional exercise in cultural history, appropriately adapted to the history of science (Roberts forthcoming).

Roberts performs an exercise akin to Darnton's, but with an extra, preparatory step typical of much recent history of science: she renders something that historians have usually regarded as "rationally" comprehensible into something alien, then makes sense of it through a kind of thick description that exploits the very sources from which she achieved the effect of strangeness itself. Isomorphism, often a weak device in studies of scientific culture, plays its part, but it is used here for a purpose different from that of attempting to show a subterranean causal link: Roberts attempts to reveal new features of Lavoisier's chemical science through a metaphorical comparison with the operation of the guillotine as a means of execution during the French Revolution. The guillotine was a rational, mechanical, impersonal, and above all *precise* dealer of death that appeared to remove direct agency from the human executioner; at the same time, attempts were made to retain the dimension of public spectacle attending an execution. Whenever things went wrong, however, the machine's operator received the blame. A thick description approach to guillotine executions renders visible parallels with a similar kind of thick description derived from investigation of Lavoisier's famous experiment to crack water into its purported elementary constituents of oxygen and hydrogen. Thus "precision" once again characterizes the way in which the accomplishment is to be seen, and just as the guillotine instantiates a universal legality in the civil realm, so the decomposition of water instantiates a universal law (inseparably entwined within Lavoisier's new chemical system) of chemical science. The argument appears to rest on the assumption that the reader will be more prepared to accept the reading of the meaning of the guillotine than that of the chemical decomposition; thus the former as it were "softens up" the reader to accept the thick description proffered for the latter. Roberts also wants to make something of the proximity in time and space of the two foci, so as to see them as similar expressions of underlying, deeper cultural forms (see also Dear 1990, for another attempt at this).

From a different direction, Steven Harris has adopted another notion of Geertz's, called "strain theory," to lend a dimension of social-scientific theory to his examination of Robert Merton's celebrated thesis concerning Puritanism and science in seventeenth-century England (Harris 1989; Geertz 1964). The idea involves understanding an ideological system as a response to tensions in the sociocultural fabric of a community. Here, in a rather more "scientistic" fashion, anthropology provides the historian with tools for understanding behavior in terms of the (independently theorized) characteristics of the society within which that behavior takes on its meaning.

VI.

Charles Gillispie once observed, at the conclusion of a Princeton gathering organized by Lawrence Stone on the "return of narrative history" (this was around 1980), that, after all, "historians are better than their theories." This remark was not, I think, made flippantly: it contains a deep truth about historiographical practice. "Historians are better than their theories," even though they must, of course, use them. Good historical research and writing do not proceed on the basis of some literally preconceived theoretical stance, which the historical material then serves to illustrate; the relationship is much more complex. The specificity of history, the ways in which historical understanding is created from a hermeneutical interaction between historian and evidentiary materials (Ashplant and Wilson 1988), yields results that transcend the confirmation of social-scientific or other theories. This is the important sense in which the discipline of history is not one of the social sciences (although there are others in which it is). The temporal relationship between past and present is not an accidental feature of historical scholarship; the crucial difference between history and other disciplines that use materials from the past is that, whereas the latter treat their subjects as if they were all located in a *virtual present*, the historian knows that temporal relationships possess meaning: dates are not simply for convenient cataloging.[6] The studies that have been discussed in this article use a variety of approaches to "cultural" historical issues, but they all attempt to engage with their materials in such a way as to illuminate some inner coherence; to render the past intelligible by conjectured reference to the present, whether substantively or in relation to current theoretical constructs.

The past becomes a virtual present when its moorings in the specificities of another time and place (literal or figurative) are lost. In the historical study of culture, including that of science, losing sight of the constituting social

world results in the study of chimeras: cultural meanings that are not also social meanings are exactly like colors that are not colors *of* anything—they are properties without a subject. Culture is real; but it is not a thing. It is a property of its society, not a social epiphenomenon but what an Aristotelian would call an essential (i.e., constitutive) property.

Notes

1. Lissa Roberts suggested to me the idea that perhaps the "new" cultural history of science was running the risk of replicating precisely this distinction.

2. This matter came up during E-mail discussions with Jan Golinski. For analogous concerns see Chartier (1982).

3. It is important to note the ambiguity of the term "sociological" in this regard. Here, used to refer to approaches indebted to the sociology of scientific knowledge (SSK), it does not refer to a dependence on explicit prior theories of such things as social structure (indeed, many sociologists, especially in the United States, are loath to accept SSK as being sociology at all). Instead, the focus is on contingency in the outcome of scientific disputes, and often (but not necessarily, or even usually) on an investigation of purported social interests to explain the playing out of that contingency and its resolution. The focus is often on the historical actors' own construals and constitutions of the issues, whether seen as "social" or conforming to some other category.

4. Cunningham's view does not necessarily lead to this conclusion, but it ought to.

5. But by no means necessarily: see Shapin 1980; much of Barnes and Shapin 1979, for examples. In the case of methodologies, there are some studies that show the practical uses of "method talk" in scientific politics, broadly and narrowly construed (Schuster and Yeo 1986).

6. This is the important sense in which Bruno Latour is a historian: see, for example, Latour 1988 on "making time," as well as Latour 1993.

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