Should the Sociology of Science Be Rated X?

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ABSTRACT: Stephen Brush once asked, "Should the history of science be rated 'X'?" Well, times have changed. Now controversy reigns over the role of sociology in the science classroom. Here, I profile several dimensions of the sociology of science and survey recent efforts by philosophers to address its most radical claims. In respecting the opposing poles of this contentious debate, I contend that it is possible to chart a path between the Scylla of scientism and the Charybdis of relativism. Most notably, educators must differentiate between the normative and descriptive elements of the 'nature of science'—and teach both. Further, they must go beyond the rhetoric of tentativeness and fallibility in science by describing just when and how individual scientists and/or a scientific community can err—and how they identify and remedy their errors. © 2004 Wiley Periodicals, Inc. *Sci Ed* **88:**934–946, 2004

INTRODUCTION: FROM X-RATED HISTORY TO THE SCIENCE WARS

In 1974, Stephen Brush famously asked in the pages of *Science*, 'Should the history of science be rated 'X'?" He noted that the ambitious and unsavory behavior of scientists of the past might not set good role models for recruiting future scientists, playfully adopting America's new film rating system (also see Holton, 1978). Well, nearly 30 years later, Brush (2002) has recanted somewhat: *some* scientists may indeed be good role models. In the meanwhile, many national education standards have come to promote history and nature of

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science as integral to scientific literacy. History of science now seems to be rated "G": for a General audience.

Instead, we are plagued by another question: "Should the *sociology* of science be rated X?" Ethnographers have profiled how sometimes contentious politics within science can shape consensus (Latour & Woolgar, 1979). Some sociologists of science, notably Harry Collins (1985), have stressed the "interpretive flexibility" of data and suggested that struggles for credibility dominate science, quite apart from objective standards of the quality of evidence. Others have underscored how cultural values permeate scientific knowledge. For example, one finds that the phrenology dispute in Edinburgh was divided along economic class lines—with each side arguing for its own social power, perhaps more than some objective "truth" (Shapin, 1979). Robert Boyle and Thomas Hobbes, in debating the nature of a vacuum and the possibility of an air pump in creating one, mirrored their political views about governance and the monarchy in 17th century England (Shapin & Schaffer, 1985). The idealized and impersonal scientific method found in textbooks seems not to describe how science actually works in practice. Many see sociology of science as thereby threatening the legitimacy of science. The fear among many seems to be that science will be reduced to nothing but politics. Social interests will be viewed as eclipsing rationality. If one cannot establish that science adheres to objective, universal standards for knowledge, then it cannot escape the awful Charybdis of utter relativism. And if there are no scientific laws, how could one justify laws of any kind? All will be chaos. Anarchy will reign. For some, then, sociology has no place in the science classroom (Slezak, 1994; Irzik & Irzik, 2002). It should be rated X.

Others, by contrast, feel that sociological studies of science offer important cautionary tales about the limits of scientific knowledge and the inappropriate use of scientific authority. History is rife with cases of science giving unwarranted authority to cultural prejudices. For example, early in the 20th century, science seemed to legitimize IQ testing as an objective measure of intelligence (Gould, 1983). These tests, in turn, were used in the United States to deny immigration to Eastern Europeans (who could not speak English) and later to set immigration quotas that discriminated against them, predominantly Jews. H. H. Goddard, Charles Davenport, and Karl Pearson, the leaders in the field, were well aware of the political overtones and saw them as supported by their science. Science may have "corrected itself" in the long run, but the discrimination supported by science for decades affected perhaps 6 million individuals who tried to escape Nazi Europe. As Gould (1983) points out, we know what happened to many of them. For some, then, sociology of science—as vividly illustrated in such cases—helps illustrate the monstrous Scylla of unbridled scientism. Scientific authority can be and has been misused. Prudential caution is warranted. Sociological case studies are thus critical in the science classroom for developing informed citizens (Kelly, Carlsen, & Cunningham, 1993; Roth, 1997). Sociology of science should be rated "E": Essential for Everyone.

Disagreement among educators persists. Despite an emerging consensus on what to teach as "the nature of science," consensus fails precisely on questions of the reliability and authority of science (Osborne et al., 2003). Among these are "the status of scientific knowledge" (whether it can be relied on as a basis for action); "the empirical basis of scientific knowledge" (as a basis for its reliability); and "the cumulative and revisionary nature of scientific knowledge" (failing to acknowledge, for example, substantive error and major conceptual reorientations) (pp. 701–703). Why does consensus on these issues continue to be problematic?

The difference of perspective about sociology of science parallels, of course, larger cultural debates: the science classroom has become contested turf in the lingering Science Wars. Of course, the disagreement about sociology of science is not an armchair intellectual debate. It is a power struggle. It is about the scope of authority of science. Hence, the discourse typically evokes deep emotions. Under standard protocol, one first declares one's allegiances, so that everyone knows whether to treat the speaker as a hero—or to despair that they have succumbed to some imagined "axis of evil" (the roles assigned alternately, according to perspective). However, I propose rejecting the terms of the debate. In my view, *both* sides are wrong. Responsible intellectuals should condemn the bickering as shamefully unscholarly (Editor, 1997; Jardine & Frasca-Spada, 1997).[1] The whole debate is misframed by a socioeconomic Darwinism that ritualizes either/or competition. Others seek to close off debate by declaring separate and independent fields of expertise in philosophy and sociology of science. But such intellectual wallpapering hides rather than resolves the problematic intersection of perspectives. Contending that divergent domains of expertise are adequate is equally irresponsible. Productive integration is due (Segerstråk, 2000). To do this, one must acknowledge that each perspective has some warrant (first section below, on "Interpreting Sociology and the Nature of Science"). The creative challenge is to map a way to simultaneously accommodate the central concerns of both (second section, on "Reconciling Philosophy and Sociology of Science"), and then translate the resolution into educational terms (final section, on "Strategies for Educators").

INTERPRETING SOCIOLOGY AND THE NATURE OF SCIENCE

How, then, might the science educator (or anyone) interpret sociology of science productively, not just as critical or negative? How might sociology be important in characterizing the nature of science in the classroom? Kelly, Carlsen, and Cunningham (1993) offered an effective survey of sociology of science earlier in this journal. Here, I want to additionally provide a framework for interpreting the scope and structure of the sociologists' work and further articulate the problems about scientific authority they raise.

To begin, one may sort sociology of science into three types, each posing different challenges (Pickering, 1992; Hess, 1997). First, there is macrosociology of science, which focuses on the institutions of science and the social framework of peer review, rewards, funding, etc. (Cole, 1992). As long as sociologists talk, like Robert Merton (1973), about institutional norms or, like Derek de Solla Price (1963), about "big science," all may be well. Here, the student may be healthily engaged in contemporary issues in science policy, from genomics to large particle accelerators (although many might argue such topics belong in a *social studies* class). One may even discuss how scientists guard their professional boundaries from those seeking its cultural authority (Gieryn, 1999; Toumey, 1997).

Here, one also encounters some notorious cases of fraud, where the system of trust and credibility fails. Is fraud part of science? For some, that depends on whether it is frequent or not. It would be foolish to suggest that the whole of science is a lie because it is nothing but fraud. Yet it would be equally blind to contend that cases of fraud are so rare as to be hard to find. The gossip runs thick and heavy in the pages of *Science* magazine, epitomized recently in the cases of erstwhile wunderkind Jan Hendrik Schön of Bell Labs (Service, 2002, 2003) and ecologist Andres Pape Møller (Vogel, Proffitt, & Stone, 2004). So, do students get the real story? It would be dishonest to deny or discount fraud. It would be equally inappropriate to condone it or legitimize it. Here, one can perhaps detect a fundamental tension in how one construes "nature of science." Is it the real or the ideal? Is it the "is" or the "ought"? The ambivalence between normative and descriptive (noted by Kelly et al., p. 217) is critical and is addressed more fully in the section below, "Strategies for

[1] The names of Paul Gross and Norman L. Levitt, as well as Alan Sokal and Lewis Wolpert, are particularly notorious. I will not honor their published work as scholarly by citing it.

Educators." Regardless, note that the U.S. National Academy of Sciences (1995) considers fraud significant enough to address substantively in its public brochure *On Being a Scientist*.

Second, there is microsociology of science, which addresses the interpersonal dynamics in the lab. How does one interpret the behavior of scientists and their cryptic "inscription devices," such as amino acid analyzers? How do scientists' activities yield knowledge? These studies often provoke persons steeped in science because the anthropologists or ethnographers generally adopt a *methodological* perspective of agnosticism towards facts. They aim to dissect facts, not read them transparently. Accordingly, they tend to focus on behavior and language over empirical content. Arch-realists cringe. But nothing in these studies ultimately excludes a realist stance. If one just pauses long enough to examine the work, one can easily see that these studies describe quite well the day-to-day practice of science. As Latour (1979, 1987), in particular, notes, our understanding of nature is the *outcome* of a deliberative process. Data are rarely unambiguous and empirical "facts" are not established without considerable "negotiation" among alternative interpretations. Determining "the simple fact of the matter" is not always as simple as suggested in widespread images of science. Considerable interpretive resources are deployed in ascertaining the meaning of experimental results. Sociologists show how much *work* is involved in science. From an epistemic perspective, they tend to profile the many factors, viewed as potential sources of error, that must be addressed before scientists feel secure about the facts. Many apparently trivial events may be significant. Sociologists also underscore that one cannot justifiably read the outcome of the debate anachronistically onto the process that led to its resolution. Microsociology has dramatized the difference between "science-in-the-making," as found in research settings, and "science-made," as found neatly edited and arranged in textbooks (Latour, 1987). Rational reconstructions hide the uncertain horizons and contingencies in how real science proceeds. If one is interested in teaching process of science, not merely science content, microsociological studies are important resources.

The language of microsociologists can be inflammatory because they claim to explain scientific knowledge causally. In their jargon, scientific knowledge is "constructed," or "socially constructed." But, again, the prudent observer does not succumb to mere rhetoric. Ultimately, *of course* knowledge is constructed. Scientists articulate facts where before there was uncertainty or ignorance. Justification, in particular, must be cobbled together from disparate shards of evidence. And *of course* science is socially constructed. Scientists work *collectively* as a *community*. Indeed, as underscored further, diversity of perspectives often serves as a vital system of checks and balances in detecting bias or error. Hence, we need to dismantle the mythic impression that to be social or constructed is to be irrational or relativist (Geelan, 1997; Bickard, 1997; Grandy, 1997; Longino, 2001). The rational/social dichotomy is false. Science cannot be divorced from its human context. Science is a human activity. And if humans err, then science will as well. We need to understand how and when social factors foster rationality, not disavow the social character of science.

The third (and perhaps most challenging) type of sociology is cultural studies of science, which probe the relations between social ideology and scientific ideas and methods.[2]

[2] Another branch of cultural studies of science underscores the cultural context of scientific methodology and the variant knowledge systems that may result in different cultures. For example, knowledge of acupuncture emerged from and remains largely embedded in traditional Chinese medicine, outside the Western canon. (Other frequently cited examples include indigenous traditions in healing, agriculture, or ecological management.) Such examples raise important questions in comparative philosophy of science (Allchin, 1996b) and alert the educator to the depth and scope of issues in cultural context discussed in the section Reconciling Philosophy and Sociology of Science. Where the primary concern is the reliability or authority of the knowledge claims, the strategies about diversity of perspective profiled below (Longino, Harding, Solomon) largely apply. In cases where Western and indigenous knowledges remain in conflict,

Scientists inevitably draw ideas and values from their culture. Bias can result, carrying the imprimatur of science, as illustrated in the case of IQ and Jewish immigration. Consider, further, the case of craniology in the 19th century. The goal was explicit for many: to justify (or perhaps understand) the natural intellectual inferiority of women and non-European races. Yet the practice seemed to follow standard norms of "good" science. For example, craniology was a quantitative science par excellence, with 600 instruments and, according to one researcher, over 5000 requisite measurements of the skull. When evidence did not fit their theories, scientists revised them, shifting from raw cranial size to more subtle measures of body weight ratios or facial angles. But they did not question the enterprise itself, nor see its potential for bias. Rather, it was good social application of science (Fee, 1979; Gould, 1980). Of course, the whole field was flawed. But why was science not immediately self-correcting? Why was the error not detected and remedied at the outset? For example, Samuel Morton, a leader in craniology in the United States made assumptions about which skulls to include in his samples for studying racial differences, skewing his results significantly. He hardly noticed his error, nor did others (Gould, 1981, pp. 50–69). Moreover, craniologists also did not notice, until women entered the field and applied more rigorous statistical tools, how their conclusions were based on incomplete reasoning (Fee, 1979). Craniology is one prime exemplar of the practice of science deeply and adversely influenced by cultural values.

Cultural influences are certainly not all negative, however. The same sources (or biases) also lead to discovery. So, for example, Michael Faraday was inspired to look for the relationship between electricity and magnetism because his religion espoused the unity of nature. (He was not so lucky investigating the relationship between gravity and electricity.) William Buckland found fossils half-buried in a cave, and reported that the relics were proof of the Biblical flood. His research earned the Royal Society's highest honor, the Copley Medal, and inspired similar research across the continent. Charles Darwin was steeped in Victorian ideas about competition in society, which helped shape his thinking about natural selection. William Harvey believed in the Renaissance notion of the microcosm, which guided his reasoning about circulation of the blood. Ideas from culture can sometimes be productive. One cannot universally equate cultural influence with error.

Cultural studies of science have amply demonstrated that scientists reflect their culture in their thinking. This should surprise no one, really. Scientists work with the cognitive resources at hand. They will draw ideas and perspectives (biographically) from the social context in which they live. Cultural context may thereby shape what topics are pursued, what questions are asked, what observations are noticed, how evidence is interpreted, and what theoretical virtues are preferred. The scope and content of science emerges from culture—for better or worse. The image of science typically promoted in science education—as pure and isolated from culture—thus needs substantial transformation. Accordingly, teachers might profile, for example, how the fervor for genomics fits in an economic and political milieu that supports discredited notions of genetic determinism (Lewontin, Kamin, & Rose, 1984; Moss, 2002). Likewise, many claims in evolutionary psychology, a trendy but immature science, can be shown (like craniology) to express sexist and ethnic stances (Lyons, 1998; Angier, 1999). Critics of global warming and anthropogenic climate change may suffer from conflict of interest. By consensus, skepticism is part of good science. Cultural studies

educators may need to adopt an additional set of strategies, based instead on communicating uncertainty in current scientific knowledge (Rowell & Pollard, 1995; Friedman, Dunwoody, & Rogers, 1999). In local settings, ethical, political, or pedagogical reasons—independently of epistemics—may dictate engagement with indigenous knowledge systems. The recent philosophical perspectives profiled in the section "Reconciling Philosophy and Sociology of Science" may offer the educator guidance about how to negotiate the discourse among different cultures.

of science help students learn when and perhaps how to exercise such skepticism. The easily hollow slogan "limits to science" becomes concrete.

One common response to sociology of science, especially cultural studies, is to say, "Yes But": "Yes But . . . that's not Science. That's *pathological* science" (Dolby, 1996; Rousseau, 1992). In this approach, all error is deftly carved away from science and attributed to interfering "extra-scientific" factors, social and psychological. Science is thereby preserved pristine. Such a response is rightly dismissed as mere rhetoric, no more than a thin political stratagem. No mechanism ensures against pathological science, nor does any reliable diagnostic test distinguish it from pure science in context. But the appeal to pathological science is itself informative. It reveals an important guiding conception of science: that is, science is—or should be—perfect and error free. It can thus guarantee its claims in every instance. Here, science is idealized as an abstracted essence. *Authentic* science, in this view, does not err *by definition*. The contrasting view, of course, acknowledges science as actually practiced. Scientists do err, sometimes with significant social consequences. Each view, of course, leads to a different assessment of the authority of science. In one case, science, appropriately defined, is insulated against, or immune to, error. Science is thus the ultimate authority, especially where values enter discourse. In the other case, error is a part of science. Biases inherently limit its reliability. Here, the authority of scientists must be kept in check, especially where values enter discourse. The dilemma is clear here. To resolve the challenges of sociology, then, one must also address the very conception of science itself. Idealized science must be distinguished from—and not substitute for—science as actually practiced.

So, is the fundamental "nature of science" the ideal or the real? Is science characterized by its epistemic norms or by its historical practice? One may well imagine how these contrasting postures can provoke vehement disagreement about what should be taught. Ultimately, then, debates about error and the role of sociology of science echo Brush's earlier question about history of science: do educators portray science and scientists as they are or as they ought to be? (One might appreciate the irony that some defenders of empiricism support an idealized, yet empirically false, view of science.) The nature of science debate may seem to be at an impasse.

One may, of course (as I suggested earlier), reject the either/or choice. The nature of science embraces *both* the ideal and the real, as well as the tension between them. The nature of science is compound. Coupling the two views may seem problematic, however, because of a perceived incommensurability between philosophy and sociology of science. Sociologists have emphasized practice. They have typically studied what scientists actually do. Philosophers of science, on the other hand, have studied methodology abstractly, endeavoring to articulate good practice. Philosophy of science has focused on the normative; sociology, by contrast, on the descriptive. Moreover, practitioners in the two fields tend to posture themselves as rivals. Hence, the is/ought dichotomy seems embedded in divergent disciplinary perspectives. But this gulf is more apparent than real. Indeed, it is merely an artifact of history. No fundamental principle dictates this conventional dichotomy. It can be dissolved. Thus, one might plausibly develop a descriptive philosophy to document epistemic strategies and perhaps link them historically with various outcomes (Darden, 1991; Bechtel & Richardson, 1993). Likewise, sociology may be normative. This was certainly the case for Robert Merton (1973) when he postulated four benchmark norms for "the growth of certified knowledge" in the 1940s. Despite rhetoric to the contrary, the perceived conflict between philosophy and sociology can be reconciled. In the section that follows I profile several scholars who have accepted this challenge. All are concerned about establishing reliable knowledge. But they also acknowledge and address the findings of recent sociology of science. As a result, they are already significantly expanding the scope of philosophical conceptions of scientific methodology. Desriptive and normative accounts complement each other.

RECONCILING PHILOSOPHY AND SOCIOLOGY OF SCIENCE

Many philosophers were motivated by feminist critiques of science, which documented underrepresentation of women in science as well as gender bias in the interpretation of results—features that now seem more closely related than one might imagine at first. Sandra Harding (1991), for example, has clarified two types of errors. In some cases, evidence was grossly misinterpreted. This was just recognizably bad science, although male researchers typically failed to *recognize* it as bad. More important, though, in other cases, claims seemed consistent with the evidence collected. All seemed well by standard methodological norms. But the evidence was essentially incomplete and hence misleading. Harding thus distinguished between "weak objectivity" and "strong objectivity." Craniology, in these terms, never achieved strong objectivity. Harding also underscored how women—indirectly disadvantaged by the findings—noticed the errors. Their *standpoint* was relevant. Thus Harding has linked epistemics with who participates in science. An abstract philosophical goal involves a concrete social component. In this view, to achieve strong objectivity, a scientific community must have the appropriate diversity of perspectives, or standpoints—especially by those potentially disempowered by the conclusions. Harding (1998) has generalized the argument, noting in particular the role of race/ethnicity and nonindustrialized cultures as being relevant to assessing scientific claims.

Helen Longino (1990) was also inspired by feminist studies. She has focused primarily on conventional concepts of justifying knowledge, shifting to the community level, much as Joan Solomon (1987) and other educators have promoted a social component in learning. In particular, Longino asks how criticism (especially from a dissenting minority) is accommodated: whether opportunities exist for voicing criticism, whether cogent criticisms are addressed responsibly and weighed equitably, and whether a community can fairly judge the arguments. These conditions mark productive discourse in an objective critical community. The method of science that ensures its objectivity, while tied to empirical evidence and individual belief, occurs *at the social level of interaction among scientists*. Thus, a field that has reached consensus but does not have certain critical perspectives represented ought not be considered—*philosophically*—to have fully validated its claims. For example, the community that endorsed IQ testing failed, even though it seemed to respect the empirical evidence. Indeed, here one need not specify evidential norms (as in conventional philosophies), because they will arise from and be validated by the community. They may even change as research develops. Like Harding, Longino has shifted epistemic norms beyond the individual, to include the social. Her analysis accommodates the cultural values and background assumptions that different participants bring to science. For Longino (2001), the widespread rational/social dichotomy is unwarranted.

Miriam Solomon (2001) also addresses epistemology at the social level, as indicated in her ideas about *social empiricism*. Unlike Longino, however, Solomon insists that rationality can *only* emerge at the social level. Individuals each have particular perspectives, or biases. None has scope great enough. Solomon borrows substantially from cognitive science, especially studies of decision making. Accordingly, she adopts a naturalistic approach to epistemology; that is, success in science must be possible with the limited cognitive abilities humans bring to their work, not according to some utopian reasoning structure. Hence, without prejudice, Solomon analyzes a wide array of "decision vectors" ranging from empirical consistency and salience of data to theoretical simplicity and peer pressure. These recall the "agonistic forces" described by microsociologists (Latour & Woolgar, 1979). Decision vectors embrace motivational factors, social ideology, theoretical virtues, nonlogical heuristics, personality, gender, and appreciation of the data. All influence scientists' thinking. One cannot stipulate, however—as in the cases of religion promoting the discoveries of Faraday, Buckland, and others—whether any vector is always beneficial or detrimental. Any outcome is contingent on the historical circumstances. Still, one may assess the aggregate of multiple decision vectors. Consensus is rational, Solomon says, when a theory demonstrates empirical success *and* the decision vectors show a balanced distribution. So, for example, if Boyle and Hobbes had agreed about the vacuum, despite their contrasting political beliefs, one could express increased confidence in the rationality of their joint conclusion. Solomon, Harding, and Longino all see the social level as providing a system of checks and balances against particular cultural and other biases just when the scientific community is appropriately diverse. Hence, objectivity (or strong objectivity) may be an emergent property of scientific communities, not achievable by individual reasoning or methodology.

The case of Andy Pickering is also informative. His book *Constructing Quarks* (Pickering, 1984) was an early sociological study adopting an agnostic posture about scientific realism. But as Pickering's research has deepened, his position has shifted (1995). Like many sociologists, Pickering appreciates that representations of nature are not cast preformed upon a shore merely to be collected. Human agency is involved. No science occurs without scientists. No knowledge emerges that is not performative. So a story of discovery is ultimately about human behavior. Yet Pickering, once a particle physicist, sees that researchers also respond to how their *apparatus* behaves. He now refers to "the dance of agency." Experimentalist and experiment act reciprocally, in tandem. The experimentalist, as primary agent, crafts an experiment. Then he "tunes in" to its material performance. He responds differently according to the outcome. In that differential response, agency is shared with the material world. For Pickering, the world is necessarily filtered through an intentional, human dialectic. The human and nonhuman are melded in a machine-like "mangle." Fellow sociologist Bruno Latour, too, has adopted realist tendencies. His study of *Laboratory Life* (Latour & Woolgar, 1979) was hailed as a paradigm of arch-relativist ethnographic studies. But Latour soon started talking about "resistances" in the laboratory or field as harbingers of reality, at least as scientists interpreted them (Latour, 1987, 1988). His original actor-network theory allowed only humans to serve as "spokespersons" for experimental events. Soon it became an *actant* network, where objects or processes of nature seemed to have a role, although still mediated by human interpretation . Pickering and Latour represent sociologists who accept how human actions and interactions are shaped by what others would call plain empiricism. The difference from conventional philosophical notions, however, is the ineliminable role of human agency and all that comes with it. The microsociological jargon of "construction" does not exclude a role for some form of materialism. But it also refuses to eclipse the mediating role of humans.

In conventional philosophical interpretations, career ambitions and political maneuvering among scientists, while inevitable perhaps, weaken the reliability of scientific knowledge. Not so for David Hull (1988). He revels in an evolutionary view of science red in tooth and claw. Hull leaves judgments of methodology and quality of evidence to the scientists themselves. He is concerned instead with the system by which they will be motivated to demand rigor and even escalate standards. Hull thus endorses good old-fashioned competitiveness. As long as there are professional rewards for making discoveries and reliable accounts, scientists will aim to produce them. Other scientists, desirous of the same rewards, will try to do better. Thus, the Edinburgh phrenology dispute was not a blemish on science, but a mechanism for indirectly improving the antagonists' ideas. As in the economic system envisioned by Adam Smith, reliability in science is ensured by the "visible hand" of competition. Hull's proposal is contingent, of course. It depends on whether scientists act with the motives he ascribes to them and whether the system as he portrays it functions effectively. But Hull does demonstrate how (contrary to Brush's assumptions, perhaps) even unsavory motives might serve science through appropriate social organization. Even if his own system is wanting, Hull raises the important question of how social organization may guide human motivations to contribute to philosophical aims.

More problems arise when considering the social transfer of knowledge. Alvin Goldman (1986), like Solomon, acknowledges the limits of humans as cognitive agents, a form of naturalized epistemology. He has long advocated the role of strategies for regulating reasoning—that is, for working toward accommodating our inherent cognitive flaws. Goldman does not pretend that some idealized method will prevent all error. But this does not preclude us from adopting strategies to minimize them or to find and fix them. More importantly, Goldman (1999, 2002) has considered the problem of distributed expertise and knowledge. To say that something "is known" is not to say who knows it. There is a *demography* of knowledge. Problems arise in a system where intellectual labor is divided—a relatively recent problem for science. How can nonexperts judge the quality of knowledge if they cannot evaluate the evidence themselves? In particular, can someone who cannot justify the knowledge directly be said to know something (in the conventional philosophical sense)? The challenge in its more general form is clearly familiar to educators who aim to develop a citizenry of nonexperts able to assess and apply expert scientific knowledge in social decision-making (Norris, 1997). Here, the general problem concerns *social epistemology*. For example, we learn from others only through the exercise of trust (Hardwig, 1991; Shapin, 1996) and through a complex system of communication (media). But trust can be unreliable. Claims may be fraudulent (see section on "Interpreting Sociology and the Nature of Science") or incomplete and misleading. Professional communities may lack intellectual diversity. In a social setting, whose testimony can be trusted and when? Justifying credibility *socially* may be as important as justifying the evidence itself, for scientists and nonscientists alike. Goldman's strategy elsewhere—regulation of potential error—underscores the importance of the social practices whereby experts are trained, certified, monitored, and sanctioned. Analysis applies equally to communication systems and the dissemination of information through the media where, as Goldman notes, the traditional free marketplace of ideas does not uniformly foster public knowledge. Educators may recognize that the critical reading skills they have long taught may be constitutive of science, not merely a feature of "consuming" science. Hence, the problems addressed by Longino and Solomon are only the beginning of understanding the social dimension of science, interpreted in a cultural context.

All these examples indicate that sociology is helping to generate a more robust *philosophical* view of scientific method. I will conclude with one more example (Allchin, 1999). Philosophers and sociologists have argued at length about how scientists make judgements—whether they assess evidence or each other's credibility (Collins, 1985). The debate seems to epitomize the divergent emphases on ideal methods versus social practice. Using the accommodating posture I have repeatedly invoked here, one may well find that scientists do both. Indeed, anyone who listens to lab talk is aware that researchers frequently comment on the authors of journal articles and on whether the work in their lab is good. This is not mere gossip. Evaluating the skills of researchers (their "track record") proves to be a good strategy for assessing the quality of their work (whether in accepting it uncritically or scrutinizing it more thoroughly). No one can afford the time or resources to assess every detail or repeat every experiment, however ideal that may seem. Indeed, researchers frequently economize by using such heuristics. Sometimes, such heuristics fail. Then (only then) do researchers adopt the bulkier but surer methods—here, by plodding through the lab protocols, perhaps trying to isolate any experimental problem themselves. Credibility is thus a proximal criterion, used heuristically as a surrogate for an ultimate criterion, the evidence itself. Evidence may be more fundamental, but credibility is generally primary, in the sense of being applied first. Both criteria are used, each in specific contexts. One needs to understand, however, how they function together by reconciling philosophical and sociological views. Sociology of science is valuable, here, in developing a more realistic and nuanced view of how science works toward its ideal of reliable knowledge.

This all too brief review of philosophical perspectives by no means documents a new consensus. For example, some philosophers contend that diverse standpoints are achievable cognitively by individual creative thinking, rather than through social diversity. Others note the difficulty in knowing *which* critical perspectives are relevant in a particular case or *when* diverse perspectives are appropriately balanced. Specifying complete criteria for gauging the credibility or testimony of others is equally problematic, nor have I profiled important further discussions about the political dimensions of shaping the directions of research (Fuller, 1988; Goldman, 1992; Kitcher, 1993; Sclove, 1998; Longino, 2001). Puzzles remain. Positions are evolving. However, the examples illustrate the fruitful intellectual work in resolving the apparently conflicting perspectives of the Science Wars. In particular, they suggest paths for educators to engage the material more deeply than indicated by some philosophers of science (Elfin, Glennan, & Reisch, 1999). Concepts in science studies deepen (as they do in all fields), and responsible educators will rightly consider them as one further dimension in their ongoing professional development. For now, educators need to recognize what questions are being posed and how the polar extremes of a highly publicized debate do not exhaust all the alternative answers. Normative goals may be coupled with realistic expectations and pragmatic management skills.

STRATEGIES FOR EDUCATORS

Well, where does all this work on the intersection of philosophy and sociology of science leave educators? As noted earlier (see "Introduction: From X-Rated History to the Science Wars"), while educators agree on many elements of "nature of science," postures towards the empirical foundations and reliability of scientific knowledge still vary substantially (Tables 1 & 2 in Osborne et al., 2003). This seems to reflect residual ambivalence about sociology of science and whether nature of science is construed normatively or descriptively. This differentiation is a critical first step.

Principle 1: Educators should clearly differentiate between normative and descriptive elements of the nature of science—and address *both* in teaching science fully.

Most current characterizations of nature of science indicate somehow that scientific knowledge is fallible. However, handwaving is insufficient. Students deserve to understand why and how scientists err. The phrase "science is tentative but durable" is irresponsibly vague, if not self-contradictory (Good & Shymansky, 2001; Johnston & Southerland, 2000). Science educators thus need to adopt a more textured notion of nature of science. An idealized version (Davson-Galle, 2002)—especially of romanticized Science Triumphant (Chalmers, 1990)—is a lie, even if a guiding model. An image of science as bumbling through endless error (Collins & Pinch, 1993), without articulating how scientists manage the error, is equally misleading. To be complete, the nature of science must encompass both normative *and* descriptive.

Most of what sociologists have documented about the less inspirational episodes of science and about how culture shapes scientific conclusions is warranted. Students need to learn about these to help understand pragmatically scientific error and the boundaries of scientific authority. At the same time, awareness of failures and flaws needs to be coupled with understanding of methodological norms. How do they work? When do they work? Why do they sometimes break down? What may be the consequences? How do we recognize them? How does one remedy the errors? All these queries can potentially be modeled in student activities. The recent work in philosophy of science based on sociological perspectives (section above, "Reconciling Philosophy and Sociology of Science") can guide educators.

Principle 2: Every science curriculum should include an example of socially based error in science and how scientists later noticed and recovered from that error.

In my experience, the quickest and most direct way for students to learn about both the foundations and limits of scientific knowledge is through participatory historical case studies of error (Papacosta, 2000; Guinta, 2001; Allchin, 2000, 2003). For example, Christiaan Eijkman shared a Nobel Prize for the discovery of vitamins. His studies helped link the disease beriberi to an exclusively white-rice diet and its cure with the rice coating. Yet Eijkman thought that he had demonstrated that beriberi was caused by a bacterial toxin. Indeed, his interpretation was consistent with a vast controlled study involving nearly 300,000 prisoners. When the vitamin deficiency hypothesis was later proposed, Eijkman initially rejected it. How could Eijkman have been both right and wrong? How could his experiments have helped prevent so many cases of beriberi, while also being based on ultimately erroneous interpretations? Working through these puzzles in a concrete case, especially as a surrogate participant (Allchin, 1996a), offers a model for the complex science we encounter in contemporary social debates.

Teaching science without error, I contend, is like teaching medicine without disease or law without crime. The result is disconnected from real practice. One of the best ways to understand how science works is to appreciate episodes where it did not work ideally—and why scientists at the time thought that it did. That is where sociology, coupled with well informed philosophy, can be fruitful.

In particular, we may broaden the concept of method in science beyond the notions of experimental or observational sources of error (Allchin, 2001). Gender imbalance may lead to error. Fraud, cultural values, theoretical bias, and imbalanced exposure to data may lead to error. Scientific methodology, as illustrated by the philosophers discussed here, includes social and cultural elements. We can learn a great deal about reliability in science and about applying scientific knowledge to public policy by studying historical errors. Like students in school (ideally), we may learn from our past mistakes.

The Scylla of scientism that upsets sociologists is legitimately monstrous. It must be tamed, and cultural expectations of science deflated. The ideals of science are misleading if not qualified by how they are realized. On the other hand, the Charybdis of relativism that riles defenders of rationality is equally unacceptable. One must not conflate instances of actual practice with all of science or its aims, nor must flaws in the past eclipse the possibility that methods may help regulate bias—even cultural bias—and that methods may evolve. To chart a course between the Scylla of idealized scientism and the Charybdis of cynical relativism, we must adopt a dual nature of science.

Principle 3: Should the sociology of science be rated X? Decidedly not, but neither should philosophy of science.

The nature of science must include *both* normative and descriptive elements, clearly articulated and carefully distinguished. Understanding of the actual practice of science through sociology, with its occasions of error, must be coupled with the philosophical principles by which we endeavor to build reliable knowledge.

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